

The Ambiguity of Hybridity

An Encounter between Constructivism and
Latvian Sociology

Emils Kilis

A thesis submitted in fulfilment of the requirements for the degree
of Ph.D. in Sociology

University of Lancaster

March 2016

Declaration of Originality

I hereby declare that this thesis is entirely my own work and that any information derived from other sources and publications has been referenced and duly acknowledged in the text.

Author: Emils Kilis

Date: 7 March 2016

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Acknowledgements

There are many people I would like to thank for their contributions and support over the three and a half years it took me to complete this thesis. First of all, I would like to thank the community of academics and fellow graduate students at Lancaster University for providing a stimulating learning environment and encouraging an interdisciplinary outlook. In particular, I would like to thank my supervisors, Adrian Mackenzie and Bronislaw Szerszynski, for their feedback, patience and support, as well as Claire Waterton, David Tyfield and Brian Wynne for their comments at various stages of my research. I have greatly benefited from my conversations with Felipe Raglianti, Joann Wilkinson, Marton Fabok, Mette Kragh Furbo and Oscar Javier Maldonado in our informal science studies reading group, and their incisive comments on drafts of Chapters 3 and 6.

This thesis would not have been possible without the insights and support of my informants. I have tried to capture their views to the best of my abilities and recount their arguments in a respectful manner. I would also like to thank my friends for their suggestions, as well as their willingness to put me on the spot and compel me to explain in a straightforward fashion some of the more esoteric and counterintuitive ideas put forward in my dissertation.

Special praise goes to Elina who read and reread an impertinent number of early drafts and offered cheerful encouragement. She really does not have to read the whole thing again. Finally, I would like to thank the Economic and Social Research Council (ESRC) for funding my research and graduate studies.

Abstract

The Ambiguity of Hybridity: An Encounter Between Constructivism and Latvian Sociology

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With few exceptions, science studies has neglected potential insights that could be gained by studying sociology, a discipline that has played an important role in its own development. This thesis addresses the specificities of sociology from the perspective of constructivist science studies and explores the tensions that arise as a result of this encounter. The theoretical framework is based on actor-network theory (the work of Bruno Latour in particular), and supplemented with the work of Pierre Bourdieu (his theory of sociology in particular) to examine issues specific to sociology. Via a document and interview-based study of sociology in Latvia set against the background of Soviet science, the thesis argues that an open-ended and normatively saturated conception of knowledge hampers the stability of the discipline. I suggest, however, that these qualities matter vitally to the long-term development of sociology as a form of knowledge. Applied to sociology, a science studies understanding of science illustrates what happens when the intimate connection between political representation and scientific representation is not concealed, and hybridity is acknowledged.

Keywords: Sociology, science studies, actor-network theory, constructivism, Bourdieu, Latour, reflexivity.

Introduction

In the introduction to the book *Sociology Responds to Fascism* Stephen Turner and Dirk Kasler suggest that sociology has often been conceived as an oppositional science. Among the episodes tasked to illustrate this characteristic of sociology is the suppression of sociological scholarship under fascist rule (Turner and Kasler 1992). While medical researchers could be persuaded to commit acts of torture and abuse in the name of ideology, sociology was immune to such co-option because of the hostility the Nazi regime had towards sociological research. Such characterisations of sociology are often supplemented by an assumption that good sociology is perpetually at odds with reactionary regimes. Bad sociology, on the other hand, is one that legitimatises the interests and philosophies of those in power. In other words, the framing of sociology as an oppositional science contains an explicit assumption as to what constitutes good examples of the discipline and a particular understanding of the relationship between knowledge and power that sociology should exemplify.

The question of the relationship between knowledge and power in the context of social formations has a long history, but it is still plagued by a series of ambiguities that continually fuel debates across the social sciences and humanities. The question that animates this thesis is whether and how power extends to the management of knowledge production in the sciences. By this I do not just mean censorship but the ability to intervene and actively mould the content of scientific knowledge and the ethos of entire scientific disciplines. Thus, while “knowledge is power” suggests that knowledge can be empowering, I am intrigued by a different kind of connection – the ability of power to (i) determine what counts as knowledge and (ii) distribute the rights to say that it does. Some clarification is in order, however.

The idea that there is a social component to the production and distribution of knowledge is an old one (Berger and Luckmann 1991; Fuller 2006b), but the influence of external factors on scientific knowledge has traditionally been treated as something akin to interference and has generally been regarded as detrimental to the progress of science and organised inquiry more generally. This was especially true of analytical philosophy of science, though the proponents of this approach were not alone in holding such views.

The sociologist of science Robert K. Merton looked upon science as a particular kind of tradition and explored the relationship between science and ambient cultural and political conditions, but the overall aim was the articulation of a binding ethos that was specific to the scientific tradition (Merton 1968; Panofsky 2010). It is certainly possible to identify episodes in the history of science where a group of scientists transgressed scientific norms, but these stand out precisely because we have a point of reference that specifies what a conscientious scientist ought to have done in similar circumstances.

Such norms, however, did not develop in a vacuum, and Merton was equally sensitive to the conditions that had to be satisfied for science to flourish. Although Merton refrained from making strong claims as to the relationship between science and the democratic social order (Kalleberg 2010: 183), he believed that the scientific ethos could express and realise itself better in democratic societies.

The persistent development of science occurs only in societies of a certain order, subject to a peculiar complex of tacit presuppositions and institutional constraints.
(Merton 1968: 592)

The political apparatus designed to put democratic values into practice may thus vary, but universalistic standards are maintained. To the extent that a society is democratic, it provides scope for the exercise of universalistic criteria in science.
(Merton 1968: 611)

This move and overall framing of the issue is telling from a sociology of knowledge point of view. Stephen Turner (2007a; 2008), David Hollinger (1995) and Ronald Giere (1994) have suggested that the normative approach to science in the middle of the twentieth century should, at least in part, be understood in the context of broader debates pertaining to the cultural significance of science in general and the fate of science in totalitarian societies, such as Nazi Germany and the Soviet Union, in particular. That is to say, Merton's claim that there is a kind of elective affinity between the democratic tradition and the scientific ethos (Kalleberg 2010) has to be viewed in relation to the concerns of Merton and his contemporaries regarding the future of democratic culture in general and the freedom of scientific inquiry in particular.

Bland as most of their formulations might seem from afar, to these intellectuals it mattered enormously to be "objective," to look upon factual realities "without prejudice," to "actually test with experience" one's opinions, and to report "honestly" the results of one's inquiries. These men and women saw a world filled with "prejudice" and with efforts to "impose certain opinions by force." Against these evils one must affirm "free inquiry" and "open-mindedness" in order that our society might be organized realistically on the basis of the conditions life actually presents. If this was what scientists did, then the idea of imitating scientists, of following a "scientific

approach", was a capital idea.

(Hollinger 1995: 444)

Such anxieties also touched upon the imposition of forms of organisation and planning that were alien to the scientific way of life. Unsurprisingly, the fate of science in totalitarian societies was a source of great concern. While liberal audiences had to be persuaded that science works best as a self-regulating tradition animated by a critical ethos (Turner 2007a; 2008), totalitarian societies were more complicated. In totalitarian societies

[t]he norms of the scientific ethos must be sacrificed, in so far as they demand a repudiation of the politically imposed criteria of scientific validity or of scientific worth.

(Merton 1968: 595)

The above quote suggests that Merton believed that unlike democratic societies, where new scientific contributions were evaluated in terms of their logical consistency and the fit between data, hypotheses and theoretical frameworks, totalitarian societies impose 'the hitherto irrelevant criteria of the race or political creed of the theorist' (Merton 1968: 596). What is more, Merton was not alone in this. For example, Ronald Giere (1994: 4-5) has suggested that Hans Reichenbach's distinction between the context of discovery and the context of justification (Reichenbach 1938) was introduced precisely to make the point that the identities of scientists should have no bearing on the validity and evaluation of their scientific contributions.

The concerns over external interference were coupled by the appreciation that scientific expertise possesses rhetorical force which can justify and legitimise political decisions, thus creating a self-sustaining vicious circle.

Partly as a result of scientific advance, therefore, the population at large has become ripe for new mysticisms clothed in apparently scientific jargon. This promotes the success of propaganda generally. The borrowed authority of science becomes a powerful prestige symbol for unscientific doctrines.

(Merton 1968: 602)

By exerting control over the production of knowledge, enforcing conformity and excising the possibility of ideologically troubling or subversive research, political institutions and regimes could rationalise their decisions by relying on the impersonal authority that science possesses. However, such a move hinges on the prestige that scientific knowledge enjoys, which in turn is based on the belief that good science is uncontaminated by the world of politics and, therefore, is rightly considered the canonical example of knowledge. In other words, the predicate "scientific" imparts a

particular kind of authority and power because it is believed to be devoid of ideology (Ezrahi 1990; 2003). Scientific knowledge is a source of power, but it is a power that derives from the belief that science is a self-regulating enterprise whose products are impervious to ambient influences and political interests, which are believed to have a corrosive effect on the integrity of the scientific enterprise (Kalleberg 2010: 194).

Science as Culture

Merton's way of talking about science, however, has been the subject of intense debate ever since, and there has been a gradual move away from his seemingly agnostic but ultimately deferential attitude towards science (Fuller 2006b; Guggenheim and Nowotny 2003). First of all, while Merton approached science as a kind of tradition, it was tradition that reflected, encouraged and cherished the spirit of critical inquiry that had come to be associated with science.

Most institutions demand unqualified faith; but the institution of science makes scepticism a virtue. Every institution involves, in this sense, a sacred area, which is resistant to profane examination in terms of scientific observation and logic. The institution of science itself involves emotional adherence to certain values. But whether it be the sacred sphere of political convictions or religious faith or economic rights, the scientific investigator does not conduct himself in the prescribed uncritical and ritualistic fashion.

(Merton 1968: 602)

In other words, Merton believed that scientists inhabited a culture whose norms were congruent with the popular idea of science as disinterested inquiry. A number of developments in the 1960s – chief among them being Thomas Kuhn's work – redefined the terms of the debate. Kuhn's account of everyday science (or normal science) viewed it as a form of material culture and craft but defined the interests of scientists internally and focused on consensus and routine (Kuhn 1996). Science may well be a self-regulating culture, but the critical spirit evident in Merton's norms was subdued, though not explicitly excised.

[B]ecause Kuhn made it clear that normal science was typical and revolutionary science aberrant, he was widely read as promoting normal science as a new model of rationality, with revolutionary science implicitly standing for irrationality.

(Fuller 2006b: 23)

The removal of the critical element from the science-as-culture approach was very much in evidence in the work of the Strong Programme in the sociology of scientific knowledge. The proponents of the latter, however, went further than Kuhn and made the boundary between science and the broader social context porous and malleable. By

doing so, they attempted to move away from treating external factors as negative or corrosive influences and proposed to give them a constitutive role in the production and development of scientific knowledge and ideas (e.g. Bloor 1991). This can be regarded as a challenge to Reichenbach's claim that the context of discovery and the context of justification had to be distinguished (Reichenbach 1938: 381-382). The compound effect of Reichenbach's views was that a theory of science should (i) aim at a rational reconstruction of knowledge that focuses on the relation between facts and theories and (ii) ignore the identity of the scientist, because her psychological idiosyncrasies and the specificities of the context in which she made her discovery have no bearing on the validity of her contributions to the scientific enterprise. The Strong Programme, however, challenged this view and aimed to show that both the identity of the scientist and the context in which she operated have been pertinent factors in determining the reach and impact of scientific contributions.

Sociology and Science Studies: An Awkward Encounter

This thesis follows in the footsteps of the approach inaugurated by the Strong Programme and is situated at the intersection between theoretical sociology and science studies. The focus of this thesis is the specificity of sociology, the difficulties inherent in understanding this discipline from a science studies perspective, and the various attempts to articulate the goals of sociology as a form of knowledge. The impetus comes from the view that '[d]espite its sensitivity to the social contexts of scientific knowledge production, STS has difficulty applying this awareness reflexively' (Fuller and Collier 2003: xviii), and I want to suggest that the peculiarities of sociology as a science can illustrate this quite well. For example, in their article on the present state of science studies Guggenheim and Nowotny (2003) argue that the field works with an implicit understanding of what deserves the attention of science studies scholars. This is based on a very traditional version of what constitutes science, and the focus is usually on uncontroversial sites of knowledge production. In general, this means that, while conventional accounts have been challenged and the boundary between science and society is figured as porous and open-ended, researchers still look to the canonical fields (e.g. physics) and settings (e.g. laboratories and universities) for insights. More particularly, science is generally assumed to mean natural science.

The word “science” in science studies still seems largely to mean physics, although the

weight is shifting in the direction of the life sciences. This statement does not imply that other fields and subjects are not studied, but that whatever is within the scope of STS is still held against some kind of standard image of science which is definitely not geology or psychology. We claim that science studies in their actual practices carry a hidden assumption of what science is.

(Guggenheim and Nowotny 2003: 237)

While the above remark was made more than a decade ago, it suggests a possible reason why a discipline like sociology has not been a prominent object of study. In particular, Guggenheim and Nowotny believe that science studies has generally focused its attention on disciplines like physics and showed little interest in the social sciences and humanities. They suggest that a contributing factor to this trend is the difference in prestige attached to the sciences themselves and the reputation that comes with studying them (Guggenheim and Nowotny 2003: 238). In other words, studying a well-established and authoritative field like physics attracts more attention and respect. I have little cause to disagree with their diagnosis, but I wish to propose a slightly different reason for why the social sciences in general (with the notable exception of economics) and sociology in particular have been less prominent than the natural sciences. A concisely summarised hint is provided by Steven Shapin.

The homage is paid from the weak to the strong: students in sociology, anthropology, and psychology commonly experience total immersion in “methods” courses, and while chemists learn how to use mass spectrometers and Bunsen burners, they are rarely exposed to courses in “scientific method.” The strongest present-day redoubts of belief in the existence, coherence, and power of the scientific method are found in the departments of human, not of natural, science.

(Shapin 2008a: 435)

Shapin is fully cognisant of the disparities in status and reputation, but the above quote suggests an interesting consequence that I wish to explore in this thesis. Namely, sociology has attracted little attention from science studies scholars precisely because it has manifested a greater interest and uncertainty in what constitutes a science. Furthermore, sociology has (i) a pronounced sensitivity to the way ambient sociopolitical conditions affect knowledge production and (ii) attempted to reflexively apply these insights to itself. Sociology is, therefore, a distinctive discipline that recognises and is open about the influence of context on the production and diffusion of knowledge, not unlike science studies itself.

However, the emphasis on context has been problematised in science studies (Asdal 2012), and a different kind of approach to the production of scientific knowledge has been put forward that focuses on the processes through which science and society

emerge as distinct categories; in other words, context is viewed as the effect, rather than the cause of interactions between science and society. Among the most prominent schools of thought to inaugurate this development is actor-network theory, and it is the work of theorists working in this tradition that serves as a sparring partner of sorts for the arguments put forward in this thesis. The choice to focus on, and work with, actor-network theory is based on a number of different considerations. The most prominent among these is the status of actor-network theory as one of the main structuring positions in the theoretical landscape of science studies and the role of Bruno Latour as a key spokesperson for the field.

This thesis proceeds on the assumption (elaborated upon in Chapter 3) that, while actor-network theory is an internally varied approach to the study of science and technology, it is based on background assumptions that are better suited to the study of the natural sciences and biomedicine. This, as I will argue, is due, in part, to the emphasis on the stability of the social and material realities that the sciences engender, and a normative indifference to the kind of alliances that make such realities possible. My contention is that sociology is, and has been, tricky to study because its weaknesses and heterogeneity are ill-suited to actor-network theory and have the potential of revealing unarticulated background assumptions that shape actor-network accounts of successful science.

During the course of this thesis I also draw upon the work of Pierre Bourdieu and Steve Fuller to produce a constructivist framework that would be sensitive to the idiosyncrasies of sociology. These attempts notwithstanding, a supplementary claim put forward in this thesis is that, while a constructivist perspective can highlight characteristics that hamper the institutional credibility of sociology, some of these characteristics are actually believed to be an integral part of the sociological enterprise and the long-term development of sociology as a form of knowledge, and positively resonate with recent developments in actor-network methodology. This leads us to a further reason for studying sociology.

The problem of reflexivity has a long-standing presence in science studies. The context for, and target of, these discussions has usually been the attempt to study science scientifically and the resulting consequences for a sociological account of science (e.g. Ashmore 1989; Woolgar 1988). My project, however, concerns the peculiar relationship between science studies and cognate disciplines, such as sociology. A secondary aim of

this thesis is to explore what happens when the analytical tools of science studies are turned against a discipline that has played a significant role in its development. Furthermore, even though the problem of reflexivity still applies, I believe that the encounter between constructivist science studies and sociology, and the insights generated from it, are qualitatively different because some of the “weaknesses” of sociology are also present in science studies. In addition, while a description of the methods I employed and the resulting limitations to my study are explored in detail in Chapter 5, the difficulties of approaching sociology from a constructivist perspective permeate the entire thesis and are an integral part of Chapters 3 and 4.

The Strange Case of Soviet and Latvian Sociology

Two rather unusual examples serve as illustrations of the issues I will discuss in this thesis. While both seem to inhabit a similar historical space, they each serve their own purpose and highlight different aspects of the overall argument.

The thesis began as an attempt to explore the heritage of Soviet science policy and attitudes towards sociology in present day Latvia. As the focus of the research shifted, so did the significance of the Soviet variant of sociology. The discussion in Chapter 3 is necessarily limited and expository by design, and its main purpose is to explore the fate of sociology as staged in Euro-American publications. My approach is inspired by the work of Shlapentokh, Shiraev and Carroll (2008) who analysed how the perception of the Soviet Union in Euro-American scholarship changed and evolved from the Russian Revolution of 1917 until the dissolution of the USSR. My analysis, however, focuses only on sociology. The reasons for studying Soviet sociology derive from the reactions it provoked among people working outside the Soviet Union. In particular, I will argue that outside commentary allows for a definition-by-implication as to what the authors believed constituted good sociology, and the kind of relationship with government institutions that this implied. In other words, diagnoses of Soviet sociology, its ailments and strengths are equally revealing of the authors' beliefs regarding the nature and goals of sociology as a discipline. These are addressed in Chapters 3 and 4, and set the stage for my main case study – sociology in Latvia.

Sociology in Latvia has a number of features that make it an interesting case, and the reasons for choosing to study it are outlined at length in Chapter 6. The main reason for choosing sociology in Latvia is that its emphasis on applied research makes it suitable for

a contrast between the respective theories of Bruno Latour and Pierre Bourdieu, and the interplay between autonomy and heteronomy more generally. The bulk of my analysis is based on interviews with sociologists working in Latvia¹. In particular, I explore their understandings of sociology as a discipline and the nature and import of the knowledge it provides. This is discussed in Chapters 6 and 7. The purpose of this case study is to examine the arguments and background assumptions of sociologists working in Latvia from a constructivist point of view and illustrate the differences and points of friction between divergent understandings of science, as well as similarities in spirit concerning the normative embeddedness of knowledge.

Structure of the Thesis

The thesis is divided into eight chapters. In **Chapter 1** I look at early attempts by social scientists to study Western science empirically and the philosophical issues that these attempts gave rise to. In particular, I focus on the rise of the Strong Programme in the sociology of scientific knowledge and its internal splintering that lead to a gradual shift away from a purely sociological explanation of science.

In **Chapter 2** I look at an attempt to resolve some of the issues I raised in Chapter 1 and focus on different forms of what Sergio Sismondo has called heterogeneous constructivism. My main example is actor-network theory, which is supplemented by the work of Isabelle Stengers and various contemporary elaborations on actor-network theory (performativity of method and ontological politics). In this chapter I argue that heterogeneous constructivism (i) allows for intricate narratives that illustrate the epistemic and ontological contingency of the worlds that the sciences produce and (ii) provides a diverse repertoire of methodological approaches to the study of science and the practices that constitute it. However, it lacks the vocabulary and grounds for a critique of the corrosion and misappropriation of science by commercial and political interests.

In **Chapter 3** I look at an example of science that scholars working in a Euro-American context find problematic – sociology in the Soviet Union. My point of departure is Loren Graham's work on the history of science in the Soviet Union. Specifically, I consider the claims that science is (i) not immune to forms of epistemic stress and (ii) can and does collapse under conditions of excessive external interference. I try to show that in the case

¹ The interviews were conducted in Latvian.

of Soviet sociology such interference did not have a straightforwardly crippling effect. Rather, it was conducive to the emergence of a particular brand of sociology that observers found problematic. I also argue that reconstructions of Soviet sociology betray epistemic commitments that hinge on highly specific interpretations of the function and political stance of sociology, rather than purely instrumental concerns. I continue by suggesting that the specificities of Soviet sociology that commentators found problematic rest on assumptions about the intellectual conditions in which science should operate and the relationship between science and politics that this implies. Such assumptions, however, are ill at ease with the entanglements between science and politics as envisaged by constructivist science studies. I suggest that this may be because the specificity of the social sciences has been given limited attention.

In **Chapter 4** I address the specificity of sociology from a constructivist perspective by contrasting the respective approaches of Bruno Latour and Pierre Bourdieu. I complement their take on sociology with the ideas of Steve Fuller and work in the history of sociology. The purpose of this chapter is to (i) outline the theoretical framework that will frame my data analysis and (ii) suggest a number of complications that can affect science studies' accounts of sociology.

Chapter 5 serves as an introduction to the main case study. In this chapter I provide a historical overview of sociology in Latvia and a general outline of the contemporary context in which my informants studied and worked. The second part of this chapter is devoted to a description of the methods I used when studying sociology in Latvia, as well as the background assumptions that informed my approach and caused difficulties in my conversations with sociologists working in Latvia; I also mention a number of limitations that resulted from this. While the section on methodology is located at the end of Chapter 5, it complements and draws upon the arguments put forward in Chapters 3 and 4. That is to say, there are methodological nuances that were specific to my case study, but some of the complications derive from more general issues concerning the relationship between sociology and science studies already outlined in the previous chapters.

Chapters 6 and 7 are distinct yet complementary. The basis of the discussion in Chapter 6 are the answers to the first section of my interview guide, which mainly dealt with the nature of sociology as a science (and whether it is one) and the specificity of the

experience and expertise possessed by sociologists (as opposed to the lay public). In this chapter I illustrate (i) the perceived complexities that are involved in positioning sociology as a science and (ii) the various argumentative strategies employed by sociologists to articulate their position. Chapter 7 deals with the public and political life of sociological knowledge and seeks to ascertain my respondents' views vis-à-vis the political potential of sociological knowledge and the mediums through which it can best effect change. The responses are interpreted in light of the theoretical insights gained in the previous chapters and discussed in relation to the arguments pertaining to the disciplinary specificity of sociology.

Finally, in **Chapter 8** I argue that my conversations with sociologists working in Latvia and analysis of the sociological literature illustrate similarities with science studies attempts to reframe our understanding of science as a matter of both studying and being in the world. I suggest that the specificity of sociology is that, in its own way, the approach to knowledge advocated by contemporary science studies has been an integral part of it, albeit in a highly dynamic and volatile form. What is more, the peculiarity of sociology and its disciplinary and institutional frailty serves as an illustration of what happens when the intimate connection between political representation and scientific representation is not concealed, and hybridity is acknowledged.

1. Studying Science, Part I: Breaking the Spell

The last few hundred years have seen a marked increase in the power, influence, respect and resources (both financial and cognitive) that science commands. From a sprawling enterprise practised by a few geographically disparate individuals it has gradually developed into a well-funded, highly organised and institutionally dominant form of inquiry. One could argue that its authority has recently been partially eroded (Collins and Evans 2002; 2007; Bijker et al. 2009; Lyotard 1984), but it is clear that science still retains a dominant role in Western intellectual ecosystems (Shapin 2008a). However, as much work in science studies has illustrated, there is considerable evidence to suggest that scientific knowledge does not only improve and expand our ability to interact with and mould our environments. It has functioned as a source of political authority (Ezrahi 1990; 2003) and provided state institutions with a vast repertoire of disciplinary, regulatory and predictive techniques (Desrosieres 1998; 2003; Foucault 1991; 1998; Wagner 2003b). That is to say, scientific knowledge has actively participated in political matters by validating and expanding the range of instruments that are deployed for the purposes of statecraft. The exact nature of the relationship between science and ambient sociopolitical conditions and values is less clear. It should come as no surprise, therefore, that various intersecting strands of thought and research have attempted to address this issue (Turner 2008). However, this has proved to be complicated to do largely because of the status of science as the standard of rationality against which all other epistemic traditions should be measured.

Early 20th century philosophers of science assumed that there was something unique about science as a form of knowledge and took it upon themselves to articulate the principles that govern it. The distinguished theorist Karl Mannheim explored the sociological dimension of knowledge, yet even he did not think that this should be done for natural science and mathematics (Mannheim 1952). Gradually, however, these views came to be challenged. The work of Thomas Kuhn, the sociology of scientific knowledge, feminism and science studies more generally raised the possibility that our understanding of science, and the way it operates and develops, may be mistaken. These authors hinted (often in no uncertain terms) that science and extra-scientific interests may be intertwined in ways that are more subtle and complex than we previously imagined.

They raised the possibility that (i) science is not a self-regulating enterprise and (ii) the content of scientific knowledge may be shaped by ambient sociopolitical conditions and non-evidential community factors. In short, it was suggested that science operates and develops according to a logic that exceeds and even contravenes its public image. In this chapter I will explore the approach put forward by the Strong Programme in the sociology of scientific knowledge and highlight a number of difficulties and objections that were raised once it was proposed.

SSK and the Imperialism of the Social

Collin and Budtz Pedersen (2013) argue that science studies grew out of a broader desire in Western countries to reach a better accommodation between science and society (Turner 2008). One of its main academic expressions was the Strong Programme in the sociology of scientific knowledge². SSK sought to study science as one would any other social institution (Barnes 1974; 1977; Bloor 1974; 1991; 1996; Barnes and Bloor 1982). The argument was that the status of science as the canonical example of knowledge had previously interfered with the ability of humanities scholars and social scientists to approach it dispassionately. Philosophy of science had attempted to provide rational reconstructions of science that straddled the line between normative theories of science or descriptive illustrations of what the particular author believed be an example of good scientific practice. Sociology of science, on the other hand, had limited itself to the organisational dimensions of science and exempted cognitive norms from sociological explanations (e.g. Merton 1968; Panofsky 2010; Turner 2008).

SSK scholars went further and challenged the claim that science was a distinctive and purely cognitive endeavour. They argued that it was an inherently social activity whose nature and internal logic could be uncovered through rigorous empirical study. The findings, they suggested, shed doubts on the privileged epistemic status that science enjoyed and, by extension, questioned the existence of a fundamental qualitative difference between science and other knowledge systems, and even other forms of social activity that humans have engaged in. Science, in other words, was a form of culture just like any other and not immune to external influences or impervious to extant belief structures. It had its own ontological commitments, habitual schemes of interpretation, complex rites of validation (e.g. peer review) and even forms of social organisation that

² Hereafter – SSK

distributed authority among its members (e.g. the hierarchy in a laboratory). Concurrently, why should it not be studied in a similar way to other cultural practices? Indeed, David Bloor (1991) was adamant that this is the way one should proceed.

In his book *Knowledge and Social Imagery* Bloor argued that there was something peculiar about the fact that knowledge (especially scientific knowledge) had not been subjected to sociological analyses. Sociology had certainly studied bundles of practices that may have looked like science, but upon closer inspection it became clear that sociology had been content to explain scientific misadventures. This, of course, was no accident, as the sociological approach to science worked with an implicit arationality assumption. That is to say, sociologists only commented upon the content of scientific theories if it could not be explained in terms of its rational merits (Laudan 1978: 202; Merton 1968: 516). Consequently, the content of institutionalised and widely disseminated forms of scientific knowledge had escaped sociological scrutiny. What we had instead was a sociology of error – only distortion and epistemic inadequacy required the skills of the social scientist. The reasons for the success of good science were presumed to be internal to it and provided us with a point of reference when explaining the misadventures of scientists whose work we did not see fit to include in our epistemic canon. Success did not require any kind of explanation that made it intelligible in terms that are irrelevant, or even alien, to the rational discourse of science. This, according to Bloor (1991) and Barnes (1974), was unacceptable – widely accepted forms of scientific knowledge could also be analysed sociologically. Indeed, SSK was opposed to the belief that good science required no explanation. In fact, claims to the contrary should be treated as a pernicious form of mysticism that is adverse to the spirit of science. Furthermore, SSK scholars did not believe that science was flawed – far from it. What they tried to suggest was that their framework simply allowed for a more thorough and, most importantly, empirical understanding of the way science operated. Consequently, it was not so much the credibility of science that SSK sought to undermine. Rather, it was the image of science perpetuated in popular and academic (e.g. philosophy of science) discourse that required correction (Collins and Pinch 1993; Labinger and Collins 2001). This is where the symmetry thesis comes in.

The symmetry thesis states that the same principles should be employed, and the same causal mechanisms invoked, when explaining both successful theories and misadventures.

In other words, scholars should be impartial with regard to truth and falsity when explaining the success or failure of a particular scientific line of thought. At the time it was proposed, the symmetry thesis attracted a great deal of academic flak, especially from philosophers of science (Collin 2011; Fuller 2002; Laudan 1981; Kim 1994). A cynical reading of the dispute would suggest that philosophers were simply indignant about another discipline invading its turf (Abbott 2001a). However, that would miss the point, as the conflict was over the nature of science. A good illustration of this is the exchange between David Bloor (1981) and Larry Laudan (1981; 1982b).

Philosophy of Science vs. SSK

The philosopher Larry Laudan (1981) argued that the SSK position, while insightful, had a number of flaws. Firstly, it caricatured philosophy of science. Laudan argued that no philosopher would seriously entertain the possibility that the gradual acceptance of beliefs deemed rational and true required no explanation. However, such an explanation did not necessarily have to be sociological in nature. This, I believe, was one of the first tripping points in the debate. Laudan seemed to be implying that SSK was implicitly advocating a sociological monopoly over, or at least the analytical primacy of sociology in, the explanation of scientific practices. Bloor (1981), on the other hand, argued that a sociological account of science was just one among many different ways of studying science. Laudan's uneasiness does not strike me as unfounded, however – especially in the case of David Bloor (see also Collins 1981b; 1982). Bloor's work (1974; 1998) posits society as the vehicle for scientific ideas, and sociology (or anthropology) would seem to be the discipline of choice for explanations vis-à-vis the social realm. Thus, there is something slightly disingenuous about the way SSK is practised and the views its practitioners profess when addressing objections raised by outsiders (Collin 2011; Fuller 2000b; see also Collins 1981a; 1981b and the response by Laudan 1982a).

The second point of conflict follows on nicely from the first one – Bloor and Laudan did not agree on which aspects of scientific practice could be the subject of a sociological explanation. A good example of this is the problem of theory choice in a situation where many theories are equally supported by the data (i.e. the underdetermination thesis). This was a topic of considerable debate in philosophy of science, and one of the main objections raised against it was that the proponents of the underdetermination thesis exaggerated how often such a situation actually arose in science (e.g. Hacking 1983).

However, even if we accept that the problem of underdetermination does not present itself often, the way such hypothetical disputes can be and are resolved is still a valid question. Laudan contended that, while it was true that scientists are socialised into a certain culture, (a) there are good reasons for choosing one theory over another and (b) these are not necessarily social in character. He used the example of simplicity, but he would probably concede that there are others like accuracy, consistency, scope and fruitfulness (Kuhn 1977; Longino 1983; 1990).

Bloor's reply was essentially twofold. Firstly, he disputed whether rigid adherence to such criteria as simplicity had always been prudent. Sometimes the more complicated theory is more accurate and has greater scope. Secondly, the criteria guiding theory choice may be an instance of social factors that are internal to science. The choice in favour of the least intricate of the available alternatives may well be done rationally (i.e. subject to respectful negotiations, reasoned argument and careful interpretation of the available data), but this is a thoroughly social form of rationality amenable to sociological explanation. This leads us nicely to the most fundamental difference between David Bloor (the sociologist) and Larry Laudan (the philosopher).

In his critique of SSK Laudan suggested that the symmetry thesis can be read in three ways – as a thesis of (i) epistemic, (ii) rational and (iii) pragmatic symmetry (1981: 186-194). Because of his commitment to the radical inaccessibility of truth (i.e. we know that some theories are false, but we do not know if our current best theories are true) Laudan was quite comfortable with epistemic symmetry. That is to say, we can easily be impartial when analysing, for example, a conflict between two theories (one of which turned out to be false) and refrain from invoking the veracity and ultimate acceptance of the victor. The protagonists had no knowledge of this, much like we have no knowledge of which of our best theories will eventually have to be discarded, so it would be unfair to judge them from our privileged vantage point. Laudan was much more concerned with rational symmetry and, by extension, suspicious of the judgemental relativism implicit in SSK (e.g. Laudan 1981; 1982a) He contended that evaluative appraisals of a belief are relevant to its explanation. An agent reasons by combining her background knowledge, sensory information and ultimate goals. In relation to this matrix (however specific) there are reasons for adopting some beliefs and dropping others. To put it simply, some beliefs are the result of a process of reflection (with all the inferential mechanisms this entails) and

others are not. Our attempts to explain beliefs should reflect this difference – sometimes the reasoning involved and the reasons offered are simply insufficient to ground beliefs or warrant conclusions. Granted, we may occasionally be deluded into giving reasons for our actions or beliefs, which are, in fact, not the “real” reasons (e.g. false consciousness), but we must presuppose or allow for a distinction between beliefs which are held rationally and beliefs which are not. What is more, it should not be assumed that rationality is something non-social. On the contrary, rational beliefs can have social origins, but SSK should allow for a difference between social belief-governing policies which are rational and those that are not. It is at this point that the starkest difference between SSK and philosophy of science reveals itself.

David Bloor, much like Laudan, believes in rationality. In other words, Bloor believes in reasoned, rule-governed and contextually sensitive judgement. However, the rationality that Bloor posits is more akin to a concession that humans can and do reason, and are thus capable of constructing arguments and responding to sensory cues in appropriate ways. Bloor is perfectly happy to accept that humans have innate cognitive competences and are capable of reflection and complex inferential thinking. They are a clever bunch that possesses a “natural” rationality (Barnes 1976). However, in the case of science and all other forms of collective activity with a pronounced epistemic dimension, natural rationality is supplemented by a normative form of rationality (Barnes 1976; Bloor 1981; Barnes and Bloor 1982) that evaluates reasons, evidence and beliefs in terms of how well they meet the criteria of a given system. In other words, they are judged by collective standards of rationality. This is something the philosopher refuses to accept, for it implies that alien epistemic traditions might be perfectly coherent (allowing for some degree of internal inconsistency characteristic of all belief systems [Barnes 1974]), yet differ considerably from Western standards of empirical adequacy and scientific rigour (Winch 1964; 2008). In such a scenario accepting (i.e. deeming rational) the standards and rules of inference adhered to in one society, and disparaging and refusing to acknowledge those of another, is simply a form of endorsement (Bloor 1981: 209; Feyerabend 1987). There are no good reasons outside of a tradition. Consequently, SSK is committed to a form of epistemic relativism that allows for the possibility of internal coherence, yet questions the possibility of meaningful (both to the outsider and insider) objections and contextually invariant standards of rationality. This point has proven rather hard to swallow for philosophers (Barnes and Bloor 1982; Kim 1994) but has acquired

considerable currency in science studies more generally. The situation is rather different in the case of pragmatic symmetry.

Laudan (1981: 193) argued that SSK theorists were committed to the belief that the success (or failure) of a theory at predicting and explaining the world was an irrelevant factor when explaining its success (or failure) in being taken up in the wider scientific community. Simply put, pragmatic considerations should not enter into our narratives. Now, I am unaware of any passage where SSK scholars state this explicitly, but their emphasis on social factors (e.g. interests, academic rank, institutional affiliation) renders their work uniquely susceptible to this sort of accusation. In fact, the move towards explaining scientific success in terms of political, disciplinary and economic interests was treated with considerable unease. For example, as Fuller (2002) and Kim (1994: 400-401) point out, invoking the causal role of social factors is analytically insufficient to account for why a particular theoretical move or innovation was accepted by contemporaries. Yes, it may well be true that some theories will have considerably more appeal in specific social contexts, or that there are disturbing homologies between political and scientific world views. What is more, this may have an impact on the way scientific debates proceed. But this in itself is not enough to explain why and how a particular scientific community reaches consensus over a controversial issue. The mechanisms through which disputes are settled and consensus achieved are given a one-sided (though intricate e.g. Collins 1975; 1981a) treatment by SSK scholars, and leave the reader slightly perplexed. It almost seems like you have to assume that the presence of particular social factors determines the nature of the debate, and that it is only these social factors that influence the decisions that participants make. The standards of reasoning characteristic of the scientific community, and how SSK case studies make sense in relation to them, are not important. Consequently, one could be forgiven for imputing to SSK accounts a kind of mysticism – not unlike the one featured in their critique of philosophy of science. You get the impression that the material world and the extant practices and belief structure are weak in their ability to alter the course of a debate and resist the mobilising power of social interests.

Indeed, one of the core principles underlying much SSK work is an open-textured approach to scientific concepts – finitism (Bloor 1996; 1997). It is based on Ludwig Wittgenstein's arguments pertaining to the application of rules (Wittgenstein 2001) and

basically boils down to the claim that prior applications of a rule do not uniquely determine future applications. This suggests that future applications of rules are open-ended, rather than determined in advance. SSK theorists generalise this argument to scientific practices and argue that the extension of meanings (of scientific terms, classes) and rules of inference to new instances is not determined in advance. Furthermore, there are no logical constraints that would prevent scientists from systematically redefining the meanings they attach to classes of entities and phenomena, or the rules that govern how this happens. Prior instances do not prescribe a definite course of action. This is where the social comes in. If philosophers of science emphasised the logical compulsion inherent in deductive rules of inference, SSK theorists argue that it is non-evidential community factors that facilitate consensus and stimulate the feeling of compulsion.

There are at least two problems with such a rendering of Wittgenstein's argument. Firstly, it is radically indeterministic. Both Tyfield (2008) and Mermin (1998) argue that, even if we accept the general thesis that past instances do not prescribe a definite course of action vis-à-vis future instances, they most certainly limit the available options. Future instances are, at best, underdetermined, rather than indeterminate. An integral aspect to Wittgenstein's argument is that the relevant community agrees of how a rule is to be applied³. David Bloor suggests that there can be no appeal to a logical connection between conventional applications of a rule and the new one, but it does not follow that the new interpretation can get away without making sense in relation to previous applications. Now, SSK theorists might accede, but then go on to say that it is social factors that determine whether or not the interpretation is consistent with previous applications.

This brings us to the second point – SSK theorists have a reified concept of the social. The social (in the form of interests, norms and beliefs) is doing an awful lot of work for them and is fairly stable in its ability to guide scientific activity. Extant beliefs, standards, and the ways in which the material world resists our attempts to describe it and interact with it, on the other hand, seem extremely pliable. SSK theorists have been emphatic in arguing that they are not idealists (e.g. Bloor 1996), but it is not clear what role, if any, the material world plays in their narratives describing scientific practice (Collin 2011). Indeed, it was gradually recognised that much SSK work was merely the inversion of

³ It should be noted that there is considerable debate as to whether this agreement can be explained sociologically (see Pickering 1992 and Collin 2011).

technological determinism and scientific exceptionalism in favour of social determinism (Feyerabend 2010; Haraway 1991; 1997; Latour 1993b; 2004a; 2005). In other words, it was argued that explaining the success of particular scientific theories by placing explanatory stress on their social acceptability, ideological pandering and the interests of individual scientists (or even entire disciplines) simply reified the social and imbued it with the power and stability previously attributed to the scientific method and its inherent superiority over other knowledge systems. SSK was accused of constructing narratives that muffled the material world and its ability to resist whatever descriptions politically-minded scientists conjured up in an attempt to further their agenda. The call was for a genuinely symmetrical approach that recognised the need to integrate and mesh the causal and agential powers of the social with those of the realm represented by scientists and engineers (Haraway 1988; 1991; 1997; Latour 1981; 1991; 1993b). The kernel of the debate can be illustrated by looking at two books that have become classics in the field of science studies.

The Two Realms

The people we now call “philosophers” are basically the natural philosophers on the losing side of the battles that we now call “scientific”.

(Fuller 2003:74)

In their study of the emergence and development of experimental practices, and the birth of The Royal Society science studies scholars Steven Shapin and Simon Schaffer explore the conflict between Thomas Hobbes and Robert Boyle (Shapin 1984; Shapin and Schaffer 1985). The authors draw our attention to something peculiar about the way we perceive these two men. Both Thomas Hobbes and Robert Boyle held strong views on both political and epistemic matters. They both had a political philosophy and a philosophy of science. However, the prevailing wisdom in academic circles seems to be that Thomas Hobbes was one of the most exceptional political philosophers in the Western tradition, whereas Robert Boyle plays an important role in narratives about the rise and development of European science. Hobbes' views on science and Boyle's views on politics are somehow left out of the picture. How so?

To answer this questions Shapin and Schaffer look at the intimate relationship between the nascent form of experimental science and politics, which was an integral part of the disagreements between Hobbes and Boyle. The debate was animated by questions pertaining to the legitimacy of different ways of practising science, but it was equally

concerned with the management of disagreement and the delegation of authority as to the resolution of such disagreement. According to Shapin (1994), gentlemanly culture provided a managed space of the requisite sort for the resolution of disputes regarding knowledge claims (1994: ch. 3). There were a number of rules that regulated how one was to conduct himself in the presence of others. This allowed for 'dissent without disaster' (1994: 103) - a peaceful resolution of conflicts over contradictory knowledge claims. This involved a complex cultural repertoire of strategies that allowed the group to manage and modify knowledge claims without explicitly negating the claims of any particular individual. The Royal Society was an example of this. The practices that were instituted to deal with discrepant knowledge claims, and scientific culture as a whole, were the result of relocating gentlemanly culture to a philosophical setting. Scientific knowledge and practices were part of a particular culture, which was inhabited and reproduced by a certain type of person. Indeed, knowledge was part of a moral order.

An important consequence of the dispute between Hobbes and Boyle was the creation of two distinct realms (i.e. society and nature) over which their respective rulers had the authority to speak. The nature of the exchanges between Hobbes and Boyle, however, preceded this. This is why Shapin and Schaffer paint a picture of science as inextricably intertwined with questions of power and political organisation. When Hobbes and Boyle were exchanging views, throwing insults and attracting followers, there was no clear separation between the two realms.

Shapin and Schaffer suggest that discussions pertaining to the validity of experimental practices and the attendant social rituals were shot through with concerns regarding the organisation of scientific practice. However, these disputes also addressed the issue of who should have the final say on political matters and on what grounds such power should be conferred. Political issues were epistemological issues and vice versa – they were not treated separately. According to Shapin and Schaffer, therefore, experimental practices were the site of intense discussions and figurations whose central objects were both epistemology and the social order. It is only in the aftermath of the conflict between Hobbes and Boyle that the cleavage between science (knowledge) and politics (power) came into being.

The Revolution that Never Was

In his book *The Scientific Revolution* Steven Shapin makes a striking claim. He suggests that

what we refer to as the Scientific Revolution never actually happened (Shapin 1998). He, of course, does not mean to suggest that the work and ideas of Isaac Newton, Galileo Galilei or Nicolaus Copernicus are historical fictions, but he does draw our attention to the possibility that we tend to overestimate how many of their contemporaries were familiar with the work done by these scientific revolutionaries. He also suggests that the revolutionaries were not engaged in a cohesive intellectual project and were divided on many issues. In other words, Shapin suggests that the Scientific Revolution is an artefact of historical scholarship. There was never a revolution. It is simply a way of relating to our intellectual heritage.

In a somewhat ironic move Bruno Latour (1991; 1993b) does something similar to Shapin's own work. While he applauds and largely concurs with the analysis of Shapin and Schaffer (1985), Latour suggests a different take on the same problem. Shapin and Schaffer seemed to be advocating the view that Hobbes' philosophy of science, which Shapin and Schaffer believe to be constructivist in nature, was actually the more accurate of the two because it highlighted the artificiality of knowledge. Remember, however, that a common criticism of SSK (in which I presume to include Shapin and Schaffer) was that it paid scant attention to the materiality of the world and its role in the process of constructing scientific knowledge. It is exactly along these lines that Latour (1993b) suggests that the balance needs to be redressed, and the insights of Boyle should also be taken into account.

After having had the stroke of genius that led them to compare the experimental practice and political organization of two major figures from the very beginning of the modern era, they [Shapin and Schaffer] back off and hesitate to treat Hobbes and his politics in the same way as they had treated Boyle and his science. Strangely enough, they seem to adhere more steadfastly to the political repertoire than to the scientific one.

(Latour 1993b: 25)

What emerges from Latour's analysis is the realisation that science/nature and politics/society co-constitute each other. Shapin and Schaffer seemed to be implying that a kind of deal had been struck between politicians and scientists. It was agreed that neither of them shall venture into the other's realm. Scientists would be permitted to speak about nature (and non-humans), whereas politicians would lay claim to society (and humans). However, does nature not interfere with the workings of society when an epidemic breaks out? Do politicians then not require the work of scientists and the elements of reality for which they speak? What is more, are scientists themselves not

engaged in politics when they claim to represent parts of nature in a bid to persuade their colleagues? And does not society and culture come into play when scientists employ socially sanctioned instruments and procedures to explore the realm of nature? In other words, Latour is suggesting that the cleavage between science (and nature) and politics (and society), and the distribution of agency among humans and non-humans, is highly problematic. These are not fixed categories but exist in a perpetual state of hybridity, yet they are always subject to intense boundary work that seeks to distinguish the normative and the descriptive, the scientific and the extra-scientific.

The first set of practices, by 'translation', creates mixtures between entirely new types of beings, hybrids of nature and culture. The second, by 'purification', creates two entirely that of human beings on the one hand; that of non-humans on the other.

(Latour 1993b: 10-11)

The distinction between society/politics on the one hand and nature/science on the other is fundamental to our understanding, but it is rather dubious. Even though there are persistent attempts to disentangle them, both sides are meshed together.

The Volatile Waters of Science and Policy

In the last two decades this form of reasoning has developed a more definite vocabulary and become the co-production idiom (Jasanoff 2004). The claim that changes in knowledge (and the practices of its production and dissemination) have attendant social consequences has acquired significant currency in the field of science studies and inspired much work that seeks to elucidate the ways in which science, extra-scientific interests (e.g. politics) and social formations co-constitute each other. In other words, the way we know the world (and the kind of knowledge this produces) both reflects (society > science) and shapes (society < science) the way we live and organise ourselves. Neither side is predefined, but evolves in the process. There are, of course, many versions of this approach, but Jasanoff (2004) herself notes that most attempts have failed since they still give explanatory primacy to one or the other. It is, apparently, exceedingly complicated to discuss socioepistemic configurations in a way that considers both sides in conjunction with each other, rather than introducing some form of analytical hierarchy that privileges one over the other.

A good example is the discussion initiated by a paper that Harry Collins and Robert Evans published in *Social Studies of Science*, which has since become the journal's most downloaded article. In it they criticised what they called the second wave of science

studies for its inability to provide robust criteria and guidelines for science-policy interactions (Collins and Evans 2002; 2007). They argued that science studies thus far have mainly been descriptive, and the plethora of case studies challenging the perception of science as qualitatively different from other forms of knowledge has only served to undermine its credibility. What is more, by extending the circle of those who can and should contribute to the settlement of technical disputes, some scholars (e.g. Brian Wynne) have blurred the boundaries between qualitatively different forms of expertise.

The debate highlighted a number of important differences between Collins and Evans, and their critics. Collins and Evans sought to disentangle politics and expertise so as to limit the number of people whose knowledge is considered pertinent to the issues at hand. Their focus was on decision-making. Sheila Jasanoff (2003; 2005) and Brian Wynne (2003), on the other hand, emphasised the entanglements of politics and expertise (Durant 2008). The latter highlighted the sensitivity of scientific expertise to the settings in which it is embedded (be they cultural or institutional) and the identities of the individuals operating in these settings. They were more interested in how the issues were framed and the resulting exclusion of knowledges and interest groups from the process of deliberation. Jasanoff and Wynne are not dismissing science but simply pointing to the fact that social and institutional factors are integral to an accurate description of how expertise operates. In addition, Brian Wynne and Alan Irwin (1995) argue that public and science studies criticisms of policy initiatives are often misunderstood because policy makers operate with a number of assumptions of what the public is like. The criticisms are seldom directed at science itself (though they can be in cases where uncertainty is intentionally concealed and/or disregarded). Quite often the reason is (i) the blatant disregard for the interests of the people affected and (ii) the unwillingness to engage with stakeholders and take their concerns and suggestions into account.

Questions of this nature have inspired a wide range of books and articles dealing with science-policy interactions and the resulting co-production of both the social and scientific order (e.g. Bijker et al. 2009; Callon et al. 2009; Desrosieres 1998; 2003; Ezrahi 1990; Jasanoff 1990; 2005; Waterton and Wynne 2004; Wynne 2011). What is quite intriguing, however, is that scholars working in the field of science studies seem quite comfortable with the task of managing the interactions between publics, policy makers and scientists (Webster 2007). Alan Irwin (2008), for example, has argued that science

studies is in a privileged position to analyse scientific governance; similar sentiments have been expressed by Wiebe Bijker (2003). Helga Nowotny (2007) has even gone so far as to argue that '[w]orking inside STS presupposes some kind of multilingual competence, an ability to communicate with very different actors and engage with different kinds of communities' (2007: 487). What is never questioned, though, is the commitment to science as the epistemic tradition of choice. This, of course, may seem like a peculiar criticism to make, but the insights provided by science studies have given us considerable reason to doubt the status of science as the fully justified canonical example of knowledge, its superiority vis-à-vis other knowledge systems and even the fact that science is something discrete (Fuller 2000b; Guggenheim and Nowotny 2003). As Dick Pels once put it,

[t]o Feyerabend's notorious question "what is so special about science?" the modern social studies of science univocally reply: little or nothing.

(Pels 1995:79)

There is certainly confusion as to how one should relate to the results of the numerous case studies (Fuller 2006b), but this has not been followed by an interrogation or re-evaluation of our attachment to science (with a few notable exceptions – see Chapter 2).

Why Science, though?

Steve Fuller (1987; 2002; 2003) has suggested that philosophy of science can be regarded as an attempt to apply political philosophy to science. He contends that Thomas Kuhn, who was one of the most important precursors to SSK⁴, was in fact an authoritarian when it came to science. How so? Well, in retrospect such an accusation does not seem all that peculiar. Kuhn (1996) is (in)famous for bringing the word paradigm into the academic mainstream. Kuhn's use of this term is varied and often difficult to pin down (Masterman 1965). It can range from the fairly narrow exemplar to something as broad as a research tradition and theoretical framework, and encompass everything from ontological commitments to conflicting preferences vis-à-vis scientific values (e.g. explanatory scope over precision and vice versa) (Matheson 2008). It is clear, however, that the underlying theme of his work concerns the conditions that make concerted scientific work possible. Paradigms are what bind researchers together and allow them to practice science whilst expending very little (if any) energy on disputes concerning the

⁴ It should be noted that Kuhn distanced himself from SSK interpretations of his work (e.g. Kuhn 1977).

theoretical and experimental apparatus at their disposal. This seems to require a modicum of consensus, which, while not violently enforced, discourages, though it does not preclude, radical innovation. Normal science is to be practised within the parameters of the one paradigm, and replaced only by way of revolution.

Karl Popper, Fuller argues, is a traditional liberal. This again seems somewhat puzzling, as the rigidity of the falsification principle (Popper 2002) seems to suggest an authoritarian approach to knowledge production. Not so, argues Fuller. Popper's suggestion that science develops by way of conjectures and refutations allows for the possibility of radical critique within a particular research tradition. In fact, such critique is to be encouraged. Fuller's invocation of Popper's open society is instructive at this point. Kuhn's paradigms do not foster critique and can be replaced after a revolution has taken place. Popper's open society, on the other hand, encourages critique and allows for a change of government that takes place non-violently (i.e. a theory is refuted by a bold conjecture). In other words, change can be effected through the available channels and by means of keeping our beliefs open to critique. No radical break or overthrow is required. Popper's criticism (Popper 1970) of Kuhnian normal science illustrates this quite well. In addition to questioning whether Kuhn's thesis regarding paradigms is an accurate representation of science, he laments Kuhn's commitment to dogmatism (however innocuous). Consensus is crucial for Kuhn. It is what enables science to function efficiently and not waste time on debates concerning its fundamentals. For example, if Fuller is correct in arguing that sociology is actively self-deconstructive (Fuller 1991), it can serve as a good illustration of what would constitute something akin to a nightmare for Kuhn, because a significant portion of work is devoted to debates as to the nature of sociology, rather than actual research. Popper, while accepting that a certain degree of dogmatism is necessary to prevent science from disintegrating, favours a critical stance towards the status quo.

Finally we come to Paul Feyerabend and Imre Lakatos, whom Steve Fuller labels an anarchist and social democrat respectively.

Popper's self-styled "open society" vision of the scientific community marks him as a classical liberal, while Feyerabend's emphasis on the "open" and Lakatos' on the "society" aspects of the Popperian vision marks them as, respectively, an anarchist (or libertarian) and a social democrat.

Fuller (2002: 6)

Fuller's characterisation of Feyerabend is less surprising, given that Feyerabend's

theoretical position is known as epistemological anarchism (Feyerabend 2010). However, as Fuller suggests, Feyerabend's connection to Popper should not be overlooked. Popper argued for the necessity of critique and an active and conscious reworking of the tradition of which you were part. One should continually make bold conjectures and incessantly test one's existing beliefs. However, Popper was committed to one tradition (i.e. science). Feyerabend generalises Popper's critical stance and drops his commitment to science. For Feyerabend science is simply one tradition among many and should not receive preferential treatment at the cost of other forms of inquiry (Feyerabend 1978). Various alternatives should be nurtured, and this goes beyond competing scientific theories. Alternatives to science itself should be explored, developed and allowed to compete with science and one another. However, I would urge the reader not to construe Feyerabend's arguments as an attack on science per se. Science is certainly the target of his literary flair, but I would suggest that it is not so much science-as-a-tradition that Feyerabend finds distasteful. Rather, it is the monopoly that science possesses. Alternatives to science should be pursued simply because an unreflective commitment to science will render us dogmatic and incapable of thinking otherwise. This is what Feyerabend finds disquieting.

Extending the Metaphor

I want to propose a reworked version of Fuller's metaphor and map it onto the field of science studies. The following should not be regarded as an accurate representation of science studies. It is, at best, a heuristic reconstruction and should not be treated as anything but. There are, of course, various combinations and the differences are more accurately represented in the form of a continuum rather than discrete categories (e.g. Ian Hacking would not be happy in either of the two groups). This narrative is merely an abstraction.

I would argue that a major turning point in the way scholars from disciplines other than the natural sciences perceived science was the work of Thomas Kuhn (Turner 2008: 33). There had, of course, been people before Kuhn who had proposed versions of scientific reasoning (e.g. Fleck's thought styles, Quine's webs of belief and Polanyi's tacit knowledge) that were not, strictly speaking, in accordance with the picture provided by the logical positivists or critical rationalists, such as A. J. Ayer (2002) or Karl Popper (2002). However, in none of the cases did the ideas seriously alter the nature of the

debates outside the core academic audiences. Kuhn's book, on the other hand, managed to create a schism between two camps that, even though both combined elements of Kuhn's authoritarianism and Popper's liberalism, ended up at odds with each other.

The first camp is populated mostly by philosophers of science. These people retained a belief in the existence of a particular method, or at least the possibility of outlining a set of rules, procedures, rules of thumb that would be specific to science and good reasoning, and distinguish these from other forms of knowledge-making. In other words, they persisted in trying to come up with a list of conditions for something to count as science and, therefore, guarantee a zone of certainty that would serve as a foundation for the construction and evaluation of candidate disciplines, theories and instruments. The scientific method (or scientific tradition) is something that is more or less context-independent and distinct. Political interests and pressures are regarded as something that is external to good science and contaminates it. They may well be co-extensive in practice, but science has a distinct logic of its own once it is purified of such accidental accretions (e.g. Robert Merton's ethos of science). These philosophers are committed to a normative project that seeks to articulate a scheme by which authoritative knowledge is to be produced and certified. It is, however, up to them and philosophically minded scientists to do this. Popper's liberalism is present in the form of a possibility of critique, and Kuhn's authoritarianism is present in the form of a tradition that limits the scope of inquiry. Functionally, however, the roles are reversed. The impetus from Kuhn comes in the form of history and the recognition of contingency, whereas Popper's contribution is expressed in the importance attached to rules of inference. In other words, history serves as a backdrop for narratives about the rationality of the choices that scientists make.

The alternative tradition is the foundation of this thesis and is represented by the diverse approaches within science studies. Science is perceived as a complex practice than cannot be reduced to, and expressed in, methodological directives and rules of inference. Knowledge and skills are embodied by practitioners and function at an unconscious level that eludes propositional treatment. Science is always situated in particular configurations of social structure, culture and political interests. It is, therefore, thoroughly context-dependent and sensitive to the specificities of the local environment (though not necessarily reducible to these). Science, according to this group of scholars, is a historically and geographically specific phenomenon, and one among many ways of

organising our experience and knowledge of the world. What is more, the right to (i) criticise and comment on what counts as good science and (ii) decide how it should be integrated with other interests extends beyond the control of scientists. This is both a political and epistemological point. The recognition of historical and geographical specificity is entwined with the idea the science produces accounts of reality that are partial and embody highly particular conceptions of knowledge and objectivity. Different perspectives should be included, and a plurality of approaches should be encouraged so as to counter the wilfully ignorant (as regards history) and necessarily particular nature of scientific research.

This group urges us to recognise the inconvenient fact that science is shot through with non-evidential reasoning. The philosophers did this too, but they gave primacy to the formal, epistemic and evidential and saw reasoning that contravened such norms (however specific historically) and expectations as incidental and secondary in a description of the specificity of science. They regarded the presence of non-epistemic factors as a regrettable nuisance. For a number of strands within science studies, on the other hand, this was an integral part of scientific reasoning, because they treated science as a collective form of inquiry where discourse and exchange followed conventions and protocols that were socially agreed upon. Science was never just about abstract formal expressions (e.g. mathematical formulae). It also involved the procedures of justification that validate the formulae, and the applications of such formal principles in practice (Harding 1986). However, it is the inherently social nature of science that necessitates intervention (especially in the case of feminist epistemology [Anderson 2011]). Thus, from Kuhn they inherited the belief that science is a form of collective culture, whereas Popper provides the belief that one should maintain a critical stance towards the status quo. There are even glimmerings of Feyerabend in the suggestion to multiply research trajectories concerned with similar questions, so as to prevent dogmatism and a monopoly of partial perspectives that work with a specific set of background assumptions both with regard to its object of study and its conception of what constitutes knowledge. This divergent framing of science was reflected in their respective politics of science. The former placed the right to define science in the hands of scientists and philosophers of science, whereas some science studies scholars were much more permissive and allowed outsiders to comment upon science (e.g. Brian Wynne). In fact, this was precisely the point – as science affects the lives of non-experts, it should be

accountable to them.

Conclusions

SSK can be regarded as an attempt to study science dispassionately and turn the analytical methods of science against itself. This move, while intriguing, attracted a great deal of critique both from philosophers of science and other sociologists. Some of this may have been due to the perception that SSK work was undermining the authority of science, but numerous authors questioned the academic merit of explanatory mechanisms that privileged the insights of sociologists and paid scant attention to the role of non-human agency. The solution was to become more reflexive about the relationships between knowledge and power, science and politics, and the distribution of agency between humans and non-humans. However, an unintended side effect of such research (and similar work in the philosophy of science) was that it became difficult to articulate what constitutes the distinctiveness of science. It is true that science studies' commitment to science as an epistemically distinctive form of inquiry is much more restrained, reserved and sometimes even implicit. What is more, there is a general acceptance among such scholars that science should be tempered, but this is done out of recognition that science is socially enmeshed in networks constituted by different and conflicting cultural logics. There is a growing appreciation that a separation between technical solutions and ethical and political considerations is no longer as compelling as it was a few decades ago, and this has led to the belief that technical expertise should not be permitted to reign without question or prevent public deliberation. One needs to carefully consider and interrogate the process by which knowledge is produced and what political and economic potentialities it embodies (and which ones it erases). The actual content of the knowledge provided by science is now only one aspect among many to consider when constructing academic narratives about science. However, this move has come at the cost of challenging and repudiating conventional accounts of science. How to square such insights with a commitment to science as a worthwhile and distinctive cognitive endeavour? This is a question I turn to in the next chapter.

2. Studying Science, Part II: Dealing with Misadventures

An important consequence of the arguments and approaches I looked at in the previous chapter was a less deferential attitude towards science. Unfortunately, this was complemented by an inability to block accusations of sociological reductionism. A possible solution to this problem was provided by a reworking of the symmetry principle that would complement a symmetrical treatment of truth and falsity with an equal appreciation for the complex processes through which both sides of the nature/culture and human/non-human divide, co-construct and depend on each other. Much like the move championed by the Strong Programme, however, the introduction of this additional dimension made science studies susceptible to a plethora of academic accusations. In particular, while a number of contemporary scholars influenced by science studies have argued for epistemically (as well as ontologically) sensitive, risky and self-aware forms of science (e.g. Haraway 1991; Law 2004; Stengers 1997; 2000), there seems to be a kind of ambivalence about science as the epistemic tradition of choice, especially in view of the accumulated evidence suggesting that its uniqueness has been overstated.

In this chapter I (i) explore a number of approaches that have been put forward to refine the constructivist tendencies inherent in science studies and (ii) identify a potentially problematic aspect of a self-aware form of what Sergio Sismondo (1992; 1996) calls heterogeneous constructivism. My main example of heterogeneous constructivism is actor-network theory, and I conclude that it and similar approaches cannot challenge particular scientific practices on the grounds that their integrity has been compromised.

Building Worlds from Heterogeneous Materials

Sandra Harding (1986) has suggested that the division of labour between philosophy of science and sociology of science has resulted in philosophers trying to elucidate an idealised form of scientific reasoning, while sociologists have been much more concerned with the actual practices of science. This is equally true of the science studies that have attempted to come up with an empirically grounded explanation for the undeniable success of the sciences and the prestige attached to them as forms of

knowledge. However, in the previous chapter I argued that one of the main issues with SSK in particular was its insistence on the importance of interests (be they political or disciplinary) and social factors in the growth and development of scientific knowledge (e.g. Barnes 1977; Barnes and Bloor 1982). It was gradually recognised that reducing the success and vigour of science to non-epistemic factors was unsatisfactory. The question remained open – what exactly is it that separates science from other forms of knowledge? A prominent line of reasoning that developed in response to this was constructivism.

Two Species of Constructivism

Much like the word paradigm, constructivism is a term drenched in confusion and ambiguity. It is no surprise, therefore, that in science studies alone there have been a number of books and articles devoted to clarifying what kind of relationship to the world this word suggests (e.g. Hacking 1999; Radder 1992; Sismondo 1992; 1996). What does it mean to say that something is constructed? What materials are being used? Science studies in particular has suffered greatly as a result of this confusion because its constructivist leanings have often been conflated with relativism (Fuller 2006b: 35-39). Furthermore, while some scholars choose to merge SSK and constructivism (e.g. Brown 2001; Panofsky 2010), I believe that there are important differences that justify distinguishing between the constructivism championed by SSK and other variants of this approach (e.g. actor-network theory).

In Chapter 1 I argued that the former emphasised the pliability and the inherently negotiable character of the rules and norms regulating scientific research. However, their decidedly interactionist approach to scientific norms was complemented by a realism as regards social entities (Collins and Yearley 1992). What is more, it was noticed by some that early science studies focused almost exclusively on high science and only paid lip service to the more mundane practices and technology-based forms of science (Giere 1993; Hacking 1983; Bijker and Pinch 1992). Early science studies scholars were concerned with the theoretical content of science, and it was argued that SSK (and early science studies work in general) was implicitly a form of social constructivism whereby science was construed as the effect of social, economic and political relations (society > science). While it would be unfair to claim that SSK paid no attention to the role of instruments (e.g. Collins 1975), the emphasis was usually placed on theoretical content or

scientific disputes. Consequently, it was a form of epistemological constructivism that focused on ideas and arguments, rather than the materials and artefacts that are an integral part of scientific work. The brand of constructivism I will discuss below highlighted the importance of practice and introduced a material and ontological dimension into the equation. This was supplemented by a move away from a representational and discursive take on science and scientific knowledge (e.g. Hacking 1983). Theories were still important, but the emphasis gradually shifted towards science as a material practice, as a form of material culture. To be sure, the constructed nature of scientific realities was central to the new understanding, but they no longer merely reflected contemporary social mores or projected these onto the world. The materials deployed in the process of construction were varied. Science was the result of work and accommodation. Specifically, the accommodation of the obdurate qualities of materials that are involved in constructing particular ontologies, and the social and material practices (e.g. experiments) that make them visible, deployable and stable (Hacking 1999; Knorr Cetina 1981; 1993; Oudshoorn 1994; Radder 1992; Sismondo 1996). Science is a form of knowledge, but it is equally involved in constructing the material artefacts (e.g. trains) that it works with and constructing the world in which the products of scientific labour are to be deployed (e.g. laying train tracks).

There are different versions of this. Some authors explicitly played down the extent to which the material world was obdurate and subscribed to fairly radical forms of nominalism and contingency (e.g. Ashmore 1989; Woolgar 1988); others were much less impressed by the idea that the materiality of the world is radically contingent, but they also acknowledged the productive nature of scientific knowledge (e.g. Hacking 1999; 2002; Radder 1992). In both cases, however, science and its renderings (both material and semiotic) of the world were treated as the result of competently marshalling social relations (collective negotiations, interests), employing scientific instruments and disciplining non-humans. Humans were no longer the only actors that had to be negotiated with. An increasingly prominent branch of this way of talking about science is what Sergio Sismondo (1992; 1996) has dubbed heterogeneous constructivism.

The networks are heterogeneous in the sense that they combine isolated parts of the material world, laboratory equipment, "black-boxed" knowledge, patrons, money, institutions, and so on. It is all of these agents together that create the successes of technoscience; no one piece of that network wholly determines the shape of the whole.

It should come as little surprise that Sismondo was referring to actor-network theory.

Might and Materiality

In addition to introducing a vast array of terminological innovations and challenging a series of entrenched assumptions in Western metaphysics, actor-network theory⁵ suggested an interesting explanation for the immense success of science. It is really quite simple – science is ruthlessly efficient at building sociomaterial networks (Callon 1986; Callon and Latour 1981; Latour 1983; 1993a; 1993b; 2005; Law 1986; 1992; 2004). Science is an effective and efficient way of building alliances between human needs, interests and non-human actors. It is not the result of genius, serendipity or strict adherence to methodological directives, but neither is it window dressing for political or non-epistemic interests. It is, in fact, the result of hard work. Science is a heterogeneous sociomaterial practice. It employs a vast array of instruments and devices, and circulates artefacts and inscriptions. Furthermore, it reconfigures ontologies by introducing new entities (e.g. bacteria) into the collective and forges links between heterogeneous actors (both human and non-human).

Research is best seen as a collective experimentation about what humans and non-humans together are able to swallow or to withstand.

(Latour 1999: 20)

Scientific research sometimes results in networks of human and non-human agents mutually reinforcing each other, and these connections reconfigure the sociomaterial landscape (composed of other networks) that preceded them. For example, Bruno Latour (1983; 1993a) argues that Louis Pasteur's laboratory was a success story because it managed to insert itself into its respective sociomaterial network by incrementally turning itself into a *sine qua non* in the battle against anthrax. Pasteur had to contend with existing knowledge and alternative explanations for the outbreaks and manifestations of diseases, and the death of livestock. Pasteur emerged victorious, however. He achieved this by, among other things, (i) introducing a new entity into society and (ii) claiming to be its representative. In other words, Pasteur and his colleagues redefined reality by weaving another thread into its fabric and positioning itself between it (the anthrax bacillus) and the actors whose lives it affected (e.g. farmers). They created interest both in the sense of arousing curiosity and positioning itself between numerous sides whose

⁵ Hereafter – ANT

interactions it was competent to manage, and created a monopoly on an aspect of reality about and for which they could speak.

The specificity of science derives from the regularity with which it is capable of forging robust and durable networks. This is not to say that science is not subject to contingency or somehow exempted from problems that are characteristic of other forms of human association (bias, intrigue, misinformation, personal dislike). Latour's account of Pasteur's rise to prominence is replete with such details and in this way similar in spirit to the work championed by proponents of SSK. However, actor-network theorists contend that science cannot be reduced to political manoeuvring. Culture can colonise the realm of nature, and humans can impose their descriptions on non-humans but only insofar as the latter cooperate and do not bump back. In effect, ANT supplements the symmetry thesis proposed by SSK with an ontological dimension (Callon 1986) and urges scholars to recognise the agency of non-humans. By doing so, it gives science studies a way to (a) explain the immense practical success of science and (b) supplement the insights of early attempts to understand science empirically.

Underlying these claims is a non-representational view of knowledge that suggests that science is actively involved in constituting the world, rather than uncovering pre-existing structures (Porter 1993). The descriptions of the world that scientists offer are certainly part of this process, but of equal importance are the instruments and practices that allow them to materialise and interact with the ontological constituents that their theories posit. The materiality and causal powers of the world should not be excluded from accounts of science, and their ability to affect our (human) lives should become a prominent part of academic narratives. The requirements of the co-production idiom seemed to have been satisfied – neither science nor society is given a privileged role.

When Might is Right

The elegance of ANT and its implicit refusal of committing to any one political agenda is commendable. From a descriptive standpoint it is innovative, but I would suggest that there is an important issue with it. What is quite striking about ANT is that, in effect, our proxy for the validity of a particular scientific rendering of reality is the resilience and durability of the sociomaterial network that constitutes and supports it (Latour 1993a; 2004b). For example, the quality of Pasteur's theories and techniques derives from how well it deals with challengers and alternative explanations and practices. Truth and agency

are the result of competition between various networks. This is particularly pronounced in the work of Bruno Latour, and his insistence on trials of strength suggests a refusal to decouple the quality of science from its durability (or truth from power). Latour says as much himself.

We cannot distinguish between those moments when we have might and those when we are right.

(Latour 1993a: 183)

Quite a few scholars have noticed this (e.g. Fuller 2000b; 2006; Collin 2011) and expressed their discomfort with such a take on science (e.g. Haraway 1997b). There is something troubling about framing the question of success in terms of durability and conflict. The history of science is replete with examples of discriminatory (e.g. racist and sexist) modes of thinking that had significant currency in scientific circles (e.g. Laqueur 1990; Schiebinger 1989; Fausto-Sterling 1995). What is more, they were exemplary of the sciences of the time. Complex argumentative strategies were developed so as to weave together measurements and beliefs about race and gender so that white males of European descent came out on top in all the relevant categories. I do not see why one should have difficulty (i) treating these symmetrically as instances of science in the past and (ii) rendering their success intelligible in a comparable manner. However, there would probably be an implicit urge to make it apparent that these kinds of projects were dissimilar to today's scientific practices, or at least a vague unease about equating them. This is the ambivalence that I alluded to at the beginning of the paragraph – we would like to be able to differentiate theories on more solid grounds than the contentious ones' eventual dissolution. This does not necessarily mean an invocation of truth, but robustness and durability seem equally unsatisfactory, since the argumentative repertoires were certainly robust and persuasive at the time.

Similarly, one could make the argument that ANT's emphasis on forging alliances and representation sounds ferociously political. Indeed, Bruno Latour has been explicit about this (Latour 1991; Brown 2009). The difference from politics, of course, is that in science humans have to negotiate with non-humans and come up with terms that work for everyone involved. What is more, there is always something outside the network that can and eventually will disrupt its smooth functioning (Latour 2005). But is it not a general point about building sociomaterial networks that seems to be the issue here? That is to say, is ANT making a point about science or science as a kind of network? It seems to me

that the latter is true, since the talk is of networks that transgress and confute the distinctions power/knowledge and society/nature (Latour 1999: ch. 3; Collin 2011; Guggenheim and Potthast 2011). This puts ANT in an awkward position. If science is only a particular instance or component of sociomaterial networks, it becomes insufficiently differentiated from other kinds of activity and epistemic endeavours that can systematically rely on the cooperation of non-human agents (e.g. religion). In other words, it loses its distinctiveness and identity as a particular kind of sociomaterial network. Consequently, we are left with an uncomfortable conclusion. ANT is an exceptionally powerful descriptive technique, but its emphasis on durability gives us little in the way of tools for dealing with conflicts between networks of comparable resilience and vitality. More recent literature has certainly moved away from using the language of force and durability, and manifested an interest in the ethical and political implications of science and technology (e.g. Latour 2004a; 2004b; 2004c; Law 2004; Mol 1999; 2003), but this is a difference in emphasis, rather than a full-blown re-imagining – the resilience and durability of networks is still present in the background.

This presents us with a problem, as it is not clear how we could approach a rendering of reality that (we believe) is problematic yet commands the assent of political or scientific authorities. Is there anything more to our unease than an unwillingness to endorse it and forge links with the sociomaterial network supporting it? The work of social scientists and humanities scholars studying science has given us ample reason to doubt its exalted status and the privilege we thrust upon it (Collin 2011; Fuller 2000b; 2006; Pels 1995; 1996; 2003). What is more, contrary to philosophers of the analytical tradition who can fall back on good standards of reasoning, rationality and the logical compulsion of the better argument, there seems to be no way out for science studies scholars – constructivists in particular.

Situating Risky Knowledge

The work of Isabelle Stengers (1997; 2000) shares ANT's non-representational take on knowledge. She provides a number of arguments that complement, draw upon and have influenced Bruno Latour and John Law, and could potentially rectify the problem I have outlined above. I read Stengers as arguing against the position that our recognition of the fallibility of science leads us to abandon it. Science is permeated by a variety of unfortunate metaphors (a lot of them gendered), unchecked assumptions and hampered

by a wilful ignorance of its own specificity and contingency. Work in science studies has made us acutely aware of our capacity for getting things wrong and failing to recognise the specificity of the positions we speak from. Stengers' response to these developments is intriguing.

Theodore Porter has argued that there is a strand of constructivism which contends that 'scientific knowledge is true, but chiefly in relation to a world we have constructed' (Porter 1993: 87). It is along these lines that the arguments of Isabelle Stengers (1997: 2000) should be understood. She contends that the specificity of the sciences lies in their ability to create material apparatuses that simultaneously (i) define what counts as a scientific problem and (ii) put potential answers to such problems to the test. This is what was so unique about Galileo. He constructed an experimental apparatus that demonstrated the veracity of his theory and provided a framework against which other theories could be assessed and legitimated. He devised a way to construct scientific facts out of fictions. This move was crucial. The power of science rests in its ability to make other narratives and knowledge systems appear as fictions and distinguish itself from them by way of procedures of justification and tests of legitimacy. Science is about putting stories to the test and inviting others to challenge them. By going through this process stories lose their fictional status and become detached from the person who created them (Latour and Woolgar 1986; Law 2004). They move from the realm of stories to the realm of facts that can travel from context to context. The trouble with science (or more precisely – scientists) is that they often want to claim more than their achievements allow.

As Theodore Porter's neat summary illustrates, the reality and durability of scientific constructions and insights is tied to particular situations, experimental apparatuses and procedures. Science is a constructive and creative process that meshes different kinds of materials, but its ability to reshape the world is historically contingent and tied to particular instruments and contexts.

Physicists cannot simply abandon these equations since it is through them that physics established a particular experimental relation with the observable world. [...] they are situated in a tradition from which they receive their instruments and their language: if they reject them they lose any possibility of communication with their colleagues, they are no longer physicists, they find themselves isolated, alone in a labyrinth of phenomena that have again become indecipherable.

Stengers (1997: 26)

The reality and authority that scientists want to claim for science goes beyond this, however, which is where the political dimension of science comes in.

While Stengers distances herself from attempts within science studies to understand science as a kind of power politics or social construction (e.g. Stengers 2000: ch. 1), she freely acknowledges that politics is part of the scientific tradition. For one, science is an active participant in the redistribution of rights and duties with regard to agency, truth and the standards of inference. However, even though the realities constructed by and with science are not reducible to power and politics, the intimate connection between science and politics becomes clear if one acknowledges that scientific knowledge is involved in the production of highly particular social and material configurations. The reason for this is simple. The content of scientific knowledge may be irreducible to politics, but the decision to test and stage some stories rather than others is clearly political and revealing of the non-epistemic dimension of science. Stengers suggests that a solution to this is a conscious integration of human affairs and the management and production of things (Stengers 2000: 147-149), not unlike the parliament of things put forward by Bruno Latour (1993b; 2004c). Science should be sensitive to the contexts in which it operates and remakes, and the individuals it describes and affects. Politics and science are, indeed, meshed together. Science reconfigures both the epistemic (redistribution of rights with regard to truth) and social (new configurations of human and non-human entities) landscapes. More importantly, however, the intimate connection between particular sciences and the repertoire of material tools and procedures that allow for the construction of their respective scientific facts should not be overlooked. A direct consequence of this is that scientists should restrain their urge to claim more than their achievements merit and should not assume that other forms of knowledge or explanation are illegitimate, for it is at this point that a politics of reason becomes a disagreeable from of power politics.

This last point in Stengers' account manifests similarities with the work of Paul Feyerabend, who also argued that scientists are often smug and condescending towards alternative knowledge traditions. The disagreeable aspect of this is that such derision has more to do with the chauvinism of science (or particular scientists), rather than any intellectual deficiencies of the opponent. Such disregard for alternatives comes from a reification of science and a fetishism about past scientific achievements and the

procedures that lead to them. Indeed, Feyerabend believed that the contemporary obsession with the scientific method was indicative of a broader tendency to equate science with what counts as scientific in the present. The problem, of course, was that the current state of science was a product of historical and philosophical development, and actually quite diverse internally. What is more, there is no reason to assume (in fact, Feyerabend believed this was counterproductive) that science will not continue to evolve. As I said in Chapter 1, Feyerabend is not an enemy of science – he disliked the reification of science and the attendant arrogance. Consequently, while this may seem counterintuitive at first, it is plausible to argue that Feyerabend shared Stengers' commitment to science as a creative and historically contingent process.

The Performativity of Method

Thus far Stengers' approach is quite similar to Latour's variant of ANT. What Latour (2004c) borrows from her is a tentative account of how one can distinguish between good and bad science. Stengers contends that scientists' interactions with the world are mediated by intricate apparatuses and conceptual frameworks that attempt to accommodate complexity (Stengers 1997). This in itself is not an issue. The problem appears when scientists force the world to correspond to these abstractions. For Stengers good science seeks to devise questions and experimental apparatuses that put beliefs at risk and subject them to experiment and the possibility of confutation.

[D]evise your inquiries so that they maximize the recalcitrance of those you interrogate.
(Latour 2004c: 217)

The vulnerability of our understandings should be exposed, and others should be allowed to challenge them. Vulnerability is, therefore, a kind of strength. For Stengers, the work of Ilya Prigogine, Barbara McClintock (Stengers 1997) and Shirley Strum (Stengers 2000) serve as examples of such sensitivity. These scientists did not impose their own descriptions and allowed their objects to articulate themselves.

A related line of argument has been developed by John Law (2004; 2008; 2009; Law et al 2011), Annemarie Mol (1999; 2003) and (to a certain extent) Bruno Latour (2010). These authors have argued that the nature of the method chosen brings with itself a particular kind of imagination as regards its object. In other words, there are certain assumptions in place that shape and limit the nature of the results generated from the data. What the aforementioned scholars emphasise is that these assumptions do not just describe

whatever their particular object might be, but they also enact a particular version of it. The renderings of reality that these methods produce, therefore, provide a description of a part of reality that is already imagined in a particular way – the results are a description of the reality of the object according to the method in question. This is somewhat similar to what Wynne (see Chapter 1) said of policy-making, in that policy makers operate with certain implicit assumptions about what the public is like and on what its objections are based.

If one method enacts a particular rendering of reality, alternative approaches might come up with something completely different because renderings are always partial. They tend to explain away or ignore certain elements of situations that are, in fact, crucial to them simply because the methods employed are only capable of dealing with ontologies that are bound in a particular way (Law 2004: ch. 4). The upshot of this argument is that there are always multiple possible realities, whose existence and practical relevance to our lives depends largely on whether, and in what contexts, we enact them (Law 2004; Law and Singleton 2005; Mol 1999; 2003). How to choose between these? This is a question of ontological politics. If, as the performativity argument suggests, reality is not fixed but enacted through the way we choose to interact with it, our choices determine the nature of our knowledge and the realities it makes possible. This is what ontological politics is about – the struggle over the recognition and enactment of different realities.

An interesting implication that follows from this is argument is that knowledge-making is indeed permeated by ethical and political questions. It goes beyond choosing which avenues of research to follow (e.g. Hacking 2000; Kitcher 2001; Oudshoorn 1994) and argues that the choices we make influence the kind (but not necessarily quality) of the knowledge we end up with. Gad and Jensen (2010) argue that this approach is salutary from an academic point of view. In particular, it undermines a relativistic account of knowledge that focuses on the multiplicity of perspectives but assumes a relatively stable material baseline (Fuller 2006b: 35-39). The work of John Law and Annemarie Mol illustrates that our perspectives, and the realities they enable, both produce and are produced by the world. It makes us uncomfortably aware that the versions of reality we choose to enact and support bring with them a series of commitments and hopes.

I would argue that something similar could be said of other work in science studies (e.g. Fausto-Sterling 2000; Barad 1998; Haraway 1997b) that highlights the intimate and highly

consequential entanglements of culture, ethics, method and materiality. They are simultaneously cautionary and enlightening. They urge us to accept responsibility for our actions and consider both the positive and negative effects of how we chose to practice science and the sorts of technologies we choose to deploy. However, it also illustrates the multitude of possibilities, which, while not infinite, subvert the presumption of inevitability in what we are and what we can be. This, I believe, is why highlighting the contingent nature of science and the realities it enables is so important. It is not necessarily a matter of showing that our beliefs and practices are false or misguided. Rather, the import of the abovementioned impulse derives from the ability to interrogate our most cherished epistemic commitments and showcase their plasticity. Things could have been otherwise. The decision to follow a particular path was determined by a multitude of factors, only some of which were not subject to human control (though they may well have been beyond the reach of individuals). I would argue, however, that this very same approach, which is so liberating academically, makes epistemic critique somewhat complicated.

The Elusiveness of Science

An important issue in the debate between Larry Laudan and David Bloor was Laudan's contention that we do not really have a clear idea of what makes science unique and whether there are any principles that distinguish it from other forms of knowing (Fuller 2002). In view of this, Laudan found Bloor's insistence on the scientific nature of SSK problematic. Laudan's argument was basically this: if we have examples of X, but we lack an understanding of what makes them Xs, how do we know if a purported instance of X is actually an instance of X? Bloor replied that it is enough to have examples of something to be able to study it and chastised Laudan for having an overly propositional understanding of knowledge; feminist epistemologists have also accused traditional philosophy of science of this, so Bloor is not alone. The work of Polanyi (2009), Kuhn (1996) and the numerous laboratory studies have provided ample evidence that knowledge is not always propositional or possessed explicitly – it is often embodied and passed on in the form of practices and particular ways of doing things, rather than explicit instructions. But even if we accept Bloor's objections, it seems that his approach only works just in case a discipline can claim scientific status. This suggests that proponents of SSK have to be content with institutionally sanctioned scientific disciplines. Without a set of necessary or sufficient conditions (in the vein of the

principles of verification or falsification) the situation becomes complicated and its resolution elusive. A discipline becomes part of the category science only when it is regarded as such by a number of heterogeneous actors. There is no straightforward list of qualities a discipline can manifest in order to qualify as a science.

This is not to say that such a list of criteria is desirable. If we had something like this, it would most likely be retrospective and would reify a particular version of science. This would complicate life for new branches (which it did – e.g. the social sciences), as their acceptance would hinge on their consistency with their more established relatives. However, the fact that we do not have such a list makes things very problematic for a distinctly prescriptive and normative take on science. Feyerabend's epistemological anarchism can be seen as a response to this. His pronouncement that anything goes (Feyerabend 2010) is not prescriptive, or even descriptive. Instead, I read it as ironic. It is an oblique way of claiming that novel forms of inquiry (or even theories in one discipline) often contradict the prevailing norms. According to Feyerabend, numerous scientific breakthroughs have been made by people who were either ignorant of or even blatantly disregarded the standards of their time. His interpretation of the arguments and theories of Galileo is illustrative of this point. Contrary to most historians of science, Feyerabend portrays Galileo as an opportunistic cheat whose methods were wildly inconsistent with what was acceptable at the time, and that was precisely the point – Galileo succeeded because he cheated. Consequently, “anything goes” is best seen as an injunction against a criterion of demarcation, as this would merely be a snapshot of what passes for science at a particular point in time. It would be a reification of science whose good health depends on risk-taking and creativity unconstrained by conventional rules.

We can say today that Galileo was on the right track, for his persistent pursuit of what once seemed to be a silly cosmology has by now created the material needed to defend it against all those who will accept a view only if it is told in a certain way and who will trust it only if it contains certain magical phrases, called “observational reports”. And this is not an exception – it is the normal case: theories become clear and 'reasonable' only *after* incoherent parts of them have been used for a long time. Such unreasonable, nonsensical, unmethodical foreplay thus turns out to be an unavoidable precondition of clarity and of empirical success.

(Feyerabend 2010: 10-11)

I would argue that contemporary developments in ANT and the work of Isabelle Stengers (among others) are aware of these issues and are promoting a more self-aware and responsible approach to the sciences. However, it remains unclear what exactly is the tradition that needs to be reworked, practised responsibly and developed so as to allow

the material world to articulate itself in interesting and potentially surprising ways. For example, feminist epistemologists have argued that we need to re-imagine science and the notions and concepts that organise it (Anderson 2011), but what is it that makes science distinctive and preferable to other forms of knowledge? Isabelle Stengers takes this question seriously by pointing to the crucial importance of experiment and risk-taking. However, she argues that these characteristics are also present in psychoanalysis (Stengers 1997: ch. 5). This is not to suggest that Stengers is wrong for identifying laudable impulses in a controversial school of thought, but by doing so Stengers reveals an interest in epistemic endeavours with a manifest willingness to put their claims and beliefs in danger, be surprised by their objects and let them speak, rather than the distinctiveness of science. Latour has suggested that one way of reading Stengers' argument is that she wants to move away from an epistemological account of what makes science distinctive and develop an argument located at the level of ontology (Latour 2004c). In other words, Stengers is perfectly aware of the epistemological difficulties involved in specifying a criterion of demarcation (Stengers 2000: 1-52) and chooses to reinterpret the issue as being one of epistemically interesting pursuits, rather than science vs. non-science. For her the crucial question is not whether something can claim scientific status, but whether it manifests a sensitivity to the specificities of the object under study and provides the object with an opportunity to challenge the queries that are being put to it. This suggests that Stengers would happily acknowledge that not everything that passes as science is worth our attention – interesting and risky articulations of social and material realities are more important.

A potential problem appears at this point, though it only affects Latour's variant of non-representational constructivism. The upshot of ANT is that reality is procedurally constituted. That is to say, reality is a product of various actors working together to construct it and acquires a definite form and set of relations only after the fact. Furthermore, an actor's agency is (i) determined after a particular network has come into being and (ii) is defined in relation to other elements constituting the network.

According to Latour, certain actants only come to exist as a result of the activities of a well-established network. The conclusion seems inevitable that those actants could not have contributed to establishing the network in the first place – although this is a corollary that Latour apparently misses.

(Collin 2011: 117)

The above might not be a big problem for Stengers, but it complicates matters if we want

to infuse Latour's account with her insights. In other words, Stengers talks of objects or phenomena in a way that allows for agency before said objects or phenomena are articulated, which is why she can chastise certain forms of science for limiting the potential of speech and confutation. It is harder to see how Latour can do the same without allowing for agency that precedes the stabilisation of the network. In his case the success and resilience of particular sociomaterial articulations are crucial to the explanation of science, and it is not clear that one can talk of bad science, rather than failure and unsuccessful science. At this point science becomes a descriptive term whose referent is polymorphous and historically contingent. It is a term designating a specific set of instruments, practices, institutions, ontologies and practitioners that constitute a form of inquiry at a particular moment in time. The boundaries of what is included in this category can be tightened and loosened. This means that, according to ANT, our approach to science is to be explained in terms of successful instances of it because these are the ones that have earned the right to be counted among the sciences. This leads us back to the importance of success – science consists of those assemblages, instruments, practices, institutions, ontologies and practitioners that can marshal support and persuade others. ANT in this instance is akin to reverse-engineering. It can explain why a particular endeavour failed or succeeded, but it can only do so from the privileged vantage point afforded by the present (Asdal 2012; Radder 1998a; 1998b). Once we have divested science of its traditional rhetorical ammunition and subjected it to analyses of objectivity and rationality, we have to justify our commitment to science in a different way. The analytical weight placed on trials of strength is not able to do this if we want to retain the option of distinguishing between being right and having might. This is especially crucial if one wants to argue that something has a corrosive effect on science.

When Things Go Wrong

The upshot of the above arguments is that numerous enactments of reality are possible. The choice to focus on, prefer and materialise certain enactments over others is open-ended (constrained by institutional and epistemic inertia). A consequence is that many ways of appropriating scientific knowledge and understanding ourselves are possible. According to Donna Haraway, we should stand up for certain ways of doing this and challenge others (Haraway 1997b). This, presumably, means "casting our votes" for more inclusive and liberal material-semiotic figurations and forms of knowledge. However, the

commitment to performative ontologies and a non-representational view of knowledge, while making us responsible for the choices we make, renders the language of biases and corrosion complicated. For example, I would argue that most science studies scholars would assent to the claim that the incapacity of people other than white males of European descent was a construction. What is more, it was not only a discursive construction, because it was made real through material practices (e.g. limiting access to education) and legislation. Consequently, it was a sociomaterial construction of the kind favoured by heterogeneous constructivism. From an ANT perspective, however, it is not clear that the scientific justification for these practices was lacking, or that they were contaminated by undesirable non-epistemic factors, because the foundation for these beliefs was located, at least in part, in widely accepted and institutionally recognised forms of knowledge. A less dramatic illustration would be a comparison between Ian Hacking's claim that child abuse was a construction (Hacking 1991; 1999) and the debates surrounding the link (or lack thereof) between smoking and cancer (Ashmore 1996) and hormones and homosexuality (Oudshoorn 1994). These are all constructions, but we should like to differentiate between different constructions and identify ones that are dubious or permeated by bias and prejudice, or perpetuated by commercial interests, regardless of how much assent they command. The problem is that ANT cannot really do this, due to the fact that assumptions about what moves were justified are collective achievements and agreements that only become clear after the fact (i.e. post-construction).

Identifying Malpractice

Different responses are possible depending on what conceptions of agency (human or non-human) you subscribe to and what form of critique you wish to practice. If you believe that agency is an emergent, relational or procedurally constituted quality, your options are somewhat limited because reality is an effect, a result. If you allow for agential potential that precedes the enactment of it in particular sociomaterial practices, you are slightly better off, but it is still unclear as to how much agency there is and what positive claims, if any, one can make. In relation to this Hans Radder (1992; 1998a; 1998b) has made the point that constructivist science studies is plagued by a slightly ambiguous politics regarding constructions. He suggests that in some cases talk of constructions is a proxy for illustrating contingency (epistemic, ontological or methodological). Such instances, I would argue, fall into the category of cautioning and

enlightening us, and raising self-awareness. However, there are instances where talk of constructions is a veiled way of saying that something is inaccurate or based on problematic assumptions.

Now, I mentioned above that constructivism is internally varied, and a seemingly pertinent distinction at this juncture is between what Andrew Abbot calls ideological and constitutive forms of constructivism⁶ (Abbott 2001b: 61-67). ANT's emphasis on the constitutive role of scientific practices (e.g. work on vaccines in the laboratory) in the construction of reality (e.g. a world with vaccinated people) suggests that it is closer to the latter. This point becomes especially pertinent in cases where an enactment, construction of issue-definition is challenged, and we want to make the case that the integrity of a scientific project has been compromised, cultural prejudices have interfered with the research process, or the interpretation of results has been narrow-minded. It is not sufficient to say that there are certain assumptions at play, because this is always the case. The challenge is to show that at least some of these assumptions are not warranted. This, however, involves making at least a tentative or implicit claim as to the actual state of affairs, but this is complicated to do for a consistent constitutive constructivist because reality is a product of sociomaterial practices, rather than a stable and pre-existing platform for them.

After all, the positive claims that “reality is complex”, or that “there is a crucial political asymmetry between experts and lay people in the twentieth century”, are simply two more opportunities for deconstruction, and not a motive for normatively relevant reflexion [sic].

(Radder 1992: 149)

The same ambivalence is evident with regard to science more generally. For example, what authority can it claim if it is permeated by ambient values and influenced by non-epistemic factors? A possible answer might be that what science studies is challenging is not science itself. Rather, it is the way institutions (be they private or public) promote loaded issue-definitions (Wynne 1998; 2003) and particular kinds of human and non-human subjectivities (Haraway 1997b). The target of critique could also be the danger inherent in the commercial appropriation of science and the resulting displacement of values which are constitutive of it (Longino 1983; 1990; 2013). The last point seems to be one of the guiding principles behind the economics of scientific knowledge, which, among other things, interrogates the consequences and effects of the commercialisation

⁶ Abbott uses the term constructionism.

of science. Similar anxieties can be identified in the work of Sheila Jasanoff (1990: 2005), Donna Haraway (1997) and Steve Fuller (2000b; 2006; Fuller and Collier 2004) and Isabelle Stengers (2000). That is to say, numerous scholars have argued that how and by whom science is organised and funded makes a difference to what kind of knowledge it produces (Anderson 2011; Fuller 1987; 1996b; 2002; Haraway 1997b; Kitcher 2001; Longino 2013; Mirowski and Sent 2002; Zamora Bonilla 2012). However, a critique of the corrosive influence of biases, prejudice and commercial appropriation presupposes a prescriptive approach to science that goes beyond the practical viability of the products a commercialised brand of science generates, the political climates it reflects (society > science) and the realities which it enables (science > society). It presupposes a particular way of doing science and draws our attention to the deficiencies and flaws of examples that fall short of such principles.

The above would be a cogent line of attack if science had a kernel that could be hijacked, perverted and twisted to serve someone's needs. That is to say, if there were a binding scientific ethos (e.g. Merton [1942; 1968]) which committed scientists to certain forms of communication, organisation and distribution of epistemic benefits, a critique would seem to be forthcoming. For example, you could expose claims to scientific objectivity or moral neutrality by (i) showing how they fail to pass tests and live up to standards the practitioners themselves endorse and (ii) argue that actual scientific practice is not congruent with the self-understanding that institutions have of themselves. It would, therefore, be a rhetorical move akin to immanent critique. If such a kernel is lacking (and there is considerable evidence to that effect) and we recognise that science is a diffuse sociomaterial practice, rather than a set of principles, the argument becomes more complicated to make. For example, we can say that commercialisation and commodification have undesirable political consequences (e.g. economic inequalities lead to health inequalities [Wilkinson and Pickett 2010]). In other words, particular kinds of funding mechanisms complicate the implementation of prevalent democratic principles. Unfortunately we cannot categorically state that this is corrosive of science, because it is not clear what science is and what are its social functions. The approach taken by Isabelle Stengers overcomes this epistemological problem by reframing the question in ontological terms. The kind of constructivism associated with actor-network theory, however, cannot benefit from this turn without an account of the agential capabilities of humans and non-humans before they form connections and establish networks. As it

stands, we have to be content with durability and success.

Conclusions

Science studies scholars have shown that science consistently deviates from the accounts of traditional philosophy of science. Their research has illustrated that scientific practice is considerably more complex and indeterminate than philosophical reconstructions would seem to suggest. These insights are invaluable and promote a responsible approach to science that takes note of its social consequences – both positive and negative. Among the most effective and philosophically intricate of science studies' responses to this issue is heterogeneous constructivism, whose key strength lies in its non-representational view of scientific knowledge and the rejection of sociological reductionism that seemed to plague earlier attempts to study science empirically.

An unintended result, however, is that science becomes a descriptive term designating a social institution, rather than a set of principles characteristic of sound epistemic conduct. This is an institution that is tied to particular examples of it, rather than predefined social or cognitive goals. Furthermore, the sources of its prestige are actually different than popular beliefs would suggest. This is a particularly problematic turn for constructivist branches of science studies that subscribe to relational, process-based or performative metaphysics (e.g. ANT). They have trouble mounting a critique of instances where the integrity of a particular piece of science seems to be suspect, because there is no predefined boundary between science and society and the entities that populate it. Such boundaries and abilities to act, and be acted upon, are the result, rather than a point of departure. If these arguments were only illustrative of how science works, there would be no need to concern oneself with normative questions. Constructivists could content themselves with criteria that reduce science to its enactment in science studies texts (e.g. Fuller 2002: 188-189). However, their projects often have ethical and political implications and sometimes even contain explicit criticism (Collin 2011; Radder 1992; 1998a; 1998b).

Such cases present a peculiar predicament. On the one hand, because the boundary between science/politics and argument/rhetoric has been dissolved, political criticisms are also criticisms of science, and only once the debate has been resolved can we distinguish between science and politics. On the other hand, constructivists cannot challenge entrenched views and practices on the grounds that they are unscientific (either

in spirit or content) or show signs of political or economic interference. Sociology in the Soviet Union is an excellent example of this.

3. Epistemic Ambivalence and Uneasy Alliances: Sociology in the Soviet Union

Many aspects of Russian science, including whole schools of thought, do indeed reveal the influence of the specific social environment in which they developed. These influences extend, surprisingly, even to “hard” sciences such as physics and mathematics. Other developments in Russian science equally convincingly show that science is not entirely a social construction, but that it does indeed have a relationship to objective reality, and this relationship to reality is the reason science in Russia is far more similar to science elsewhere than is Russian philosophy or literature to those disciplines elsewhere.

(Graham 1997: 470)

As I tried to show in Chapters 1 and 2, science studies has provided numerous examples that illustrate the multifaceted entanglements of science and society, and show that the sciences are complex sociomaterial practices that are situated in particular configurations of ambient values, social philosophies and political climates. The historian of Soviet science Loren Graham (1997; 1998), however, poses an interesting question to scholars with constructivist inclinations: if the sociopolitical context plays such a significant role in the development of scientific knowledge, how is it that numerous scientific projects with a distinctly Soviet political and philosophical underpinning failed? That is to say, if the idealistic assumptions underlying constructivism (as Graham sees it) were true, science would develop differently in different contexts and the materiality of the world would offer little, if any, resistance to the inquisitiveness and creativity of human scientists. To be sure, Graham is perfectly comfortable with the claim that cultural beliefs and political factors contribute to and mould the specific form that science takes, but a kind of qualified realism seems implicit in his writings and overall stance towards matters epistemological (e.g. Graham 1987; 1994; 1998). In other words, Graham rejects social constructivism, but he is prepared to accept an attenuated form of it – one that emphasises the social embeddedness of science, but does not reduce science to its social conditions of emergence.

In the previous chapter I argued that certain branches of constructivist science studies (e.g. actor-network theory) shared Graham's suspicions as to the validity of purely sociological explanations of science. However, this was complemented by a difficulty articulating criticisms of science and its social functions in a way that would allow one to conceive of and identify instances where a particular piece of research or an entire scientific discipline has been compromised. Science in the Soviet Union presents an

interesting challenge to such approaches and illustrates some of the fears that stimulated the development of Merton's account of the scientific tradition. Merton was concerned that science would be constrained in totalitarian societies. Loren Graham, on the other hand, was quite determined to show that even in a totalitarian society such as the Soviet Union the attendant intrusions in science did not manage to excise, though they certainly managed to stifle, scientific creativity. The simple fact that science is always embedded in particular social contexts is not sufficient to refute the claim that science can remain robust even under conditions of significant epistemic stress.

Graham is fully cognisant of the ideological constraints placed upon Soviet science, but he makes a point of drawing attention to numerous instances where Soviet scientists were at the forefront of their respective scientific fields (Graham 1987; 1998). Consequently, Graham makes a few concessions to constructivism but suggests that explanations emphasising the importance of external encroachment (e.g. censorship, ideology) are insufficient to understand the peculiarities of Soviet science. However, what becomes clear after examining Graham's work on Soviet science is that he devotes very little attention to the development of the social sciences (though he does touch upon social psychology [Graham 1987]).

The question that I shall explore in this chapter is whether Soviet sociology followed the same or at least a similar kind of pattern to the one introduced by Graham. I believe that this question is made all the more pertinent given the ideologically sensitive subject matter of sociology. I will begin this chapter by looking at some of the arguments as to the nature of dialectical materialism and its influence on science in the Soviet Union. The overview is necessarily limited and expository by design and it does not aim to capture the variety of interactions between the various scientific disciplines and the officially sanctioned philosophy of science. Its main purpose is to characterise Soviet understandings of, and approaches to, science. The second part is devoted to the fate of sociology as staged in Euro-American publications. My analysis is influenced by the work of Shlapentokh, Shiraev and Carroll (2008) who analysed how the perception of the Soviet Union in Euro-American scholarship changed and evolved from the Russian Revolution of 1917 until the dissolution of the USSR. This chapter, however, will only focus on sociology and explore how scholars working in the West⁷ staged the fate and

⁷ This also includes the work of Soviet émigrés.

attempted to understand the peculiarities of sociology in a totalitarian state. Finally, I consider whether Soviet sociology and sociology in general exhibit characteristics that complicate matters for a constructivist take on science. I conclude that examples like Soviet sociology show that constructivist science studies in general and actor-network theory in particular are ill-equipped to deal with situations where the balance of force is asymmetrical, because there is an implicit assumption that the sciences are strong enough to counterbalance political rhetoric and participate in the construction of common reality – something that may be lacking in the case of sociology.

The Scientific Life of Dialectical Materialism

Although “science” is often perceived as an opposite of “ideology,” in the Soviet case the language of science proved inextricably linked with newspeak.

(Gerovitch 2002: 26)

According to Loren Graham, dialectical materialism has been both neglected and vilified outside the Soviet Union. The reason for this seems obvious. It is a philosophy that, in the eyes of many, stunted scientific development and let (pseudo)scientific theories, such as those of Trofim Lysenko, establish themselves (Pollock 2006). In other words, it was a debilitating philosophy of science that was responsible for the failures of Soviet science. For example, it has been argued that dialectical materialism rapidly devolved from a scientific meta-theory (however contentious) to a pernicious ideology that defined scientific validity in terms of practical relevance (Feuer 1949). That is to say, relevance was defined in terms of how a particular piece of research relates to practical goals, and research that failed this test and did not have any immediate practical applications was disregarded (and even castigated). Or, as Slava Gerovitch (2002) has argued, dialectical materialism functioned as an integral part of Soviet newspeak, which fused politics, ideology and science (Krementsov 1997; Pollock 2006).

The controversy surrounding Trofim Lysenko is an example of a scientist appropriating a particular philosophy for the purposes of a kind of political pragmatism. Lysenko's approach resonated with the dominant views on science and the political agendas of those at the top of the bureaucratic apparatus. It succeeded at retarding Soviet genetics, which at the time was the forefront of international research before Lysenko's rise to prominence (Graham 1987; 1994: ch. 6). What is more, Lysenko and his supporters did

this by opposing themselves to other approaches that were much more conservative in their claims and not as concerned with practical applications as was Lysenko. Furthermore, as Graham's (1987; 1994) analyses of the historical developments of physiology, psychology, physics and debates pertaining to the question of nature vs. nurture in humans suggest, dialectical materialism did have a profound and often negative effect on scientific disputes. However, as Graham himself argues, the positive effects of this philosophy of science should not be overlooked. That is to say, dialectical materialism should not just be accused of being the cause of all scientific misadventures in the Soviet Union – it should also be credited with the successes of Soviet science. Thus, Graham is arguing for a symmetrical treatment of dialectical materialism that does not regard it as an ideological or philosophical malformation in the corpus of science - a mere ideological appendix with little, if any, epistemic fecundity.

The implications of this are quite intriguing and politically unsettling. Graham is urging us to consider the possibility that a politically sanctioned philosophy of science can be epistemically productive. What is more, he has pointed out that it might be mistaken to think that a certain level of political freedom is a prerequisite for a flourishing scientific culture (Graham 1998). The reason for this is quite simple – Soviet scientists were leading experts in many scientific fields, such as mathematics and theoretical physics (Graham 1992). However, this alone does not seem like enough to establish the scientific relevance and fecundity of dialectical materialism.

Now, Graham himself has pointed out that the history of Soviet science illustrates the need to consider science as a set of material practices pursued by professional groups who were interested in both epistemic and economic rewards (Graham 1998). The work of Slava Gerovitch is interesting in this regard. Gerovitch (2002) has argued that the scientific newspeak provided a kind of ideologically infused linguistic repertoire, which, when skilfully deployed, could be used to frame scientific questions and theoretical positions in a politically sensitive manner. In other words, Gerovitch seems to be suggesting that the theoretical framework afforded by dialectical materialism served as a way to keep up appearances. It does not necessarily mean that science suffered. Rather, survival in this environment depended on how rhetorically nimble you were with the conceptual tools at your disposal.

This seems congruent with David Holloway's (1974) analysis of the formative years of

Soviet cybernetics. In his paper Holloway suggests that in the early years the struggle between the critics of cybernetics (who saw it as an expression of bourgeois idealism with obtrusive capitalist overtones) and its defenders involved the deployment of “techniques of persuasive argumentation”. These techniques were the tools with which the opponents fought over who had the right to pass judgement on scientific innovations. However, Holloway differs from Gerovitch in that he seems to be working with at least an operational distinction between science and politics. That is to say, Gerovitch's newspeak merged scientific, political and philosophical discourses. Holloway, on the other hand, seems to be suggesting that, while scientists had to take part in philosophical debates (and thus engage in a fair bit of newspeak), the overall aim was to move the locus of recognition to the scientific community; even though, as Holloway is quick to point out, a clear-cut distinction between the inside and the outside of science is problematic.

Kuhn speaks of 'the techniques of persuasive argumentation effective within the quite special groups that constitute the community of scientists'. The arguments about cybernetics in the Soviet Union indicate that it may not always be clear who those quite special groups are; nor what techniques of persuasive argumentation are effective within them.

(Holloway 1974: 335-336)

What both Holloway and Gerovitch have in common is a discursivist attitude towards dialectical materialism, whereas Loren Graham is a realist. In other words, Holloway and Gerovitch are much more concerned with the public persona of dialectical materialism. They are implicitly agnostic as to its cognitive import. Graham, on the other hand, is attempting to persuade us that dialectical materialism also had a positive and productive effect upon scientific inquiry in the Soviet Union (Graham 1987). Graham is, therefore, trying to dissociate facets of dialectical materialism which are (or at least have the potential to be) of genuine interest to scientists from their ideological trappings. The difference between the approach of Graham and that of Holloway and Gerovitch is an excellent illustration of the ambiguity surrounding the impact that external factors play in the development of scientific knowledge – an issue of great interest to a discipline called *naukovedenie*, which can be regarded as the Soviet variant of science studies.

Science as an Object of Study: Naukovedenie

As I mentioned above, Soviet philosophy of science recognised two very important aspects of scientific work: (i) the problematic nature of perception/cognition and (ii) the interrelationship between science, practical goals and needs (Lubrano 1976). Soviet

approaches to the history of science in the period preceding World War II seem to have taken these two problems quite seriously. At first glance this only seems natural, as it would appear quite reasonable that Marxist scholars would seek to locate scientific developments in a socioeconomic context and analyse the attendant implications for our understanding of science and scientific progress. For example, David Joravsky (1955) argued that early Soviet approaches to the history of science were dominated by a kind of vulgar sociology. However, as Alexander Vucinich (1982) has shown, Soviet historians of science were not immune to the internalism/externalism debate alluded to in the previous section and were aware of the attendant complexities (Josephson 1985; Lubrano 1976).

The tradition of reconstructing the development of Western science in a manner more reminiscent of classical philosophy of science stretches all the way back to the beginnings of Soviet history of science (Graham 1994: ch. 7). Early work in historical reconstruction was characterised by an emphasis on the argumentative repertoires deployed in scientific debates. This was the cause of significant philosophical problems. For example, Vladimir Vernadskii (the first head of the Commission on the History of Knowledge⁸) argued that conceptual changes in science often resulted in epistemic discontinuities between the old and the new schools of thought. This contradicted the prevailing interpretations of historical change that regarded revolutionary episodes as expressions of continuity. What is more, Vernadskii also stressed the importance of creative genius, thus pushing the role of the wider sociohistorical context into the background. His, therefore, was an internalist history of science that only paid lip service to external factors. The result of such a policy was that Vernadskii and his school were accused of being idealists (Graham 1994: ch. 7).

Nikolai Bukharin⁹, on the other hand, stressed the socially contingent nature of science. He argued that science should be understood as a social product and therefore could not be separated from its context of emergence. By this he meant the institutional and economic arrangements that constitute science in a particular society, as well as the nature of perception itself (i.e. perception is never pure – it is always mediated). His contemporary Boris Hessen went even further. If Bukharin's views now seem like trite

⁸ A group in the Academy of Sciences devoted to the historical analysis of both the social and natural sciences.

⁹ An important ideologue and one of Stalin's main contenders in the wake of Lenin's death.

banalities for anyone coming from a science studies background, Hessen's arguments often feel like caricatures, but this belies the forceful impact his views had on those who heard his presentation in London in 1931. The main thrust of his paper was that Newton's physics would not have emerged if not for mercantile capitalism and the new technologies this particular economic system required (Graham 1985). Newton's physics, according to Hessen, was an expression of particular historical, economic and class (bourgeoisie) needs and interests. Thus, while Bukharin emphasised the interrelationship between science and society, Hessen seemingly reduced science to society. However, the situation was not as simple as it would appear at first glance.

The Sociohistorical Contingency of Naukovedenie

Boris Hessen's *The Social and Economic Roots of Newton's Principia* is a seminal paper in the history of science that highlights the historical contingency of scientific thought. However, as Loren Graham (1985) has tried to show, the paper itself is the result of a highly specific configuration of academic pursuits and political circumstances. Prior to the conference in London Hessen had gotten himself into trouble by trying to persuade Soviet academics that the ideological content of quantum mechanics and Einstein's special theory of relativity could be separated from their scientific core. In doing so Hessen had attracted a considerable amount of ideological flak. The situation was so grave that the conference in London was regarded as Hessen's last chance to redeem himself in the eyes of the Soviet academic and political establishments. Consequently, his paper on Newton *Principia* can be seen as a pragmatic move in line with the conventions of Gerovitch's newspeak or Holloway's rendering of Kuhn's techniques of persuasive argumentation. In other words, Hessen's emphasis on the social and economic factors that contributed to the development of Newton's theory could be seen as ideological pandering, regardless of how fruitful this move turned out to be for future historians of science. However, Graham suggests an additional reason why Hessen chose this particular line of argument.

In view of Hessen's earlier work, it is possible to read his paper on Newton as a subtle move to vindicate his earlier arguments about the ideological shell and scientific core of a theory (Graham 1985). Newton's theories were undisputed in the Soviet Union. By showing that the emergence of Newton's theories was contingent upon a particular mix of social and economic interests he achieved two goals. Firstly, he redeemed himself in

the eyes of his critics because his argument stressed the importance of the economic base. Secondly, he used a widely accepted and highly regarded episode from the history of science to make the point that the scientific core could be extracted from its ideological shell. The study of science was, therefore, itself just as subject to sociohistorical contingencies (Graham 1985).

Debates such as these constituted the emerging field of *naukovedenie* that approached science as one would any social institution. In the case of science, however, there was an additional pragmatic dimension to this. There was a hope to understand science in a way that would allow for the possibility of planning it according to the principles of a socialist state (Lubrano 1976; Mirsky 1972; Mongili 1998). Science and state were intimately intertwined, as is evidenced by various strands of utopian thought during the first decades of the Soviet Union (Stites 1989) and the prominent role that the engineer played in public debates (Graham 1994). Stalin's purges, however, put an end to such aspirations, as well as the field of *naukovedenie*.

The field was resurrected in the 1960s – about the same time as two other ideologically contentious disciplines (i.e. sociology and cybernetics) that had incurred considerable rhetorical flak (Rabkin 1976). Much like its Euro-American counterpart, practitioners came from various different fields and attempted to integrate approaches from sociology, philosophy, history and other disciplines (Lubrano 1976; Mirsky 1972; Mongili 1998). The interdisciplinary character of *naukovedenie* hampered the articulation of a unified research programme. Nonetheless, it was publicly positioned as a kind of science of science (Aronova 2011), and put to work in the service of the state, especially in relation to the Scientific-Technical Revolution (Lubrano 1974; Mirsky 1972). The Scientific-Technical Revolution¹⁰ was a term used to refer to recent innovations in science and technology, which were believed to presage significant socioeconomic changes (Nystrom 1974; Aronova 2011). Philosophy and the social sciences (sociology and *naukovedenie* among them) were set the task of articulating a plan of action that would allow the socialist economy to adjust to these rapid changes without challenging or inordinately Westernising the Soviet system. What is more, it was believed that the areas of science and technology were key if the Soviet Union were to match the success of its political nemesis (Rabkin 1976). As a consequence, STR scholars developed theories that (i) dealt

¹⁰ Hereafter – STR

with questions pertaining to the relationship between society and technology, and technology transfer from the West and (ii) articulated possible ways in which the coordination and funding of scientific activities would yield better results (Aronova 2011; Lubrano 1976; Rabkin 1976). In addition, STR theorists, contrary to traditional Marxist renderings of social transformation that postulated class-based social dynamics as the main source of change, argued that recent innovations in science and technology had become the most prominent source of societal transformations (Aronova 2011: 188; Lubrano 1976: 43-45). Naukovedenie, therefore, played a role that is not dissimilar to science policy studies in the West. What is more, it was often modelled upon advances in scholarship and management techniques developed in the West (Rabkin 1976). Practitioners of naukovedenie, however, responded to developments that were specific to the Soviet system (i.e. STR), and its political and institutional (i.e. as a field of academic inquiry) legitimacy hinged upon its being regarded as a source of information, analysis and technical advice on managing the course and development of scientific inquiry.

The apolitical character of naukovedenie was crucial. While it was true that naukovedenie was political in the sense that there was an implicit (and mandatory) acceptance of, commitment to, a Marxist vision of social development, its practitioners were expected to refrain from making politically sensitive remarks about topics which seemingly fell outside their immediate areas of expertise. As the ethnographic work of Mongili (1998) at the *Institute for the History of Science and Technology*¹¹ of the *USSR Academy of Sciences* illustrates, this created a rift between those who defined themselves in terms of how useful they were to the current political establishment, and others who saw themselves as being committed to standards of academic excellence. However, the perception of naukovedenie as apolitical was integral to it being recognised as a valid form of inquiry. Curiously enough, this perception may have had something to do with the disciplinary background of most of its practitioners, as an overwhelming majority of them came from the natural or mathematical sciences (Rabkin 1976). Social sciences, such as sociology, were just beginning to re-establish themselves (see below) and there was very little prestige attached to them. The situation was such that, if the social sciences had had a visible presence in naukovedenie, its credibility would have suffered (Rabkin 1976). The combination of these various factors contributed to the specificity of the Soviet

¹¹ An important academic hub for naukovedenie

equivalent to science studies. It may have developed out of similar concerns and raised similar sorts of questions, but *naukovedenie* was somewhat different to Euro-American science studies.

Firstly, it was considerably more applied than its Western equivalent. To understand the full import of this point one has to juxtapose the views of Rabkin (1976) with those of Levin (1984) and Mongili (1998). The latter argue that the Soviet equivalent of science studies was a stunted and academically irrelevant field of inquiry whose achievements pale in comparison to those of scholars in the UK and USA, for example. However, one should note that both Levin and Mongili place the locus of identity squarely in the academic environment. But, as Rabkin, Aronova (2011) and Lubrano (1976) show, the practitioners of *naukovedenie* addressed questions and problems of both philosophical and practical interest to people living in the Soviet Union. Its post-World War II incarnation was concerned with, and animated by, the social and economic implications of STR, and sporadically addressed more general questions pertaining to the nature of scientific knowledge that captured the interest of scholars working in the 1920s. Rabkin's contention that *naukovedenie* is better understood as a form of science policy studies (and, consequently, a subfield of science studies) is telling in this regard, though this would exclude the work done at the Institute for the History of Science and Technology and other theoretically driven research (Aronova 2011; Lubrano 1976; Mirsky 1972). Secondly, as I mentioned earlier, the contingent of academics practising *naukovedenie* was populated mostly by people from the natural and exact sciences, and this was also true of the more philosophically inclined practitioners of *naukovedenie* (Mongili 1998). The reason for this was quite simple – the social sciences had a very limited presence in the Soviet Union.

Soviet Sociology

In my discussion of *naukovedenie* I tried to illustrate the complexities inherent in attempting to understand the relationship between the state and scientific knowledge in the Soviet Union. Dialectical materialism may have been an ideologically sanctioned philosophy of science, but this, as Loren Graham suggested, does not invalidate it or discredit the scientific breakthroughs it facilitated. Similarly, *naukovedenie*, while envisioned as a study of science with potential future applications, had to strike a balance between attempts to optimise scientific research, and theories of science and forms of

organisation that were congruent with the political climate. Soviet sociology, however, fares worse in Euro-American commentary.

According to Alex Simirenko (1967), speculation of a sociological nature was reasonably developed in 19th century Russia. However, during the first decades of the Soviet period the academic liberties of this discipline were radically curtailed. The reason for this seems to have been that it was not clear how regime-friendly this discipline was or could possibly be (Shalin 1990). Nonetheless, some empirical and even theoretical work in the social sciences was done in the 1920s, though this was primarily devoted to ascertaining the theoretical import of historical materialism (Lane 1970; Weinberg 1974).

Historical materialism is an approach to the study of history and society that regards (i) human labour and (ii) the production of the basic means of subsistence as the kernel of human society. The way human labour is organised determines the principles by which members of a society are divided into classes (relations of production). Much like dialectical materialism, historical materialism emphasises the importance of change both in nature and society. In the case of society contradictions are resolved by transitioning to a new way of organising labour and productive forces; though views differ on the mechanics of this process (e.g. is it teleological or subject to social, economic and historical contingencies?). A corollary of this approach is the belief that the mode of production (relations of production in conjunction with the means of production and the human labour operating them – what Karl Marx called the forces of production) shapes the superstructure (the culture, belief systems and modes of social organisation prevalent in a society). Or, to put it another way, being determines consciousness. Hence, a capitalist ideology is more likely to represent the relations of production existing under capitalism in a positive light and protect the interests of those who benefit most from this particular arrangement.

Thus, early forms of Soviet sociology were preoccupied with elucidating the relationship between historical materialism and empirical research. The question was whether historical materialism was a theory of social and historical change or a method with which change could be studied (Lane 1970; Novikov 1982; Vucinich 1974). Under Stalin such discussions were brought to a speedy conclusion and the fate of sociology was decided unequivocally – sociology was a pseudo-science, a vulgar and bourgeois discipline that had no place in a progressive socialist state (Hollander 1978; Nahirny 1958;

Shalin 1980; 1990; Shlapentokh 1987). Sociology, therefore, did not exist in the Soviet Union as an academic discipline until the end of the 1950s, and even then its disciplinary status and relationship to historical materialism was unclear. This is not to say that there was no social research, but it was largely done by ethnographers, whose work was seen as too esoteric to be of any political danger, and literary theorists working within the Formalist tradition (Greenfield 1988; Gerovitch 2002; Weinberg 1974). As regards social theory, it was almost exclusively derived from, or consonant with, historical materialism (Shalin 1980; Simirenko 1973). When it did address developments outside the Soviet Union it was mostly to criticise the reactionary nature of bourgeois sociology and its attempts to protect and perpetuate the interests of the capitalist class by hiding behind the veil of value-free inquiry (Kassof 1965; Labedz 1967b; Nahirny 1958; Novikov 1982; Simirenko 1967a). The reasons for the re-emergence of sociology, however, are not so clear and illustrate a point of disagreement in the literature on Soviet sociology.

Politics, Rhetoric and Academic Interests

By far the most popular explanation among the different alternatives is the political thaw associated with the death of Stalin and the policies of Nikita Khrushchev. In other words, the re-emergence of sociology is associated with a loosening of restrictions and obstacles imposed by politically-minded academic censorship. This view is indirectly supported by similar developments in the fields of economics, cybernetics and *naukovedenie*. However, a sudden (though considerable) loosening of restraints is, at best, a necessary, but not sufficient, condition for the emergence of an entire academic field. A more incisive line of reasoning, adopted by Beliaev and Butorin (1982), suggests that the reasons behind the emergence of sociology are to be articulated in terms of practical needs and various actors working together – sometimes consciously and sometimes unwittingly – to establish sociology as an institutionally recognised form of academic inquiry. The impetus for this was the growing interest in the superstructure and the recognition of the need to study it which became prominent after Stalin's death. The explanatory stress should, therefore, be placed on the aspirations of the Communist Party to employ less coercive and repressive forms of social management, and implement policies that are based on evidence gathered in a systematic and sound manner (e.g. Kolaja 1978; Hollander 1978). This suggests that the causes behind the re-emergence of sociology are rooted in interests that were not strictly speaking intellectual. The rebirth of Soviet sociology is instead connected to the need to manage the production process and

the population without resorting to violence and coercion. However, there is some confusion as to the motivations and involvement of the Communist Party in this process. For example, the preceding argument stresses the importance of instrumental needs, but Beliaev and Butorin (1982) have argued that it is equally plausible that the re-emergence of sociology was a party-directed process, but not necessarily an attempt by the party to develop better methods of management (Beliaev and Butorin 1982: 423). Political assistance, to put it somewhat simplistically, was necessary because a number of factions within the academic establishment refused to recognise sociology as an independent discipline, as this would displace a purely theoretical historical materialist discourse of social change (Novikov 1982). However, it is also true that several members of the Community Party itself were equally ambivalent about sociology (Shlapentokh 1987; Zaslavsky 1977), so obtaining political support was not a straightforward task.

This state of affairs is reminiscent of the work of David Holloway on Soviet cybernetics (Holloway 1974; 1976). In it he argues that a major obstacle to the establishment of a cybernetic research programme in the Soviet Union was a contingent of philosophically minded scholars and bureaucrats whose arsenal consisted mainly of different strategies that highlighted the incompatibilities between cybernetics and dialectical materialism. The fate of sociology was similar. What Beliaev and Butorin (1982; Shlapentokh 1987) seem to be suggesting is that the birth of Soviet sociology was not the result of a recognition of the usefulness of sociology by the political leadership or the loosening of ideological censorship. Instead, the reintroduction of sociology was facilitated by a loosely structured network of academics and politicians who tried to establish a new form of inquiry in a context where there was suspicion from the political and academic establishments. Sociology was not just an ideological problem – it was also a philosophical threat to certain groups within academia.¹² In other words, the question of sociology straddled the line between rhetorical manoeuvring and genuine argument. This becomes clearer when one looks at early attempts to establish it as a discrete academic discipline.

The Early Forms of Soviet Sociology

The *Soviet Sociological Association* was founded in 1956. At that point, however, sociology was not yet an established academic discipline. It had its own association, but it had very

¹² This perspective is congruent with an argument put forward by Lubrano (1993). She contends that Soviet scientists developed complex informal academic networks access to which largely determined whether or not a particular area of research could be pursued.

little else. Firstly, most of its practitioners came from other disciplines (mostly philosophy) and had very little training in, and knowledge of, sociological research methods and theory (Gray 1994; Shalin 1978). Secondly, there was little institutional support for the recruitment and training of new sociologists. And thirdly, it had no academic journal of its own, though sociologists could use existing academic outlets to publish their work¹³. Other than a lack of any discernible institutional foundations, it was also destitute in political and academic credibility, since it was still regarded as the illegitimate and politically subversive offspring of philosophy (Weinberg 1974; Beliaev and Butorin 1982). Shlapentokh (1987: 18), for example, argues that during the 1950s sociologists often had to associate themselves with economists to avoid arousing suspicion.

The solution to these problems was not straightforward, but a prominent strategy was to link up with the objectives and practical needs of the Communist Party and prove that sociologists could provide valuable assistance to those in charge (Simirenko 1967a; Shalin 1978). During the first two decades of Soviet sociology this was especially true in relation to work and productivity (Weinberg 1974), which led George Fischer (1964; 1967b) to argue that Soviet sociology was primarily a sociology of work. In practice this meant that Soviet sociology was characterised by a practical orientation and was seen primarily as a tool for social transformation in line with pre-defined goals and interests. In addition to aligning itself with these interests, sociology also engaged in what has been referred to as “creative debunking” (Simirenko 1967a; Vucinch 1982). That is, a significant portion of theoretical work in Soviet sociology was devoted to criticising Western (bourgeois) sociology. The nature of these criticisms varied. Some were, at best, expressions of ridicule and irony, while others were more conventional academic discussions (Kassof 1965), and, according to Labedz (1967b), the overall tone became much less abusive than under Stalin. The main criticism was that Western sociology was either implicitly or explicitly protecting the class interests of the bourgeoisie. However, similarly harsh remarks were levelled against its atomistic (societies as aggregates of individuals) and idealistic (over-emphasising the role of ideas) tendencies, as well as its internal heterogeneity, which was contrasted with the holistic and homogeneous nature of Soviet sociology (Fischer 1964; 1967a). An interesting case of such a criticism is presented by

¹³ Some of the work was made available to English-speaking audiences through *Soviet Sociology* and *Soviet Review* (now *Russian Social Science Review*).

Alexander Vucinich (1974) in his article on Parsons' functionalism.

The attitude towards structural-functionalism in Soviet sociology was ambivalent. On the one hand, it was criticised for being a theory devoted to protecting the status quo (whereas Soviet sociology was concerned with the dynamics of change). On the other hand, it resonated with the emerging fascination with cybernetics (Gerovitch 2002 and Holloway 1974; 1976) as an approach to rationally managing different kinds of systems (including social and economic ones). In the post-Stalin period the idea that it was possible to centrally manage a socioeconomic system as vast and complicated as the Soviet Union came under attack from economists; even though not all of them favoured a cybernetic approach. Economists offered numerous alternative scenarios which would increase efficiency at the cost of decreasing centralisation (Bernstein 1964; Knox Lovell 1968; Spechler 1970; Sutela 1991 and Zauberman 1963). Parsons' emphasis on functional relations tied in rather nicely with cybernetics and the analysis of the dynamics of system maintenance. The problem was that for Parsons' theory to become acceptable it would have to be integrated with a satisfactory theory of historical change (i.e. historical materialism). Unlike cybernetics (which was problematic from the perspective of dialectical materialism because information was neither matter nor energy), Parsons' theory failed to clear its philosophical hurdle.

However, even though Soviet sociology emphasised homogeneity, it was itself internally divided. Much like during the pre-Stalin period, an important problem was the role of the materialisms (both dialectical and historical). This, however, went beyond the problem of theory-ladenness, and the significance of empirical research itself was problematised and came under scrutiny (Greenfeld 1988; Novikov 1982; Shalin 1978). It was argued that sociology should operate at a higher level of generality and abstraction, and that it should first and foremost be a theoretical discipline. Consequently, during its first two decades sociology in the Soviet Union can be divided into at least three camps. First, we have a group of Soviet philosophers who were opposed to an independent empirical sociology. Historical materialism was the explanatory theory par excellence when it came to social change (Katz 1971; Novikov 1982; Shalin 1978), and an empirical science with dubious social origins could not hope to dislodge it. As I mentioned earlier, many people who regarded themselves sociologists came from other disciplines. This group was in favour of a philosophically inclined sociology that would study general laws of social systems in

line with the basic tenets of historical materialism and opposed the idea of an independent academic discipline (Beliaev and Butorin 1982; Novikov 1982; Zaslavsky 1977). A second group of researchers did not yet see sociology as an independent discipline, so they regarded sociological research as part of general social research. This, I believe, is why Katz's (1971) enthusiastic description of the ubiquitousness of sociological research in the Soviet Union is somewhat inaccurate, as much of what he describes as sociology is mostly low-level data gathering (Shalin 1978; Horowitz 1978). Finally, there was a group of researchers who wanted to pursue a scientific sociology, which, while still working within the framework provided by Marxism, would study regularities specific to Soviet society and seek to refine theoretical principles directly useful to empirical research (Shalin 1978; Shlapentokh 1987).

Sociology and Politics

As I have already hinted above, opinions and emphases differed on the involvement of the Party and its ability and interest to facilitate the institutionalisation of sociology in a hostile academic environment, but there was a general recognition that the institutionalisation of sociology required both political and academic support. However, there is a widespread belief that sociologists were not allowed to remain politically neutral, and commentary on the partisan orientation of Soviet sociology is characterised by widely shared concerns as to the effect this had on the integrity of the discipline and its output. A number of authors have emphasised that the Soviet Union had no equivalent to Western Sociology (e.g. Greenfield 1988: 1991; Labeledz 1967a). That is to say, even though a discipline that went under the name sociology gradually emerged in the Soviet Union, it was a profoundly different kind of activity. For example, it has been argued that most sociological research explicitly or implicitly tested, or (more often) worked within the parameters set by, Marxism (Simirenko 1967b; 1973; Horowitz 1978). In practice this meant that social realities in the Soviet Union had to be interpreted in light of Marxist theories of social change and economic development. This often resulted in an imperative to reinterpret discrepancies present in the data in such a way as to render them compatible with the belief that the Soviet Union was moving towards communism. Another common strategy was to treat truth in a normative and future-oriented manner. That is to say, the line between what is and what could be (for example, the potential of socialism to produce social equality and deploy technology for the benefit of all) was obliterated, with the discourse of what could in principle be the case

under socialism displacing the description of the current state of affairs. The future and the potential inherent in socialism became reality (Marcuse 1967; Nystrom 1974). This, of course, was not always the case, and more moderate assessments are also available.

Some authors point to the recognition on the part of the authorities that the Soviet Union had problems that were either specific to the Soviet Union or resembled difficulties familiar to governments in the West. For example, the attitude of young people towards work was considered problematic, as it contradicted the belief that the Soviet Union was the home of a new kind of person (Katz 1971; Weinberg 1974; Zaslavsky 1977). A more prominent alternative, however, was to treat social problems (e.g. crime, alcoholism, class differences) as vestiges of capitalism or the result of Western propaganda (Weinberg 1992; Katz 1971; Kubat 1961). In view of the above, many authors contend that, in spite of the official claim that empirical research would correct the inadequacies of Marxist social theory, this was seldom the case. Empirical research was consigned to the role of confirming, rather than verifying, elements of the dominant social theory (Brym 1990). In case of blatant discrepancies the quality of the data was questioned, rather than the adequacy of the theory (Shalin 1978).

Now, this may seem like an over-exaggeration. It is true that only in the conference of the *Academy of Social Sciences* in 1967 was it admitted that the Soviet Union had significant social problems that needed to be dealt with. However, as Katz (1971) shows, many politically inconvenient studies were published unscathed. In other words, it would be a mistake to assume that politically contentious sociological research was systematically ignored, explained away or eradicated. Sociology did, indeed, manage to establish itself as a worthy and instrumentally useful form of academic inquiry, which is exemplified by the establishment of a sociological research institute in 1968 and a journal in 1974. This, however, brings us to a further problem – even though the usefulness of sociology was recognised, its institutions and publications were periodically subject to politically motivated purges (Beliaev and Butorin 1982; Novikov 1982; Shalin 1978; Shlapentokh 1987; Zaslavsky 1977). A number of critics of early Soviet sociology pointed out that, while the official position with regard to sociology was that it was a kind of fact gathering for policy purposes, the political pressure on this process was overbearing. Thus, while the impetus for fact-gathering may have been practical, the empirical aspect of it was questionable, for, as I mentioned above, discrepancies were explained away with relative

ease. The central theme of the criticisms made by Western scholars was that, in the case of Soviet sociology, science and politics were fused together (Hollander 1967; Fischer 1964; 1967a). Yes, the state wanted a better understanding of the dynamics of social processes to be able to deal with practical problems. However, sociology was (i) treated simply as a sophisticated tool for social management (ii) constrained both by institutional (the interests of the Communist Party) and conceptual (Marxism) limits.

As I mentioned above, the ideological control and censorship imposed upon sociology was not total, and some politically inconvenient research was allowed to circulate. This point introduces some complexity into the accounts of Soviet sociology because it implies a measure of independence. What is more, even from the perspective of Western scholars, sociology had a liberating effect (however slight) on Soviet society. For example, some authors have argued that public opinion research, while plagued by methodological issues (e.g. White 1964), had positive political effects. The sociologist steadily emerged as a representative of public opinion, and, given the role that sociologists gradually acquired in policy circles, research on public opinion allowed for the possibility of public participation in policy-making (Mickiewicz 1972; Weinberg 1974; 1992). Granted, the questionnaires dealt with politically innocuous topics, but research on public opinion created a channel between the public and policy makers.

One way to make sense of such equivocation is to look at the publications chronologically. Papers and books published in the sixties recognised and discussed the consequences of constraints placed upon sociological research, but also manifested a measure of optimism about the future of sociology (e.g. Lane 1970). This is expressed most clearly by Alex Simirenko.

An important open question is what kind of science Soviet sociology will be. Will it be primarily an administrative arm for fact-gathering and planning? Or will it also be capable of turning itself into a theoretical discipline satisfying intellectual curiosity? None of the answers to these questions can be given with any certainty at this time. However, on the basis of the contributions by Soviet sociologists in the new discipline's short period of revival, the future of Soviet sociology looks very promising.

Simirenko (1969: 42)

Soviet sociology had made enormous strides towards becoming an established academic discipline in a short period of time, so Simirenko's cautious optimism must have seemed well founded. Furthermore, there was also a line of argument that outsiders tended to overestimate the extent to which Euro-American sociology is different from Soviet

sociology (e.g. Brodersen 1967; Dunn 1979a; Gouldner 1970; Kassof 1965)¹⁴. Commentary published in the 1970s, however, manifested a gradual dissipation of optimism about the future of Soviet sociology and a return to a bleak take on the relationship between sociology and the political regime. For example, the Soviet émigré Vladimir Shlapentokh published a history of sociology in the Soviet Union in which he described institutional purges and politics that affected sociology in the 1970s (Shlapentokh 1987). Much like Beliaev and Butorin (1982), Shlapentokh suggests that sociology was given relative freedom only during liberal periods in Soviet history. Most of the time, however, both sociologists and their work were vigilantly monitored and interfered with (e.g. censored, publicly criticised) if that became necessary.

In response to such developments in the Soviet Union, commentators were implicitly and explicitly arguing against political intervention in, and censorship of, scientific inquiry. For example, Hollander (1967) argued that sociology simply could not function properly in a totalitarian state without damaging its scientific integrity and claimed that political freedom was a prerequisite for a scientifically robust form of sociology. This brings us back to the point raised by Loren Graham. Remember that for him the Soviet scientific establishment illustrated the possibility that political freedom was not a necessary condition for a thriving scientific culture (Graham 1998). However, as I mentioned in the introduction, Graham pays little, if any, attention to the fate of the social sciences in the Soviet Union. This would explain why he does not address the pervasive pessimism regarding Soviet sociology.

Greenfield (1988; 1991) is an excellent illustration of the discomfort that outsiders felt. In her writings she explicitly opposed an instrumental form of sociology, whose primary occupation was generating research useful for policy purposes, to a sociology whose ultimate aim is an understanding and description of social reality. Furthermore, while the Soviet emphasis on applied research was deemed problematic, it was complemented by concerns as to the effect that a necessary connection to Marxism had on the autonomy of sociology and its status as a self-regulating form of scientific inquiry. This, however, was not regarded as a problem in the Soviet Union – officially at least. For example, the official position of the journal *Sociological Research* was that sociology was inextricably

¹⁴ I will return to this point later in the chapter, but the underlying assumption was that there was nothing inherently different or unique about the Soviet interpretation of sociology as a research branch of public administration.

intertwined with Marxism-Leninism in its search for solutions to problems faced by the government (Greenfield 1988). What is more, the credibility and value of sociology was based on its partisan nature – sociologists had to be committed to building communism and provide an interpretation of facts from the perspective of the class whose interests coincided with the inexorable trajectory of historical development. Consequently, the fact that *Sociological Research* aligned itself with the needs of the Party and defined objectivity in terms of *partyness* (i.e. a partiality towards the interests of the party) is understandable. Nonetheless, this point did not meet with acceptance among émigrés working in Europe and the US (Novikov 1982; Shalin 1978; 1979; 1980; Shlapentokh 1987; Zaslavsky 1977) and Euro-American commentators (Brym 1990; Greenfeld 1988; 1991; Hollander 1978; Horowitz 1978; Simirenko 1967; 1973). The objections, while varied, focused on the role of data in relation Marxist social theory and the monopoly that the former enjoyed in Soviet sociology.

The nature of the commentary changed again during the period when Mikhail Gorbachev was the General Secretary of the Communist Party of the Soviet Union. It was noted that sociology became much more prominent in public discourse (Gray 1994; Shalin 1990; Weinberg 1992), its flagship journal *Sociological Research* doubled in circulation (Shalin 1990), and a number of issues specific to the socialist state were discussed publicly and were not explained away simply as vestiges of capitalism. Prominent Soviet sociologists became Gorbachev's advisers, and the discipline promoted itself as a tool of *perestroika*¹⁵. This led the likes of Shalin (1990) and Weinberg (1992) to argue that the work of sociologists was a significant contributing factor to perestroika; Shalin (1990) even argued that the ethos of ideology so characteristic of Soviet sociology was finally giving way to the ethos of science.

Two Theses

The response to Soviet sociology presents a somewhat ambiguous answer to Loren Graham's question. It was argued that the development of sociology was impeded and hampered by the institutional and theoretical restrictions placed upon it by the Soviet state and the academic culture of which it was a part. Sociology emerged as something akin to a research branch of public administration and had a very uneasy relationship

¹⁵ A political reform movement in the 1980s whose goal was the restructuring of the Soviet political and economic system.

with the dominant philosophical schools. Its development was uneven and plagued by political uncertainties. What is quite intriguing, however, is that such a context, allegedly, spawned a sociology that was confident about its potential and duty to inform public policy and shape society. Granted, it did not have much say in the matter, but the end result seems slightly baffling. According to the authors I have looked at, this may have been because of a mixture of the applied nature of Soviet sociology, the active role of party officials in managing the course of sociological inquiry, and the partisan stance inherent in Marxism. Thus, while Soviet sociology employed research methods that were quite similar to those of Western sociology, it was distinct from it. In this case, therefore, the political situation and the prevailing philosophical discourse significantly altered the nature and aspirations of a form of inquiry.

Soviet sociology presents an interesting challenge to social studies of science (especially its constructivist branches). Both the methods it employed and the philosophical repertoires it drew upon (e.g. Marxism) are familiar and intelligible to Euro-American audiences, but the fusion of the disciplinary ethos with political commitments strikes scholars as problematic. This ostensibly has to do with traditional expectations of what science ought to be and what values (i.e. cognitive, rather than political) should govern its practice. However, as social studies of science have shown in some detail (see Chapters 1 and 2), scientific integrity and its imperviousness to external influences (be they corrosive or epistemically fruitful) is an inherently problematic and contentious topic if we consider actual examples of science. A possible reason is that the authors commenting on Soviet sociology may not have shared the intuitions and theoretical inclinations of science studies scholars. In fact, they may have been working with a highly idealised variant of sociology that is more in line with pre-Kuhnian philosophy of science. Were they justified in doing this?

We can distinguish between two different ways of stating the problem. The first is that sociology in the Soviet Union was flawed because external interference and the needs of the state directed the focus of sociological research. The problem in this case is the lack of independence (from the Party) and an implicit normative orientation towards preserving the status quo, or improving it in line with a highly specific vision of what society should be like and how it should be organised. The state interfered with sociology insofar as it was made to work with a set of assumptions that could not be challenged.

Sociology was a partisan and politically complicit form of technical inquiry, rather than a self-regulating and politically neutral discipline whose goal was the study of social reality. In other words, the discipline was carefully regulated. I shall refer to this as the *Complicity Thesis* (its target – administrative sociology). Notice, however, that this thesis locates the source of the problem outside of sociology. Sociology should be something, but it is not allowed to achieve its potential and work as it should because the political situation places constraints upon it.

The second thesis is more complicated. It states that sociology in the Soviet Union was flawed because it was set up in such a way as to rule out the possibility of confuting facts (either by way of omission or creative integration). Its theoretical backbone was compromised. This is perfectly compatible with the Complicity Thesis, but introduces an element of alterity. Soviet sociology was not simply repressed. The political context spawned a highly peculiar species of sociology. Power in this case was not repressive, but conducive to the emergence of something new and distinctly Soviet that approached data with a highly specific and circumscribed set of assumptions. What is more, Marxism (officially at least) enjoyed a theoretical monopoly. Let us, therefore, call this the *Monistic Thesis* (its target – monistic sociology). The problem in this scenario is not so much that sociology was forced to serve a particular political regime (though it can be this as well), but that the theoretical constitution of the discipline was flawed.

The claim that these two theses highlight problems specific to Soviet sociology can be challenged, however. A theory of sociology is implicit in both of them. The Complicity Thesis seems to be less demanding, since all that is required to sustain it is that the state not dictate what can and should be researched. The Monistic Thesis, on the other hand, is more complicated because it suggests foul play that compromised the very core of sociology – theoretical eclecticism is a virtue, and the limitations placed upon the range of academic resources that a discipline could draw upon maimed Soviet sociology. The problem is that it is not always clear which thesis is at play. The above is a precise description of a fuzzy and complicated problem, but I believe it is a fair approximation. Nonetheless, while the emphases may vary, we can surmise that the crux of the matter was the instrumental, partisan and theoretically monistic nature of sociology (*the cause*), which had a negative effect upon its integrity and credibility among non-Soviet scholars (*the effect*). What is more, while some reservations were expressed, the implication was that

this was unlike Euro-American sociology. This is not simply a matter of intrusion or external interference, but depends on the relationship that sociology and its practitioners build with publics that are the target audience for their work or the source of their funding. The complicated relationship of the sociological profession and the state makes sociology especially vulnerable to external interference, as the autonomy of sociology depends in part on institutions who may seek to curtail it (Halliday 1992: 15). For Halliday the precarious status of sociology derives from the ambivalence that governments have towards it. The middle part of the 20th century saw a number of states using sociological expertise for their purposes and sociology obliging the demands of its main benefactor – the state (Fridjonsdottir 1991; Halliday and Janowitz 1992; Halsey 2004; King 2007). This, of course, does not prove that sociological expertise was tampered with or co-opted in an intentional way. As Bulmer (1992) argues, matters are seldom so simple as to substantiate the claim that the providers of funding automatically determine the focus of sociological research and the political loyalties of its practitioners (Smelser 1992). The above does, however, have interesting implications for a discussion of the Complicity Thesis.

The history of sociology as a discipline and social data in general appears to suggest that administrative sociology is a prominent theme and a familiar mode of existence for sociology, so it seems quite plausible to argue that the Complicity Thesis does not point to anything that is peculiar to Soviet sociology. Indeed, the response of critical theorists to positivist sociology in the West (e.g. Adorno 2002; Adorno et al. 1976; Habermas 1971) is based on arguments not unlike those levelled against Soviet sociology. Furthermore, as I mentioned above there were authors who argued that a number of the criticisms directed at Soviet sociology were somewhat unfair and based on an idealised conception of sociology at home, especially since a prominent issue was the embeddedness of sociology in administrative apparatuses. The main difference was that in the Soviet Union the discipline was represented by a school that, historically speaking, had constituted the partisan wing of Euro-American sociology. For example, in his history of sociology in the United Kingdom Halsey (2004: 122) laments the politicisation of sociology by Marxists and feminists, and similar sentiments have been expressed in relation to American sociology (Horowitz 1993; Lipset 1994).

At this point we can make sense of the work of Alvin Gouldner and Robert Friedrichs,

both known for their sociology of sociology. They were both arguing against the conception of sociology as value-free inquiry. From the perspective of contemporary debates such a move seems innocuous, but this would ignore the fact that a positivist or Weberian understanding and presentation of the sociological self seems to have been integral to sociology's becoming a dependable ally of the administrative apparatus (Smelser 1992). It was precisely because sociology was not a partisan discipline that it could be trusted and relied upon. The introduction and re-discovery of Marxist theorising, however, jeopardised such a relationship and, according to some, had a destabilising effect vis-à-vis the credibility of sociology. In the Soviet Union, on the other hand, the partisan inclination of Marxism is what made sociology politically and philosophically acceptable.

The Petrification of Sociology: Is Complicity the Wrong Target?

An interesting analysis of Soviet sociology can be found in the work of Alvin Gouldner (1970). While the vast majority of his book is concerned with the development of sociology in the West (USA in particular), he devotes one chapter to the institutionalisation of sociology in the Soviet Union. Gouldner illustrates similarities between the development of sociology in the USA and the Soviet Union. His diagnosis of the situation is strikingly simple – sociology can only function without interference once it has convinced the political establishment that it is either innocuous or implicitly supports the current regime (Gouldner 1970: 470).

Sociology in the UK and USA was an appropriately liberal form of expertise because it was, in principle, value-free and shorn of partisan inclinations. It was used for the purposes of liberal statecraft, but this was because sociological research was value-relevant and pertinent to normatively saturated topics. In the Soviet Union, on the other hand, Marxist sociology was acceptable precisely because it was a partisan discipline. The reasons behind the relationship between sociology and the state may have been different, but in both cases sociology reflected the imagined relationship between individuals and the state and, in principle, facilitated the implementation of policy. Sociology was, therefore, complicit in both cases – the main difference was the political regime under which it lived.

This, I believe, is why the Monistic Thesis cuts deeper – it addresses transgressions against intellectual autonomy and academic freedom. Simply put, the Monistic Thesis

claims that Marxism had ossified and become the dominant school. The criticisms of Soviet sociology that fell under this heading were based on the assumption that the discipline has to be autonomous, tolerate theoretical heterogeneity and introduce alternative and conflicting explanations as needed. In this case the problem was not the politicisation of research but academic censorship and conservatism facilitated by the state. A weak response to Soviet sociology would seem to be forthcoming. That is to say, underlying the criticisms is the notion that sociology has to be autonomous and able to be as eclectic and internally divergent as necessary. This, however, is exactly the point where the specificities of constructivism come into play.

Conceptual Symmetry and Contextual Asymmetry

The conventional wisdom in constructivist science studies in general and actor-network theory in particular is that the relationship between science and society is dynamic. The upshot is that the distinction between science proper and contaminant political matter is difficult to establish and maintain. We are confronted by a material-semiotic mesh where science and society emerge as distinct categories only after the fact; a straightforward conception of autonomy is a dubious notion. The issue with this approach was that it was unclear how one could identify cases of external encroachment upon the integrity of a particular discipline and argue against heteronomous constructions, enactments or articulations of the social and material world. To put it simply, it seems to me that constructivists have dropped categories such as truth or verisimilitude, but in doing so have deprived themselves of a clear point of reference against which to contrast instances of scientific misconduct and external interference. Instead, the weight is placed on ethical and political considerations, and a sensitivity to the realities that scientific constructions enable. On the other hand, one of the main reasons why actor-network theory in particular was attractive was its recognition of the insufficiency of a purely sociological description of science and technology. A common move was to supplement the symmetrical treatment of truth and falsity already present in sociological forms of constructivism with a recognition of non-human agency and a greater attentiveness to the material dimension of scientific practices. While social constructivism focused on intra-science politics, actor-network theory looked at the social and material dynamics of the relationship between science and politics. Approached from this perspective, sociology becomes an interesting and tricky case for the latter form of constructivism

because sociology is a relatively weak discipline with an ambiguous relationship to its object and a pronounced internal heterogeneity. These aspects of sociology, however, suggest possible reasons why a symmetrical treatment of various forces and actors (be they political or scientific) may be problematic.

Carving Out a Niche for Science

In her book *Designs on Nature* Sheila Jasanoff (2005) argues that policy cultures vary among countries. Even countries that are broadly similar culturally have markedly different practices for coping with the influx and determining the quality of knowledge claims. Her work is sensitive to the local specificities of the institutional apparatus that processes knowledge claims, but, I would argue, she simply takes it for granted that the differences between the three countries (USA, UK and Germany) she looks at are to be found in the way they respond to (scientific) knowledge. In other words, she proceeds on the assumption that science is enmeshed in attempts to construct a particular kind of national identity (vis-à-vis the policy apparatus) and is shaped by these local specificities. She does not consider the possibility that expertise may, in fact, be (made) marginal to the debate, and a civic epistemology may go beyond a particular balance between politics and science. It is, I would argue, possible to conceive of a civic epistemology where scientific expertise and research are made irrelevant by, or subordinated to, the exigencies of the political situation. By this I do not mean that knowledge is shaped or distorted by political interests. Rather, I would like to suggest that expertise is simply circumvented – it is not an obligatory point of passage (Callon 1986). That is to say, the nature of the civic epistemology is such that that epistemic practitioners and their contributions can exert influence only sporadically.

A good illustration of my point is a case study done by Bent Flyvbjerg (1998) in which he shows how a particular urban development plan (The Aalborg Project) was thoroughly dependent upon a precarious balance of power between and within various municipal and state authorities, and the local Chamber of Industry and Commerce, which had a long history of getting its way in Aalborg. What is interesting for my purposes, however, is that he shows how a number of studies were made peripheral to the debate on whether or not a particular aspect of the Aalborg Project should be implemented. Research only made an impact when political confrontation was avoided. When the issue under discussion was particularly contentious, “naked power” prevailed. In view of this,

Flyvbjerg suggests that the rationality of power is more deeply rooted historically than the power of rationality. Consequently, the exercise of power has traditions which are much more developed and intricate than the deployment of rationality, and he is unequivocal about the fact that democratic governments need to be aware of this.

Flyvbjerg's invocation of traditions resonates with the cultural specificity of civic epistemologies. However, as I have argued above, the discussion surrounding civic epistemologies is generally restricted to the reception of knowledge claims and the rituals of justification surrounding them. Jasanoff's take on civic epistemology assumes that science is something that has to be and is dealt with by the political establishment. While I am sympathetic to the symmetry and co-construction theses championed by constructivist science studies, I believe that studies such as the one carried out by Flyvbjerg point to a slightly different issue. If science studies scholars are interested in the politics of knowledge, the likes of Flyvbjerg argue that there are situations where knowledge (in this case scientific) first has to be infused in and be able to counterbalance politics. In other words, knowledge does something slightly different in the case of Flyvbjerg.

If we assume that science is a powerful agent that allows for various different enactments and models of social organisation, we are right to be suspicious of those individuals or institutions who obfuscate and refuse to acknowledge that the technical solutions they propose have attendant social consequences. We are justified in politicising knowledge and highlighting the social and political costs of practising science in a certain way. However, in cases where a scientific discipline is destitute in political potency and institutional credibility, its ability to participate in, and have an impact on, a confrontation is severely limited. In short, the first version approaches science from the assumption that knowledge operates in a context where it has agency and actively competes with other considerations. The production and application of knowledge is relatively unconstrained, and the issue that concerns us is which of the available alternatives to pursue so as to build the kind of society we want to live in. The other option is a situation where science is still in a subordinate and institutionally precarious position.

In the latter scenario epistemic practitioners are more like science studies renderings of Robert Boyle (Latour 1993b; Shapin and Schaffer 1985). Instead of simply challenging the practices that constitute good science, the experimentalists challenged existing power

configurations and the way disagreement was managed (see Chapter 1). The experimentalists introduced a fundamentally different form of politics – a politics where matters of fact bear on our values and political aspirations (Latour 1993b; 2004a; Pels 1996; 2003). However, even though Bruno Latour criticised the Strong Programme for granting reality and causal powers to the social while downplaying the importance of non-humans and material artefacts, he made a number of assumptions as to the relationship between knowledge and power that are contentious in the case of sociology.

Revisiting Latourian Symmetry

Dick Pels has argued that Latour's brand of constructivism operates on an assumption of generalised symmetry and co-production where different groups constantly compete with one another (Pels 1996: 2003). One of Pels' main claims is that constructivist science studies are sometimes not clear as to whether their call for symmetry is to be understood in a descriptive or normative sense. In other words, is symmetry a move in the knowledge game or is it instead a description of how knowledge claims evolve and assert themselves? Or maybe both? Even if it is the latter, the problem is that symmetry at a conceptual level may be ill-suited to analyse a situation where the mobilising capacities of individual actors are asymmetrical. In such situations a symmetrical approach will provide a partial and distorted picture of the process because the principle of symmetry will be tasked to deal with a configuration where there is, in point of fact, a glaring asymmetry. An example would be a scholarly dispute in which one player (or tradition) has become dominant and marginalised all the others (Pels 2003: 134). Furthermore, Steve Fuller has suggested that this flaw is characteristic of constructivism as a whole.

Constructivists tend to be insensitive to pre-existent ("structural" or "historical") power relations between the parties to an exchange that may overdetermine the outcome of the ensuing negotiations, as in British imperial encounters with African natives in the 1930s and '40s.

(Fuller 2006b: 39)

Soviet sociology seems to fit this pattern rather well. As I argued above, Marxism's becoming the dominant school within Soviet sociology was the result of competition and negotiation between adversaries with disparate levels of support abilities to mobilise people and arguments. However, Marxism did not emerge victorious after long and complicated disputes between proponents of divergent theoretical positions. It was, to be sure, the result of cooperation between government bureaucrats and a number of scholars, but the negotiations concerned the details and extent to which sociology would

be allowed to develop – there was never any real alternative to Marxism. It would certainly be unfair to prejudge the situation, but it would be equally unwise to ignore the asymmetry between sociologists and the theoretical position supported by the Soviet administrative apparatus and its allies. Sociologists were not well-organised, lacked a clear identity and could not resist heteronomous interests. However, it is not clear that constructivists would assent to the claim that Soviet sociology was compromised in some way, and this is precisely my point. I would suggest that this stems in part from background assumptions that posit a symmetrical relationship between sociology and the ambient political situation. Latour's rendering of the Hobbes-Boyle dispute provides a good illustration of this.

Revisiting the Two Realms

As I mentioned in Chapter 1, Bruno Latour has suggested that attempts to disentangle science (and nature) and politics (and society) are characteristic of a Modern malady that overlooks the ubiquitousness of hybrids that mesh the former and the latter.

Hobbes's State is impotent without science and technology, but Hobbes speaks only of the representation of naked citizens; Boyle's science is impotent without a precise delimitation of the religious, political and scientific spheres, and that is why he makes such an effort to counteract Hobbes's monism.

(Latour 1993b: 27-28)

In other words, politics and science are not distinct, clearly separated spheres of action but exist in a perpetual state of hybridity and co-constitute each other. However, Latour's take on the Hobbes-Boyle dispute betrays an implicit belief that the diffuse and heterogeneous activities of scientists are potent enough to engage with, resist and correct the alliance-building and ideological machinations that are characteristic of the world of politics.

At first this appears innocuous – an attempt to point out the deficiencies of an approach that tries to explain science and nature in terms of interests and alliances between humans (see Chapter 1). The problem with Latour's rendering of the dispute is that, while it offers to highlight the complexity of the conflict, it is inextricably tied to forms of science that are more closely related to the natural sciences. This, I would argue, goes some way towards explaining why Latour believes that the objects of scientific investigation are presumed to be tractable to, and articulated in, semiotic constructions, but essentially irreducible to them (e.g. Latour 1999: ch. 3 and ch. 4). More importantly,

however, the ability to affect the course of scientific disputes is distributed among different agents (humans/non-humans and politicians/scientists), and the resolution is figured as the result of an open-ended struggle between different alliances. This, I believe, is where the argument betrays an equation of science with the natural sciences and relies upon the authority and prestige the latter presently command.

While the bulk of the analysis of Shapin and Schaffer's book explores the various intersecting debates, disputes and alliances in the 17th century, their interest for doing so is animated by concerns that are unequivocally contemporary. The examination of the exchange between Hobbes and Boyle is so potent at animating scholarly interest largely because it explores the relationship between knowledge and power whose contemporary relevance is (i) transposed onto people arguing in the 17th century (Pels 1996; 2003) and (ii) predicated upon a resolution that, in the eyes of the general academic community at least, gave a special place to science. In other words, what makes this book interesting is that Robert Boyle is whom we now identify as the philosopher of science and Thomas Hobbes is regarded as the political philosopher. Constructivists may hold that in actual practice we have forms of knowledge that deal with hybrid questions that mesh political concerns with technical solutions, but the rhetorical potency of scientific arguments and technical proposals is predicated on the distinction between science and politics, and a rejection of hybridity.

In other words, social constructivist accounts of science are largely correct, but science's high epistemic status in contemporary society depends on the principled rejection of such accounts.

(Fuller 2006b: 17)

Indeed, Bruno Latour would probably concur.

The two branches of government that Boyle and Hobbes develop, each on his own side, possess authority only if they are dearly separated.

(Latour 1993b: 27)

Politics is distinct from science, and science is distinct from politics. However, not all forms of scientific knowledge are framed in this way. It is important to note an important aspect of the allegory that Shapin, Schaffer and Latour draw upon to discuss the relationship between power and knowledge – namely, 'The Royal Society was given a royal charter once it was agreed that it would not 'experiment in metaphysical, religious, and political matters' (Fuller 1993: 78). In other words, the story told by Shapin, Schaffer and Latour omits an important detail – the agreement between the Scientists and the

Politicians did not extend to what would now be called the human sciences. What is more, this development has not been kind to the social sciences, which

have often been treated as a joke, a more or less expensive way of discovering the obvious; or as an impenetrable thicket of jargon; or as congenitally indecisive (“on the one hand, on the other”). Many have imagined that, apart perhaps from economics, they are not serious: that, if they are not a joke or a set of neologisms, then they are the last redoubt of an outmoded radical politics.

(Law and Urry 2005: 391)

In the case of the social sciences the possibility of making the distinction between knowledge and politics has been routinely questioned (e.g. Friedrichs 1970; Gouldner 1970).

Conclusions

The response to Soviet sociology suggests that it followed a pattern unlike the one Loren Graham identified in the case of the natural sciences and mathematics, and it presents an interesting challenge to social studies of science (especially its constructivist branches). The methods and philosophical sources it drew upon are familiar and intelligible to Euro-American audiences, but the fusion of the disciplinary ethos with political commitments strikes scholars as problematic. I would argue that this was because the fate of Soviet sociology upsets traditional expectations of what science ought to be and what values should govern its practice. Crafting a response to Soviet sociology from the perspective of constructivist science studies, however, is more complicated.

Constructivism provides a powerful arsenal of analytical tools for describing the processes through which scientific claims become institutionalised, but it has difficulty dealing with (and identifying) asymmetries between different forces and identifying transgressions against the logic of a particular discipline. Soviet sociology is an excellent example of this. True to its Marxist heritage, it was partisan in inclination and explicit about its political ideals and aspirations, but a number of outside commentators raised concerns as to the debilitating effect that this had on sociology and the quality of the knowledge that it could produce. Their objections were not based solely on concerns as to whose interests sociologists served (Complicity Thesis) and addressed the epistemic integrity of the sociological enterprise (Monistic Thesis). However, constructivism cannot identify with such a diagnosis of Soviet sociology, because the latter rests on assumptions about scientific autonomy that do not sit well with the constructivist theory of science – actor-network theory in particular. One of the reasons for this tension is that

constructivism is based on assumptions that make it ill-equipped to deal with situations where the balance of force is asymmetrical. That is to say, constructivism can analyse configurations where there are obvious discrepancies between the resources, mobilising capabilities and agential capacities of individual agents, but there is an implicit assumption that the sciences are strong enough to counterbalance political rhetoric and participate in the construction of common reality. What kind of theory of sociology could justify the objections I looked at in this chapter? Most importantly, can such a theory be compatible with the insights provided by constructivist science studies? I believe that the work of Pierre Bourdieu goes some way towards providing us with the necessary tools to address this issue.

4. A Discipline that Makes Trouble (for Itself)

In Chapter 3 I argued that constructivist discussions of symmetry were insufficiently attuned to the specificity of the human sciences. As I suggested in the Introduction, this is not an isolated problem and illustrates a more general issue pertaining to the relationship between science studies and disciplines like sociology. In particular, it concerns the fact that the “science” in science studies is usually one of the canonical scientific disciplines (e.g. physics) or has the potential to produce technological artefacts that alter the sociomaterial landscape (e.g. vaccines, cars, television sets). In response to this, some doubts have been raised whether, for example, the distinction between Mode 1 knowledge and Mode 2 knowledge¹⁶ can be applied to sociology (Wittrock 2003). Bjorn Wittrock, for example, argues that this distinction does not apply itself easily to the institutional trajectory of sociology. In other words, there is something about sociology as a discipline that does not conform to the narrative put forward by science studies. I believe that the previous chapter illustrated this quite well. My point was to show that the peculiarity of Soviet sociology derived from its intellectual and institutional fragility, rather than the fact that it served the political regime and focused on applied research. What is more, this was figured as a problem. However, such a diagnosis did not sit well with a constructivist understanding of science, because the latter implicitly assumed that the authority that science possesses can offset attempts to mangle it.

The work of Pierre Bourdieu becomes relevant at this point. It combines a constructivist theory of science with a thorough discussion of the specificity of sociology and addresses the attendant relationship between power and intellectual autonomy. Bourdieu's diagnosis is particularly interesting in view of the fact that his approach runs counter to the sentiments expressed by actor-network theory in general and Bruno Latour in particular (Fuller 2000b: 16).

In this chapter I will outline Bourdieu's theory of sociology and highlight a number of its advantages and flaws. In view of the fact that my target is the theory of science provided

¹⁶ Mode 2 refers to the way scientific knowledge has been produced in recent times. The key characteristic of this mode is that teams of researchers are brought together for short periods of time (e.g. the duration of a project) and are generally interdisciplinary in composition. This is contrasted with so-called Mode 1 knowledge production that focuses on fundamental, rather than applied, research and is generally organised around discrete disciplines (Gibbons et al. 1994).

by Bruno Latour, I will proceed by using the chapter “Science's Blood Flow” from *Pandora's Hope* (Latour 1999) as a point of reference. I will then move on to discuss Bourdieu's theory of science in general and sociology in particular. This chapter argues that Bourdieu's brand of constructivism highlights a number of issues specific to the science of sociology, but constructivist science studies glosses over certain integral aspects of sociological practice and self-perception which may be damaging vis-à-vis the latter's quest for institutional and social recognition.

The Four Loops

In his book *Pandora's Hope* Latour provides a kind of framework against which to understand science studies attempts to explain science. He eschews the dichotomy that opposes internal and external factors, and suggests that science studies would be better off giving up on the idea of a conceptual core. Scholars should instead approach science as a set of interconnected loops, and, while Latour's discussion would suggest that all the elements have to work together, his diagram (Latour 1999: 100) illustrates that one can analytically disaggregate the dimensions or loops.

The first loop is the mobilisation of the world, which refers to 'all the means by which non-humans are progressively loaded into discourse' (Latour 1999: 99). Now, Latour immediately qualifies this by saying that each science does this differently, and it appears that the social sciences have to make do with surveys, whereas other disciplines can rely on all sorts of instruments and equipment (e.g. microscopes). A prime example of what Latour's approach implies is illustrated by the work of Alain Desrosieres, who, in exploring questions pertaining to the use of statistics by states, analyses the creation of social taxonomies and subsequent deployment of statistical categories for the purposes of administrative record-keeping (1991; 1998). In general, mobilisation refers to the methods that scientists use to practice science and gather information about the world. However, Latour is quick to point out that mobilisation also pertains to the objects that the scientist seeks to articulate in her work. Again, he makes a passing reference to society and economy as potential objects for the social sciences (Latour 1999: 99).

This leads us to the second dimension – autonomy, whereby a discipline 'becomes independent and forms its own criteria of evaluation and relevance' (Latour 1999: 102). This, I believe, illustrates a problem I raised in the closing argument of the previous chapter. I argued that the brand of constructivism exemplified by Latour seemed to be

better suited to understanding the vicissitudes of the natural sciences largely because of their institutional strength and the ability of natural scientists to hold their own against other agents without resorting to the use of force. Granted, this is an achievement, and the relative independence of the natural sciences should be construed as the effect of autonomisation, rather than its cause. That is to say, the natural sciences have successfully carved out a space of action in which the opinions of fellow scientists are valued over and above those of outsiders (activists, politicians or entrepreneurs), but there was nothing inevitable about this. What is more, this state of affairs is, in general, believed to be desirable – hence the suspicions directed towards science studies scholars who consistently illustrate how such claims to autonomy are both historically and situationally variable. The ability of sociologists to impose their own criteria of evaluation and relevance is a more contentious issue. Indeed, the prominent examples of social science among science studies scholars seem to be those with a pronounced mathematical dimension (e.g. Callon 2009; Law 2009; MacKenzie 2006). As Theodore Porter's (1996) work has suggested, this may have to do with that trust that numbers inspire; or, if it is, in fact, based on a trust in the social sciences, it has more to do with the characteristics they share with natural science (Smelser 1992). Furthermore, claims to autonomy are problematic for the social sciences exactly because of the third loop in Latour's account – alliance-building, or the attraction of interest from heterogeneous actors.

As Latour is quick to point out,

[t]he alliances do not pervert the pure flow of scientific information but are what makes this blood flow much faster and with a much higher pulse rate.

(Latour 1999: 104)

With the exception of political science, Latour's examples come from the natural sciences. While this may have been a decision motivated by rhetorical expediency, I believe that the issue runs deeper. As I showed in Chapters 3, the close association of sociology and the state was a source of great concern and a topic of great interest for sociologists and academics commenting upon the history of sociology. The issue was not just externally defined research questions, but the effect that an alliance with the state had on the integrity of the sociological enterprise. Furthermore, the case of Soviet sociology in particular illustrated a further complication.

Latour seems to be suggesting that autonomy and alliance-building are both important components of science. My discussion of Soviet sociology also indicated that alliances

had to be forged in order to make possible a situation where sociologists could practice their craft in the way they saw fit. Disciplinary autonomy had to be earned by convincing philosophers that sociology was a legitimate alternative (or supplement) to historical materialism, and co-operation with the political establishment was equally important. However, the autonomy of sociology, such as it was, could be curtailed with impunity if sociologists could not maintain a co-operative relationship with other actors (be they other academics, politicians or bureaucrats). The independence of sociology was precarious and restricted at moments when the allegiance of sociologists was in question. As I tried to show in Chapter 4, it was not so much the alliance-building that was peculiar to the Soviet version of sociology but the impact this had on the autonomy of intellectual work. This illustrated my problem with actor-network theory – it is insufficiently sensitive to structural conditions that over-determine the outcome of a debate or confrontation between a scientific discipline and the environment in which it is embedded. Furthermore, Latour's instrumental take on knowledge seems ill at ease with the idea that something can work and yet still be counter to what a discipline should be (see Chapter 2). The criticisms directed against Soviet sociology, however, work with, and elaborate upon, the assumption that Soviet sociology was, in many ways, mangled. Can Bourdieu's theory of science provide us with the tools to address these issues? This is a question I turn to next.

The Sociologist and Her Object

Pierre Bourdieu's sociology is widely renowned for its orientation towards, and an emphasis upon, the practical dimensions of social life as mediated by embodied dispositions. This is exemplified by his frequent use of *habitus* (Bourdieu 1984) – a concept that supplements discursive notions of socialisation with the internalisation of social structures and norms at the somatic level. Not coincidentally, a number of his criticisms against contemporary sociology are rooted in his belief that sociologists have a tenuous and inadequate appreciation of the practical and intuitive dimensions of social life which have been ignored in favour of the discursive and intellectual aspects (Bourdieu 1990a; 1990b; 1993).

This malady is characteristic of all intellectuals (including sociologists). Bourdieu's diagnosis is that intellectuals have simply generalised their relation to, and experience of, the world. What is more, they are unaware of the conditions that structure and make

possible their perceptions of the world. They have forgotten

the collective history that has produced our categories of thought, and the individual history through which they have been inculcated in us.

(Bourdieu 2000: 9)

While intellectuals are capable of articulating the principles that structure the perceptions of their subjects, they appear to be oblivious to the fact that they possess a habitus which constitutes, and is constituted by, a particular position in, and experience of, the broader social field. This has serious implications for the accounts of the social world that social scientists produce because they tend to project their own relationship to the world onto their subjects.

Projecting his [sic] theoretical thinking into the heads of acting agents, the researcher presents the world as he thinks it (that is, as an object of contemplation, a representation, a spectacle) as if it were the world as it presents itself to those who do not have the leisure (or the desire) to withdraw from it in order to think it.

(Bourdieu 2000: 51)

This prevents researchers from gaining a clear and accurate understanding of the actual mechanisms at play, which are seldom governed by conscious reflection on whether an action or utterance is appropriate in a particular setting. Furthermore, this blinds researchers to the constitutive role that their training plays in studying social reality.

Constructing the Object

Much like Latour, Pierre Bourdieu sees the object of sociological research as a construction (Bourdieu 2000; 2004; Bourdieu et al. 1991) and believes that the object of scientific inquiry does not present itself to the untrained eye – it is shaped by the tools available to the scientist. What is more, she can only identify the object once she has become accustomed to the rules that regulate the scientific game and the categories of perception that her tradition licenses. However, unlike Thomas Kuhn, Pierre Bourdieu does not believe that scientific culture is characterised by consensus and puzzle-solving. Instead, he treats scientific games as struggles in which a number of different actors and perspectives compete over how a particular phenomenon will be represented.

[W]hat clashes in the field are competing social constructions, representations (with all that this word implies of theatrical presentation, 'staging'), but realistic representations, which claim to be grounded in a 'reality' endowed with all the means of imposing its verdict through the arsenal of methods, instruments and experimental techniques collectively accumulated and implemented.

(Bourdieu 2000: 113)

Sociologists are involved in this struggle in a number of different ways. Firstly, there is

the linguistic dimension. Bourdieu sees language as an important part of constituting the object of sociological inquiry and social reality more generally. However, Bourdieu also believes that the complexity and alien nature of sociological language is key to sociology being able to claim parts of reality for itself. This move reveals a colonising tendency in Bourdieu's sociology. It flies in the face of the belief that the proliferation of scientific disciplines reflects our growing awareness of reality's complexity. Instead, it offers a picture of science as a breeding ground for competitive groups who claim parts of reality for themselves by struggling with other contenders (Fuller 2006b: 48; Whitley 2001). To use Latour's terminology, Bourdieu discusses the relationship between the mobilisation of the world and the autonomisation of the discipline in tandem. This view, of course, is hardly surprising to a practitioner of science studies. What makes it special is that Bourdieu has integrated what has traditionally been an outsider's perspective into his theory of sociology. By redescribing an object, sociology claims it for itself and frees it from common and simplistic ways of seeing it – sociology imposes its own description upon the object. This makes it sound like a purely political gesture. However, while the process may be political (and remarkably similar to early actor-network theory), the reasons for doing so reveal a concern for scientific adequacy.

The Importance of Distance

The sociologist qua member of society is already familiar with the object of sociological research. This is a source of great concern for Bourdieu (e.g. Bourdieu et al. 1991: 13). There is a danger that the sociologist will resort to a spontaneous sociology that comes naturally as a result of her proximity to the object. That is to say, the spontaneous sociologist will simply reproduce common social categories and distinctions in her own discourse – there will be no difference between proper sociology and pandering folk sociology. Consequently, familiarity with their object (albeit in a sprawling and unconstituted form) is more of a problem than advantage to the social sciences. Sociological language, and the interpretive grid that it provides, alters the relationship that a sociologist has to her object. The subjects of sociological research, on the other hand, filter reality through markedly different categories of perception, which suggests that there is a clear distinction between sociologists and the people they study. Moreover, sociologists should be aware of this distinction (see above) and bear in mind that what they are engaged in is, in fact, a constructive process that is aided by research methods and intricate theoretical structures, and conditioned by their position in social space. This

brings us to the second point.

Much like Max Weber, Bourdieu is keen to show that the objects of sociology are scholarly constructions. Weber used ideal-types, but he urged his readers to always bear in mind that ideal-types are abstractions that are distinct from real objects (Weber 1949). They serve as points of reference against which to calibrate accounts of actual phenomena. In a similar vein, Bourdieu recognises the importance of interpretive grids and the ways they focus our attention. The experience this allows us is not a direct one. The scholarly gaze is mediated by theoretical constructions. Sociologists internalise constellations of concepts and theoretical insights, but they should at all times be aware of this process and remain vigilant so as not to fall into dogmatism (Bourdieu et al. 1991: Bourdieu 2000). What Bourdieu is arguing for is, in fact, reflexivity – a practice and principle that is quite prevalent in contemporary sociology. However, Bourdieu's brand of reflexivity is not particularly concerned with sociologists qua individuals (unlike Gouldner 1970). The object of reflexivity is the entire conceptual apparatus of the discipline (Wacquant 1992: 40). This has implications for the quality of scholarly work.

By endeavouring to intensify awareness of the limits that thought owes to its social conditions of production and to destroy the illusion of the absence of limits or of freedom from all determinations which leaves thought defenceless against these determinations, it aims to offer the possibility of a real freedom with respect to the determinations that it reveals.

(Bourdieu 2000: 121)

In the case of Bourdieu, however, this argument has implications that extend beyond the academic realm – the purpose of good sociology extends beyond self-reflection and bears directly on what Latour called alliance-building.

Vexing Administrators

The social and political conditions that allow for the possibility of sociological knowledge point towards a tension that is quite revealing of Pierre Bourdieu's views. Let us go back to Emile Durkheim. He tried to establish sociology as (i) an independent academic discipline with its own subject matter and (ii) a politically engaged profession. Max Weber, on the other hand, believed that sociologists should internalise the fact-value dichotomy and tread carefully on politically sensitive topics (Weber 1949). The hope was that such a compromise would persuade politicians to grant and respect the autonomy of sociological discourse. Pierre Bourdieu appears to be following a similar line of reasoning when discussing the institutionalisation of sociology, but there is a pragmatic twist to his

take on Weber's compromise.

[F]rom the very beginning, sociology has been an ambiguous, dual, masked science; one that had to conceal and renounce its own nature as a political science in order to gain acceptance as an academic science.

(Bourdieu 1993: 27-28)

Now, depending on how you read Weber, the need to separate science and politics was (i) a compromise necessitated by the institutionalisation of scientific inquiry or (ii) the result of accepting that knowledge of the world prescribes no definite way of being and acting in it. Bourdieu's approach seems closer to the former. In other words, the commitment to value-neutrality was a necessary concession. Sociology had to renounce its political inclinations to establish itself. It had to conceal its true identity as a discipline whose subject matter is highly sensitive and controversial. Unlike other scientific disciplines, such as physics or chemistry, the stuff of sociological research bears directly on our everyday experiences and mundane practices. However, this does not automatically suggest that there is something inherently subversive about sociology. Max Weber, the champion of value-neutral sociology, recognised that research in the cultural and social sciences was politically pertinent, but there was a sense in which its pertinence was unclear. That is to say, the findings of a sociologist tell us something about the trends, beliefs or practices she has investigated, but it is not clear that this knowledge has straightforward and definite normative implications without a political context and will to act upon this knowledge. In fact, Bourdieu's reasoning seems closer to Emile Durkheim.

A significant difference between Durkheim and Weber was that the former was quite comfortable with the idea that descriptive knowledge can bear directly on normative reflections (Durkheim 2010; Turner 1993). More precisely, it could actually have definite prescriptive implications. While this is more implicit in Bourdieu's case, he seems to be working with similar assumptions (Pels 2003: 116). However, unlike Durkheim, Bourdieu does not seem to be comfortable with integrating sociological knowledge with administrative projects (Swartz 2003). So, while he is fine with a dynamic relationship between sociological knowledge and forms of normative reasoning, he does not see sociology as an instrument that could or would allow government bureaucrats to be more effective at managing society.

Why the reticence? Well, according to Bourdieu, there is something inherently subversive and problematic about sociology. It is a discipline that makes trouble and reveals things

that have thus far been hidden from view. In fact, Bourdieu even went so far as to say that the scientific success of sociology can be measured in terms of how much it vexes the powers that be.

[T]he likelihood that sociology will disappoint or vex the powers that be rises to the extent that it successfully fulfils its strictly scientific function.

(Bourdieu 1993: 14)

Sociology, therefore, is a strange and subversive discipline whose default relationship with the administrative apparatus is ambivalent. However, it is important to note that Bourdieu did not believe that there was an inevitability to sociology being subversive. This is where a comparison with Critical Theory (Theodor Adorno and Max Horkheimer in particular) is instructive.

Theodor Adorno chastised traditional sociology and its positivist leanings. He believed that positivism made sociology susceptible to being integrated into a technocratic apparatus whose main goal was the perpetuation of the established order (Adorno et al 1976; Adorno 2002; Benzer 2011). In the previous paragraph I argued that Pierre Bourdieu shared Adorno's suspicion towards administrative sociology. The reasons for this are slightly different, however. Adorno's objections stem from his commitment to a specific relationship between knowledge and its object. Administrative sociology is undesirable because it (i) is insufficiently attuned to the complexities of its object, (ii) conceals its knowledge-constitutive interests by appealing to disinterestedness and (iii) does not aim at emancipation. It is conservative and induces social and epistemic stasis – a common criticism running through the work of critical theorists. This is not a problem that is particular to sociology and can be identified in most forms of traditional scientific inquiry (Horkheimer 1982).

For Bourdieu, on the other hand, the problem has to do with the nature of sociology as a discipline, rather than science in general. The knowledge you can produce about the social world depends on your relation to the social world; if your needs and position are purely administrative, the knowledge you produce will reflect this.

[T]heir interests are bound up with silence because they have no bones to pick with the world they dominate, which consequently appears to them as self-evident, a world that goes without saying. In other words, I repeat, the type of social science that one can do depends on the relationship one has to the social world, and therefore on the position one occupies within that world.

(Bourdieu 1993: 13)

In this Pierre Bourdieu is quite similar to Jurgen Habermas who argued that different sciences embody different knowledge-constitutive interests, which means that they are constituted in such a way as to be useful for specific purposes (Habermas 1971). Bourdieu's approach, however, allows for more diffuse knowledge-constitutive interests determined by the position of individuals or groups within social space. Sociology certainly has a mode of existence as a research branch of public administration, so there is little problem identifying forms of European sociology to which the Complicity Thesis would apply equally well. The issue for Bourdieu is that sociology practised in this way is somehow flawed. It is not true to what sociology should be like. The people who engage in this type of sociology legitimise the established order by providing politicians with research and the authority of independent expertise (Bourdieu 1993: 2000). True sociology, however, should strive to unveil hidden aspects of social experience, make visible the necessity that conditions the lives of individuals, and analyse the struggles that are rooted in the very fabric of social life (Bourdieu 1990a; 1993; 2008). The ultimate aim of this exercise is freedom through recognition of necessity and a subsequent intervention in the mechanisms that reproduce necessity (Bourdieu 1993; 2008). In other words, sociology should attempt to alter the existing situation, rather than caress or perpetuate it.

Thus far there is no glaring discrepancy between Bourdieu and Critical Theory. Both see knowledge as implicated in social struggles and both argue that sociology has a critical function to play in these confrontations. Consequently, Bourdieu's response to administrative sociology could be pretty much the same as Critical Theory. However, Bourdieu's emphasis on struggles reveals aspects of his sociology that make it different and closer in spirit to constructivism.

Revisiting the Politics of Reality

A significant reason why it is paramount to maintain constant epistemic vigilance is that, while sociology is involved in struggles over descriptions of the social world and its intricacies, the products of sociological labour can start to work against it once they are deployed in social struggles by other actors (Bourdieu 1990a). As I mentioned before, sociological constructions mingle and clash with previously established constructions – much like in the natural sciences (Latour and Woolgar 1986; Law 2004). These may be constructions established by previous generations of sociologists, but they can just as

easily be folk constructions or constructions perpetuated by the administrative apparatus. This is done both consciously (i.e. interest-driven) and unconsciously (i.e. insufficient care to produce adequate sociological instruments). A good example of the latter is methodologically suspect polling (Bourdieu 1993: 149-157).

Pierre Bourdieu's dislike for polling is not based on a suspicion of quantitative data or the method as such. The issues he raises are remarkably similar to some of the arguments I discussed in Chapter 2. I am referring specifically to the arguments regarding the performative nature of method. Now, a simple recapitulation of the argument is that methods work with implicit notions about what their object is like, and the results they generate produce particular enactments of said object. Bourdieu makes similar observations about the nature of polling. This method claims to investigate people's opinions about specific topics, but the people who design the questionnaires often presume that their respondents share their social philosophy (e.g. not everyone will agree that something is a social problem) and ignore a number of factors (mostly pertaining to education, class and gender) that determine whether an individual will feel comfortable to voice her opinion in the first place. The compound effect is that polling often provides a distorted picture of social reality whose authors are ignorant of their own ignorance.

The significance of the last point relates back to the nature of sociology. It is a discipline whose work is commented upon by, and bears directly upon the lives of, non-sociologists. However, non-sociologists relate to sociological knowledge in a different way. For example, they may treat statistical regularities and the results of opinion polls in a fatalistic manner, since they will usually be ignorant of the assumptions at play in the design of a particular piece of research and insufficiently sensitive to the historical conditions and social mechanisms that reproduce the tendencies that are being articulated. As a result, the sociologist will sometimes be castigated for promoting fatalism and the preservation of the status quo. A more problematic alternative is that such regularities are treated in a normative fashion by politicians and bureaucrats. That is to say, they are treated as inevitable or desirable, in which case mechanisms are altered to reinforce the existing tendencies (Bourdieu criticises economics for this [see Bourdieu 2008]; Theodor Adorno raised this point against positivist sociology in general), and the potential for change is foreclosed. In view of this, sociologists have to be mindful of the consequences their pronouncements may have and cultivate a sensitivity to the repertoire

of extant constructions that circulate in society. However, there may be a potential problem lurking in the background.

The Autonomy of Science

Bourdieu believes that the social field and the way it is represented is characterised by constant struggles between various groups who want to assert their right to define what is actually the case. All these groups are trying to promote and legitimise their particular brand of social philosophy and discredit competing versions.

One of the key reasons why sociology has so much difficulty in acquiring its autonomy is that those who peddle common sense always have their chance in the field according to a principle familiar to economists: bad money chases away good.

(Bourdieu 1992: 184)

If this is indeed the case, what makes sociologists special? Why are they deserving of our time and attention? Well, you may recall that Bourdieu seemed to be working with a clear distinction between the observer and the observed. This suggests that sociologists have a special status. But what exactly are the reasons why we should privilege their opinion over that of non-sociologists?

In both his theoretical and empirical work Bourdieu often emphasises the role of struggles. Different groups compete for various social, economic and political goods and status positions. Sociology is not exempt or immune from this. However, the peculiar thing about sociology is that it studies social struggles and is itself a participant in these struggles. One dimension is sociologists' struggles with existing disciplinary formations, such as philosophy and anthropology (in the French context), for its right to exist as an independent academic discipline. The second dimension is the struggle for the right to define social processes and discredit alternatives. Various groups have proposed different versions as to what the social is like, and these are challenged by sociology.

This conflict over the rights to representation applies equally well to the question of science. As I mentioned before, sociology is a participant in social struggles, because it has to prove itself as a science, and Bourdieu sometimes does this by defining it in opposition to philosophy. What is more, when Bourdieu tries to explain his reasoning as to why sociology is a science, his arguments seem curiously dated and ill at ease within the context of contemporary science studies.

You have to produce coherent explanatory systems of variables, propositions assembled into parsimonious models that account for a large number of empirically observable

facts and which can be opposed only by other, more powerful models which have to obey the same conditions of logical coherence, systematicity, and empirical falsifiability. I am struck, when I speak with my friends who are chemists, physicians, or neurobiologists, by the similarities between their practice and that of the sociologist.
(Bourdieu 1992: 185)

There are coherent systems of hypotheses, concepts and methods of verification, everything that is normally associated with the idea of science. And so, why not say it's a science, if it is one?
(Bourdieu 1993: 9)

This emphasis on procedures of verification, systems of concepts and hypotheses seems very dated, especially in relation to his sophisticated theory of practice and social reproduction. He does make a few concessions, though. For example, he does not overlook the somewhat awkward problem that sociology is a rather dispersed discipline.

[T]he fact remains that, for obvious sociological reasons, sociology is a very dispersed discipline (in the statistical sense), in several respects. That's why it gives the impression of being a divided discipline, closer to philosophy than to the other sciences.
(Bourdieu 1993: 8)

On the whole, though, his definition is inadequate, but I believe it can be improved by looking at his theory of science.

Bourdieu was adamant that a sociology of intellectual life is a prerequisite for sociology in general. One of the tasks of sociology, therefore, is to produce a sociological account of science. This, again, is a site of source of conflict and struggles. Sociological theories do not develop in a vacuum and often have to deal with a fair amount of flak for trying to displace entrenched views (e.g. the debate between Bloor [1981] and Laudan [1981]). What is more, while trying to provide an account of the scientific field, sociology is also defining its relationship to itself (Law 2008). In other words, sociology is simultaneously a participant in the scientific field and a participant in the struggle in which the nature of the scientific field is being contested. The above would suggest that Bourdieu is quite comfortable with a sociology of science and science studies more generally. However, he has a rather different take on what sociology reveals about science.

In his response to science studies literature Bourdieu (1975; 2000; 2004) criticises a number of authors for what he believes to be inadequate accounts of science. Early sociology of science (e.g. Robert Merton) is criticised for paying insufficient attention to struggles within the scientific field. Sociology of science, Bourdieu argues, focused its attention on the norms and reward system that govern scientific discourse, but approached science as a kind of profession. Bourdieu recognises that Merton was writing

at a specific point of history where the image of science had to be rehabilitated (see Introduction), but this does not invalidate Bourdieu's criticism that insufficient attention was paid to the settlement and resolution of scientific disputes and the unquestioned acceptance of a division of labour that ceded the right to describe good science to philosophers.

Thomas Kuhn is praised for recognising the importance of discontinuities and epistemic breaks, but the emphasis on paradigms is criticised for leading to a very problematic theory of scientific change – one that locates the source of change inside science. The likes of Bruno Latour are taken to task for focusing on the semiotic aspects of science and dissolving science into politics (see also Chapter 2). Bourdieu is equally dismissive of the Strong Programme (both the Edinburgh and Bath variants), who are noted for emphasising the plasticity of data but accused of a decidedly interactionist approach to scientific norms (Collins and Yearley 1992). In place of these visions of science Bourdieu proposes to consider science as a field of forces, which is both constituted by, and reproduced through the actions of, scientists, laboratories and research centres (Bourdieu 2004: 69-70).

Fields of Forces

Bourdieu's social universe is populated by what he calls fields. A field can be regarded as a kind of autonomous and competitive social setting in which individuals pursue strategies according to the rules of that particular field (Thomson 2008). Fields impose upon their members particular rules of conduct through which individuals can define their relationship to the goals of the field and their competitors within the field. Education is a field, literature is a field and so is science. The influence that any one individual unit can exert on others depends on how much capital (understood broadly) and recognition it commands. In the case of science, recognition would most likely depend upon the material resources a scientist commands (e.g. equipment, grant money) and the recognition that her work has managed to earn. The distribution of these various forms of capital determines the structure of the field and the positions that individual units (e.g. scientists, universities) occupy within it.

Now, thus far there seems to be nothing controversial about Bourdieu's theory of science. In fact, it has definite constructivist overtones – the field is the result of interactions and collective negotiations between individual scientists, academic

institutions and the resources at their disposal. However, what is missing from this picture is the political dimension. The reason for this is quite simple – Bourdieu considers science to be an autonomous field (much like art or politics itself).

A scientific field is a universe in which researchers are autonomous and where, to confront one another, they have to drop all non-scientific weapons – beginning with the weapons of academic authority.

(Bourdieu 1992: 177)

There is certainly intra-field politics (as regards the distribution of recognition, for example), but this form of politics follows a logic that is unique to the field. The scientific field is both regulated and constituted by norms of argument and other cognitive instruments that the collective of scientists has agreed upon.

Like the artistic field, each scientific universe has its specific doxa, a set of inseparably cognitive and evaluative presuppositions whose acceptance is implied in membership itself.

(Bourdieu 2000: 100)

Logic itself, logical necessity, is the social norm of a particular category of social universes, scientific fields, and it is exerted through the constraints (especially the censorships) socially instituted in these universes.

(Bourdieu 2004: 70)

The specificity of the scientific field derives from the fact that norms of argumentation are the social norms that regulate struggles (Bourdieu 2004: 70-73). In other words, while the norms that constrain individual moves and strategies within the field are social, they are social in the sense that they have become generalised and are collectively maintained by the scientific community. Such norms might be recognised forms of inference, data interpretation (and collection) and techniques of persuasion. They are collective norms that are peculiar to the field – peculiar to science, and successful participation in the scientific field is contingent upon abiding by them (Bourdieu 2004: 72).

Such principles are socially sanctioned, but they are irreducible to the will of any single scientist, ambient values, or economic circumstances. In this sense Bourdieu is close in spirit to Karl Popper (1976). They both seem to be advocating a shift from discussing objectivity as a relation between an individual and an object to treating it as a relationship between subjects who are committed to socially defined principles of conduct and argumentation (Bourdieu et al. 1991: 74). Science is a collective endeavour, and the acceptance of scientific facts is contingent upon peer recognition. The conflicts within the field can, of course, be rather nasty, and the players may be motivated by the pursuit

of economic and social rewards and forge alliances against their opponents, but they are compelled to do so in a way that respects the norms governing the scientific field. By engaging in conflicts (however petty) and intra-field politics, scientists actually further the cause of science as an institution (Bourdieu 2000: 93-94).

The logic described by Bourdieu seems relatively easy to falsify, but that would miss the point of what he was trying to say. The idea was not to articulate a logic that describes everyday routines. If this were the case, Bourdieu's theory would be easily refuted. Bourdieu was instead trying to show that there are constitutive and regulative norms that have to be satisfied in order to play the scientific game. Even though they may not always be followed in practice, the recognition of practices as scientific depends on (i) them being identified with these norms and (ii) them being seen as following these norms. At first this seems quite difficult to square off with a negative reading of Soviet sociology. The point of the two theses outlined in Chapter 3 was to illustrate that certain forms of reasoning had ossified and achieved universal acceptance. In fact, there seems to be something peculiar about simply assuming the autonomy of logical reasoning and then postulating it as the basis of good science. Surely intricate systems of thought, such as historical materialism, can remain logical, garner collective support and so satisfy Bourdieu's criteria. For example, Slava Gerovitch's discussion of dialectical materialism illustrated that it had become a rather autonomous field of disputes where success was contingent upon being a competent user of the argumentative strategies and literary canon of dialectical materialism. In other words, appealing to a logic of science does not seem to be a straightforward way of doing away with the problems associated with Soviet sociology. However, there is another aspect of Bourdieu's theory of science that is useful for crafting a response.

Sociology as a Dominated Science

Pierre Bourdieu repeatedly stressed that science is an autonomous field. However, he explicitly rejected the idea of science as a bundle of norms or forms of argumentation against which to measure all other cognitive pursuits. There is no inevitability about science succeeding – it requires the right social conditions to be able to function and exert its influence; or, as Bourdieu put it, 'there are historical conditions for the emergence of reason' Bourdieu (2000: 70). Intellectual autonomy is a historically contingent and fragile construction that requires constant maintenance work. However,

Bourdieu's choice of words can have a few unfortunate connotations. When he talks about autonomy, he does not mean that scientists, or intellectuals more generally, are socially detached or unaffected. Quite the contrary. They occupy a particular place in social space, and each of them can be individually positioned within the scientific field (Bourdieu 2000: 10). What is more, Bourdieu does not believe that intellectual activity is indifferent to broader social concerns, nor is he committed to a disjunction between facts and values. What he does argue against is the subjection of science and scientific research to economic and political interests. This is especially clear in the case of sociology.

Bourdieu believed that sociology is a science that makes trouble (Bourdieu 1993: 8). What is more, its success should be measured by how successful it is at disturbing patterns of domination. The problem for sociology is that it has trouble achieving its potential. Sociology is an academically weak discipline that has to constantly justify its right to exist, and define itself in relation to more institutionally established competitors (e.g. philosophy and economics). Such a situation has a structurally destabilising effect vis-à-vis autonomy. Sociology, unlike physics, has not managed to set up an autonomous disciplinary field with norms that distinguish between good and bad sociological conduct (Bourdieu 2004: 87). Sociology's precarious position as a participant in both the scientific and political fields robs it of a clear backbone that would allow it to resist external demands and challenges from other groups who feel they have an equal right to comment upon social matters and explain social processes. This very same lack of an established disciplinary ethos allows some parts of sociology to be used for administrative purposes and generally be subject to heteronomous interests. This, according to Bourdieu, is related directly to the lack of autonomy because 'recognised scientific authority protects you from the temptation of heteronomy' (1992: 183-184; Whitley 2001). However, as Bourdieu has argued, this is not proper sociology, and the only reason why such sociology can exist is because the field has failed to install robust quality controls that would disallow various practices from passing as sociology. In other words, sociology can be mangled because researchers can pander to ideological imperatives, and the diffuse and poorly regulated structure of the sociological field can do nothing to correct these forms of behaviour (Bourdieu 2004: 87). The norms and ideals that censure dilettantes out of the more distinguished academic disciplines are not operational in the case of sociology, which is simultaneously involved in social struggles over its object and scientific struggles to define itself as a science. Consequently, you

could argue that both theses about Soviet sociology simply reflect academic anxieties about a structurally unstable scientific field. Soviet sociology was a weak scientific discipline whose credibility was contingent upon its relationship to the Party and official academic prescriptions. It was not an autonomous field with internally defined criteria of quality. Sociology never broke free from the world of practical affairs and so could not hope for the autonomy Bourdieu deemed necessary for it to function as a structurally sound field with sufficiently robust criteria of entry.

Bourdieu's focus on internal quality control, however, runs the risk of reducing the ills of sociology to factors that derive from the inherent nature of sociology. In particular, this relates to the standards governing sociological work and its ability to claim parts of reality for itself and institute collective standards according to which new contributions can be evaluated. This is where Latour's sensibilities show their strength by introducing the final loop – public representation.

Sociology as a Self-Undermining Science?

Public representation refers to the integration of scientific knowledge (or technology) and the human collective. Latour emphasises that, while this aspect may appear to be superfluous and trivial, students of science must be attentive to the important role this final loop plays in the circulation of scientific knowledge and technology.

Our sensitivity to the public representation of science must be all the greater because information does not simply flow from the three other loops to the fourth, it also makes up a lot of the presuppositions of scientists themselves about their objects of study. Thus, far from being a marginal appendage of science, this loop too is part and parcel of the fabric of facts and cannot be left to educational theorists and students of media.

(Latour 1999: 106)

This, I would argue, is the Achilles' heel of sociology, and is elaborated upon at some length by Steve Fuller.

Fuller (1991; 2002; Fuller and Collier 2003) has argued that a persistent problem for sociology is that it is poorly defined as a disciplinary unit. This is not a recent phenomenon and goes back to its inception that saw the simultaneous emergence of various schools of thought. This trend has only become more pronounced since then as a result of the proliferation of academic traditions within sociology. Bourdieu seemed to be equally aware of this, but Fuller's argument paints a more detailed picture of why this is so. Underlying Fuller's claim that sociology is a loosely structured disciplinary

formation (i.e. there is no discipline) is a set of arguments pertaining to the way the sciences write their history. To put it simply, one of the reasons why the natural sciences enjoy the authority and prestige they do is because they have control over how their history is told and the role that history is delegated (Fuller 2002: 177). This, of course, goes right back to Thomas Kuhn (1977; 1996), who discussed the importance of textbooks – textbooks provide a carefully edited version of the history of a particular discipline. In the social sciences, however, less of their history is 'consigned to silence' (Fuller and Collier 2003: 94), and history plays a significant role in understanding the development of these disciplines. For example, Wagner (1991) deconstructs the notion that the period of classical sociology was a major turning point and recasts it as a failure; a number of historical accounts discuss the emergence of sociology in relation to the needs and particular sociohistorical configurations of various nation states (Bannister 2003; Fridjonsdottir 1991; Gouldner 1970; 1973; Ross 1991; 2003; Wagner 1991; 2001; 2003a; 2003b). Consequently, the historical narrative produces an image of sociology as a decentralised form of knowledge that is both in and about the world. Sociological knowledge is figured by way of entanglements with extra-scientific factors, and, while textbooks present a streamlined version of theoretical developments (Best and Schweingruber 2003; Lynch and Bogen 1997; Manza, Sauder and Wright 2010), overviews and reconsiderations of classical theory are more explicit about the role of context in understanding social theory (e.g. Giddens 1995; Gouldner 1970; Turner 1999).

Given the social sciences' uphill battle to secure epistemic legitimacy, the rhetorical seams of their attempts to represent the world, without appearing to intervene in it, are easy to see.

(Fuller and Collier 2003: 88)

Science studies has tried to show that this is true of all forms of knowledge. All sciences are only partially autonomous from the societies that support them, but it is telling that this had to be done for the natural sciences and biomedicine, whereas it could simply be assumed about sociology. This, I would argue, is partly because sociologists themselves do this on a regular basis (Fuller 1991; Jones 1983; Hamilton 2003; Turner 1998), but it is possible that such routine exercises in academic integrity may actually run counter to the colonising tendencies evident in the work of Bourdieu and Latour.

Conclusions

The work of Bruno Latour and Pierre Bourdieu illustrates a tension in constructivist

attempts to understand sociology from a science studies perspective. In particular, the tension concerns the difficulties that sociology encounters whilst trying to establish itself as a distinct and autonomous form of inquiry. For example, even though the notion of scientific autonomy is important to Bourdieu, he, much like Latour, contends that it is an achievement, rather than a given. Bourdieu differs, however, in that he argues for the importance of having at least some notion as to what constitutes a transgression against the logic of a particular discipline. The way he proceeds to argue for this is both a strength and a potential weakness of his theory.

Bourdieu's optimism about science and scientists is shared by a number of scholars I have looked at. What differentiates these authors, however, is that their theories of science seem to be more open to other considerations. Unlike Bourdieu, the likes of Stengers and Latour work with a model of science that is more responsive to signals and interference that Bourdieu would have regarded as external. What is more, this responsiveness of science to different concerns is treated as a bonus, as something that should be cultivated. To use Latour's language, alliance-building is encouraged because this allows different voices to participate in the construction of the common world. Furthermore, such alliances are a key component of successful scientific projects. The peculiarity of sociology, however, is that there is a pervasive ambivalence about the scientific cost of building certain kinds of alliances. This is a blind spot for Latour, but an integral component of Bourdieu's theory of sociology.

Bourdieu stresses the importance of instituting robust criteria of entry and participation in the sociological field. Furthermore, the perceived lack of such criteria and a clear disciplinary identity are believed to be the cause of sociology's ailments. However, this insistence on factors that are internal to the discipline overlooks an aspect of sociology that is rather more telling. While constructivism aims to show the processes and arsenal of methods and techniques for constructing and stabilising social and material realities, it also uncovers strategies through which science is purified of accretions that would shed doubt on its claim to represent the world as it is. The peculiarity of sociology is that it has (i) not managed to purify itself consistently and (ii) on occasion even made a feature of its own hybridity by illustrating the ways in which it has both described and participated in the realities it posits.

The claim I wish to explore in the next three chapters is that constructivism, while

perceptive as to the issues that hamper sociology, may be overly rigid and gloss over certain aspects of sociological practice, sociologists' self-perception and their understanding of sociological knowledge more generally. Furthermore, in Chapter 8 I will argue that these characteristics of sociology resonate positively with recent developments in constructivist methodology.

5. Conversations with Latvian Sociologists, Part I: Context and Method

In Chapters 3 and 4 I explored a number of issues pertaining to the relationship between science studies and sociology as a distinctive species of science, and tried to articulate a constructivist take on sociology that took seriously its specificity and place in the scientific field. A question that remained open was whether a constructivist treatment of sociology was congruent with how sociologists themselves understand and relate to the activities they are engaged in. Moreover, I tried to suggest that the discrepancy was intentional and, in fact, a contributing factor to the weakness of sociology as discipline. In order to explore these issues in more detail I now turn to sociology in Latvia.

Sociology in Latvia has a number of features that make it an interesting object of study, and the reasons for choosing to study it are both methodological and theoretical in nature.

Firstly, sociology in Latvia is primarily applied in nature (Tisenkopfs 2008a; 2010b), which makes it an interesting case for the theoretical framework informing this study. The main outlet for sociology in Latvia is commissioned research carried out by both university-based researchers or privately owned research centres (see below). A small fraction of the research is funded by government grants via the Latvian Council of Science, and sociologists are also involved in various research projects funded by the European Union. What is more, undergraduate programmes place an emphasis on providing a new generation of sociologists with the practical skills and methodological competences necessary to function as a researcher in the existing arrangement. Sociology, therefore, functions primarily as a profession, rather than a form of disinterested inquiry. Consequently, sociology in Latvia is a pertinent example of how funding and extra-academic needs, interests and discourses shape and mould the structure and character of an entire discipline. Moreover, its emphasis on applied research makes it a good foundation for a contrast between the respective theories of Bruno Latour and Pierre Bourdieu and the interplay between autonomy and heteronomy more generally.

Secondly, while it would not be true to say that sociological knowledge has had no bearing on the implementation of policies (e.g. Muižnieks [2012] refers to a number of

policies based on sociological research), sociology has had very little impact on policy-making (Tisenkopfs 2010b; Aivars Tabūns, personal communication). For example, in his overview of the development of sociology in Latvia since 1966 Tisenkopfs (2010b) argues that sociological knowledge has been of little use to policy makers, because, while it has been (and still is) applied in nature, it has not been heavily involved in the design and implementation of practical innovations and solutions; Berdņikovs (2011), Kūle (2009) and Tisenkopfs (2011) have also argued that expertise generated by the social sciences and humanities is thoroughly undervalued in general. In view of my discussion in the preceding chapters, I would like to suggest that sociology in Latvia is interesting precisely because of its ambiguous status: (i) it is primarily applied (with little in the way of theoretical and speculative content), (ii) societal challenges that sociology could help to resolve are explicitly discussed in a number of strategic policy documents adopted by the government of Latvia (e.g. The Latvian National Development Plan 2007-2013) and (iii) the knowledge sociology can provide is recognised as a valuable resources for civic education and social integration (Latvia 2030: 92)¹⁷, and yet it is an undervalued form of knowledge whose pertinence is routinely questioned or disregarded.

Thirdly, it is a peculiar example in that the development of sociology in Latvia has followed a trajectory that is unlike that of most Western states. However, the discipline is involved in Western projects (e.g. International Social Survey Programme, the 7th Framework Programme) and its practitioners attempt to forge links with Western academia.

Finally, the sociological community in Latvia is relatively small and, therefore, well-suited to an in-depth study limited by the time constraints of a doctoral dissertation.

In this chapter I will attempt to situate the analysis of the two subsequent chapters. In the first part I provide a general overview of sociology in Latvia. In the second part I discuss my methodology, the difficulties I encountered while interviewing sociologists working in Latvia, and the resulting limitations to my analysis in Chapters 6 and 7.

Sociology in Latvia: Early Days

According to Aivars Tabūns (1996; 1998; 2002; 2010), Latvia has a fairly substantial sociological tradition stretching back to the period prior to World War I. The foundations

¹⁷ <http://www.varam.gov.lv/lat/pol/ppd/?doc=13857>, accessed on 01/03/2015.

of this tradition were based on translations of original texts and overviews thereof, as well as the theoretical syncretism of practitioners working in other disciplines (i.e. history and philosophy). Tabūns argues that the roots of Latvian sociology can be traced back to the nationalism of the 19th century and the sociological ideas that circulated in 19th century Russia (see also Stites 1989; Simirenko 1967a). In order to understand the nature of social (though not necessarily sociological) thought in pre-war Latvia Tabūns singles out a book written by Pēteris Birkerts, who studied philosophy and sociology at Columbia University. The book he is referring to was published in 1921 and was called *Zocioloģija*. Tabūns argues that the most prominent strands in Latvian inter-war social thought are discernible in this book. The first of these is that of the early Chicago school. The second is the experimental psychology of Wilhelm Wundt and the social psychology of Gabriel Tarde (Birkerts wrote a number of books on psychology as well). Seen in conjunction, the first two strands show an inclination towards micro-sociology with a psychological twist. The third strand consisted of the protosociological ideas implicit in the works of Russian and Latvian authors, which, as Tabūns emphasises, were seldom Marxist in nature.

The inter-war period, according to Tabūns, was characterised by great theoretical diversity. Publications covered topics ranging from national culture (which intensified during Kārlis Ulmanis' dictatorship), demography and meta-philosophical commentary on the nature of sociology. The latter is significant in that one of the more prominent books on this topic was written by the distinguished Latvian philosopher Teodors Celms, whose work has attracted considerable attention from philosophers in post-1991 Latvia.

Sociology in Latvia: Latvian SSR

The Latvian story seems to have followed a trajectory comparable to the one I outlined in Chapter 3. Sociology was plagued by similar kinds of problems and faced comparable forms of censorship. What is more, the emergence of sociology in Latvia (then Latvian SSR) was characterised by equally protracted attempts to define its relationship to, and obtain independence from, philosophy. Sociological theorising suffered a serious blow after Latvia was annexed by the Soviet Union in 1940. It was isolated from theoretical and methodological developments in Western Europe and the United States, and was given only limited access to local traditions and schools of thought (ethnographic research was better off in this regard, see Gellner 1988; Dunn and Dunn 1979;

Greenfield 1988). The institutional emergence of sociology in Latvia began in 1966 with the establishment of a *Cathedra*¹⁸ of Applied Sociology in the Department of Philosophy. This coincided with the formation of similar academic units in the other Baltic states (Tabūns 2010; Titma 2002; Vosyliute 2002). These were intended as places where a new generation of sociologists would be trained. The impetus came from the belief that sociology could be a useful tool for social planning. However, this process was hampered by the damage incurred by the sociological establishment in the preceding decades; there was a distinct lack of international contacts and cooperation, little access to books and very limited proficiency in the use of sociological methods. Only a select number of people had access to sociological books published in the preceding decades, and there was no way to obtain them if you were not part of the academic establishment. The work of sociologists who had escaped the attention of Soviet censors (e.g. Max Weber and Georg Simmel), however, was available. Additional complications arose due to the unclear disciplinary status of sociology. In 1970 the Department of Philosophy was merged with the Department of History (Zellis 2010). As a result, sociology and philosophy were lumped together in the Cathedra of Philosophy and Concrete Social Research. One of the issues that came up as part of various attempts to reorganise and rejuvenate (e.g. ageing academic staff) the faculty was the problematic status of sociologists (Zellis 2010: 224). The problem had two sides to it.

Firstly, sociology was an ambiguous academic formation in that it had roots in philosophy (i.e. historical materialism), but it had gradually evolved into something independent. This independence, however, had not been institutionally acknowledged – sociologists did not even have a separate office to work in. The intimate connection between sociology and philosophy, and their shared space of origin, has been commented upon by Tisenkopfs (2008a; 2010b). He argues that the intellectual climate in the faculty was liberal and fostered critical thinking. Nonetheless, the nascence of sociology is directly related to its attempts to sever the umbilical chord with the stem discipline. This, he argues, to a certain extent explains why philosophers defended sociology, yet also treated it as an ungrateful offspring.

Secondly, it was unclear where and how graduates would work after finishing their studies. This was compounded by the peculiar situation that the importance of

¹⁸ An academic department. The *cathedra* was a unit in the Faculty of History and Philosophy until 2000.

sociological knowledge for the long-term development of Soviet society was (officially) recognised, but sociologists had no clear professional/occupational status or designated place of work (Zellis 2010: 244). That is to say, even though sociology had achieved a tentative and somewhat precarious academic (and political) credibility, there was no demand or understanding from employers as to what exactly sociologists could offer, even though there were attempts to rectify this problem (Zellis 2010: 246). This was equally evident in the case of philosophers, even though they were (officially) important figures in the ideological war against the West and charged with playing an edifying role in Soviet life (Runce 2010; Zellis 2010). A tentative solution to this issue was the (re)establishment of a separate Cathedra of Applied Sociology in 1977. Such material constraints notwithstanding, sociological work was done.

The first arguably sociological book was written by Tālvāldis Vilciņš (educated as an historian). The topic was professional prestige, and the study was carried out in 1965 with the help of questionnaires. According to the academic Jānis Stradiņš (both a scientist and a historian of science), Vilciņš was politically cautious and could survive in the Soviet academic system without actively supporting the political regime (Tisenkopfs 2010a). In addition to writing the first sociological book in the Latvian SSR, he also wrote a book about science as an object of scientific study (Vilciņš 1979). The first doctoral dissertation in sociology was defended in 1970. The author was Rita Kvelde, and her study concerned radio shows and the formation of public opinion (a prominent topic in Soviet sociology, see Chapter 3). What is intriguing about the work of Rita Kvelde is that she carried out her research while working for a radio station that had its own laboratory for social research, which supports Elisabeth Ann Weinberg's claim that the media (rather than academia) popularised public opinion research in the Soviet Union (Weinberg 1974). Kvelde's dissertation is also conveniently positioned on the timeline of sociology in Latvia. The 1960s saw the establishment of numerous centres and laboratories for social research. The same is true of Lithuania and Estonia (Tītma 2002; Vosyliute 2002). The 1970s, on the other hand, were characterised by the development of various research programmes. However, the emergence of an institutional network supporting sociology did not necessarily mean that research could proceed smoothly.

Though not mentioned explicitly in the historical overviews provided by Aivars Tabūns, a central theme in a number of interviews with Latvian sociologists (see Tisenkopfs 2010a)

and publications dealing with the history of the Department of History and Philosophy (Keruss et al. 2010) was the lack of sociological training, especially as regards the application of sociological methods. That is to say, even though there were practitioners of what we would call sociology, they lacked experience in the use of their methods. This echoed issues raised in the articles on the development of sociology in Estonia and Lithuania. People doing sociology were often ignorant of statistical techniques beyond basic descriptive statistics. What is more, they did not have the resources or the technology to process the data adequately (Zellis 2010; Runce 2010). Thus, while an institutional base was being established, the skills of the people populating these institutions were lagging behind, and the resources and equipment they needed were in scarce supply. The causes of this ignorance are quite easy to identify. Much like sociologists in the rest of the Soviet Union, most Latvian sociologists were not trained as sociologists. The vast majority of people working as sociologists were trained as philosophers, economists or historians; philosophers could specialise in sociology, but the core of their training consisted of philosophy. What training in sociological research methods these people did get came from further-education seminars.

Sociology and Censorship

Thus far the development of sociology in Latvia follows a trajectory that is roughly similar to the rest of the Soviet Union. Latvia's story deviates, however, in one very important respect. I argued in Chapter 3 that scholars commenting on Soviet sociology felt that it was akin to a research division of public administration with an overly pronounced political stance. The picture put forward in the interviews with Latvian academics (Keruss et al. 2010; Tisenkopfs 2010a and the interviews conducted by me), however, is slightly less bleak. Sociologists invariably acknowledge that ideology did play a part in sociological work. There were limits to what you could study, the kind of questions you could ask and the kind of behaviour and pronouncements that were acceptable in public. However, when it came to the kind of sociological work one was allowed to do, the ideological constraints were mainly formal and research could proceed without significant interference or close supervision. In other words, one had to refer to one of the classics in the Marxist-Leninist canon and add an ideologically pandering sentence to appease the censors (Keruss et al. 2010; Tisenkopfs 2010a). This was corroborated by a number of sociologists I interviewed in July 2012. They claimed that most students were fully aware of the unofficial procedures one had to follow in order to

avoid antagonising the political establishment. Sociologists could, therefore, retain a kind of intransigent line, which provided intellectual distance between them and the ideological apparatus of the Soviet system. What is more, it implies that academic censorship was not as pervasive as Western scholars would have us believe, though one should be cautious about dismissing its impact on the content of sociological literature and reports.

This created a peculiar situation. There were ideological constraints, but in practice this only affected the surface of sociological research. This allowed for the possibility that, even though historical materialism and its derivatives were the officially supported theories of society and sociohistorical development, the vast majority of sociologists employed a theoretical and methodological outlook that was more reminiscent of traditional positivism (Tabūns 2010: 112; Gray 1994). The reason for this was surprisingly simple. The abstract and theoretical nature of Marxist social theory did not provide sociologists with the necessary tools to study social processes empirically. It restricted the sociologist to armchair speculation and provided few concrete suggestions for empirical research. If you recall the discussion in Chapter 3, this was partly by design. American positivism, however, provided the methodological tools required for empirical work. Furthermore, other than Marxism, sociological theory was somewhat neglected; this is a trend that is still characteristic of contemporary Latvian sociology (see below).

Towards the end of the 1980s Latvian sociologists finally had access to internationally respected statistical software packages and were allowed to regularly attend international conferences, and, a few setbacks notwithstanding¹⁹, the discipline of sociology had (re)established itself in Latvia within the space of 20-25 years.

Sociology in Latvia: Post-1991

The transition to a capitalist economy precipitated great institutional changes. Loren Graham (1998) argues that in the aftermath of the collapse of the Soviet Union Russian scientists were much worse off than under Soviet rule. The reason for this was simple – the amount of money devoted to science was significantly decreased. Consequently, Russian science fell behind; research centres simply did not have enough money to pay their employees and buy new equipment. This resulted in a brain-drain from academic

¹⁹ The Faculty of History and Philosophy, where sociology was based, was temporarily closed in 1983, because the loyalty of the students and academic staff was put into question.

institutions – scientists emigrated or went to work for commercial organisations.

The situation is not much different in the case of Latvian sociology. A number of research centres and academic units were closed following the collapse of the Soviet Union. Funding rapidly transitioned to a grant system. Before this, funding was allocated to research institutions, rather than specific projects on a case-by-case basis (Tabūns 2010: 112-113). This move was not kind to sociology. For example, in 1997 the Latvian Council of Science funded 19 sociological projects with a grand total of 94 852 lats (Tabūns 1998; 2010). Judging by more recent reports (e.g. 2011) on the social sciences and humanities (Latvian Council of Science), the situation seems to have improved somewhat, and more money is made available, though this may have something to do with the hybrid nature of some of the projects (i.e. sociologists working with historians and media studies scholars). A prominent example of this phenomenon is the state research programme *National Identity*, which is a 3-year project with a total budget of just over 5.1 million euros. Sociology is one among many other social science and humanities disciplines (e.g. philology, history) carrying out research as part of this project. On the other hand, the last two reports where sociology is listed as a discipline²⁰ show that funding for sociological projects is down to 45736 lats (2009)²¹ and 42636 lats (2010)²². What is more, the number of projects is also lower (9 and 3 respectively). This has to be put into perspective, however, as most of the money supporting research activities in Latvia comes from the European Social Fund²³, the European Regional Development Fund, or as part of 7th Framework Programme, rather than the state budget. These projects are a chance for researchers to obtain funding for their research, foster international contacts and introduce novel theoretical insights and methodological approaches into Latvian sociology. A prominent example of this are projects in rural sociology and regional development (see Šūmane 2010).

The situation is complicated somewhat by the fact that many of the projects are interdisciplinary and involve sociologists working with, or assisting members of, other disciplines. Thus, even though there is a sociological component to a number of research projects, it is difficult to gauge exactly how much money is devoted to sociological work.

²⁰ Since 2011 sociology has been included under the heading “social sciences”, which also includes economics, anthropology and law.

²¹ http://www.lzp.gov.lv/lemumi/L09_1-1-2_piel.htm, accessed on 19/05/2014.

²² http://www.lzp.gov.lv/images/stories/dokumenti/finsaraksts_2010.doc, accessed on 19/05/2014.

²³ Hereafter - ESF

However, a project on migrant communities where sociology is a leading discipline (i.e. most researchers are sociologists) attracted 492 049 euros²⁴, so it is clear that the state budget only accounts for a small fraction of the total sum available for research activities. This is not particular to sociology or even the social sciences and humanities in general; although it should be mentioned that the humanities and social sciences only receive about 20% of state money allocated to scientific research (Kunda 2013).

A consequence of the lack of funding in the 1990s was that sociologists started to make a living primarily by doing applied research (commercial or otherwise) with little in the way of academic output. Tālis Tisenkopfs refers to this as 'the privatisation of sociology', which is a development that has had a dramatic impact on the theoretical and academic development of the discipline (Tisenkopfs 2008a: 10). This is also evident in the case of academic publications, as the overall majority of them are based on applied research. To illustrate this point we can turn to an overview of current research activity in Latvia, which is provided in the book *Socioloģija Latvijā* [*Sociology in Latvia*] (Tisenkopfs 2010)²⁵. In the section dealing with current research, most articles refer to reports produced at the end of research projects. The projects range from research done for local institutions²⁶ (e.g. ministries, state chancellery, NGOs) and international organisations (e.g. World Bank). A prominent example of a project where local sociologists participate on a regular basis is the *United Nations Human Development Report* in which sociologists are frequently involved both as editors and contributors. The second group of publications are books and articles that are mainly data-driven and applied in nature. While this is to be expected from overview articles, there are few local publications that discuss the theoretical aspects of sociological work (a notable exception is Tisenkopfs 2008b; 2010c). What is more, the absence of sociological theory is not restricted to the chapters in *Socioloģija Latvijā*. An examination of academic work in sociology published by the Faculty of Social Sciences, University of Latvia²⁷, the Institute for Philosophy and Sociology, University of Latvia²⁸ and the University of Latvia Academic Press presents a similar picture in that theoretical and reflective publications are few and far between. This is not to say that there are no publications of purely academic interest – one can certainly identify examples of

²⁴ <http://sf.viaa.gov.lv/lat/zinatne/>, accessed on 22/05/2014.

²⁵ Many of the chapters are reworked versions of articles published in *Latvijas Universitātes Raksti*, Volume 736 [available at www.lu.lv/apgads/izdevumi/lu-raksti-pdf/736-sejums/].

²⁶ Many can be found at: <http://petijumi.mk.gov.lv/ui/>

²⁷ <https://sites.google.com/site/szfworkingpapers/>, accessed on 25/05/2014.

²⁸ <http://www.fsi.lu.lv/?sadala=59>, accessed on 25/05/2014.

theoretical reflection on local practices and publications that place a greater emphasis on theory (e.g. Beitnere 2012; Kļave and Šūpule 2013). Furthermore, it must be noted that the situation has changed in the last few years, and a number of Latvian sociologists have started to regularly publish academic articles in internationally peer-reviewed journals both individually and with colleagues working at other European universities. The surge in academic activity was concomitant with a significant increase in the number of sociologists who hold a doctorate.

The chronology of doctoral theses written in Latvia is also illustrative of the state of Latvian sociology. The first new thesis since Latvia regained its independence was written by Dagmāra Beitnere (on self-reference in Latvian culture). She obtained her doctorate in 2003. It was followed shortly by the thesis of Baiba Bela in 2005. These two, however, were the only two doctorates until 2010, at which point there was a sudden outburst – 25 new sociology doctorates in the period 2010-2014. A possible explanation for this is provided by Tisenkopfs (2008a) who argues that the new generation of researchers did not seek to translate their considerable professional experience into institutionally recognised forms of academic capital. However, one should be cautious about making the claim that there are two generations of sociologists. It is true that the people who received their qualifications prior to 1991 now occupy prominent academic positions and are heads of research centres (e.g. Brigita Zepa at Baltic Institute of Social Science; Tālis Tisenkopfs at Baltic Studies Centre), but there are a number of sociologists who complicate the distinction between the pre-1991 and post-1991 generation. For example, Līga Rasnača and Anda Laķe received their undergraduate degrees before 1991, but only received their doctorates in 2011 and 2012 respectively. Furthermore, while not having a doctorate is not a major professional setback in Latvia (e.g. Ilze Trapenciere and Oksana Žabko are prominent members of the community, yet they do not possess a doctorate), a significant factor influencing the decision not to write a thesis may have been the inability to combine professional commitments (little to no funding for doctoral students) with academic activities. ESF funding, however, allowed a number of people with significant research experience to take a year off from work and focus solely on their thesis (Kunda 2013).

Institutions

A number of universities offer undergraduate degrees in sociology. By far the most

prominent one is the University of Latvia – it has the most professors (4) and a significant portion of sociological research is carried out by people who are either working at the faculty or received their degrees there. It is also possible to study sociology at Latvia University of Agriculture (Sociology of Organisations and Public Administration) and Riga Stradins University (Sociology of Organisations and Management). There are two universities that grant doctorates in sociology – University of Latvia and Riga Stradiņš University. Both institutions have been quite productive in the last few years, largely as a result of the availability of ESF funds. The range of topics is quite broad, though for the purposes of this thesis I should note that only one thesis could be classified as belonging to science studies (Ādamsone-Fiskoviča 2012). All the abovementioned universities also carry out sociological research.

A common complaint voiced by sociologists from the three Baltic states is the lack of data archives. There have been attempts to set one up, but due to the fact that the license for *Nesstar* was not renewed the site is currently offline, even though in 2009 the Latvian Council of Science funded a project whose aim was the preservation and accessibility of data²⁹. A similar problem plagues the Latvian Association of Sociologists, whose primary function is the dissemination of information, but it does this irregularly. Furthermore, the professional commitments of its board members prevent them from devoting time to organising academic events.

An equally important form of existence for sociology are research centres and organisations. A list of such institutions can be found in the book *Socioloģija Latvijā* (Tisenkopfs 2010a: 518-533), where research centres are divided into two groups – social and market research companies and academic research units (see **Table 1**). While the number of research centres might suggest that sociological research in Latvia is ubiquitous, a recent research assessment exercise claims that sociology is still a minority discipline³⁰. A potential reason for this could be that the dominant form of inquiry is social research, rather than sociological research (see Williams [2000] and Savage and Burrows [2007] for a more general discussion). That is to say, most research in Latvia is empirical and 'innocent of specific disciplinary ties' (Williams 2000: 159).

²⁹ http://www.lzp.gov.lv/lemumi/L09_1-1-2_piel.htm, accessed on 28/05/2014.

³⁰ Research Assessment Exercise: Social Sciences, http://www.izm.gov.lv/images/zinatne/ZISI/zisi_09.pdf, accessed on 01/04/2015.

Name	Social and Market Research Companies	Academic Research Units
Analītisko Pētījumu un Stratēģiju Laboratorija, SIA	*	
Baltijas Monitors, SIA	*	
Data Serviss, SIA	*	
Eurodata, SIA	*	
Factum, SIA	*	
Fieldex, SIA	*	
GfK Custom Research Baltic, SIA	*	
Kvalitatīvo Pētījumu Studija, SIA	*	
Latvijas Fakti, SIA	*	
Market Data, SIA	*	
Latvijas Reitingi, SIA	*	
Nikolo Grupa, SIA	*	
SKDS, SIA	*	
Sociālās Alternatīvas Institūts	*	
Socioloģisko Pētījumu Institūts, SIA	*	
TNS Latvia, SIA	*	
Baltic Institute of Social Sciences		*
Baltic Studies Centre		*
Institute of Social Investigations, Daugavpils University		*
Centre for Scientific Research, Latvian Academy of Culture		*
Social Research Group, Latvia University of Agriculture		*
Institute of Philosophy and Sociology, University of Latvia		*
Social and Political Research Institute, University of Latvia		*
Sociological Research Centre, Liepaja University		*

Table 1: Social Research Centres in Latvia

I will return to this point in more detail in the next chapter, but my contention is that, even when it is not strictly speaking applied, and is of considerable academic interest, research is driven by data, and is not necessarily located in a theoretical context, with

theory playing a kind of supplementary role. Furthermore, the situation becomes even starker when one considers that most of the research centres listed in **Table 1** do market research for commercial institutions.

Topics Covered in Academic Publications

Even though sociology in Latvia is relatively small, the thematic range of sociological studies and research in which sociologists play a prominent part is fairly broad, so it would certainly be possible to identify examples of sociological work pertaining to a wide variety of topics and issues. For the purposes of brevity, I shall provide a general overview of the most prominent topics addressed in sociological work published by sociologists working in Latvia. In most cases the topics overlap with other areas of interest (e.g. oral histories address questions pertaining to national identity), and it should also be noted that, while the topics listed below are the most prominent in terms of the number of publications, the small size of Latvian sociology leaves open the possibility that these topics are representative of the interests of a few sociologists, rather than indicative of the research ethos characteristic of sociology in Latvia. Furthermore, my list is somewhat different from the one provided in *Socioloģija Latvijā*. For example, I will not discuss market research and studies of entrepreneurial activity (Žabko 2010). The reason for this is that most of the publications that the author refers to are research reports, rather than academic publications. The following is based on the information available in university home pages, library catalogues, personal CVs and various publication databases (e.g. Scopus, Web of Science).

Main (Local) Academic Outlets

In addition to book-length monographs or edited collections (e.g. published by Zinātne and University of Latvia Academic Press), sociologists in Latvia publish in a wide variety of academic journals, but there are a few prominent ones that I would single out. The first one is *Humanities and Social Science: Latvia*. It was founded in 1992 and 'reflects particularly on the changing situation in Baltic States and Eastern Europe, and the fast developing status of different research spheres there, as well as contribution of Latvian scientists to understanding of European problems' (home page³¹). Each issue is devoted to a specific topic, and, as the title suggests, it deals with questions of interest to many

³¹ <http://www.hssl.lv/>, accessed on 20/05/2014.

different disciplines. The journal *Ethnicity* is similar in that it contains articles written by practitioners of different academic disciplines, but the focus is much narrower – it only publishes papers pertaining to ethnicity and identity. The third outlet is a series of edited volumes called *Latvijas Universitātes Raksti* (*Scientific Papers, University of Latvia*). These are collections of academic articles that are open to all scientific disciplines, and in the last 20 years at least 5 volumes (629, 701, 714, 736 and 769) have been devoted to sociology. Finally, we come to the *Social Sciences Bulletin*, which is an outlet for academics working in the social sciences and is published every six months.

Ethnicity and Identity

A popular topic researched by sociologists in Latvia is ethnicity. Compared to other topics of interest to Latvian sociologists ethnicity occupies a rather prominent role in academic publications. In addition to a high number of published books and articles it has its own academic journal – the abovementioned *Ethnicity*. The range of questions explored in these publications is rather broad and covers issues pertaining to minorities, discrimination, education, integration, language policies and identities (both ethnic and national). Indeed, a prominent social anthropologist has suggested that Latvians are obsessed with questions of identity³². In view of this, I believe it is also appropriate to include research carried out as part of the state research programme *National Identity* under this rubric. Even though the scope of this project was broad and included a number of different pathways and researchers from different disciplines (e.g. history, economics, philology, sociology and politics), numerous papers dealt with questions pertaining to both ethnic and cultural identities.

Regional and Rural Development

Sociological research under the above heading can be classified as a series of disparate attempts to gain a better understanding of (i) social and economic processes and developments in provincial towns and rural areas, and (ii) study collectivities and forms of collaboration that have developed among farmers after the dissolution of the Soviet Union. The importance of Latvia's accession to the European Union cannot be overestimated in this regard, both in terms of the money available for regional development and the funding available to research projects interested in studying

³² <http://www.la.lv/varam-pretoties-ar-varciem-un-rakstiem/>, accessed on 27/02/2015.

socioeconomic development outside Riga (Šūmane 2010). Research in this area has produced publications on a myriad of different topics pertaining to the socioeconomic situation in small-to-medium-sized towns, agency and community-building, sustainability, employment, food supply chains and innovation in rural contexts. A prominent example of this is the work of Tālis Tisenkopfs, whose research on rural innovation and food chains is published in international journals (e.g. Tisenkopfs et al. 2011). What is more, the Baltic Studies Centre he leads was the only sociological research centre to attract funding as part of the 7th Framework Programme (Kunda 2013).

Migration

Interest in this topic is not specific to sociologists, but questions pertaining to immigration and emigration have become prominent in recent years. The focus is on understanding the causes of emigration and studying Latvian communities in other European Union countries in the context of re-emigration policies. A prominent example of this kind of research where sociologists play a leading role is a project on migration funded by ESF, the state budget and the Institute of Philosophy and Sociology, University of Latvia³³. In addition, a research centre³⁴ dedicated to this topic was established in 2014.

Sociology of Youth

This topic is rather diffuse and includes publications on a number of different factors influencing, and pertaining to, the lives of young people. Koroļeva, Rungule and Trapenciere (2010) argue that the interest in the lives and experiences of young people stretches back to the Soviet Union, and their overview of recent sociological publications shows that this interest has not diminished. The range of studies is quite broad and includes research on education, employment, social integration and exclusion, deviant behaviour, use of alcoholic and narcotic substances, and youth culture in general.

Oral Histories and Narrative Analysis

This is a somewhat controversial category for at least two reasons. Firstly, it is possible that Latvian historians and anthropologists would also lay claim to it. However, there are a number of publications where a prominent role is played by sociologists (Baiba Bela

³³ <http://migracija.lv/>, accessed on 01/12/2014.

³⁴ <http://www.diaspora.lu.lv/eng/>, accessed on 27/02/2015.

and Dagmāra Beitnere in particular) that place it equally under the purview of sociology. Secondly, it is based around a particular method, rather than a topic. However, the number of researchers in this field is relatively small and the publications are organised around the overlaps between identity, life stories and social memory.

Science Studies in Latvia

This topic is not popular among sociologists working in Latvia, but I believe it is necessary to mention it in view of the theoretical literature informing this project and the difficulties I encountered during my fieldwork. The presence of Latvian scholars on the international science studies scene is almost non-existent, but there is a significant amount of scholarship on issues pertaining to contemporary developments in science and technology. An overview of this literature is provided by Ādamsone-Fiskoviča (2011); although she is explicit about the fact that science studies does not yet exist as a distinct field of inquiry, it is certainly possible to identify publications that have dealt with issues that would be of interest to science studies scholars (Ādamsone-Fiskoviča 2011: 100). Traditional branches of science studies (such as history and philosophy of science) have a long presence in Latvian academia, and there are also examples of studies that have approached scientific communities in a manner reminiscent of traditional sociology of science (e.g. Vilciņš 1979). This includes a number of papers written on the topic of research ethics. In this context she mentions the work of a group of bioethicists working at Riga Stradiņš University. Two of these scholars hold doctorates in sociology (Vents Sīlis and Signe Mežinska), and their work is located at the intersection between bioethics (in which they both hold a Master's degree) and the sociology of health and illness (Sīlis 2010; Gefenas et al. 2010; Dranseika et al. 2010; Salmane-Kuļikovska et al. 2011). Another branch of science studies where sociologists play a prominent role is the study of various forms of innovation and the intersection between social forms of innovation (e.g. new partnerships), technological innovation and sustainability. Ādamsone-Fiskoviča argues that, in contrast to economists and natural scientists, sociologists have focused on the social dimensions of innovation and have primarily employed qualitative methods (Ādamsone-Fiskoviča 2011: 115).

Methodology and Fieldwork

The analysis of qualitative data has occasionally been treated as a problem 'because of the nature of qualitative data, which are invariably described as voluminous, unstructured

and unwieldy' (Bryman and Burgess 1994: 216). One of the issues that has attracted the attention of scholars is the intimate connection and interplay between data collection and data analysis, and my thesis illustrates this. My research was carried out within the framework of theory-based qualitative methodology. The primary method used in this study was the semi-structured interview, but the interviews were complemented by an analysis of sociological literature and textbooks published in Latvia (see above), a number of documents pertaining to the use of social science expertise in the context of policy-making (e.g. Kļaviņa et al. 2005; Latvija 2030) and funding reports. Further insight was provided by informal conversations with sociologists and anthropologists.

As I mentioned in the Introduction, the thesis began as an attempt to explore the heritage of Soviet science policy and attitudes towards sociology in present day Latvia. In particular, I was interested in the relationship between sociology and policy-making. After the first round of interviews the focus of my research gradually shifted towards sociology as a form of knowledge and the difficulties of approaching the discipline from a science studies perspective. This, in turn, affected my selection of informants (see below) and analytical focus. In short, the following two chapters deploy different kinds of data and relate these to the theoretical debates and issues fleshed out in the previous chapters. However, the questions I address and the resulting analysis took shape during the research process and in response to new information. The constructivist framework was present throughout the project, but its significance shifted in view of my conversations with sociologists working in Latvia – data gathering and data analysis were co-constitutive.

Selecting a Method

The decision to study sociologists in Latvia by way of semi-structured interviews was based on a number of factors. This format allowed the interviews to follow a predefined and comparable logic, but it also permitted me to ask additional questions and explore issues and experiences that were particular to, and deemed important by, the specific interviewee. In addition, I believed that semi-structured interviews were a good choice, since I intended to combine my informants' answers with other kinds of data (Fife 2005: 101; Atkinson and Hammersley 2007). This was well suited to the nature of the subject under investigation because only the most basic information was available in publicly accessible documents and publications. The philosophies of science practised by

sociologists and their understanding of the discipline could not be clearly identified by reading their publications, and an undue focus on such texts would have run the risk of presenting an overly idealised picture of their views on sociology as a form of knowledge and the philosophical assumptions underpinning it. What is more, given that sociology in Latvia is primarily applied in nature (Tisenkopfs 2010b), there was a high probability (verified upon further analysis) that these publications would not dwell on technicalities, and that the philosophical assumptions would be implicit and, therefore, hard to identify clearly. This, coupled with the fact that I had little knowledge of the theoretical preferences of sociologists working in Latvia, suggested that loosely structured conversations with the practitioners would (i) allow me to gain a better understanding of the importance they attach to the issues of interest to me and (ii) allow them to challenge my assumptions and way of framing questions.

Selecting Informants

When selecting my informants, I employed a strategy that combined elements of purposive (because the number of people with the relevant knowledge was small) and expert (because the required information is highly specific) sampling. Choosing informants presented few difficulties, but selecting and narrowing down the target group of experts was based on a number of considerations that derive from the aims of my research and have to be made explicit (Littig 2009: 103).

Even though the community of sociologists in Latvia is relatively small, interviewing every single sociologist in Latvia would have been impractical, so I limited my attention to sociologists with a doctorate³⁵. The decision to focus on sociologists who held a doctorate in sociology was based on two considerations. First and foremost it was motivated by expediency – this was an easily identifiable group whose defining characteristics could be stated unambiguously and in non-trivial academic terms. A potential drawback was that this excluded scholars who work with sociologists, employ concepts derived from sociological publications or whose research is sociologically pertinent, but who were not trained as sociologists. This objection can be countered by considering my second motivation for limiting my attention to sociologists with doctorates – I believed that a doctorate in sociology was a solid indicator that the

³⁵ Almost all of my informants had obtained a doctorate prior to the interview. The only exception was a sociologist who had not finished her studies, but was interviewed when the sample population was broader and included sociologists working in prominent research centres.

scholar's knowledge of, and proficiency in, sociology was (i) institutionally acknowledged and (ii) extended beyond professional skills as a researcher. This criterion justified the exclusion of scholars who practised sociology but held doctorates in other disciplines (e.g. economics and philosophy). Finally, a doctorate in sociology suggested that the scholar would have a solid grasp of the issues I wanted to discuss in the interview. In view of the above, the data I obtained from my informants cannot claim to be representative of all forms of sociology practised in Latvia. However, it represents the views of those individuals whose abilities and expertise as sociologists have been certified by the Latvian academic establishment.

The interviews were conducted in two waves. The first 12 interviews took place in July-August 2012, and a further 10 during July-September 2013. Due to a change of focus after the first wave, I decided to re-interview some of the sociologists whom I had already met in the summer of 2012. I re-interviewed a total of 6 sociologists. The analysis in Chapters 6 and 7 is based on a total of 28 interviews with 22 sociologists. The list of sociologists was obtained via personal communication, from book chapters and articles on the history of Latvian sociology (e.g. Tabūns 1996; 1998; 2010) and the databases of the only two universities in Latvia that grant doctorates in sociology – University of Latvia³⁶ and Riga Stradiņš University³⁷. The informants were contacted by email.

The Interview Guide

The first section of the interview guide³⁸ focused on epistemological and ontological preliminaries pertaining to sociology as a form of knowledge, whereas the second section dealt with the role and value of sociology in the context of politics and policy-making. In practice, however, the sections often overlapped, but this was to be expected, as semi-structured conversations seldom evolve in the same way and not all of my informants focused on the same issues. What is more, some of the interviews were hampered by time constraints and my informants' hectic schedule, which meant that three of the interviews had to be truncated, so I had to drop questions that I thought (i) were inessential or (ii) had already been answered (albeit indirectly).

When designing the interview guide, I brought a number of assumptions into play. I treated sociology as a diffuse activity whose stability and coherence has to be enacted and

³⁶ <https://luis.lu.lv/pls/pub/wct.doktd?l=1>, [accessed on 28/05/2014].

³⁷ <http://www.rsu.lv/petnieciba/promocija/aizstavetie-promocijas-darbi>, accessed on 28/05/2014.

³⁸ See Appendix 1

maintained through its institutional foundation, adherence to a specific theoretical cannon and forms argumentation, the deployment of a particular methodological repertoire and the right to speak of and for the variables, interactions and associations constituting the social. The assumptions and analytical parameters I have outlined above derive from the arguments I discussed in Chapters 1-4. I subscribe to a constructivist theory of science, and I recognise the social and normative embeddedness of all forms of inquiry. I proceed on the assumption that scientific explanations, techniques and instruments are actively involved in constructing enactments of reality with normative implications, but the ability of different disciplines to do so is contextually varied. For example, sociological studies present versions of reality that posit the existence of certain entities and phenomena, describe the relationships between them, and establish different forms of association between heterogeneous actors. The success or veracity of a particular piece of research, however, is subject to negotiations between different parties. I, therefore, accept the insights of constructivist science studies and proceed from the belief that knowledge is contested and emerges from struggles and interactions between various perspectives and different factors. Such an approach presumes that the cogency and persuasiveness of an argument depends on how well it meshes with or successfully transforms pre-existing sociomaterial configurations. That is to say, the quality of a piece of research needs to be collectively established and should not be treated as if it were something that is intrinsic to it. The reality of its quality is procedurally constituted. The specificity of sociology, however, is to be found in its relative weakness and inability to achieve the abovementioned on a regular basis.

The aim of my fieldwork was to derive an account of sociology from the arguments offered by Latvian sociologists and relate it to the debates and issues addressed in the previous chapters. For this reason I chose to pose questions that, I believed, were fundamental to an understanding of sociology. However, I had to take into account the specificities of my own theoretical outlook and research interests. This meant that I had to pose my questions in a way that was sufficiently open-ended and articulated in a language that was comprehensible to scholars whose training and theoretical background differed from mine. This gave my interviews an ethnographic dimension (see below), and I had to invoke characterisations of sociology that I personally did not subscribe to in order to facilitate the conversation. In cases where my questions enacted traditional dualisms (e.g. natural vs. social sciences, quantitative vs. qualitative methods) I presumed

that my informants would challenge or correct me if they felt that my way of setting up the problem was inaccurate or contextually inappropriate (Murchinson 2010: 91). My choice of method and interviewing strategy, therefore, worked with an implicit model of what my informants would be like – active and confrontational, rather than passive and accommodating. Given that my informants were senior to me and professional researchers and educators with considerable experience in carrying out academic projects, I believe that my choice was justified.

Ethical Issues

This project received ethical approval from the Lancaster University Research Ethics Committee in July 2012. Interviewees were asked to sign a consent form, informed about the uses to which the data will be put and given an accurate description of the project and its aims. The interviews were recorded and kept on an encrypted external hard drive. All recordings were anonymised, and all participants were given a pseudonym. However, as I have already mentioned, the sociological community in Latvia is quite small. This means that a possible issue (partially confirmed in informal conversations) might be that it will be relatively easy for the informants to identify themselves and, more importantly, others in the final version of the thesis. That is to say, the use of pseudonyms might prevent outsiders from identifying a sociologist, but this will not necessarily preclude insiders from identifying their colleagues (Platt 1976). In an attempt to prevent this I (i) refrained from using quotes or information that could identify specific sociologists and (ii) used gender-neutral pseudonyms.

Reflections on Possible Limitations

Jennifer Platt argues that a significant feature of interviewing other sociologists (or one's peers in general) is the existence of a common stock of background knowledge and shared understandings that shape the interaction (Platt 1976; 1981). Some of the difficulties I encountered during my research, however, derive from the opposite problem. Prior to my research visit in July 2012 my knowledge of Latvian sociology was limited to reading an introduction by Pēteris Laķis (2005), analytical commentaries by Tālis Tisenkopfs (2010b) and Aivars Tabūns (1998; 2010), and examination of theses and journal articles written by Latvian sociologists. Therefore, while I had a general understanding of the literature published in Latvia, I was not well versed in the academic culture characteristic of Latvian sociology. This, I believe, goes some way towards

explaining why my first interview went worse than I had anticipated and why I was forced to rephrase most of the questions during the interview and rewrite them for the purposes of all subsequent interviews. Some of this was undoubtedly due to my limited experience as an interviewer. I found that I kept revising the interview outline as I went along and tried to come up with examples for the questions my interviewees found most confusing. What is more, my initial encounters made me acutely aware of the specificities of my own academic upbringing and a number of taken for granted assumptions (see previous section) which necessitated fundamental revisions to both my questions and the language I used when raising objections. For example, I dropped references to performative approaches to ontology, refrained from discussing the persuasiveness of academic publications in Latourian (Latour 1983; 1993b) terms and offered tentative definitions of what I meant by the predicate objective. Additional confusion may have been the result of my asking questions that may have appeared naïve (e.g. What is the object of sociological inquiry?).

I would suggest that there are at least three possible reasons for this. Firstly, it could be an indicator of just how deeply ingrained and foregone these questions are for an experienced sociologist. Secondly, the confusion may have been caused by their assuming that I was trained in sociology and, therefore, well aware of the answers they could and would give. That is to say, the questions appeared odd, because the answers were obvious. This suspicion was confirmed on a number of occasions when my informants distinguished between answers they would give to academics and those they would offer to curious outsiders. In other words, my informants assumed that they were talking to a fellow practitioner and could take certain things for granted (Pfadenhauer 2009: 85; Platt 1981). Thirdly, as I indicated before, sociology in Latvia is primarily applied in nature, which means that theoretical debates are not a common practice in sociology. Consequently, my informants may simply not have been used to discussing these sorts of questions and unsure about the pertinence of these aspects of sociological work to an understanding of sociology in Latvia. Finally, my identity as an outsider exploring questions that were uncommon in the Latvian sociological landscape may have created additional suspicion. I did, however, try to address this by (i) not hiding my ignorance and (ii) encouraging my informants to correct me if they felt I focused on aspects that they believed to be unimportant for understanding sociology in Latvia; this, coincidentally, is why I prefer to use the word informant (rather than respondent) when referring to the

people I interviewed. Nonetheless, bafflement persisted no matter how explicit I tried to be about the naïve nature of the questions.

As my fieldwork progressed I realised that the informants found my questions baffling for slightly more complex reasons, some of which had a clear impact on our interactions during the interview.

Firstly, while I tried to describe my project (both in the email I sent them and information sheet I provided with the consent form) as clearly as I could without giving away everything I was interested in, some seemed to be under the impression that I was interviewing them as experts on Latvian society in general and sociology in Latvia in particular, rather than people who just happen to be members of a particular professional group. This manifested itself in raised eyebrows and astonishment in response to questions from the first section of my interview guide, e.g.

How does the work of a professional surgeon differ from, say, the work of amateurs in villages where old ladies want to cure something?

Dagr

This question is... peculiar.

Isyllus

Sociologists felt that I would be better off consulting introductory textbooks and course notes intended for first-year students. This was also evident in their refusing to speculate about topics on which they felt they did not possess sufficient information. In cases where they did speculate, they felt the need to let me know that they were in fact speculating. While this was quite evident in the first interviews, it gradually disappeared. I would argue that this was the result of me stressing the fact that I was not interviewing them as experts but as people who are involved in a particular activity. I also included this disclaimer in the second wave of emails because I thought that this might assuage the doubts of sociologists who felt that they had nothing concrete to contribute on the topics I was interested in.

Secondly, a further reason may have been role reversal. With the exception of one interviewee who had left her doctoral studies due to professional commitments, all of my informants held a doctorate, had at least some teaching experience and considerable research experience. In the context of the interview, however, they were the ones who had to answer exam-like questions with an inexperienced student posing follow-up questions and urging them to elaborate and unpack their answers. I feel it is necessary to

mention that role reversal may have had an equally strange effect on me. I was fully conscious of the irregular nature of the situation and painfully aware that I was interviewing someone who was higher up in the academic hierarchy (Platt 1976; 1981), which resulted in sporadic spells of anxiety and inability to pose my questions clearly. Thirdly, there was a discrepancy between my informants' conceptions of sociology and mine. The background assumptions on the nature of sociology and sociological knowledge listed above contrasted sharply with the beliefs implicit (and sometimes made explicit) in the responses provided by Latvian sociologists who treated sociology as a profession and in general did not seem concerned with the issues I discussed in the previous chapters. I would, for example, hazard a guess that they would have found it more sensible to study sociology as a profession and institution, rather than an academic discipline. In other words, there is a sense in which my project may have missed the mark, at least as far as my informants were concerned, and worked with a peculiar understanding of what was relevant (Pfadenhauer 2009: 85), though this may have had something to do with the unclear relationship between science studies and sociology.

Sociology and Science Studies: An Awkward Encounter

I am approaching sociology in Latvia from a science studies perspective and using the former to illustrate possible solutions to, and explanations for, the problems and frictions I discussed in the preceding chapters. A potential objection, however, might be that this project is an instance of a discipline examining itself with its own analytical apparatus – a sociology of sociology. One way to counter this argument would be to point out well-known examples within the sociological tradition that have attempted to study the discipline on a much broader scale (e.g. Cole and Zuckermann 1975; Friedrichs 1970; Gouldner 1970; 1973; Mullins 1973). This would leave the problems of self-reference and recursivity unaddressed, although this is characteristic of most projects in the sociology of scientific knowledge (Ashmore 1989). A more productive approach, I believe, is to reframe the relationship between science studies and sociology (Law 2008) and so reflect on how the problem of reflexivity applies to this particular project.

My point of departure is that science studies uses methods, insights and background assumptions from sociology, but it borrows equally from anthropology, philosophy, feminism and cultural studies (Fuller 2006b). In other words, it would be problematic to deny the link between sociology and science studies, but it would be equally tricky to

define the role of science studies in relation to sociology. This project proceeds on the assumption that, while one could make the case that science studies is related to sociology, the theoretical repertoire that I draw upon in this thesis is hybrid in nature. This is not just a question of boundary-drawing (Gieryn 1983) and relates directly to the explanatory approaches discernible in the science studies' accounts that I employ.

The specificity of sociology, however, complicates the distinction between subject and object. While there is a long tradition of the social sciences and humanities using concepts borrowed from the natural sciences in a figurative way (Fuller 2006; Mirowski 1989; Stengers 1997), the perceived difference between these two ways of knowing has made it *de facto* acceptable for science studies to examine the natural sciences (at least as far as science studies scholars are concerned). The social sciences (as objects of study) have been more problematic. There have been attempts to approach the social sciences from within a broadly constructivist framework (Latour 2005; 2010; Law 2004; 2009; Law et al. 2011). I would argue, however, that these efforts are different from traditional sociology of knowledge as they do not try to provide a distinctly sociological explanation of trends within sociology. I would suggest that this is, at least partly, because the latter have been obsessed with their own status as sciences (Fuller 1991; Law 2008), but of equal importance are the vast number of publications dealing with the social and philosophical genesis of sociology (e.g. Gouldner 1970; 1973; Halsey 2004; McCarthy 2003; Turner 1999; Zeitlin 1968), and the issues raised in the last section of Chapter 4. In other words, sociologists have (i) often taken a historical and meta perspective on their activities (Ritzer 2011) and (ii) been conscious of the status of sociology as a form of knowledge that is simultaneously in and about the world (Fuller and Collier 2003).

The conceptual ambiguities alluded to above, and the relationship between science studies and sociology they illustrate, have a geographic dimension to them. That is to say, the link between these two fields varies greatly depending on context. For example, in the United Kingdom there is overlap between sociology and science studies: there is an official study group in the British Sociological Association, as well as a number of university departments where science studies has a significant presence (e.g. Lancaster, Edinburgh, Cardiff). I do not mean to imply that either of these fields is well defined - only to suggest that there is communication and exchange between the two.

This is not so in Latvia where the only academic unit that could be classified as belonging

to science studies used to be located at the Latvian Academy of Sciences³⁹. Indeed, the distant and evasive relationship between sociology and science studies is exemplified by a doctoral thesis submitted in 2013. The thesis was about the perception of environmental risks in Latvia, and it also addressed the socialisation of nature (Briška 2013). The theoretical framework for this study, however, was based on the work of Niklas Luhmann, Ulrich Beck and Anthony Giddens – it made no reference to science studies (or science and technology studies). This, I believe, goes some way towards illuminating some of the issues that I faced during my interviews. My informants were expecting a conversation with a sociologist, and some of the friction generated during the interviews may have been due to the fact that we were operating with diverging interpretations of what was pertinent for an account of sociology in Latvia, or sociology in general. That is to say, the interview situation itself exemplified the unclear relationship and divergence between sociology (represented by my informants) and a particular branch of science studies (represented by me), and illustrated an issue and limitation specific to interviews with practitioners of the social sciences and humanities. That is to say,

[i]nterviewing sociologists about matters germane to their own discipline is a highly rewarding experience. [...] But sociologists, being constantly aware of values, social relationships, ideological distortions, etc., tend to give answers which are more than mere statements of fact. This is a strength and a weakness of my material at the same time because the answers are almost invariably as much statements of fact as interpretations of these facts in the light of the respondents' own theories.

(Faludi 1978: 104)

Conclusions

The specificities of sociology in Latvia are a source of potentially interesting insights for, and challenges to, the science studies literature that this project draws upon. Some of this has to do with the history and structure of the sociological profession in Latvia. In particular, its emphasis on applied, externally funded research serves as a useful platform for a contrast between the theories of Bruno Latour and Pierre Bourdieu, and the interplay between autonomy and heteronomy more generally. However, the distinctiveness of sociology and sociologists as objects, and the peculiar relationship between sociology and science studies that my conversations with Latvian sociologists illustrated are of equal importance. My contention is that such awkward encounters between cognate fields of inquiry, while subject to a series of difficulties and limitations, are conducive to a more detailed understanding of how different disciplines position

³⁹ http://ww3.lza.lv/csts/csts_main.htm, accessed on 28/05/2014.

themselves in relation to their objects and their responsibility for the knowledge they provide. Furthermore, the idiosyncrasies of sociology are useful points of reference when articulating and challenging the background assumptions of a constructivist theory of science. It is with this mindset that I approach the interviews with sociologists working in Latvia.

6. Conversations with Latvian Sociologists, Part II: Sociology as Science, Sociology as Profession

Scientific education as we know it today has precisely this aim. It simplifies “science” by simplifying its participants: first, a domain of research is defined. The domain is separated from the rest of history [...] and given a “logic” of its own. A thorough training in such a “logic” then conditions those working in the domain; it makes their actions more uniform and freezes large parts of the historical process as well.

(Feyerabend 2010: 3)

In the sciences, as I shall suggest below, it is often better to do one's best with the tools at hand than to pause for contemplation of divergent approaches.

(Kuhn 1977: 225)

The above quotes hint at something peculiar about science and science education in particular. It proceeds by isolating a part of reality about which you, as a member of a particular discipline, are qualified to speak and whose nature and intricacies you are expected to explain. For Paul Feyerabend, however, this comes at a regrettable price – uniformity and rigidity. These characteristics, he believes, are antithetical to the inquisitive spirit animating science – it needs diversity to foster methodological creativity and conceptual divergence. To a certain extent, therefore, conflict is what drives science, whereas norms stifle it and repress its vitality.

Even a cursory examination of the natural sciences would reveal that there is never perfect consensus and theoretical disputes have a constant presence (e.g. phyletic gradualism vs. punctuated equilibrium in evolutionary biology). What is more, even in cases where scientists seem to be in agreement over the feasibility of a particular theoretical approach, a closer inspection might reveal that surface similarities obscure highly divergent interpretations and practical applications of the same theory (Fuller 1986; 2002: ch. 9). However, I would argue that this issue is much more pronounced in the case of sociology. There is rampant disagreement as to the nature of the object of sociological inquiry, the proper way to study it and the normative implications of the resulting knowledge (e.g. critical theory). The consequence is a myriad of different approaches that do not always get along and reinterpret their opponent's turf with their own concepts and metaphysics (Abbott 2001a). Furthermore, as I tried to show in Chapters 3 and 4, there is internal friction as to what ends sociology should serve and a perpetual reinterpretation of its own history and heritage in view of this. What I am suggesting, therefore, is that, while there is diversity and theoretical conflict in the natural

sciences, it is rather more pronounced in the case of sociology. Furthermore, such disputes are an integral part of the discipline – sociology is openly heterogeneous as regards both methodological and axiological questions. This, however, is antithetical to Kuhnian normal science, since diversity leads to different factions competing with one another, which in turn means that a lot of energy and resources are expended on theoretical altercations. I would argue that such internal heterogeneity is at least partially responsible for the epistemically ambiguous status of sociology, which is illustrated well by sociologists working in Latvia.

The basis of this chapter are the answers to the first section of my interview guide, which mainly dealt with the nature of sociology as a science and the specificity of the experience and expertise possessed by sociologists (as opposed to the lay public). In this chapter I illustrate (i) the perceived complexities that are involved in positioning sociology as a science and (ii) the various argumentative strategies employed by sociologists to articulate their position.

What is Sociology?

In 1996 the sociology *cathedra* at the University of Latvia published a sociology textbook aimed at high-school students (Zepa and Zobena 1996)⁴⁰. This was supplemented by a dictionary that was made available the following year (Zepa and Zobena 1997)⁴¹. Ostensibly the aim of these two books was to give students an overview of the topics and issues that they would have a chance to explore should they choose to study sociology at university. However, in conjunction with a more advanced introductory course published by Pēteris Laķis (2005)⁴² they can also serve as a point of departure when trying to ascertain how sociologists in Latvia construct their own discipline.

All three books provide a definition of sociology, but what is striking is that both *Humans and Life* and *Introduction to Sociology* proceed by explicitly discussing the difficulties inherent in defining sociology. For example, in the introductory chapter to *Introduction to Sociology* Pēteris Laķis makes the following claim:

In trying to outline the specificity of the sociological outlook on society it is important to bear in mind that the object of this form of knowledge is the relatively broad, though not always conceptually delineated, self- awareness of human culture. [...] In a situation

⁴⁰ Hereafter – *Humans and Life*

⁴¹ Hereafter – *Dictionary*

⁴² Hereafter – *Introduction to Sociology*

when the conceptual apparatus of sociology is still in the process of hierarchical arrangement, yet the empirical focus becomes ever broader, the specificity of this field of knowledge is defined in a rather general – even abstract – manner.

(Laķis 2005: 7)

Something similar, though in a more muted form, is present in the books edited by Brigita Zepa and Aija Zobena. The books provide long and detailed definitions of sociology that situate it in relation to economics, philosophy, political science and anthropology, which in the case of *Humans and Life* is supplemented by a two-page discussion of the historical difficulties that sociology has faced in trying to establish itself and define its area of expertise. An awareness of this complexity was also evident in some of my interviews.

This is a very difficult question... The more you know, the harder it becomes to explain it in simple terms... I've tried it in my capacity as director of undergraduate studies... in very general terms [...] society as a whole, how it functions, how it works at a very general level.

Freya

I've always had a problem with these... What is the object, what is the focus... Gosh, I'm ashamed when I remember how many years I've been teaching students, and I try to avoid these questions.

Fulla

However, the overall framing of sociology reveals a striking detail inherent in the rather mundane descriptions of sociology. For example, *Humans and Life* starts with an ambitious claim.

We need sociology if we wish to understand our world. It is a science that provides a spiritual model of its time. Contrary to most other sciences that gives us insight into the world, sociology is reflexive: it is a mirror in which society can see itself. Even though sociology thinks in theoretical and abstract terms, it is always in dialogue with society.

(Zepa and Zobena 1996: 5)

Straight away sociology is defined as a form of knowledge whose relationship to its object is interactive. This sentiment is echoed in *Introduction to Sociology*.

The task of sociology is not only to explain social reality in theoretical terms and predict possible trends and alternatives for future development. It has other more practical functions as well. Sociological knowledge can serve as the foundation for expert assessment at all levels of public policy.

(Laķis 2005: 3)

This complicates things somewhat. While both books seem to be implying a bidirectional feedback loop between sociology and its object (society), they seem to be equally aware of the remarkably diffuse and nebulous nature of sociology. For example, the *Dictionary* starts its definition by saying that sociology is 'the scientific study of society' (Zepa and

Zobena 1997: 10). This is followed by a rather broad account of sociological inquiry. In and of itself this seems unproblematic. Viewed in conjunction with *Humans and Life*, however, both the nature of sociology and its object are revealed to be categories that have been, and still are, subject to revision and historical change. This also extends to society, which all three books posit as the object of sociology.

Let us compare two definitions of society.

Society is a more or less organised unit of individuals, social groups and forms of organisation. These relationships manifest themselves both vertically and horizontally. The vertical dimension is expressed through the workings of power, whereas the horizontal is expressed through forms of competition and cooperation. Both dimensions form the existential basis of social organisations, institutions and forms of sociality, while at the same time determining the social existence of the individual.

(Laķis 2005: 8)

Society is a territorially bound social system that contains within itself both individuals and social groups. It is believed that members of a particular society share a common culture, more or less universally accepted norms, values, social roles and institutions. By the way, some proponents of symbolic interactionism argue that there is no such thing as society; it is just a term we use when we talk about things with which we are not well acquainted.

(Zepa and Zobena 1997: 15)

These definitions are comparable and, lip service to symbolic interactionism aside, imply remarkably similar ontological assumptions as to the nature of society. Society is constituted both by its members and the relations that exist between them. This is echoed in the responses of the sociologists I interviewed.

Society is an aggregate of individuals who live by both written and unwritten norms in some sort of system... how this system works, how it is maintained, and what conflicts emerge and how they are resolved so that the society in question can function.

Eir

Society consists of people and relations of communication that have been institutionalised... these relationships have been institutionalised, reproduced etc. It's society as a system.

Dagr

There are many levels. One... the easiest way would be to say that society consists of its members...both at the national and maybe global level, but in this case we also have different... structures and structural units that contain within themselves various social forms. For example, organisations and different social groups.

Nanna

However, when asked to give an explanation as to what constituted the object of sociological inquiry, not everyone opted for society; some even questioned whether such a thing as society actually existed.

Society... that's a complicated notion. First of all, there is no such thing as society. It's convenient for us to talk about it. It's convenient to be able to use some sort of blanket term... but there is a way of looking at people as a society.

Mimir

It [sociology] defines itself as a science that studies... action... to put it simply. It studies groups – both big and small – within society, because... the notion of society is an abstraction, wouldn't you agree?

Saga

On the whole, however, there was a palpable sense of uncertainty and reticence about the way the questions were answered. There was little consensus as to what exactly constituted the aspect of reality of which sociologists were qualified to speak and which they were qualified to study, though society and the elements that constitute it was a prominent theme. This is hardly surprising given that most sociologists do not regularly concern themselves with questions pertaining directly to the foundations of their discipline. What is more, this is even less remarkable in view of the fact that in Latvia practitioners treat sociology as an applied discipline. Consequently, it is quite reasonable that a number of my respondents focused on smaller questions.

You see, on the one hand there's the analytical approach – ways of differentiating sociology from the natural sciences. That's one question. The other question is about research. Studies always focus on particular issues and only focus on one aspect... only one aspect. As regards society as such... there is no sociological study that, how shall I say, had as its objective the study of society as a system.

Dagr

In and of itself this is hardly noteworthy. It is likely that practitioners of other disciplines would be equally fazed when prompted to explain what the object of their discipline was. There is little reason to assume that natural scientists would readily produce an articulate and precise account of which aspects of reality they are trying to understand. Furthermore, it is not a given that these responses would give us a straightforward picture. However, in the case of sociology this uncertainty goes deeper, because, as the quotes below indicate, my respondents generally opted for responses that resisted simplification and obvious rhetorical potency.

Latvian sociologists had issues defining the epistemological and ontological parameters in which sociology operates. For example, the responses showed considerable variety as regards the nature of sociology. A number of my respondents freely acknowledged the epistemic hybridity of sociology and were explicit about the thematic overlaps with philosophy, political science and anthropology.

Sociology is a hybrid. Something from political science, something from mathematics, bits from philosophy... and it all sums up. I think... sociology is interdisciplinary at its core, but it's given itself a frame.

Eir

If we look at how the sciences are classified... There is a considerable amount of overlap. It is probably greater than people acknowledge.

Delling

This is amplified by the peculiar and historically emergent nature of its object. For example, in *Introduction to Sociology* Pēteris Laķis argues that the emergence of sociology was contingent upon a group of individuals recognising itself as a unit with complex internal dynamics (Laķis 2005: 5-6). As a result of the clash and discrepancy between ideas of human progress and unity, and the glaring social problems and countervailing tendencies, people became aware of the fact that there was something about social groups that was not reducible to their constituent elements. This required systematic exploration, so questions pertaining to the nature of the bonds that bind people together became part of science. However, as Zepa and Zobena note, the force exerted upon, and the compulsion experienced by, individuals has always been a tricky issue in sociology (Zepa and Zobena 1996: 6-7). My interviews illustrated that Latvian sociologists were painfully aware of this. For example, in cases where this issue was brought up, most of the respondents believed that societies have particular inertias and internal logics which are specific to them and allow these societies to reproduce themselves. However, they were reticent and evasive when it came to discussing the problem of the structural determination of agency.

It's the old dialectic, right?

Narvi

In some cases a balance was sought – structure and agency were integrated and worked side by side. For example, it was argued that the internal dynamics of a social system are 'the result of a clash between different forces', which, in turn, is the result of a 'plurality of causes'. The dominant trend, however, was to emphasise the subtle nature of social determination. But even if the respondent emphasised the socially patterned nature of practices, some reference was made to the creative and agential capacities of individuals. Some respondents emphasised the role of structure. Others were more optimistic about the power of agency, but both versions implied some sort of dialectical relationship. There was no mention of society as an inherently deterministic system (though there was talk of systems – see above) or an entity *sui generis*. At best, societies were regarded as

systems of norms and ideas that (i) are reproduced through practices and (ii) exist in the form of these practices and the ideas/norms that govern them. This suggests that a certain amount of ambiguity vis-à-vis its object is fairly explicit and the practitioners themselves seem to be aware of this. This becomes even more apparent when we look at their opinions on natural science.

Identity and Difference

The juxtaposition of sociology and the natural sciences is a fairly traditional one, and Latvian sociologists employed different strategies to deal with this question. One was to emphasise that the sociology did not command great respect in the eyes of the general public, but this did not make sociology less scientific.

There is, of course, a stereotype that the social sciences are easier than the natural sciences, but... In my opinion that is only an assumption, because, like I said, if you wish to be knowledgeable in any scientific field, it makes no difference whether it's the natural sciences or the social sciences. Maybe the question is... the person... what he likes. Some people might prefer working in a laboratory... alone. Others might prefer communicating with people.

Borr

It was acknowledged, however, that sociology is more interpretive and the research process is fraught with uncertainty.

Sociology differs from the natural sciences in that humans are determined socially as well as subjectively and psychologically. What is more, all this changes as time goes on.

Kvasir

The natural sciences appear to be more precise. The laws they posit cannot be disputed as easily. In the social sciences we have interpretations, a greater variety of situations and factors....

Forseti

In the social sciences... the fact that we're studying people means that there will be problems with replicability... Of course we can't conduct experiments... This makes the knowledge... it is much more subject to various contingencies... and this makes them more interpretable...

Weth

A related view (that often overlapped with the first one) pointed to the dynamic and fluid nature of social forms. This had two consequences. The first has to do with nature of the research process itself – it is subject to various contingencies that the researcher cannot fully control for and eliminate. The respondents generally believed that this was not caused by the methodological immaturity of sociology. Rather, it was the peculiarity and complexity of its object that defied a clear articulation of it.

God damn! If your object was this complex... you'd screw up as well.

Delling

The second was that the natural sciences were much better at controlling and predicting the behaviours of their respective objects.

The complexity of the mechanisms means that the knowledge becomes increasingly more probabilistic in nature.

Var

On the whole, however, my respondents did not agree that sociology or the social sciences in general were less scientific. By far the most popular strategy was to challenge the distinction by pointing to the increase in uncertainty in the natural sciences and intimating that the differences in scope, rigour and explanatory power were more apparent than real. They recognised that, in terms of reputation at least, the social sciences were a distant second to their cousins in the natural sciences. Forms of knowledge with a more pronounced quantitative dimension enjoy higher public esteem, but a number of sociologists indicated that these were simply better at concealing the interpretive and contingent elements. This does not make them more scientific.

The differences seem fairly apparent at first, because it is easier for the natural sciences to identify their object and it is assumed that in the natural sciences the object isn't as changeable [...] as society [...]. I did a course as an undergraduate. It was called...philosophy... physics... philosophical issues in physics [...], and the impression I got was that the differences are much less pronounced than we are inclined to think.

Fulla

This suggests that something peculiar is going on. Sociologists freely acknowledged that the exact nature of their research object was somewhat ambiguous and the research process reflected this. They were also fully aware of the disparity in terms of reputation between the natural sciences and their own field. What they refused to accept, however, was that this is somehow due to the feebleness of sociology. There are differences, of course, but these are not qualitative differences that would make it justifiable to assign the social sciences to a lesser epistemic order. How, then, to explain this disparity in reputation?

I think that the social sciences are... are finding it more difficult to be scientific... It is easier for the natural sciences, because the way it is set up. You know, ideas about what it means to be useful... It is deeply rooted in the minds of individuals and society in general, but... Well, the social sciences ... There is a great variety of opinions and debates, and this makes people... Well, they don't find it convincing and they don't see of what use it is.

Nanna

A seemingly pertinent point of reference in this context would be my respondents' views on whether sociological knowledge can claim objectivity, but this, again, turned out to be more complicated than it would appear at first.

The question of objectivity in the social sciences has a long and illustrious history, but a prominent theme in my interviews was that this dichotomy (subjective/objective) was not a useful one.

In my opinion this distinction adds very little to our... understanding. It is more appropriate to talk about... well... classical.... procedural norms. And to say that this makes it objective... I don't know.

Delling

Questions about objectivity are fundamentally naïve – these are questions about the researcher.

Tyr

A possible explanation for this is that this dichotomy is polysemous, which means that the predicates subjective and objective imply slightly different things depending on the context in which they are placed. My respondents were fully aware of this and either asked me to clarify what I meant or provided a definition themselves.

You have all these methods... that, well, help to... I don't know. Objective in relation to what? The methods we use allow us to understand to what extent and what opinion... well, which opinion is being represented with the methods we are employing. You know, what the sample is and how representative the data are... That's what we can say.

Nanna

However, as with the previous questions, the overall situation was messy. For every response that hinted at the possibility of objectivity there was a counter-position that challenged this belief on those very same grounds. For example, some sociologists located the source of objectivity in the proper use of methods (see above). But there were others who were quick to point out that this is not as straightforward, because someone has to interpret the data.

Now, while there were instances where interpretative work was treated as a potentially contaminating factor, interpretation itself was considered a key part of the process. I pursued this issue further by discussing the differences between qualitative and quantitative research. The responses were tentative because a number of my respondents said that they were proficient in the use of either qualitative or quantitative methods but not both. Consequently, even though a number of the sociologists I interviewed preferred mixed methods research, I suspect that they would claim expertise of only one

set of methods.

A prominent theme in the responses was the reticence to claim that quantitative studies were superior to qualitative studies. For the purposes of clarity I defined objectivity in terms of observer-neutrality. That is to say, I asked my respondents whether quantitative methods were better at limiting the effect that the researcher had on the research process. The responses were varied. Some believed that quantitative studies, if done correctly, were more objective than qualitative studies.

It is very easy to slide into subjectivity when you are using qualitative methodology.

Freya

Qualitative methods give you greater interpretive flexibility and a range of different paths you can follow. Quantitative methods arrange everything in an orderly fashion, but I think that quantitative studies become more interesting when they can show us what's behind this order.

Var

As the second quote suggests, however, this did not necessarily result in the studies being of higher academic value, because the importance was still placed on the interpretive skills of the sociologist. The interpretive dimension was also prominent among those who felt that quantitative methods were simply better at hiding the subtle ways and methodological choices with which the sociologist affects the course of the research process.

Again, this is where qualitative research differs from quantitative research. Where...where you have quantitative methods and mathematical methods you can hope for a quantitative... well, a more objective use of methods and results, but... but... I like this one idea that I've heard in a different discipline, unrelated to sociology, is that... For example, how we assess the risk involved in particular projects etc. - qualitatively and quantitatively. But the quantitative is also based on human evaluations.

Narvi

I don't think that quantitative methods are a sign of objectivity, because your choice of method has an impact on what the results are going to be... definitely.

Kvasir

Theoretically there is a decrease in subjectivity, but... you can still influence the data and there is a certain amount of interpretability. On the one hand... The results themselves... they represent something, they are somewhat objective. The question is – how are they interpreted?

Borr

This, however, became relevant when considering public perception of sociology.

Quantitative methodology has chosen the broad application of mathematical statistics as

the criterion of objectivity for the realisation of the ever important scientific principle. Formalisation and quantification are characteristics that must guarantee the successful functioning of this methodology in the development of sociology by building a knowledge-foundation appropriate for the tradition of scientism.

(Laķis 2005: 14)

As the above quote suggests, there is nothing surprising about the rhetorical force of quantitative studies, not least because they can draw on the authority possessed by the natural sciences. The family resemblance that exists between quantitative sociology and the natural sciences means that it is easier for quantitative studies to pass as science, because, as one respondent puts, 'the rules are much clearer' (Delling). In other words, it is easier for quantitative studies to appear scientific, because they have the trappings of what people take to be science (e.g. hypotheses and numerical data). This, of course, impacts on the efficacy of sociological data.

If we approach them with qualitative ideas... there is no understanding or comprehension. However, if we go with... in 70% of the cases... Well, then... there is a greater chance of success, there is greater impact. But personally... I trust qualitative data.

Narvi

Qualitative methods, on the other hand, while still an acceptable form of gathering knowledge are more complicated to evaluate. What is more, this is not a problem that is specific to outsiders who only have vague notions of natural science as points of reference when relating to a qualitative study.

Students often choose qualitative studies, because they think it will be easier. But that's not true. It is not easier. Not in the least bit. It is more complicated.

Freya

The classic definition of objectivity... Most qualitative studies would not fall under it, however... to say that they are not objective... Well, you clearly need a different word for it. Everyone who's done qualitative research has reached the point of saturation, where the data start repeating themselves, where certain patterns emerge and you can say with certainty 'Yes, that's the way things are'... And you would say that it isn't objective simply because you can't quantify it? All patterns are interpretable, but so is quantitative data

Fulla

It is clear that qualitative researchers can never rid themselves of... well, she can try to do it and she, in fact, tries to do it, but it remains in some form.

The researcher must do her best to limit subjectivism and evaluative statements, [...] otherwise it wouldn't be science. The question is – where do you draw the line?

Weth

This raises an important question. Remember that, according to Kuhn, an integral part of a paradigm is consensus. This allows scientific work to proceed smoothly with little

attention and resources diverted to disputing the finer theoretical points of the disciplinary engine. In the case of sociology, however, such disputes and uncertainties are not played down – they are integral to the way sociologists understand sociology. That is to say, sociologists constantly reflect upon their analytical categories and the methods they use.

To a certain extent this is unsurprising. Steven Shapin (2008a) has said that academic disciplines that have not quite managed to persuade the public of their validity as scientific endeavours spend a great deal of time exploring and interrogating the nature of the scientific method. This is very true of sociology (Law 2008) which encourages its practitioners to be reflexive about the methodological choices they make. This has currency in the academic world, but, I would argue, is also a contributing factor to the ambiguity surrounding sociological knowledge. That is to say, sociology is a form of knowledge that does not consciously seek to eradicate self-deconstruction and equivocation.

From a constructivist point of view this is a highly precarious strategy. While constructivists have tried to show that stability and consensus is an achievement facilitated by intricate forms of collaboration between human and non-human agents, these very same qualities are used as proxies for success. Furthermore, special attention is paid to attempts to divert attention from the artificiality of the networks that guarantee it (i.e. purification). This strategy may be more pronounced in the case of Bruno Latour, but it is equally apparent in the work of Pierre Bourdieu. For example, even though Bourdieu was adamant that politics should not be used as an analogy for the logic governing good science (quite the opposite – the logic of science should be a template for politics), his approach highlighted the importance of a robust disciplinary identity. This allowed sociology to impose its own descriptions on certain aspects of common reality and so assert the relevance of the discipline. My conversations with Latvian sociologists, on the other hand, seem to suggest that sociology has a molten, rather than solid, core. What is more, this does not seem to bother them – not in the abstract at least.

What is Good Sociology?

Constructivist science studies proceed from the position that knowledge is constituted through a process of contestation, negotiation and interaction between various perspectives and heterogeneous agents. For example, the quality of a piece of research is

collectively established and should not be treated as if it were something that was intrinsic to it – the reality of its quality is procedurally constituted (Latour and Woolgar 1986; Law 2004). This project proceeds on the assumption that the above applies equally well to sociology as a discipline, whose very nature, purpose and integrity is subject to negotiations and dispute. My interviews with Latvian sociologists, however, suggest that, at the discursive level at least, disputes as to what determines quality are not present within the discipline.

After the first couple of interviews I realised that my informants and I were working with very different background assumptions as to what constituted good quality academic work. I included a question about the quality of research output after I noticed that my respondents used words such as “cogent”, “persuasive”, “compelling” and “sound” as if it were self-evident what they meant in relation to an academic or any other research publication. In other words, it seemed to me that my respondents were working with the assumption that (i) the distinction between a good piece of research and a bad one was easy to draw and (ii) the criteria and considerations they used were utterly uncontentious. By addressing this issue explicitly, however, a more complicated picture arose.

This seems broadly consonant with the findings of Michèle Lamont who studied how quality is defined by members of grant panels (Lamont 2009; Lamont et al. 2004). She suggests that it would be more appropriate to treat quality and excellence as polymorphic qualities that emerge from emotionally and epistemologically charged interactions between scholars with conflicting views. A stabilising element in these interactions is what she calls “disciplinary cultures” that largely determine what members of a particular academic field take to be signs of quality and excellence. According to Lamont,

[h]umanists often define interpretative skills as quintessential for the production of high-quality scholarship. Social scientists, especially those who champion empiricism, more often deride interpretation as a corrupting force in the production of truth.

(Lamont 2009: 61)

What needs to be borne in mind is that Lamont is referring to the more interpretive branches of social science, because, as she herself puts it, she

was unable to gain access to the more scientific social science panels, such as those of the National Science Foundation.

(Lamont 2009: 56)

Now, a number of my respondents expressed sentiments that seemed to support

Lamont's conclusions. What is more, this was not a belief that was unique to sociologists who generally favoured the more quantitative side of sociology. A few sociologists who use qualitative methods (e.g. interviews and critical discourse analysis) were equally committed to the belief that a clear separation between data and interpretation is a prerequisite of a good research report.

I have also made this mistake. I wanted to pass my interpretation as...data, yes.

Sif

If we are doing statistical analysis, that is objective. Our interpretations will be subjective, but the results are objective.

Nerthus

It is normal that the results match. It would be strange if they didn't. Then we'd have reason to suspect that there was a problem at one end. We'd have to start thinking about who has imposed his interpretation on the data.

Snotra

The main reason for this, I believe, is that, as both Tabūns (2010) and Tisenkopfs (2010) mention, sociologists in Latvia are mainly engaged in applied research, and there have been few theoretical developments. In other words, theoretical reflection does not constitute a significant part of sociological work. One of the respondents explicitly references this as a problem and claims that Latvian sociologists need to start developing their own theoretical traditions and stop adapting Western theories.

You see, if we keep talking about it like this...like we have to adapt anything that comes from the West without our own... suggestions or innovations, we will always be in a dependent position. We should, I think, change our attitude. We have to stop simply borrowing from the West and try to adapt it to our circumstances. We should... these questions... We should critically, reflexively.... at least start talking about these issues.

Dagr

This is also addressed in *Humans and Life*, in which the authors claim that, in addition to familiarising themselves with Western theories, Latvian sociologists should develop theories that reflect the idiosyncrasies and sociohistorical trajectory of Latvian society (Zepa and Zobena 1996: 9). The distribution of funds among the sciences, however, has created a situation where researchers devote most of their time to applied research.

Sociologists in Latvia... what they mostly do is applied research.

Hoenir

A direct result of this is that none of the people I interviewed expressed an attachment to a particular theoretical framework, even though some certainly had their preferences. Most claimed that (i) their theoretical choices were determined by the aims of the

research and (ii) they would have no problem mixing theories and methods if the situation called for it. This lack of attachment to particular theoretical traditions has created a situation where the quality of a piece of research is generally believed to be located at the methodological level, because the theoretical content of most sociological research is fairly limited.

The fact that such consensus exists as to the determinants of quality is slightly puzzling in the context of Lamont's claim that agreement on what constitutes quality and excellence is usually indicative of consensus and uniformity in the field in general. For example, the arguments used by economists and political scientists reflected similar ideas and academic values, whereas fields such as English literature and anthropology exhibited greater diversity of opinion and even dissensus (Lamont 2009: ch. 3). Now, while the responses to the first few questions did not exactly suggest dissensus, they did manifest signs of internal heterogeneity (molten core). Furthermore, Lamont suggests that the fields that had achieved a generalised homogeneity had done so through either theoretical consolidation (political science) or a commitment to mathematical formalism (economics). Neither of these strategies are reminiscent of Latvian sociology, as there was little faith in abstract mathematical models or the value of a particular theoretical framework. In fact, the closest discipline to Latvian sociology seems to be history in which 'a relatively strong consensus is based on a shared sense of craftsmanship' (Lamont 2009: 4). While the materials that the craft-oriented sociologist has at her disposal are certainly different, questions pertaining to quality were mostly answered with reference to the skills necessary to provide a competent account of the research process.

A competent use of method in relation to... yes... in relation to the theoretical foundation on which the researcher is standing. The data process, of course. How the data was gathered, how the researcher has... let's say... connected the data... how the relationships within the data have been explained.

Forseti

You have to clearly define the problem, the perspective and the method. You have to show how... your contribution and how your results support your claims.

Kvasir

When I'm reading an article I look at how transparent the section on methodology is. The principles underlying the study, the selection of participants and the sample. So... the description of the methodology and the process, after which... after reading which I can... make a decision.

Fulla

As with the historians that Lamont interviewed, the emphasis on methods proved to be

the common ground for practitioners with different theoretical outlooks. However, this very same attachment to a competent use and deployment of methods is what seemed to be central to sociologists' attempts to draw a boundary between themselves and amateurs, or even charlatans.

Experts or Professionals?

Michèle Lamont mentions that when discussing questions pertaining to quality and excellence some of her respondents referred to ineffable qualities (Lamont 2009: ch. 5). While this was not particularly pronounced in the case of Latvian sociologists, there was the occasional reference to uncoded professional standards or the experience and competence of the researcher that did not derive exclusively from the professional training she had received. This seems relatively uncontentious at first.

James Scott (1999) has argued that modern state-building technologies have relied excessively on the importance of abstract criteria and principles, which, in most cases, are characterised by (i) a certain rigidity that does not allow them to attend to the specificities of the locality and (ii) a disregard for the unwritten rules that allow a particular social arrangement to function. Such tools, as Scott notes, are extremely powerful and productive, but a heavy-handed application of these can have devastating consequences because they tend to ignore the importance of seemingly irrelevant or coincidental aspects of a situation which are in fact crucial to its proper functioning. He contrasts this to the experience gained via practice (e.g. an experienced doctor or ship captain). An individual with such experience not only knows the rules, but also knows how and when to bend and adjust them if the situation calls for it. This would seem to be the kind of experience that could serve as a proxy for quality.

A similar approach to expertise in the social sciences is provided by Bent Flyvbjerg (2001). Drawing on the work of Aristotle and Hubert Dreyfus he argues that a social scientist is someone whose rendering of the situation can and usually will be different from that of the participants. According to Dreyfus, an expert is someone who is no longer bound by the rules and instructions that guide the actions of a beginner or even a competent user. An expert is someone who has an intuitive grasp of the situation. Following explicit rules is not characteristic of human experts and, therefore, a formal description and justification of the process that lead to a particular conclusion is often retrospective. Experts come up with a justification, which is, in fact, not representative of

how and why they chose to do things a certain way. It is merely a rationalised reconstruction that makes the process appear congruent with the official framework – the rules beginners have to learn. A good social scientist is just such an expert. The experience she has acquired by practising her craft allows her to make intuitive leaps. Sociologists are not by definition more competent than non-experts. They are simply more dogged in their pursuit of answers and through practice have refined their techniques for doing this.

It's because they have been trained to consider these kinds of things... and... If they don't think about these kinds of things, they analyse statistical data... interviews... This intuition comes as a bonus... as an understanding.

Narvi

Sociologists simply are simply more focused. They work on, and think about, these questions. It is their job.

Sif

Even though sometimes preceded by self-directed irony e.g.

A theoretical framework, a conceptual approach [laughs], systematic... claims based on systematic data analysis.

Fulla

this kind of sentiment was echoed in a number of responses that located the source of the sociologist's expertise in her professional competence.

A professional knows that things must be done a certain way, because there are laws. It is the same with sociology. There are people who are naturally gifted at posing the right questions... and obtaining the necessary information, but he cannot really explain why that is. Why was this the right question to ask? He probably had an intuition. These people who have not studied...they... they would probably be unable to offer an explanation if they were asked for one.

Borr

The knowledge is more reliable... because they have been obtained [telephone rings in the background – distracts us both]. Well, induction, deduction... Ultimately, what is... If we approach this sociologically. We observe facts and make generalisations. The question is how we chose these facts and how valid are the generalisations. [...]. The sociologist should be someone who can explain why his generalisation is reliable.

Weth

This requires special skills and techniques. It requires a trained mentality and... communication skills, the ability to solve problems and the methods for doing so and skills etc.

Kvasir

The sociologist was also believed to possess the ability to contextualise data and arguments, and attempt to transcend, or at least not ignore, her own inherent situatedness.

Sociologists can step back from the way they look at the world... create a new way of looking at things... and see... and provide a take on the situation that can maybe let you compare ten different perspectives or the one that is most important.

Tyr

As first-order observers we cannot always be objective, but there has to be this... ability... tendency... to broaden our gaze and incorporate many different opinions.

Saga

That is the ability to see that I, as a member of society, am not the whole of society. That my social position differs fundamentally... that the other person is different from me not only because his psychological qualities and temperament, but also due to his social status and social experience...

Freya

Occasionally sociological expertise was related to professional experience alluded to above. As in – the more experience a sociologist has, the better she is at her job. An experienced sociologist can often ignore the official guidelines, because his/her experience, and the refined form of intuition that comes with it, is better suited to dealing with complex issues.

When we as sociologists look at society we need a sociological imagination, because we cannot approach things mechanically and try to describe something. The precision of our description is inextricably intertwined with how experienced and trained our gaze is.

Saga

If... sociologists with experience... why shouldn't their subjective views also be a part of it?

Narvi

Of course, if we are talking about an inexperienced sociologist, a novice... Well, then anything can happen – even differences in interpretation. This would seem to imply that quantitative studies are more objective, because you have all these numbers... But even numbers cannot protect you from making blunders. In qualitative studies it has more to do with the researcher's life experiences and her experience with methods. It is a question of practice.

Snotra

In many cases, however, my respondents were unsure as to what distinguished them from people who had not gone through rigorous academic training. More often than not their responses were a combination of the following:

- (i) both groups have the same knowledge, but from different perspectives;
- (ii) sociologists have a much more systematic and conceptual form of this knowledge;
- (iii) sociologists have access to different perspectives, which makes them realise that problems are seldom as simple as they seem to ordinary people;
- (iv) sociologists have an imagination that can render explicit the codes and beliefs implicit in social practices;
- (v) sociological knowledge is susceptible to falsification and responsive to recalcitrant evidence (because sociology is a science).

In other words, while expertise, as understood by Scott and Flyvbjerg, was invoked, a parallel strategy was to emphasise the importance of formal training that located the source of expertise in the methods, theories and research practices that constitute sociology. I realised the importance of the second strategy when it became clear that questions of quality and competence were integral components in a broader issue pertaining to boundary-work (Gieryn 1983; Lamont and Molnar 2002). In other words, depending on whether sociologists had to define themselves in relation to lay people or other professionals encroaching upon their territory, they emphasised different attributes of the sociological identity. For example, formal qualifications were more important when sociologists were trying to differentiate themselves from other professionals, whereas experience was prominent when talking about the lay public.

The emergence and institutionalisation of disciplinary formations has attracted a fair amount of scholarly attention, and sociology has not been immune to this trend (Abbott 2001a; Gouldner 1970; 1973; Wagner et al. 1991; Wagner 2001). The issues that my respondents raised, however, pertained to the appropriation of sociological methods by outsiders. This question was addressed by Pierre Bourdieu who discussed it as a struggle over intellectual turf (see also Abbott 2001a) and stressed the importance of asserting sociological authority over other forms of knowledge attempting to explain the social. A similar approach is evident in Latour's work on Louis Pasteur and his laboratory (Latour 1983; 1993). According to Latour, Pasteur redefined reality by weaving another thread into its fabric and positioning itself between it (the anthrax bacillus) and the actors whose lives it affected. He mobilised a set of instruments and acquired a monopoly on an aspect of reality about and for which he could speak.

In the case of Latvian sociology, however, the competitors are not other disciplines. The specificity of Latvian sociology (mostly applied research) means that its territorial disputes are with people who have no training in sociology but still use similar tools of mobilisation (sociological methods – surveys in particular) to strengthen their claims. Such anxieties are certainly not unique to sociology, but I would like to propose that the meaning of these disputes for sociologists is at least twofold.

Method and Credibility

Firstly, sociology was often juxtaposed with the knowledge possessed, and forms of

sociology practised, by marketing research⁴³ companies. The people working there had read books and possessed a rudimentary understanding of the methods they used, but the quality of their research was considered dreadful and laughable. For example, one sociologist caricatured the head researcher of a well-known social research company:

“See, I’m so successful as a sociologist, because I am an engineer”, which, in my opinion, is hooey.

Freya

These very same marketing research companies were also mentioned as having a damaging effect on public perception of sociology. Consequently, a number of my informants said they would not be averse to introducing some form of professional certification that would authorise holders of said certificate to practice social research and employ the sociologist’s arsenal of methods, though most expressed some concern about the prevalence of such pseudosociology.

Secondly, they were concerned about effect this had on the credibility of sociology, and some of the people I interviewed expressed their dislike for what they considered a simplistic understanding of sociology both by the general public and politicians. In the case of the latter the problem was fairly obvious – they simply did not know what sociology could offer. This, of course, resulted in very limited demand for professionally designed and implemented research but did not necessarily have a damaging effect on public perception of sociology. The main problem was associated with the general ignorance of the nature of sociology and the methodological rigour involved in carrying out sound sociological research.

Most people have no understanding about what happens in qualitative research and what it allows you to find out. There have even been instances where... someone has commissioned research on a particular topic. You give him a qualitative study... He just doesn’t get it. He wants numbers and percentages. [...] He wants a survey, so that it is clear... this is the way things are. Because then it is indisputable – you can’t argue with numbers. Whereas a focus group... Well, they met up and had a chat... what of it?

Freya

There is a widespread opinion that sociology is something primitive. People think that they can put a survey with three questions on the website of their fitness club... What do you think will be the favourite sport of the young prince? And then I can publish the results that 23 people said... I don’t know... basketball, and then I can declare that Latvians think that the prince will play basketball. Something along those lines.

Forseti

The sociological expert was believed to be someone who can be evaluated by other

⁴³ My informants used pejorative terms for which I am struggling to find an English analogue.

competent sociologists and practitioners of cognate academic fields, because, even though indicators of quality are external, lay people lack the requisite expertise to distinguish between good sociology and pseudosociology. In most cases they can be no more than competent users of research that has already been certified by the sociological establishment. I do not mean to suggest that my respondents subscribed to a kind of intellectual elitism. They rightfully acknowledged that amateurs can show remarkable sociological insight that should be taken into account. There are plenty of highly perceptive lay sociologists, but the intuitive, methodologically undisciplined and empirically dubious nature of such exercises in lay sociology makes them ill-suited for professional research. Professional sociologists, on the other hand, have a systematic and rigorous approach to the issues they study, which gives their pronouncements greater epistemic weight and credibility.

Sociology and Fiction

The above statement hints at something special about sociology. It is presumed that professional sociology is somehow different from the intuitive leaps made by lay sociologists. To address this issue in more detail I decided to include questions about the differences between sociological theory and literature that borders on fiction or a kind of investigative journalism. In my conversations with Latvian sociologists I tried to determine what they thought were the differences between sociology and, as I referred to them, literary-sociological allegories. When asked for examples of what I meant, I mentioned dystopian literature and a number of prominent authors in contemporary social theory (e.g. Jean Baudrillard, Slavoj Žižek and Zygmunt Bauman). Furthermore, I also asked them whether these are academically acceptable contributions to sociological discourse (e.g. would they allow their students to reference these authors in their assignments). The responses were manifold and contradictory. On the one hand, it was believed that such writings had a place in sociology. Indeed, a number of my respondents were confident that such literature is of paramount importance to sociology, because it alters the way we approach social life and provides food for thought. In other words, it is a source of hitherto unexplored options and perspectives on social phenomena.

I consider it social philosophy, but I think that...for highly abstract discussions... they are appropriate. They are provocative and interesting for intellectual... intellectual exercises. I really don't have any objections. They also have a place in the field of social theory.

Sif

If these theorists had not done what they did, who began to study quality of life with new conceptual resources, I don't know with what frameworks we'd study them today. It was necessary.

Nerthus

In addition, it is a great way to package sociological ideas in a form that is more appealing to a wider audience. Yes, it may be lacking in academic rigour, but it can get the point across more easily and effectively.

The positive thing about all this is that, if we disregard the speculative nature of such writings... Well, it is a kind of PR move. If you're not speculative, people will... Well... The ability to successfully communicate knowledge and your perspective... it requires... a less, less rigid approach to methodology, which, in most cases, is shockingly boring.

Mimir

However, there were a number of reservations about such an approach. First of all, this concerned the relationship between theory and data. One could certainly look to such texts for inspiration, but when it came to sociological research theoretical musings should not eclipse the role of data.

You've let theory take over the data, but this is not acceptable, because data... Theories exist only to help us identify potential links and connections. Data is primary, because if you let theory run amok... why the hell did you need the data in the first place?

Tyr

This point can be developed further by homing in on one of the more prominent differences between such texts. Namely, academic texts have to be written in a certain way, whereas fiction and journalism have more diffuse literary requirements. When it comes to sociology, the Latvian version tends to be fairly traditional in its commitment to a literary technology in the vein of aperspectival objectivity (Daston 1992). There are marked formal differences between academic texts and language, and colloquial uses of language and tropes in everyday situations. It would be easy to dismiss this as a kind of formalism, but, in conjunction with the concern regarding the dominance of theory over data, it is more accurately viewed as an attempt to maintain the integrity of sociological discourse. That is to say, theoretical speculation and frivolous use of language are dangerous allies to a form of sociology that is committed to professionalism, because the former blurs the boundary between proper sociology and ill-disciplined lay sociology.

Conclusions

At the end of Chapter 4 I suggested that sociology may be self-undermining. The reason for this was that sociology had routinely done to itself exactly what science studies tried to do to other disciplines, and such excessive self-consciousness had a destabilising effect

vis-à-vis a convergent disciplinary identity. I believe that the responses of Latvian sociologists provide an illustration of a different expression of this tendency. This group of people can certainly point to examples of sociological work. They can provide tentative definitions of what their discipline is about and a general outline of the complexities involved in studying it. In some cases these answers are concise, but what is striking is the ambiguity and the multiple directions that their answers take you. The research object is multifaceted and ontologically ambiguous, and sociologists are acutely aware of this. This is reflected in their answers, which are reflexive and opt for complexity rather than rhetorical punch. There is little in the way of authoritative statements. What is more, a measured and thorough approach to questions is believed to be key to conscientious and diligent sociological work. However, this comes at a cost – a variety of research styles co-exist side by side. The further away we go from classical sociology the less widely-recognised exemplars you have. This, by extension, suggests that sociology is characterised by the pluralism that Feyerabend was fond of, rather than the convergent thinking of Kuhnian normal science⁴⁴. This is not to say that sociology in Latvia is a sprawling mess. There is a kind of family resemblance about the way Latvian sociologists characterise their discipline, and their commitment to methodological competence is something that unifies an otherwise decentred discipline. However, rather than stating that sociology has not yet reached scientific maturity (in the Kuhnian sense) or successfully autonomised its discourse and tools of mobilisation (in the Latourian sense), I believe it is useful to consider the possibility that its diffuse and epistemically heterogeneous nature may be key to its health. This would bring us back full circle to the issues I discussed in the introduction. Paul Feyerabend, who is often regarded as an enemy of science, should, I believe, more accurately be treated as an enemy of ossification and institutionalisation, whereas Thomas Kuhn was more comfortable with institutionalised science (Fuller 2003; 2006b). This is a conflict between two competing normative positions within the philosophy of science, but a similar tension is very much in evidence in the responses recounted above. In other words, a dispute in the philosophy of science is an equally central issue for sociology as a discipline. It is believed that, if the issues were to be resolved, sociology would become a more respected academic discipline. However, what is good for sociology as an institution is not necessarily good for sociology as a form of knowledge (and vice versa). From a constructivist standpoint

⁴⁴ Indeed, it has been suggested that sociology is a multiple paradigm science (Ritzer 2001).

this is a highly complicated situation. On the one hand, constructivists focus on the need for stability and a monopoly over certain aspects of reality. On the other hand, their normative inclinations (e.g. ontological politics, situated knowledges) are accompanied by an acknowledgement of partiality and contingency – an impulse that seems to be conducive to the proliferation of different perspectives. However, it is not always possible to practice science in this way, and this is what I will discuss in the next chapter.

7. Conversations with Latvian Sociologists, Part III: Knowledge, Communication and Politics

In his book *A Sociology of Sociology* Robert Friedrichs (1970) argues that, unlike the natural sciences, theories in the social sciences operate at two distinct levels. The first level consists of the specific theory of society that the particular author wants to propose and argue for, and the second level involves a theory of sociology and the role of the sociologist. In other words, Friedrichs wants to suggest that a social theory comes bundled with a certain self-image of both sociology and its practitioners. My sketch of Soviet interpretations of Marxism is one example. The sociologist is not a detached researcher but a fully committed and politically conscientious social engineer. Gouldner's (1970) indictment of structural-functionalism as a politically innocuous academic tradition is another.

It could be argued that Friedrichs overlooks the extent to which a researcher's self-understanding has been important in the natural sciences. This point has been made by McLaughlin (1972), and it can be backed up by looking at the work of Steven Shapin, who has shown that, historically speaking, discussions of science have come bundled with arguments regarding the character of those who choose to practice it (Shapin 1991; 1994; 2008b). For example, I argued in Chapter I that in 17th century England gentility was regarded as a powerful instrument in the disputes pertaining to the recognition and protection of truth (Shapin 1994: 42). The gentleman was presumed to be a virtuous and honourable being, and this imbued his claims with integrity and truthfulness. A similar emphasis on the moral character of scientists is evident in Shapin's analysis of modern science and the scientific entrepreneur (Shapin 2008b). Shapin argues that the figure of the natural scientist as a virtuous human being was prevalent in early modernity (Shapin 2008b: ch. 2). However, towards the beginning of the 20th century this claim was called into question, and scientists were reimagined as morally equivalent to other people, though certainly constrained by an institution that values objectivity and integrity (e.g. Merton 1968). Nonetheless, a belief in the special moral qualities of the scientist persisted. For example, Shapin contends that the role of the expert witness was a complex mix of the professionalisation of scientific culture and a belief in the virtuousness of scientists (Shapin 2008b: 45).

However, while the personal characteristics and convictions of its practitioners had a bearing on the perception of the natural sciences in early modernity, there does not seem to be a necessary link between scientific theories and social roles such as the one between Marxism and a politically active stance, for example. Sociology in Latvia is interesting in this regard because, as I argued in Chapter 6, it is mainly applied, theories have been reduced to a kind of secondary, supplementary role and no particular theoretical tradition has claimed dominance in Latvian sociology. Moreover, my conversations with sociologists working in Latvia revealed signs of internal heterogeneity. These, however, were offset by an emphasis on professional competence as a key component of a sociologist's identity.

At this point Shapin's analysis becomes relevant once more. Curiously enough, one of the main aspects that gradually altered the way scientists were perceived was the professionalisation of the scientific trade. That is to say, if the scientist of early modernity was also a gentleman, the new generation of scientists were simply a group of professionals who did science for money. This created a number of discourses that even went so far as to state that the inutility of the scientist's findings was the mark of a pure scientist (Shapin 2008b: 58); there was a sense that money and industry were the causes of ethical erosion. In the last three chapters of his book *The Scientific Life* Shapin explores moral uncertainty in the context of choices that scientists make regarding their careers. He discusses the issues that arise for scientists working within a capitalist research culture and emphasises the importance of the moral authority and charisma of individuals in the absence of clear guidelines and institutional homogeneity. Among the questions he explores is how practising scientists negotiate and compromise between the freedom to pursue different lines of enquiry and the financial constraints of working in an institution that does science for money. In other words, Shapin focuses on the importance of personal integrity and fit with the institutional goals and requirements, and individual strategies for coping with the institutional arrangements of which she is a part.

Concerns such as the ones discussed above also have a presence in the sociological literature (Burawoy 2005a; Gouldner 1970; 1973; McClung Lee 1976) and were touched upon in Chapter 3. In this chapter I will explore whether and how these commitments mesh with the complex and ever-present tension between (a) the sociologist as a provider of knowledge about society and social trends and (b) the normative confusion

surrounding the import and nature of such knowledge, and the attendant responsibilities of the sociologist.

Revisiting Weber and the Royal Society

A key characteristic of the ideal gentleman-scholar was his ability to restrain the urge to speculate on ultimate causes and stick to matters of fact. Matters of fact were the solid ground upon which discourse could be built and conflict and dissent kept at bay. Theories and hypotheses could be revised and discarded, but matters of fact were permanent (Shapin and Schaffer 1985: 23). The former were matters of philosophical opinion, whereas the latter were not. A similar duality was at work in Weber's discussion regarding objectivity in the social sciences (Weber 1949). The findings of a study were in the realm of facts and scientific inquiry. Facts were susceptible to empirical confutation, and scientific disputes could, in principle, be resolved by way of data and argument. Values were more complicated. They were not cognitive, and conflicts between different positions could not be resolved by way of argument, no matter how persuasive. As Alvin Gouldner's reading of Weber suggested, however, the injunction against value judgements in social research was prescriptive in nature. It was an ideal that researchers should strive for, even though it was elusive in practice (Gouldner 1973: 3-26; Burawoy 2012), and it is a testament to Weber's influence that value-neutrality has become a prominent way to figure the relationship between sociologists and their work.

The complexity of this issue is equally apparent in the case of Latvian sociology. The responses illustrate a range of possible alternatives to dealing with the relationship between the descriptive and the evaluative dimensions of their work. There was, for example, a group who were adamant that it is of utmost importance to keep them separate and refrain from using loaded language. Such arguments were related to professionalism, and the ability to keep one's personal views in check was treated as a hallmark of skill, quality and commitment to the scientific way of life.

If a person is very knowledgeable and, say, they acquire a vast amount of information about it all, and carefully assess... [what] the actual facts [are], then subjectivity is reduced.

Borr

Many... not-so-professional sociologists spout and use their prejudice.

Kvasir

Researchers have to make an effort to free themselves from subjectivism and value judgements, [...] otherwise it is not science. The question is, where do you draw the line?

Weth

I still think it depends [highly] on one's professionalism.

Freya

Even when the above link was not made explicit, others argued that one should try to separate and clearly distinguish between the arguments derived from the data and the researcher's own personal evaluations.

Sometimes it helps with studying something in depth and looking for evidence, ... but I have this instinct – so it wouldn't be too big of a sham, or just some sort of demagoguery – I try to keep track of what it is based on.

Var

Neutrality can be maintained through data. Neutrality is assured by the scientific quality and nature of – of the researcher's focus on what he's interested in as an object of study... more than, say, various other influences...
If you're aware of your likes, your dislikes...your...doubts. At the same time, it allows you to be more neutral.

Forseti

Value judgements have to be made, but only at the very end. They can't be made at the beginning, when theories are chosen. It shouldn't influence our verification of hypotheses. Well, perhaps later on when you see it coming together... Well, then you make value judgements. You shouldn't be afraid. Things have changed since Max Weber's time.

Delling

Or, if not possible, at least make the reader aware of your position so as to give her the opportunity to judge for herself. This link was often made in the context of a thorough description of the methodology.

The more extensive and more detailed my description of something, well... the more I let my reader judge for herself. I'm not... trying to declare that something is right, that something is good, rather... with all that I've done, how I've described it...Well, I make it...Well, I don't know... more transparent, more understandable, so that they can see my line of thinking.

Nanna

Well, you have to make an effort on some level, but... it's impossible, yet you have to think about it. Meaning... this... you have to reflect on the impact of the researcher's values on a specific topic. What are his prejudices and ideas, that can influence... you have to think about it.

Sif

Perhaps it would be more honest to show yourself completely, instead of hiding behind...well... behind statements about trying to ignore something. Just admit that you're aware of it.

Tyr

The responses suggest a mingling of different aspects of the professional stance – it was framed as a virtue, as well as an indicator of the integrity of the final product. Most of my informants, however, chose to challenge the question and expressed scepticism as to the possibility of clearly distinguishing between the descriptive and the evaluative dimensions of a piece of sociological research. This was due to the fact that most sociological work in Latvia is applied, which means that the research goals are specified for the researchers. In practice this often requires that researchers produce project reports that must include a list of recommendations in the form of suggestions for future research and advice for particular courses of action. This, however, does not result in tension with Weber's call for value-neutrality in sociological research. As you may recall, Weber was perfectly comfortable with sociology and researchers in general being tasked to consult an interested party and providing technical solutions to a problem. The state of affairs in Latvia means that questions pertaining to value-neutrality are often simply circumvented, because the value relevance of a piece of research is externally defined, as are the ends to which researchers are tasked with finding the means.

Research projects always have a source of funding, certain goals and certain interests.

Freya

Initially, research is neutral, of course...in a way. Of course, you have to follow the rules. You have to send recommendations to the European Commission, and to the local government, but the rules are clear, aren't they, and the recommendations aren't politically motivated, they come out of the research.

Kvasir

This, of course, suggests that sociologists themselves are not creatively involved in the co-construction of the normative context and must content themselves with being placed in a technical or advisory capacity. In other words, while Weber was concerned with keeping politics and research separate, Latvian sociologists construe themselves as actors in an environment where research is done in the context of political action. Latour would probably concur and highlight the importance of building alliances and emphasising one's value to a successful and well founded decision. That is to say, for Latour there would be little reason to lament the role that sociology has acquired. This, however, illustrates a point of disagreement between Bourdieu and Latour – namely, the former thought that sociology should be fussy about whose ends it serves.

A Camera, not a Photographer

In the previous chapter I suggested that sociological textbooks argued for a kind of back-

and-forth movement between sociology and the society in question. Interestingly enough, sociology was figured as a mirror, and this very same metaphor was invoked on a number of occasions.

Well, you'll probably hear the same thing from Aivars Tabūns. That's what he taught me when he was my professor – that the best thing we could become was a mirror to society – to show our little faces and let us see what we look like.

Hoenir

The choice to go with a mirror is a peculiar one, not least because of the connotations that come with it. The above quote suggests that the mirror metaphor is meant to convey the idea that sociological work shows us what we as a society actually look like. Read this way, mirroring inevitably leads to questions of representation, an implicit representational theory of knowledge and friction with the constructivist framework explored in this thesis. It also portrays sociologists as passive. A more active role was suggested by one of my informants who compared sociologists to photographers.

I'll tell you what a student of mine told me. I like this metaphor very much. We're photographers at a wedding, right? Sociologists, right? A metaphor. You're not really part of it. Well, perhaps you drink a bit, but... you're the photographer, right? And the photos you take also depend on... if you take a picture of the best man with his mouth open... all sorts of ridiculous poses... You've seen these kinds of wedding pictures, right?

Me: Yes

Or you've captured something that says a bit more about these people... I don't know, if you have the eye for it, because such photographers are...

Saga

As the above quote suggests, sociology is a profession that requires skill, but it also involves choices as to what to focus on and emphasise. Constructivist literature within science studies has argued that this is also a profoundly political question because research involves a decision as to what can be made absent and so rendered inessential (Law 2004); this has also been cast in terms of scientific representation being a form of political representation (Latour 2004a; 2005). Thus, while at first glance the photographer is simply an extension of the mirror metaphor, it reveals the political, or even partisan, dimension of sociological work and opens up the relationship between knowledge, politics, and gives the sociologist a more active role in enacting her object

We reveal some sort of position in every word, in every sentence – like, the way we look at a certain issue. There are many aspects to every issue, and the aspect we choose to emphasise, without even expressing our opinion, already creates a certain perspective...

Eir

This has interesting implications for the question of objectivity. Daston and Gallison (2007) suggest that our understanding of objectivity has undergone significant changes in the last two centuries alone. What is most striking about Daston and Galison's analysis is the claim that objectivity has historically contained an ethical dimension. In order to achieve objectivity of the requisite sort the scientist had to discipline himself and cultivate a certain kind of self (Daston and Galison 2007: 40). Furthermore, the authors show in great detail that an important question in relation to objectivity was not just distortion in the form of prejudice or bias – the role of trained judgement was equally prominent. In the 18th century the skill of the experienced scientist was valued because it enabled him to see past surface diversity of appearance and grasp the essence of his object. This conception of objectivity was gradually supplanted by a more exacting interpretation – mechanical objectivity. The latter replaced the emphasis on skill with an ethical disposition towards the restraint of the self and its proclivities for discrimination and idealisation. The idiosyncrasies of the scientist were supposed to be kept in check and prevented from influencing her work. In the 20th century, however, the role of experience was reintroduced in the form of trained judgement. It was believed that 19th century objectivity was inadequate for scientific work. Indeed, the return of trained judgement in the 20th century suggests the possibility that objectivity may not be the most important of epistemic virtues because, as Daston (1992) suggests, an perspectival objectivity may, in fact, produce an impoverished account of an object or phenomenon.

However, not all of my informants felt this way, and a more restrained version of objectivity (more camera than photographer) was prevalent. You may recall that earlier I argued that Latvian sociologists were cognisant of the complexities of disentangling evaluative statements from purely descriptive ones. This very same confusion manifested itself in relation to objectivity. On the one hand, you had people who emphasised the importance of objectivity.

Well, you see, if the scientist herself, the sociologist, wants to obtain objective facts, if she has the will, and the resources, to conduct an honest study, then and only then a sociologist's [...] work can claim to be objective.

Dagr

Others, on the other hand, claimed that objectivity is elusive but it is something to strive for.

I've never thought about this. It has always seemed self-evident that... my goal is to be objective. But, of course... to say that the way I see society is 100% objective... well, I don't think I can. It's what I aim for. Objectivity – it's a goal.

Vali

Objectivity should be seen as an ideal-type – as in – it is something we strive for.

Eir

Objectivity was seen as a characteristic of good sociological work, but it was made clear that it is something that we can only hope to achieve because our perspectives are limited. Similarly to Daston and Galison (2007), discussions of objectivity had axiological overtones. That is to say, objectivity was figured as a value, rather than a clearly identifiable characteristic that sociological research can exhibit. Similar sentiments were equally apparent in the responses of those sociologists who did not believe that objectivity was, in fact, possible in sociology.

Well, sociologists try, they do try to strive for objectivity, but... in fact they understand that absolute objectivity is impossible, yes.

Sif

There can be many theoretical approaches, so long as they don't become dogmatic and aren't applied dogmatically, so it doesn't become a bother. There is a certain objectivity to it, yes. You understand, don't you, that the framework... You don't just put the material in a framework and then try to force it in.

Var

If objectivity was unobtainable, other techniques were invoked to restrain subjectivism and situate the perspectives of sociologists. Thus, even though the way we chose to frame and phrase our research (even inadvertently) hampers our quest for objectivity, we can try to limit this. The link between objectivity and neutrality, however, was tenuous. Objectivity pertained to the quality of the research output, whereas neutrality pertained to politics. e.g.

I don't know if objectivity is a burden to...the public or critical function of sociology. Rather it pertains to methodology and academic integrity...in research. It's like the ABCs of research... You can't be tendentious, you have to consider all the facts, look for contradictions... it's part of the inventory of sociological work. That is, if somebody ignores it, it's a violation of academic ethics.

Fulla

The situation was more complicated than it appears at first, however, and the question of political neutrality proved to be a tricky one to pose and discuss. This was not due to my informants' commitment to value-neutrality or a lingering preference for descriptive sociology. Rather, it may have been because of the negative connotations that partyness has in Latvia.

In Chapter 3 I argued that Soviet sociology had aligned itself (or was forced to align itself) with the needs of the Party and defined objectivity in terms of partyness (i.e. a partiality towards the interests of the party). Furthermore, this was one of the few things that did not emerge from Western commentary on Soviet sociology but was actually an explicitly stated goal of the journal *Sociological Research*. In view of this, some of my respondents expressed concerns that derived from a specific take on scientific integrity and the role of sociological knowledge in political matters. Sociologists study the world. They have no business trying attempting to alter it. Indeed, this was even framed as one of the preconditions for being able to give critical feedback.

A scientist mostly sticks to inquiry...It's the others that have to change the world.

Freya

Sociology has to stand on its own two feet, right in the middle. Not only should sociology distance itself from...from various interests – it's definitely a matter of interest, because sociology should be a mirror in which society can see itself. It means that sociology should be a trustworthy ally in studying the current situation, not in manipulating people's opinions and forcing them to change something, or to incline them towards something...That would be a sin.

Nerthus

Well, then...we can sit back and take a critical look at what's happened because we're not involved in any of their...group fights, fights between parties. I don't care who'll make it to the next round, because I have no idea who's applying. I don't know which way the vote will go. If you look at it this way, we can easily tell them: Look, looks like you've made a mistake here. Something isn't right. Society doesn't understand you. All right, you did something, but you failed. I have no problem telling them that, but I always do it behind closed doors. Never in public. [...] For me, it's much easier to provide feedback if I'm not seen as the opposition.

Delling

However, when I qualified my question by saying that I did not mean preferences as regards political parties, the responses indicated a situation that was similar to the relationship between descriptive and evaluative statements, and it was acknowledged that sociological work exhibits certain political preferences.

All research is partial towards some political agenda, and even if you're not interested in politics, politics is interested in you.

Saga

No, it isn't neutral. It isn't ideologically neutral either. A researcher has her political position or...her ideological position which she also expresses, yes, in her research.

Sif

This opinion, however, was not dominant, and the discussion concerning neutrality elicited a series of ambivalent responses. For example, some acknowledged that

sociological knowledge has this peculiar characteristic of being able to change how we perceive certain phenomena. Furthermore, these aspects of social life are usually value-laden, which means that sociological research is situated in the midst of normative struggles. This comes back to the issue of responsibility because such research can be used for myriad different purposes. Thus, the question “for whom” becomes very important, and this was acknowledged by my respondents.

Who would use this information? Let's say we're finally able to understand how a certain group thinks, and how to alter their way of thinking.. And who do you think will use this information? Those self-same members of society, or some sort of corporate faction...or an ideological group? Who will be the first to use it?
If you look at it this way, I'm not at all grieved that sociology everywhere is slowly pining away.

Delling

Like in any science. There is no... difference, except the responsibility is greater perhaps. [...] There's no room for delusion. In a way, it's very easy... to delude society... and I've had to deal with certain politicians who wanted me to provide them with favourable results which were not... real.

Vali

Bourdieu contended that, while sociology is a science, it produces knowledge that is socially relevant and potentially subversive. Furthermore, sociologists should not allow themselves to be assimilated to just any political project. The above introduces a similar point of friction between different conceptions of neutrality. On the one hand, neutrality is a positive quality because it is a cornerstone of scientific integrity. On the other hand, it can start to work against the goals of sociology. Some clarification is in order, however.

Studying the Object

Some of my informants had difficulty articulating their opinion as to the functions of sociology. To clarify the purpose of my questions I reworked the arguments of Michael Burawoy. In his 2004 ASA Presidential Address and a number of subsequent articles Burawoy suggested that one can distinguish between four types of sociology that are, nonetheless, intimately and necessarily connected (Burawoy 2004; 2005a; 2005b; 2005c; 2005d). These are: professional sociology, policy sociology, public sociology and critical sociology.

Professional sociology designates forms of sociological activity whose intended audience is other academics – an example would be an academic publication. For the purposes of avoiding confusion I referred to this as academic sociology, since I believed that this better captured what Burawoy meant and clearly differentiated between academic and

applied sociology – a distinction of great importance in the Latvian context. It was quite clear that Latvian sociologists had doubts that academic sociology was a vital component of Latvian sociology. This point was made both indirectly (e.g. references to their own work) and explicitly. The reasons for this would appear to be simple. In Chapter 5 I mentioned that the vast majority of work done by Latvian sociologists consists of various kinds of reports, rather than academic publications. There were publications that would be of mainly academic interest, but it was made clear to me that these articles or books were published sporadically and, in their opinion, were generally of low quality. Furthermore, there was pervasive scepticism as to whether the situation would improve significantly in the near future. Consequently, while my respondents believed that academic work is of great value and a desirable form of existence for sociology, the prospects for Latvian sociology were rather grim.

Pure sociology... in the sense that it contains discussion or reflection – there's very little of that.

Hoenir

The critical function is very weak, yes. In my opinion. Rather, there is this technical... public function? What else was there? Academic... well, it's rather weak, but at least something is being done, yes.

Sif

The scarcity of academic output has consequences for what Burawoy calls critical sociology (not to be confused with critical social science [e.g. critical theory]). According to Burawoy, critical sociology grows out of a disaffection with dominant trends and assumptions in professional sociology. The main forms of expression of critical sociology are arguments that call into question the conventional wisdom in academic circles and reflect on the broader implications of practising sociology in a certain way. Its object of critique is academic sociology, rather than civil society or popular culture. C. Wright Mills (1943; 2000) and Alvin Gouldner (1970; 1973) are both prominent examples of this tradition. As I mentioned earlier, however, the academic dimension of sociological work is fairly muted in Latvian sociology, which means that critical sociology in Burawoy's sense is actually non-existent in Latvia. That is to say, any discussion of critical sociology is moot largely because there is nothing resembling academic consensus in the Latvian context – or so it would seem. In spite of the fact that sociology is an underfunded and misunderstood (more on that later) form of knowledge that has little in the way of concrete research programmes, there were glimmerings of critical sociology in

the responses of Latvian sociologists.

Those so-called government commissioned studies degrade the sociological market – they crash it. We'll give money to him, but we won't fund him etc.

Delling

If we imagine a flat field of knowledge... Everyone studying something, everyone fighting for themselves. But if I'm ... stronger I can push my findings higher up. They become more visible. They attract more attention. But a research of equal scientific... quality, of equally good results, or... a good research that lacks funding is less visible.

Weth

The quotes address practices that have been brought about by the way sociological research is funded and what kind of sociological research is funded, and suggest an implicit distinction between quality and the ability to attract funding and make your opinion heard. Furthermore, this also affects how well sociologists can perform the critical function.

If we were somewhat independent financially – to the extent that we could choose our own research topics and be... well... If we could, in a sense, survey what's going on in society, then I think that... that it would be... the critical function would be stronger. At the moment... at the moment we're largely dependent on our funding, on the ministries which have very specific goals. And we have to answer very specific questions.

Narvi

The above quotes hint at a kind of internal tension within sociology whereby there are perceived differences between the goals and interests of those funding research and the interests of sociologists and sociology. This, according to Burawoy, is a common characteristic of critical sociology. In particular he refers to a speech given by Alfred McClung Lee (1976) in which the latter raised two questions that are fundamental to critical sociology: Sociology for Whom? and Sociology for What?. The former concerns the perceived audience of sociological publications and the fears associated with sociology becoming an inward-looking and overly technical discipline with little to offer to the non-specialist. "Sociology for What?" relates to the goals of sociological inquiry. For example, are we as researchers content to simply fulfil our obligations to our employers and/or clients? Should we be mindful of the wider consequences of our research and the uses to which it is put and the realities it enables?

To address the first question in more detail I decided to enquire as to why my informants thought sociological knowledge was valuable. In addition to the expected answers invoking the instrumental value of social information in the context of policy-making (more on that later), there was a strong sense that the knowledge that sociologists could

provide and the insights they could potentially offer to non-sociologists had value in their own right. Some of this derived from the opportunity to satisfy one's own curiosity and explore issues that have aroused one's interest. This was sometimes related to conceptions of knowledge as valuable for its own sake, though occasionally it was simply a matter of sating one's personal interest.

I'm interested in how society functions. I am interested.

Hoenir

I think sometimes knowledge has value in itself.. even if you can't use it right away, because I think... it's this understanding that governs our actions.

Fulla

I think sociology has value in and of itself – as a form of knowledge, a specific form of knowledge.

Weth

However, it was also argued that such intellectual exercises were worth our time because they broadened people's horizons and allowed us to get a fuller understanding of the situation we were in. This would hopefully translate into people being better equipped to make informed and competent choices about future courses of action. In a sense, this was also about stimulating the possibilities for individual freedom.

By not knowing, we are likely to cause more damage and harm

Mimir

New values emerge. Not just in an instrumental sense, but... at the level of how we view of the world, you know.

Dagr

On the whole, I want to know what the point is – of what I'm doing. Regardless of what I do. And I sense that, say, independence is very, very important in order to see this point, and it's knowledge that increases freedom.

Forseti

At a certain point a person who has considered the views of 100-200 interested parties can provide a more...multifaceted vision of the issue. And that... on the one hand, it's sociological ingenuity that's passed on. On the other hand, it's... a variety of perspectives. A variety of viewpoints that allow us to show, in a more well-grounded manner, how these viewpoints cluster and come into contact, and then – to present an argument... a common direction of development, for everyone's benefit.

Tyr

This would seem to suggest that, in answer to Alfred McClung Lee's questions about the intended audience of sociological knowledge, Latvian sociologists refused to locate its value squarely in the academic realm or even in relation to policy-making and the preferences of individuals populating the respective institutions.

Studying the Object or Expediting Politics?

In Burawoy's division of sociological labour policy sociology is a form of instrumental knowledge that refers to sociological research that has been carried out for a client. He discusses the use of sociological research in the context of policy-making, and it is quite clear that what he means is government policy. In the interviews, however, I broadened this category and also included research for non-governmental organisations, municipalities and banks. The overall view was that sociological knowledge should, indeed, play a role in policy-making and any kind of decision-making that concerns large groups of people. The reasons for this were uncontroversial and mostly derived from my informants' belief that political decision-making and policies would simply have a greater chance of success if they were based on sound information.

Sociology is very valuable in that it captures what is happening – which is very important for decision-making.

Mimir

Sociology is necessary '[t]o take a broader view of the problem and its many aspects, than provided by politicians and their limited experience.'

Nanna

There are probably two roles. One is actually providing data... Well, sound, to-the-point data. The other is helping us understand, because a politician charged with making decisions might not have a lot of theoretical... Well, a sociologist has her knowledge of society and that... And all those theories and everything is fresh in her mind, unlike a politician who's making decisions... Well, the ability to explain the consequences that will follow this or that decision etc.

Snotra

In other words, rather than assuming that they know best, managers and bureaucrats would be wise to consult experts with potentially useful suggestions and solutions. The responses were, therefore, in line with Burawoy's thesis that policy sociology is an instrumental form of knowledge. However, even though my informants believed that sociological knowledge should play a part in decision-making, they were cautious and sceptical as to whether it has any actual impact on the decisions and policies that politicians or government bureaucrats implement. Some of this relates to the professional experience of my informants. For example, those who were working or had worked for research centres with a history of being commissioned by the state chancellery or ministries were generally quite optimistic. This brings up an important issue that was raised by my respondents on a number of occasions – namely, credibility and competence are tied to specific individuals, rather than the discipline as a whole.

Even those of my informants who had professional experience working for the state chancellery or government institutions acknowledged that not all of their research had the desired effect. Sometimes this was because the findings conflicted with the current political climate or the simple fact that there was a change of staff at the institution in question. Indeed, this was not unlike the story put forward by Flyvbjerg (1998). Knowledge could exert itself only sporadically, at opportune moments. Furthermore, impact may be limited simply because researchers do not have the time and resources to devote to explaining their findings to the relevant audiences and bring the important issues and insights to the attention of policy-makers. In other words, impact is limited by time constraints which hampers the ability of sociologists to build alliances and achieve tangible results.

Researchers work in this...this [constant] cycle of preparing, carrying out and delivering reports, while also preparing for their next project.

Kvasir

I think there's a point to it only if a sociologist is part of an actual group in charge of establishing the normative framework. Then, if it changes, she can take part in it, discuss it regularly over an extended period of time and actually do something, influence things. But when you're studying something and working on the recommendations, then suddenly the research is over and you're looking for money to survive the next few months. In the current situation, you have to work so hard that you don't have time to follow up on the results of your previous research – on a purely idealistic impulse.

Fulla

Not all of my informants raised this issue, but Jennifer Platt's study (Platt 1976) suggests that this is a common problem that is not particular to sociologists working in Latvia. In spite of these complications there was a definite belief that sociology is, and should be, part of the decision-making process. Its involvement, however, was framed in instrumental, means-to-an-end terms, which is somewhat at odds with the reflexive and horizon-expanding narrative that I encountered when talking about the value of sociological knowledge. This may simply be a matter of genre. That is to say, policy sociology is different in kind to public sociology and critical sociology. The latter are reflexive about the needs sociology serves and the realities it enables, whereas policy sociology is not. I want to suggest an alternative explanation, however, that has to do with the financial situation of Latvian sociology.

While the lack of funding certainly affects the production of academic sociology (e.g. books and articles in peer reviewed journals), it also hampers the possibility of critique.

Most research in Latvia is contract-based, which means that sociologists are generally in a position where they are providing a service to a client. Furthermore, their livelihood depends upon them maintaining a good working relationship with their clients. In practice this means avoiding direct criticism. Financial insecurity is what drastically hampers the freedom of the sociologist to voice dissenting opinions without fear of long-term financial repercussions. In response to this my informants argued that a prerequisite of sociological critique was financial independence.

If we could establish, say, an independent intellectual... tradition in Latvia, of course, always... If it's independent, it'll be critical as well.

Dagr

To maintain this critical function, one has to attain... well...independence...from clients and funding... Probably yes, but in a practical sense it's quite difficult to accomplish... especially in the current market situation.

Freya

This should not be construed as a matter of political censorship. Sociologists are free to express disapproval with policies, produce and report politically inexpedient findings or challenge the way a particular issue is framed, but such moves are subject to careful management and professional constraints. Indeed, it was even argued that neutrality and professional restraint were a prerequisite for successfully articulating a critical response. Nonetheless, the relationship that my informants seemed to be suggesting was based on the belief that their clients want instrumental, rather than reflexive knowledge. A similar point has been made by Anda Laķe (2012) who looked at evaluation research as an example of the research-policy nexus. She argued that communication between researchers, bureaucrats and politicians is often hampered by different conceptions of the value of research and discrepant forms of rationality guiding their actions. Thus, while sociologists themselves might see the value of their work in terms of the insights it provides, their clients are more interested in strategically deploying the simple fact that research was, in fact, carried out. Their interest is instrumental, rather than reflexive.

Credibility and Communication

As I indicated in the previous paragraph, not all the ills of sociology can be traced back to a lack of financial resources allocated to sociological research. For example, some of my informants were sceptical about whether sociology plays a significant role in policy-making. Sociological research is commissioned and carried out, but it may simply be a matter of ticking a box and satisfying official requirements.

During the “Fat years”⁴⁵, there was more demand for sociological studies – mostly for keeping up appearances, I think, and also to get those graphs and percentages, right. Once I was even told: “Gosh, can I even show any of this at the seminar in Brussels.”

Freya

Public administrators have long since understood that they have a need for some sort of research. Sometimes I think they commission research just for the sake of commissioning something, [...] to justify the decisions they've made.

Weth

In other words, research was treated as a formality – done only to keep up appearances. This means that there are certainly research projects, but they fail to make much in the way of difference. The reasons for this were numerous. Some pertained to a lack of respect and insight into what sociology can offer.

I'm going to be honest. I'm not...I'm not used to this sort of question, and mostly...these questions are mostly asked by... the people who...who do not want to hear the answer, and the answer is... what Mr. Kalvītis⁴⁶ already said, that we don't need political scientists and sociologists. And I can't justify it to these people, I can't tell them why we need sociologists, or what is it that sociologists do, because they're not interested in my answer.

Hoenir

If there were no EU funding, I think we'd have gone on, not knowing what it is. Of course, most people still don't understand who needs it or what it's for, or what to do with it.

Eir

Politicians think that they're smart, that they don't need sociologists, that they'll handle it on their own. If they need to research something, they'll pay a company to do it for them. It's not a question of quality, they don't care about it.

Nerthus

This suggests a peculiar situation that can be understood with the help of Pierre Bourdieu. In his book *Pascalian Meditations* Bourdieu claimed that the authority of the Prince derives in part from the perceived autonomy that his legitimators (e.g. poets, jurists) enjoy (Bourdieu 2000: 105). It could be argued that sociology in Latvia is in a similar position. It is financially dependent on outside sources, yet carries little credibility among them. However, the perceived status of sociology as an independent source of knowledge and expertise (however ambivalent the opinions of it) confers credibility upon its client. In other words, it is good to be seen as soliciting expert advice, but there is no desire to actually act upon this advice. Why this discrepancy?

A common explanation for this state of affairs involved invoking ignorance (see above).

⁴⁵ An informal term denoting a period a rapid economic growth in Latvian economy (2006-2008).

⁴⁶ Ex-Prime Minister

Politicians did not have an adequate appreciation for what sociology could offer and the insights it could generate. In particular, this was a problem plaguing qualitative studies and their possible application. This relates to a point I made in the previous chapter. I argued that ignorance was regarded a problem from a professional standpoint. It was believed that both the general public and bureaucrats had a rather tenuous grasp of what could count as sociology. There was, however, one instance of sociologists taking a stand on this issue.

In 2010, as part of the pre-election campaign, the political party *Unity* commissioned the market research company *GfK Group* to carry out a poll. The poll was about people's preferences as to the future prime minister. The choice was between the candidate put forward by *Unity* (Valdis Dombrovskis) and the candidate put forward by their (presumed) main rivals *Harmony Centre* (Jānis Urbanovičš). *The Latvian Sociological Association* (i) challenged the validity and reliability of the poll, (ii) accused *GfK* of committing numerous methodological mistakes and (iii) argued that the poll was a form of political technology, rather than a sociological survey. The attempt was to disassociate the activities of *GfK* from what actual sociologists do.

They renamed it. Now it is a simple poll. That's it. Without the word "sociological". At least that much we could get across – don't go about putting on our "coat". Anyone can do a poll. A journalist can do a poll, a street sweeper can do a poll, but... don't go about discrediting our "coat".

Delling

Nonetheless, this dispute had to be appropriately managed so as to prevent blowback that would actually be damaging to sociology.

Over there, nobody's thinking about GfK, nobody knows that it's [mainly] economists... managers. Everyone will think it's sociologists... again... forging data. You know, making a muck of it. They'll never make the distinction.

Delling

This belief was echoed in a number of, though not all, interviews. My informants claimed that the general public has a poor grasp of sociology. While they never claimed ignorance was the cause of this, my informants believed that this was complemented by scepticism as regards the value of sociological knowledge. This attitude was, however, not specific to sociology, but concerned all forms of knowledge and expertise (Ostrovskaya 2009). As a consequence, my informants believed that political decision-making was generally of low quality, because bureaucrats did not use, or had little experience in using, the expertise provided by sociologists. However, not all the blame was placed on politicians or the

incompetence of civil servants – sociologists were believed to be equally at fault.

Communication Breakdown

Knowledge is one thing, but sociologists who go about doing things with this knowledge is another matter, and I think this is more important. What's currently happening is that they have this knowledge but, in practice, they don't really use it all that much.

Fulla

In Chapter 4 I suggested that Bourdieu's theory of sociology placed an undue emphasis on the nature of sociological knowledge and paid scant attention to its public representation. Latour, however, suggested that the fourth loop (public representation) 'is all the more important because the three others largely depend on it' (Latour 1999: 106). Fuller's analysis of the ills plaguing sociology indicated that the discipline has a bad track record of drawing attention away from its hybrid nature – something that the natural sciences do well.

The interviews illustrate a related yet different problem that sociologists have – communication with the public, and my informants considered the communicative dimension of sociology to be a sore spot. On the one hand, it was widely acknowledged that the media played a big role in exposing the public to what counts as sociological knowledge. On the other hand, journalists were said to have a poor handle of what to expect from sociologists and sociology more generally. A common complaint was that sociology was equated with questionnaires and public opinion polls and nothing else besides that. Such perceptions complicated communication with journalists, because the result would usually be an article that summarised a sociologist's comment in a simplistic manner.

I had a conversation with a journalist once. And I had the feeling that she didn't understand any of it. That she needed some sort of... Yes, that's it, right. That she had no use for it. Of course she didn't. "It's very complicated. On the one hand, on the other hand". All right, it wasn't even like that, but... there's no single interpretation. Of course. Even in the exact sciences the age of mechanical... classical physics has come to an end. All the more so in society. Yes, there might be a tendency, but...

Me: You have to consider this and that...

Yes, exactly. No, but she needed some sort of... unequivocal sensation.

Var

Thus, my informants were worried that journalists would simplify complex matters and

pay scant attention to the qualifications that accompany most comments. This was a particular point of concern since journalists were generally educated just a few doors down the hall (at the Department of Communication Science).

Further still, it was argued that communication with the public is hampered by the fact that there is simply too little in the way of interest about what sociologists get up to. The media were only interested if the findings have something shocking about them, whereas the more mundane research attracts little attention.

Almost every month I get idiotic emails from the media... Yesterday, the princess' baby was born, has there been a survey on public interest about the princess' baby... Something along those lines. Science is perceived as... as some sort... well, at least that's the way I see it – it's perceived simplistically, as a source of sensational news.

Forseti

The role of the media in representing sociology to the wider public, however, was believed to be key. This was a matter of interviewing the same people again and again, because their credibility was tied to them as individuals – they were not interviewed because they represented a particular profession. Consequently, it was acknowledged that journalists and reporters are simply drawn to people whose way of expressing themselves was appropriate for their needs, but an equally important factor influencing their decision was how well the comments of the sociologist in questions meshed with the political orientation of the newspaper or the broader political climate.

The media control the current situation. They'll decide how far they'll let you into the media space.

Saga

Well, I guess you could say that, hypothetically, mass media don't really understand, they don't appreciate what a sociologist can provide. But that's the case... On both sides... Meaning, how we present ourselves, what we say about ourselves, what we can and can't do. And vice versa – what those at the receiving end are prepared to receive.

Narvi

It's hard talking to sociologists. Usually the journalists, the media have their own idea of how things should be. Who's to blame etc. And if the opinions don't match, well, they're not comfortable.

Vali

This was complemented by a rather dismissive attitude from the public. It was sometimes argued that, even if sociologists took their time to organise public events, people would simply not turn up (a similar comment was made by a historian about a series of lectures he organised). Time and money were in scarce supply, so a prominent theme running through my interviews was the lack resources to inform the public and engage them in

discussions. This was also framed in a less self-sympathetic manner. That is to say, the blame was placed on a lack of initiative – sociologists themselves were at fault for not engaging with the public on a regular basis.

The community is such that... they do a lot of work among themselves, and there's no outward communication. People from the outside have no idea what this community is up to. It's too insular.

Borr

I think that, in a way, it's our own fault because we don't do enough educating.

Snotra

Up until now, the sociological community has lacked the... strength, the capacity, the motivation... the ability to go... to go out in public and explain what sociology is.

Sif

It was quickly acknowledged, however, that this was complicated to do. It may be true that sociologists lack the time, resources and initiative, but there was also the recognition that communicating sociological knowledge to the public is exceedingly complicated. This echoes an issue I raised in the previous chapter. I argued that, while Latvian sociologists were positively disposed towards figurative sociology, their enthusiasm was tempered by a recognition that the public was not always receptive to ideas presented in this form. Of course, the problem was not peculiar to more figurative modes of sociological expression. Dry and technical styles of communicating information and viewpoints were just as problematic.

They need juicy phrases that sound radical. The sort... the sort of handy generalisation that you can pick up for yourself and use in a quote.

Mimir

Unclear Identity

These different forms of expression, however, come back to the question of identity. There was a distinct belief that sociologists should communicate with the public directly and not rely on the executive and legislative branches of government, or municipal governments putting sociological knowledge in action. The question of the form such communication should take, however, was characterised by considerable variety. I have alluded to some of the difficulties above, but an equally important facet of this dimension of sociological work relates to the personal characteristics of individual sociologists, rather than how well they exemplify a sociologist. This is neatly captured in the quote below.

There is no sociology as such. There are just specific people with...with their

understanding of what to do and how to do it and...we're very different. If we were united, then... then... we would... act differently in public.

Delling

The quote hints at something quite similar to the issues discussed in the previous chapter – namely, the internal heterogeneity of sociology as a field. I argued that this was not regarded as detrimental to sociology as a form knowledge. In terms of addressing the public, however, a few reservations started creeping in that had to do with the fact that many sociologists did not have a clear sense of identity and belonging.

I don't know if I am a sociologist, but I call myself a sociologist.

Tyr

I don't know if the other... respondents have also said this, but we... sociologists... we are not united. We don't have a real community. I don't feel like I were a member of a sociological community in Latvia. That's not how I feel.

Narvi

This may, of course, have to do with the specific institutional trajectories and professional experiences of my informants. That is to say, while there are only two universities that grant doctorates in sociology, my informants simply work for different research collectives and build professional ties with researchers working on similar topics, rather than those with whom they share a disciplinary background. Indeed, this was one of the issues I identified in Chapter 5. There are a number of researchers whose disciplinary identity is unclear – either they received their undergraduate or postgraduate training in sociology but did their advanced degrees in a different field (e.g. Aija Lulle and Laura Sūna), or they have a doctorate in, for example, philosophy, but mostly work with sociologists (e.g. Anna Stepčenko, Taņa Lāce). However, it also suggests a lack of a clearly defined institutional identity. In and of itself such a state of affairs would be unremarkable. However, the disunity of the sociological community in Latvia is significant mainly because their collective identity is defined in terms of professional standards, rather than institutional affiliation, political preferences or theoretical outlooks. Indeed, they are professionals with their own personal views and concerns that have to be modulated to insure that they can perform their duties. In other words, addressing the public in the name of a community is problematic because there is no clearly defined community to speak for.

Conclusions

While some of the dichotomies I chose to deploy were simplistic (for reasons outlined in

Chapter 5), they captured a peculiar fact about Latvian sociology. It was widely acknowledged that distinguishing between the descriptive dimension of sociological work and the evaluations, value-judgements and implicit normative orientations was complicated. This would seem to open the door to charges concerning the politicisation of sociological research; indeed, some authors have gone down this route (e.g. Horowitz 1993). However, due to the fact that most sociological work in Latvia is applied, this was not perceived as a pressing practical issue by the sociologists themselves. The simple fact that the impulse to study a particular topic was defined with specific needs and interests in mind meant that sociologists did not have to concern themselves too much with questions pertaining to value-neutrality. In a way this was similar to a thought expressed by one of the anthropologists I interviewed who argued that value judgements were not a significant problem, because of the relativism implicit in the anthropological stance. In good Weberian fashion sociologists can content themselves with dutifully carrying out a research project. Value-relevance would be defined for them and so would the ultimate ends. However, an autonomous form of sociological discourse was constrained for those very same reasons, and this was freely acknowledged. This is where complications started to arise. While their professional commitments placed them in an advisory capacity, it was quite clear that my informants' responses indicated ambivalence and ambiguity as to the effect this has on sociology and the interests and publics it serves. Thus, similar to Steven Shapin's analysis of natural scientists working outside academia, there was a sense in which my informants were unsure about the effect that applied sociology would have on the long-term development of sociology in Latvia. However, unlike the natural scientists, my informants' responses also addressed the axiological dimension of sociological work by hinting at a perceived tension between sociology as a form of knowledge and sociology as a profession. This should not be construed as a simple juxtaposition of theory and practice, the ideal and the actual, and is, I would argue, best understood as a species of ambivalence that is inherent to debates as to the nature of sociology that are similar in spirit to recent developments in constructivist literature on science.

8. Rethinking Weakness and Ambivalence

In Chapters 6 and 7 I tried to show that the responses of Latvian sociologists illustrated the ambiguities of internal heterogeneity and the way professional commitments and access to funding affect the quality of, and limits the audience for, sociological work. These themes resonate with the arguments I explored in my discussion of science studies. The field provides analyses of science that mesh discussions of politics, economics and epistemology and endeavours to turn philosophical questions into topics that could be researched empirically. The views of Latvian sociologists, however, painted a picture that was at odds with the one provided by constructivist branches of science studies in general and actor-network theory in particular. The latter emphasised the autonomisation and stability of the disciplinary apparatus, whereas the former brought up considerations that figured internal divergence as a source of novel insights. There was a distinct desire to carve out a niche for sociology and define it in opposition to generic and low quality social research, but it was also apparent that instituting clear-cut criteria of acceptance into sociology was complicated and could actually be harmful to sociology as a form of knowledge in the long-term. Likewise, while actor-network theory emphasised the importance of building alliances, my informants expressed ambivalent sentiments about the fact that sociological research can be used to expedite political decisions that do not necessarily serve the needs of the public(s) it affects.

A possible explanation for the discrepancy between actor-network theory and the responses of Latvian sociologists is that the former has posited an ambiguous link between the qualities that characterise good research practices (i.e. criteria applied to science studies scholars) and the strategies that guaranteed the success of the examples of recognised science described in case studies (i.e. criteria applied to the protagonists of case studies). In this chapter I wish to explore this tension and highlight similarities between sociology and science studies. I begin by looking at the discussion ignited by Michael Burawoy's call for public sociology. I then move on to a discussion of the relationship that actor-network theory posits between the way science is practised and the way constructivists should practice their craft. I conclude by suggesting that sociology serves as an example of what happens when a discipline internalises the tension between

political representation and scientific representation present in recent methodological contributions to actor-network theory.

The Ethos of Sociology

Feyerabend's attitude toward science was closer to a Protestant's than an atheist's toward Christianity. Unfortunately, in our blinkered times, to be against the scientific establishment is to be against science itself.

(Fuller 2003: 110)

Paul Feyerabend is often described as a philosopher of science with a certain dislike towards science. However, as I tried to suggest in Chapter 1, his attitude is best characterised as ambivalent. Some of his publications can be accurately described as attempts to foster a critical and sometimes even dismissive attitude towards science (e.g. Feyerabend 1978), but it is very important to note that his arguments are not directed at science but the authority that science commands and the parochial and condescending attitude towards alternative traditions of knowledge that the scientific establishment fosters. Feyerabend considers science to be only one among many ways of relating to, and interacting with, the material world and other people, but this in itself is not a problem for him. In fact, he has argued that, even if schools were to expose children to many different forms of knowledge, science would have little trouble recruiting its fair share of bright minds (Feyerabend 1978; 1999). Furthermore, he believed that science has been a liberating force in the Western world. The problem, as he saw it, was that the current role of science is less clear.

Any ideology that breaks the hold a comprehensive system of thought has on the minds of men [sic] contributes to the liberation of man [sic]. Any ideology that makes man [sic] question inherited beliefs is an aid to enlightenment. A truth that reigns without checks and balances is a tyrant who must be overthrown, and any falsehood that can aid us in the overthrow of this tyrant is to be welcomed. It follows that seventeenth- and eighteenth-century science indeed was an instrument of liberation and enlightenment. It does not follow that science is bound to remain such an instrument. There is nothing inherent in science or in any other ideology that makes it essentially liberating.

(Feyerabend 1999: 181-182)

Feyerabend was, therefore, sceptical as to whether science was still a vehicle for emancipatory ideas. It may well have been a liberating force in the 19th century, but it did not follow that science would continue to be such a force, especially in view of its gradual institutionalisation and ossification. His narrative is, consequently, somewhat at odds with the view of sociology Michael Burawoy provided in his ASA Presidential address (Burawoy 2005a). In it Burawoy argued that the emergence of sociology was tied to (i)

attempts at social reform and amelioration and (ii) the fostering of connections with different public(s) in civil society. His call for sociology to become more public and true to its historical roots suggests that, unlike Feyerabend, Burawoy believed that there was something about sociology that made it essentially liberating. The debate that Burawoy's speech ignited, however, showed that other sociologists were not so sure.

Public Sociology and its Shortcomings

Burawoy's speech and subsequent articulations as to the future of public sociology (Burawoy 2004; 2005a; 2005b; 2005c; 2005d) provoked a wide range of responses. This is not surprising, because the scope and ambition of Burawoy's project is astounding. In different versions of the same constellation of arguments he attempts to (i) provide a stripped-down version of the historical development of sociology, (ii) discern a kind of disciplinary ethos and (iii) theorise on how sociological labour should be divided. Due to fact that very little time is devoted to each of these tasks, not all the necessary qualifications can be made, which leaves the door open for ambiguity and misunderstandings. There are, however, more specific issues with Burawoy's interpretation. These have to do mainly with his take on the history of sociology and the sociological ethos he seeks to derive from it.

While Burawoy is fully aware that he is speaking about American sociology, he seems to ignore a number of issues that would complicate his historical narrative. For example, Evans (2009) argues that American sociology did have its roots amidst social and religious reform and, by consequence, developed a close and direct relationship with various publics in civil society (Ross 1991). However, this was quickly followed by an attempt to solidify its institutional presence and credibility, and distance the activities of professional sociologists from the work done by social activists (Ross 1991; Halliday and Janowitz 1992). Now, there is no obvious tension between what Burawoy is saying and the argument put forward by Evans, but it does illustrate the extent to which Burawoy downplays the importance of events that complicate his narrative.

Burawoy addresses the emergence of professional sociology and the attendant shift from engaging publics to working with the government and various foundations. However, he believes that it is no mere accident of birth that sociology emerged the way it did. Indeed, when Burawoy claims that sociology has its roots in civil society, it should not be understood as a simple statement of fact but as something akin to an image of what

sociology should be like. Furthermore, this special relationship should be nurtured because the fate of sociology is entwined with the vibrancy of civil society.

When civil society flourishes – Perestroika Russia or late Apartheid South Africa – so does sociology.

(Burawoy 2005a: 24)

One of the ways that the erosion of civil society manifests itself, according to Burawoy, is in terms of what kind of sociological knowledge is valued. The less vital civil society becomes, the less reflexive knowledge is required – instrumental knowledge (policy and professional sociology) will do just fine (Burawoy 2005a: 21). In view of this, a number of sociologists have argued that Burawoy has an overly idealised view of civil society and, by extension, sociology (Calhoun 2005; Christensen 2013; Holmwood 2007). For example, Tony Christensen remarked upon a peculiar implication of Burawoy's views:

[i]n framing sociology as the defender of humanity, the sociological ethos is commensurate with the ethos of civil society. Thus, only the publics that reflect this ethos need be engaged. By doing this, the nature of the dialogue between sociologists and their publics poses minimal threat to the values of sociologists themselves.

(Christensen 2013: 38)

In other words, there seems to be something very convenient about Burawoy's idealisation of civil society. Seeing as how Burawoy believes that the fate of civil society and sociology go hand in hand, this move also has clear implications for how he narrates the history of sociology and how and why Burawoy attributes significance to individual turning points in the history of sociology. His discussion of critical sociology illustrates this quite well.

If you recall, critical sociology is a type of sociology that calls into question and challenges dominant assumptions within the field. It is a form of reflexive knowledge whose intended audience is other academics. Burawoy argues that critical sociology was a response to professional sociology and mentions a number authors who exemplify this approach (e.g. C. Wright Mills and Alvin Gouldner). In one way or another, the target of these thinkers was structural functionalism and the dominance it had achieved in American sociology (Gouldner 1970; Friedrichs 1970). However, the aftermath of this “attack” is more complicated to comprehend and analyse. While Burawoy praises the work of Alvin Gouldner, many authors have expressed a more pessimistic view of the turning point in sociology associated with his work. A prominent example of this is Irving Louis Horowitz (1993), who argues that sociology has become overly politicised

and so suffered as a form of scientific knowledge. Burawoy himself, on the other hand, represents the converse view and believes that sociologists should not shy away from political matters and express political opinions. Indeed, his “scissors movement” makes this quite clear:

Over the last half century the political center of gravity of sociology has moved in a critical direction while the world it studies has moved in the opposite direction.

(Burawoy 2005a: 6)

As a description of the political commitments of numerous sociologists it may well be accurate, but his phrasing betrays a semantic ambiguity. Accounts of social statistics illustrate the ambiguity associated with the word normal whereby its meaning oscillated from normal as typical, standard or average to normal as an evaluative term (Hacking 1990: ch. 19). Something similar is going on in Burawoy's work. That is to say, while many or even most sociologists may share Burawoy's political views, it is not clear that there is a necessary connection between this branch of social science and left-wing politics.

Burawoy's intimations provoked a series of different responses. There were those who approved of Burawoy's call, while at the same time elaborating on his arguments, sharing their experiences of what it was like to be a sociologist and to engage different publics, and making suggestions as to how sociology could become truly public (Aronowitz 2005; Baiocchi 2005; Calhoun 2005; Etzioni 2005; Ghamari-Tabrizi 2005; Scott 2005; Stacey 2004; Turner 2007b). On the other hand, there were also sociologists who were not convinced that Burawoy's attempt to inject politics into sociology should be encouraged and thought that it could, in fact, backfire (Brady 2004; Holmwood 2007; Deflem 2005; 2013; Nielsen 2004; Tittle 2004). In particular, there was a belief that politics should be kept out of sociological practice so as not to undermine the credibility of sociology and affect the quality of sociological work. Furthermore, there was concern that Burawoy's call for public sociology would foster a culture whereby scholars with political opinions that were at odds with the sociological mainstream would be marginalised (Christensen 2013; Deflem 2005; 2013). In other words, the argument that public sociology was (i) a desirable form of expression for sociologists as professionals and (ii) an expression of an underlying sociological ethos was problematised.

It is important to note that there was variety in both camps. Those who were critical also expressed sympathy and admiration and vice versa. Nonetheless, most discussants were more cautious than Burawoy about the potential of public sociology and what it

would mean in practice. Moreover, the questions addressed by the participants of this discussion oscillated between (i) whether Burawoy's diagnosis of contemporary sociology was accurate and (ii) whether his vision of the future was appropriate and desirable. In other words, the topics straddled the line between descriptive and normative views as to the future of sociology as a form of knowledge.

Meshing Axiology and Methodology

Normative debate about the kind of sociology we would like to see in the future is an essential component of a healthy discipline.

(McLaughlin and Turcotte 2007: 814)

In 1994 an issue of *Sociological Forum* was dedicated to the question: *What's wrong with sociology?* A number of prominent American sociologists offered their opinions. Among the problems discussed were (i) internal fragmentation (Rule 1994; Stinchcombe 1994), (ii) the inability to impose restrictions on what passes as sociology (Davis 1994), (iii) the lack of a unified theoretical core (Cole 1994), (iv) the lack of a genealogy of research techniques that consigned sociologists to endless debates and conflicts between divergent theoretical positions (Collins 1994), and (v) the politicisation of the discipline (Lipset 1994). All this sounds familiar, and it is not surprising that Thomas Kuhn was mentioned several times. In a subsequent issue, however, there was a response from a collective of feminist scholars who argued that most of the weaknesses of sociology could just as easily be regarded as its strengths (Fitzgerald et al. 1995; Bleiberg Seperson 1995).

Our contention is that what some suggest is the downfall of sociology is actually one of its strengths. The benefits to be derived from contributions of marginalized perspectives far outweigh any short-term advantages that may arise from clinging uncritically to traditional explanations of the social world. Feminism, postmodernism, queer theories, and race and ethnic studies offer sociology the tools and the impetus to look critically at the core, to ask meaningful questions, and to obtain valuable insights.

(Fitzgerald et al. 1995: 496)

What is more, upon closer inspection it becomes apparent that not everyone in the previous issue thought that the weaknesses they listed were actually bad for sociology.

My own belief is that this disintegrated state of sociology represents the optimum state of affairs, both for the advance of knowledge and for the expansion of mind of undergraduates.

(Stinchcombe 1994: 290)

In view of the aforementioned, the argument I want to put forward is that sociology is a form of knowledge in which discussions of methodology do not proceed in isolation from axiological concerns and broader reflections on the value-relevance and

consequences attached to the content of sociological research, and the responsibilities of the practitioners themselves. I would like to clarify, however, that this is not meant to challenge the thesis of value-neutrality, nor am I trying to suggest that value judgements cannot be eliminated from sociological discourse. There have been numerous attempts to sever the link between sociology as a form of descriptive knowledge and the evaluative and prescriptive tendencies characteristic of the work that sociologists produce. These are questions whose scope necessarily exceeds the ambitions of my thesis. Nonetheless, what I am trying to argue for is that even cursory examinations of debates among scholars as to the nature and, more importantly, goals of sociology suggest that such exchanges are fraught with uncertainty and confusion, and mesh discussions of sociology as (i) an empirical and (ii) a moral project. The response to Soviet sociology, my discussions with Latvian sociologists and the debate ignited by Michael Burawoy's ASA presidential address point to a similar issue. These exchanges illustrate an entanglement between knowledge and its purpose, and an awareness of the consequences attendant to different ways of practising sociology. Furthermore, this is supplemented by reflections on what can count as sociology, the kind of sources that sociologists can draw upon and the implications of what this has on the knowledge that is used by clients and audiences.

In other words, there is no great divide in sociology – facts and values, knowledge and politics are mutually responsive and co-constitutive. Theoretical reflection and empirical investigation are subject to push-and-pull mechanisms that are irreducible to either cognitive norms or ideology and unchecked or factually unresponsive value judgements. This is reminiscent of Latour's discussion of hybridity, which regarded science as a blend of politics, material practices and knowledge. However, the peculiar thing about sociology, as I will argue, is that it is a hybrid of a very different sort.

(Re) Describing Science

I would argue that actor-network theory, much like the Strong Programme before it, has succeeded in challenging and repudiating the purity characteristic of popular notions of Western scientific projects. It has illustrated the different ways in which scientific practices have been patterned by factors and influences that were traditionally believed to be alien to sound scientific conduct. Crucially, the notion of hybridity Latour put forward was an outsider's take on science that attempted to rectify a malady characteristic of Western thought (Latour 1991; 1993b; 2004a). Latour explored the ambiguity of

representation and merged two modes of representation that were traditionally believed to be distinct – political representation and scientific representation.

The word “representation” is the same, but the controversy between Hobbes and Boyle renders any likeness between the two senses of the word unthinkable. Today, now that we are no longer entirely modern, these two senses are moving closer together again.

(Latour 1993b: 27)

Both involve an actor speaking on behalf of another, and each is a matter of delegation. Politicians speak for their constituents, and scientists speak for microbes, whales, chimpanzees etc. The homology that Latour posits, however, manifests a difference between his take on science and more conventional ones. Traditionally speaking, political representation involves a delegation of authority, but it also entails a measure of accountability to those in whose name a politician speaks (Brown 2009: 179). Scientific representation, on the other hand, is a question of accuracy and approximation, and pertains to how well a subject or object is reproduced in another medium. For example, a statistical sample can stand in for a population (scientific representation). The quality of the sampling strategy and the persuasiveness of the interpretation can be disputed by other researchers, but neither the statistician nor the sample are held accountable to the population they claim to represent (political representation).

Bruno Latour and science studies more generally would challenge this distinction and argue that the sample defines a collective and its constituents whilst claiming only to render visible certain trends in an ostensibly predefined group (e.g. Law 2009). That is to say, the sample does not represent a pre-existing collective but gives it a definite shape by acting as its representative. This is precisely the point of hybridity – political representation and scientific representations are not insulated from each other. They are entwined in a symbiotic relationship of co-production.

This leads us to the connection between hybridity and sociology. Hybridity may not be a category that individual sociologists would use to describe their own work, but, I would argue, that it is equally plausible that they would recognise that discussions as to the hybrid nature of their work are characteristic of sociology as a field. In other words, sociologists themselves would recognise that their work exists at the intersection between political representation and scientific representation. The issue, of course, is that sociologists are not the only ones who recognise this.

Insiders and Outsiders

In Chapter 4 I argued that sociology was peculiar in that it has been curious about its own history and, consequently, produced narratives that (i) locate the development of the discipline in specific historical circumstances and (ii) portray it as a form of knowledge simultaneously in and about the world. Unlike the natural sciences, therefore, the history of sociology is not written in a carefully edited and cumulative manner. Textbooks may, of course, share some structural similarities (Lynch and Bogen 1997; Manza, Sauder and Wright 2010), but a significant portion of scholarship within sociology is devoted to a thorough understanding of the historical circumstances and political climates that have shaped sociology both as an academic discipline and a form of knowledge. This, I argued in Chapter 3, goes some way towards explaining why early forms of science studies focused on the natural sciences and biomedicine. Indeed, Guggenheim and Nowotny (2003) hypothesise that the development of science studies unconsciously mimics the topical trajectory of social anthropology in that it first studies others. This, however, has made the relationship with cognate disciplines somewhat complicated.

The books of John Law (2004) and Bruno Latour (2005) are two influential examples of slightly different yet fundamentally compatible versions of contemporary developments in actor-network theory. Both are akin to methodological treatises for the social sciences that attempt to reinvigorate extant approaches with insights obtained from science studies.

We know too well that, even in 'hard' sciences, authors clumsily try to write texts about difficult matters of concern. There is no plausible reason why our texts would be more transparent and unmediated than the reports coming out of their laboratories.

(Latour 2005: 124)

In both cases, however, the lessons they take from science, and the theories of science they operate with, produce a slightly ambiguous result. Both authors fully appreciate and accept the fact that the power of science lies, at least in part, in its ability to actively engage with and rework the materiality of the world. Science constructs – it does not merely represent.

Enactments and the realities that they produce do not automatically stay in place. Instead they are made, and remade. This means that they can, at least in principle, be remade in other ways.

(Law 2004: 143)

Law and Latour are keen to learn from this and make a number of suggestions how these insights could be used to benefit the social sciences. However, they do not want the social sciences to become mere copies of their more established cousins in the natural sciences. Their concern is not credibility but a desire to produce enactments of reality that are sufficiently attuned to the complexity and forms of association between humans and non-humans. Furthermore, they are equally interested in being responsible for the enactments they produce and the collectives they enable.

[T]his type of science for that type of social should be as slow as the multiplicity of objections and objects it has to register in its path; it should be as costly as it is necessary to establish connections among the many mediators it finds swarming at every step; and it should be as reflexive, articulated, and idiosyncratic as the actors cooperating in its elaboration. It has to be able to register differences, to absorb multiplicity, to be remade for each new case at hand.

(Latour 2005: 121)

If politics is about better social (and now, we learn) non-social arrangements, and about the struggles to achieve these, then method assemblage and its products can also be judged politically. It does politics, and it is not innocent. In its different versions it operates to make certain (political) arrangements more probable, stronger, more real, whilst eroding others and making them less real.

(Law 2004: 149)

In other words, they illustrate Robert Friedrichs' point about the two levels at which social theories operate – the theory itself and the role for the sociologist this theory entails. Friedrichs posited that this was one of the main differences between sociology and the natural sciences, and is a point where ambiguity kicks in. Do Law and Latour believe that the conscious integration of scientific representation and political representations in the case of sociology is a significant enough difference to make a distinction between the likes of anthropology and sociology on the one hand and the likes of physics and chemistry on the other?

As I suggested in Chapters 1 and 2, science studies aims to challenge dominant conceptions of science, but this is not necessarily a matter of challenging science. Such a reading, however, is not shared by scientists who have voiced their concerns on a number of occasions. Some of the more emphatic reactions were expressed during the so called Science Wars that erupted in the 1990s. A number of scientists and philosophers of science took issue with the way post-modern theorists in general and science studies scholars in particular approached scientific inquiry and intellectual work. An infamous move in these exchanges was a paper published by Alan Sokal. Sokal submitted an article to the journal *Social Text*. The article was entitled *Transgressing the Boundaries: Towards a*

Transformative Hermeneutics of Quantum Gravity (Sokal 1996), and it was accepted for publication. After the paper was accepted, Sokal revealed that it was a hoax – an intentionally nonsensical compilation of left-wing political correctness and quotes from popular post-modernists that pandered to the ideological position of the editors. This was believed to illustrate the lax quality control of *Social Text* and the pollution of scholarship by ideology.

The Science Wars provoked a series of responses from science studies scholars, and a number of them argued in a way that fundamentally mimics the accusations levelled against science studies by the so called science warriors. That is to say, the response of the likes of Sokal was framed in terms of ignorance and a misunderstanding of the import of social studies of science (e.g. Callon 1999; Franklin 1996; Haraway 1997a; Nelkin 1996). An excellent example of a measured response of this nature can be discerned in the work of Ian Hacking (1999). In his book *The Social Construction of What?* Hacking analyses a number of approaches within constructivist science studies and argues that scientists opposed to science studies have confused two different things. According to Hacking, constructivism does not attempt to discredit or refute science but rather to supplement our understanding of it, challenge traditional and oversimplified accounts of scientific inquiry, and point out the social and political consequences of practising science in a certain way. I largely concur with Hacking's point. Indeed, the likes of the Strong Programme and actor-network theory have attempted to justify the credibility of their output by arguing that they exemplify the scientific approach. Hacking's reading of the dispute, however, ignores an important dimension of the debate.

It may be true that the science warriors had misdirected their anger (Nelkin 1996), but it is plausible that they had, nonetheless, accurately diagnosed the problem. That is to say, even if science studies scholars were not to blame for the decrease in funding and increase in suspicion towards science, their work foreshadowed a potential problem for science as an institution. For example, Steve Fuller has raised the point that the suspicions of the science warriors, while based on an inadequate understanding of science and how it works, are based on a fundamentally correct assumption as to the potential consequences of the public accepting science studies' accounts of science (Fuller 2006b; Fuller and Collier 2003). Science warriors do not seem to be as sanguine as Latour about

the public giving up on the idea of science as a pure and disconnected form of activity. Their concerns, I would argue, are caused at least in part from the ambiguity of Latour's position. For example:

The difference between Science and ideology, purity and pollution, even though it has occupied and continues to occupy a great number of intellectuals, thus does not have the efficacy [sic] that one might suppose, considering the energy spent on it, as well as the size of the police forces that patrol the border.

(Latour 2004c: 100)

The above quote contrasts with the argument put forward in *We Have Never Been Modern* where Latour suggested that the authority of science was contingent upon it being regarded as separate from, and purified of, politics (Latour 1993b: 27). Science studies' accounts of science, on the other hand, are perceived as doing the opposite – drawing attention to the seams that hold the science-society relationship together, and the resulting conflict with science warriors is over the ramifications of accepting a disenchanted view of science that highlights its embeddedness in, and formative influence upon, social and political life.

Scientists' concerns are personified by the philosopher Paul Feyerabend (1978). As I have tried to argue above, he was not anti-science. However, the disenchanted view of science he held led him to explicitly argue that science should no longer be given preferential treatment at the cost of other ways of studying and interacting with the world. Moreover, Fuller's argument has the curious implication that scientists are actually more perceptive than science studies scholars as to the attendant consequences of the various case studies the field has produced. Scientists may act upon highly dubious assumptions as to how science operates and what makes it distinct, but their enchanted and primarily methodological vision of science is remarkably productive. This relates back to Latour's point I introduced above. While non-representational forms of constructivism want to suggest that the questions science tackles are hybrid in nature, the rhetorical potency of science comes from narrating a highly edited version of its history and drawing people's attention away from its hybrid nature. The likes of sociology and anthropology are different because both outsiders and insiders systematically bring up this issue in relation to them. Reflection on the goals and ends of inquiry (axiology) is not an afterthought but an integral component of debates pertaining to how it should be carried out (methodology). This is why John Law and Bruno Latour can openly learn from a disenchanted view of science and integrate normative concerns with appeals to

descriptive adequacy. Robert Friedrichs was, therefore, only partially correct in his diagnosis of the difference between sociology and the natural sciences. It may be true that social theories come bundled with an implicit understanding of the sociologist's role and responsibility towards society, but the more pertinent difference is that the natural sciences have simply extricated themselves from the perception that their theories and practices are equally implicated in the formation of both human and non-human collectivities and realities (Latour 1993b). Sociology, on the other hand, has been more receptive to this idea (though not necessarily about the role of non-humans) and subjected this question to debate, though there have been attempts to learn from the natural sciences in a way that replicates their strategy of maintaining institutional credibility (e.g. positivism). In Chapters 6 and 7 I tried to suggest that an awareness of disciplinary and institutional frailty was accompanied by ambivalence with regards to the best way to respond to it. Funding may be limited and public perception of sociologists and sociological research may be somewhat simplistic, but this does not give way to consolidation, imposition of rigid criteria of quality and an indiscriminate forging of alliances

The Sociological Self

Science studies in general and actor-network in particular draw our attention to the ways in which our forms of knowledge and our ways of life are entangled. However, our awareness of this does not discredit our attempts to understand and interact with the world. The insights provided by science studies raise the possibility that ethical dilemmas are not external to science and technology but frame and go hand in hand with seemingly technical solutions (see above). The issue is that the implications of science studies accounts of science for our understanding of science have largely been dealt with implicitly and it is sometimes no longer clear why science should be our tradition of choice in view of the evidence that it lacks the distinctiveness that traditional (e.g. philosophy of science, sociology of science) accounts attributed to it. By emphasising the importance of purified and resilient socio-material networks actor-network theory tried to address this issue, but applying these principles to the discipline of sociology proved difficult. As I have tried to show, sociology's diffuse and tentative nature seems like a clear example of failure, but a closer inspection of constructivist literature reveals a more complicated picture, beautifully illustrated by a quote at the end of Dick Pels' book

Unhastening Science.

Speaking in comparative terms across the spectrum of social institutions, we could say that such craving for strength and certainty better fits the practices of politics and business than the production of scientific knowledge – a ‘strong Britain’ with a ‘strong economy’, shored up by a ‘strong pound’, as Blair and Brown would have it. In this constellation, weakness and uncertainty could be the typical contributions of (social) science to the shape of the world. It could say things that are interestingly feeble, shaky, risky, and weird.

(Pels 2003: 219)

Pels' characterisation of the social sciences shares some similarities with the work of Isabelle Stengers, John Law and Bruno Latour, and points to the tension within constructivist science studies. Their accounts of why a particular group of scientists or technology was successful highlight strategies through which stability and order were achieved. That is to say, sciences work by (i) building a resilient network of humans and non-humans and (ii) concealing the seams that hold the network together. Bourdieu's emphasis on clear standards and criteria of quality betrayed a rigidity that stemmed, in part, from his attempt to model sociology on the other sciences. This was echoed in Bruno Latour's discussion of autonomy and mobilisation whereby a science claims parts of reality for itself by developing methods and stabilising a specialist discourse to identify competent practitioners. My conversations with Latvian sociologists and discussion of Burawoy's call for public sociology, however, illustrated an ambivalence about the question of determining what could legitimately be called sociology, and indicated a plurality of views as to the political and scientific project of sociology. What to make of this?

In his book *The Chaos of Disciplines* Andrew Abbott suggests that a common characteristic of sociology is that it is interstitial in character (Abbott 2001a: 6). It is a loosely structured discipline and lacks clear criteria according to which it can deny entry to a particular school of thought or methodological approach. The response to this state of affairs, however, is varied and often takes the form of fractal splintering. Curiously enough, Abbott suggests that such splintering is also evident in the constructivist wing of science studies (Abbott 2001a: ch. 3). The common enemy of early science studies was the deferential attitude towards science exemplified by analytical philosophy of science and Mertonian sociology of science. The rejection of these approaches was followed almost immediately by internal splintering. My discussion in Chapters 1 and 2 followed a similar narrative. The crucial point, however, is that there are important similarities

between sociology and constructivist science studies. Abbott's analysis focuses on the potential for internal divergence, but an equally interesting possibility to consider is that, much like sociology, contemporary constructivist science studies scholars (ANT/post-ANT in particular) emphasise the need to reveal the contingency, open-endedness and fragility of social and material realities.

The question is: what does standard method assemblage silence? Which possible realities does it refuse to enact in its dominant insistence on that which is smooth? And how might it be crafted differently?

(Law 2004: 144)

Methodological treatises written from a constructivist perspective manifest a peculiar tension in that what works for others is not necessarily something that we as social scientists should emulate in our own research. We should learn lessons from how other scientists approach their objects, but the pursuit of purity that is so characteristic of the natural sciences is recast in a slightly different manner. In fact, Pels' invocation of 'feeble, shaky, risky and weird findings' seems to be more in line with what the likes of John Law and Bruno Latour have in mind when they talk about the qualities that our research should exhibit.

I will call such a description a risky account, meaning that it can easily fail—it does fail most of the time—since it can put aside neither the complete artificiality of the enterprise nor its claim to accuracy and truthfulness.

(Latour 2005: 133)

Furthermore, detachment and autonomy are not prominent features of their accounts. This, as I argued in Chapter 4, is largely due to the fact that the responsiveness of science to political and practical concerns is not figured in a negative light. Different voices should be allowed to voice their concerns and participate in the construction of the common world.

[T]he opposition between a detached, disinterested, objective science and an engaged, militant, passionate action becomes meaningless as soon as one considers the formidable collecting power of any scientific discipline—and it makes no difference if it's 'natural' or 'social'.

(Latour 2005: 253)

In response to this, Steve Fuller has argued that actor-network theory in general and Latour in particular exemplify 'the Mode 2 conception of policy-driven “postdisciplinary” research, which welcomes the university's permeability to extramural concerns' (Fuller 2000b: 9). Mode 2 refers to the way scientific knowledge has been produced in recent

times. The key characteristics of this mode of production is that teams of researchers are brought together for short periods of time (e.g. the duration of a project) and are generally interdisciplinary in composition (Gibbons et al. 1994). This is contrasted with so-called Mode 1 knowledge production that focuses on fundamental, rather than applied, research and is generally organised around discrete disciplines. As I mentioned earlier, Bjorn Wittrock (2003) has suggested that the distinction between Mode 1 and Mode 2 science does not lend itself easily to an analysis of the social sciences. This is mainly because the social sciences have never been in a position where they primarily generate fundamental research. On the contrary – applied research has been more prominent. However, this has historically been supplemented by an interrogation of the interests that such research serves and the goals it furthers. This is why I claimed that the peculiarity of sociology lies in a conscious integration of methodological and axiological concerns. That is to say, sociological debates as regards how best to study the world do not proceed in isolation from the question of why and for whom such knowledge could be useful. What is more, such questions are never answered definitively. Indeed, most definitive answers are challenged and deconstructed as a matter of course. It could be argued, therefore, that the issues associated with Soviet sociology derive from the attempt to stabilise, impose closure upon, and give a conclusive answer to, the question of whom sociology should serve and the form such knowledge can take. The uncertainties evident in my discussions with Latvian sociologists represent a converse strategy and, by extension, the conflicted nature of integrating scientific representation and political representation, and an awareness of one's own hybridity.

Conclusions

In Chapters 6 and 7 I analysed my conversations with sociologists working in Latvia and suggested that they painted a picture of an internally diffuse and heterogeneous disciplinary unit whose members were unsure as to the best way to respond to the current state of the discipline. The discussion initiated by Michael Burawoy's ASA Presidential address was a catalyst and platform for the expression of similar uncertainties in various prominent sociology journals. A number of commentators took issue with Burawoy's analysis of sociology and his suggestions for the future development of the discipline. Among the points raised against his version of sociology was Burawoy's belief in a necessary link between the sociological ethos, civil society and

left-wing politics. What is more, even though he himself argued for the need to provincialise American sociology, and so recognise its specificity, his vision of how roles and duties are distributed among the different types of sociology betray his position as a sociologist speaking from within an American context (Baiocchi 2005; Urry 2005; McLaughlin and Turcotte 2007). Nonetheless, his attempts to ground public sociology in a sociological ethos sparked a lively debate and, more importantly, addressed the relationship that sociological knowledge has to its putative object (i.e. society).

This resonated with my conversations with Latvian sociologists and analysis of outside responses to Soviet sociology. They highlighted an intermingling of methodological and axiological concerns that figured sociology as implicated in the worlds it attempts to describe. Contrary to the likes of Michael Burawoy and Pierre Bourdieu, however, the debates I looked at showed signs of suspicion about attributing a particular political stance to sociology.

I continued by suggesting that such exchanges can be regarded as similar in spirit to science studies attempts to reframe our understanding of science in a manner that reconnects ways of knowing with ways of being in the world. The specificity of sociology, as compared to the other sciences, is that, in its own way, this approach to knowledge has traditionally been a part of it, albeit in a highly dynamic and volatile form that is conducive to internal divergence and disagreement. What is more, the peculiarity of sociology and its disciplinary and institutional frailty serves as an illustration of the ambiguity at the heart of recent methodological developments in actor-network theory, as the characteristics that make sociology a weak discipline from a constructivist point of view are those that resonate most with the qualities that a constructivist study itself should manifest.

Conclusions

This thesis has been an attempt to understand the specificities of sociology from the perspective of constructivist science studies. However, my approach to this topic has also been motivated by an interest in the normative dimension of science studies, or lack thereof. I commenced my discussion by introducing the work of Robert K. Merton. In particular, I focused on his attempts to articulate a scientific ethos, as well as the debates that served as the impetus for his take on science. I continued by suggesting that science studies has challenged and often successfully repudiated the theories of science provided by analytical philosophy of science and early sociology of science. The Strong Programme was emblematic of this approach. By foregrounding the importance and impact of external factors on the production and reach of scientific knowledge the Strong Programme redefined the link between science and society in a way that gave a prominent role to political interests and the social context. Not coincidentally, this strategy was perceived as a threat to the philosophical tradition because it promoted a thoroughly socialised form of scientific rationality and challenged the distinctiveness of the scientific tradition. This was seen by some as an attempt to argue for relativism and a sociological monopoly over the analysis of science.

In Chapter 1 I noted that the Strong Programme's way of analysing and explaining particular episodes in the history of science had been called into question from within science studies itself. This, as I suggested, was based on the concern that the Strong Programme assumed the stability of social categories, such as power and interests, but encouraged researchers to see the plasticity and interpretive flexibility of the theories, instruments and phenomena with which natural scientists worked. The recognition of the abovementioned asymmetry and subsequent attempts to rectify it have given rise to a form of analysis that explores the entanglements of science and society in a way that seeks to curb the excesses of scientific exceptionalism and technological determinism on the one hand, and sociological determinism on the other. Among the main proponents of such an approach to the study of science have been the authors working in the tradition of actor-network theory.

In Chapter 2 I argued that, contrary to the Strong Programme, actor-network theorists

did not assume that the social was robust enough to mould the content of scientific theories and shape the course of scientific debates. Instead the latter contended that the social was itself a product sociomaterial networks and heterogeneous connections between humans and non-humans. The particular forms that such networks take, and the realities they enable, are certainly revealing and subject to contingency, but these are the result of complex entanglements that combine heterogeneous materials, not a mere reflection of a pre-existing context. Much like the Strong Programme, actor-network theory manifested a dislike for philosophical accounts of the analytical tradition and endeavoured to illustrate the permeability of the boundary between science and society. Unlike the Strong Programme, however, society was not given a privileged role, and focus was placed on the practices and processes through which particular renderings of the science-society relationship took form.

However, the fluid and artefactual nature of the boundary between science and society, as well as the entanglement between science and politics, undermined the possibility of outlining a prescriptive approach to science or criticising particular pieces of scientific research on epistemic or ontological grounds. To address this issue, I turned to the work of Isabelle Stengers who aimed to explicate the difference between interesting and inquisitive articulations of social and material realities, and flawed approaches that force their objects to conform to their way of seeing it. Among other things, she did this by looking to ontology, rather than epistemology, and, while Bruno Latour has drawn on her work, the latter's relational approach was difficult to combine with Stengers' normative account. A side effect, I suggested, was that Latour's variant of actor-network theory was ill-suited to identifying forms of science that have been contaminated by cultural prejudice, political ideology or economic interests.

I tried to illustrate this in Chapter 3 and looked at the debate that emerged in Western academic outlets over the nature, limits and idiosyncrasies of the Soviet variant of sociology. The discussions were varied in tone and focus, but the common elements and points of convergence concerned the fusion of political commitments and scholarly pursuits and ambitions in general, and the dominance of the former over the latter in particular. I tried to challenge the claim that this made Soviet sociology different from its Western counterpart, and I suggested that a close relationship between sociological research and government institutions was not unique to the Soviet Union. However, the

fusion that occurred in the Soviet Union between the disciplinary ethos of sociology and a particular form of ideology that imposed restrictions on sociological research and the repertoire of sources and arguments it could draw upon was a more contentious issue. Nonetheless, to criticise this move is to assume that (i) practitioners of a discipline should have autonomy and freedom as regards the scholarly resources they can use and (ii) the locus of recognition and evaluation for scholarly contributions is and should be the scientific community. This combination of privileges, as I tried to suggest, has been complicated to obtain for sociology. Furthermore, science studies literature has paid little attention to this. In an attempt to rectify this I contrasted the work of Bruno Latour and the account of sociology provided by Pierre Bourdieu.

Bourdieu suggested that sociology was plagued by a number of weaknesses that were less pronounced in the other sciences. In particular, the issues he discussed revolved around the difficulties associated with obtaining autonomy and developing criteria that would allow one to distinguish between good sociology and politically complicit sociology or folk sociology. In Latour's terminology, the problem with sociology was that it had not managed to develop methods of mobilising the parts of reality for which it aimed to speak, nor had it outlined principles according to which practitioners of a discipline could identify and evaluate one another's contributions. I suggested that this may be so because, unlike the other sciences, sociology had been ambivalent about the alliances that had allowed the discipline to flourish. Bourdieu was equally sensitive to this characteristic of sociology, but his emphasis on factors that derived from the politically subversive nature of sociological knowledge disregarded the extent to which the potency and mobilising abilities of sociology were diminished by the way it was perceived. This brought me back to a discussion of Bruno Latour's work and his argument that public representation was a crucial component of the flow of scientific knowledge. The work of Steve Fuller served as an illustration. He argued that sociology had experienced difficulty establishing itself as a science partly because it had been open about its status as a form of knowledge that was simultaneously in and about the world. I tried to explore this in more detail by looking at attempts to connect the development of sociology to ambient conditions in the political landscape and academia.

Overall, my explorations in Chapters 1-4 revealed a tension and parting of the ways between the explanatory strategies favoured by constructivism and the normative project

exemplified by early sociology of science and philosophy of science. What is more, in Chapter 3 I made the case that the approach to science favoured by constructivist science studies is better suited to the study of the natural sciences. This claim was explored further in my conversations with sociologists working in Latvia. They were practitioners of a discipline whose primary output was, and still is, applied research, and their stories and comments suggested that the current state of affairs is not conducive to a thriving form of sociology. This, however, was not accompanied by a pervasive desire to establish forms of professional certification that prevented non-sociologists from contributing to the stock of sociological knowledge. There were concerns that this might harm the reputation of sociology, but an openness to alternative perspectives and novel insights was believed to be invaluable to the long-term health of the discipline. Similarly, unlike actor-network theory, which emphasises the importance of building alliances, Latvian sociologists expressed ambivalent sentiments about sociological research being used to expedite political decisions that do not necessarily serve the needs of the public(s) it affects. Most importantly, my conversations raised the possibility that actor-network accounts may fall short of understanding sociology in a broad sense. This, however, was not due to some flaw in the descriptive apparatus of constructivism or actor-network theory. Quite the contrary, constructivism is a useful framework for understanding the way that disciplines are established, claim parts of reality for themselves and forge alliances with human and non-human actors. These accounts, however, are plagued by a bifurcated explanatory strategy. For example, actor-network theory focuses on and unpacks the processes through which networks are built, relationships are forged, and stability is achieved, but it is noted that the success of particular scientists or technologies is accompanied by a concealment of the seams that connect ways of knowing and reworking the world with particular modes of living in it. Indeed, Latour's claim that science is politics by other means (1993a: 229) can sometimes be taken to mean just that – the reconfiguration of the political landscape with the tools afforded by science and technology. The efficacy of such a strategy, however, is based on science and politics being kept apart. That is to say, science and its products are successful because they are perceived as disinterested. They are a reflection of what the world is like, what it allows and what it will countenance. The aims and goals of such exercises are seldom questioned. This, again, is different in the case of sociology. Sometimes it is dismissed as an overly complicated and ornate way of stating the obvious. Other times its integrity and

credibility is challenged by drawing attention to the partisan inclinations of its practitioners. Furthermore, these are issues that continue to attract attention both within the discipline and from outside commentators.

A prominent recent example is the debate initiated by Michael Burawoy's call for public sociology and his attempt to ground this project in a sociological ethos. Burawoy's arguments sparked a lively debate and, more importantly, tackled the relationship that sociological knowledge has to its putative object (i.e. society). In particular, he highlighted the intimate connection between different kinds of sociology, the varied purposes which they served, and the realities they enabled. This inspired a range of responses that illustrated a plurality of views as regards the form that sociology should take, and the goals that it can – and should – serve. As I chose to put it in Chapter 8 – methodological discussions do not proceed in isolation from, and without regard for, axiological concerns. Furthermore, this suggested a number of similarities between sociology and contemporary science studies that lead us to the problem of reflexivity.

In the Introduction I argued that the problem of reflexivity has a special meaning for the arguments put forward in this thesis. Traditionally, the issues associated with reflexivity were raised on account of sociology's presuming that it could identify the factors that influence the development of scientific knowledge whilst itself being exempt from the formative power of context (understood broadly). Furthermore, a potential objection against this thesis might be that it is an instance of a discipline examining itself. However, as I suggested earlier, the specificity of sociology introduces a number of complexities.

First of all, this thesis is different because it concerns a discipline that has played a significant part in the development of science studies. Indeed, the simple fact that science studies is occasionally subsumed under the headings “the new sociology of science” or “constructivist sociology of science” betrays the intimacy of the link. Secondly, my explorations suggested that a constructivist study of sociology revealed a discrepancy between the perspectives of the subject and the object. In itself this is hardly surprising. In fact, the so-called Science Wars illustrated that natural scientists and their allies were concerned about the perceived threat that the social study of science poses to the authority of the scientific enterprise. Yet the consequences and implications of science studies accounts for our understanding of, and attitude towards, science have been difficult to articulate clearly. This is neatly summarised in the quote below.

It is true that STS'ers are not out to debunk science in the trivial sense of denouncing its results as false, or unsupported, nor would they dismiss its products as generally harmful. Many STS'ers fully appreciate the great benefits that mankind [sic] has reaped from the scientific institution. But it is equally true that they radically reconstrue the source of its validity and authority.

(Collin 2011: 202)

In Chapter 2 I tried to make a very similar point – namely, that actor-network theory and constructivism more generally lack a clear account of what constitutes good science, even though the analytical tools they afford can explain why particular examples of scientific research were successful. Things are different in the case of sociology, whose hold on the title of science is, and has historically been, tenuous – in the eyes of the public at least – and the fragility of sociology makes it a complicated object for science studies in general and actor-network theory in particular. Sociologists' attempts at autonomisation are routinely re-evaluated and lead to internal splintering as to the best way to practice sociology and to study social groups. The alliances sociologists build with government institutions or interested publics are vigilantly examined so as not to allow ideological allegiances and partisan inclinations to interfere with sociological work. Indeed, the mobilising power that is so crucial to the success of the natural sciences is elusive and unpredictable in the case of sociology, which, from a Latourian point of view, seems like an example of failure. However, I have argued that this is not simply a matter of sociology being an immature science. Rather, as I have tried to suggest above, it is due to the fact that sociologists – occasionally to their own detriment – have shown a greater interest in what constitutes a science and a more pronounced concern as to the goals that such a science should aspire to. As such, sociology shares the science studies impulse to gain a richer understanding of science, the source of the prestige and authority it commands, and the realities in whose service it is deployed. Crucially, sociology is a useful illustration of what happens when the intimate connection between political representation and scientific representation is not concealed, and hybridity is acknowledged.

In conclusion, my analysis of sociology shows that Latour may have been right in suggesting that purity is the result of concealing the way politics and culture permeate knowledge, as sociology's inability to consistently purify itself has been a source of difficulty. However, I have also tried to show that purity is not something that actor-network accounts themselves strive for. This suggests that a lack of purity or, indeed, a lack of stability is not something that can be unequivocally regarded as a weakness or

fault, even though the disciplines that science studies generally looks at are better at concealing the seams that keep their authority intact.

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Appendix 1: Interview Guide

1. When and why did you develop an interest in sociology?
2. What made you consider doing a Ph.D.?
3. What would you say is the object of study (of sociology) if you had to explain it to someone who knew nothing about sociology?
4. What, if anything, differentiates sociology from the natural sciences?
5. Why is sociological knowledge (be it theory or applied research) valuable?
6. Can sociology claim to be objective?
7. Are quantitative methods better than qualitative methods at limiting the researcher's influence on how data is interpreted?
8. What, in your opinion, are the indicators of good quality research?
9. What distinguishes the knowledge possessed by sociologists from lay sociological intuitions and the practical skills necessary to participate in everyday social rituals?
10. What, if any, are the difference between sociological theory and figurative social theories and allegories? Are they an academically acceptable addition to the sociological debate?
11. Is it possible (and desirable) to distinguish between the descriptive and normative (e.g. value judgements) aspects of sociological research?
12. Is sociology a politically neutral discipline? Should it be?
13. What, in your opinion, are the functions of sociology?
14. What, in your opinion, is the role of sociology in the development and practical implementation of public policy?
15. What are the insights that sociologists could provide to policy makers?
16. In your opinion, is there enough institutional and financial support for sociology to successfully participate in policy-making?
17. Is the sociologist responsible for how her findings and interpretation are used?