JAANA EIGI

The Social Organisation of Science as a Question for Philosophy of Science

University of Tartu
2016

The dissertation has been accepted for defence of the degree of Doctor of Philosophy (PhD) in Philosophy by the Council of the Institute of Philosophy and Semiotics, University of Tartu, on the 6th of June 2016.

Supervisor: Dr Endla Lõhkivi

Opponent: Dr Kristina Rolin (University of Helsinki, Finland)

Defence: the dissertation will be defended at the University of Tartu, Estonia, on the 17th of August 2016, at 14.15, in the Senate Hall of the University of Tartu.

The work on the dissertation has been supported by the Graduate School of Linguistics, Philosophy and Semiotics at the University of Tartu; the European Social Fund's Doctoral Studies and Internationalisation Programme DoRa, which is carried out by Foundation Archimedes; the national scholarship programme Kristjan Jaak, which is funded and managed by Archimedes Foundation in collaboration with the Ministry of Education and Research; the Centre of Excellence in Estonian Studies (European Union, European Regional Development Fund) and the research projects IUT20-5 (Estonian Ministry of Education and Research) and PUT732 (Estonian Research Council).

European Union European Regional Development Fund
Investing in your future
European Union European Social Fund
Investing in your future
DoRa

ARCHIMEDES
HARIDUS- JA TEADUSMINISTEERIUM

ACKNOWLEDGEMENTS

Completing the thesis would not have been possible without the help of many people. I am happy that now I have an occasion to express some of the gratitude I feel.

I would like to thank my supervisor Endla Lõhkivi who was immensely supportive and encouraging over these years. I am also grateful to Endla for inviting me to take part in several research projects under her supervision. Taking part in empirical studies of Estonian academic culture and practices enriched my understanding of science. I greatly enjoyed working with Endla and another wonderful colleague—Katrin Velbaum; I learnt much from them.

I would like to thank my colleagues at the department of philosophy of the University of Tartu; they make the department a good place to be. My special thanks go to Alexander Stewart Davies, Ave Mets, Kadri Simm and especially the former colleague Michiru Nagatsu who at different times acted as opponents at my PhD seminar presentations, and to Riin Kõiv, Taavi Laanpere, Katrin Laas-Mikko and other fellow PhD students who commented on my presentations. I also owe a debt of gratitude to the late Rein Vihalemm; I am sad I will never be able to thank him in person.

In 2014/2015 I spent four months at the University of Vienna. I would like to thank Martin Kusch who with exceptional generosity accepted me as a visiting PhD student and found time to meet with me regularly and to act as my supervisor for the duration of my stay. I would also like to thank two members of the department of philosophy in Vienna, Katherina Kinzel and Veli Mitova, who very kindly found time to read parts of my work and provide feedback.

Since 2013, I have worked as an assistant at the Centre for Ethics of the University of Tartu. I would like to thank the head of the Centre Margit Sutrop and my colleagues; they are a great collective to be involved with.

My research benefited from the possibility to present my work at several international conferences; I would like to thank the audiences at those conferences for the feedback and encouragement. My conference travels were at different times made possible by financial support from the Graduate School of Linguistics, Philosophy and Semiotics at the University of Tartu; the European Social Fund's Doctoral Studies and Internationalisation Programme DoRa, which is carried out by Foundation Archimedes; and the national scholarship programme Kristjan Jaak, which is funded and managed by Archimedes Foundation in collaboration with the Ministry of Education and Research. My stay in Vienna was possible thanks to the grant from the European Social Fund's Doctoral Studies and Internationalisation Programme DoRa, which is carried out by Foundation Archimedes. The work on the thesis was also supported by the Centre of Excellence in Estonian Studies (European Union, European Regional Development Fund) and the research projects IUT20-5 (Estonian Ministry of Education and Research) and PUT732 (Estonian Research Council). I am grateful to these institutions for their generous support.

Some material in the thesis has previously appeared in the article form. The section on Sheila Jasanoff's civic epistemologies and the petunia controversy is based on my paper in *Acta Baltica Historiae et Philosophiae Scientiarum* (Jaana Eigi. "'Knowing things in common': Sheila Jasanoff and Helen Longino on the social nature of knowledge", *Acta Baltica Historiae et Philosophiae Scientiarum*, Vol. 1, No. 2 (2013), pp. 26–37). The section on Justin Biddle's argument and Helen Longino's conception of objectivity is based on my paper in *THEORIA*: *An International Journal for Theory, History and Foundations of Science* (Jaana Eigi. "On the social nature of objectivity: Helen Longino and Justin Biddle", *THEORIA: An International Journal for Theory, History and Foundations of Science*, Vol.

30, No. 3 (2015), pp. 449–463). The sections that establish connections between philosophy of science and science policy using Mark Brown's ideas, discuss Alison Wylie's analysis of archaeological ethics and show the relation of my position to other versions of more applied philosophy of science are based on my paper in *Synthese: An International Journal for Epistemology, Methodology and Philosophy of Science* (Jaana Eigi. "Different motivations, similar proposals: objectivity in scientific community and democratic science policy", *Synthese: An International Journal for Epistemology, Methodology and Philosophy of Science* (2016), DOI 10.1007/s11229-016-1077-1, here used with permission of Springer). I am grateful to the journals for the kind permission to reuse this work.

I would like to thank Ruth Jürjo who did a wonderful job editing the Estonian summary of the thesis.

Finally, I would like to thank several important people in my life. I am grateful to my parents, Tatjana Morozova and Raido Eigi, and to my extended family—I have always felt their love and support. And I am grateful to Adam Watkin, who makes everything in my life better.

INTRODUCTION

Reading philosophy of science writings of roughly the last twenty-five years one may be struck by something of a new theme emerging alongside the more traditional topics. The arguments within this trend may have remarkable differences in scope, object of application and the ultimate aim and may show considerable disagreement with each other. They all, nevertheless, share two common features. First, they are introduced as an attempt to provide a more adequate account of science in which the recognition that science is in some sense social plays a crucial role. Second, they use such an account to justify the desirability, or the necessity, of changes in the way science is organised socially. Some examples show the forms these arguments may take. Justin Biddle argues that the social circumstances of research have epistemic significance: for example (Biddle 2014), the current organisation of patenting and licensing in biotechnology may potentially inhibit research. Therefore, there are reasons to modify the system and Biddle outlines possible changes. Heather Douglas (2009) develops an account of the role of values of science that is meant to replace the unsustainable idea about the value-freedom of science; as the current practices of decision-making and justification of claims in science are premised on the ideal of value-freedom, they need to change too. Philip Kitcher's (2003; 2011c) model of well-ordered science offers a considerable revision of the traditional philosophical and lay ideas about science in order to enable a discussion of science as a part of democratic society. On its basis, Kitcher proceeds to show how science may be brought into a greater harmony with democracy. Miriam Solomon (2001) argues with her social empiricist account that rationality of science is a social rather than an individual phenomenon. This, in turn, has implications for the research planning and funding practices that are currently based on the individualistic approach.

The aim of my thesis is to discuss several issues related to this new theme, to analyse one prominent and influential approach to the question of the social organisation of science— that of Kitcher's— and to propose how this question could be approached so as to avoid the problems I demonstrate in the case of Kitcher's account.

In the thesis, I am interested, first, in the factors that have prompted this social and meliorative trend in philosophy of science and the reasons that can be given for it. Why do philosophers argue, with a palpable sense of urgency, that science is social and that changes it the way it is organised and practiced are necessary? While I do not attempt to offer *the* definite story, I offer *a* story explaining the growing prominence of this theme. I also discuss considerations that a philosophical proposal about the social organisation of science should take into account. Second, I am interested in different ways the claim that science is social can be understood in philosophy of science and used to support specific proposals about the organisation of science. Again, I do not aim to offer *the* definite overview of existing arguments. Rather, I overview a range of possibilities for making this kind of argument in philosophy of science. Third, my aim is to discuss probably the most ambitious argument of this kind, Kitcher's well-ordered science, and to offer a detailed critique of it. After that, I outline an alternative approach to the social nature and the social organisation of science, based on Helen Longino's (1990b; 2002a) ideas. The analysis of the development of Kitcher's account, my criticism of this account and the characterisation and the defence of the alternative approach constitute the bulk of the thesis.

The themes outlined are developed in the thesis as follows. In the first chapter I discuss the growing prominence of philosophical arguments that have the social organisation of science as their main subject. Philosophical interest in the social aspects of science is not by itself new, as I show with the help of some examples, and the philosophers writing about the social aspects of science usually acknowledge that. In the chapter, I discuss how the renewed interest in the social aspects of science can be explained by the convergence of several factors. I describe a number of challenges to the traditional philosophical picture of science, coming, first, from the rise of alternative analyses of science and, second, brought about by important changes in the character of science itself. Philosophical accounts of social aspects of science may be seen as a response to these challenges. Besides that, the interest in these aspects of science can be further supported by the trend of practice-oriented approach to science that has been gaining ground within philosophy. The first aim of the chapter is thus to show that there are good reasons for philosophy of science to be interested in the social aspects of science and its social organisation. The second aim is to outline some considerations such a philosophical argument should take into account. I use the contrast between two approaches to making a proposal about the social organisation of science, James Brown's (e.g., 2008a) and Janet Kourany's (e.g., 2010), as the starting point. James Brown formulates his proposal as a purely epistemic, or methodological, proposal; Kourany proposes a reform with explicitly political and ethical motivations. I argue that the position of immunity to practical considerations that James Brown takes is unsustainable and that a proposal about the organisation of science should recognise both epistemic and practical consequences of a form of the social organisation of research.

In the second chapter, I give an overview of various arguments to the effect that science is social in some sense or that some of its social aspects are important, contrasting them with what is taken to be the traditional, a-social position. Some of these arguments can be in principle, and are in practice, used to support proposals how science should be organised, practiced and governed. I explore the connections between the two, showing how on the basis of a particular account of science as social, certain organisational proposals may follow. In the chapter, I distinguish accounts that primarily focus on the social understood as social values and accounts that understand the social in the sense of interactions and institutions. These different understandings of the social, in turn, may offer different opportunities for making a proposal about the social organisation of science. These differences are due to the fact that different accounts bring to the fore different aspects of science as open to modification, at the same time allowing others to fade into the background. I conclude that paying attention to different aspects of the social and the questions related to their integration is another important consideration to take into account when developing proposals about the organisation of science. I introduce Longino's critical contextual empiricism as an example of such an integrative approach.

The central part of the thesis—chapters 3 and 4—are dedicated to Kitcher's well-ordered science. I describe in detail the development of Kitcher's account from the account that shows the importance of the social aspects of science for the achievement of its epistemic aims to the account that allows discussing this epistemic dimension in connection with aims and values of democratic society. I find Kitcher's concerns congenial and his arguments deserving attention. I believe that a proposal for the social organisation of science should attend, as Kitcher's account does, to the interplay of various senses of the social within science, the practical consequences of the way science is organised, the place of science in democratic society and its relations with the public. I argue, however, that Kitcher's proposal for well-ordered science is problematic in several important respects and ultimately inadequate for the achievement of the aims Kitcher sets. The criticism in the chapter 4 focuses on the intertwined issues related to Kitcher's conception of expert in well-ordered science, experts' knowledge and the role of the public. So, I show some important unresolved tensions in Kitcher's characterisation of experts and the expectations about their role in well-ordered science. I then discuss the possibility of important blind spots and gaps in experts' knowledge when working out solutions addressing various groups' practical needs, and Kitcher's failure to use opportunities to remedy them. Finally, I argue that despite the strong democratic orientation, Kitcher's proposal fails to support actual public involvement in well-ordered science. I show how these issues interconnect and exacerbate each other and I trace their origins to a particular global approach to science that Kitcher adopts. I conclude that the problems in Kitcher's well-ordered science are serious enough to necessitate a search for an alternative approach.

The final part of the thesis, consisting of chapters 5 and 6, describes and defends such an alternative, using Longino's social account of science as the foundation. I argue that it allows giving a systematic treatment of different senses of the social that Kitcher's model covers and enables to address the issues of the epistemic quality of science and its place in democratic society that Kitcher addresses, avoiding the difficulties that I have identified. In particular, it allows for a more coherent approach to ensuring experts' objectivity and the validity and applicability of their knowledge, and for a more democratic approach to the public involvement in science and science policy.

Another important advantage of Longino's account is the recognition of the importance of changes in the wider social and political context of science for the possibility to improve the social organisation of science. The relations between philosophy of science and developments in science policy thus acquire crucial importance for a proposal about the social organisation of science. Drawing on Mark Brown's (2009) work on representation in science and democracy, I show that in science policy there exists an approach to objectivity and public participation similar enough to Longino's account of objectivity. As a result, certain changes in the organisation of science and science policy can be recommended for similar reasons in both the philosophical and the political sphere. Accordingly, I suggest that some developments in science policy, albeit initiated for non-philosophical reasons, may be close enough to what the philosophical proposal recommends to be useful for understanding opportunities and obstacles for realising such a proposal in a particular local context. To substantiate this suggestion, I present two case studies based on Sheila Jasanoff's (2005) comparative analysis of biotechnology politics and Alison Wylie's (e.g., 1996 and 1999) work on the development of professional ethics in American archaeology. These case studies are an example of a strongly local philosophy of science that uses a social account of science as the starting point for discussing particular knowledge-producing communities and the way they are embedded in local political and cultural contexts. My thesis is an attempt to show the reasonableness, and the desirability, of such a philosophical approach to the question of the social organisation of science.

CHAPTER 1. PHILOSOPHY GONE SOCIAL

1.1 Introduction

As described in the introduction, the aim of the thesis is to explore a number of issues raised by a novel type of argument in philosophy of science: the argument that proposes changes in the way science is organised and practiced on the basis of an account that shows that science is social. This chapter prepares the ground for this exploration and offers answers to two of the questions that I formulated in the Introduction.

First, as I show in the next section, the arguments that form the subject of my thesis are often introduced as a novel and urgently needed development. One may wonder what explains this perception of the urgent need to develop a social account of science. I suggest that an answer can be given by discussing the situation in which this "socially-oriented" philosophy of science finds itself. This situation is characterised by profound changes in the way philosophy sees itself and its past, in the ways science is understood within philosophy and outside of it, and in science itself. These changes support the need for an alternative, and socially sensitive, philosophy of science.

Second, even if it is agreed that the social aspects of science deserve philosophical attention and that philosophical proposals about the social organisation of science are warranted, there may be important disagreements about the aims and justification of such proposals. The disagreement that I find necessary to discuss concerns the question whether it is enough that such a proposal can be shown to have an epistemic motivation and to promote epistemic improvement of science or whether a discussion of the practical consequences of the proposal, including social, political and ethical, is necessary. I discuss James Brown's (2008a) "methodological" proposal about the organisation of biomedical research that I contrast with Kourany's (2010) account of the socially responsible science and I argue that the attempt to preclude a discussion of practical consequences as irrelevant fails. Furthermore, I suggest that a discussion of practical consequences of research can be linked with a more general "practice turn" in philosophy's approach to understanding science and that approaching science as practiced may support the interest in the social aspects of science more generally.

In the next section of the chapter I show how contemporary arguments introduce the interest in the social aspect of science as a novel and urgent development and I give a very brief overview of the history of this interest in philosophy of science. The third section provides an explanation why the theme has gathered this urgency by discussing a number of changes in the intellectual context of philosophy of science and its object, science, to which philosophers find it necessary to react. In the fourth section I discuss what considerations such an argument should take into account and I argue that it should acknowledge the practical consequences of a form of organisation of science. I do so by criticising James Brown's attempt to limit the argument to epistemic considerations. I also attract attention to some issues raised by Kourany's argument that I use as a contrasting case. In the penultimate section I connect the discussion of the practical consequences of the organisation of research with the wider practice-oriented trend in philosophy and I suggest that analysing the social aspects of science and making proposals about its social organisation can be seen as a part of this development. I conclude that there are good reasons to pay philosophical attention to the social side of science and that both epistemic and practical considerations are relevant for a philosophical argument about the social organisation of science. The subsequent argument in the thesis is defined by this conviction.

1.2 Old traditions and urgent new beginnings

One of the clearest formulations of what I think of as a philosophical proposal about the social organisation of science is given by Solomon when introducing her social empiricism:

> Traditional epistemologies, from the time of Plato and Aristotle, and through the contributions of Descartes, Bacon, Newton, Mill and more lately Hempel, Laudan and others, have produced rules and heuristics for individual scientists. Social empiricism, while acknowledging the utility of *some* of these individual guidelines, develops rules and heuristics that are socially applicable. This means that the traditional focus on methods and heuristics to be individually applied by all working scientists is rejected. Instead, the normative emphasis is on science funding, administration and policy. […] My goal is to positively affect scientific decision making through practical social recommendations. (Solomon 2001, 12–13, italics in the original)

Solomon describes her project as belonging to a distinctly philosophical tradition; as focusing on the social level of science; as offering practically applicable recommendations to introduce changes in science funding, administration and policy. This combination of the philosophical orientation, the interest in the social aspects of science and the ambition to offer recommendations about its social organisation is what the developments I discuss in my thesis have in common.

Another, less fundamental but nevertheless intriguing feature of these accounts, is also evident in Solomon's quote: social empiricism is introduced as a novel development, as doing something that "traditional epistemologies" did not do and as rejecting the focus they had.

The accounts of science that I discuss are, indeed, often described by their authors as a recent, novel and urgently needed development, and this perception of novelty and urgency seems to persist. The two authors at the centre of the argument in my thesis, Kitcher and Longino, provide some examples of that. Writing in 1990, Longino explains that her book was conceived in the spirit of "frustration that traditional philosophy of science had so little to say about the relation between social values and scientific inquiry" (Longino 1990b, ix). Writing in 2002, Longino (2002a) still sees the state of the field as unsatisfactory: the aim of her new book is to provide an account of science that recognises both rational and social aspects of science—something that existing philosophical analyses of science have failed to achieve. Introducing his *Advancement of Science*, Kitcher describes his project as resting on a new way to approach science: as "a process in which cognitively limited biological entities combine their efforts in *a social context*" (Kitcher 1995a, 9, italics mine). Introducing his *Science, Truth, and Democracy*, Kitcher characterises it as "the first attempt" to provide "a compelling perspective on the sciences and their place in *democratic society*" (Kitcher 2003, xi, italics mine) and "to venture into areas that philosophers of science have neglected" (Kitcher 2003, xiii).

The claims of novelty are not limited to the introductions of specific works: the way the entire trend of socially oriented philosophy of science is introduced similarly contributes to the impression of a relatively recent phenomenon. For example, in 2014, Biddle calls "the epistemic implications of the social organisation of research" "a topic of growing importance within the philosophy of science" (Biddle 2014, 14) and illustrates this claim by referring to a number of papers published between 1990 and 2012.[1]

Yet, philosophical interest in the social aspects of science has a long history, both when it is understood as the interest in the social aspects of science itself and its place in wider society. The first edition of Thomas Kuhn's *Structure of Scientific Revolutions* (1996) where discussion of scientific communities plays a prominent role appeared already in 1962.

[1] As I subsequently show, the perception of novelty may coexist with the recognition of the long historical tradition behind arguments about the social organisation of science: for example, in his thesis Biddle (2006) discusses the arguments made by James B. Conant in the 1950s and Arthur Kantrowitz in the 1960s and 1970s.

His was by no means the only discussion of the social aspects of science in the middle of the 20th century. Richard Rudner's argument about the necessity of value judgements in science with the title "The scientist q*ua* scientist makes value judgments" was published in 1953; earlier, C. West Churchman (see, e.g., 1948) made a similar point.[2] A discussion of science as a social endeavour or the role of social values of science thus predates Longino (1990b) by three or four decades. Discussion about the place of science in democratic society is at least as old. In the 1960s, debates about the aims of science and the possibility to govern it in a planned manner were active in the United States (see, e.g., Shils (1968) for a collection of papers on this topic, including papers by Michael Polanyi and Stephen Toulmin).[3] In the 1970s, the "finalisation thesis", whose proponents, drawing on Kuhn's ideas, argued for the possibility of systematically applying "mature" theories to social problems, was debated in Germany (see, e.g., Pfetsch (1979) for an overview of the debate and Niiniluoto (1984) for a philosophical discussion).

The interest of philosophy of science in the social aspects of science may be traced further back in the past, to the period between the two World Wars when philosophy of science was beginning to take its recognisably contemporary shape in logical positivism/empiricism. By now, there are numerous studies challenging the received view of logical positivism as profoundly a-social. According to these studies, the members of the Vienna Circle during their European period felt considerable interest in the social and political aspects of science and the social impact of the kind of philosophy of science they were developing (see, e.g., Uebel (2005) on the "left Vienna Circle"). In this, logical empiricism had important similarities with American pragmatism of the same period (see, e.g., A. Richardson (2002) on the commitment to the "social engineering" potential of philosophy that pragmatists such as John Dewey and logical empiricists shared). Also in the interwar period, Western philosophers and historians of science were exposed to the strongly social and political approach to science in the Marxist tradition of history and philosophy of science (see Bukharin et al. (1971) for the an influential collection of works in this tradition and an overview of their effect on British history and philosophy of science since their first presentation in 1931; see Sheehan (2007) for a discussion of this episode and the fates of Marxism in Western studies of science). The question of science's place in society was also prominent at that time: in 1939, J. D. Bernal's *Social Function of Science* (1967) argued that science should be reorganised and managed to make it serve social ends. It is thus possible to argue that the interest in the social aspects of science is as old as the professional philosophy of science itself. Furthermore, some ideas later taken up and developed in the socially oriented philosophy of science may predate this period. This is obvious in the case of Marx-inspired approaches to philosophy;[4] another example is the classical argument about the epistemic benefits of diversity and the importance of social exchange of opinions for the advancement of knowledge that was presented in John Stuart Mill's (1978) *On Liberty,* published in 1859.[5]

Even such an enormously compressed and incomplete overview shows the long history of philosophical interest in the social aspects of science. Moreover, many contemporary philosophers openly acknowledge the connection of their proposals with earlier

[2] Douglas (2009, ch. 3) describes these papers and the debate they provoked in the period when the value-free ideal of science was taking shape but had not yet come to dominate philosophy of science fully.

[3] Kitcher (2003, 212) mentions the importance of this debate for his thinking about science.

[4] In addition to the previously mentioned Marxist approaches, Rein Vihalemm, whom I briefly discuss later, explicitly builds his practical realism on Marx's insights (e.g., Vihalemm 2011 and 2012).

[5] Longino (2002a, 3–7) names Mill one of the predecessors of her social account, alongside Charles Sanders Peirce and Karl Popper; a thread of social arguments about science is thus shown to extend from the mid-19th century through the American pragmatism and the mid-20th century developments.

discussions (see footnotes concerning Biddle, Douglas, Kitcher and Longino). The heightened feeling of urgency that characterises recent accounts cannot therefore be attributed to the fact that the social aspects of science receive philosophical attention for the very first time.

One explanation may be that this attention has so far failed to define the development of philosophy of science (in the case of logical empiricism and pragmatism) or to change the prevailing view of the field (in the case of later arguments). No matter how important the earlier arguments about the social aspects of science or how close to contemporary concerns, the mainstream philosophy of science has mostly followed a different path. As a result, contemporary socially oriented work in philosophy stands in sharp contrast to the a-social vision of the field's subject and aims that at one point came to dominate, and continued to define, most of work in philosophy of science. (There are now discussions how this a-social and value-free turn in philosophy of science happened in the social and political climate of the 1950s America—see, e.g., Howard (2003), which also discusses the social character of earlier logical empiricism and American pragmatism, and Howard (2009), which argues that this social disengagement is yet to be overcome.) The necessity to continue to fight for the recognition against the background of mostly a-social philosophical tradition may explain the persistent feeling that a socially oriented approach in philosophy has to be introduced again and again. So, for example, when Kourany (2010, ch. 2) sets the background for her philosophical project of analysing socially responsible science, she describes the social and political interests of the Vienna Circle and their abandonment in American philosophy of science by the mid-20th century. Kourany then goes on to argue that even after the turn to the actual history and practice of science initiated by Kuhn and others, crucial questions about the social context of science remained unexplored—an omission that according to her has mostly persisted into the 21st century.[6] On this view, philosophy of science still awaits appropriate "socialisation".

The historical situation of contemporary philosophy of science thus provides one explanation why a social approach to science in philosophy is introduced as novel and overdue: if philosophy once showed interest in the social aspects of science, this orientation, since lost, is yet to be fully restored. The aim of the next section is to discuss the factors that support the feeling that such a social reorientation is currently needed—that an alternative approach in philosophy of science is now called for more urgently than ever and that such an alternative has to be sensitive to the social aspects of science.

1.3 Responding to challenges

I have suggested that the manner of introducing social philosophical accounts of science may be explained by the philosophers' view of the situation in which they develop these accounts. In this section, I discuss an aspect of the perception of this situation that plays some role in many of these arguments—the feeling that the traditional philosophical view of science has become unsustainable, as there exists, increasingly prominently, evidence that science is social in some sense, putting into doubt traditional philosophical accounts of science. So, the situation for the philosopher can be seen as characterised by a number of challenges. These challenges uncover something of philosophical relevance about science—and so they cannot be ignored. Simultaneously, they question something of traditional importance for philosophy—and so they cannot be unproblematically accepted either. Accordingly, there is the perceived need to respond to such challenges, to offer an alternative account of them or to work relevant insights into a philosophical account—and to do so quickly.

[6] Douglas (2009, 60–64) argues that in one important sense Kuhn contributed to making philosophy of science less social—stressing the insulated character of scientific community and focusing on its inner dynamics helped to reinforce the value-free ideal for science. On this view, too, a philosophical account discussing the social context and the social values of science is a novel development.

I suggest that these challenges can be divided into two broad categories: first, there have been challenges to the traditional ideas about science; second, there has been the recognition that science itself has changed profoundly, making new approaches and arguments necessary. What follows is a discussion of some examples of these two kinds of challenges, together with the examples of philosophical accounts that explicitly acknowledge their relevance.[7]

Challenges to the traditional view of science might have arrived from several intellectual directions. First, there have emerged approaches within philosophy of science itself that demonstrate a new interest in the previously neglected aspects of science. *The Structure of Scientific Revolutions* (Kuhn 1996), with its interest in the operation of scientific communities and their history is one prominent example among other historically oriented works that appeared around that time, including Paul Feyerabend's (2002) provocative *Against Method* that was first published in 1975 (Rouse (1998, 71–78) gives an overview of these historically oriented approaches). In a less mainstream part of philosophy, feminist epistemology and philosophy of science have been interested in the social aspects of knowledge and science from their very inception. They have analysed various social aspects of science addressing issues of concern for feminists, from the influence of gender biases, and more generally social factors, on research to the consequences of the exclusion and marginalisation of women in scientific community and to the impact of science and science-based technologies on women (see, e.g., S. Richardson (2010) for an overview of the history and contributions of feminist philosophy of science; ch. 3 of Kourany (2010) also discusses several major trends in feminist philosophy of science).

Second, over the recent decades an approach to analysing science has emerged that is often perceived as a direct challenge to philosophy of science (sometimes as a part of the more general "postmodernist" or "Science Wars" threat): social studies of science. From offering explanations of specific episodes in history of science in order to show how scientific knowledge and its development may be accounted for sociologically to observing and analysing the production of knowledge in scientific laboratories, these extremely diverse studies can be seen as a source of evidence that science is social in some sense that is crucial for understanding its character or its products (see, e.g., Shapin (1995) for an overview of sociology of scientific knowledge and related developments; ch. 2 of Longino (2002a) overviews the "Strong Programme" and the laboratory studies).

A number of philosophical accounts are introduced as offering a response to one or several of these challenges. One example of this view of reasons to do a more socially oriented philosophy of science is expressed in Alvin Goldman's (2003, vii–viii) preface to his *Knowledge in the Social World* (mostly dedicated to the general questions of social epistemology but also discussing science). On the one hand, Goldman describes his project as an exploration of the traditional philosophical problem of knowledge, a widening of the traditional, strongly individualistic, epistemology in order to analyse the role of social interactions in the creation of knowledge. On the other hand, Goldman is concerned with what he characterises as the rise of "postmodernism" and "(radical) social constructionism". As the

[7] This discussion is not meant to be complete as I focus on the developments whose impact is openly acknowledged in the philosophical arguments I discuss. My discussion is thus structured somewhat differently from, for example, the more general overview Longino's (2015) article for the *Stanford Encyclopedia of Philosophy* gives. According to Longino, the factors that have helped to bring social aspects of science to philosophical attention include

> the emergence of social movements, like environmentalism and feminism, critical of mainstream science; concerns about the social effects of science-based technologies; epistemological questions made salient by big science; new trends in the history of science, especially the move away from internalist historiography; anti-normative approaches in the sociology of science; turns in philosophy to naturalism and pragmatism.

result of these developments, the notions of objectivity and truth are losing their credibility in various fields from philosophy to law and education, bringing what Goldman sees as worrying consequences. Accordingly, "sustained, philosophical responses" (Goldman 2003, viii) are necessary and the kind of social epistemology Goldman develops is meant to contrast with what he considers the defining features of these postmodernist movements—relativism and anti-objectivism.

Both Kitcher and Longino also describe their respective projects as an attempt to respond to a number of challenges. In their case, however, the suitable response is understood as the creation of a synthesis between various insights about science. So, Kitcher describes his philosophical project as a response to the opposition between the proponents of Legend-view of science (the traditional philosophical image of science) and the critics of Legend (Kitcher 1995a) or "scientific faithful" and "debunkers" (Kitcher 2003). This response is supposed to take seriously and sympathetically, but also critically, both the achievements of the traditional philosophy of science and the insights of those criticising these traditional philosophical accounts, such as historians, sociologists and other researchers working in social studies of science. (In Kitcher (2011c), these competing "*theoretical pictures* of scientific inquiry" (Kitcher 2011c, 16, italics in the original) play a secondary role—the more general dissatisfaction with science and democracy is the defining feature of the situation that Kitcher's account is to address; the account that Kitcher offers, however, still can be seen as integrating traditional philosophical ideas and insights from other fields.) Longino (1990b) introduces and develops her account in relation to two conflicting traditions, logical positivism and wholism (the latter represented by such philosophers as Kuhn and Feyerabend) and also discusses its relations to social and political and especially feminist critiques of science. Longino's (2002a) sets the aim of developing "an account of scientific knowledge that is responsive to the normative uses of the term 'knowledge' *and* to the social conditions in which scientific knowledge is produced" (Longino 2002a, 1, italics in the original). So, it attempts to integrate insights from philosophical analyses on the one hand and social studies of science on the other, and to overcome the traditional opposition between the two approaches.

Yet another possibility to pursue a more socially oriented philosophy of science is to embrace the already strongly social traditions and their challenge to mainstream philosophy. This is what Kourany (2010) does with her *Philosophy of Science After Feminism* where "a comprehensive new programme of research for philosophy of science" (Kourany 2010, vii) is developed on the basis of decades of feminist philosophy of science.

Alternative traditions within philosophy of science or alternative approaches to studying science may thus be seen as a source of insights about science as well as a challenge. Due to them, philosophy is forced to become aware about the aspects of science's practices or the details of episodes in history of science that it previously neglected or supposedly misunderstood. As a result, it now has to work out philosophical arguments that are capable of addressing them.

In addition to these intellectual developments, there might have developed awareness that science itself has changed in ways that require philosophical attention. First, there is the fact that "pure" or basic science is no longer the predominating mode of science. One form of science that is gaining increasing significance is applied science defined by the connection with specific practical concerns. As a result, it may be expected to differ from the traditional pure science in its aims and standards (see, e.g., Adam et al. (2006); Carrier (2004 and 2008b); Wilholt (2006) for a discussion of potential problems for knowledge production that the growth of applied science brings and the mechanisms that may counter them). Another increasingly prominent form of science is scientific advice closely related to policy and governance. It is to discuss the issues of relevance to science employed in policy-making, or advisory science—the science's role whose prominence had been steadily growing throughout the 20th century—that Douglas's (2009) *Science, Policy, and the Value-Free Ideal* is written.

Second, there is the recognition of the important changes in the way science, no matter what its form, is organised and practiced. It can no longer be taken for granted that science is publicly funded and produces knowledge that is a public good: science is increasingly commercialised, privatised and turned into a source of intellectual property.[8] Biomedical research where these developments have been especially extensive has become the subject of possibly the most intense attention of socially minded philosophers of science over the last decade. So, Matthias Adam (2008), Biddle (2006; 2007 and 2013a), James Brown (2000; 2002; 2008a and 2008b), Kristen Intemann and Inmaculada de Melo-Martín (2014), Gürol Irzik (2010), Saana Jukola (2016) Manuela Fernández Pinto (2014 and 2015), Julian Reiss (2010; some of these themes are also discussed in Reiss and Kitcher 2009) and Torsten Wilholt (2009) have written on the epistemic consequences of commercialised biomedical science for the selection of research directions, the choice of methods, and the quality and trustworthiness of published results, as well as other epistemic problems in connection with privately funded research. There are also more specific concerns discussed in connection with these developments. For example, Biddle (2012; 2014) has analysed the impact of the current regime of intellectual property on research and its epistemic quality. Hans Radder (2013) has also discussed the impact of patenting, focusing on its influence on the understanding of several traditional themes in philosophy of science. Kevin Elliott (2008) has explored effectiveness of various approaches for addressing conflicts of interest. Rebecca Kukla (2012) has written about the epistemic impact of "radical collaboration" in biomedical research and its consequences for the notion of authorship, connecting the analysis of biomedical research with the theme of interdependence of knowers and knowledge producers and the role of trust (see Hardwig (1985) for a classical introduction of this issue); the discussion of radical collaboration has been continued in Winsberg et al. (2014).

Finally, there may be the general recognition that modern science has an immense practical impact on individuals and society and is in this sense different from science as once analysed by philosophers. So, discussing the "social turn" in contemporary philosophy, Longino (2006) points out that in some important sense it is a "return" to the kind of philosophy done in the early Vienna Circle. Simultaneously, she stresses that the situation of philosophy vis-à-vis science is now very different: our world is "saturated" with science and science-based technologies (Longino 2006, 168). This, in turn, has important implications for attempting to understand science philosophically, even if the primary aim of such a philosophical account is understanding science's epistemic side—as Longino writes,

> The modern sciences, however, are not merely knowledge producers; they are also commodity producers, weapons producers, instruments of governance, ideology transmitters, engines of social and economic transformation. They are not just elements of a productive system, but also of a persuasive and communicative system. […] One detaches the knowledge productive function from these other roles at risk of distorting one's understanding of them all. (Longino 2006, 176)[9]

Different philosophical arguments about the social nature of science may see the source of inspiration (or challenge) in different empirical or theoretical claims about science and focus on different aspects of science that can be characterised as social. In any case, both changes in the way the aspects of science that philosophy traditionally analyses are understood and the

[8] The issues related to commercialisation of research and the growth of applied research may be intertwined: for example, Adam et al. (2006), Carrier (2004; 2008b) and Wilholt (2006) discuss them together.

[9] I continue the discussion of the consequential character of contemporary science in the following two sections.

changes in science itself and the emergence of new relevant issues call for a philosophical response. Together with philosophers whose arguments I discuss, I am convinced that giving such a response, sensitive to the work already done on the social aspects of science, is an important task for philosophy of science. There is too much of what is already known about the significance of the social aspects of science, in philosophy or elsewhere, to ignore it when analysing science philosophically. The arguments I discuss in my thesis, however do not limit themselves to accounting for these aspects of science philosophically: they also make proposals about desirable changes in the social organisation of science. The aim of the following section is to discuss how these proposals can be justified and developed. After showing several possible approaches leading to such proposals, I discuss the role of epistemic and practical considerations in making and defending a philosophical proposal about the social organisation of science.

1.4 Making meliorative proposals and justifying them

As described in the preceding text, my thesis is concerned with the arguments that not only see the social organisation of science as a matter of philosophical relevance but also make proposals about the way science should be socially organised. In this section I discuss how the move from analysing science to proposing a reform may be justified. I suggest that both approaches that offer a new analysis of science and approaches that analyse novel forms of science can support such a move quite seamlessly when philosophy is understood as a source of normative epistemic advice.

Some philosophical arguments, such as the one offered by Kourany (2003a; 2010), however, leave epistemic domain and make explicitly politically motivated recommendations about the organisation of science. In the main part of this section I discuss the roles of epistemic and practical considerations in such a proposal. More specifically, I ask whether arguments that are presented as purely epistemic and that explicitly reject the relevance of political considerations can be sustained. As an example, I analyse the proposals developed by James Brown (2000; 2002; 2008a and 2008b). I conclude that Brown's attempt to insulate his argument from practical considerations fails. I thus suggest that Kourany's argument is an extreme expression of the position that is considerably stronger than the alternative to it.

The quote from Solomon that opens the discussion in this chapter shows that making recommendations about specific aspects of the social organisation of science can be seen as an unproblematic part of a philosophical account that analyses the social level of science. By focusing on the previously neglected aspects of science social empiricism enables an improved philosophical understanding of science. (Introducing her account, Solomon (2001, 2) writes that her aim is to advance the debate past "piecemeal new insights and remnants of past unworkable positions" of philosophers of science and sociologists of scientific knowledge.) This, in turn, makes possible to offer recommendations that previously were inconceivable. Traditional epistemic advice was meant to help individuals' epistemic improvement; now it becomes possible to show how the social organisation in science may be improved.

Two assumptions may be distinguished behind such a position. First, philosophy is seen as a normative enterprise committed to good epistemic practices. Providing particular recommendations for scientists and science policy makers is an expression of this commitment. Solomon's proposals are presented as both contrasting with traditional epistemic recommendations and belonging, in principle, to the same philosophical tradition of producing "rules and heuristics". Second, these recommendations are supposed to reflect the best philosophical understanding of science available. Accordingly, novel recommendations are called for if the understanding of science has considerably changed, shifting the focus to different aspects of science (in this case social) and re-evaluating the relative importance of the previously known aspects.

This is the argument that can be made in the case of a novel philosophical account of science; a similar argument can be made in the case of a philosophical account of novel developments within science itself. Also in this case, as long as the philosopher has the duty to provide sound epistemic advice based on the best available understanding of science, discovering the epistemic consequences of a particular form of organisation should prompt new philosophical recommendations. If anything, these novel developments may be especially inviting for philosophical intervention. An ongoing development can be readily compared with previously existing forms of the organisation of science, showing that it is neither immutable nor the only possible approach to organising research.

These considerations can be seen in the argument Biddle (2014) makes. Biddle's aim is to analyse specific forms of social organisation of science from an epistemic point of view:

> Some ways of organising research are conducive to the production and dissemination of knowledge, and others are not; the examination of which is which is an important project in social epistemology, especially given the recent changes in how research is structured. (Biddle 2014, 14)

In his argument, Biddle focuses on the way the patenting and licensing system is organised. Its influence on science has been growing with the number of patents taken by researchers and, as Biddle argues, the current situation has a number of serious epistemic drawbacks. The identification of epistemically problematic ways of organisation, in turn, calls for formulating and putting into practice meliorative proposals—proposals "for improving the situation, so as to better achieve our epistemic aims", which may require "a significant reorganisation of research" (Biddle 2014, 15). Again, the commitment to the epistemic quality of science and to a more adequate understanding of science (especially as it is changing) leads unproblematically to the formulation of specific reorganisation proposals.

Some proposals of socially oriented philosophers of science can thus be seen as firmly within the tradition of normative epistemic advice. However, there are also others, such as Kourany's (2003a; 2010) plea for a socially responsible philosophy of science, that go beyond the epistemic considerations; for them, ethical and political considerations are guiding.

As noted before, Kourany builds on the tradition of feminist criticism of science—criticism showing that science has the potential to advance equality but historically has strongly contributed to the perpetuation of prejudice or neglect of women's interests. Accordingly, Kourany suggests that what should be required from science, and by extension from philosophy of science, is "social responsibility". Socially responsible philosophy of science recognises the profound social consequences of science and assesses it in light of the "egalitarian ideal of human flourishing" (Kourany 2003a, 6). Kourany argues that this ideal should serve as the basis for decision-making on different levels of scientific practice: particular epistemic values to be pursued, particular theories to be preferred and particular research projects to be prioritised. Evaluation from the point of view of the ideal is not meant to replace epistemic evaluation—the theories preferred must still be empirically adequate. Yet the choices made on all these levels should be guided by the principle of maximising the probability of contributing to the achievement of the egalitarian ideal. Thus, in Kourany's argument the proposed changes in current practices of research planning and evaluation are explicitly connected with non-epistemic considerations that reflect particular social values. Philosophers are expected to act in a variety of advisory and other politically relevant roles to help to bring these changes to life.[10]

[10] I return to Kourany's model of doing philosophy of science in the concluding chapter.

Such an ambition may be seen as taking philosophy too far—for example, Ronald Giere (2003) claims that in the case of Kourany's argument, the political project of getting philosophy of science politically engaged is so removed from the contemporary professional philosophy of science that its philosophical criticism becomes pointless. (Kourany's naturalistic project of analysing science as a practical human activity in social context and the critical project of appraising various research programmes are, according to Giere, unproblematic.)

Given this criticism, a proposal that denies the relevance of social and moral values and stresses its purely epistemic rationale may seem to be safer philosophically. James Brown's (2008a; he also discusses this topic in J. Brown 2000; 2002 and 2008b) argument concerning the organisation of biomedical research can be seen as an example of such a secure approach.

James Brown begins by summarising a number of problems he attributes to the increasing commercialisation of biomedical research, such as the incomplete publication of data, the influence of the interests of the funding company on published conclusions, the maintenance of deliberate ignorance about inconvenient topics, and the preference for research directions that promise patentable and profitable results. Besides their obvious social cost, these developments undermine trust in published results and diminish the number of alternative theories that could be used for criticism and improvement of mainstream theories. The current funding and patenting regime in biomedical research thus harms science as an epistemic enterprise.

According to James Brown, this is a fact about research that philosophers and scientists have learnt similarly to how they once learnt about the placebo effect. In the case of the placebo effect, blind tests were introduced to counteract its influence on reliability of research results. James Brown argues that in order to counteract the effects of commercialisation, countermeasures are also necessary. Specifically, he recommends the introduction of what he calls socialised research, where patents in medical research are abolished and public funding is increased to drive out for-profit companies. According to James Brown, no considerations need be involved in the justification of this proposal besides epistemic ones: just as in the case of the placebo effect, the discovery of a fact about research leads to the establishment of a new epistemic standard.

James Brown does acknowledge that such a proposal may take one into the sphere of politics, because its realisation may require political action. As he stresses, in the situation where the problem is caused by a particular social arrangement, a solution that simply requires a better application of the existing epistemic standards may be insufficient. Rather, an alternative social arrangement may be required and so, as James Brown writes, what is ultimately recommended is "a social reorganisation of scientific research, achieved through political action" (J. Brown 2008a, 190). Besides this political connection, however, James Brown denies the relevance of non-epistemic considerations for discussing his proposal. In particular, he stresses that while the proposal in question—socialised organisation of research—may appeal to one's moral sense, moral or social considerations need not be involved in making or accepting it:

> Facts have been uncovered that require a methodological response, not a moral one. The right response, I urge, is to socialise medical research. The fact that scientific socialism, as I am here calling it, harmonises well with one's moral sense, at least for me, is a happy accident. (J. Brown 2008a, 213)

The continuity of the proposal with traditional epistemic concerns of philosophy of science is thus stressed while the philosophical relevance of its relation to one's preferred social values is denied. So, such an argument may be seen as safe from Giere's criticism: what James

Brown advocates is a naturalistic and critical project, even if it does ultimately require political action for its implementation. My aim in the subsequent paragraphs is to argue that James Brown's attempt to make his proposal immune to an appraisal in terms of social values fails.

With his argument, James Brown claims that once the epistemic rationale for a form of the social organisation of science is given, it is immune to criticism in terms of values that its implementation is likely to promote: the rejection or the endorsement of these values has no impact on the epistemic proposal. In my argument I discuss three broad strategies one can use to argue for this position.[11]

The first possibility to argue that an epistemically motivated proposal for reorganisation of science does not need to be discussed in terms of social values that this reorganisation may help to advance is to invoke the difference between pure research and its application. One could argue that the proposal for the form of organisation of science preferable on epistemic grounds is applicable to research, which is by its nature divorced from practical consequences and thus exempt from evaluation in terms of social values. Rather, it is the application of research that is consequential and could be appraised from the perspective of its practical impact or the values it may help to promote. The ever-growing role of applied and privately funded science that I have previously discussed, however, furnishes an objection to this argument: what is called science is increasingly done for practical application. Moreover, biomedical research that James Brown discusses is to a very considerable degree meant to be practically applicable.

Some aspects of James Brown's argumentation do in fact show the recognition of the practical consequences of research his proposal is meant to regulate. When James Brown writes about the harmony between his epistemic proposal and his moral sense, he recognises that the implementation of his epistemic proposal may be expected to bring certain practical consequences he values. Among them are, for example, fairer and more egalitarian access to the achievements of medical science and individuals' access to all treatments that could benefit them, including currently underexplored non-patentable behavioural changes and underpromoted older medicines. So, this is not the case where the form of social organisation of science is seen as lacking practical consequences beyond the creation of objective knowledge. The research promoted is expected to result in practical results, be it the introduction of medicines for currently ignored diseases or the encouragement of certain behaviours among patients.

The second possibility to exclude a proposal for a form of organisation of science from the discussion of its consequences is to argue that the advancement of epistemic aims of science overrides any other values. In this case, even if certain consequences for social values are associated with the proposal, they will not matter as long as the proposed form of organisation does indeed help the advancement of scientific knowledge. The wide recognition of the ethical limitations on research, however, makes such a position problematic. While particular cases may be debatable, the recognition that certain kinds of research, particularly involving humans, are impermissible on moral grounds seems to dominate the current understanding of science—indeed, Carrier (2008a, 2) calls it uncontested in his discussion of the role of values in science. Routinely, the value of the advancement of knowledge is

[11] Kitcher (2003, ch. 12) discusses another strategy that can be interpreted as disposing of the question of value-consequences of research wholesale. He summarises it as the idea that "Truth is good for us": the improvement of scientific knowledge does have an impact on social values but this impact is ultimately beneficial as it leads to a better value-system. I do not discuss this strategy because I believe it to be in an important sense different: it offers a value-based reason to support epistemic progress; what Brown's argument denies is precisely the relevance of such reasons. This strategy is in any case not without its problems: Kitcher discusses them at length.

weighed against the values such as persons' wellbeing and autonomy, and the latter may sometimes trump the value of epistemic progress.

Similarly to the previous case, James Brown does not attempt to save this strategy from its problems; some aspects of his argument in fact seem to undermine it. In particular, in his discussion of the value of biomedical research James Brown describes it as deriving from both the valuable aim of advancing knowledge and the valuable aim of reducing human suffering, and the latter comes first:

> There are a handful of human activities that are completely ennobling. The list is no doubt headed by anything that alleviates poverty and suffering. It also includes the production of great art and great science. Medical research should be near the top of this list. (J. Brown 2008a, 211)

Thus, the value of knowledge is not the only value to be taken into account when discussing the organisation of medical research.

The final possibility, which does acknowledge that the proposed form of organisation has practical consequences that can be evaluated from the point of view of social values, is to argue that the epistemically justified form of organisation does not benefit any system of values in particular. Therefore, one cannot criticise it for advancing someone's preferred values specifically. This is the element of the idea of value-freedom of science that Hugh Lacey (1999) calls the thesis of neutrality. According to Lacey (1999, 75), the thesis of neutrality includes three claims: first, accepted scientific theories are "consistent with all value judgements"; second, they have "no value consequences" and third, they demonstrate "evenhandedness in application". In other words, they do not dictate particular value commitments, neither support nor undermine existing value commitments and can be used with equal success in service of any system of values.

If the theses of neutrality describe the character of science adequately, one can maintain that the preference for a particular form of the social organisation of science for epistemic reasons cannot be criticised on the grounds of particular social values it may serve—it may serve any other values equally well. As Lacey (1999, 74) remarks, neutrality forms an important part of the self-understanding and public image of modern science. Nevertheless, throughout his 1999 book Lacey discusses numerous difficulties connected with these theses. In particular, some systems of values may benefit from scientific knowledge more than others—for example, if scientific knowledge is important for the central activities in one system but remains to be of marginal importance for another. Neutrality also sits uneasily with another common idea about science—the idea that science serves progress; the idea of progress, however, is not compatible with all values. Most importantly, Lacey argues that contemporary research is conducted in a way that makes it particularly suitable for the advancement of modern values of control. The strategies predominating in scientific research and these values of control are in a mutually supporting relation. Lacey concludes that science as it is practiced now is not neutral, as it is best suited for advancing the value complexes where values of control play an important role.

Similarly to the previous two, the third strategy faces serious problems that James Brown's argument does not even start to address. Also similarly, some aspects of his argumentation seem to undermine the very possibility to argue for it. The acknowledgement that one may object to James Brown's proposal by pointing out particular values it may serve shows the recognition that this proposal can be reasonably expected to benefit some social values more than others. James Brown's characterisation of his preferred form of organisation of science as socialised research in contrast with the free market and the regulated market also shows the recognition that the proposal is not meant to cohere with values associated with preference for free markets or the minimal state intervention.

James Brown's position requires the possibility to insulate a proposal made on epistemic grounds from the evaluation of its consequences in terms of social and moral values. I have discussed several strategies to defend this possibility and I have shown that James Brown does not provide argumentation necessary to support these strategies; besides that, his argument contains elements that contradict each of them. Accordingly, I conclude that James Brown's attempt to present his argument as immune to discussion in terms of social values fails. It is important to stress that I do not claim that there are no epistemic reasons behind James Brown's proposal; rather, my claim is that the discussion of his proposal cannot be limited to discussing them. The epistemic benefits that the proposal attempts to bring should be discussed in the context of other values this proposal is likely to advance or to thwart. It may be possible that this discussion of relevant social, ethical and epistemic considerations would lead to the ultimate acceptance of James Brown's argument; however, I conclude that he has no justification for ruling such a discussion out as unnecessary.

Where does it leave Kourany's argument? Initially there seemed to be a stark contrast between Kourany's approach that focuses on the social consequences of the proposed changes and James Brown's "methodological, not moral" response that focuses on the epistemic improvement of the current situation of science. I have argued that the proposal that James Brown makes is not in fact devoid of social impact. From the realisation of James Brown's proposal, certain practical consequences may be expected; these consequences are likely to have a different impact on social groups with different interests and values. What James Brown offers is in the end not so different from what Kourany offers. In both cases, the changes proposed may be expected to lead to a system with a specific distribution of epistemic and practical benefits and burdens. Given the impossibility to sustain the separation between an epistemic proposal and its practical consequences, Kourany's socially responsible philosophy of science may be praised for discussing an aspect of the more social philosophy of science that other philosophers may neglect.

Kourany's proposal, however, is not limited to that—her argument reverses the logical order, as instead of discussing the practical consequences following from an epistemically preferable proposal she starts with the preferable practical outcomes. I suggest that the prominent role of practical considerations in Kourany's proposal makes especially visible the problem of weighing consequences of various kinds that such a proposal may be expected to bring. Kourany's socially responsible science requires considerable changes in the priorities assigned to various research programmes and in the choice of methods and hypotheses.[12] The hypotheses preferred under the new regime may be both empirically adequate and reflecting desirable values, as Kourany demands. Yet, their acceptance may simultaneously mean the abandonment of alternative hypotheses that express other (epistemic) values traditionally held in high regard (for example, giving up universality for particularity, one of the usual examples on the list of feminist values). A different way to prioritise approaches may mean the abandonment of potentially fruitful or previously successful lines of inquiry. So, alongside the promise to produce knowledge about the issues that are currently neglected, there is the possibility that some potentially achievable and currently valued research possibilities would not be realised.[13] The changes to be made are motivated by ethical and political

[12] Kourany stresses that the ideal of socially responsible science does not mean the prohibition of research projects deemed incompatible with the ideal. Such projects may be assigned lower priority "in view of our pressing social needs" (Kourany 2003a, 8) but they would not be prohibited. The considerations I discuss nevertheless apply—assigning to some line of research lower priority may diminish our chances of acquiring respective knowledge.

[13] Kourany stresses that socially responsible science should conform to both adequate epistemic and adequate social standards—"only when both kinds of requirements are fulfilled should talk of scientific advance or

considerations, so in effect epistemic losses of one kind are expected to be offset not only by epistemic gains of a different kind but also sometimes by important social gains. The question how to balance various kinds of consequences thus becomes pressing.

While I believe that the issue of weighing consequences of various kinds constitutes an important and currently underexplored problem for Kourany's approach, I want to somewhat mitigate this objection against Kourany's proposal. The problem of balancing is not solely applicable to practically motivated proposals such as Kourany's. As Kourany herself points out, giving up certain epistemic possibilities for the sake of non-epistemic values already constitutes a feature of contemporary science. For example, in biomedical sciences we give up the possibility to acquire certain kinds of knowledge out of respect for the rights of research subjects (Kourany 2010, 74). Moreover, even if one focuses on epistemic considerations only, one has to acknowledge that science does not currently pursue "all truth" (Kourany 2003a, 8): in the situation of limited resources and time, choices have to be made and priorities assigned. Kourany points out that currently these choices are primarily dictated by the interests of those funding science, above all the military and various industries (Kourany 2003a, 8–9; Kourany (2010, 120–125) discusses the current choices in biomedical research and their epistemic consequences). So, any proposal for change has to be compared with this state of affairs, rather than the situation of unconstrained pursuit of everything epistemically desirable. The problem of potential epistemic losses is not exclusive to Kourany's proposal. (The losses associated with contemporary science—the failure to produce many kinds of knowledge that are necessary for enabling "egalitarian human flourishing"—have been a prominent theme in feminist criticisms of science, on which, in turn, Kourany's proposal is built.)

So, I propose to use the preceding discussion in order to draw a more general lesson for various philosophical proposals. Every form of organisation, including the one Kourany recommends but also, importantly, both the status quo and the epistemically motivated proposals such as James Brown's, can be characterised through the possibilities it opens for the realisation of particular epistemic and social values. For example, the proposal James Brown makes can be expected to trade some possibilities for gaining pharmaceutical knowledge for the possibilities to increase knowledge about behavioural interventions. Any change in the organisation of science may be expected to shift the balance of what we can, or cannot, expect to achieve epistemically and practically. So, I suggest that also from this point of view, proposals about the social organisation of science prioritising epistemic considerations have much in common with Kourany's proposal: if realised, they would result in a certain trade-off between consequences of various kinds.

Most generally, I suggest that the recognition of a certain trade-off between the possibilities opened or closed by a particular form of organisation of science calls for an explicit discussion of the likely balance of consequences that the respective philosophical proposal may be expected to bring. So, a discussion of the epistemic consequences to which her proposal may be expected to lead compared to the present epistemic state of affairs is as warranted in the case of Kourany's proposal as the discussion of specific practical consequences was warranted in the case of James Brown's proposal. In order to reinforce this point, in the next section I continue the discussion of the practical consequences of research. As the discussion will show, this question is also relevant for the justification of the philosophical interest in the social organisation of science, tying the themes of this chapter together.

---

scientific progress be considered appropriate" (Kourany 2010, 85), so the losses I discuss are better seen in terms of research left de-prioritised rather than the lower epistemic quality of novel research.

1.5 Seeing science as practice

The medical research James Brown discusses or the research fields that are most relevant for "human flourishing" in Kourany's argument may be seen as a special case where the separation of the epistemic and the social cannot be sustained. After all, both the development of new medicines and the research on violence against women (the example in Kourany 2010) are expected to result in practical effects as well as new knowledge. In this section I discuss some philosophical arguments that support the rejection of such a separation more generally and thus provide further support for the need to explore both the epistemic and the practical consequences of a form of the social organisation of science.

The philosophical arguments in question are united by their attention to the role of practice in sciences. The "practice turn" covers a wide range of work done in different fields over several decades (see Soler et al.'s (2014) overview of its history and main characteristics in the introduction to a collection of papers on the practice turn). In what follows, I discuss some practice-oriented arguments that are most relevant for my claim of the impossibility to delineate parts of science that can be free from the assessment in terms of practical consequences. As the discussion shows, the same arguments simultaneously support the interest in the social aspects of science.

At the centre of the practice-oriented analyses of science is, in Hasok Chang's words, "what it is that we actually do in scientific work" (Chang 2011, 208). Taking this question seriously means, in turn, a different perspective on traditional philosophical issues:

> instead of thinking about the nature of a definition, we can consider what one has to do in defining a scientific term: formulate formal conditions, construct physical instruments and procedures for measurement, round people up on a committee to monitor the agreed uses of the concept, and devise methods to punish people who do not adhere to the agreed uses. In one stroke, we have brought into consideration all kinds of things, ranging from operationalism to the sociology of scientific institutions. (Chang 2011, 208)

It is the idea about the central role of actually doing things—constructing instruments and performing procedures—in scientific activities that I use in order to support my argument about the impossibility to distinguish pure knowledge and its application.

The theme of the practical, material, consequential nature of research is expressed powerfully in the arguments developed by Joseph Rouse.[14] (My summary of Rouse's arguments on this topic does not attempt to do full justice to the themes of knowledge and power or philosophical naturalism that he develops—Rouse (1999 and 2014) overview some of the central themes in Rouse's political and naturalistic philosophical project as well as allowing one to see their development over time). Rouse (1990) argues that the developments brought about by the philosophers that he calls new empiricists, such as Ian Hacking and Nancy Cartwright (but also Kuhn under the reading Rouse proposes), and the sociologists of scientific knowledge focusing on laboratory studies have made possible a crucial shift in understanding of science. Most generally, after this shift observation and representation are no longer seen as the central activities of science; instead, experimental manipulation takes centre stage. This shift of focus from representation to manipulation in understanding knowledge is, obviously, related to the increasing recognition of the importance of experimental activities (the beginning of the practice turn is often connected with the emergence of New Experimentalism—see, e.g., Soler et al. 2014, 7–11). Rouse, however, also stresses the importance of the emergence of a related approach to the theoretical side of

[14] Rouse's 1990 and 1996 accounts are another interesting example of a philosophy drawing on a variety of traditions that are often seen as challenging philosophy of science as usual: in addition to the already mentioned Kuhn's philosophy, social studies of science and feminist critiques Rouse also invokes Continental philosophers such as Michel Foucault and Martin Heidegger.

science that demonstrates the necessity of concrete models for making sense of theories. This attracts attention to the processes of creating models and constructing laboratory phenomena, which are often intertwined (phenomena are meant to realise specific models; models are meant to guide the construction and control of phenomena) but may also have dynamics of their own.[15] This recognition of the central role of "doing" in science—of constructing, manipulating and controlling—undermines the separation between experimental and theoretical activity.

Rouse argues that this recognition similarly undermines the usual distinction between pure knowledge and its application: the new view of science

> makes untenable the traditional account, that technical control is a result of theoretical knowledge. Technical control, the *power* to intervene in and manipulate natural events, is not the application of antecedent knowledge but the form scientific knowledge now predominantly takes. (Rouse 1990, 20, italics in the original)

As Rouse argues (1990, especially ch. 4 and 7; ch. 5 and 7 of Rouse (1996) further elaborate Rouse's understanding of scientific practices and power), science is an activity that has consequences in the world. Sciences construct effectively controllable and manipulatable "microworlds" in laboratory (in laboratory, phenomena of interests can be prepared, isolated from undesirable influences and closely monitored as they are being manipulated). Thus, they make possible the introduction of new entities and substances, novel ways to manipulate objects, novel equipment, processes and skills, novel conceptual models and examples for problem solving into the world. Equally importantly, they simultaneously prompt important changes in individuals' behaviours and self-understanding, as it takes a particular kind of mental, bodily and social discipline to practice science within laboratory and outside. Ultimately, these practices result in changes in the world, as the transfer of knowledge achieved in laboratory into the world requires the latter to be more like a laboratory, making possible some degree of manipulation, construction and control that are realised so fully in laboratories. Scientific practice thus provides novel possibilities for action and self-conception, and changes the world where the action takes place.[16] As Rouse argues,

> The construction of simplified and controlled "microworlds" within laboratories provides models and strategies for reconstructing the world around us. This reconstruction changes the political possibilities open to us and creates new issues we have to respond to. Understood in these terms, the growth of the natural sciences presents us with some of the most important political issues we face today, issues that cannot be separated from the epistemological concerns that have traditionally been the focus of the philosophy of science. (Rouse 1990, xiv)

On the practise-based view that Rouse represents, the creation of controllable and manipulatable experimental arrangements is the central activity of science and this activity is above all practical. The notion of scientific knowledge that can be separated from its realisation in particular experimental objects and arrangements is inadequate. Scientific research is never pure in the sense of being fully separable from practical consequences in the world. Accordingly, the separation between discussing the epistemic consequences of a form of organisation of research and its practical consequences that James Brown attempts is unsustainable in principle, even for research areas that are traditionally seen as pure.

Also importantly, the general turn to approaching science as a practice can be seen as another reason for the interest in the social aspects of science. As Chang's quote shows, the importance of social aspects of science becomes obvious once the attention shifts to thinking

[15] Soler et al. (2014, 9) point out that after initial neglect in New Experimentalism, the practical character of theory has been recognised in practice-based philosophy of science more generally.

[16] The theme of the fundamental effects of knowledge production on what is possible or impossible in the world is developed powerfully in Karen Barad's (2007) agential realism.

about science as something being actually done (Soler et al. (2014, 19–20) also describe the interest in the social and political aspects of science as one of the important shifts brought by the practice turn). In addition to the more obvious "sociology of scientific institutions" that Chang mentions, the attention to experimentation and theory-making as practices also invites the exploration of the less immediately visible social dimensions of science. For example, once it is recognised that particular models play a crucial role in the interpretation of theories, analysing the way those models are made, including the role of culture- and value-laden assumptions that may be involved in them becomes significant for understanding these theories. Similarly, if particular experimental and technical practices play a crucial role in the development of knowledge, analysing the social relations of researchers taking part in them and attending to questions such as particular forms of distribution of labour, assignments of authority and credibility or the social makeup and structure of communities of practitioners becomes significant for understanding scientific research. Or, if scientific research depends crucially on technical opportunities and laboratory spaces, understanding the legal, financial and other arrangements that enable them also becomes an epistemically significant question.

In Vihalemm's words, thinking about science as practice means thinking about it as "a social-historical, critically purposeful-normative, constructive, material interference with nature and society" (Vihalemm 2012, 10). Thinking of science in terms of practices thus allows one to tie together several themes I have been discussing so far: the importance of the social aspects of science and its wider social context, the consequential character of scientific research, and the responsibility for the practical and epistemic consequences of a specific form of organisation. I suggest that the socially oriented arguments that explore these themes can be seen in the context of this wider practice-oriented development in philosophy of science. By placing them in this context, it is possible to establish connections between "social" philosophical accounts of science and work in other areas of philosophy of science after the practice turn, such as the philosophical analysis of practices of experimentation or modelling or naturalistic philosophical exploration of humans as epistemic agents. Social philosophy of science would not then be seen as a maverick development but—importantly—neither would there be reasons to see it as an especially marginal part of philosophy of science.

1.6 Conclusion

The aim of this chapter was to give some substance to the notion of philosophical argument about the social organisation of science and to discuss some issues raised by such arguments. I began by giving some examples of these arguments and showing how they are often introduced as a novel and urgently needed development; this perception may nevertheless be tempered by the recognition of the long tradition of discussing social aspects of science in philosophy. I described how this persistent urgency can be understood as a response to challenges to the traditional understanding of science in philosophy of science. These challenges are brought about by both alternative accounts of science (such as those developed in history of science, social studies of science and feminist critiques of science) and changes in the organisation and practices of science (such as the big science and commercialisation). As long as philosophy is understood as the source of normative advice for science, both the changes in the understanding of science and changes in the conditions for science may prompt novel philosophical advice—advice concerning the social organisation of science.

In the second part of the chapter I juxtaposed James Brown's and Kourany's proposals about the organisation of science in order to discuss what considerations such a proposal should take into account. Discussing James Brown's attempt to present a purely methodological argument about the social organisation of a practically consequential field and criticising three possible strategies for maintaining its immunity, I argued that even the philosophical proposal that has the epistemic improvement of science as the primary aim cannot be absolved from a discussion of the practical consequences its implementation is likely to bring. Simultaneously I pointed out that Kourany's proposal, with its focus on the social consequences, raises important questions about weighing various epistemic and practical consequences of a proposal.

Finally, I suggested that practice-based approaches in philosophy of science support the inseparability of science as an epistemic practice and a practically consequential enterprise more generally, further reinforcing the argument I made against James Brown. These practice-based approaches also support interest in the social aspects of scientific practice. I thus returned to the discussion of the reasons to develop a more social philosophy of science and I suggested that these arguments may be seen as a part of the wider "practice turn" in addition to being a response to external challenges.

So, in the chapter, I argued that there are good reasons to pay philosophical attention to the social aspects of science and to take into account both epistemic and practical consequences of a particular form of organisation of science when doing so. These considerations hold both when the primary aim is improving science epistemically and when it is making science more socially relevant. Also importantly, in both cases, the realisation of the philosophical proposal may be expected (as James Brown and Kourany point out) to require political action, thus bringing philosophy in touch with politics. This raises an important question about the possibility to motivate and put into practice such a political action. The next chapter adds several other considerations for an approach to the social organisation of science to take into account. It begins with an overview of different possibilities to understand what "science is social" may mean philosophically.

CHAPTER 2. THE SOCIAL CHARACTER OF SCIENCE: SOCIAL VALUES AND SOCIAL INTERACTIONS IN SCIENCE

2.1 Introduction

The previous chapter introduced some examples of the arguments that my thesis aims to explore—arguments made by philosophers of science who see science as social and make proposals about the social organisation of science on this basis. The chapter discussed the reasons to pursue this theme in philosophy of science and outlined some considerations that such a proposal should take into account. The present chapter similarly has the dual aim of introducing the arguments that form the subject of my thesis in greater detail and discussing some desiderata that, I suggest, a proposal about the social organisation of science should satisfy. Ultimately, both chapters serve to prepare the ground for the discussion of Kitcher's account and for the defence of the alternative I propose to it in the thesis.

It may in principle be possible to argue that science should be reorganised in a particular way in order to serve certain social aims without attending to the social aspects of science itself. Science is seen in this case as a black box that currently functions as a part of a certain social and political arrangement but can be made a part of a different one.[17] The accounts that my thesis discusses are the opposite of such a black-boxing approach. Instead, they start from demonstrating that science itself is in some crucial respects social; proposals for the social organisation of science build on the aspects of science identified as social. Such an approach has the important benefit of making it possible to argue that the proposal in question reflects an adequate understanding of science (showing, for example, that certain features of science can only be realised, or realised more fully, under a specific form of its organisation), rather than introducing social considerations totally foreign to it.

This close connection between a specific account of science and a specific form of organisation that can be offered on its basis means that different ways for a philosophical account of science to be social and the focus on different aspects of the sociality of science may open science for different proposals about its organisation. In the chapter I use what is taken to be the standard a-social position in philosophy of science as the starting point for discussing how the idea "science is social" is developed in socially oriented arguments. I do not attempt to provide a complete overview of the arguments to the effect that science is social; rather, my aim is to map out some possibilities for arguing against the a-social understanding of science and offering a social alternative to it, and to show how different proposals about the social organisation of science can be supported by different philosophical accounts.

In the chapter, I classify social accounts of science as reflecting two general approaches to the question of the social in science. Within one of them, the discussion is centred on the role of social values in science; in the other, science is understood as social in the sense that it is not individualistic—the focus is on the social interactions and social

---

[17] There is an early discussion of such a black-boxing approach to the organisation of science that is very critical of it but also explains how it may arise quite naturally in thinking about science policy:

> As an institution, science is a subsystem of a more inclusive nation-system. Because of our effort to focus on science it is easy to fall into the trap of thinking of it as a relatively detached subsystem, rather than as one which is closely coupled to the other subsystems that make up a nation. In addition to the danger of looking at science as a closed rather than an open system, there is another danger which arises from viewing it as a black-box whose internal operations are of no interest. From this latter point of view all one needs to consider is the relationship between inputs and outputs. (Ackoff 1968, 84)

Ackoff claims that this "'input-output' orientation" tended to dominate science planning of the era. I will not go into the question whether this orientation has changed: what is important for me is that such an approach is in principle possible when thinking about the social organisation of science and that the arguments I discuss follow a different approach.

structures in science. This is not an original approach to structuring the discussion: a number of programmatic papers for the socially oriented philosophy of science argue that these are the two distinctive themes philosophy should attend to. For example, Kitcher writes about the two groups of themes that work in science studies combining history, philosophy and sociology has begun to explore:

> The first concerns the relation between the practice of science and the values of the broader society; the second focuses on the ways in which social relations and structures of various types figure in the doing of science. (Kitcher 2000a, 45)

Similarly, Don Howard (2009) argues that "theorising the place of motives and values in science" and "theorising the social nature of science" are the two areas whose philosophical analysis is necessary for ending the tradition of socially disengaged philosophy of science.

The distinction between these two understandings of the social can thus be seen as a convenient way to map out the terrain. I do not see them as necessarily exhausting it or as necessarily mutually exclusive—indeed, as the discussion will show, these themes are sometimes intertwined within specific arguments. I believe that this distinction is nevertheless useful for structuring the discussion because, as I argue, there are some important differences in possibilities that these approaches provide for making proposals about the social organisation of science. I suggest that these differences make it necessary to strive for a philosophical account that recognises and integrates different senses of the social. This is another consideration to take into account when making proposals about the social organisation of science.

In the following section of the chapter I discuss the value-free ideal of science and some socially oriented arguments that challenge it by demonstrating the role for values in science, excluded by this ideal. The third section discusses another cluster of possibilities for approaching science as social, introducing the arguments that challenge the individualistic position in philosophy of science and show the epistemic relevance of social interactions and social structures in science. The fourth section discusses possibilities and limitations that different accounts offer for proposals about the social organisation of science, opening some aspects of science to critical examination and reorganisation while allowing others to be taken for granted. I suggest that the recognition of these limitations requires an approach to science that combines different understandings of the social. The fifth section introduces what I take to be one example of such an approach—Longino's critical contextual empiricism; Longino's account will subsequently serve as the basis for the proposals I develop in chapters 5 and 6. In the conclusion of the chapter I return to the theme of considerations I believe to be relevant for a proposal about the social organisation of science.

2.2 Science and social values: challenging the value-free ideal

A common way to talk about the social in science is to talk about values, where the values *simpliciter* usually mean social, ethical, moral and similar values.[18] Discussion of values, in turn, may be structured in several different ways. One possibility is to follow a kind of checklist for different aspects of values' involvement in science. Different arguments are then described according to their stance on, in Harold Kincaid, Alison Wylie and John Dupré's words, "the kinds of values involved, how they are involved, where they are involved, and what effect their involvement has" (Kincaid et al. 2007a, 10). Different positions follow from different answers to these questions depending on, for example, whether the values involved

[18] Biddle (2013b) argues that the terminology of values is misleading: there are many relevant contextual factors that cannot be called values. I am sympathetic to Biddle's argument: in many cases where I talk about values "social values and other contextual factors" would be more accurate. In this section I nevertheless use the notion of values for the sake of consistency with the notions of the value-free ideal and cognitive values on which my discussion is centred.

in science include social values or are limited to epistemic values or whether the influence of a specific kind of values is seen as accidental and detrimental or as inevitable and constructive. Peter Machamer and Gereon Wolters propose another approach, offering to think about the role of values at different stages of a research project as reflected (in a strongly idealised way) in the typical sections of a science paper: the choice of the problem, the choice of the approach, the experiment, the interpretation of data, the discussion of the results, and the discussion of practical implications (Machamer and Wolters 2004, 4–5).

My argument in this section follows a somewhat different route—as mentioned in the introduction, I start with outlining what can be taken to be the standard, or the a-social, position and then describe a number of arguments challenging it. In the case of the question of values in science the contrasting case seems to be relatively straightforward: arguments that show the role of various values in science oppose the position that sees no such role—the position that science is value-free. Indeed, titles such as Douglas's (2009) *Science, Policy, and the Value-Free Ideal,* Kincaid et al.'s (2007b) *Value-Free Science? Ideals and Illusions,* and Lacey's (1999) *Is Science Value Free? Values and Scientific Understanding* can be taken as an indicator of the importance of the notion of value-freedom of science for structuring the contemporary discussion (a recent history of the value-free ideal is given in Douglas 2009, ch. 3). The aim of this section is to describe what the position of value-freedom may mean and how it may be challenged.

The idea behind the value-free ideal seems to be intuitively clear: properly, scientific knowledge reflects what there is rather than what our values make us want that were (or were not); proper scientific knowledge stands independent from political, social, religious, moral etc. values. As Longino notes (2004, 128), value-freedom may be seen as a necessary precondition for the universality of science: scientific knowledge can only be something that is valid for every individual or community, regardless of their value differences, if it is itself value-free. However, as the subsequent discussion shows, it is possible to argue that science is value-free while simultaneously admitting the role for some values in science or some role for values in science. The questions that Kincaid et al. pose can thus be useful for understanding the value-free ideal as well; it turns out to be more complex than may initially seem.

First, the proponents of the value-free ideal may accept that certain values are in fact central for science, but maintain that these values, usually called epistemic or cognitive values, are distinct from moral, social and other values. So, for example, Lacey (1999, 2–11 and ch. 4) explicates the traditional value-free ideal through the thesis of neutrality (as described earlier, this is the thesis that accepted scientific theories are "consistent with all value judgements", have "no value consequences" and demonstrate "evenhandedness in application") but also the thesis of impartiality. The latter is formulated in terms of cognitive values: impartiality means that scientific theories are properly accepted on the basis of cognitive values only. The only appropriate basis for the acceptance of a theory is that it manifests these values highly, and higher than alternatives. (The third thesis is the thesis of autonomy: science develops theories in accordance with the requirements of impartiality and neutrality without outside interference.)[19]

Probably the most famous list of these special scientific values is given by Kuhn (1977): accuracy, consistency, scope, simplicity, and fruitfulness. However, there are other lists aiming to express the traditional values of science: for example, Ernan McMullin's list,

[19] As previously discussed, Lacey argues that science as currently practiced is not neutral (he discusses some ways how science could be made more so). Lacey also criticises the thesis of autonomy as an inadequate characterisation of science. The thesis of impartiality and the notion of distinctive cognitive values, however, remain crucial for Lacey's account of science despite the very considerable reworking of the value-free ideal in his account (the reworked version is presented in Lacey 1999, ch. 10).

reworked “just a little” (McMullin 1983, 15) from Kuhn’s, includes predictive accuracy, internal coherence, external consistency, unifying power, and fertility; it also calls simplicity problematic. Lacey (1999, 109) lists empirical adequacy, explanatory power, power to encapsulate possibilities, internal consistency, consonance, source of interpretative power, and rejection of ad hoc features. The understanding of the nature of these values may also differ; in particular, cognitive values and epistemic values may be taken to be synonymous or distinguished as two separate categories.[20] For example, for Laudan there are two categories of values properly involved in science (see, e.g., Laudan 2004). It is the epistemic values that are directly connected with the epistemic justification, or the question of the truth or falsity of a theory. Cognitive values are a wider set of values associated with a good theory, such as scope, generality, range of application, etc. Cognitive values do not have an epistemic rationale—they are neither necessary nor sufficient for the truth of a theory; instead, they are something that is valued in a good theory—a cognitive virtue, a value. Unlike Laudan, Lacey operates with the single category of cognitive values whose definition combines the distinct roles Laudan ascribes to epistemic and cognitive values. For Lacey, cognitive values are linked to truth—the higher is the degree to which theories manifest these values, the more rationally acceptable they are and “we have no other indicator of truth other than rational acceptability” (Lacey 1999, 46)—and simultaneously reflect what we consider the qualities desired of theories in order to advance the aims of science (Lacey 1999, 109).

Despite the differences, arguments about epistemic/cognitive values show the recognition that in one sense values are central for science. Still, as long as these essential values are seen as purely epistemic or cognitive, science could be seen as in an important sense value-free: there is no place for social values in science proper and, by extension, in philosophy of science. The preface to Laudan’s *Science and Values* (1984) provides an example of such a position. In the preface, Laudan discusses the reader’s expectation that the book with this title will contribute to the discussion of moral values in science; yet, Laudan explicitly rejects the discussion of any other kind but cognitive values as a theme for the kind of philosophical account he develops.

Another possibility to maintain the philosophical irrelevance of social values is to acknowledge that values in general, including social values, do play a role in science, arguing simultaneously that this role is limited to certain external locations. On this view, social values are excluded from the distinctly epistemic core of science. As Machamer and Wolters point out, two locations where these values are allowed can easily be acknowledged (Machamer and Wolters 2004, 1). The first of them is the stage that precedes the research proper (choosing research problems and planning research); the other is the one that follows it (deciding upon the application of the research findings). As already mentioned in the discussion of James Brown’s argument, Martin Carrier points out another generally acknowledged location—the constraints on the pursuit of inquiry justified in accordance with ethical values (Carrier 2008a, 2). In addition to those three, Lacey lists several others locations where science and values “may touch but not interpenetrate” (Lacey 1999, 17) without threatening the value-free ideal. For example, values may play a role in the “context of discovery”, motivating scientists to pursue certain lines of research or attracting their attention to certain aspects of evidence; scientists may be expected to manifest certain values in their behaviour; and science itself may be a value (Lacey 1999, 17–18).

[20] There may be finer-grained distinctions as well—for example, Douglas (2013b) distinguishes four categories of epistemic values. I focus on Larry Laudan’s distinction because it is sufficient to attract attention to the possibility for other factors to play a role in the justification of specific cognitive values, once it is recognised that they are not by themselves truth-conducive.

In all these cases, values remain in an important sense external to the core activities of science, such as the theory acceptance described by Lacey in terms of cognitive values. Social values are seen as existing on the fringes of science, "around" it rather than "inside" it, on its interface with society rather than inside the science proper. This is the "externality" model of the role of social values in science, as Longino (1990b, 85) calls it. In this model too there is no philosophical need to analyse social values because they play no role in the epistemic core of science that constitutes the proper object for philosophy.

A considerable role for values in science can thus be acknowledged without giving up the idea that science is in an important sense value-free. The aim of the subsequent discussion is to show how this idea may be challenged. On the one reading of the value-free ideal, as previously described, it depends on the possibility to show that cognitive and non-cognitive values can be clearly distinguished and that cognitive values are able to fulfil their function in science (such as determining the theory choice) on their own. On the other reading, this ideal presupposes the possibility to distinguish clearly the locations in science where non-cognitive values are allowed to play a role and the core practices of science where they are not. Arguments that deny the possibility of these distinctions undermine the viability of the value-free ideal. The ideal is clearly inadequate as a description and as normative guidance for science if other values cannot but play a role in science and their role in science cannot be restricted to limited locations.[21]

Given these preconditions of the value-free ideal, I focus on three general possibilities to argue against it: first, arguing that cognitive values are by themselves insufficient and the operation of science requires them be supplemented; second, challenging the distinct epistemic identity of these values and showing their connections with the social values; and, finally, challenging the separation between the areas where social values are presumed to operate from those where only cognitive values are acceptable.

The first possibility to argue for the inevitable involvement of other kinds of values in science is to argue that even if the distinction between different kinds of values can be sustained, epistemic and cognitive values alone are not sufficient to determine the theory choice. For example, Carrier (2008a, 3–5; 2012) points out that the classical account of cognitive values developed by Kuhn sees them as allowing a considerable freedom of interpretation when applying them; indeed, differences of interpretation are all but inevitable. Accordingly, alternative choices are possible on the basis of the same cognitive values—the choice of any particular theory is underdetermined by cognitive values (Carrier calls this problem the "Kuhn-underdetermination"). In the words of Longino (1992b, 285) who has also discussed this issue,

> to answer the question why a cognitive value was understood in a particular way in a particular context and why it was granted greater or lesser importance than other cognitive values, it is necessary to appeal to explanatory factors other than cognitive values themselves.

When a particular theory choice is made, something else must be a necessary supplement for making the definite choice—and social values may be one of such supplementing factors.

Despite endorsing the thesis of impartiality, Lacey shows another way in which cognitive values are insufficient for fully defining the kind of theories we end up with. Lacey's argument focuses on the stage before the theory choice happens on the basis of cognitive values, showing the importance of what he calls cognitive strategies. Strategies (which, by Lacey's (1999, 261) admission, share important similarities with Kuhn's

---

[21] The recognition of the essential and extensive role of "non-scientific" values is important here. As Kourany (2008b, 88–90) points out, historical and sociological work uncovering failures of science in following the value-free ideal has sometimes prompted attempts to reform science on the basis of the value-free ideal (seeing failures as contingent) rather than the rejection of the ideal.

paradigms) provide some very general constraints on the types of theories that are sought, the kind of data that is gathered and the terms in which it is described. Before the acceptance of particular theories on the basis of cognitive values is possible, those choices have to be made and they are always made under a particular strategy. Lacey argues that the choice of a strategy is in turn influenced by particular social values. Strategies and values mutually reinforce each other—for example, what Lacey calls materialist strategies have this relation of mutual dependency with what he calls the modern values of control (Lacey 1999, 109–110 and ch. 6). Lacey's account thus recognises the essential influence of social values on science as a whole—as Lacey (1999, 256) concludes, values "pervade, and must pervade, scientific practices and (in significant part) account for the direction of inquiry and for the kinds of possibilities attempted to be encapsulated in theories". In the case of Lacey's model of science, this influence is mediated through cognitive strategies in which the social preferences (for example, the preference for a certain kind of control) are transformed into epistemic preferences (for example, by deciding what objects are studied and what kind of knowledge is sought).

Longino makes a similar point when discussing the way the object of inquiry is constituted. She argues that the object of inquiry is not pre-given by nature: the "object of inquiry is never just nature or some discrete part of the natural world but nature under some description" (Longino 1990b, 99); this description makes certain questions and certain kinds of explanations appropriate and excludes others as inappropriate. The way a particular description is chosen, in turn, reflects particular interests and needs: "the characterisation of the object of inquiry depends not on what nature tells us but on what we wish to know about it" (Longino 1990b, 99). So, crucial decisions that are both epistemic (as they specify the kind of knowledge sought) and value-laden have to be made before the judgements concerning particular hypotheses and the cognitive values they exhibit become possible.

The second challenge to the value-free ideal questions another of its presuppositions: the possibility to separate cognitive values from non-cognitive ones. The possibility of such a separation is maintained, for example, by Margaret Morrison (2008) when she argues that cognitive values can be distinguished from other values even in messy situations where the two kinds seem to align. Morrison acknowledges that Karl Pearson and Ronald A. Fisher's work on statistical methods in evolutionary theory could be used to support the eugenic goals they both promoted. Nevertheless, she argues that the origins of their approaches can be traced to their methodological ideals and cognitive values. The alignment with their eugenic sympathies thus should not be taken as discrediting the mathematical approach to biology because the latter is traceable to proper scientific values; it has had "a life of its own" (Morrison 2008, 52), independent from its creators' ideological sympathies.

This possibility is challenged when one argues that cognitive values are not fully independent from social values. This is what Longino does in a number of arguments. In a series of Longino's papers (e.g., Longino 1993b; 1994c; 1995; 1996; 1997), values are understood in the sense of desirable qualities of theories. In these papers, Longino discusses a set of alternative cognitive values—empirical adequacy, novelty, ontological heterogeneity, complexity of relationship (or mutuality of interaction), applicability to current human needs, and diffusion of power—that have their source in the work of feminist scientists and critics of science. Discussion of these alternative values and their comparison with the traditional Kuhnian list allows Longino to show the usually invisible socio-political implications of the traditional values. For example, the value of novelty has a clear socio-political valence—it is appropriate for the research that aims to challenge the theories that may be involved in supporting the social and political status quo. Thinking about that, however, helps one to realise that the contrasting traditional value of consistency similarly has socio-political implications, as agreement with theories that support the status quo contributes to its reinforcement (see, e.g., Longino 1995, 392–393; she also discusses other pairs of values there). What is valued in the case of cognitive values is thus not limited to purely cognitive virtues of theories but may reflect certain social preferences as well.

Another argument of Longino's (2008b; Longino in Longino and Lennon 1997) focuses on the understanding of values that connects them with the notion of truth. The consideration that Laudan points out—that cognitive values, unlike epistemic values, have no direct connection with the truth-value of a theory—plays a crucial role in this argument. Again, Longino contrasts the commonly accepted set of cognitive values with the set of values seen as desirable in feminist accounts of science. Longino argues that values in both sets can be taken as indicative of a theory's truth-value only in complex with further assumptions. For example, the value of simplicity can only be truth-conducive if the world is ontologically simple. By themselves, these values do not prove the truth of a theory—in Longino's words, their value is heuristic rather than probative. The preference for a particular set of values thus cannot be defended as determined by their truth-conduciveness; other considerations must be involved, implicitly or explicitly, such as the preference for particular cognitive aims or certain assumptions about nature. These preferences may in turn include social values or rely on value-laden assumptions.

The strategies discussed so far focus on the nature of cognitive values. Yet another possibility to reject the value-free ideal is to focus on the location instead and to argue that there is no distinction between the external locations in science where social values have a place from internal locations where supposedly only cognitive values are relevant. This is the strategy Douglas (2009) pursues in her *Science, Policy, and the Value-Free Ideal.*

Douglas's (2009, ch. 4) argument about the essential role of values builds on the notions of uncertainty and inductive risk (as noted in the previous chapter, doing so Douglas references the earlier arguments by Churchman and Rudner). As Douglas points out, science is characterised by inescapable uncertainty. The need to make judgements under uncertainty permeates all scientific practices. For example, such judgements are made in the process of choosing the level of statistical significance for a study and the way to characterise borderline cases of evidence (should one choose to minimise false positives or false negatives?) and deciding whether a particular hypothesis is well-supported by evidence (should one rather risk accepting a false hypothesis or rejecting a true one?). These choices are not determined by evidence and they inevitably involve value-judgements: ultimately, the decision depends on ascribing values to potential consequences (what kind of mistake would have worse consequences?). As having to make decisions in the conditions of uncertainty is inescapable, so is the role of values in those decisions.

Douglas nevertheless wants to retain the idea of science as epistemically successful enterprise that can be discussed from an epistemic and normative point of view: her aim is an account that "accepts a pervasive role for social and ethical values in scientific reasoning, but one that still protects the integrity of science" (Douglas 2009, 1). In order to ensure this integrity, Douglas (2009, ch. 5) proposes to distinguish direct and indirect roles for values. The direct role means that values play the same role as empirical evidence and function as a direct reason for the scientist to accept a particular claim. There are certain stages of research where values are allowed to play such a direct role. Unsurprisingly, Douglas mentions the stage of setting the research aims and ethical limitations on research, the locations that are relatively uncontroversial even for those who defend the value-free ideal. However, if values played the direct role during the central stages of inquiry—in the process of accepting and interpreting data and accepting or rejecting conclusions on the basis of it—they would pose a threat to the reliability and integrity of science. Accordingly, the direct role for values should not be allowed there. Instead, they should only be used in the indirect role, helping to make decisions about the sufficiency of evidence and the price of potential mistakes. By proposing this mechanism for maintaining integrity of science, Douglas shows that the value-free ideal can be abandoned without epistemic damage.

Douglas's account thus rejects the possibility to distinguish the proper locations for cognitive and non-cognitive values in science: in their indirect role, non-cognitive values have an essential role in the core scientific practices. Simultaneously, Douglas's account challenges the meaningfulness of such a distinction in principle, as cognitive values are not exempt from the prohibition on the direct role of values at the stage of hypotheses acceptance. The distinction between the direct and indirect role of values cuts across the distinction between cognitive and social values: it is the kind of the role rather than the nature of the value that matters. The value-free ideal thus receives another challenge.

The aim of this section was to show how it is possible to argue against the value-free ideal and for an essential role of social values in science. As there are two forms that the characterisation of this ideal may take, the challenges to it may focus either on the nature of values involved or the location of their involvement. These challenges, in turn, open possibilities to argue for changes in the current organisation of science to the degree that it is premised on the value-free ideal. As I intend to compare these possibilities with those offered by a different understanding of the social in science, I postpone the discussion of proposals about the social organisation of science until I have discussed philosophical challenges to the individualistic approach to science.

2.3 Science and social interactions: challenging cognitive individualism

Introducing the two groups of issues related to the question of the social in science, I mentioned Kitcher's expression "social relations and structures of various types" when characterising the second cluster of issues; this is the cluster that I discuss in this section. Following the structure of the previous section, I first describe how the traditional position that denies the philosophical relevance of these social aspects looks like and then describe some possible approaches for arguing against it.

In the case of values, the value-free ideal provides a convenient starting point both for its defenders and its critics; in the case of the understanding of the social that forms the subject of this section, no comparable common shorthand exists. When it comes to the role of social interactions in science, the traditional epistemological and philosophical position seems to be that there is none. This absence of a role for social interactions, however, may be in an important sense different from the denial of an appropriate role for social values in science. Unlike the influence of social values according to the argument for value-freedom of science, the possible effect of social interactions on science is not necessarily seen as something to prevent in order to maintain integrity of science. Rather, these interactions are simply ignored as a subject for epistemology and philosophy. The traditional position is thus not so much anti-social as a-social. As Goldman describes it, "Traditional epistemology has long preserved the Cartesian image of inquiry as an activity of isolated thinkers, each pursuing truth in a spirit of individualism and pure self-reliance" (Goldman 2003, vii). If this image is accurate, analysing the individual's cognition will deliver everything that there is to say philosophically about cognition. (Stephen Downes (1993, 452) calls this position cognitive individualism—this is where the subtitle of this section is derived from.)[22]

[22] Frederick Schmitt (1994, 1–3) points out, however, that some traditional epistemologies do see social factors as a threat to the proper exercise of reason: Descartes himself is an example, as well as Francis Bacon with his "idols". McMullin (1992, 1–3) similarly mentions Bacon's view of the social factors as interfering with the discovery of nature (he (McMullin 1992, 12–16) also discusses Bacon's views on the division of labour and the creation of socially applicable knowledge). As the quote from Goldman show, it is nevertheless possible to see

Goldman argues that this long tradition in epistemology is problematic: “This image ignores the interpersonal and institutional contexts in which most knowledge endeavours are actually undertaken” (Goldman 2003, vii); his aim is to develop an extension of epistemology that will help to understand these contexts—a social epistemology. The development of this kind of epistemology, however, is not meant to challenge the traditional epistemology the way an account of the role of social values challenges the value-free ideal—it is an opening of a new area for inquiry rather than an attempt to refute the traditional approach. In Goldman’s words, he has “no general objection to individual epistemology” (Goldman 2003, 4); it is just that the individual epistemology needs a “social counterpart”. The primary focus of Goldman’s work is epistemological rather than that of philosophy of science—he begins with analysing the most general “social paths or routes to knowledge” (Goldman 2003, 4) that are not specific to the domain of science: testimony and argumentation. However, Goldman also discusses some social and epistemic issues in science, such as the distribution of research effort, the system of credit, practices of scientific publication, reliance on the expertise of others and ways of assigning authority to them (Goldman 2003, 173–182 and 250–271). In both cases, the standard position of the epistemic irrelevance of the social is rejected, as it is shown how these practices may be beneficial for knowledge creation under some conditions and damage its prospects under others.

In the remainder of the section, I discuss two arguments within philosophy of science that show the philosophical relevance of these social features of science. According to the one of them, these social interactions and relations are helpful; according to the other, they are essential or constitutive.

Susan Haack’s (1996) “crossword solving” argument is an example of the argument that brings forward the helpfulness of social interactions in science while denying that science is social in other senses (“It is false that social values are inseparable from scientific inquiry; false that the purpose of science is the achievement of social goals; … false that science should be more democratic …”—Haack 1996, 79). Haack argues that the fact that science is a cooperative and competitive enterprise plays an important role in science’s epistemic success. According to her, this role is not adequately captured by the explanation that the involvement of several individuals allows completing a task quicker or completing a task that surpasses the powers of a single individual. Instead, Haack bases her explanation on her approach to the appraisal of evidence. Haack suggests seeing it as similar to solving a crossword: evidence provides the clues; background information means that some parts of the crossword are already solved. The better an empirical proposition—a proposed “crossword solution”—is supported by the evidence, the better it fits the background information and the more secure that information is by itself, the more reasonable it is to take the proposition as well supported and to write it in. Building on this metaphor, Haack proposes to see science as a collective and competitive process of solving an enormous crossword. Its solution advances more effectively not only because many individuals are working on it simultaneously but also because their work is organised in a particular way. Due to the distribution of labour, individuals can work on different parts of the crossword, benefiting from the parts solved by others; due to the differences between individuals, different problems can be addressed by those best suited for tackling them.

Despite aiming to offer a relatively restricted view of the social, Haack shows how the community-level phenomena may be more than a simple aggregation of the individual-level ones. For example, the way biases can be overcome on the level of community differs from the way an individual could overcome them. In community, individuals that prefer to defend

---

the traditional epistemology as ignoring the social factors rather than fighting them. This possibility, in turn, offers an interesting contrast to the case of the value-free ideal.

the old theory and those quick to adopt a new one balance each other, the proneness of the followers of a particular approach to overlook its problems is counteracted by the readiness of the followers of an alternative approach to point these problems out etc. Haack suggests that as a result, community is better at avoiding a mismatch between the objective warrant for a theory and its acceptance as the psychological state of the knower (Haack's 1996, 82). What community achieves epistemically is thus superior to what the same individuals could achieve on their own; importantly, it is also achieved through different mechanisms.

On Haack's view, the sociality of science—the cooperative and competitive relations between individuals structured in a particular way—is epistemically significant as it contributes to the success of science. Nevertheless, while this social dimension of science is helpful and may be indispensable in practice, it is not necessarily indispensable in principle. It is in principle possible to formulate propositions in light of evidence and background knowledge and to resist improper acceptance of hypotheses on one's own as well. Haack's metaphor supports this possibility of dispensability, as solving a crossword is originally, and in most cases, a solitary activity. Sociality in the form of cooperation and competition may be helpful but is not essential for solving a crossword successfully.[23]

Thinking in terms of community-level phenomena may support a position that sees science as social in a different, and stronger, sense. On this alternative view, certain epistemically important characteristics of science and/or scientific knowledge are social. It makes sense to discuss them on the level of community rather than on the level of individuals; they are socially constituted and as such indispensably social. Some aspects of science discussed by social epistemologists and socially oriented philosophers are obviously community-level phenomena and cannot be otherwise: one can only discuss phenomena such as trust or the division of cognitive effort on the level of community. What makes the position I now describe more radically social is that this community-level view is applied to the phenomena that are traditionally discussed on the level of individuals, such as rationality.

Solomon's (2001) social empiricism is one example of the approach that aims to show the socially emergent nature of certain core characteristics of science. Solomon's aim is to provide an account of scientific rationality and scientific progress that overcomes the persistent conflict between accounts proposed by philosophers and sociologists of scientific knowledge, further complicated by the findings of historians, psychologists and cognitive scientists. Solomon sees a solution in a thoroughly social account of scientific change. It is not simply that collective practices of inquiry improve individuals' rationality; rationality itself is located on the social rather than individual level and is to a degree independent from individuals' cognitive characteristics. Rationality is shown to emerge from a particular social distribution of choices made in community.

Solomon begins the introduction of her account of scientific rationality with her account of aims of science. Robust (reproducible) and significant empirical success is one of the primary goals of science (along with truth; however, judgements about truth, according to Solomon's "Whig realism", can only be made in hindsight). Scientific rationality is understood as instrumental with respect to this goal: whatever leads to empirical success is rational. So, factors that are traditionally seen as non-epistemic or incompatible with proper inquiry can contribute to rationality, as long as they help to achieve empirical success. For example, personal values or interests may motivate a scientist to prefer an empirically successful theory, helping to increase its successfulness further. Solomon suggests that some of the factors that can influence scientists' decision-making (she calls them decision vectors) are empirical—they make one prefer a theory that is empirically successful in some respects.

[23] I return to the discussion of the essentially social phenomena and the social phenomena that can in principle be achieved individually in the section discussing Biddle's criticism of Longino.

Other decision vectors are non-empirical—in this case, a theory is preferred for reasons unrelated to its empirical success. This distinction cuts across the more traditional distinctions such as those between cognitive and social values, or between proper cognitive motives and cognitive biases. The preference for a theory with readily available or particularly salient data is an empirical decision vector although it is usually considered a bias; the preference for a simple theory is a non-empirical decision vector although it is usually considered a cognitive value.

To judge rationality of scientific community, it is necessary to evaluate the distribution of decision vectors there. As maximising empirical success is the aim, rational distribution of effort requires it be distributed among all theories that have some empirical successes, proportionally to the degree of success of each; otherwise, some theories promising success may not be pursued sufficiently. In the case of non-empirical decision vectors, it is important that they do not skew the choice among theories—ideally, they should favour them in equal measure. Solomon (2001, 77) succinctly expresses this requirement by saying that empirical decision vectors should be distributed equitably between theories and non-empirical should be distributed equally in order for a distribution of research effort to count as a normatively appropriate dissent. (The situation where a particular theory gathers all empirical decision vectors because no other theory has any empirical successes is a special case of this general situation.) What is subject to normative assessment and can be evaluated as rational is what emerges on the level of community on the basis of individual choices that are not (and need not be) rational in the sense of a particular attitude or particular behaviour on the part of the individual. As Solomon (2001, 120) summarises her account,

> Social empiricism is social because what matters, normatively speaking, is the distribution of empirical and non-empirical decision vectors across a community of investigators. Normative judgements are not made of the thoughts and decisions of individual scientists.

Despite the difference between Haack's and Solomon's arguments, in both cases the discussion of the importance of social interactions within scientific community invites attention to the social structures and institutions that enable or facilitate these interactions. For example, the collective crossword solving Haack describes would not be as effective if there were no possibility to trust others' results, if others' results were not disseminated effectively or if the choice of problems were skewed and so some parts of the crossword remained unsolved. (Haack (1996, 82) herself remarks that the institutionalised scrutiny and the possibility for scientists with overlapping competencies to check each other's results are important features of science.) Similarly, the equal and equitable distribution of decision vectors that Solomon describes may not be possible if this distribution is skewed due to the way funding is distributed among alternatives. Various aspects of the social organisation of science, such as scientific communication, and a variety of related issues, from peer-review to intellectual property laws, research planning and funding, legal and ethical limitations on research etc., can thus be highly relevant for science as a collective epistemic enterprise.

The recognition of the ways these aspects of social organisation have an epistemic impact, in turn, helps to challenge another variation of the argument that the social aspects of science are philosophically irrelevant. This argument parallels one of the approaches to defending value-freedom of science—there, the stages where social values obviously do play a role are seen as external to science, preceding or following the research proper. Discussing the role of social structures and institutions in science it is similarly possible to move the social to peripheral locations. In this case, one can agree that the choice of research problems or the application of results are social in the sense of being influenced by various forms of organisation of science in society, while denying that science is social in other respects. These organisational aspects of science are then seen as subject to evaluation in ethical or pragmatic, rather than epistemic terms and as such not as a topic for the philosopher of science.

The arguments that show the importance of particular social interactions or social characteristics of scientific community may challenge this a-social view of science indirectly, by attracting attention to the organisational conditions that enable these interactions and characteristics. Other arguments that focus on the problems of the current form of organisation of research, especially in biomedical sciences, can sometimes formulate and reject this a-social view openly, as in this argument of Reiss's (2010, 428):

> Some readers with a philosophy of science background may be sympathetic to the concerns regarding biomedical research (BMR) voiced here but wonder why the problem of how to organise research is their's [sic]. Aren't the metaphysical and epistemic dimensions of science the subject of the philosophy of science, and aren't there specialists—such as bioethicists or medical ethicists—better qualified to deal with this issue? Let me adduce five arguments to the effect that this way of thinking would be mistaken.

It is thus possible to distinguish two aspects of the a-social position with respect to the social interactions in science—the social side of science is thought irrelevant, first, because epistemic practices are seen as individualistic and, second, because the locations where the role of social structures is obvious are seen as having no epistemic relevance. Social challenges to these positions may differ in their targets accordingly. However, there is an important connection between them, as arguments focusing on such social dimensions of science as the distribution of research effort, communication of results and the possibility of mutual checks (and, on some accounts, such fundamental features of science as rationality) support the interest in the epistemic impact of specific forms of organisation of science. Given the epistemic significance of these social aspects of science, they become an obvious target for meliorative proposals. The aim of the next section is to discuss the possibilities and limitations for such proposals and to compare them with those made possible by the exploration of the role of values in science.

2.4 Proposals, possibilities and limitations

It may be possible to maintain that science (in the sense of its core epistemic practices) is free from social values or that it is reducible to the individual's cognitive activities and yet agree that some forms of organisation of science—for example, a specific approach to distributing funding or a specific set of ethical regulations—are better socially than others. These forms of organisation, however, would in this case be seen as in important sense external to science—again, as belonging to the external context of science rather than its epistemic core.

Such an argument differs from the socially oriented arguments that I discuss in two crucial respects. First, it treats the question of the social organisation of science as quite independent from questions related to its epistemic dimension. Following this argument, one may acknowledge, for example, that a certain research project should not receive funding because other projects have a higher priority for practical reasons, and yet maintain that the epistemic significance of the project is a matter independent from social values and structures. Social values might have defined the ranking of projects for funding and a particular approach to the distribution of funding may be realised in the form of particular social relations and structures; epistemic significance, however, does not depend on any of these, or other, social factors and stands unaffected by them. Or, one may acknowledge that the application of particular scientific conclusions would be detrimental for certain important social values and thus should be blocked, and yet maintain that no social values or other social factors were involved in the epistemic practices through which these conclusions were reached and deemed epistemically significant and virtuous. Second, treating the social organisation of science as external to its epistemic dimension undermines the rationale for discussing it within philosophy of science. If social values or social relations and structures have no significance for the science proper, the question concerning them would not be a question about science as science and thus not really a question for philosophy of science. This is the position that can be seen behind Laudan's (1984, xi–xii) admission that he has nothing to say on the matter of ethical values or behind Reiss's (2010, 428) imaginary opponent's suggestion to turn to bioethicists.

Philosophers offering the socially oriented arguments that I discuss in the thesis reject both elements of this position: as they see a role for the social within the science proper, discussing the social organisation of science is seen as an appropriate task for philosophy of science. The aim of this section is to offer an overview of the opportunities that the recognition of the epistemic significance of a certain social aspect of science opens for making proposals about the social organisation of science. In the section, I also argue that both the approach focusing on the social values and the approach focusing on the social interactions and structures have important limitations that can only be overcome by establishing connections between the two. A social account of science that treats systematically different aspects of the social is thus desirable and in the next section I introduce Longino's account as one such approach.

To the degree the current organisation of science relies on the value-free ideal or cognitive individualism, the refutation of these a-social positions invites certain changes. At the very least, the recognition that science does have a social dimension is called for both within philosophy of science and in the public's understanding of science, even if the practices and institutions of science remain otherwise unchanged. This is a relatively conservative position, as the change is limited to the eradication of a mismatch between how science actually functions and how its functioning is understood.

Different challenges to the value-free ideal open possibilities for proposing more extensive changes as well. To begin with, they may show that with the value-free ideal more is at stake than just an adequate understanding of science. For example, Douglas argues that the value-free ideal not only fails to characterise science adequately but is also profoundly undesirable, as it masks the role of value-judgements in science and the responsibility of scientists for the value-laden choices they make (see, e.g., Douglas 2009, 175–177). These considerations add support for the need to make proposals for change.

These proposals may develop in several directions. One possibility that the arguments targeting the idea of independent and self-sufficient cognitive values open is challenging science as it is currently organised and practiced, in terms of values. So, it becomes possible to ask what social values are involved in the presently endorsed cognitive values and to criticise them on these grounds or to offer alternatives involving different values. If cognitive values are indeed intertwined with social ones, those alternative lists of cognitive values may in principle be as defensible as the current one—it is not the case that the traditional cognitive values are independent from social values and only the alternative ones are defined by them. The establishment of an alternative form of science that consciously pursues different values becomes then a possibility—such as the science inspired by the feminist values that Longino discusses.[24] Lacey also explores the possibility of alternative science, framing it in terms of an alternative cognitive strategy, related to a different value complex and a different social and economic arrangement that would allow this alternative value complex to thrive. Lacey describes two such alternatives, the "grassroots empowerment" approach (Lacey 1999, ch. 8) and the "feminist" approach (Lacey 1999, ch. 9). So, once the value-free ideal is rejected as empirically inadequate and normatively undesirable, it becomes possible to think about the social values with which the current form of organisation of science is shot through and to discuss whether an alternative form of science reflecting different values may be more preferable.

[24] I return to the discussion of Longino's proposals for feminist science in the next section.

Douglas's argument about the role of values in judgements about the sufficiency of evidence may similarly support a discussion of the values involved and their social desirability. In Douglas's proposals, however, the focus is on procedures that allow making judgements about values in a more transparent, morally responsible and socially desirable way rather than on specific values to be realised in science. Douglas (2009, ch. 7) shows that the model for organising advisory science that has predominated since the 1970s fails to recognise the inevitable role of social values in experts' judgements. According to this model, one is to approach risk analysis as consisting of two stages: purely scientific value-free risk assessment (determining risks) and political value-laden risk management (deciding whether a particular degree of risk is acceptable in given circumstances and planning the action accordingly). If, as Douglas argues, risk assessment inevitably involves judgements about the sufficiency of evidence, which in turns involve value-laden weighing of the consequences of an error, this distinction cannot be sustained. Accordingly, Douglas proposes a novel approach to risk analysis. Scientists in their advisory role should be open about the factors, including values, that play a role in their judgements under the conditions of uncertainty and the institutional conditions facilitating this openness should be created. This would make these judgements both more transparent and more credible, because discussing the role of values openly would help to determine whether the role of the values involved is appropriate. It would help to distinguish cases of "junk science" from the cases where disagreements between experts are legitimate and simply reflect different judgements about the sufficient amount of evidence and the price of a possible error.

The acknowledgement of the role of values also allows proposing changes in the way laypersons are involved with science. Douglas suggests that the burden of identifying values relevant for a particular case and choosing appropriate values could be eased by a greater involvement of non-scientists.[25] In some cases, they could take part in the "analytic-deliberative process" directly, discussing the relevant values (but without attempting to influence the outcomes of the analyses). This kind of participation is possible if there are a limited number of well-defined stakeholders who can jointly organise and fund a risk assessment and are ready to discuss the values they bring to the process. As an example, Douglas (2009, 164–167) describes the risk analysis jointly planned, funded and overseen by the oil industry, the local community group and the governmental agencies for addressing a specific issue, the choice of the tug boat to be used so as to prevent oil spills, in Valdez, Alaska. If the issue has a more global character, the number of stakeholders is large or there are no clearly defined stakeholders, a different form of involvement is needed. Douglas proposes that as an alternative, representatives of the public could participate in a separate deliberation exercise (Douglas (2009, 167–169) discusses consensus conferences as one example), defining the values that should be taken into account in subsequent risk assessments. The consensus over the values achieved would then provide guidance for scientists in decisions under uncertainty.

Different challenges to cognitive individualism in understanding science may also offer possibilities for making proposals about the organisation of science. Once the epistemic importance of certain social relations and structures is recognised, an entire new field is open for normative epistemological advice: evaluating different alternative social knowledge-

[25] An earlier article of Douglas's (2005, 158) similarly focuses on value judgements but brings out other possible contributions from the public that concern framings and local knowledge:

> 1) Citizens can help to better frame the problem to be addressed. (Are the appropriate range of issues and potential solutions being considered? Is the scope of the analysis appropriate?) 2) Citizens can help provide key knowledge of local conditions and practices relevant to the analyses. 3) Citizens can provide insight into the values that should shape the analyses. (How do citizens weigh the potential consequences of error? What kinds of uncertainties are acceptable or unacceptable? What assumptions should be used to structure the analyses?)

producing practices and proposing improvements. So, for example, Goldman argues that unlimited free speech ("free marketplace of ideas") is worse for promoting the growth of true beliefs than certain alternative arrangements that regulate speech (Goldman and Cox, 1996; Goldman 2003, ch.7). This argument, in turn, has implications for the organisation of various practices in science, including, for example, scientific publishing, presentation of testimony on the part of scientific experts or resolution of scientific controversies.

The recognition of the epistemic significance of certain social aspects of science, such as cooperation, communication or trust (without which, for example, Haack's collective "crossword-solving" is impossible), may also prompt philosophical proposals for change if the current organisation undermines these aspects of science. Haack herself lists a number of potential obstacles, from the burdensome procedures for obtaining funding to the special interest preferences for getting specific results (Haack 1996, 83). A number of arguments referenced when discussing the themes of privatisation and commercialisation of science can be seen as an attempt to address such obstacles that the recent developments in science policy and changes in science's social position are making increasingly serious.

Simultaneously, these arguments can be seen as building on yet another dimension of the social in science—the newly recognised epistemic significance of such aspects of the organisation of science in the context of wider society as funding and intellectual property regimes. The previously discussed proposal of James Brown's to socialise biomedical research is one example of such an argument. There, the demonstration of the negative epistemic consequences of the current funding and patenting system, including the consequences for such social aspects of science as trust in published results and the possibility of mutual checks, but also such important epistemic questions as the choice of directions for research, prompts a proposal for changes in this system.

The proposals discussed so far under the heading of social structures concern the aspects of science that are indisputably social—it is their epistemic importance that was traditionally denied, rather than their social character. A proposal for change may also be invited by an account that aims to show the social dimension of a phenomenon that is not usually conceived in social terms, such as rationality in Solomon's account. On Solomon's view, rationality is understood in terms of an appropriate distribution of decision vectors, and this is what the measures concerned with the improvement of science's rationality should target. Solomon (2001, 148–151) suggests that an individual or a collective body in the position to influence the direction of research (for example, a journal editor or a grant committee) should analyse the distribution of vectors in a particular case in cooperation with the scientists involved in the case and researchers that are expert at identifying decision vectors (for example, cognitive scientists or sociologists). If the distribution of decision vectors is shown to be improper, decision-makers are to propose ways to balance decision vectors between options. Solomon's account thus offers a new basis for the operation of institutions concerned with making decisions about research planning, funding and publication.

So, both social accounts of science that focus on the role of social values and those focusing on the role of social interactions and structures open a variety of possibilities for proposals about the reorganisation of science. The aims of this reorganisation may include bringing approaches to organising and governing science into a better alignment with how science actually is (for example, by acknowledging the role of social values or by thinking about rationality in social terms). Or, these proposals may aim to change the social structures, practices and perceptions that threaten the successful functioning of science (for example, by exposing the undesirable effects of the value-free ideal on scientific advice or reforming the epistemically detrimental funding system). Or, the aim of the proposals may be improving science epistemically or socially (for example, by developing research under a different cognitive strategy or devising social practices that deliver true beliefs more effectively). There are thus certain overlaps between what different approaches may achieve. In the remainder of the section I suggest that even so, focusing on one aspect of the sociality of science exclusively has some important limitations when it comes to making proposals about the social organisation of science. Overcoming these limitations requires taking into account both senses of the social.

Discussing the way the process of hypothesis acceptance is depicted as free from social values with the help of the distinction between the context of discovery and the context of justification, Longino points out that this approach allows for a role of non-empirical factors in science. This role, however, is limited to the context of discovery and these factors are understood as random peculiarities of individuals:

> They are treated as randomising factors that promote novelty rather than as beliefs or attitudes that are systematically related to the culture, social structure, or socioeconomic interests of the context within which an individual scientist works. (Longino 1990b, 64)

So, there is a danger in focusing on the social values alone. A discussion of values in isolation is not sufficient: it has to go on to include a discussion of social structures and institutions that enable the realisation of these values (or could enable it for an alternative set of values, if a reorganisation of science is proposed). The arguments challenging the value-free ideal that I have discussed so far, fortunately, avoid this danger. They do allow seeing the systematic relations between, for example, specific cognitive values and the social preference for a particular kind of control, or between specific judgements about risk and the judgement that one type of mistake is worse than the other. The recognition of these systematic relations, in turn, makes it necessary to discuss the social structures involved, such as the funding systems or the ways scientific research is interconnected with the interests of the private enterprise or the government. The new recognition of the role of values may also require a discussion of novel forms of organisation to manage it, such as the ways to involve the public in the discussion of values in the context of specific research projects that Douglas outlines.

The accounts that focus on the social relations and structures in science, on the other hand, may seem to have less need for a discussion of values. These accounts analyse the epistemic significance of social relations and structures. In this case, it is in principle possible to focus on the epistemic dimension alone and to leave unanalysed the social factors that might have played a role in the constitution of epistemic aims or epistemic values. The isolation of the epistemic dimension is not an unavoidable feature of these accounts—many of the arguments concerned with privatisation and commercialisation of biomedical research, such as the previously mentioned argument of Reiss's, discuss the intertwining of epistemic and social consequences of commercialisation. Such an insulating position, however, remains a possibility and some arguments openly adopt it. For example, Goldman explains from the very beginning that he is interested in one dimension of social practices—the truth-related—despite the recognition that social institutions and practices usually have more than one dimension and have to balance several values and aims (see, e.g., Goldman 2003, 6). Moreover, even arguments that discuss both epistemic and practical problems of the contemporary organisation of science may treat the epistemic dimension of science as ultimately independent—such is James Brown's position that I have criticised.

The possibility of treating the question about the best social practices and structures to realise the epistemic aims of science as a standalone question, in turn, may help questions about the social constitution of these aims to fade into background. These aims, such as the growth of the body of true beliefs or the creation of theories exhibiting certain epistemic qualities are taken for granted. This, in turn, limits what aspects of science are open to criticism and change. The proposals based on such accounts may primarily help to improve science so that it could do better what it has been doing rather than helping to question why it has been doing that or whether it should be doing that. So, the question asked may be how to better enable the discovery of epistemically significant truths or the creation of theories that realise widely acknowledged cognitive values, rather than what social factors might have played a role in the constitution of these aims as epistemically significant or whether we want these values advanced. These arguments are thus in important sense different from, for example, Lacey's approach that shows the crucial role of social values for the choice of a cognitive strategy and thus opens the possibility to discuss current cognitive strategies and the social arrangements that support them, on the basis of values.

I suggest that the exclusive focus on the social structures to promote epistemic advancement of science is problematic for two reasons. First, the discussion of challenges to the value-free ideal has demonstrated reasons to acknowledge the connections of cognitive values with other kinds of values and the role of values in different epistemic practices of science, from defining the object of inquiry and the kind of knowledge sought to making judgements about the sufficiency of evidence. These challenges make the idea about independent, self-sufficient and self-evident cognitive values and aims deeply problematic. Second, as I argued in the previous chapter, thinking about the practical consequences of science and, more generally, about science as practice undermines the possibility to insulate its epistemic dimension from its practical one. The approach that tends to focus on the epistemic advancement of science alone is thus unsustainable, once the practice-based view of science is adopted. (The approach that focuses on the role of social values, on the other hand, helps to see the intertwining of epistemic and practical consequences of research. For example, the epistemic values that research advances may be influenced by certain social values and preferences and the results of this research in turn may further advance these social values. This is what happens, as Lacey argues, in the case of the symbiosis between materialistic strategies and the value systems that assign high significance to the value of control.)

There are thus reasons to take into account both dimensions of the social in science and to seek a synthesis between different approaches. The aim of the next section is to introduce an account that can be seen as a combination of the two approaches to the social that I have discussed so far, as it shows both the role of values in the core practices of science and the importance of the social structures of science for the epistemic integrity of these practices—Longino's critical contextual empiricism.

2.5 Longino's critical contextual empiricism

The aim of this section is to provide a systematic overview of the account of science that Longino has been developing, structuring it around the two general philosophy of science books of hers, *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry* (Longino 1990b) and *The Fate of Knowledge* (Longino 2002a). One of the aims of the section is to demonstrate how different senses of the social can be discussed together in an account of science and how this account may in turn support proposals for the organisation of science; in this sense, it continues the themes of the previous sections. However, as my subsequent argument builds on Longino's ideas, the overview is considerably more detailed than the other examples I discuss in this chapter.

In the introduction to *Science as Social Knowledge*, Longino explains its origins with the frustration caused by the inability of philosophy of science to account for scientific debates that have both evidential and ideological aspects, and to address the relation between science and values more generally. Longino describes the context of her work as characterised by the growing awareness about the links between scientific inquiry and social values, uncovered by historians and sociologists of science, feminist critics of science and scientists themselves. Longino suggests that these social connections should be taken into account when discussing traditional philosophical topics such as objectivity and justification. According to Longino, philosophy of science has not done it yet and her book is an attempt to address this need by providing an account that "reconciles the objectivity of science with its social and cultural construction" (Longino 1990b, ix).

Discussing the role of values of science Longino proposes to distinguish constitutive values that are grounded in a particular understanding of the aims of science and contextual values—social and political values that characterise the social context in which science is done (Longino 1990b, 4–7). The idea of value-freedom is usually interpreted as meaning that these types of values are independent from each other and that science is free from contextual values. The extent of this freedom can be understood differently. According to the proponents of the stronger thesis of autonomy, the development and direction of science is unaffected by its social context. The proponents of a weaker interpretation, the thesis of integrity, recognise the influence of social values on the choice of research questions but maintain that the internal practices of science, such as observation, experiment and theory construction are free from contextual values.

The aim of Longino's account is to argue, against both versions of value-freedom, that contextual values and scientific practices interact and this interaction is necessary for science. However, the recognition of this interaction does not mean the abandonment of certain standards of rationality. For Longino, upholding such standards is enabled by the fact that science is social in another sense—that is, cooperative and interactive. Longino's approach to science is based on a particular understanding of the relation between the social as value-laden and the social as interactive:

> the development of knowledge is a necessarily social rather than individual activity, and it is the social character of scientific knowledge that both protects it from and renders its vulnerable to social and political interests and values. (Longino 1990b, 12)

This necessarily social character of science is brought forward in Longino's analysis of evidential reasoning (Longino 1990b, ch. 3; 2002a, 124–128). Her aim is to provide a general descriptive account, using a discussion of both everyday reasoning and examples from the history of science, how a state of affairs is taken to be evidence for a hypothesis. Longino begins by pointing out that there is a logical gap between the two. One way to demonstrate this starts with the practice of reasoning from a particular state of affairs to a particular hypothesis. As Longino argues, a state of affairs does not dictate a particular hypothesis: there is no unique correct characterisation for a state of affairs and no unique one-to-one relation between it and a particular hypothesis. Another way to make this point is to focus on the relation between the language used in hypotheses and the language used in the descriptions of evidence. As Longino points out, the content of a hypothesis (which typically focuses on underlying mechanisms, principles and processes) always exceeds that of the observational statements taken to be the evidence for it (as they describe particular observable objects and events). In many cases, the hypothesis contains concepts that are absent in the observational statements; even in the case of generalisations that do not involve new concepts, there is a logical gap between particular instances and the general statement.

Longino argues that this gap can be closed and a state of affairs seen as evidence only in light of some beliefs about existing regularities or relations between states of affairs (including the belief in inductive generalisation). If these beliefs (that Longino calls background beliefs if they are explicitly adopted and background assumptions if they remain implicit) are sufficiently different, the same state of affairs may be taken as evidence for different, possibly contradicting hypotheses; different assumptions may also lead to different descriptions of the state of affairs, making different aspects of it salient as evidence.

Longino points out that such an analysis of evidential reasoning needs not threaten the notion of rationality: it still describes the acceptance of beliefs on the basis of evidence. The notion of objectivity, however, may seem problematic in light of this account. Longino sees background assumptions as inevitably involved in evidential reasoning—as the enabling conditions for evidential reasoning, as the necessary filler for the logical gap between evidence and hypothesis. This raises the question how background assumptions are themselves to be justified (requiring evidential support only shifts the problem, as the determination of the evidential relations would have to involve some assumptions on each new level). In particular, it raises the concern that contextual values can influence evidential reasoning and acceptance of hypotheses via their presence among background assumptions. While contextual values and interests need not be involved in all research, or all research to the same degree, their influence on assumptions, practices and descriptions of data cannot be excluded in principle.

Longino's argument for the possibility of objectivity in this contextualised account of evidential reasoning begins with distinguishing two senses of objectivity (Longino 1990b, ch. 4; 2002a, 128–135). In one sense the notion of objectivity (for a hypothesis) is connected with the notion of truth, of depicting its object accurately. In another sense, objectivity (for a method) can be explained as non-subjectivity or non-arbitrariness, independence from subjective preference or bias. Longino focuses on objectivity in the second sense and argues that it is possible thanks to the social nature of science. According to Longino, sociality is a necessary characteristic of science. It is not simply that science is characterised by cooperative activity; the crucial fact is that the products of individuals' activities become a part of scientific knowledge as a result of social processes. Knowledge is not a simple aggregate of individual contributions: these contributions undergo, both before publication (above all, in the process of peer-review) and afterwards (in citation and use, replication, application in new contexts and modification), processes of collective assessment, criticism and reworking. Whether an individual's claim would be recognised as part of scientific knowledge is thus never defined solely by the individual but depends on the activities of community. Scientific knowledge is produced collectively.

This collective production of knowledge, in turn, makes it possible to address the threat that subjective preference or bias could enter scientific knowledge uncontrollably. The basis for objectivity in Longino's account is intersubjective criticism, particularly the kind of conceptual criticism that questions the relevance of evidence for a particular hypothesis, thus targeting the background assumptions involved in connecting the two. Accordingly, this criticism provides a possibility to block the influence of subjective factors through background assumptions. As Longino (1990b, 73–74) writes,

> As long as background beliefs can be articulated and subjected to criticism from the scientific community, they can be defended, modified, or abandoned in response to such criticism. As long as this kind of response is possible, the incorporation of hypotheses into the canon of scientific knowledge can be independent of any individual's subjective preferences.

Objectivity (in the sense of freedom from subjective bias) is thus understood as a characteristic of community rather than of individuals' actions or attitudes: objectivity is possible thanks to community's processes of criticism that Longino calls "transformative" (Longino 1990b, 76).

An important consequence of understanding objectivity in this way is the recognition that objectivity is a matter of degree, as it depends on effectiveness of criticism. Longino suggests that this effectiveness requires conforming to certain norms, so that criticism could be presented freely and have an effect on community's claims and procedures. She formulates four such requirements (Longino 1990b, 76–81 and 2002a, 128–135):

1. Recognised venues for criticism, so that criticism could be published and valued in the same way as original contributions;
2. Shared standards, so that the relevance of critical arguments and responses to them could be judged on a non-subjective and non-arbitrary basis;
3. Community response (uptake), so that critical discussion could have an effect on community knowledge;
4. (Tempered) equality of intellectual authority, so that the authority or lack thereof of a claim would not be determined by political power of its proponents. The "temperedness" requirement acknowledges at the same time that different members of community may have different levels of authority due to their previous track record and other relevant factors.[26]

One particular form of the violation of the requirement of equality that Longino specifically discusses is the practice that excludes members of particular groups—such as women and racial minorities—from scientific communities, or devalues their contributions (Longino 2002a, 132). The ensuing homogeneity threatens objectivity of community, because even in the situation of ongoing critical discussion, background assumptions that are shared by all participants are likely to remain hidden (Longino 1990b, 223). So, for example, in the absence of women sexist assumptions may remain unchecked. Most generally, a limited variety of perspectives in critical dialogue makes it less effective. Longino's criteria thus show the importance of inclusiveness and cultivation of alternative points of view. The wider is the variety of perspectives in community, the more likely the exposure of otherwise invisible assumptions is and thus the greater community's objectivity.

In *The Fate of Knowledge*, Longino continues the exploration of these themes. This time Longino describes the situation her account is to address as the stalemate between philosophical accounts, which explore normative questions (such as the relation between the notions of knowledge and justification) but fail to account for the actual practice of science, and sociological accounts, which describe the actual practice but treat normative requirements as irrelevant or inapplicable. Longino sees the cause of this conflict in the acceptance of the "rational-social dichotomy" (Longino 2002a, 1) by both sides. According to this dichotomy, the rational and the social exclude each other and, as a result, a normative rational account cannot involve social aspects, and a social account cannot be normative. Longino's account attempts to expose and overcome this dichotomy; the previously developed account of evidential reasoning and objectivity becomes one of the elements of this more general argument.

Longino attributes the persistence of the dichotomy to the confusion between different senses of "knowledge", the dichotomous understanding of the positions that are possible with respect to each sense and the inability to see alternative possibilities of interpretation. In order to disentangle this confusion, Longino distinguishes three senses of knowledge (Longino 2002a, 77–89).

Knowledge can be understood as knowledge-productive practices. On the social side of the dichotomy, these practices are any practices that are recognised in the given community as knowledge-productive, with the resulting beliefs accepted in community as knowledge. This is the primary sense of knowledge for the social side. On the rational side, only practices

[26] Longino acknowledges that the requirement of the "temperedness" of authority raises a number of important questions. I return to the relations between different requirements and possible tensions in section on the German petunia controversy.

that rationally justify beliefs and normatively warrant their acceptance can be called knowledge-productive.

Knowledge can be understood as knowing, as the relation of the knower to the content. On the social side of the dichotomy, knowing is understood as the individual's acceptance of what is acceptable as knowledge in the respective community. On the rational side, it is understood as the relation to what is true and acceptance of which can be justified.

Knowledge can be understood as content. On the social side it is understood as what is accepted in the given community as the result of its knowledge-productive practices. On the rational side it is understood as the set of truths that is known to the knower or the community. This is the primary sense of knowledge for the rational side.

According to Longino (2002a, 89–93), the two sides in the conflict embrace a dichotomous approach to the positions possible with respect to each sense of knowledge. The social side maintains relativism with respect to practices, nonindividualism with respect to agency and nonmonism with respect to content. The rational side holds nonrelativism with respect to practices, individualism with respect to agency and monism with respect to content. Both complexes are taken as a whole: the acceptance of one element is believed to be inseparable from the acceptance of the others and the criticism aimed against one element is often taken as undermining the others. Longino proposes to overcome the dichotomy by severing these ties and offering alternative interpretations for each element. Her position includes nonrelativism with respect to practices, understood through a contextualist account of justification; nonindividualism with respect to agency, understood socially, as interdependence of knowers; and nonmonism with respect to content, understood as pluralism.

Longino's account of evidential reasoning and the related account of objectivity are used to provide a contextualist account of knowledge-producing practices (Longino 2002a, 97–107). The account of justificatory reasoning, understood as a social, critical, interaction-based process, is supplemented with a similar account of observation. Evidence is social, because in order for the individual's perceptual data to count as scientific data, it has to be ordered on the basis of concepts and categories shared with others and to be reproducible and transferable between individuals.[27] Observation thus involves the same requirements of being able to meet intersubjective criticism and having to pass through the process of criticism and reworking from multiple points of view before being publicly recognised, as evidential reasoning does. Given these accounts of observation and evidential reasoning, Longino concludes that the rational-social dichotomy is overcome in the case of knowledge-productive practices, because they are processes that are both cognitive and social.

In accordance with the aim to dismantle the rational-social dichotomy across all three senses of knowledge, Longino (2002a, 107–108) discusses cognitive agency as social both in the sense that individuals are socially located and in the sense that they are interdependent.[28] Yet again, this sociality is seen as compatible with normativity, as Longino's account of objectivity provides normative constraints on individuals' and communities' claims to knowledge.

[27] Some remarks about the social nature of observation are already present in the earlier book, for example, when Longino writes about the intersubjective side of public descriptions of experience: "[o]nce we say of a mark on a gauge that it represents 10 units of a measure, we cannot read it as 7 or 13 without the consent of those with whom we need to communicate about the gauge" (Longino 1990b, 221). The discussion of the ways values may influence the characterisation of evidence through the use of value-laden terms throughout Longino (1990b) brings forward the need for social checks in the case of data just as in the case of reasoning.

[28] I return to the discussion of Longino's conception of cognitive agent in the section dealing with Biddle's argument about the character of Longino's account.

Finally, in order to provide a pluralist and social account of knowledge as content, Longino (2002a, 108–121) introduces the notion of conformity. Content is understood in terms of models that can conform to their object in a different respect and to a different degree (like different maps can conform to the same terrain in different ways). The notion of conformity is similar to the notion of truth because both designate successful content. Conformity, however, unlike truth, admits different degrees. (Truth is absolute and as such can be treated as the limiting case of conformity; however, there are also cases such as similarity, fit etc. in the case of which it is possible to say in what respect and to what degree the model conforms.) Due to the variations in degree or respect of conformity, there may be numerous, and not necessarily compatible, successful representations of the same object. Knowledge is thus partial, provisional and plural: there are many possible models, their adequacy is judged in a context and may change with changes in this context, and none of them exhausts its object. There is an element of sociality in the notion of conformity, as the choice of the perspective and the degree of accuracy reflects knowers' interests and aims, and the acceptance of a model depends on its capacity to enable community to act successfully with respect to its object. Yet the successfulness of a given model ultimately depends on the relation between the model and its object: like with maps, once the mapping conventions are set, the features of the terrain define the adequacy of the map. Accordingly, the notion of conformity provides constraints on what counts as knowledge similarly to the constraints imposed by the standards of objectivity. Social and normative aspects are integrated.

In her books, Longino does not offer detailed proposals for the reorganisation of science; she does, however, discuss some implications of her account. There can be distinguished three major aspects of this discussion.

First, Longino's account attracts attention to the conditions of effective critical dialogue as a crucial precondition of objectivity. Accordingly, identifying factors that can interfere with critical discussion becomes important. Some of these factors, such as the traditional exclusion of women and minorities from science, have characterised science for most of its history. Practices of exclusion may continue to play a role in more inclusive contemporary communities as well: Longino (2008b, 83–84) discusses the need to identify and address practices that marginalise members of certain social groups in scientific community while privileging others. Other potential obstacles for critical dialogue are a more recent development. In particular, Longino (1990b, 80 and 90–92) discusses two major consequences of commercialisation of science.[29] First, the importance attached to criticism diminishes in the situation where it is the novelty, originality and commercial value of the results that are predominantly valued. Second, the treatment of knowledge as commercially valuable property that is to be protected disrupts the process of critical discussion where claims are both assessed for objectivity and used to assess other claims. As a result of secrecy, the overall critical discussion becomes less effective. While Longino does not propose specific changes in science (although she mentions (Longino 1990b, 91) the proposal that scientists be prevented from receiving immediate financial profit from their research), the discussion of concerns associated with present trends in the organisation of science can be seen as a call for action to counter them.

Longino's account of evidential reasoning makes the social structure of scientific community and relations within it a crucial location for assessing the epistemic character of

[29] In her earlier papers, Longino (1984; 1986) also discusses secrecy and related obstacles to critical dialogue; besides commercialisation, her particular focus is on the influence of military interests and the way they are reflected in the state's science policy, especially in the sphere of scientific communication. I will return to the discussion of issues related to commercialisation in chapters 5 and 6.

community. Attracting attention to them also has a political dimension. As Longino writes about the "critique of the demographic structure of scientific communities that exclude, via various mechanisms, significant portions of a population", "[a]lthough epistemological in character, such a critique is political in effect, being directed at structural features that are political in origin (and must be fixed by political action)" (Longino 1997, 119). Throughout Longino's discussion of the possibility of feminist science a similar point is made repeatedly: in a society characterised by power inequalities, including the less powerful in scientific community in order to improve its objectivity is a political question as much as epistemological—a matter of "conflict and hope" (Longino 1993a, 118).

Longino's criteria thus open the possibility to discuss the conditions for objectivity that exist in a particular community or in a particular social and political context, to draw attention to their failures and to invite and justify changes in order to improve these conditions. In addition to that, her contextual account of justification opens the possibility to discuss particular hypotheses, models or approaches from the point of view of values embedded in their background assumptions, their interactions with wider contextual values and their material and social consequences. Longino develops this possibility in two directions.

First, Longino offers (Longino 1990b, ch. 6–8 and the entire 2013 book *Studying Human Behavior*) several detailed analysis of research programmes in human biology. (In 1990b, Longino juxtaposes the "linear-hormonal model" and the "selectionist theory of higher brain function" in research on sex differences. In 2013, she discusses five approaches to studying sexual orientation and aggression: quantitative behavioural genetics, social-environmental approaches, molecular behavioural genetics, neurobiological approaches, and integrative approaches such as the developmental systems theory; human ecology approaches are briefly discussed as a contrasting case.) Longino discusses at length the evidence, the hypotheses and the models and assumptions necessary for connecting the two, and also the possible social uses of the hypotheses. Exposing the role of contextual values does not automatically classify research as bad science—Longino's account shows that science cannot be insulated from them in principle—but it provides a new perspective for discussing it. In particular, it enables one to analyse the interplay between the values embedded in researchers' assumptions and the values of wider society, and the implications for the uptake, interpretation and social uses of research.

Second, in Longino (1990b) and a series of articles (e.g., Longino 1987a; 1992a; 1993a; 2003), she discusses, on the example of "feminist science", the possibility to develop, intentionally and openly, alternative approaches and explanatory models on the basis of one's commitments and values. As the influence of contextual values cannot be fully eliminated from science, developing such an account should not be automatically classified as epistemically improper. Mainstream research is not in principle insulated from the influence of its social context. Accordingly, there is no situation of the contrast between properly value–free and improperly value-laden science but rather a situation where different approaches proceed from different background assumptions, which in turn reflect different values. As Longino (1990b, 191) writes,

> The idea of a value-free science presupposes that the object of inquiry is given in and by nature, whereas the contextual analysis shows that such objects are constituted in part by social needs and interests that become encoded in the assumptions of research programmes. Instead of remaining passive with respect to the data and what the data suggests, we can, therefore, acknowledge our ability to affect the course of knowledge and fashion or favour research programmes that are consistent with the values and commitments we express in the rest of our lives.

More generally, the recognition of the role of background assumption may encourage the development of a wider spectrum of alternatives (and a greater attention to them), benefiting research community with new insights (Longino 1990b, 219).

Simultaneously, Longino stresses that the development of an alternative approach has to take into account the requirements for objectivity in general and, in order to be recognised as scientific, some of the norms of the relevant scientific community. It means that such an account has to remain local. Besides the possibility to be recognised in, and interact with, scientific community, there are other reasons to maintain continuation with traditional science: in particular, Longino (1990b, 213–214) mentions the need to address global problems, which requires scientific knowledge and thus the continuing development of existing science.

The requirement of *Science as Social Knowledge* that both the analysis of the assumptions of existing research programmes and the development of alternatives be sensitive to the local context is further developed in *The Fate of Knowledge*, as Longino (2002a, 173–174) introduces the distinction between general and local epistemology. General epistemology is an exploration and interpretation of general epistemic concepts such as objectivity and justification. In particular, Longino suggests that her norms of transformative criticism should be understood as an explication of the notion of objectivity or knowledge. The norms are meant to reflect certain shared epistemic intuitions, such as the distinction between opinion and knowledge, which consists in the ability to withstand intersubjective criticism. Local epistemology, on the other hand, is the set of public norms characterising a specific local community; they are binding for members of the community and those wishing their claims to be accepted by the community. A community's local epistemology includes a set of substantial and methodological assumptions, questions, aims and kinds of knowledge sought; its standards, including preferred theoretical virtues, are derived from those questions and aims. Elements of such a local epistemology may reflect influence of the wider intellectual, institutional and social context of science. The contingent nature of local epistemologies, however, does not mean that they should be abandoned—rather, it means that their validation depends on their ability to withstand criticism and thus requires openness to criticism on the part of the respective community. So, the account that Longino ultimately offers is local, social and normative.

Longino's account can be called social in several senses that I have previously discussed. It shows the role of social (contextual) values in science, thus offering a distinct type of an argument against the value-free ideal (Kristina Rolin (2015; 2016) calls it "the argument from value-laden background assumptions"[30]). It shows how the fundamental epistemic processes such as evidential reasoning involve contextual values and are in this sense social. It shows how certain crucial aspects of science, such as objectivity, are fundamentally social in the sense of being the result of social interactions. It shows how certain aspects of the social organisation of community, such as its social inclusiveness or the opportunities for presenting criticism, are significant for the possibility of objectivity. This integrative account, in turn, serves as the basis for discussing possibilities for change, for example, improving the conditions for objectivity in community or developing an alternative "activist" science intentionally built on preferred values.

[30] The other two types Rolin describes are the argument from inductive risk and the argument from pluralism with respect to epistemic values (I have discussed the latter under the title of Kuhn-underdetermination).

2.6 Conclusion

The aim of the chapter was to show what offering a social account of science may mean, in contrast to an a-social one, and to discuss how such an account may support specific proposals about the social organisation of science.

One important cluster of social accounts that I discussed focuses on the role of social values. Accordingly, I began by outlining the value-free ideal as the a-social position that these accounts challenge. The value-free ideal relies on the possibility to distinguish the category of cognitive values that appropriately play a role in epistemic practices and to banish other values to external locations in science. I summarised a number of arguments that challenge these assumptions, showing that cognitive values are not sufficient for the task ascribed to them or are not fully independent from other values (Carrier, Lacey, and Longino) and that other values are inevitably involved in the core practices of science (Douglas).

Another cluster of socially oriented arguments shows the importance of social relations and structures in science, challenging the traditional a-social position that understands epistemic activity as individualistic. Introducing this cluster, I summarised the arguments that analyse the most general social cognitive practices (Goldman), discuss the way the social nature of science may be helpful for achieving the aims of science (Haack) or propose, more radically, that certain defining characteristics of science, such as rationality, are constituted socially (Solomon).

Arguments in both clusters may give rise to proposals about the social organisation of science, for example, in order to bring the philosophical and lay understanding of science into a better agreement with its actual functioning or to offer meliorative advice about the structures of science that have not received philosophical attention previously. Some of these proposals also offer a more radical reorganisation of the existing practices and institutions of science. However, I suggested that there is an important difference between the accounts that focus on the role of social values in science and the accounts that focus on the role of social relations and structures for the achievement of epistemic aims of science. The former open the possibility to discuss the constitution of the aims of science and the cognitive values that guide the theory acceptance, as well as the different roles social values may play in scientific judgements, to criticise them on the basis of the values involved and to propose alternative forms of science based on different values. The latter may in principle focus on the epistemic side of science alone, showing how social structures of science help to achieve cognitive aims or to realise cognitive values that are taken for granted.

As the analysis of the role of values makes the taking of the epistemic purity and independence of the aims and values of science for granted deeply problematic, I suggested that a philosophical account of science should seek to integrate different aspects of the sociality of science. I summarised Longino's account of evidential reasoning and objectivity that shows both the inevitable role of various values in science and the possibility to keep subjective value preferences in check thanks to the social nature of epistemic practices of science. Some of the meliorative proposals to which this account may give rise were discussed. Subsequently, Longino's account will serve as the basis for the proposals of my own that I make in the final chapters of the thesis.

The attention to different aspects of the social in science and their integration emerged in this chapter as another important desideratum for a philosophical proposal about the organisation of science. Besides that, the proposals discussed in this chapter reinforce the importance of political action and the relevance of the wider social and political order, already discussed in the previous chapter. They, especially Douglas's argument, also help to see two aspects of the issue that have not yet been discussed: the possibility that the lay public (persons who are neither scientists nor policy-makers) may be involved in the novel forms of organisation of science and the related possibility that research and experimentation with specific forms of this involvement (such as the know-how about consensus conferences) may be relevant for philosophy of science. These themes will feature prominently in the subsequent discussion.

Together with the previous chapter, this chapter set the stage for discussing in detail Kitcher's ambitious proposal about the social organisation of science based on an equally ambitious rethinking of the philosophical understanding of science. The next chapter analyses the development of Kitcher's account, continuing to expose connections between a philosophical account of the nature and role of the social in science and meliorative proposals about the social organisation of science. The criticism of the most recent form of Kitcher's argument is developed in chapter 4.

CHAPTER 3. KITCHER'S ACCOUNT: FROM CARVING NATURE AT THE JOINTS TO UNCOVERING UNIDENTIFIABLE OPPRESSION

3.1 Introduction

The aim of the previous two chapters was to give an overview of the possibilities to argue for particular changes in the social organisation of science on the basis of an account of the social nature of science. The aim of this chapter is to discuss in detail the development of one such argument—Kitcher's proposals for the social organisation of science. Kitcher's account is remarkable both because of the scope and ambition of the proposal Kitcher has come to defend and because of the path its development has followed. In the chapter I discuss Kitcher's three books on the general philosophy of science—*The Advancement of Science: Science Without Legend, Objectivity Without Illusions* (1995a; first published 1993), *Science, Truth, and Democracy* (2003; first published 2001) and *Science in a Democratic Society* (2011c) as different stages of this development.[31] As its discussion will show, Kitcher's account of science has undergone considerable changes over time: while certain questions, themes and approaches have retained their importance throughout, different aspects of the social in science were taken as the most prominent at different times and different proposals for the social organisation of science were made. As a result, Kitcher's books provide superb material for exploring accounts of different senses of the sociality of science and connections between a particular social account of science and the related proposal for its reform.

This chapter is thus to show how a particular account of the social in science is used to support a particular vision of organisation of science and how these accounts and proposals have been developing in parallel over time. In particular, I demonstrate how the development of Kitcher's account can be usefully approached in light of the distinctions that I discussed in the preceding chapter. I conclude the chapter with a summary of the most recent version of Kitcher's argument and the specific recommendation he makes. While this chapter does not have the aim of criticising Kitcher's account, throughout it I indicate the aspects of Kitcher's proposal that I (and sometimes other authors discussing Kitcher's ideas) consider problematic. A systematic critique of Kitcher's argument follows in the next chapter.

The chapter discusses the development of Kitcher's account chronologically, focusing almost exclusively on the three books but referring occasionally to other Kitcher's writings when they help to show the development of his ideas. Accordingly, each of the next three sections is dedicated to a particular book, describing the version of Kitcher's account of science and his proposals about the social organisation of science presented there. Each section closes with a summary of some prominent features of the given version of Kitcher's account and the changes compared to the previous versions; it also highlights the aspects of Kitcher's account that will receive critical attention in the next chapter. The final section summarises Kitcher's proposals concerning the social organisation of science as they are presented in *Science in a Democratic Society*. It is this most recent version of the proposals that I am going to discuss in the next chapter dedicated to a critical engagement with Kitcher's ideas.

[31] A number of papers collected in Gonzalez (2011b) use a similar approach, as they discuss the development of Kitcher's arguments from one book to another. The aims of these papers are varied and different from my own aim to analyse connections between the account of science and the proposal about its social organisation: Antonio Bereijo (2011) discusses the category of applied science, Antonio Diéguez (2011) writes about realism and correspondence, Wenceslao Gonzalez (2011a), who provides the most extensive overview, focuses on evolution of realism and naturalism in Kitcher's account, and Inmaculada Perdomo (2011) is interested in the issues of realism and empiricism.

3.2 The Advancement of Science

3.2.1 The context and the questions

The Advancement of Science (The Advancement in the subsequent text) makes understanding how science should be organised one of its primary tasks, directly connected with the task of understanding scientific aims and the advancement towards them—*the Advancement* of the title.

> Our primary prescriptive tasks are to give an account of the goals of science and to derive from it a theory of what constitutes progress in science, to understand how individuals ought to behave and *how their social relations should be designed* to facilitate attainment of the goals. (Kitcher 1995a, 61, italics mine)

As this design is to serve the achievement of aims of science, its discussion is preceded by an extended discussion of aims and progress. Similarly to several other accounts discussed in the previous chapters (such as Longino's), Kitcher sees his account as a response to a particular intellectual challenge. For Kitcher, it is the conflict between two unsustainable views of science: the traditional view of science—what Kitcher calls Legend—on the one hand and the challenges to this Legend on the other (Kitcher 1995a, 3–6). According to Legend, science has certain fundamental aims, such as the attainment of truth, and has been generally successful at achieving them, proceeding rationally and objectively. Critics of Legend challenge the adequacy of the picture of science Legend presents. Although Kitcher (1995a, 5) mentions "science bashers" who revolt against intellectual and political authority of science, he limits his focus to critics that attack Legend as an "unreal image of a worthy enterprise" (Kitcher 1995a, 5). They question the traditional ideas about the coherence of the aims of science, their attainability, and the notions of rationality and progress with respect to these aims. Addressing more sweeping criticisms is nevertheless seen as an important future task for which the current project is to serve as a preparatory step.

A resolution to the conflict is to be provided by combining what Kitcher calls the "commonsensical ideas that underlie Legend" and "important (and currently underappreciated) insights of logical empiricism" (Kitcher 1995a, 9) with insights of the critics and results from historical, sociological and cognitive studies of science in order to work out a defensible account of scientific progress and rationality. Such a synthesis is achieved thanks to a particular approach to science, seen an enterprise where "cognitively limited biological entities combine their efforts in a social context" (Kitcher 1995a, 9). Taking into account the social dimension of science is thus seen as crucial for a more adequate philosophical picture of science. In this sense Kitcher's account is another example of the approach that I discussed in the first chapter—the approach according to which, in order to allow a more adequate understanding of science to emerge, it is necessary for philosophy of science to take into account certain claims about science as social that are made in other disciplines.

Kitcher's aims, however, are not exhausted by a reply to the critics of Legend. He also envisages the general project of extending traditional epistemology with the help of an analysis of epistemic practices on community level and proposals for improvements there. As Kitcher writes,

> A rightly respected tradition has contributed much to one side of the meliorative epistemological project: thanks to the efforts of Locke and Hume, Kant, Whewell and Mill, Frege, Russell, and Carnap, we have a far clearer vision of good individual reasoning. The other facet of the meliorative project has been, as I have noted, almost completely neglected. Yet, just as it is important to uncover rules for the right direction of the individual mind, so too, it is necessary to understand how *community* strategies for

> advancing knowledge might be well or ill designed. (Kitcher 1995a, 389, italics in the original)[32]

The exploration of the social dimension of science would thus be necessary even if there were no need to respond to the critics of Legend. The presence of critics simply makes this need more urgent. This urgency, however, remains philosophical rather than political. On these grounds, Kitcher's project can be classified as belonging to what Martin Kusch (2004, 2–3) calls in his discussion of two strands in contemporary social epistemology the "complementary programme" of supplementing the traditional individual epistemology with a discussion of the social aspects of knowledge. Even so, my summary will show that Kitcher's account has important implications for science policy.

3.2.2 Aims, progress and rationality

As providing an account of scientific aims, progress and rationality that incorporates insights from both Legend and its critics is at the centre of Kitcher's project, the bulk of the book is dedicated to developing such an account. Ironically for a project that stresses sociological insights, the starting point for Kitcher's account of the aims of science decidedly excludes the social. Discussing aims, which he arranges along the two axes: epistemic and non-epistemic and impersonal and personal (Kitcher 1995a, 72–74), Kitcher explicitly limits his discussion to epistemic goals and epistemic progress. According to him, the set of epistemic goals is relatively narrow and thus the notion of cognitive progress is easier to tackle. The development of the notion of practical progress, on the other hand, would first require a very broad exploration of human values and aims in order to provide a "very general account of human flourishing" (Kitcher 1995a, 92). So, it is presupposed that it is possible to discuss epistemic aims of science independently from its practical aims.

The aims in question are supposed to be universal in several senses—they are aims that "all people share—or ought to share" (Kitcher 1995a, 92) and that are "independent of field and time, independent of how we think [they] might be achieved" (Kitcher 1995a, 157). Truth is the most obvious candidate for such a universal aim for science; yet according to Kitcher truth by itself is not particularly important, as it is easy to generate lots of truths lacking any interest. What is important is *significant* truth (Kitcher 1995a, 93–95). Kitcher defines significance as connected with the aim to uncover the structure of nature (if one holds a strongly realist view) or to order our—our community's—experience of nature (if one adopts a weaker position). Different aspects of the significant—significant questions, significant experimental problems, significant instruments etc.—derive their significance from this ultimate aim.

In pursuit of this main aim two varieties of progress can be made. Conceptual progress is made when scientists adjust the boundaries of categories they use—ideally, categories refer to natural kinds, their specifications are adequate and non-referring categories are removed. Kitcher uses the Platonic metaphor of "carving nature at the joints" as a shorthand for this type of epistemic advancement (Kitcher 1995a, 96, fn). Explanatory progress is made when scientists improve their understanding about dependencies between phenomena and thus about the structure of nature. In Kitcher's terms, they improve their explanatory schemata (ideal explanatory texts) by generalising and extending correct schemata, discarding incorrect ones and suggesting new schemata. To give a succinct characterisation of the aim this type of

[32] It is interesting to note parallels between this programme and Solomon's (2001) statement of aims for social empiricism discussed in chapter 1; as the subsequent discussion of *The Advancement* will show, however, there are important differences in Kitcher's and Solomon's approaches.

progress advances Kitcher refers to the Aristotelian notion of "order of being" (Kitcher 1995a, 106, fn). The aim of science consists in uncovering this order.[33]

For Kitcher's analysis of the notion of progress with respect to these aims, the notion of practice takes centre stage. Individual practice (Kitcher 1995a, 31) includes elements such as theoretical language and accepted statements about the state of the field; significant questions and schemata that are used for generating answers; judgements about proper methodology and standards; exemplary experiments and observations; judgements about others' reliability and authority, etc. These individual practices change, reflecting changes in individuals' cognitive states as a result of both a-social and social interactions—interactions with nature and with other individuals (Kitcher 1995a, 59). As Kitcher (e.g., 1995a, 166, fn) remarks, philosophers traditionally prioritise the impact of nature while sociologists focus on the social factors; Kitcher sees his account as giving both their due. Differences between individual practices may cause debates; as they are gradually resolved, all, or most, of scientists accept a particular way to modify their practice, and the modified element becomes a part of the consensus practice. The consensus practice characterises the state of the field at a given time: at its core is what all practitioners in the field accept as a part of their individual practices. The resulting change in the consensus practice is progressive if successive consensus practices improve in some, or all, respects with time (Kitcher 1995a, 90–92).

A crucial question for Kitcher's account is the rationality of these changes. He contrasts two views. According to the (Legend) rationalists, the closure of a debate happens when scientists on the winning side make changes in their practice as a result of undergoing cognitive processes that are superior to those on the losing side—the processes that are better suited for promoting cognitive progress (Kitcher 1995a, 196–197). According to the sociological critics of Legend, the closure happens when a group in community modifies its practice and is powerful enough to exclude the rivals from scientific community. Processes undergone by the ultimate victors are not cognitively superior compared to those on the losing side (Kitcher 1995a, 198).

As a solution, Kitcher proposes what he calls the "compromise model" of the closure (Kitcher 1995a, 200–201). Kitcher argues that the ultimate victory in the debate depends on the acceptance of modifications that instantiate superior cognitive processes; simultaneously, he stresses that social processes in community may play an important role in this victory. There may be no difference between the processes employed by the opposing sides at the beginning of a debate. However, in the course of the debate, as a result of a-social and social interactions, there emerge on one side of the debate processes that are better at promoting cognitive progress. Arguments are presented, challenged and defended; positions that initially seemed reasonable face difficulties; problems that initially seemed irresolvable are addressed. As a result, the arguments on one side can be reformulated in an improved form while the other side loses ground.

Social interactions are crucial for the shaping of the ultimate argument: as Kitcher stresses, "social interactions among proponents of different individual practices craft cognitive strategies that are superior to anything that underwrites the belief of any single individual" (Kitcher 1995a, 263, fn) and that might have been "cognitively impossible for the pioneering investigator who initiates that process [of debate and exchange]" (Kitcher 1995a, 290). These superior processes are embodied in the winning argument and it wins precisely because most of the community members will ultimately recognise the superiority of the

[33] Kitcher defends his realist account of scientific progress against anti-realist criticisms in ch. 5 of *The Advancement.* As I am interested in Kitcher's account of scientific aims as the starting point for the organisation of science, and its changes in subsequent books, rather than in questions of realism, I will not attempt to discuss his arguments in detail.

cognitive processes that underlie it. This model allows for rationality of science as it assigns the main power to close a debate to the rational argument, but simultaneously opens the possibility to explore how various social factors also play a constructive role in scientific change.

Kitcher's account of progress and rationality is thus social in the sense of recognising their social aspects on several levels. First, the contribution of social interactions to changes in individual practices is acknowledged and so social practices are recognised as epistemically significant. Second, the consensus practice is understood as socially emerging with the help of interactions between individual practices; thus, the importance of collective and interactive nature of science is recognised. This is one of the aspects of Kitcher's account that Goldman (2002, 193) stresses when discussing Kitcher's account as an example of social epistemology. An even stronger claim about science as social emerges from Kitcher's discussion of a superior cognitive process becoming possible thanks to social interactions. In social interactions, individuals achieve what might be unachievable for each of them in isolation. So, the possibility of rational change in science depends to an important degree on the collective character of science.

Kitcher, however, argues that the role of the social in the advancement of science is more limited than some critics of Legend maintain. In particular, he attempts to show that underdetermination does not threaten the possibility to develop theories that are in important sense determined by inputs from nature, by data, rather than by social factors. One form of underdetermination Kitcher (1995a, 160–169) argues against is the position that changes in the consensus practice are fully determined by social factors and nature cannot provide any input. Kitcher argues that in order to prove that, the demonstration of complex social interactions involved in the acceptance of a claim (as uncovered, for example, by the laboratory studies of Bruno Latour and Steve Woolgar) is not sufficient. One also has to demonstrate that the result would have been the same no matter what encounters with nature. To challenge this possibility, Kitcher offers a thought experiment: several groups of scientists, identical socially, are exposed to different inputs from nature (for example, they unknowingly study samples with different structures). Would all the groups conclude that the sample has the same structure, as the thesis of determination by social factors seems to presume? Kitcher suggests that we have a strong intuition (supported by daily experience of human cognitive behaviour in different situations) that they would not: there would be different results and the results would be correlated with the structure of the sample.

As another challenge, Kitcher (1995a, 247–263) discusses the two forms of underdetermination associated with the name of Pierre Duhem. According to the first of them, there are endlessly many possible alternatives to any hypothesis. Kitcher argues against this possibility by pointing out that the existing consensus practice severely limits the number of hypotheses taken seriously—in fact, it may be difficult to propose even one. Constraints provided by the existing practice also figure in Kitcher's reply to the second Duhemian claim—the claim that in the case of a contradiction between evidence and a hypothesis, any of the elements involved (the hypothesis, the reports about evidence, or the relevant auxiliary hypotheses) can be revised or given up. Kitcher's strategy is to argue that in many cases the epistemic cost of revising background assumptions would be higher than that of giving up the hypothesis. Giving up an assumption, one would have to give up the claims in the justification of which the assumption is involved and the reasoning behind accepting the assumption in the first place. This acceptance, in turn, might have involved various explanatory dependencies applied elsewhere; they would have to be given up as well. Putting the opponents in the situation where they face an inconsistency and all possibilities to eliminate it are too costly is in fact an important move in the development of a scientific controversy that Kitcher describes with his compromise model. Kitcher concludes that these considerations limit the possibility that (persistent) underdetermination is ubiquitous or even common.[34]

The recognition of the importance of some social features of science coexists in Kitcher's account with the denial that science may be social in several other senses—the possibility that social values may play a role is mostly excluded. As described before, in Kitcher's account epistemic aims of science are pre-given by nature—there are the joints to cut at and the dependencies to uncover. As a result, his account does not allow social values to play any role in formulating the aims or in constituting the object of inquiry. There is also no role for values in core scientific practices as Kitcher opposes the thesis of underdetermination and does not discuss inductive risk. This character of the social in Kitcher's account, including what is barred from his account of the epistemically significant social aspects of science, in turn, influences his proposals about the social organisation of science.

3.2.3 The social organisation of science

Progress towards epistemic aims happens thanks to the gradual absorption of changes into the consensus practice of science. Accordingly, factors that may influence this process acquire epistemic significance and a task for social epistemology is to analyse how this process could be best organised in community. A task for philosophy of science is

> to identify the properties of epistemically well-designed social systems, that is, to specify the conditions under which a group of individuals … succeed, through their interactions, in generating a progressive sequence of consensus practices. According to this conception, social structures are viewed as relations among individuals: thus [the] departure from the tradition of epistemological theorising remains relatively conservative. (Kitcher 1995a, 303)

The question is thus to understand which states of community (for example, what kind of division of effort between two hypotheses) are well-suited for the advancement of epistemic aims of community and to analyse how community can be brought closer to the desirable states.

More specifically, Kitcher's analyses focus on the questions of trust, authority and division of cognitive effort (Kitcher 1995a, ch. 8). Trust benefits individuals. Relying on others' work or accepting others' judgements as authoritative may help scientists to complete the epistemic projects on which they are working quicker or to complete projects for which one could not possibly find all the relevant information on one's own. Division of cognitive labour is beneficial from the point of view of community. Distributing efforts between several hypotheses or methods or distributing efforts in such a way that some scientists attempt to replicate challenging innovative results and others do not may in many cases be the best strategy for ensuring that community will achieve its goals and will achieve them quicker (community is "hedging the bets"). Kitcher's compromise model of debate closure provides another reason why a distribution of effort is more beneficial for community than the early uniformity. As the ultimate argument is shaped in the process of interactions between scientists supporting rival views, it is important that there are scientists who refine views on both sides of the debate.[35]

For the division of cognitive labour, cognitive diversity—variety on different levels of scientists' individual practices—can in turn be beneficial. In particular, Kitcher suggests that what Legend may consider an epistemic failure on the part of the individual—the failure to be

[34] It is important to note that Kitcher's understanding of underdetermination is considerably different from the one discussed by Longino. I will return to the discussion of these differences in chapter 5.

[35] As Kitcher notes, this is not a novel point—the origins of this positions may be traced at least to Mill's *On Liberty*; more recently it is an important point for Kuhn and Feyerabend (Kitcher 1995a, 344, fn).

motivated by epistemic aims only and the susceptibility to factors such as reliance on authority, adherence to tradition, competitiveness and desire for credit—do not necessarily hinder community's cognitive progress. The formalised analyses of scientists' decision-making that Kitcher develops using the tools of the Bayesian decision theory, microeconomics and population biology show how such non-epistemic factors may play a positive role in maintaining cognitive diversity and division of cognitive labour (Kitcher 1995a, 344–389). The main conclusion of Kitcher's analyses is that in some cases scientists pursuing personal non-epistemic aims (e.g., credit), or "epistemically sullied" agents as Kitcher calls them, can achieve a distribution of effort that is closer to the optimum than that of scientists motivated by purely epistemic aims. All "epistemically pure" scientists would only be interested in achieving truth and thus will tend towards homogeneity, choosing to work on theories that seem to be more promising or making decisions about replication in the same way. "Epistemically sullied", on the other hand, would be less uniform when making decisions of this kind and may choose a variety of alternatives. So, they may choose approaches that seem to have a smaller chance of achieving truth, motivated by the higher chances to be the first to succeed and gain credit on a less trodden path. As Kitcher concludes, it is possible that "social structures within the scientific community can work to the advantage of the community epistemic projects by exploiting the personal motives of individuals" (Kitcher 1995a, 357).

This account, in turn, opens a possibility for society to play a role in organising science to help the achievement of its epistemic aims, as institutional frameworks and incentives that may motivate scientists are open to shaping by the means of science policy. Accordingly, the kind of detailed analysis that Kitcher proposes could be the basis for making changes—for example, adjusting the system of allocating funding and credit—in such a way that the decisions individual scientists make would be more likely to lead to the optimal distribution of research effort in pursuit of epistemic aims of science. Kitcher is cautious when discussing such possibility—he warns that his analyses are too idealised to be directly applicable in practice. Yet Kitcher finds it possible that philosophy of science may be able to provide specific recommendations for organising science (Kitcher 1995a, 305).

3.2.4 Taking stock

Kitcher's analyses of the final chapter are meant to demonstrate the importance of certain features of science—for example, the distribution of labour in community—that exist on the level of community. Thus, they contribute to the appreciation of the social aspects of science. Moreover, an important conclusion of Kitcher's is that the way the optimal distribution emerges on the community level may differ from what epistemology would traditionally prescribe to the individual. Kitcher has demonstrated that "epistemically pure" scientists are likely to achieve a worse distribution of labour than "epistemically sullied". So, following impeccably the prescriptions of individualistic epistemology does not lead automatically to what would be the ideal epistemic state for a community.[36] This argument opens the possibility to develop a social account that departs further from traditional individualistic epistemology instead of complementing it—for example, an account like Solomon's, where individual scientists' and scientific community's rationality are decoupled. Yet, Kitcher himself sees his results as supporting his account of rational development of individual and consensus practices. He takes them to confirm his point that a mere demonstration of the social aspects of scientific communities does not exclude the possibility of rationality and

[36] Kitcher makes this point already in 1990: when presenting an earlier version of the argument about the division of cognitive labour, Kitcher (1990, 6) remarks that the conflict between the demands of individual and collective rationality is an important and neglected problem.

objectivity of science or "epistemically virtuous individual reasoning" (Kitcher 1995a, 388). Kitcher thus chooses to stress the continuity with individualistic approach.

This restriction on the extent of "socialising" can be seen as reflecting the central role of the individual in Kitcher's account. As noted previously, Kitcher understands the social as social relations rather than as social values and this understanding is rather severely restricted. Discussing his proposals for the organisation of science Kitcher fully reduces the social to relations between independent individuals. Even in discussion of variation among scientists that plays an important role for Kitcher's analyses (and Kitcher claims that "celebrating human cognitive variation" (Kitcher 1995a, 68) is a distinguishing feature of his approach), factors like ideologies, social values and social aims are excluded from specific game-theoretical arguments. (So, while there are some similarities with Solomon's idea of the social emergence of rationality as a result of individual choices, Kitcher discusses a much narrower range of factors than Solomon does.[37]) Individuals in Kitcher's account are individualistic indeed: there is no discussion that they may be shaped by social communities to which they belong or the values of their society.

This conception of sociality has provoked some comments among Kitcher's reviewers. For example, Hacking (1994, 214) remarks that Kitcher's model of interacting independent individuals may be "too conservative for [contemporary social epistemologists'] tastes"; and Jarrett Leplin (1994) makes a similar point. D. Wade Hands (1995 and 1997) discusses the understanding of the social as the sum of individuals as one of the crucial issues when using economics as a resource for philosophy of science. Such an understanding brings into philosophy the problems of the individualistic economic approach that does not allow explaining the emergence of the social from the individual (Hands 1995) or accounting for the social that is qualitatively different from the individual (Hands 1997). Philip Mirowski (1995 and 1996) also comments on Kitcher's individualistic stance and discusses the relation of Kitcher's project to the developments in neoclassical economics and the important problems they have run into. So, there may be doubts whether Kitcher's conception of the social enables to provide an adequate account of the social dimensions of science and to give their due to insights of Legend's critics.

From the point of view of my aim to analyse the development of Kitcher's account over time, the most important consequence of this approach to the sociality of science concerns the possibility to address certain questions Kitcher intends to address in future.[38] The reorganisation of science that Kitcher proposes in *The Advancement* focuses exclusively on the epistemic interests of science. Kitcher describes the issues with which he is concerned as "those that would face a philosopher-monarch, interested in organising the scientific work force so as to promote the collective achievement of significant truth" (Kitcher 1995a, 305). At the same time, Kitcher introduces his account as a preliminary step for the larger project of addressing most radical criticisms against science. Ultimately, the aim is to provide an account of science as a part of a general account of human flourishing—the task that Kitcher

[37] Kitcher suggests that his account and the accounts that focus on psychological and cognitive variation, such as Solomon's, could be seen as complementary (Kitcher 1995a, 345, fn). Solomon (1995), however, criticises Kitcher's account as inadequate; according to her, improving it would require giving up the conception of individual rationality—something that, as I have stressed, Kitcher is unwilling to do.

[38] Kitcher (1995b) makes it clear that an adequate account of science is also meant to provide the basis for critical evaluation of "socially consequential issues" (Kitcher 1995b, 654) in scientific research—criticism of the kind Kitcher has already presented for creationism (Kitcher 1996, first published in 1982) and sociobiology (Kitcher 1985). This aspect of *The Advancement* can thus be said to be retrospective. I am mostly interested in the possibility to fulfil the future promise of exploring the general theme of human flourishing on the basis of *The Advancement*.

elsewhere calls "*the* issue for a critical philosophy of science" (Kitcher 1995c, 617, italics in the original). In the concluding part of the book he envisages this future project:

> To claim, as I have done, that the sciences achieve certain epistemic goals that we rightly prize is not enough—for the practice of science might be disadvantageous to human well-being in more direct, practical ways. A convincing account of practical progress will depend ultimately on articulating an ideal of human flourishing against which we can appraise various strategies for doing science. [...] Given an ideal of human flourishing, how should we pursue our collective investigation of nature? (Kitcher 1995a, 391)

In the preceding chapter I suggested that accounts that understand the social in the sense of social relations and social structures are especially well suited for supporting proposals that help the achievement of the epistemic aims of science. The possibility to discuss the aims themselves, in particular, their possible value preconditions or value implications, may at the same time remain mostly out of the picture. Given the exclusive focus of Kitcher's *Advancement* on the social structures of scientific community, I suggest that Kitcher's approach does not provide good grounds for discussing questions related to values, interests and non-epistemic aims.[39] In particular, it makes it difficult to propose value-motivated changes to the organisation of science that would not be seen as an imposition of essentially foreign considerations on the epistemically significant enterprise. Accordingly, if challenges related to practical aims and interests are to take centre stage, opening a possibility for discussing them may require giving up some of the elements of *The Advancement.*

3.3 Science, Truth, and Democracy

3.3.1 The context and the questions

Kitcher's *Science, Truth, and Democracy* opens with an imaginary conversation (Kitcher 2003, xi). Kitcher, a philosopher of science, introduces himself to a non-philosopher. The interlocutor responds by listing some questions that, according to the interlocutor's commonsensical understanding, philosophy of science addresses, such as the impact of scientific research on society's values and the role of science in democratic society. Kitcher the philosopher acknowledges importance and urgency of these questions but is forced to admit that they have mostly been neglected.[40] The aim of Kitcher's book is to provide a philosophical account that will enable to address these neglected issues—the questions about science and values in democratic society. Explaining his path to this project, Kitcher attributes a crucial role in the rethinking of his earlier account to the issues raised by the application of scientific results.[41] The idea that the philosophical account of science can separate the

[39] Steve Fuller (1994) makes a somewhat similar point arguing that Kitcher's economics-inspired approach makes his account blind—just as many economics accounts are—to the larger social context that provides the enabling conditions for the "market". Fuller is concerned with what he sees as an inadequacy of *The Advancement*; I am ready to accept that it fulfils its clearly delineated aims. I suggest, however, that due to certain aspects of Kitcher's approach, *The Advancement* cannot serve as the basis for the envisaged future account of human flourishing.

[40] In a sense, this vignette reads as a reversal of Laudan's (1984) introduction where the philosopher acknowledges that the title *Science and Values* creates certain expectations in the reader but refuses to go along with them. An earlier use of a similar trope—arguing that philosophy of science disappoints certain legitimate expectations—is made in Kitcher and Cartwright (1996).

[41] Kitcher's interest in the Human Genome Project and its social and ethical consequences has played an important role in this development; Kitcher references his book on the societal implications of human genetics *Lives to Come* (Kitcher 1997b; first published 1996) as the predecessor of the position he presents in *Science, Truth, and Democracy* (Kitcher 2003, 203). Some important issues, including the mismatch between the project's genuine epistemic significance and the way it is presented to gain popular support, were already formulated in Kitcher (1995d). As noted earlier, *The Advancement* is also seen by Kitcher as providing the basis for critically engaging with socially consequential research programmes; however, as my summary will show, Kitcher's approach has considerably changed by the time of *Science, Truth, and Democracy*.

achievement of epistemic aims from the issue of practical aims and practical consequences of research is thus decidedly rejected.

The new approach still has important similarities with that of *The Advancement*, as Kitcher sets the familiar aim to overcome the confrontation between those celebrating science and those attacking it, and to provide a more adequate account of science by combining insights from both sides. According to the side that celebrates science, “the faithful” as Kitcher (2003, 3) calls them, science provides true knowledge of nature. Moral values may influence ethical constraints on research; technological applications may be subject to moral evaluation. Yet, two crucial aspects of scientific inquiry, the formulation of research questions and judgements about the evidence for conclusions, can, and should, be free from the influence of moral, social and political considerations. The knowledge achieved has an intrinsic value that overrides many, if not all, other values. The “debunkers” (Kitcher 2003, 4) consider such a picture of pure science a myth, question the ability of science to uncover truth and see science as serving the interests of political elites. According to debunkers, social values enter inevitably in the process of both formulating research aims and appraising evidence.

Addressing this conflict, Kitcher widens considerably what is expected from the resulting account of science. It is not just to provide a more adequate picture of science but to “articulate a picture of the aims and accomplishments of the sciences so that the moral and social questions can be brought into clearer focus” (Kitcher 2003, xii). The aim is to be ultimately able to account for the role of science in democratic society. This task, in turn, requires some important changes in the account of science that Kitcher defends.

3.3.2 Scientific aims and progress

Many important elements of Kitcher’s previous account are retained in the updated version, as it remains mostly realist and rationalist. According to Kitcher, a considerable part of criticism against science focuses on the belief that science delivers truths. He opposes this line of criticism and defends a form of “modest” (Kitcher 2003, 16, fn) realism, according to which science is able to, and does, disclose truths about nature. Kitcher sees this position as an extension of common intuitions about human relations with the environment. When pursuing their aims, humans interact with objects that exist independently of them; in the process, they form representations about these mind-independent objects. The accuracy of representations can be estimated by evaluating how well humans achieve their aims when relying on these representations. Accurate representations get constituents of the world and their relations right and this is what makes systematic success when acting in the world possible (Kitcher 2003, 11–12 and 24–28).[42]

Besides the generally realist position that maintains a continuity with the account in *The Advancement*, Kitcher continues to argue against the possibility of *permanent* (not resolvable by any amount of additional evidence) and *global* (ubiquitous) underdetermination; temporary and resolvable *transient* underdetermination is acknowledged but deemed non-threatening (Kitcher 2003, ch. 3). Kitcher claims that examples of underdetermination used in philosophical arguments either focus on very unusual real cases or rely on “philosophical devices” (Kitcher 2003, 36), such as the variations on the problem of generalising induction, that cannot generate alternatives that would be taken seriously in scientific practice. In practice, there are no numerous cases of alternative, equally confirmed theories. Rather, the hypotheses that would account for all existing data and successful predictions and interventions are often not available at all. In addition, Kitcher summarises one of the long

[42] Kitcher also repeats many of the responses to anti-realist criticisms developed in greater detail in *The Advancement.* Again, I will not attempt to discuss them.

historical arguments of *The Advancement* (the victory of Lavoisier over the proponents of the phlogiston theory) in order to argue that initially plausible alternatives can be eliminated by showing how they run into inconsistencies, the resolving of which is associated with unacceptable epistemic losses. The same story also serves as a reintroduction of Kitcher's ("compromise" but ultimately rationalist) model of the closure of a debate, where the cognitively superior winning argument emerges as a result of social processes.

This realist and rationalist position, however, is now seen as compatible with the rejection of the notion of natural boundaries and structures in nature. The belief in natural boundaries is an unsustainable belief of the "faithful". Kitcher himself now argues that the "picture of science as providing objective knowledge does not entail that there is some unique, context-independent goal toward which inquiry aims" (Kitcher 2003, xii). There is no "agenda set for our inquiries by nature"; rather, there is "a place for human values and human interests in the constitution of the goals of the sciences" (Kitcher 2003, 44). This is a radical departure from *The Advancement*.

Kitcher replaces the image of objective joints in nature with the metaphor of sculpturing or map-making (Kitcher 2003, 44–48 and ch. 5). Both have a connection with mind-independent reality: the sculptor works with a block of stone; the map represents a terrain. Yet there are endless ways of making a statue. In the same way, there are endlessly many ways of drawing boundaries around natural objects, conceptualising events and processes and making classifications on the basis of different criteria—endlessly many languages, so to say. Truths expressed in one of the languages can be also expressed in others—according to Kitcher, all truths in all possible languages are consistent. Therefore, pluralism does not threaten Kitcher's realism. However, Kitcher rejects the notion that either the natural language or the scientific classification captures the actual divisions in nature. The divisions given by a language seem natural because they correspond to its creators' cognitive capabilities and interests. Given different interests, different classifications are suitable, and it is actually the case for many concepts in sciences (such as the species). Kitcher argues that even in the case of scientific concepts that seem to admit no alternatives because they uncover underlying fundamental features of things (such as the chemical element), human interests play an important role. It is the human interest in certain visible aspects of things that has prompted search for their systematic explanation.[43]

The role of human interests is stressed by the metaphor of map-making as well. It is still possible to talk about the accuracy of maps and of the progress in terms of the increasing accuracy. In this respect, the demonstration of the role of human interests is compatible with realism that Kitcher advocates. Yet there are endlessly many ways to map a region of the world, depending on the aims and interests of the users that dictate what aspects of reality are relevant and what degree of detail and accuracy is required. These interests and aims change with time, and so must maps. Given the variety of aims and interests, there is no ultimate map. Kitcher claims that science focuses on the issues that are important for a particular society at a particular time, just like map-making does. As a result, the aims of science evolve

[43] Kitcher (2003, 206) cites his discussions of the concept of species (Kitcher 1984b and 1989) (one could also add Kitcher 1984a) as an earlier expression of his pluralist position. Kitcher's paper on the concept of gene (Kitcher 1982) similarly defends a plurality of ways to conceptualise the gene that follows from the plurality of geneticists' interests in various aspects of their object of study. None of these concepts can be chosen as *the* natural one: "Depending on one's interests, there are *various natural ways* to segment the chromosomes" (Kitcher 1982, 356, italics mine). The account of *Science, Truth, and Democracy* can thus be seen as a generalisation (to cover the entire science) of the account first developed for particular concepts in particular sciences; in his exchange with Longino on the issue of pluralism Kitcher (2002a, 571, fn) himself stresses this continuity. The contrast with the position presented in *The Advancement* is nevertheless striking.

and change. There is no grand context-independent aim for science, just as there is no aim to create one ideal atlas.

In addition to the metaphors, Kitcher (2003, 66–76) also presents an argument against attempts to argue for some universal context-independent aim for science. He discusses several approaches stemming from the idea that science aims to provide a systematic basis for giving explanations (understood in terms of providing general laws or exposing general causes) or uncovering the place of different phenomena in the unified picture of the world. Similarly to his argument about the plurality of classifications in science, Kitcher points out the plurality and varied nature of laws and explanations. Many of significant achievements in different sciences cannot be easily incorporated into a unified framework. For example, contra the Unity of Science view, many significant results in biology cannot be subsumed under more general chemical and ultimately physical laws; neither are concepts used in biology necessarily translatable into physico-chemical terms. If there is no such unified picture, ordering laws or truths that can play a role in explanations becomes problematic. There may be endlessly many local systems of laws and endlessly many truths involved in giving explanations. Unless one brings into picture human interests (with their context-dependent and evolving nature), it is impossible to explain why some elements on these lists are picked up as significant.

After having rejected several common accounts of context-independent scientific significance, Kitcher suggests (Kitcher 2003, 76–82) that significance should be understood as context-dependent and changing. There is no single hierarchy where significance would derive from the most general questions about the fundamental structure of nature. Instead, there is a web of interconnections where significance is related to particular human interests, both pragmatic and expressing "natural curiosity", and concerns about particular entities, processes and regions of nature. Different questions, statements, instruments etc. receive their significance from their connection with other elements of this web that Kitcher proposes to represent with the help of "significance graphs" (Kitcher 2003, 78).[44]

The intertwining of curiosity-driven and practical interests in judgements of significance, in turn, means that epistemic aims of science cannot be separated from non-epistemic, practical ones. Some highly esoteric piece of scientific work may be shown to derive its significance from a present or past practical project; some project started in the spirit of pure curiosity may become relevant for future practical projects. The epistemically significant projects today are partially shaped by projects and judgements of the past: the projects undertaken modify the physical environment that researchers explore and make particular concerns and questions salient. The previous discussion of classifying and map-making suggests the same conclusion. The ways humans draw boundaries and categorise have consequences for the environment and society. These categories become embedded in particular practices, organisational forms, artefacts and visions that open or block certain possibilities for human action (Kitcher 2003, 51–53).

This view destroys the kind of double immunity that is granted to science by the traditional "faithful" account. As scientific aims are no longer seen as context-independent, they cannot be treated as immune to social values; as pure science is no longer seen as separate from pursuit of practical projects, its social consequences cannot escape social appraisal. As Kitcher (2003, 65) summarises his view,

> An allegedly context-independent notion of epistemic significance insulates science, or "basic science" at least, from social and moral values, by claiming that the achievement of epistemically significant truth is valuable in principle... . Because I believe no such conception can be found, I take moral and social values to be intrinsic to the practice of the sciences.

[44] Kitcher (2003, 207) refers to Kitcher (1999) when discussing his new approach to scientific significance.

This, in turn, opens the possibility to discuss science from the point of view of values it advances and consequences it brings; social values can be discussed as an intrinsic part of science.

3.3.3 The social organisation of science

In *The Advancement*, the aim of the meliorative project was to facilitate the achievement of epistemic aims of science. In *Science, Truth, and Democracy*, reflecting a novel orientation, the organisation of science is to be assessed on a wider basis than its ability to achieve truths, or even significant truths, effectively: "To assess the proper functioning of scientific inquiry we must consider if collective research is organised in a way to promote our collective values in the most encompassing sense" (Kitcher 2003, 111). Kitcher is convinced that the current practice of science fails to promote those collective values of democratic society (Kitcher 2003, 108). So, the changes Kitcher proposes for reorganising science are considerable.

Kitcher points out that attempting to extend the game-theoretical approach of *The Advancement* would be highly problematic. A formal analysis of the social institutions that, given the motivations of the actors, are best suited for the promotion of certain aims may be possible in the case of a local problem but becomes overwhelmingly complex if one attempts to extend it across a wider selection of aims in various social contexts (see Kitcher 2003, 113–114 for a discussion). Elsewhere, discussing his models in comparison with models in biology, Kitcher stresses the reasonableness of using specific models and the absurdity of universalising them:

> That would be like trying to achieve some characterisation of the inorganic physical environment and then constructing a theoretical ecology that would identify types of organisms with maximal reproductive success. (Kitcher 2002b, 267; Kitcher (2002d) expresses a similar position).

This is not what biologists do. In developing models of science Kitcher proposes to follow suit and to limit the use of formal models to particular problem contexts.

Accordingly, in order to discuss the general question of science and democracy in *Science, Truth, and Democracy*, Kitcher has to develop a different approach for discussing the aims that science should have and the ways to organise it in order to achieve these aims. Kitcher suggests a model that starts with individual preferences about what is good and valuable and then shows how in democratic society individual preferences can be discussed and aggregated into a statement about the collective good that science should promote (Kitcher 2003, 116). The notion of collective good thus relies on aggregating and balancing personal preferences.

Kitcher's vision of science pursuing this collective good is described in his "well-ordered science" (Kitcher 2003, 117–123). Inquiry is well-ordered, if science policy decisions mimic the results that would be achieved by an idealised group of deliberators. The decisions to be made in this process concern three aspects of scientific practice: the choice of research projects, the moral constraints on research and the application of scientific results.

In the course of deliberation, a representative group first discuss individual preferences concerning scientific projects that should be pursued. As the first step, participants acquire additional information concerning available research options and their epistemic and practical significance. As a result of this "tutoring", their initial preferences are modified. As the second step, participants become familiar with each other's preferences and preferences of outsiders (persons who do not belong to democratic societies). They are expected to approach each other's wishes sympathetically and to leave no interests unfairly ignored. As a result of familiarity with others' needs and interests, further modifications happen in each participant's list of preferences. On the basis of these twice-adjusted preferences the list of common preferences and their relative weights is formulated (through consensus formation or, if failed, voting) in a way that reflects interests and values of all relevant parties.

During the next stage, scientific experts (expected to be objective and disinterested) estimate the probabilities of achieving desired outcomes by pursuing particular research projects. The "disinterested arbitrator" uses these probabilities and the list of preferences to propose what resources each line of inquiry should receive, given a particular overall budget for inquiry. The deliberators make the final decision concerning the budget and its distribution between projects on the basis of this proposal. The aim of this stage is to set agendas for research in a way that maximises the chances of achieving the collectively agreed-upon aims, given the experts' estimations of probabilities of success. At the same time, ideal deliberators decide upon the moral limitations on research on the basis of a sympathetic discussion of various rights to be taken into account and appropriate constraints for defending these rights. This is the second aspect of inquiry where deliberators' input is required.

When it comes to the third stage, the question of application, the procedure of the first stage is repeated. On the basis of the updated information and changes in scientific significance, the list of preferences may be revised and probabilities of achieving them updated. Taking this into account, "the arbitrator" proposes ways of realising different policies, given a particular budget, and deliberators make the decision about expenses to make and policies to pursue.

Well-ordered science thus suggests the involvement of the public and the employment of democratic procedure at several crucial stages of decision-making in science policy. Scientific experts, however, continue to play an important role, as they tutor the deliberators and estimate the chances of achieving specific aims by pursuing specific projects.

Kitcher does not discuss in detail how the model of well-organised science is to be implemented in practice and does not set the aim to do so in the first place. Instead, Kitcher characterises his work as a "first shot" (Kitcher 2003, 146) indicating the areas others could later explore. The model of well-ordered science is meant to help this exploration by outlining the ideal to use as the basis when planning institutions that would deliver decisions coinciding with those achieved in the process of ideal deliberation. The notion of "*coincid[ing]*" (Kitcher 2003, 122, italics in the original) or "match[ing] the outcomes" (Kitcher 2003, 123) is crucial here. Kitcher stresses that his model does not require the actual realisation of deliberative processes in practice, as actual public discussion is likely to be costly and to fall short of the desired result. As Kitcher writes, "there's no thought that well-ordered science must *actually institute* the complicated discussions [Kitcher has] envisaged" and instead "the challenge is to find institutions that generate roughly the right results" (Kitcher 2003, 123, italics in the original).

Besides this guiding role, well-ordered science can serve as an aid for diagnosing problems of the current organisation of science and judging the adequacy of various aspects of science policy, such as planning, funding and ethical oversight. Kitcher discusses several ways in which the state of science in contemporary democracies deviates from well-orderedness (Kitcher 2003, 126–133). Currently, the course of inquiry is decided by non-systematic interactions between the majority's untutored preferences, interests of funding sources and interests of scientists (and neither of the latter two groups represents the entire population). As a result, preferences of underprivileged groups are likely to be systematically ignored (Inadequate Representation),[45] genuinely significant questions may be neglected in

[45] Kitcher (2003, 128) remarks that the optimist may believe in "invisible hand" when it comes to addressing unrepresented interests; Kitcher argues, however, that there are grounds for suspecting its systematic failure. This is remarkable because, while Kitcher himself does not use the notion of invisible hand with respect to his account in *The Advancement* (and later objects (Kitcher 2000b) to such a reading), many of his reviewers do—

favour of untutored preferences (Tyranny of the Ignorant), genuinely valuable research projects are likely to be presented in a misleading way that appeals to the untutored preferences (False Consciousness), and even for genuinely defensible preferences a sub-optimal line of research may be selected (Parochial Application). The ideal of well-ordered science thus can serve for identifying locations where a change is most needed.

Well-ordered science also provides a guiding ideal for individuals who, according to Kitcher (2003, 192–197) have the duty to act in a way that helps to make current scientific practice closer to well-orderedness. It is with advice to the individual scientist—to fight the problem of False Consciousness, to improve the public understanding of science, to work with groups that are likely to be disproportionally affected by particular research projects and, as a last resort, to abandon problematic projects—that Kitcher's book closes.

3.3.4 Taking stock

The account developed in *Science, Truth, and Democracy* does not invalidate the insights about the role of the social in science developed in *The Advancement*.[46] However, devising the social system to enable the optimal distribution of research effort now has a very modest place in Kitcher's proposal next to the more general project of organising science to make it compatible with democratic values.

The possibility to address those value questions demands two important changes compared to the approach developed in *The Advancement.* First, the achievement of epistemic aims is no longer seen as separable from the discussion of its consequences in the world. Both Kitcher's argumentation and his choice of metaphors are intended to demonstrate that the world is changed in very material and lasting ways due to the choice of particular research directions, classifications and courses of inquiry. Accordingly, these choices should be subject to moral appraisal. Second, the understanding of the nature of the epistemically significant towards which inquiry strives is now fully revised. Significant aims are not pre-given by nature. Instead, they can be traced back to human interests and they develop and change as those interests develop and as the situation changes as a result of the previous choices. There is no epistemic significance beyond this context-dependent and interest- and value-laden notion of significance. The introduction of interests and needs into the picture of science enables a richer conception of individual, compared to the severely limited individualistic conception of *The Advancement,* and simultaneously opens the possibility for reorganisation of science on a wider basis.

The proposal Kitcher makes, his model of well-ordered science, requires the involvement of deliberative procedure in three locations in science: the setting of research aims, the choice of moral limitations on research and the decisions about the application of research. The role of social values in these locations is a traditional theme—as I showed in the second chapter, the acceptance of this role for values may be compatible with the position that the core practices of science are value-free. In one sense, Kitcher's ambitious proposal for the reorganisation of science has important similarities with a very traditional "external" view of the role of values in science. Kitcher's approach, however, goes beyond the purely external view, as Kitcher's account of epistemic significance shows the role of interests and values in what might be considered purely epistemic projects.

---

e.g., Fuller (1994); Hands (1995 and 1997); Matheson (1996); Mirowski (1995; 1996 and 2004). Again, an element of earlier Kitcher's approach has to be given up when it comes to wider social aims.

[46] For example, *Science, Truth, and Democracy*, Kitcher (2003, 111–114) contains Kitcher's argument that the situation where individuals choose different options for a variety of personal non-epistemic reasons may often be epistemically preferable to the situation where individuals follow the prescriptions of individualistic methodology and all choose the same option—one of the conclusions about the division of cognitive labour in *The Advancement.*

Discussing different possibilities to ground a proposal for the organisation of science in a philosophical account of science, I have argued that there are important differences between accounts focusing on the role of social values and those focusing on the role of social relations and structures. Kitcher's account of *The Advancement* belongs to the latter group and I have suggested that addressing certain questions related to the role of values would require changes in Kitcher's approach to science. One could say that the account of *Science, Truth, and Democracy* makes these changes. In one sense Kitcher's new approach represents a considerable departure from the value-free view of science that *The Advancement* supports; it can be classified as an account that focuses on the social in the sense of social values.

The role of these values, however, remains in some important respects limited—for example, there is no serious problem of underdetermination or uncertainty for Kitcher. Kitcher's account is also different from many discussions of the role of social values in science, as the primary focus is on the wishes, or interests, rather than values as such or value-laden assumptions and perspectives. Neither do these wishes reign supreme: as they are subject to tutoring, experts retain a crucial role at the stage of aims-setting. Some aspects of planning, such as the estimation of probabilities of success, not to mention the inquiry proper, remain fully theirs. This limited idea of what the public can contribute to science and science policy will be at the centre of my critique.

Another idiosyncratic aspect of Kitcher's approach is the possibility that the organisation of science could take the public's wishes into account without actually instituting public participation in science policy. As noted before, Kitcher stresses that well-ordered science does not require an actual society-wide conversation; the aim is to create institutions that would deliver the right results. It is thus conceivable that the right results can be approximated without actually involving the public in science policy making at all. In terms of Mark Brown's (2004), who makes it one of the central themes in his discussion of well-ordered science, Kitcher is concerned with the "substantive outcomes" of science policy rather than the "procedures" of its making (Mark Brown 2004, 82).[47] This, in turn, raises important question about the democratic character of Kitcher's proposal. For example, Longino (2002c, 566, fn), who also uses the notions of outcome and procedure, warns that the focus on right outcomes may smuggle in elitism. The possibility is especially disturbing because there seems to be no way to offer a principled objection to it. Given the focus on outcomes in well-ordered science, it is conceivable that, as Jeroen Van Bouwel remarks, "at least in principle, it might be accomplished by a central planner giving orders to every scientist" (Van Bouwel 2012, 45).

Finally, another remarkable aspect of Kitcher's account is the very general character of the social organisation he proposes. Kitcher's aim is to develop an approach that would ensure harmony between scientific aims and collective good in democratic society. The possibility of such a generalised solution may raise questions. For example, in her critical review, Jasanoff (2004) argues that Kitcher's approach suffers from the problems of "symmetry", or the failure to recognise that democracy is as complex and multi-faceted as science in Kitcher's analysis, and "singularity", or the failure to take into account the impossibility to create a single form of organisation for the relations between science and democracy.

In response, one could point out that Kitcher's self-proclaimed aim is to provide a very general outline of the ideal without attempting to specify ways to realise it; a richer notion of democracy may follow later. Clarifying his position in response to Longino's (2002c) critical review, Kitcher (2002a, 569–570) states that "[w]ell-ordered science is intended as an ideal", that working out the way to realise it "requires a significant body of

[47] I return to Mark Brown's arguments later, when questioning the democratic character of Kitcher's proposal.

empirical knowledge" and that by describing the ideal and the failures of current science policy Kitcher attempts to "invite a collaboration between philosophy of science and the social sciences" to analyse how to move science policy closer to the ideal.[48]

While my argument will primarily focus on the themes of expertise and public participation in Kitcher's proposal and the consequences of his approach to them for the realisation of the aims of well-ordered science, the question of the generality of the model and its practical application will also play an important role in the subsequent discussion. Before I present these arguments, however, the next section will give an overview of the latest version of Kitcher's account, showing the further development of the proposal for well-ordered science.

3.4 Science in a Democratic Society

3.4.1 The context and the questions

Science in a Democratic Society continues the exploration of science's place in democracy that began in *Science, Truth, and Democracy* but the nature of the challenge it is to face has changed again. Kitcher opens the book with the observation that there is a common perception that relations between science and democracy are unsatisfactory. The familiar opposition between two sides is this time recast as the conflict between those who put the blame for this problem on science and those who blame society's anti-scientific prejudice. Although Kitcher (2011c, 15–16) mentions historical and sociological studies of science, as well as more general criticisms of Enlightenment, he believes their influence on public attitudes underlying the conflict to be negligible. Accordingly, the conflict between competing theoretical accounts of science that Kitcher attempted to overcome in the previous books no longer takes centre stage. Instead, Kitcher is concerned with public opposition to scientific theories such as evolution, science-based technologies such as genetic engineering and scientific pronouncements on issues such as the global climate change.

Kitcher relates these public discontents to a complex of attitudes. Among them are the perception that science poses a threat to important values, alienation caused by overconfident scientism, and the suspicion that experts' disagreements expose the biased and politicised nature of science. Kitcher traces these attitudes back to the general dissatisfaction with the current division of epistemic labour in society. What is at issue is how to distinguish social problems about which each individual has the authority to decide on one's own from those with respect to which the authority should be given to experts. A related question concerns the criteria for designating experts. Among those dissatisfied with science, the current criteria for selecting experts may be considered inadequate or, if adequate criteria are available, current experts may be believed to fail them. The central question for Kitcher's project (and, as he suggests, for philosophy of science in general) is to propose a solution for the problem of expertise in democratic society. The task is to describe how the division of epistemic labour should be realised and how the system of public knowledge should be organised so as to help the realisation of the values and ideals of democracy (Kitcher 2011c, 25–26). The question of the social organisation of science becomes a part of a more general discussion of knowledge and society.

[48] This position has attracted some criticism—for example Jay Aronson (2003) considers the lack of discussion of current developments in democratisation of science policy and the lack of proposals for the practical implementation of Kitcher's project a failure. (In addition to the lack of familiarity with relevant empirical knowledge, Kitcher can be criticised for mischaracterising its state. As Mark Brown (2004 and 2013) points out, Kitcher is mistaken when claiming that there are no relevant empirical studies.) While I believe that it is important to keep in mind the aims Kitcher sets for himself, I will later discuss the question whether Kitcher's proposal can be combined with insights from political science as Kitcher promises.

Providing an account of the division of epistemic labour, in turn, requires addressing some of the central issues in philosophy of science. In particular, Kitcher diagnoses the conviction that science is value-free as one of the causes of public discontent.[49] Kitcher is mostly concerned with cases where sound scientific claims (his primary example is the scientific consensus on the global climate change) are dismissed in public debate on the grounds that their presentation and the call for action on their basis can be shown to involve particular values. Given the problem situation, challenging the value-free ideal in particular and the usual way to understand debates over values in general becomes one of the central tasks of Kitcher's argument.

3.4.2 Values

Kitcher's (2011c, 31–40) position on values in *Science in a Democratic Society* constitutes a further departure from the value-freedom of *The Advancement* or even the external model *of Science, Truth, and Democracy.* The admissible role for values is no longer limited to the stages of choosing the research project, setting moral limitations on research and making decisions about its application. Instead, values are seen as ubiquitous in scientific practice. Uncontroversially, choosing a research project requires judgements of value (this is the location for values that even the proponents of the value-free ideal usually accept). What is often resisted is the idea that the judgements about the adequacy of evidence and evidential support are influenced by values. To prove this more controversial point, Kitcher appeals to the idea that a high social cost of a mistake requires particularly high standards of evidence. More generally, any judgements made in the situation of an ongoing scientific debate, where the evidence is incomplete, available solutions partial and further steps uncertain, involve values. A scientific debate is in effect a clash of conflicting value schemes. When the opposing sides in the conflict see different problems and solutions as crucial, they endorse different schemes of values. As the winning side extends successes, the other side has to modify its scheme of values until there are no defensible schemes left. This is when the debate closes.

This admission for the role of values in the debate closure can be interpreted in a relatively conservative for philosophy of science way if values in question are epistemic. Kitcher, however, does not discuss the role of values in terms of this distinction. Instead, he proposes a tripartite model of values. Values of different levels are related and influence each other: one's *probative* scheme of values (values involved in particular judgements about significant problems and the sufficiency of evidence) and the *cognitive* scheme of values (valuing certain kinds of knowledge) are not isolated from one's general *broad* scheme of values. Thus, science is definitely not value-free.

Nevertheless, Kitcher maintains that it is possible to distinguish proper and improper instances of the role of values in science. Kitcher's solution is to argue that a rational resolution of conflicts over values is possible (Kitcher 2011c, ch. 2). However, rather than arguing that reasonably acceptable values are something pre-given to be rationally discovered, Kitcher suggests that they acquire their standing in a kind of deliberative conversation. This conversation, in turn, is discussed in the context of "the ethical project" (Kitcher 2011c, 41). In the book with this title (Kitcher 2011a), Kitcher presents his pragmatic naturalist account of the emergence of the ethical. Kitcher's aim is what he calls a "how possibly" (Kitcher 2011a, 12) history of ethics. Given what we know about human evolution and history, the question for Kitcher is to explain how ethics could have developed in principle.

[49] This is one of the problems Douglas (2009) addresses with her argument about the inadequacy of the value-free ideal for advisory science (Kitcher mentions Douglas in the book, although not when discussing inductive risk).

Kitcher argues that the social life of early human bands was made possible by the capacity for psychological altruism: the individual's wishes sometimes change in response to the perceived wishes of others. This capacity is, however, limited and its failures make social life difficult. In order to address these failures, humans have been engaged in the "ethical project", developing the ability for normative guidance, or the ability to follow ethical norms, and working out appropriate norms. While there is no discovery of independent truths in this ethical conversation, some of these developments can be called progressive—they provide increasingly better solutions to failures of altruism.

Originally, in small bands, participation in this project could be equal and immediate. All members of the band could take part in discussions of the rules that were to regulate their social life, on the conditions of equality. Simple rules of the first human bands have gradually evolved: as a result of this development, they have ultimately acquired complexity that characterises contemporary ethical systems. The recognition of their origin in the ethical conversation, however, has been lost in this process. Kitcher proposes that the original approach to ethics through the ethical conversation be taken up again.

Actual realisation of a face-to-face conversation on the conditions of equality is impossible due to the problems of scale, failures of information and sympathy, and the threat of cacophony (Kitcher 2011c, 51). The conception of ethical conversation nevertheless provides an ideal against which judgements involving values can be checked.[50] On Kitcher's view (Kitcher 2011c, 50–51), such an ideal conversation should satisfy certain conditions. Its potential participants should include the entire human species (including the representation of the interests of future generations). The affective requirements of the conversation demand that it happen on the conditions of mutual engagement: participants communicate on terms of equality and sympathy and take each other's preferences as seriously as their own. The cognitive conditions of the conversation mean that they proceed on the basis of true beliefs about the natural world, the consequences of their choices, and each other's wishes. The participants seek to find the best balance between various desires by assessing them from a variety of perspectives involved in the discussion. The ultimate aim of the conversation is to provide everyone with equal chances to live a worthwhile life.

Given that science is seen as thoroughly value-laden, the ideal of the ethical conversation is also applicable for the evaluation of the role of values in science. A value scheme, including broad schemes of values, is admissible as long as it could be sustained in the ideal conversation. If a scheme violates the cognitive and affective conditions of mutual engagement, it is not admissible. The appraisal of scientific debates thus relies on the same ideal of ethical conversation that is supposed to guide discussions over all values. The organisation of science in democratic society is to reflect this acknowledgement of the significance of values and ethical conversation.

3.4.3 The social organisation of the system of public knowledge

Discussing the role of science, and the public system of knowledge more generally, Kitcher stresses their importance for exposing the "unidentifiable oppression" (Kitcher 2011c, 78). The notion of unidentifiable oppression describes threats to freedom and the equal distribution of freedom, and thus to the equal chances to live a worthwhile life, that are not traceable to identifiable persons. Contemporary science, however, has never been specifically designed for the role it ought to play in democratic society (Kitcher 2011c, ch. 4). In the organisation of contemporary science, it is possible to distinguish traces of the functions it fulfilled in very different (non-democratic) societies, from Ancient Egypt and Mesopotamia, via Ancient

[50] Again, the focus seems to be on matching the outcomes of an ideal procedure rather than attempting to institute it, although, as the further discussion will show, Kitcher's position is somewhat more complex.

Greece and medieval Europe, via Royal Society and the 19th–century science to the Cold War-era United States and to the present day. Accordingly, the changes Kitcher proposes are extensive.

In his argument, Kitcher distinguishes four aspects of the system of public knowledge: the questions of investigation, submission, certification, and transmission (Kitcher 2011c, 89–93).[51]

The model of well-ordered science from *Science, Truth, and Democracy* is absorbed in the current project to cover the first of these aspects—questions related to investigation, or research planning. The main features of the model remain in place. A representative group of deliberators is to identify significant projects, subject to tutoring by experts concerning current judgements about significance and research possibilities and with the expectation of sympathetic attention to others' needs. After that, experts provide estimates of probabilities of success, so that final decisions about budgets and their allocation between projects can be made. Moral restrictions on inquiry are discussed in a similar way. An important change, however, is that the process of deliberation is now approached in the framework of the ideal conversation. So, the result of the deliberation is expected to be not simply a collective preference, but a preference satisfying the cognitive and affective requirements of this panhuman conversation. Various research projects are thus ultimately judged from the point of view of the overarching project to provide everyone with a chance to live well.

Kitcher (2011c, 125–130) makes several proposals related to the realisation of the ideal. First, the ideal helps to uncover the most glaring omissions in the way current priorities are set. Among them are the lasting influence of the past forms of organisation of research, the untutored character of preferences reflected in current science policy, distortions of aims-setting caused by privatisation, and the neglect of the interests of people in poorer countries. In order to avoid the perpetuation of these problems, Kitcher suggests that two indexes should be developed. An "atlas of scientific significance" (Kitcher 2011c, 127–128) presents and constantly updates current significance graphs in various fields, helping to see their achievements and possibilities for development. An "index of human needs" (Kitcher 2011c, 129) does the same for various needs that different social groups perceive as the most urgent for them, helping to make decisions about research priorities.

Second, Kitcher briefly discusses some practical strategies that could make the participation of the public in science policy closer to the ideal. One approach focuses on popularisation of science. Kitcher proposes to supplement the traditional forms of popularisation by the organisation of small representative groups of citizens that would be tutored about the current state of research. They can then act to facilitate communication between scientists and non-scientists. In addition to that, various experiments in the sphere of deliberation and democratic participation can be tested for their suitability to help to approximate the ideal of well-ordered science. Kitcher (2011c, 222–226) briefly discusses two such experiments, the citizen juries organised by the Jefferson Centre[52] and James Fishkin's (2011) experiments with deliberative polling. Kitcher considers the latter, with their focus on educating deliberators, especially appropriate for well-ordered science. His general recommendation, however, is that devising and testing the ability of various forms of organisation to win the trust of the general public is a matter for empirical research in political science.

[51] In earlier versions (Kitcher 2006 and 2007b), the "Inquiry-and-Information System" has three main divisions: inquiry, certification and dissemination. These three aspects continue to be at the centre of the discussion in *Science in a Democratic Society*. Many of the themes of *Science in a Democratic Society*, including the problem of unidentified oppression, are introduced in the 2006 paper.

[52] http://jefferson-center.org/, accessed 13.03.2016.

The proposal to set the research aims democratically in *Science, Truth, and Democracy* builds on the relatively conservative idea that social values can permissibly play a role at the stage of research planning. Reflecting the concern about mistrust of science in society, *Science in a Democratic Society* extends democratic involvement to the process of certification of scientific knowledge. Certification is well-ordered, if the ideal deliberators would agree that methodological guidelines for certification, and particular judgements involving them, are reliable.[53] Reliability means that they lead to conclusions that are true enough frequently enough. Methods generating conclusions that are unacceptably far from truth or fail unacceptably often would not be certified. Well-orderedness alone, however, may not be sufficient for trust. In order to ensure that the process of certification is trusted Kitcher proposes the notion of "ideal transparency" (Kitcher 2011c, 151–153). Public knowledge is ideally transparent if everyone would recognise and accept the methods used in certification of knowledge. Mistrust and alienation with respect to science can often be traced back to the lack of transparency. Accordingly, improving the transparency acquires crucial importance for Kitcher's project.

Discussing the possibilities to approach the well-ordered state, Kitcher points out that the issue of certification has been one of the central topics in philosophy of science (Kitcher 2011c, 153–155). By working on the traditional topics of explanation and evidential support, philosophers could contribute to the improvement of scientific practice, making certification better ordered. Simultaneously, they may help to improve the understanding of science in society, thus making certification more transparent. Another crucial task for meliorative philosophical work is the dismantling of the value-free ideal as the basis for understanding of science by the public. Scientific claims and judgements about certification inevitably involve values. The predominating conviction that the exposure of the values involved automatically disqualifies a scientific claim blocks the possibility to discuss these values openly and to show their reasonableness and defensibleness. More generally, popularisation of science and tutoring of small representative groups to act as mediators, which are recommended for the improvement of aims-setting, can also be used for improving society's confidence in existing certification procedures.

The ideal of well-orderedness is also supposed to govern the application and dissemination of results (the issues of transmission), as it did in *Science, Truth, and Democracy*. In the earlier book it was seen as a straightforward repetition of the stage of research-planning with the aim to accommodate changes in information brought by research. *Science in a Democratic Society,* on the other hand, finds it necessary to address the problem of dissent that may arise at this stage (Kitcher 2011c, ch. 7). The majority of the public may fail to recognise a sound consensus achieved by the experts and lack information necessary to appreciate its grounds. In other cases, the public may fail to recognise the sound reasons for the lack of consensus. Given the importance of public knowledge for democracy, making application better ordered acquires great importance for a proper system of knowledge.

When proposing a solution, Kitcher argues that a free and unrestricted discussion, which is often believed to be synonymous with democracy, may be counterproductive in the case of scientific disagreements. First, in situations where there is no scientific consensus, no decisive argument on either of the sides and where claims are highly technical, the involvement of the public is unlikely to be helpful. Second, unrestricted free debate cannot be expected to end with a universally recognised victory of truth in situations where most of the public lacks relevant information, mistrust experts and feels alienated from science. Encouraging free speech and public involvement is therefore likely to be harmful. Elitism,

---

[53] Again, what is required is that the actual judgements coincide—"accord" (Kitcher 2011c, 150) with those that would be produced by the ideal procedure.

however, is not a sustainable option either. Judgements involved are permeated with values and their justifications can only be achieved in the course of the ethical conversation.

In order to avoid both the elitism of experts and vulgar democracy, Kitcher recommends approximating the situation of well-ordered science. In case of a controversy, a representative group of deliberators could be made familiar with the relevant information and discuss it on the conditions of the ideal conversation (Kitcher 2011c, 185–186 and 217–222). In some cases, such a group would learn about an instance of clear consensus among experts. In others, it may have to decide between alternative courses of action in the situation where there is no consensus yet. After learning how the scientific consensus has been achieved and how the factors involved (including value judgements) can be justified, these groups are to act as mediators between research community and the public. There is no duty to enable public discussion beyond this duty of very thorough communication by public representatives and researchers. On the contrary, measures should be taken to expose the lack of substance or novelty in old objections to the established consensus, thus imposing certain regulations on free speech. Another of Kitcher's recommendation is to encourage popularisation of science and to reform teaching of science in a way that makes the production of sympathetic and knowledgeable audience ("happy *consumers*" as Kitcher (2011c, 190, italics in the original) calls them) for scientific information one of its priorities.

In addition to the general system of public knowledge Kitcher envisages, he also discusses the internal organisation of scientific community, drawing on his earlier arguments about diversity and division of labour (Kitcher 2011c, 193–217). Unlike the unruly public debate, disagreement within scientific community can often be expected to be productive. The argument about the benefits of dividing efforts between different approaches is, however, extended in several important respects. First, Kitcher widens the repertoire of methods for sustaining diversity in scientific community. In addition to the creation of social structures that put individuals' private aims to use, like in *The Advancement*, Kitcher also discusses the institution of suitable social norms and the encouragement of cooperative behaviour. Scientists are encouraged to reflect what can be done to improve the state of their field. Recommendations about the desirable diversity can also be given by the groups of deliberators after tutoring.

Second, reflecting his new acceptance of the ubiquitous role of values, Kitcher discusses the epistemic benefits of social diversity in science. He argues that a more extensive inclusion of women and other underrepresented groups in science, including persons from poorer countries, may benefit community by introducing potentially relevant points of view and judgements about significance and standards. This variety of perspectives can help to expose failures of certification, such as the readiness to accept certain conclusions without sufficient evidence on the basis of beliefs (for example, racist or sexist beliefs) that could not be sustained in the ideal conversation where the subjects of these beliefs are represented (Kitcher 2011c, 150).

The presentation of well-ordered science is completed by a discussion, in the final chapter of the book, of several specific examples. In the chapter, Kitcher outlines positions that would be taken with respect to a number of controversies—the opposition to the theory of evolution, biomedical research, genetic engineering, and the scientific consensus on global warming—were science well-ordered.

3.4.4 Taking stock

In a sense, Kitcher's latest book fulfils the promise given in *The Advancement*. *Science in a Democratic Society* provides a picture of science in the context of a general discussion of "human flourishing". It also goes beyond addressing alternative theoretical accounts of science and turns to the most dramatic cases of anti-science sentiment. However, in the development of this picture the approach to the organisation of science presented in *The Advancement* has moved to a relatively minor position among the proposals for well-ordered science, which in turn have been considerably extended compared to their first introduction in *Science, Truth, and Democracy*.

When first developed, the notion of well-ordered science relied on Kitcher's new understanding of epistemic significance as constituted by human interests. The nature of problems Kitcher addresses in *Science in a Democratic Society*, above all the persistent public dissent with respect to established scientific conclusions, requires attention to other aspects of science, such as the processes of certification and dissemination of results. With some modifications, Kitcher extends his model of well-ordered science to cover these stages. First of all, these decisions should be open to a discussion in terms of values, because relevant scientific judgements about the reliability of methods, the sufficiency of evidence and the rational acceptability of a closure are permeated with values. Kitcher's account of the nature of values, in turn, supports the necessity of a particular kind of public conversation wherever values are involved. The development of Kitcher's account thus allows one to appreciate the connections between the social account of science and the proposal about the organisation of science. A more extensive account of values in science supports the proposal for a more radical reorganisation of science.

Discussing *Science, Truth, and Democracy,* I stressed several features of Kitcher's approach. Among them were the focus on wishes, or interests, in the constitution of scientific significance and through that their prominent role at the stage of aims-setting, the preference for the right outcomes over the right procedure and a very general approach to devising the optimal organisation of science. The universalistic ambition has in fact grown: in *Science in a Democratic Society* well-ordered science is supposed to include the entire humankind and address the global problems it faces. The development of other aspects of Kitcher's account shows a more complex pattern.

At first glance, the account of *Science in a Democratic Society* may be said to address worries caused by some aspects of the proposal in *Science, Truth, and Democracy.* The ubiquitous role of values is now recognised—the presence of values at all stages of inquiry is acknowledged. As a result, it becomes possible to discuss the organisation of science not only in terms of wishes at the stage of aims-setting but also in terms of schemes of values that individuals bring into inquiry. Particular forms of actual democratic deliberation that would mimic the ideal procedure, such as citizen juries, are now discussed. Their discussion helps to disperse the worry that Kitcher is uninterested in the democratic procedure. It also fulfils the promise that Kitcher's account could be further developed with the help from political and social sciences. The question may nevertheless remain how serious this change is. For some of Kitcher's critics (e.g., Matthew Brown 2012; Mark Brown 2013; Douglas 2013a), *Science in a Democratic Society* is still dominated by the preference for the right results over actual deliberative conversation. If so, some form of elitism can be justified as long as the right results are produced.[54] I will return to this question in my critical discussion.

Even without doubting the thoroughness of change, one can detect other tensions in the new account, as *Science in a Democratic Society* preserves some idiosyncrasies of Kitcher's approach and reinforces them. The notion of interest continues to play an important

[54] There are other concerns about the procedure and outcomes in Kitcher's account as well. Pinto (2015) argues that the failure to connect the ideal with actual practices undermines the plausibility of Kitcher's account. As Pinto points out, Kitcher simultaneously states that the ideal conversation cannot be realised in practice full-scale and that the results of the conversation cannot be known in advance: the ideal of well-ordered science is offered "without any ability to predict how that conversation should turn out" (Kitcher 2011c, 248). So, there is no description of either the method or the outcomes to guide the realisation of the ideal.

role when envisaging the role for science in democratic society. The way interests are to be taken into account has, however, been made subject to certain restrictions. What is ultimately at stake is the possibility to satisfy the universal human interest in living a worthwhile life. This is why the ethical conversation with its aim of overcoming failures of altruism is necessary and this is what the public system of knowledge helps to realise. Specific interests that deliberators bring to the aims-setting stage of well-ordered science are now subject to the cognitive and affective constraints. Only the interests that can pass the general requirements of the ideal conversation are to play a role in the planning of science. Thus, there is a shift from particular interests that individuals may have to interests that can be justified in a universal conversation. Kitcher (e.g., Kitcher 2011c, 128) stresses repeatedly that many individuals do not recognise either others' urgent needs or the conflict between their own deep and justifiable needs and shallower but more immediately perceived ones. In one sense, what the ideal deliberators are expected to contribute is even more limited than in *Science, Truth, and Democracy.*

The requirements imposed on interests that may be admitted into research planning, certification and application, in turn, have profound implications for the forms of deliberation that Kitcher discusses. The notions of tutoring and popularisation of science take centre stage when envisaging the small-scale arrangements that are meant to help to approximate well-ordered science. Deliberators' roles are carefully circumscribed, they are expected to deter to experts for tutoring and to provide very specific contributions, and the speech is carefully regulated. Kitcher's attitudes towards the public and the experts may raise concerns. For example, Kei Yoshida criticises Kitcher for ignoring the problem of "the tyranny of the experts" as he focuses on avoiding the problem of "the tyranny of the ignorant" (Yoshida 2012, 373). In the next chapter, I will discuss this highly demanding view of the ideal procedure to raise important questions about its character and relation to actual forms of democratic deliberation. Is the model Kitcher offers actually democratic? Is it close enough to democratic practices to be able to learn from political and social sciences? Most importantly, is it conducive to the achievement of the results Kitcher wants well-ordered science to achieve?

In the last three sections I highlighted a number of aspects of Kitcher's account: Kitcher's expectations about the contributions of deliberators and experts, the character of the institutions he proposes, and the universal character of his well-ordered science. These are the issues that are going to be at the centre of my critical discussion. I will argue that Kitcher's approach to these issues undermines the aims he sets for well-ordered science as well as its democratic character. Before that, however, I provide a summary of Kitcher's resulting proposal.

3.5 Conclusion: a well-ordered system

As the overview of the development of Kitcher's arguments shows, he started with a very restricted view of the role of the social in science. On this initial view, epistemic aims could be separated from practical ones and traced back to the objective structure of the world. The aim for a social account of science was to understand how individuals' relations should be organised in order to achieve these aims. From this minimalistic position, Kitcher's account has come to discuss a wide variety of senses in which science can be called social. On the revised view, epistemically significant questions are shaped by practical interests and natural curiosity. Value schemes play a crucial role in the evaluation of evidence and the development of scientific debates. In addition to the beneficial distribution of labour that may be a result of the social system where self-interested individuals compete for credit, a wider representation of different perspectives in scientific community may help to introduce fruitful new approaches. It may also help to improve the standards of evidence that rely too readily on stereotypes about the previously excluded groups.

Different aspects of the social are in turn used to support proposals concerning aspects of science that should be changed and actions that are necessary to bring this change about. For example, one may focus on optimising the system of incentives for scientists or improving the "Millian arena" (see Kitcher 1997a and 2003, ch. 8 for the discussion of this notion) so that different perspectives could compete fairly to common epistemic benefit. Or, one may argue for the necessity of a systematic public input into decision-making about the aims of research. Or, the aim may be to change certification procedures towards a greater transparency and well-orderedness. Kitcher's approach to the role of values and the central role of deliberative conversation in his account ultimately lead to a very specific system—well-ordered science. Its main aspects are summarised in the following table.

Aspect of science	Present state	Well-ordering: the role of experts	Well-ordering: the role of the public
Selection of epistemically significant research questions and particular research projects	Sediments of past unsystematic choices reflecting interests and curiosity of their makers; results of unsystematic and parochial pressures from scientists, politicians, businesses and interest groups	Experts tutor deliberators about the epistemic significance of past achievements and future possibilities and estimate probabilities of achieving the aims selected after deliberation	Representative groups of deliberators set aims for research, subject to tutoring and the cognitive and affective conditions of the ideal conversation
Setting of moral limitations on research	Relatively close to well-orderedness: public and professional discussion, regulations to ensure respect and fairness towards research subjects	Experts tutor deliberators about the significance of the research to be regulated	Representative groups of deliberators propose regulations, subject to tutoring and the cognitive and affective conditions of the ideal conversation
Certification of results	Widespread failure of transparency: even in the cases where certification is reliable, there are doubts, denial and rejection	Experts tutor deliberators about standards of certification	On the basis of tutoring, ideal deliberators decide whether standards of certification provide true enough results often enough
Application of results in the situation of dissent	Widespread failure to accept sound scientific consensus with respect to important and urgent issues	Experts tutor deliberators; within scientific community, keeping dissent alive is permissible for experts and may be productive	Representative groups of deliberators are tutored, make decisions about the course of action based on the scientific consensus and warn the public about spurious dissent
Cognitive diversity within scientific community as a precondition for beneficial distribution of cognitive labour, pluralism of perspectives and improvement of certification standards	Some institutional incentives for division of effort (credit), some initiatives to cooperate, somewhat increasing diversity among community members	Systematically improving system of incentives, norms and cooperative spirit; experts are to assume the "ideal lawmaker" perspective on diversity in one's field	Representative groups of tutored deliberators may give recommendations for increasing diversity

In this chapter, I showed how Kitcher's proposals about the social organisation of science have evolved before taking their most recent form and how this evolution has been connected with the evolution of Kitcher's view of the nature, aims and progress of science. The aim of the next chapter is to provide a critical discussion of Kitcher's proposals. There, I begin with discussing the points of agreement with Kitcher's approach. After that, I focus on the issues that I described as potentially problematic when summarising Kitcher's account.

CHAPTER 4. A CRITIQUE OF KITCHER'S ACCOUNT: EXPERTS, KNOWLEDGE AND PARTICIPATION

4.1 Introduction

As the previous chapter showed, the development of Kitcher's arguments has resulted in a highly sophisticated model. Specific roles are delineated for the experts, the representative groups of deliberators and the general public, and the actions and contributions expected from each group are carefully balanced. The aim of this chapter is to discuss critically this model. I argue that the vision of the expert and the public in well-ordered science that Kitcher offers is inadequate and that public participation that Kitcher proposes as a crucial element of well-ordered science is in several important respects too limited. These limitations, in turn, make problematic the achievement of the aims Kitcher sets for well-ordered science. They also undermine the very possibility of calling his account truly social, if the social is understood in the sense of democracy and participation. I suggest that these restrictions follow from Kitcher's ambition to offer a universal system for the organisation of science. In order to avoid the problems that I identify in Kitcher's proposals, an alternative approach that ensures a more inclusive and immediate public participation in science and science policy is necessary. This participation, however, is incompatible with the dream of a universal system.

Developing my criticisms, I discuss four interrelated groups of issues. I begin with a discussion of the role of experts in well-ordered science and I argue that Kitcher's conception of expert contains contradictions that undermine the internal consistency of his approach and its credibility as a part of Kitcher's account of science. In the second part of my argument, I discuss several issues related to the kind of knowledge that the realisation of the aims of well-ordered science may require. Drawing on Kitcher's own discussion of scientific classifications and the notion of adequate solution I argue that experts in Kitcher's model may lack the knowledge required. As there are no mechanisms for overcoming this deficit, the achievement of the aims of well-ordered science is threatened. In the third part, I discuss the character of participation expected from laypersons in well-ordered science and the restrictions imposed on them by the affective and cognitive conditions of the ideal conversation. I argue that the way the public is seen in Kitcher's model and restrictions imposed on it create serious inconsistencies in Kitcher's account and further exacerbate the problem of achieving the aims of well-ordered science. They also have profound negative implications for the spirit of active democratic participation that Kitcher describes as an important element of his approach. Finally, I attempt to explain why Kitcher proposes these restrictions. I suggest that an alternative approach that avoids these problems may be possible, albeit at the cost of giving up some of Kitcher's expectations with respect to such a proposal.

Despite the aim to offer a critical examination of Kitcher' account, the arguments I present in this chapter are developed against the background of several important points of agreement with Kitcher's approach. Accordingly, the next section discusses the aspects of Kitcher's proposal I find highly congenial. The section that follows presents in detail my disagreements with Kitcher. Its first three subsections are dedicated, respectively, to the problems related to the conception and the role of the expert in well-ordered science, the issues related to experts' knowledge and the theme of democratic participation in Kitcher's account. The order of discussion shows how the problems described are interrelated. The final subsection discusses possible reasons for adopting the approach Kitcher pursues and begins the formulation of an alternative that is to be developed in the remaining chapters of my thesis.

4.2 Agreeing with Kitcher

Regardless of particular points of agreement and disagreement with Kitcher's model of well-ordered science, it is impossible not to acknowledge the sheer scope and philosophical ambition of his attempt to address the theme of the social in science in a systematic and wide-ranging way. So, in addition to arguing that social values are inevitably involved in science and that the way science is organised should take this into account, Kitcher also provides a general account of values and their development. Similarly, in addition to describing the place of science in democratic society, Kitcher also discusses the nature and aims of democracy more generally. Moreover, Kitcher develops the most recent version of his account in order to address what indisputably are hard cases. The controversies around evolution, biotechnology and global warming are neither resolved nor close to a satisfactory resolution, while the urgently needed political action is delayed. Earlier I discussed how discussions of social aspects of science belong to a philosophical tradition with a considerable history. Even so, one can agree that Kitcher takes the development of this theme impressively far both politically and philosophically.

One of the most important points that I consider congenial in Kitcher's approach is the ambition to provide a systematic account of different social aspects of science. The first two chapters of the thesis gave some idea about the variety of ways in which science can be understood as social. Discussing them, I suggested that accounts focusing on different aspects of the sociality of science may provide different possibilities to argue for the social reorganisation of science. An approach that brings to the fore the role of social relations in scientific community for the achievement of its epistemic aims may tend to downplay connections between these aims and social values. As a result, it may treat the issue of practical consequences of scientific research, including the consequences for values, as purely external. Such a position is harder to defend if it is acknowledged that social values do play a role in the core practices of science, including the constitution of epistemic aims. As a result, within one approach, it is possible to develop a proposal that focuses on the reorganisation of incentives for scientists in order to help the achievement of epistemic aims that are taken for granted. Within a different approach it becomes possible to propose to reorganise science so as to make the aims and the products of scientific inquiry better cohere with particular social values. Given the growing recognition of various social dimensions of science and especially the highly consequential character of science, I concluded that an account that attends to different senses of the social is needed in order to address adequately the question of the social organisation of science.

The development of Kitcher's account can be seen as unfolding of such an attempt to demonstrate, and treat systematically, a variety of ways in which science is social. On the one hand, Kitcher discusses the role of social relations and interactions in science. For example, the system of recognition and credit in scientific community makes possible effective distribution of labour. Another example is the emergence of the winning argument in a controversy through social encounters between researchers. On the other hand, Kitcher argues that science is permeated with values. For example, human interests play a crucial role in the constitution of the epistemically significant. Values are also inevitably involved when judging the sufficiency of evidence or the rationality of a consensus. Kitcher attempts to unify these different facets of sociality of science within a single approach to the social organisation of science. In this chapter, I intend to argue that Kitcher's proposals are ultimately problematic in light of the aims and values his account is meant to realise. Yet, I agree that what Kitcher attempts to achieve—a kind of integration that shows how different aspects of the sociality of science intertwine and influence the character of science and the knowledge it produces—is an important aim for the socially oriented philosophy of science.

The second point of agreement concerns the position I defended in my argument about the considerations to take into account when making philosophical proposals for the social organisation of science. In the first chapter I argued that the interest in social aspects of science can be understood as a part of a wider practice-oriented approach to science in contemporary philosophy—the approach that attempts to understand science the way it is actually practiced. This focus on practices of science, in turn, brings to the fore the consequential nature of science. Science is a material practice that has real and lasting consequences in the world, from the introduction of new objects and substances to the remaking of concepts and possibilities for action in light of which individuals and societies plan their lives. Accordingly, I suggested that attention to social consequences of particular forms of organisation of science is necessary for a defensible philosophical proposal about the organisation of science.

Beginning with *Science, Truth, and Democracy,* this theme of consequences of scientific research acquires ever growing importance for Kitcher's account. First, Kitcher brings this theme up with his use of the map metaphor. Mapping a terrain in a particular way may have consequences for the terrain. The map helps particular activities (for example, it exposes to the public previously unfrequented areas of wilderness) and these activities in turn have impact on the terrain (Kitcher 2003, 60–62). Second, Kitcher offers an argument against the traditional distinction between pure research, which can be insulated from the appraisal of practical consequences, and technology, which can be appraised in these terms. (Ch. 7 of *Science, Truth, and Democracy* is dedicated to discussing and rejecting several common approaches to defending the distinction between pure research, applied research, and technology.)

In addition to the general recognition of practical consequences of scientific research, Kitcher's approach is characterised by attention to the questions of justice, in particular fairness in distribution of benefits and burdens that result from scientific research. Different social groups have different chances to benefit from contemporary science. Medical research is one of the most obvious examples: research on diseases that are widespread in affluent countries receives disproportionally more funding than research addressing common diseases of poorer countries. As a result, sufferers of different diseases have different chances of receiving relief (besides discussions throughout Kitcher (2011c), see Flory and Kitcher 2004; Reiss and Kitcher 2009). Different social groups may face consequences of different severity if possible lines of research are abandoned. For example, Kitcher argues that blocking research into genetically modified crops affects disproportionally those living in the areas where current agricultural practices often fail and the food safety is the most fragile (Kitcher 2011c, 237–242). Different social groups may also face different consequences as a result of certain socially consequential research projects. Social groups that are subject to common prejudices may suffer if research confirms these prejudices, undermining their social position and their feeling of self-worth. This may happen even if research in question has problematic scientific credentials and particular conclusions are subsequently debunked.[55] Popular support for research of this nature thus has vastly different consequences for members of underprivileged and privileged social groups.

Besides that, in what has become an increasingly important theme in the development from *Science, Truth, and Democracy* to *Science in a Democratic Society*, there is the

[55] This argument is presented in detail in Kitcher (1997a) where Kitcher argues that certain lines of research should not be pursued and that these considerations should guide scientists' behaviour as a kind of imperative. I attempted to show that it is possible to recognise this issue and yet address it differently than Kitcher does in Eigi (2012). There, I argue, drawing on Longino's ideas, that instead of trying to stop certain lines of research, community should focus on criticism of problematic background assumptions involved in them.

recognition that members of different social groups are in vastly different positions with respect to the possibility to appreciate scientific research and self-identify with science. Members of certain social and economic groups have a considerably better chance of joining scientific community and seeing themselves as a part of this enterprise. They are also more likely to receive the kind of education that allows them to appreciate scientific results and accept them as a beneficial contribution to their world. For many others, science is, and likely to remain, remote, incomprehensible, and alienating (see, e.g., Kitcher 2011c, 166 and 174).

Kitcher is not the first to raise questions of justice and fairness in philosophy of science. It has been an important theme for feminist philosophers of science (such as Longino), those interested in science for poorer countries (such as Lacey) and those advocating strongly political versions of philosophy of science (such as Barad). Nevertheless, I believe that Kitcher's attention to this theme and his attempt to address it systematically is one of the most admirable aspects of his approach, setting the bar high for other philosophical proposals for the social organisation of science.

Discussing the necessity to take the practical consequences of research into account, I touched the issue of finding a balance between an epistemically desirable and a socially desirable form of organisation of science. Analysing James Brown's and Kourany's arguments in the first chapter, I argued that the sanguine position according to which an epistemically desirable form of organisation does not need an appraisal in terms of its practical consequence is not sustainable. The mirror position that a socially desirable form of organisation would not lead to epistemic losses was also shown to be problematic. Instead, both kinds of consequences should be discussed and the particular trade-offs that emerge from a particular proposal about the organisation of science should be defended.

Kitcher's readiness to acknowledge the reality and the difficulty of balancing different kinds of desiderata is another aspect of his argument with which I am in agreement. Already in *Science, Truth, and Democracy*, Kitcher discusses the difficult choices of this kind that one may face. Even if a line of research is epistemically valuable, mostly "pure" and unlikely to lead to undesirable practical consequences, one has to take into account the fact that pursuing it uses resources that could support more practically relevant lines of inquiry (Kitcher 2003, 90–91). Inevitably, decisions about the acceptable trade-off between the epistemically desirable and the practically desirable have to be made and justified.[56] The discussion of this issue is further developed in *Science in a Democratic Society,* where Kitcher (2011c, 110–111 and 123–125) openly admits that the well-ordering of science may lead to the suspension of epistemically valuable projects that are aimed at satisfying pure curiosity. This may be expected not because these projects are problematic, epistemically or socially, but because they lose in comparison with more urgently needed research projects. Particular trade-offs have to be made and the framework of the ethical conversation can be used as the basis for justifying them, but the choice remains difficult and the sense of epistemic loss palpable. While Kitcher's proposals about the character of such decisions under well-ordered science are inevitably very general, I applaud Kitcher's bringing this issue forcefully to the fore.

Finally, in my discussion of various proposals in the first two chapters the importance of the wider political context for science and the necessity of political action for reorganising science emerged. Establishing a relation with the sphere of science policy and politics more generally is thus another desideratum for the philosophical approach to the organisation of science.

[56] One example of a more concrete discussion of trade-offs is Kitcher's (2007a) essay on the concept of race. Kitcher's arguments show how difficult it is to find a proper balance between considerations of different kinds.

Kitcher's discussion of experiments with citizens' juries and other forms of public deliberation as a method for realising his proposal is one example of the way it can be done. Making this plea for cooperation, Kitcher has a very specific vision of its form. As noted previously, he sees his ideal as the basis on which one can build with the help of empirical research in political science. More specifically, Kitcher suggests that his model of well-ordered science indicates the locations where general methods for the improvement of deliberative practices could be applied for the aims he specifies. As Kitcher puts it, "If you like, Fishkin's question is 'How?' and mine is 'Where?'" (Kitcher 2011c, 225). Similarly to my overall position with respect to Kitcher's approach, I am more in agreement with the principle than its realisation. I will subsequently argue for a different approach to bringing philosophy of science and political analysis together. Nevertheless, I share with Kitcher the conviction that philosophy of science should acknowledge the relevance that work in political science may have for a philosophical project.

The desiderata that I have formulated and that I will attempt to satisfy with my own argument have certain important similarities with what Kitcher aims to achieve. I believe that for a philosophical attempt to discuss the social organisation of science it is important to pay attention to different aspects of the sociality of science and their connections and interactions; to social consequences of science and the fairness in their distribution; to the fact that every proposal means a certain trade-off between the socially and the epistemically desirable; and to insights that political analyses of science policy and democracy can provide. At the same time I believe that the way Kitcher develops his account undermines the aims he sets for it. Showing that and arguing for an alternative approach takes the reminder of my thesis.

4.3 Disagreeing with Kitcher

4.3.1 Introduction: a system of careful restrictions

As the previous chapter showed, Kitcher arguments have moved from proposals about the organisation of scientific community to much more general proposals how science should be reorganised as a part of democratic society. One of the consequences of this extension is that Kitcher's most recent proposal (summarised in the conclusion of the preceding chapter) is developed on two levels and targets two different groups.[57] Most of the proposals in *Science in a Democratic Society* (well-ordering research planning, research ethics regulations, certification and application) are related primarily to the theme of science in society. They concern the system of public knowledge and the division of epistemic labour in the wide sense. One group of proposals (improving diversity, cooperation, norms and incentives in scientific community) focuses on the theme of the organisation and functioning of scientific community. It addresses the division of cognitive labour in the narrow sense and continues the approaches Kitcher has been developing since introducing his models of division of cognitive labour (Kitcher 1990; 1995a).

There are some important similarities underlying Kitcher's treatment of these questions, in particular, the general theme of representation and participation. It is good to have a maximally representative group of deliberators when deciding upon the aims of research because it is reasonable to expect that people know best what their needs and desires are. It is good to have a diverse scientific community because it is reasonable to expect that someone genuinely believing in a hypothesis or an approach would work to improve it most effectively. Another of the common themes is the theme of control. Participation of both

[57] Pinto (2015) similarly comments on the distinction between the "internal" and "external" aspects of organisation in Kitcher's account and argues that "black-boxing" (Pinto 2015, 180) of the internal organisation of scientific community is an important limitation. Pinto is concerned with the inability of this approach to deal with the influence of a wider context on the internal organisation; I will mostly discuss internal tensions in Kitcher's view and their consequences for the aims of well-ordered science.

experts and the public is indispensable for well-ordered science. At the same time, it should be carefully organised in order not to undermine well-orderedness by the "tyranny of the ignorant" and vulgar democracy (Kitcher 2003, 117) on the one hand and the kallipolis's elitism of experts (Kitcher 2011c, 22) on the other.

Some of the most obvious restrictions in this form of organisation concern the role of experts. Well-ordered science limits the freedom of experts to make a number of crucial decisions concerning inquiry. In particular, decisions about the selection of directions for research (and the moral limitations on it), the standards of sufficient reliability and the course of action in the absence of a consensus should not, according to Kitcher, be left to experts. Experts' autonomy is restricted in these spheres by the introduction of the procedure of deliberative conversation. Experts' role is limited to providing, in an objective and disinterested fashion, their expert scientific knowledge in order to tutor laypersons and deliver estimations of the probabilities of success of various plans for research.

These limitations on scientific freedom and autonomy can be explained in light of the overall aim of Kitcher's project, his account of the ubiquitous role of value judgements in science, and the framework of the ideal ethical conversation. Ultimately, the aim is the state where all members of human race have equal chances to live a worthwhile life. Scientific knowledge is important because it can contribute to the achievement of this state, addressing the needs of both particular individuals and the entire humankind. (In particular, science can help to stop processes that threaten to turn the Earth unliveable.) Given this aim, one of the central tasks for well-ordered science is the setting of appropriate aims, so that research pursued does indeed help everyone's chances to have a worthwhile life, without ignoring unfairly individuals' or groups' interests. This is the core of Kitcher's (2003) initial proposal for well-ordered science. This is also one of the most important places for the public input in the later version of well-ordered science. This is the location where lay deliberators contribute what only they can provide—the first-hand knowledge of what they need for a worthwhile life. The kind of input they are expected to provide at the stages of certification and application is similarly related to the immediate knowledge of their visions of a worthwhile life and their central values. It is on the basis of these values that they can indicate what degree of reliability of knowledge and what risks they are ready to accept and what course of action they support in the situation of uncertainty. Accordingly, Kitcher sees these decisions as properly belonging to the sphere of deliberation in the conditions of ideal conversation rather than experts' decision-making.

Deliberators themselves, however, are also subject to a number of restrictions. Some of those are related to the kind of input they are supposed to provide in the system. Deliberators are to articulate their needs and interests. This is the sphere where they are expected to have authoritative knowledge, which is in turn necessary for making appropriate decisions about science policy. However, they are not expected to contribute substantial factual or expert knowledge. The knowledge they are to contribute is supposed to be different in kind: the "type of authority that non-scientists have … is different from and complementary to the sophisticated understanding achieved collectively by the scientific community" (Kitcher 2007b, 179–180). The possibility that laypersons can make contributions other than judgements about what is important for them—for example, that they can share their own substantial knowledge to supplement that of experts, is not built into well-ordered science. Another set of restrictions concerns the way their input is to be presented. Deliberators' desires are subject both to tutoring in current achievements and possibilities of science, performed by experts, and to the cognitive and affective requirements of the ideal conversation. The importance of tutoring is justified in Kitcher's proposal by the need to avoid the tyranny of the ignorant. The requirements of the ideal conversation are dictated by its aims—both the lack of information and the lack of sympathy with respect to others' needs would interfere with the aim to ensure the universal chances to live a worthwhile life. This aim explains why "raw"—poorly informed and egocentric—preferences cannot serve as the proper basis for decision-making in well-ordered science. To prevent their intrusion, the freedom of the public to express wishes is regulated by the orderly and demanding procedure of the ideal conversation and the requirement of tutoring by experts as the precondition for decision-making.

The restrictions discussed so far concern the roles of experts and the public in the wider system of division of epistemic labour in society. However, in the proposal for well-ordered science, there is also the level of the organisation of cognitive labour within scientific community. On this level, in the role of scientists as members of scientific community rather than tutors, experts seem to enjoy a relative lack of restrictions. The argument that scientists' personal aims and ambitions may serve the advancement of science retains its importance also in well-ordered science. It supports the allocation of considerable freedom to scientists within scientific community to act how they see fit and to be what they are. There is no comparable freedom for laypersons on any level of well-ordered science. Neither are experts themselves as free when they enter the sphere of making decisions about the directions of research and the certification and application of results in wider society.

The aim of the next three subsections is to analyse these aspects of Kitcher's vision of experts and the public, and their implications for the success of Kitcher's proposal. The target of my argument is the ideal of well-ordered science as Kitcher describes it. The same problems, however, would affect any approximation of well-ordered science that reproduces the relations between experts and deliberators that the ideal sets, as small groups of deliberators to be tutored about science in *Science in a Democratic Society* seem to.

The first point of my criticism concerns some important tensions in Kitcher's conception of expert in well-ordered science.

4.3.2 Experts

In well-ordered science, ideal deliberators are supposed to contribute unique knowledge without which helping everyone's chances to live a worthwhile life is impossible—the immediate knowledge of what they need for such a life. As Kitcher puts it,

> One of the most fundamental thoughts behind democracy is that individual people have a better understanding of aspects of their own predicament than do outsiders, however wise and well-intentioned. (Kitcher 2011c, 118)

Making these contributions the basis for research planning that would benefit society, however, requires clearing them from factual errors and making them better informed. This is why scientific tutoring of deliberators is a necessary preliminary step. As a result, deliberators are to become acquainted with what is considered epistemically significant in a given field at the moment and with directions in which research can be taken in future. After tutoring, the initial preferences of deliberators are expected to change. It is those changed preferences that form the basis for deciding what particular projects are to be undertaken.

The organisation of aims-setting is thus seen as a road with the orderly two-way traffic. Deliberators provide the information about their needs; experts provide the information about a particular scientific field, its significant questions and the probabilities of success for various projects. Experts must not dictate the list of priorities for research; ideal deliberators must not impose their untutored preferences on it. There are distinct roles and distinct restrictions for each party in this system; both parties are necessary for its successful functioning.

An important complication enters this picture, however, because scientists are also supposed to be represented among ideal deliberators. The point of view of those who see certain epistemic projects as central for their lives should be represented in the deliberative process so that it could be taken into account alongside other visions of good life. As Kitcher explains to imaginary concerned scientists, well-ordered science is not meant to slight their interests—"their own fascination with (say) the hominid family tree is conveyed to their fellow discussants, who feel its force as they do" (Kitcher 2011c, 124; Kitcher return to related issues on pages 134–136). Experts in the ideal conversation thus have a dual role—they tutor deliberators and they are themselves represented among them.

In this dual role, experts introduce the objective picture of the epistemically significant and at the same time speak for themselves as persons for whom some of these significant questions are at the centre of their life projects. The possibility of a conflict of interest seems imminent. Experts are to remain objective and disinterested when presenting information about epistemic significance and promises of various research lines even when some of them are crucial for their vision of a fulfilling life. Laypersons may be allowed (within the limits set by the affective conditions) to be passionate about their life projects. Their passion is one the reasons they are expected to present their case best. Experts, however, as tutors and as participants of the conversation simultaneously are and are not allowed that.

Despite the possibility of this conflict Kitcher mostly avoids discussing how the roles of a deliberator and an expert could be combined. Instead, he simply postulates that experts are objective and disinterested:

> it's assumed that the experts identified are disinterested—or that any members of a group whose personal preferences would be affected by the project under scrutiny are disqualified from participating in the process. (Kitcher 2003, 120)

Later the theme of the representation of scientists' life projects is openly discussed as one of the issues for well-ordered science and yet the vision of the disinterested expert continues to stand. The problem of a potential conflict of interest remains unaddressed, as if it were sufficient to say that "for our purposes, it's enough that there are ideal experts, who share all the knowledge of actual judges but have no personal stakes in the line of inquiry" (Kitcher 2003, 120, fn).

The question whether scientists should represent their interests in the ideal conversation may seem relatively minor. I suggest that the tensions within the role of the expert that become evident when discussing this question have a considerably further-reaching impact, undermining the credibility of Kitcher's vision of the role of experts. The postulation of objective and disinterested experts as a necessary element of well-ordered science stands in sharp contrast with the conception of epistemic agent that Kitcher has been developing since *The Advancement*. There, Kitcher remarks that his idealised analyses of scientific decision-making represent "toy scientists" and "toy communities" (Kitcher 1995a, 305). Even so, his analyses start with the recognition that scientists may not always be motivated by purely epistemic considerations and instead may act on the basis of personal interest, ambition, stubbornness, deference to authority, cynicism etc. Starting from this understanding of the scientific condition, Kitcher shows that overcoming these "deviations" is not always necessary for the achievement of epistemic aims on the community level. There is no analogous argument for the situation of tutoring—it is simply postulated that experts are epistemically impeccable. The perfectly disinterested experts are thus not even the simplified "toy scientists".

The notion of disinterested expert also contradicts the way Kitcher describes actual scientists throughout his discussions of currently not-well-ordered science. Describing how research agendas are usually set, Kitcher points out that positions of scientists in this process reflect their particular interests and forms of expertise (Kitcher 2003, 126). Later he diagnoses scientists with a kind of myopia that makes them overestimate the epistemic significance and research promise of their own field (Kitcher 2011c, 119). This deep-seated scientific egocentrism does not combine harmoniously with the requirement of disinterested expertise. The "sullied" researcher, the self-centred actual scientist, the person speaking passionately about the personal life project, and the ideal disinterested tutor are in considerable tension when the same person is expected to be them all.

Kitcher's experts as members of scientific community on the one hand and as tutors in well-ordered science on the other are conceived in drastically different ways. The conception of objective tutor lacks the kind of realism that the approach to scientists as agents with heterogeneous motivations and tendency to epistemic egocentrism promises. The tension between different characteristics ascribed to experts in Kitcher's proposal, which Kitcher does not attempt to explicate or address, threatens the overall credibility of his proposal for well-ordered research planning.

In addition to that, I suggest that the introduction of the ideal expert in Kitcher's account of science and the organisation of science poses another important threat for Kitcher's project—a threat to some of the central elements of Kitcher's approach to science and democracy. In order to do so, I turn to Mark Brown's (2009) analysis of scientific expertise and representative democracy.

The main question for Mark Brown's analysis is the "relationship between political and scientific representation in democratic theory and practice" (Mark Brown 2009, 3). He argues that despite important differences between scientific and political aims and institutions, there are equally important parallels in the development of these two kinds of representation. In particular, Mark Brown shows how one traditional way to understand the two kinds of representation holds certain common assumptions about both. According to what Mark Brown calls the "correspondence model" (Mark Brown 2009, 6), the essence of scientific representation is the correspondence to nature that exists prior to and independently from research as an activity. On this view, scientific representation is "a mirror of nature" (Mark Brown 2009, 18). According to its equivalent in the political sphere, the essence of political representation is the correspondence with the popular will or the public interest that exists prior to and independently from political practices and institutions. Political representation is a mirror of popular will, or what popular will would ideally be (Mark Brown 2009, 18). In the correspondence model, it is possible to have knowledge that truthfully shows the world independently from the processes of knowledge production. Similarly, it is possible to have political representation that is true to its constituents' interests independently from political institutions and interactions.

In his book, Mark Brown discusses the historical development of this view of representation and challenges it. Instead, he offers an account of representation as a practice that interacts with its object and changes it. Scientists do not simply hold a mirror to nature but work with it and modify it. Neither this activity nor its results are insulated from the political sphere and the public in the way the traditional model presupposes. In developing this alternative view of scientific research, Mark Brown relies, among others, on analyses of scientific research as a practical (and laborious) activity involving various material and social resources and institutions. In parallel to this, he offers an account of political representation that focuses on the realisation of representation in various political institutions and practices and the way interests and identities to be represented are themselves constituted in this process. In both cases, representation is understood as "practices of mediation that engage and transform what they represent" (Mark Brown 2009, 7). This understanding brings to the fore the aspects of representation ignored by the traditional correspondence model.

I use Mark Brown's argument about the two approaches to representation in order to suggest that Kitcher's idea of the disinterested expert goes contrary to the development of

Kitcher's account away from a version of the correspondence model. Kitcher's discussion of scientific significance and the role of values in science can be seen as a challenge to this traditional understanding and an attempt to replace it with a more constructivist vision of inquiry. This challenge has a counterpart in the development of Kitcher's approach to values at the centre of which is the notion of deliberative conversation. The introduction of the role of the objective and disinterested expert in Kitcher's model of well-ordered science, however, gives up these developments and falls back on something strongly reminding of the correspondence model.

Moving away from *The Advancement,* with its assumptions about the objective epistemic aims for research, Kitcher has been arguing for the ubiquitous presence of values in science and the crucial role of human projects in shaping epistemic significance. Doing so, he has rejected the idea that research simply lets the natural world show itself. Extending the map-making metaphor that Kitcher employs, one can say that making a map does not equal holding a mirror to the landscape. Instead, map-making inevitably involves numerous human choices and is intertwined with various human activities. It is enabled by some activities and enables others, and in the process of mapping its objects helps to modify them. As Kitcher has been arguing since *Science, Truth, and Democracy*, so does science. In this sense Kitcher's account of science parts with the correspondence model and offers a different view of science as shaped by particular human choices (which in turn could, and should, be discussed democratically).

Kitcher's introduction of the ethical conversation as the means for approaching values in science parallels this rejection of the correspondence model. According to Kitcher's approach to ethics, there are no pre-given ethical truths. Since there are no ethical truths prior to the ethical conversation, there can be no representation of some pre-given values when setting aims for research. Instead, what is to be taken into account in science policy emerges as a result of particular interactive procedures.

Kitcher's discussion of the roles of experts and the public during tutoring in well-ordered science, however, abandons this newer non-correspondent understanding. Experts are required to be able to provide the objective view of the state of a field—to hold a mirror to it, so to say. There is no admission that the role of the expert involves the "mediation practices" that Mark Brown writes about. In the end Kitcher's conception of disinterested experts strongly resembles the traditional view of expert committees that Mark Brown criticises: "potential expert members of advisory committees [are to be evaluated] solely in terms of their professional qualifications, and nonexpert members in terms of their political interests" (Mark Brown 2009, 94). This practice, in turn, reflects what Mark Brown calls "two widespread but mistaken assumptions: experts do not have interests, and representatives do not have expertise" (Mark Brown 2009, 103).[58] This notion of objective expert providing a disinterested view of the state of a field is profoundly foreign for the account that sees science as constructing maps in a thoroughly value-laden way, with values themselves constructed in a human conversation. The introduction of this notion thus threatens to undo the development Kitcher's account has undergone between *The Advancement*, still strongly relying on the model of correspondence to the independent structure of the world, and a more constructivist alternative developed in *Science, Truth, and Democracy* and *Science in a Democratic Society.*

Is there an escape path from this predicament for Kitcher?[59] One may suggest that approaching the idea of the disinterested expert in a different way offers an opportunity for avoiding the inconsistency between different characteristics ascribed to the expert in well-

[58] I return to the discussion of the assumptions about the public's lack of expertise in the next subsection.

[59] Here I intentionally echo Kitcher's terminology from his discussion of the development of a scientific debate (Kitcher 1995a, 256–263).

ordered science. The image of the objective expert should be thought of as an ideal rather than an idealisation. It is not a description, no matter how idealised, of actual agents, but a prescription for these agents, an ideal towards which to strive. Kitcher's overall approach in *Science in a Democratic Society* is compatible with a role for ideals of this kind. As noted before, in addition to the system of incentives for researchers Kitcher also discusses other methods for improving scientific community, such as establishing suitable norms and cultivating appropriate attitudes on the part of scientists (Kitcher 2011c, 208–209). The idea of objective and disinterested experts can be seen as an ideal for encouraging such an appropriate attitude when fulfilling the role of the expert.

Given Kitcher's view of actual scientists or their idealised counterparts, this standard may be impossible to satisfy. Kitcher, however, suggests that striving to approximate an unattainable ideal may be a worthwhile path. When Kitcher discusses the value-free ideal, similarly problematic in light of his account of values, he states that he is in agreement "with the thought of viewing freedom from value-judgements as a standard we might do well to approximate, when and to the extent we can" (Kitcher 2011c, 39–40). The disinterested expert may be a similar impossible to achieve but valuable ideal.

Does it resolve the predicament? Moving the conception of objective expert to the level of ideals somewhat relieves the tension between the roles demanded from the expert. This move puts the notion of disinterested expert on a different—normative—plane from descriptions of experts' actual behaviour. I suggest that an important problem nevertheless remains. This problem turns on a crucial difference between the ideal of disinterested expertise and the ideal of value-free science. In the case of the latter and, before that, in the case of the ideal of rationality, Kitcher presents an argument showing how this ideal can be approximated in the non-ideal situation of actual science. The entire *Advancement* is an elaborate argument showing how imperfect agents with various non-epistemic aims, motivations and manners of action may rationally advance science. Similarly, although less formally, *Science in a Democratic Society* is an argument showing how debates about values on different levels in science can be resolved in a reasonable way so that science can, and often is, free from objectionable values.

There is no analogous argument in the case of the ideal expert. There is not even a "how possible" story that would show how researchers as Kitcher describes them could individually (or, in line with the approach to other ideals, collectively) approximate this ideal. I conclude that the tension caused by the requirements imposed on expert persists. The escape path thus runs into problems similar to those that prompted the need to escape in the first place.

In this section, I discussed the roles experts are supposed to play in well-ordered research planning. I argued that the expectations about experts' ability to combine different roles are in tension with Kitcher's account of the character of actual researchers, his conception of epistemic agents and his approach to inquiry more generally. I concluded that the way experts are conceived undermines the credibility of this aspect of Kitcher's proposal. In the next section I argue that there is another deeply problematic element in Kitcher's proposal, concerning this time expectations about experts' knowledge.

4.3.3 Expertise

4.3.3.1 Introduction

On Kitcher's view of the role of science in democratic society, its ultimate aim is to help everyone to realise chances to live a worthwhile life. Ideally, everyone should have access to scientific knowledge bearing on the success of one's life plans. Research should be planned in such a way as to address the urgent needs for which relevant information or scientific solutions are at the moment absent. According to Kitcher's model of well-ordered research planning, the best way to realise these objectives is to have representatives of the public provide information about their needs, while experts provide the information about the epistemically and practically significant developments in a field and probabilities of success for particular research projects. There is no requirement, or even admission, that deliberators could provide other kinds of information at this stage.[60]

As noted in the previous section, this view of the respective roles of experts and representatives of the public has similarities with a very traditional view that, for example, has been commonly applied when creating advisory committees. As Mark Brown writes, the rules concerning the balance of interests in these committees rely on the traditional notion of representation and the related assumption

> that those suing to be included on federal advisory committees must demonstrate a narrow individual or group interest in the committee's topic—rather than, say, information, arguments, or social perspectives relevant to its deliberations. (Mark Brown 2009, 98)

Experts, on the other hand, are expected to be the source of interest-free knowledge. In the previous subsection I argued that this model of expertise sits uneasily with Kitcher's approach to the nature of science and scientists. In particular, it seems questionable whether scientists as Kitcher has been describing them are capable to fulfil the role expected from them. In this section I argue that this view of the roles of experts and deliberators can be detrimental to the achievement of the aims of well-ordered science in other, more substantial ways. Even if experts were as objective and disinterested, there may be important gaps in their knowledge. These gaps could be filled if a less restrictive view of contributions that deliberators could make were taken; Kitcher, however, ignores this opportunity.

Developing my criticism, I first show how Kitcher's own arguments give reasons to worry about the limitations of experts' knowledge. I then suggest how these limitations could be addressed with the help of a greater involvement of deliberators in substantial discussion and criticism of the information experts provide. I support my argument with examples from two analyses of lay perspectives and local knowledge.

4.3.3.2 Research directions, concepts and values

Bringing human needs and research planning decisions into a better alignment is the central task for well-ordered research planning. There may be uncertainties and delays in this matching but some coordination is always possible. Given a clearly delineated need, be it preventing catastrophic consequences of the global climate change or creating vaccines against common diseases in poorer countries, one can decide with reasonable certainty that many lines of research would be irrelevant, while others hold some promise. The latter should then be prioritised according to the strength of the need in question. At the centre of well-ordered science is the idea that science can offer everyone some relevant information for formulating and pursuing life plans, identifying forms of oppression that may interfere with their realisation and solving problems that persons and social groups face. After all, this is why society has its system of public knowledge. In this, and the next, subsections, I argue that Kitcher's well-ordered science with its distinct roles for experts and laypersons may fail to fulfil this promise optimally. Specifically, I show that science may not be in position to address important needs in the optimal way, if the bearers of these needs are not encouraged to provide substantial criticism and knowledge in their interactions with experts. The line of criticism developed in this subsection focuses on the problems related to research directions experts have been pursuing and values and interests that may be embedded within concepts and classifications they use. Developing it, I suggest that Kitcher himself recognises the

[60] Kitcher does suggest that amateur scientists could sometimes provide valuable contributions to science; I return to the discussion of this issue later in the subsection.

possibility of these problems but fails to address them sufficiently in his model of well-ordered science.

Kitcher's approach to science as thoroughly imbued with values and influenced by various past interests attracts attention to the fact that even classifications that seem natural and research projects that seem purely epistemic may carry particular values and interests within. This, in turn, may have consequences for the ability of research to meet interests of certain groups. This ability may be diminished if, for example, the research directions selected previously do not allow addressing their needs in the optimal way, or if the language and classifications used are influenced by values that are incompatible with their interests.

For Kitcher, such a situation is one of the indicators that science currently is not well-ordered and a change is necessary, and he discusses several examples of its occurrence. Among other criticisms of not-well-ordered science, Kitcher (2003, 127–129) introduces the notions of Inadequate Representation and Nonrepresentational Ratchet. A group of people is inadequately represented if their interests are systematically neglected in research planning. This neglect, in turn, may easily become self-perpetuating because the initially chosen research directions have considerable inertia. As a result, it may be easier to continue to follow them, and later attempts to address the neglected needs are likely to be in a less favourable position. Kitcher suggests that this is what happened in the case of the development of hormonal birth control. The initial preference of women was to have birth control that men would take. This preference was ignored and birth control was developed for women. Once it became available, male birth control had to compete with the option that had had a considerable head start. As a result, it is likely that it will never become as cheap as to displace the female pill.

Kitcher believes that this problem would be resolved in well-ordered science, because its organisation is meant to prevent situations where important interests are unfairly neglected. Contrary to Kitcher's optimism, I suggest that what well-ordering of science ideally achieves is the prevention of new instances of this problem. In the conditions of well-ordered science, there would be no systematic neglect of anyone's interests. However, well-ordered science does not necessarily do enough to address the problem of limited possibilities that the current state of research may offer for some groups. While well-ordered science aims to satisfy needs of citizens of democratic society, participants in aims-setting are supposed to modify their initial preferences as they learn about achievements of science and possibilities of their future development. If the interests of a group that deliberators represent have been historically neglected, it may happen that none of the readily available opportunities inherent in current significant research projects can help to satisfy their needs in the way they would prefer it most. In this case tutoring would not result in a successful match between a need and a research possibility. Instead, deliberators may have to modify their preferences, giving up some of their important interests as a result of the acceptance of what experts have described as epistemically significant and feasible.

A similar problem of limitations due to the earlier choices encapsulated in experts' judgements concerns the language and concepts used in a field. This is the problem to which Dupré attracts attention in his discussion of Kitcher's well-ordered science. Dupré argues that Kitcher fails to discuss how concepts involved in scientific research are often value- and interest laden. Well-ordered science does not take into account that "the identification of kinds of things for possible scientific investigation already carries with it something of our own evaluation" (Dupré 2004, 510) and—crucially for my argument—that "possible research questions that science presents for democratic consideration are already more or less value-laden" (Dupré 2004, 512). Van Boewel (2012) makes a similar point, formulating it in terms of background assumptions: well-ordered science fails to attend to ways background assumptions may be involved in experts' categories. As these assumptions are not subject to discussion involving members of different social groups with different backgrounds, problematic assumptions may be uncritically used when setting research aims and pursuing them.[61]

I share these concerns and for me they are related to the problems of Kitcher's approach to the roles of experts and the public. Similarly to the previous case, I suggest that in Kitcher's account of science the possibility of these problems is recognised (my position thus differs from Dupré's criticism that Kitcher fails to acknowledge the ubiquitous presence of values in science). This recognition, however, is not taken into account when devising well-ordered science. Well-ordered science does not provide a possibility to explore systematically values involved in scientific concepts and frameworks.

Discussing classifications, Kitcher (2003, 51–53) stresses that they are consequential. For example, they may open objects classified to certain kinds of action or limit in some important ways opportunities of persons classified. Classifications and concepts have power. They are also value-laden but their relations to particular values and interests are not necessarily evident and their effects are not necessarily easily undone by pointing their origins out. Given the nature of classification, Kitcher proposes that a kind of "archaeology" (the notion inspired by Foucault's ideas) is necessary in order to understand the history and the influence of scientific concepts. However, in Kitcher's well-ordered science this archaeology is not required as a part of the tutoring process or a part of professional activities of experts in general. I suggest that this lack of systematic critical attention to concepts used by experts poses a problem for well-ordered science and that for addressing it "archaeology" performed by experts may not be enough.

The problem concerns the match between needs and research possibilities again. It is possible that the research promises introduced during tutoring are coached in terms laden by values that deliberators would rather not endorse, as they contradict their interests and self-conceptions. The acceptance of these concepts as self-evident or natural may harm interests of deliberators or limit their possibilities—just as Kitcher warns when discussing the consequential character of classifications. If deliberators recognise certain concepts as problematically value-laden, it may also have an impact on the credibility of information experts present, both at the stage of aims-setting and later at the stages of certification and application.[62]

Experts may not be aware of this value-laden character of their concepts or may not be in a position to see it due to their specific interests and training as researchers. As noted before, Kitcher believes researchers to be prone to myopia with respect to wider social picture when evaluating their own fields. They may also fail to see problematic values inherent in classifications applied to other social groups due to their own relatively privileged social position and background. As Kitcher himself remarks, scientific community has historically been very limited in terms of its social composition (Kitcher 2011c, 199). This supports a relatively pessimistic view of the ability of its members to perceive problems of classifications applying to members of less privileged groups or describing certain aspects of these groups' lives. This problem is not fully attributable to the personal failure of experts to be disinterested. Rather, the problem is caused by limitations of a perspective relative to a

[61] Van Bouwel goes on to contrast well-ordered science with Longino's approach that prioritises the exposure and criticism of background assumptions. In the subsequent development of my argument I also draw on Longino's ideas to address problems of Kitcher's approach but use them as an element of a more general alternative to Kitcher's proposal.

[62] Here my concerns are related to Wilholt's concerns about "the notorious framing issues" (Wilholt 2014, 170) that Kitcher's model fails to address. I will return to this theme when discussing the German attempt to democratise biotechnology policy.

particular social location and experience—limitations that may not always be overcome by experts' striving towards the ideal of objectivity. So, experts may not always succeed in critical evaluation of concepts used in their field even if they attempt to. Moreover, in well-ordered science they are not even required to attempt to do that systematically.

I have thus suggested that making "archaeology" of concepts a regular element of experts' activities may not be enough due a kind of blindness experts may have with respect to concepts describing groups to which they do not themselves belong, or some aspects of these groups' lives. There is a possibility, however, that members of these groups, who are supposed to be represented among deliberators, may sometimes be in the position to see this value luggage. It needs not be explicitly formulated knowledge about the problems of a concept. Instead, it may be a perspective formed by particular experience and particular social knowledge that allows its bearer to point out that the traditional concept is laden with problematic values.

Kitcher actually discusses such a possibility when he describes how certain framing assumptions in a field, taken as self-evident before, were challenged when the field became more socially diverse. For example, in primatology, important changes happened when the previous focus on behaviours of males was replaced with approaches studying a wider range of behaviours, including those of females. The increasing number of women working in primatology played an important role in this development. Yet Kitcher uses this example to support the more limited proposal for the "[r]representation of a broader set of perspectives within the scientific community" (Kitcher 2011c, 150) in the matters of certification, without extending it to the stage of aims-setting and its lay participants.

The restriction of this proposal to scientific community is surprising, given Kitcher's recognition that the breadth of perspectives represented in scientific community remains limited: science that is in principle open to everyone remains limited in its geographical and social makeup.[63] Why not to mitigate these limitations by using perspectives of lay deliberators, encouraging their substantive critical engagement with the information they are given during tutoring? Yet in Kitcher's proposal, deliberators are not expected to provide this kind of "conceptual criticism". While deliberators are supposed to clarify scientists' (mis)conceptions about what the groups represented want and need, there is no expectation that they could productively contribute to substantial criticism of the concepts experts use, and no opportunities instituted for deliberators to do so. As I have argued, this may ultimately diminish the ability of science to fulfil its function in democratic society, as Kitcher envisages it.

My criticism so far has been developed on the basis of Kitcher's own arguments, showing how well-ordered science fails to take into account certain aspects of the historical development of science and the nature of scientific concepts as Kitcher describes them. In order to provide an example of related problems arising in practice, I next turn to the case study that demonstrates how certain values that are inherent in researchers' categories and projects may undermine the values of the social groups whose needs these projects are supposed to address. In a number of papers, Lacey (see, e.g., 1997; 1999, 95–96 and 189–198; 2000; 2003) has discussed the values inherent in the concept of seed in contemporary biotechnological research (primarily "green revolution" but also genetic engineering approaches). The example of the seed is particularly appropriate for the discussion of Kitcher's account, given the fact that genetically modified crops constitute one of the test cases for Kitcher's well-ordered science (Kitcher 2011c, 237–242).

[63] Kourany (2010, 60–62) points out an additional problem. Even if members of previously unrepresented groups begin to enter scientific profession, predominating perspectives in scientific community may remain relatively unchanged because these new members would have to be socialised in community and adapt to it.

For Lacey, the question of values inherent in researchers' concepts is brought to the fore by his account of the role of values in science (most fully presented in Lacey 1999).[64] As described in chapter 2, Lacey argues that research always proceeds under certain very general cognitive approaches that are inevitably interconnected with social and moral values. In Lacey's words,

> it is not possible to pursue the objective of science (gaining understanding) except within the confines of a particular approach, where each approach is defined by the adoption of particular strategies which interact in mutually reinforcing ways with particular (social and moral) values. (Lacey 1999, 21)

Lacey argues that the defining strategies of contemporary science—materialist strategies—are such that they harmonise with the value of control over material objects. In the framework of these strategies the seed is conceptualised in a particular way: it is a biological object that can be fully understood in terms of underlying biological structures and processes. These structures and processes in interactions with the immediate material environment of the seed define certain possibilities, most importantly, the yield of the seed. The seed is seen in terms of its yields. This conceptualisation opens way to modifications of the seed (hybridisation or genetic modification) so as to increase yields. Lacey argues that this research orientation means the simultaneous commodification of the seed. The seed becomes something to be produced and traded on an ever larger scale (and also something to be patented and then licensed to users). The approach to the seed that focuses on quantifiable yields and their material conditions abstracts from the ecological, social and cultural contexts a part of which the seed may be. Questions related to these wider contexts are treated as a separate issue that supposedly does not threaten the validity and the neutrality of the knowledge achieved.

A particular approach to the creation of knowledge, including the embodiment of knowledge in objects such as hybrid seeds, is thus intertwined with particular values. These values include both explicitly declared values and values embodied in particular institutions and practices, such as the globalised market economy. Specific values support the approach and the results achieved with its help in turn reinforce these values. Lacey argues that due to these interconnections between strategies and values, knowledge achieved under materialist strategies may be harmful to value complexes that do not value control to a sufficiently high (very high) degree. Approaching the seed in the exclusively materialist way and commodifying it as a result may undermine value complexes and associated social practices in which the seed is not seen as a commodity. Lacey lists a variety of ways in which the new biotechnology interferes with these alternative forms of life. Among them are environmental and social costs of the high-intensity market-oriented agriculture that focuses on a limited range of crops, and the rise of large-scale capital-intensive farming, with the associated decline of small independent farms.

According to Lacey, it is not possible to pursue an approach that commodifies the seed and simultaneously create opportunities for alternative value complexes that focus on sustainable farming, cooperation and empowerment of the local communities. In order to support these forms of life, a different approach to the seed is necessary. In alternative approaches, the seed is seen as a part of specific social and ecological contexts, an element of specific practices rather than an abstract commodity. Planting the seed is seen as a part of repeating cycles of activity rather than an action to enable ever-increasing harvests for the market. Such alternative approaches and alternative social and political practices that could

[64] By using Lacey's example I do not endorse his particular way to distinguish the locations where social values play an inevitable role (the choice of strategies) from those where only cognitive values should be properly allowed (the acceptance of a hypothesis once the strategies are chosen). Instead, I use the fact that Lacey's approach makes him particularly sensitive to the issues that are relevant for my argument—the role of values in the conceptualisation of research possibilities.

support them, however, are increasingly displaced by materialist strategies and practices centred on the values of control. Moreover, when materialist strategies are seen as the basis of value-neutral research, the very possibility that this research is incompatible with some value complexes remains hidden. Lacey specifically warns against the inability to recognise alternative possibilities for social and epistemic developments that the adoption of the values underlying mainstream scientific practices is likely to cause (Lacey 1997, 41–42). In the case of the seed it means that the biotechnology approach is seen as *the* possibility to end hunger—to achieve something of universal value that is equally beneficial for any value complex. The questions about the consequences of commodification of the seed are never asked.

Lacey analysis attracts attention to the possibility that value complexes serving as the basis for a worthwhile life for groups of people, including many people in poorer countries, would not be best served by starting uncritically from the epistemically significant achievements of modern science and continuing research under the same strategies. Ironically, Kitcher's discussion of the position of well-ordered science on genetic modification of organisms demonstrates some of the features Lacey criticises. Kitcher's argument seems to presuppose that biotechnology research is, first, the only serious possibility, and, second, ultimately value-neutral.[65] (Douglas 2013a, 905 also comments on the way Kitcher frames the issue and ignores alternatives such as the agroecology discussed by Lacey.) Kitcher suggests that well-ordered science would support genetic engineering of crops because it enables to address the needs of the poor in the regions where currently available forms of agriculture regularly fail:

> For many of the world's people, particularly in Africa and parts of Asia, current agriculture is unable to provide them, in the environments in which they live, with ways of reliably growing the food they need. […] Well-ordered science would respond to the agricultural needs of the poor as it does to the neglect of their health. (Kitcher 2011c, 239)

Kitcher does not claim genetic engineering to be the only way to address the problem of hunger, instead talking of genetically modified organisms as "potentially valuable tools" (Kitcher 2011c, 239). However, there is no discussion whether the approach of contemporary biotechnological research and the values it represents may have negative consequences for the very people biotechnology intends to help. Rather, Kitcher argues that concerns about the biological safety of genetically modified organisms are to be addressed on the basis of existing scientific standards for testing safety. Public fears are to be ended by education and popularisation. Concerns about the profit-driven behaviours of biotechnological companies and the practice of patenting seeds are seen as a separate issue that can be addressed by political regulations alone, without attempting to explicate and analyse values inherent in the approaches to the seed in genetic engineering. Lacey's analysis of the seed shows, however, that such assumptions about the value-neutrality of research projects are deeply problematic.

Discussing Kitcher's account of judgements about significance and classifications in this section, I have pointed out the possibility that the uncritical transmission of existing approaches and concepts may undermine the promise of well-ordered science to address the needs of some groups of the public in the optimal way. It is possible that research directions and classifications are shaped by values that are incompatible with the interests of those groups or diminish the possibilities for their realisation. Lacey's analysis of the concept of seed shows that it may be the case for biotechnology research, including the genetic modification of crops. The way the seed is conceptualised when approached as the object of these modifications is incompatible with alternative traditional and "grassroots" ways to conceptualise the seed and undermines alternative social and cultural practices. An attempt to

[65] I will later discuss a different kind of irony that becomes visible when discussing Kitcher's position on biotechnology in the context of Jasanoff's analysis of politics of biotechnology.

solve the problem of hunger in this way may disrupt the lives of the hungry ones in dramatic ways.

Attention to alternative ways to approach the seed and alternative social practices around them could help to avoid these disruptions. This, however, may require the help of practices that lie outside of science. As Lacey himself stresses, in order to overcome experts' limitations, a development "which essentially engages not only the scientific 'expert' but also the peasant practitioner" (Lacey 1997, 48) may be required. In this exchange, experts could acquire substantial information that goes beyond the information about the public needs, including information relevant for the revision of experts' concepts, ways to frame issues and judgements about significant research possibilities. Kitcher's well-ordered science, however, does not presuppose the existence of such knowledge and fails to establish practices that could help to explicate and use it.

4.3.3.3 Local knowledge

The question whether laypersons could contribute substantive criticism of experts' approaches and concepts that I discussed in the previous section leads to the more general question of expertise. In Kitcher's model, are experts the only ones in possession of substantial knowledge? Given Kitcher's model of tutoring, the answer seems to be in the affirmative. This is how Douglas interprets Kitcher's position. Douglas (2013a) criticises Kitcher for his underestimation of the public's interest and ability to contribute to science, and the passive role given to the public in well-ordered science. As Douglas stresses, Kitcher's proposal for educating "happy consumers" does not attempt to encourage "actual critical engagement" (Douglas 2013a, 904) with science. In this section, I begin by pointing out the aspects of Kitcher's account that demonstrate some acknowledgement of the ability of the lay public to contribute to science. Moreover, Kitcher's account of science attracts attention to the locations where such contributions, more widely understood, would be relevant, given the aims of well-ordered science. I then argue that these insights do not find an adequate reflection in the organisation of well-ordered science. As a result, the ability to achieve the aims of well-ordering may be compromised.

Kitcher makes a brief argument (which Douglas does not discuss in her criticism) that the difference in the ability to contribute to science between "outsiders" and members of scientific community may not always be dramatic (Kitcher 2011c, 136–137). Historically, science has been open to contributions of outsider eyewitnesses. Nowadays, science could also benefit if contributions from knowledgeable laypersons were encouraged. For example, amateur astronomers and naturalists could be a source of valuable information and well-designed game environments could put to scientific use laypersons' skills (Kitcher gives the example of "Foldit", the computer game that has been used to study protein folding). Kitcher points out that cultivating such contributions would be both epistemically advantageous and good for the democratisation of science.

I suggest that the vision of lay participation in science that Kitcher envisages still limits potential contributions of competent laypersons. My concern echoes Douglas's worry about the lack of appreciation for "actual *critical* engagement" (italics mine) between laypersons and experts. Kitcher's examples are very brief, but they all seem to focus on the extension of knowledge in the framework of existing approaches and research priorities. Amateurs are expected to help researchers to do what researchers are doing already. There is no indication that laypersons' substantial contributions may be relevant at the stage of aims-setting, or the stage of certification, or the stage of application. Similarly, there is no suggestion that laypersons may have knowledge that may supplement experts' knowledge.

I suggest that this failure to pay attention to substantial knowledge laypersons may possess and to possible gaps in experts' knowledge has important consequences for the ability of well-ordered science to achieve its aims. Arguing for that, I first discuss the application of scientific results in particular contexts—the topic that is given an important place in Kitcher's approach. I then suggest that what has been called lay expertise may be highly relevant for ensuring this kind of successful application. Accordingly, a failure to recognise and utilise this kind of expertise may make well-ordered science less able to solve the problems Kitcher wants it to solve.

When discussing the failure of science to address many urgent human needs, Kitcher points out that the availability of problem solutions in principle does not translate automatically into effective solutions in particular real-life contexts. For example, solutions to numerous medical problems—vaccines, prevention methods, cures—may be successfully applicable in affluent countries but fail to be usable in poorer countries where different conditions prevail (Kitcher 2011c, 121–122).

The problem with these solutions is not that they do not work outside of restricted laboratory conditions.[66] While the problem of general transferability of results between laboratory and the world is important, it is not a problem for biomedical solutions Kitcher discusses. They do work well and may be highly successful in the context that is taken to be standard, the conditions of an affluent country. (Given Kitcher's discussion of the still limited geographic and social makeup of scientific community, one may suggest that it is also the context with which many scientists are most immediately familiar.) Rather, the problem is that they do not work in many contexts where it would be required, were science well-ordered. In his discussion of well-ordered science Kitcher stresses repeatedly this notion of successful application in a context: "an adequate solution is a statement 'true enough' to enable those who have it to achieve whatever ends made the question significant" and "an adequate solution is one allowing people to proceed sufficiently successfully in the contexts of intended use" (Kitcher 2011c, 105–106). As the discussion of medical solutions shows, these contexts may be different enough so that a particular solution may be satisfactory in some contexts but not in others.

The theme of applicability of a solution in specific contexts is related to a number of general questions about the nature of scientific results and the warrant for their application in practice. In her discussion of well-ordered science, Cartwright (2006) calls this issue the "evidence for use".[67] She suggests that philosophy has traditionally been concerned with the issue of testing scientific results and has not paid enough attention to their use. Once justified, results are believed to be stable and unambiguous, and ready to be applied in this form in any context. Cartwright argues that this is not so: "What justifies a claim depends on what we are going to do with that claim, and evidence for one use may provide no support for others" (Cartwright 2006, 983). Cartwright goes on to discuss a number of cases, from quantum theory to agriculture and epidemiology. She argues that in all these cases the question what would warrant the use of particular scientific results in particular real-life contexts and how far the warrant would stretch is a question philosophers and decision-makers do not know how to start to address. In particular, it is not known how to combine different kinds of evidence, especially the complex and highly context-dependent evidence about particular social contexts where knowledge is used.

[66] Robyn Bluhm (2012) suggests in her review that one of the problems of Kitcher's account is the failure to discuss how in many cases "the answer to the question cannot be applied directly outside of the experimental context in which it was determined". For me, the problem is that solutions that have been successfully applied outside of laboratory may not be usable in other locations beyond the original context of application.

[67] Bluhm (2012) also mentions Cartwright when arguing that Kitcher's confidence about the straightforward applicability of scientific results ignores relevant work in philosophy of science; however, she focuses on Cartwright's work on pluralism.

Cartwright develops the theme of evidence for use in the context of her discussion of causality, or understanding and using causes (see, e.g., Cartwright 2012 and 2013). From this discussion, the importance of knowing the local context where knowledge is to be used emerges. As Cartwright stresses, evidence about factors relevant for the success of application may come from a variety of sources: they include "theory, big and little, consilience of inductions, and *a great deal of local information about study and target situations*" (Cartwright 2012, 988, italics mine). For example, in the case that Cartwright discusses (the failure of the programme to improve children's nutrition in a region of Bangladesh) the relevant information came from anthropological literature.

The recognition of the importance of local knowledge attracts attention to the possibility that the knowledge those familiar with the context of application have can be useful for supplementing experts' knowledge of scientific solutions as controlled in the context of testing. In his discussion of agriculture, Lacey (1997, 45, fn) argues, similarly to Cartwright, that the possibility of a simple transferral of a working solution to a different context cannot be taken for granted. "Experimental spaces" where scientific results are achieved and tested are different from "technological spaces" and even more different from "natural spaces" where results are to be applied. Lacey suggests that long-term sustainable application of scientific results requires involvement of those who know the local condition from practice—such as local farmers—in order to "mediate between experiment and practice".

The issue of knowing the local context may be highly relevant for decisions at all stages of well-ordered science. At the stage of aims-setting, it has important implications for using experts' judgements about significance and probabilities of success as the basis for decision-making. As Kitcher's discussion of vaccines and other medical solutions shows, their success in a particular context does not necessarily mean their success in other contexts; this puts into doubt their significance for the latter. (At the stage of selecting research projects that ultimately resulted in these solutions one would have been correct to argue that the research was both practically significant and likely to succeed. However, it was so only in the context of affluent countries.) At the stage of research certification, this issue has important implications for the evaluation of certification procedures, because deciding whether the methods produce true enough conclusions often enough depends crucially on whether they succeed across the required variety of contexts. As Kitcher's example shows, what counts as a successful solution may vary with the context. Finally, it is important at the stage of application where the decisions about transferring solutions working in one location (in laboratory or in the real world conditions taken to be standard) to a different location are made.

Knowing the local conditions of contexts of application seems highly relevant for well-ordering science. This knowledge in turn can be most naturally expected from those most familiar with these contexts. Such an approach seems to be in line with Kitcher's insistence that people know the circumstances of their lives best. Kitcher, however, limits the contribution of the public to the information about their needs and values. There is no discussion that they could provide substantial information about the local context that may be relevant for experts and may help to fill in gaps in experts' knowledge. As a result, there are no mechanisms in well-ordered science for encouraging this kind of substantial contribution from laypersons. To the degree successful solutions depend on this local knowledge, the failure to gain it may diminish the ability of research to produce solutions that would work (or work as well as they potentially could) in all relevant contexts. The failure to attend to the possibility of such knowledge and its utilisation thus poses a problem for the achievement of aims of well-ordered science. This failure is especially surprising given the recognition of the importance of the context of application in Kitcher's account.

I have argued that the realisation of the aims of well-ordered science may sometimes require supplementation of experts' knowledge with relevant local knowledge that laypersons may have. The failure to use this local knowledge may threaten the success of the solutions researchers propose. To demonstrate how such a situation may unfold in practice, I turn to the example from probably the most famous discussion of lay knowledge—Brian Wynne's (1989; 1992; 1996) study of Cumbrian sheep farmers.[68]

Wynne analyses interactions between the farmers and the researchers working to help them during the aftermath of the Chernobyl accident. Following the accident in April 1986, Cumbria in the north of England was among the areas most affected by radioactive fallout in the United Kingdom. Initially, the British government described the situation as safe. Some weeks later, however, sheep meat from Cumbria showed a high level of contamination by radioactive caesium. After the initial official reassurances that the level of contamination would soon fall, the ban on selling and slaughtering sheep in several areas, including Cumbria, was suddenly issued in June 1986, at first for three weeks. Again, it was claimed that the level of caesium in sheep would quickly fall and the ban would be lifted before the usual sheep selling time arrives in August. The timing was important as livelihoods of farmers depend on the possibility to sell spring lambs in late summer and early autumn—as the usual practice, more lambs are produced in spring that could be sustained on pastures beyond the usual selling time. This prediction about quick decontamination, however, failed and the radioactivity levels remained high; as a result, in July 1986 the ban was extended indefinitely. Over time, the area of restrictions was redefined and reduced and selling was allowed with some restrictions (sheep had to be marked and could be moved but not slaughtered). A considerable number of farms, however, remained under restriction for years.

Interactions between the sheep farmers and the governmental and non-governmental experts were centred on the issues related to contamination, the failure of the initial prediction about its fall in sheep and the solutions for decontamination. Wynne discusses a number of problems in relations between the farmers and the experts, including the issue of trust and the role that previous experiences and general expectations about the appropriate approaches to knowledge played. Some of these problems can be traced back to the experts' failure to take into account relevant knowledge about local conditions and practices. Three examples of this neglect of local knowledge are particularly relevant in light of Kitcher's notion of solutions appropriate for a particular context.[69]

The first of the examples concerns the model that underlay the failed predictions about quick decontamination of sheep. According to the model, caesium would be immobilised by soil and therefore could not be taken up by plants. New uncontaminated grass would replace the grass contaminated by the initial fallout and, as sheep would be eating new grass and would not ingest any more radioactive caesium, the initially consumed caesium in their bodies would decay. This model was established for lowland alkaline clay soils. As it turned out, it was not applicable in Cumbria's highland acid peaty soils: there, caesium remained mobile

[68] There are other arguments that mention Wynne's case study in the context of philosophy of science—see, e.g., Whyte and Crease (2010) and Leuschner (2012).

[69] It is important to stress that here I focus on just one strand within Wynne's analysis. I discuss particular facts that the sheep farmers knew and the researchers did not and that were highly relevant for the solutions the researchers were offering. My use of this analysis is somewhat similar to what Collins and Evans (2002, 238) make of it with their notion of "experience-based" expertise. This caveat is important because Wynne (2003) criticises Collins and Evans for focusing on lay experts' propositional knowledge and ignoring the wider issues about the meaning of the matter at stake, salience of particular questions and particular kinds of knowledge, and the social identities (Collins and Evans (2003) disagree with this criticism). I focus on lay factual knowledge because I consider the failure to take its possibility into account an important problem for Kitcher. I return to the issues of meaning and framings in chapter 6, where I suggest that they provide an additional reason for public participation that is not as restricted as in well-ordered science.

and continued to be taken up by plants. As a result, sheep continued to eat grass contaminated by caesium and the levels of caesium in their flesh increased instead of the predicted decrease. The failure to take into account local conditions led to the failure of predictions—in Cartwright's (2013) terms, the model worked "there" but not "here".

The first example stresses the importance of local knowledge but one may doubt whether the local farmers specifically possessed the knowledge that would help to avoid the problem. (Wynne does stress the farmers' appreciation of local variability compared to the researchers' belief in universal and standardised knowledge.) The following example touches the specialist knowledge of the farmers directly. When it became clear that the contamination level would not fall naturally, the experts proposed that sheep could be decontaminated by moving them from hills to valleys where the caesium levels were considerably lower. While such a recommendation could work in principle, it was not an applicable solution for the farming practices in the local context. The farmers knew, as a part of their experience, that the valley grass was a limited, precious and slowly renewing resource that was necessary for ensuring sheep feeds in winter. Were the sheep moved there early, this resource would be quickly depleted, with negative consequences reverberating for a long time. In this case, what the farmers knew (and could, in principle, explain) and the experts did not, had direct implications for the success of the experts' recommendations.

Another solution proposed by the experts was similarly inapplicable due to the failure to take into account local practices. A series of experiments was organised to test the ability of the mineral bentonite to absorb radioactive caesium from the soil and plants. The mineral was spread at different concentrations in separate locations and sheep grazing on these locations were tested and compared with control groups from non-treated locations. In order to proceed with the experiment, the plots treated with bentonite had to be surrounded with fences and the sheep kept inside. However, as the farmers could have pointed out, sheep were used to roaming freely. Keeping them confined made their condition deteriorate, undermining the experiment. It was precisely for this reason that the experiment was later abandoned, although the farmers' knowledge was never openly acknowledged.

Wynne's study has important implications for well-ordered science. It shows that sometimes the kind of results that well-ordered science is supposed to deliver—solutions that are immediately relevant for urgent human needs and successfully applicable in the context where these needs arise—may be impossible without taking into account local knowledge. Experts cannot be expected to have this kind of knowledge in every case due to its connection with specific locations and practices. Also in this case, the dependence of knowledge on the location and experience means it is not a shortcoming to be overcome by following the ideal of objectivity better. What a solution would require is to look for knowledge that may be available, if habitually ignored—local knowledge of laypersons that have relevant experience. As my argument shows, taking this local knowledge into account may be necessary for the success of well-ordered science.

Kitcher, however, does not seem to recognise that some groups among the public may possess considerable specialist knowledge. Instead, he tends to contrast the experts and the ignorant public: "scientists who have thought hard about the predictive and explanatory successes and failures are in a better position to judge than people who do not know these things" (Kitcher 2011c, 140) and, accordingly

> if you ask whether a group of well-trained researchers, thoroughly familiar with the details of the issues, proceeding in the patient—indeed, ideal—way described will be *more likely* to be right than an uninformed public, the answer seems obvious: even if you cannot be sure, you know where to place your bets. (Kitcher 2011c, 219, italics in the original)

Wynne’s case studies suggest that the public may not be uniformly uninformed; that some members of the public may possess specialist knowledge that has similarly been acquired as a result of hard and patient practice; that explanatory and predictive successes cannot always be easily transferred to a new location; and that details necessary for success may be local. As the failure of the predictions and interventions proposed by experts (and the resulting damages to the farmers’ livelihoods and their trust in scientific and governmental institutions) shows, ignoring these factors can have a high cost. I have argued that Kitcher’s approach to expertise does not acknowledge or address this issue.

4.3.3.4 An escape path?

In the preceding subsections, I discussed how experts may not always be in the position to see ways in which scientific concepts carry traces of past interests and values, so that concepts used when addressing the needs of a group may reproduce and reinforce values that may be harmful for the group. Similarly, I discussed how the successful solution of the group’s problems may require knowledge of the local context that experts may not have. I suggested that laypersons may sometimes have knowledge or perspectives that could supplement, correct or modify experts’ knowledge. I argued that the failure to put this knowledge and perspectives to use constitutes a serious problem for Kitcher’s model of well-ordered science.

As a response to my criticism, one may point out that it depends on a very restrictive interpretation of communication that Kitcher’s model presupposes. It is true that at its centre is the communication of the needs of the public. One may nevertheless suggest that this communication may be rich enough to convey relevant contextual information or relevant perspectives. After all, one of the points that Kitcher stresses when discussing the need to let people speak for themselves is that even the most perfectly well-meaning outsider knows their needs worse than they do. As an example, Kitcher tells the anecdote of the scientists’ offer to develop a vaccine for children in an African tribe. After some consideration, the representatives of the tribe asked for a vaccine for their goats instead: without the herds, the children would not survive in any case (Kitcher 2007b, 180 and 2011c, 118–119).

Actual conversation between experts and the public is necessary in order to avoid preconceived notions of this kind. One may suggest that it may similarly help to expose problematic values inherent in experts’ categories or help experts to become familiar with relevant facts about the local context. If a free conversation that allows the transmission of this kind of information is encouraged, concerns about the restrictions placed on the public are alleviated. Even if there are no dedicated practices or institutions for utilising lay knowledge or critical perspectives, there is the fundamental commitment to the substantial public involvement with experts that can make experts recognise and use this knowledge and perspectives.

Interpreting communication between experts and laypersons in this extended sense seems to offer an escape path from the criticism I have presented. The aim of the next section is to show that this escape path is blocked. In the section, I argue that despite the declared commitment to public participation, Kitcher’s arguments pull away from actual participation and towards a hypothetical reconstruction of public needs. This means that there is no actual conversation that could help to supplement experts’ knowledge or improve their concepts; the problems I have identify stand. This also means an important internal contradiction for Kitcher’s democratic account.

4.3.4 Public participation

The aim of this section is to discuss a number of problems I identify in connection with the theme of public participation in Kitcher’s account. The theme is important for Kitcher, given the crucial role of deliberative conversation in well-ordered science and Kitcher’s commitment to democracy. It also seems to be a natural extension of Kitcher’s discussion of the importance of social and interactive aspects of scientific community. Nevertheless, I show that Kitcher's approach to public participation is deeply problematic. I argue that some elements of his account make public participation in well-ordered science seem both impossible to succeed and ultimately unnecessary.

I begin by describing some important aspects of Kitcher's approach to epistemic agents and their interactions within scientific community. Against this background, I show how Kitcher's approach to public participation differs. As a result, considerations that support actual inclusiveness and social interaction in scientific community are not applicable to the case of public participation. Kitcher's characterisation of the actual public further supports the impression that involving the public would exacerbate problems of science in democracy instead of addressing them. As a result, the motivation to widen public participation is weakened.

In the second step of my argument, I focus on one requirement of Kitcher's ideal conversation—the requirement of "real needs"—as the starting point to argue that actual public participation in well-ordered science may not be necessary. If members of the public are mistaken about their needs, involving them in discussion may be less effective than attempting to reconstruct these needs hypothetically. I discuss some ways in which this hypothetical reconstruction already plays a prominent role in Kitcher's argument. The possibility to forgo public participation without a substantial loss further undermines the motivation to actually institute it.

I then return to the argument of the previous section and suggest that in the absence of motivation to encourage public participation, the possibility to utilise lay knowledge and critical perspectives as a part of inclusive conversation is precluded. The problematic view of public participation thus has important negative consequences for the ability of well-ordered science to fulfil its functions. I also discuss how it creates important tensions with the characterisation of Kitcher's account as democratic.

Beginning with *The Advancement*, an important feature of Kitcher's account has been the idea that an epistemically successful enterprise can be sustained by activities of epistemically imperfect agents. The very starting point for Kitcher's philosophy of science is the view of science as constituted by activities of "cognitively limited biological entities" (Kitcher 1995a, 9). His compromise model of the closure shows that these cognitive limitations may be considerable. At the beginning of a debate, none of the arguments may provide the complete solution and none of the participants may be able to formulate, or even to grasp fully, the ultimately winning argument. Simply following the development of the debate and keeping all the relevant considerations in mind may at times exceed even the most gifted participants' abilities. In his discussion of the debate that ended with the general acceptance of Lavoisier's new chemistry, Kitcher (1995a, 290 and 2003, 40) describes the difficulties associated with keeping all partial solutions and relevant constraints accounted for. The difficulties were so great that Lavoisier sometimes forgot his own earlier arguments. It is the way science is practiced as an interactive social enterprise that allows cognitively limited individuals to contribute to the whole of remarkable epistemic success and progressiveness.

The agents in questions are also imperfect when it comes to their morals as researchers. One of the main conclusions of Kitcher's (1990; 1995a) analyses of scientific decision-making is that "epistemically sullied" agents, who act on the basis of personal interests, often achieve a better distribution of effort than "epistemically pure" agents. So, not only the individual's ability to act "epistemically virtuously" may be limited but scientific community as a whole may benefit from these limitations. This, in principle, allows designing a form of organisation of science without appealing to the "epistemic moral sense" of individuals at all and focusing instead on working out a suitable system of incentives for self-interested individuals. (In *Science in a Democratic Society*, Kitcher adds establishing norms and cultivating a particular moral stance on cooperation to this market approach, but the latter still plays a role.) Again, and even more prominently, it is acknowledged that epistemic agents have limitations and it is shown how these limitations may be put to good use in a social system.

There is another dimension of researchers' character that is approached in a similar laissez-faire spirit. Kitcher (e.g., 1995a) points out that it is good to have cognitive variation in scientific community. This variation may happen along different dimensions. Some researchers may prefer safer approaches, others may be willing to take risks by following less certain ones; some may be fast when adopting conclusions, other researchers may be slower; some may tend to join the forming consensus and others may have the makings of a resisting maverick. When Kitcher discusses these variations, there is no argument that the origins of these varied strategies matter. A researcher may prefer a high-risk strategy due to the personal belief in its promising character, the general inclination to take risks or the hope to receive credit; it does not matter for community as long as the beneficial distribution of labour in community is sustained.

In *Science in a Democratic Society* Kitcher discusses a more general kind of variation—the variety of perspectives, approaches and ways of seeing particular kinds of evidence as salient. He connects this kind of cognitive diversity with gender and social diversity in community. Increasing the number of women and members of other previously underrepresented groups in scientific community may benefit it by introducing novel perspectives and approaches. Kitcher (2011c, 200–201) stresses that his proposal does not involve any assumptions whether these different perspectives reflect some "authentic" characteristics of members of these groups. Just as in the case of risk-taking and risk-avoiding researchers, the underlying causes are not important. What matters is that, for example, growing numbers of women scientists do at least sometimes mean a wider variety of approaches, changing the field (Kitcher's example is primatology). Also in this case, science is seen as a social system that can put to good use individual variation, no matter what the factors behind it are. Therefore, individual researchers are not required to analyse and discipline their motives or to search for the "real" and "deep" ones.

I have thus suggested that Kitcher's approach to the social nature of scientific community does not require scientists to overcome completely their limitations. They are not required to be impeccable either cognitively or "epistemically-morally". There is no requirement that the individual's contribution to the collective enterprise be perfectly attuned to the interests of the entire community, or motivated solely by epistemic aims, or by the "real nature" of the researcher. Instead, it is shown how individual imperfections are put to good use on the community level.

This lack of restriction on what researchers and their contributions are expected to be stands in sharp contrast with the demands that *Science in a Democratic Society* places on the contributions of lay deliberators. In Kitcher's well-ordered science the results of deliberation are ideally such as to be defensible in the ideal conversation. Accordingly, the wishes on the resulting list of preferences are subject to two kinds of limitations, epistemic and affective. As previously mentioned, the epistemic requirements mean that participants must not rely on false beliefs about the natural world, they must have true beliefs about the consequences of their choices and they must have true beliefs about the wishes of other participants. These conditions place a considerable cognitive burden on the individual. Yet there is no discussion of cognitive limitations of deliberators.

The discussion of the affective conditions—the requirement that others' needs be given equal weight to one's own—similarly differs from the discussion of scientists' “epistemic morals” as it places considerable moral burden on each individual. In the case of scientists, it is recognised that individuals are not typically “epistemically pure” and it is the suitable social system that enables good use of their self-interested behaviour. Even when developing the idealised game-theoretical models of *The Advancement*, Kitcher does not idealise the model agents in the sense of expecting them to be impeccable. In the case of the ideal conversation, on the other hand, the affective conditions are meant to apply to individuals and failures of sympathy are seen as an important failure on the part of individuals that is to be remedied (see, e.g., Kitcher 2011c, 129–130).

One concern that these requirements for deliberators immediately raise is the question of cognitive and moral possibility. Could “cognitively limited biological entities” ever approximate results that would emerge in the ideal conversation in accordance with these conditions? Kitcher discusses how Lavoisier would sometimes lose the way in the development of his own argument. There is no comparable discussion of difficulties participants face in the development of the humankind-wide deliberative conversation of extreme complexity. In this deliberative conversation, there are numerous human needs, facts about the world, knowledge about potential outcomes and relative weights of various needs and values that have to be taken into account. Similarly, one may doubt whether an individual is able to realise the demanding moral requirements placed on the participants of the conversation. There is a considerable gap between what is expected from ideal deliberators and what individuals as conceptualised in other Kitcher’s arguments—“cognitively limited biological entities”, “sullied epistemic agents”—may be expected to deliver.

The contrast between Kitcher’s usual approach to human agents and the requirements of the ideal conversation raises doubts about the ability of actual laypersons to fulfil successfully the role of the deliberator. These doubts are further reinforced by Kitcher’s pessimistic view of the promise of actual free discussion in the sphere of science and science policy. Kitcher expresses important reservations about the ability of free discussion to let the truth win already in Kitcher (1997a), arguing that in practice the debate is likely to be skewed by biases (for example, the evidential support for hypotheses that accord with ingrained prejudices is likely to be overestimated). In *Science, Truth, and Democracy* Kitcher suggests that trying to realise well-ordered science is likely to fail because the conversation of real human agents would fail very much short of what the ideal requires. In Kitcher’s words,

> Quite probably, setting up a vast populationwide discussion that mimicked the ideal procedure would be an extraordinarily bad idea, precisely because transactions among nonideal agents are both imperfect and costly. (Kitcher 2003, 123)

Throughout *Science in a Democratic Society* Kitcher warns against the “shibboleth of ‘free discussion’” (Kitcher 2011c, 178), arguing that making a scientific disagreement the subject of public debate is likely to be extremely counterproductive. Ignorance and denial are ubiquitous (concerns about the public ignorance and resistance are also expressed in Kitcher 2006; 2010; 2011b); institutions for the transmission of information are often biased. As a result, the victory of truth is unlikely. Rather, in many cases the opposite happens and the credibility of proper experts suffers. So, unrestricted public participation is not expected to bring any benefit in the discussion of substantial scientific questions. On the contrary, it is expected to make the situation worse.

Kitcher is similarly sceptical about the perspectives of public participation in the conversation about values. Discussing the possibility to put the ideal of the ethical conversation into practice, Kitcher points out that it has no chance to succeed due to inevitable affective and cognitive failures:

> any attempt to orchestrate even a sample of voices representative of the diverse perspectives of living people would produce a vast cacophony, in which the divisions and distortions produced in our history would doom any chance of serious discussion. (Kitcher 2011c, 51)

Kitcher's view of the public's ability to provide a valuable contribution to actual conversation is thus remarkably dim even in the case of the supposedly universal ethical conversation.

Many of Kitcher's reviewers comment on this view of the public. For example, Mark Brown writes about Kitcher's "intense scepticism" (Mark Brown 2013, 394) about the public ability to take part in discussions of science policy. Some critics go further than that. In the review of *Science, Truth, and Democracy* Stephen Turner calls Kitcher's position a "striking expression of contempt" (Turner 2003, 601) for the rationality of the public and the institutions of democracy and Yoshida comments on Kitcher's "intellectual arrogance" (Yoshida 2012, 371) with respect to the public. Other criticisms put into doubt the factual accuracy of Kitcher's views: for example, Mark Brown (2004, 84–85) points out the considerable number of analyses of public participation and activism showing that members of the lay public can make reasonable decisions about science policy matters. Jasanoff (2004, 152–154) criticises Kitcher for constructing a "straw man" of the public that blindly resists science, and for failing to pay attention to the research that shows more nuanced public attitudes.

Kitcher's attitude towards the public seems problematic, given the democratic ethos of his proposal (not to mention indications that his view of the public may be inaccurate). In addition, I want to point out that this attitude of Kitcher's further widens the gap between what Kitcher expects from the actual public and what is required in the ethical conversation. This, in turn, puts into doubt the feasibility of actually involving the public in decision-making in science. What would be the point of public participation, if the public would not be able to deliver what is required? The way Kitcher sets the ideal on the one hand and describes the actual public on the other works against the idea of increasing actual public participation in science policy.

One may nevertheless suggest that public participation remains necessary for generating inputs into well-ordered science. On this view, public participation, no matter how problem-ridden, is indispensable for Kitcher's model. Kitcher's proposals to institute small-scale approximations to the ideal conversation and to draw on experiments attempting to improve public deliberation, such as deliberative polling, may be seen as a further confirmation of this commitment. If public participation is indispensable (and if the public's abilities are so dismal), it is natural to try to make it better. Conversely, there would be no point in trying to learn "How?" if there were no need to actually involve the public in well-ordered science. In the second part of my argument I suggest that another aspect of Kitcher's approach makes public participation to a considerable degree unnecessary.

I have already described the gap between the requirements of the ideal conversation and the characteristics of human agents in Kitcher's account. I suggest that the difference between actual persons' perspectives and contributions expected in the ideal conversation is made even more drastic by Kitcher's stress that needs in question must be "real" or "deep" needs. Kitcher stresses repeatedly that persons may have strongly felt needs without realising that they contradict other, more important needs. Kitcher's (2011c, 128) main example is the situation where one wishes to continue habitual ways of using natural resources without realising that they contribute to the global climate change, dramatically worsening life conditions for one's children and grandchildren. Yet, it is the good life of one's children and grandchildren that according to Kitcher constitutes one's real wish.

The notion of real needs demonstrates again the differences in the ideal set for researchers within scientific community and deliberators in the ideal conversation. In these two cases, Kitcher approaches the similar issues of the relation between individuals' contributions and the community's collective position in very different ways. In the case of scientific community, Kitcher focuses on what scientists, as they are, do in fact contribute. A scientist may have a particular perspective due to her experience as the first generation immigrant woman; one may prefer risky strategies hoping to be the first thanks to choosing a less-trodden path; one may choose to remain a maverick due to the birth order in one's family etc. An important aim for Kitcher's arguments about the social nature of science has been to show how the individual variation of this kind can be epistemically beneficial. This, in turn, supports the argument for increasing diversity in scientific community.

I suggest that Kitcher's treatment of contributions of non-scientists differs radically from his accepting "come as you are (and we will build a suitable system of incentives, norms and institutions to use your contributions)" attitude. As a result, there is no unambiguous support for actual public participation. Due to the cognitive and affective conditions and the distinction between deep and superficial needs, the input ideal deliberators are to provide is less about what they actually can say than it is about what they could potentially say, were all the conditions fulfilled. Given Kitcher's focus on deep wishes and the worthwhile life as the ultimate aim of the ethical project, refining wishes that will serve as the basis for aims of well-ordered science becomes to a large degree an exercise in reconstruction. Ultimately, what decision-makers in well-ordered science have to know is the needs that have to be satisfied in order for everyone to have a worthwhile life (even if the majority fails to recognise them due, for example, to consumerism). As Henry Richardson (2014, E109) comments on this aspect of Kitcher's proposal, Kitcher "seems unwilling to trust the specification of public ends to actual citizens", instead postulating their fundamental needs as the basis of science policy. Kitcher's idea of the "index of human needs" (Kitcher 2011c, 129) further supports this view of the input for well-ordered science as something that can be abstracted from actual agents and actual conversations in which they could take a part.

So, unlike the input expected from members of scientific community, the input from ideal deliberators is at a considerable remove from what actual individuals could bring to discussion. This, in turn, opens the possibility to rely on reconstructing positions that should be taken into account, instead of inviting actual agents into discussion. This kind of reconstruction is openly used to address some issues for well-ordered science with its universalist ambition. Ideally, well-ordered science requires taking into account perspectives of those who cannot speak for themselves. Above all, it concerns future human generations, but it is also the case for animals and for persons who cannot speak for themselves for various reasons (see, e.g., Kitcher's discussion of moral limitations on research—Kitcher 2011c, 131–133). Accordingly, a considerable part of needs to be taken into account in well-ordered science is in fact reconstructed by others.

Kitcher does stress repeatedly that one is best able to represent one's needs. Accordingly, actual participation seems to be the desideratum for those who can in fact speak for themselves. Nevertheless, the stress on taking into account wishes as they should be rather than as they are may push towards increasing the proportion of theoretical reconstruction of wishes and needs. This tendency is in a mutually reinforcing relation with a persistent feature of Kitcher's approach upon which many of his critics comment—the priority of the right results over the right procedures. As discussed previously, the first version of the ideal of well-ordered science is formulated in such a way that it may in principle be realised without any actual public participation—"the challenge is to find institutions that generate roughly the right results" (Kitcher 2003, 123). So, if the right results may be produced by the benevolent and wise "philosopher-king" or an elite group of experts, it may on this view be an acceptable basis for the well-ordering of science. I suggest that the possibility of a "philosopher-king" doing all the work required continues to haunt Kitcher's approach even when he discusses approximating institutions or democratisation of science (as Kitcher does in Kitcher 2011c).

Kitcher sometimes writes as it were possible to identify in abstracto what the wishes of the public would be, had they undergone the procedures of well-ordered science. So, Kitcher diagnoses the problem of False Consciousness in connection with the Human Genome Project. The project is marketed as a source of medical benefits whereas one should more realistically expect improved scientific understanding in biology and whereas these more modest promises are in fact sufficient to justify the project. Kitcher then suggests that

> If the significance graphs for the pertinent fields were clearly articulated and their historical development explained, it is quite possible that the wisdom of the strategy would be evident and that the inquiries envisaged would accord with the tutored preferences of the citizens whose taxes support the genomes project. (Kitcher 2003, 131)

Despite the tentative "possible", the next sentence is more confident: "the research agenda actually pursued adequately represents the preferences people would acquire as the outcome of ideal deliberation". It seems that according to Kitcher it is possible to know, at least in some areas of research, what the tutored preferences would be, without actually involving the public or establishing "approximating" institutions.

The entire final chapter of Kitcher (2011c) is dedicated to a discussion of the positions of well-ordered science on several issues. Some of these issues are large-scale (what would be the position of well-ordered science on the development of genetically modified organisms?); others are relatively specific (what would be the position of well-ordered science on the development of reproductive technologies that would allow two women to have a biological child?). Although Kitcher stresses that these are just proposals, this very exercise supports the idea that the input for decision-making can be, at least sometimes, generated by this kind of abstract reconstruction of what the real needs of various groups comprising humankind are. As Mark Brown (2013, 393), who also comments on this peculiarity of Kitcher's position, remarks, despite the insistence on the open-ended character of the required deliberative conversation, Kitcher reaches some very specific recommendations for science policy.[70]

In the first part of my argument I suggested that the cognitive and affective conditions and the requirement of authenticity are impossibly demanding for human agents as Kitcher usually describes them in the context of scientific activities or as he characterises the actual public in contemporary democracies. This makes the usefulness of involving actual laypersons for the realisation of this ideal questionable. Now I have argued that the remoteness from what individuals want in practice in favour of what they would want under the ideal conditions undermines the rationale for encouraging actual participation of the public in deliberations of well-ordered science. If it is possible to formulate the directions for well-ordered science in abstraction, what are the reasons to involve the public at all?

It is important to be precise about the conclusions my argument establishes. Kitcher never argues against the public participation in principle. Many elements of his argument—the ideal of the panhuman deliberative conversation; the commitment to the democratisation of science; the proposal about instituting small-scale groups of deliberators; and the hope for political science research to make these groups better—all demonstrate Kitcher's commitment to public participation. What I have attempted to show is that other elements of Kitcher's

[70] So, there is a tension between the way Kitcher characterises his account—the open-endedness on which Pinto (2015) comments—and the particular proposals he makes. These proposals may be taken as a guidance when approximating the ideal of well-ordered science—for example, we would need institutions that would reach such and such position on genetic engineering—thus somewhat assuaging Pinto's worry about missing institutions in Kitcher's account. I suggest that these proposals create a more fundamental problem for Kitcher's approach due to their tension with the purportedly democratic character of Kitcher's approach.

account pull in the opposite direction. The ideal conversation does not function the same way that division of cognitive labour in scientific community does, so there are no reasons to increase diversity and inclusiveness in the same way. The actual public Kitcher describes does not seem to be able to satisfy the conditions of the ideal conversation. As a result, the motivation to limit actual participation is strong, particularly as the priority of "real" needs and the possibility to reconstruct them in an abstract way make public participation seem dispensable. These tendencies reinforce each other. The further is the actual public from ideal deliberators, the stronger is the temptation to rely on hypothetical reconstruction of its needs; the more needs are reconstructed in abstracto, the higher the expectations the public has to satisfy to make its participation worthwhile are. I suggest that these tensions are serious enough to make one question the stance of well-ordered science on democratisation of science.

At the end of the previous subsection I suggested that the question of public participation is connected with the themes of lay knowledge and conceptual criticism. If there is no actual involvement of the public, including not just representatives of the public in general but also members of the local publics who have relevant lay expertise, there is no possibility that experts improve their knowledge as a part of the conversation about the public's needs. The stronger is the reliance on the hypothetical reconstruction of the "real needs" of the public, the more serious the possibility of failing to address these needs in the optimal way due to the unchallenged use of problematic concepts, or the failure to take into account relevant local knowledge is. Actual conversation with the public was proposed as an escape path for the problem situation described in the previous subsection. The inherently controversial approach to public participation in Kitcher's well-ordered science blocks this path.

Mark Brown's (2009) discussion of representation shows another problematic aspect of Kitcher's reliance on hypothetical representation. Such an approach falls back on a very traditional notion of representation. As previously mentioned, Mark Brown (2009, 7 and 18–19) argues that in parallel to the idea that science provides a mirror to nature, there exists the idea that political representation provides a mirror to the popular will. In both cases, no public participation is necessary either in science or politics. I have already suggested that these intertwined traditional ideas of representation cannot be easily combined with Kitcher's account of science and ethics, which shows that neither scientific nor ethical inquiry simply mirror existing reality. The treatment of public participation further exacerbates this general problem of the coherence of Kitcher's approach to science.

The problem of public participation in well-ordered science thus has important implications for the ability of well-ordered science to satisfy the public needs Kitcher wants it to satisfy, and for the overall plausibility of Kitcher's approach. In the concluding part of my argument I suggest that it also contradicts the general democratic spirit of Kitcher's proposal. Doing so, I draw on Mark Brown's (2004) discussion of Kitcher's arguments.

In his review of Kitcher's *Science, Truth, and Democracy*, Mark Brown (2004, 85–88) indicates several ways in which it is in tension with the themes that are at the centre of discussions of democracy. Mark Brown traces three of these problems back to Kitcher's focus on outcomes rather than procedures. First, the possibility to represent the public's wishes in a highly idealised way is inconsistent with the idea that representatives should both promote their constituents' interests (which may allow some abstract reconstruction) and be responsive to the actual expressions of citizens' wishes. Second, this abstract approach makes it impossible for the members of the public to learn to participate in science policy by actually participating in it. However, it is only this learning that could lead in future to better outcomes of public participation. Third, it excludes secondary benefits of participation such as the improvement of citizens' knowledge and skills, which may not be directly necessary for improving science policy but seems desirable, given Kitcher's wider democratic orientation.

Kitcher's argument in *Science in a Democratic Society* shows some acknowledgement of the previously ignored aspects of democracy. When discussing the concept of worthwhile life, Kitcher (2011c, 55–56) stresses the importance of human connections and human relationships. Human relationships also take centre stage in Kitcher's discussion of democracy: in the chapter on democracy (Kitcher 2011c, ch. 3), Dewey's ideas about democracy as a way of life with the focus on participation and shared experience feature prominently. Actual participation is thus an important part of Kitcher's view of democracy. It is important that people are involved in the process of collective deliberation and decision-making, just as it is generally important to cultivate people's involvement with each other as a part of any worthwhile life.

Given this commitment, the possibility that well-ordered science may function well without actual public involvement introduces a considerable contradiction into Kitcher's account. I suggest that this problem has actually become more serious with the development of Kitcher's arguments over time. Due to the introduction of the notion of real needs and the scepticism about the public, the pull away from actual public participation and towards the hypothetical representation of needs becomes stronger in *Science in a Democratic Society* than it was *in Science, Truth, and Democracy.* To the degree actual participation is downplayed, the problems described by Mark Brown continue to plague Kitcher's latest arguments. (In his review of Kitcher (2011c), Mark Brown (2013) argues that despite some evidence to the contrary, Kitcher still seems to support "hypothetical" deliberation rather than actual public participation.)

The lack of substantial public participation has a number of negative consequences for the political side of Kitcher's proposals. It threatens the values of sociality and human interaction that Kitcher stresses in his discussion of good life and it his references to Dewey. Matthew Brown (2012) even calls Kitcher's account quasi-Deweyan, arguing that for a Deweyan account actual communication and cooperation would take priority over the ideal results. The tensions between Kitcher's approach and usual approaches to the benefits of democracy threaten the possibility to call Kitcher's account democratic or to use it as the basis for democratising science. Why would one feel motivated to democratise science by well-ordering it, if it would not bring any of the benefits usually associated with democratisation? The dispensability of public participation also puts into doubt the possibility of productive cooperation between philosophy of science and research in political science. If well-ordered science can be achieved on the basis of hypothetical reconstruction of the public's wishes, learning how to improve public deliberation becomes unnecessary. Finally and most generally, one can say that there is certain irony when a social account of science concludes that science does not really need to be social in some evident sense of the word. Given that *democracy* features in the titles of two of Kitcher's books, the fragility of democratic participation in his account makes the failure to be social in this sense especially noticeable.[71] Previously, I have shown that in Kitcher's case this problem has implications for the practical successfulness of well-ordered science as well.

4.3.5 A diagnosis and an alternative

Talking about what I called the "careful restrictions" in Kitcher's account, I suggested that they can be explained by Kitcher's desire to avoid two kinds of tyranny, both of which are exposed as indefensible by his account of science, ethics and democracy. Given the way

[71] I later discuss Biddle (2009) that relies on this kind of irony, arguing that Longino's presumably social account of objectivity does not require community.

science is permeated with values and given the origin of values in the ethical conversation, elitism—leaving these value-related decisions in the hands of experts (or experts and their financial backers)—becomes unsustainable.[72] Kitcher discusses several approaches to the organisation of science that he considers elitist, from Francis Bacon's Solomon House and Vannevar Bush's *Endless Frontier* report (in Kitcher 2003, ch. 11) to Plato's kallipolis (in Kitcher 2011c, 22–24), as one of the unsatisfactory extremes that well-ordered science escapes. Another unsatisfactory extreme—vulgar democracy—is avoided even more decidedly than elitism. (Despite the rejection of elitism, Kitcher concedes that if any single group should make decisions about research on its own, researchers would be the best choice—Kitcher 2011c, 118.) Given the role the system of public knowledge is to play in democracy and given the aim of ensuring equal chances to live a worthwhile life, capricious, egotistical or poorly informed lay perspectives cannot be admitted as the basis for decision-making in science.

Concerns about both extremes seem eminently reasonable—indeed, they seem to be the two options any account of science and democracy that values both would want to avoid. Yet, as I argued in the previous three subsections, the way Kitcher addresses the problem threatens the practical success of well-ordered science, imposes implausible obligations on both experts and the public, undermines the democratic ethos of Kitcher's proposal and reintroduces the problematic traditional notions of disinterested experts, ignorant laity and representation as mirroring. The aim of this section is to suggest that there is an alternative way to address the problem of vulgar democracy and to discuss some considerations that might have made Kitcher prefer his problematic solution.

The problem of vulgar democracy, or untutored majority vote, features prominently in Kitcher's discussion of well-ordered science. He considers it highly likely that vulgar democracy would result in the "tyranny of the ignorant" (Kitcher 2003, 117), where projects of genuine epistemic worth may be neglected and the choice may be dictated by impulse or ignorance instead of being a reasonable decision made to address genuine needs. This concern is further reinforced by Kitcher's pessimistic views of unregulated free speech in the public sphere that I have previously described. Kitcher is concerned about the "cacophony" (Kitcher 2011c, 51) that could result from an attempt to bring together various perspectives directly in the conditions of an unregulated speech arena. Cacophony constitutes an important threat even if all perspectives involved are defensible and factually accurate. In practice, such a discussion in the public arena is often subject to massive ignorance of participants, massive distortions of communication, and systematic biases that influence the interpretation and assessment of claims. As a result, accurate factual claims and defensible and relevant perspectives are likely to be drowned by ignorant, biased and self-interested ones. Vulgar democracy and unregulated public debate would thus be detrimental to interests of both research community and society.

Both these concerns are relevant for any proposal where public input and discussion between heterogeneous points of view play an important role. If anything, these concerns are even more pressing for the approach that I began to introduce in the previous subsections—the approach stressing that substantial critical contributions from laypersons may be desirable. I have argued that the involvement of different forms of certified and lay expertise and various perspectives may in some cases be necessary to achieve what well-ordered science attempts to achieve—to produce scientific solutions to urgent problems in an effective and trust-inducing way. As the number of potential participants grows and the nature of potential

[72] Kitcher (2003, 133) distinguishes two types of elitism: the internal elitism of experts and the external elitism where experts are joined by those who provide funding for their research. Both versions are inappropriate, once the nature of epistemic significance is recognised.

contributions becomes more diverse, however, the possibility of the discussion descending into chaos also increases.

I suggest that there can be two broad strategies for addressing this threat of cacophony. I tentatively call these strategies “local-level” and “universal”.[73]

One approach for improving the chances of a productive discussion where relevant and defensible positions are heeded to and attempts to hijack the debate blocked is to focus on creating local conditions that favour such a discussion. This is what Longino aims to achieve with the criteria she offers for judging community’s ability for transformative criticism (Longino 1990b; 2002a). These conditions may include improving channels for communication and ensuring access of different parties to them, agreeing on some shared aims, working out shared norms—in particular, norms to decide what counts as a relevant criticism and an adequate response. The norms concerning relevant criticisms are important in order not to close the debate for potentially relevant critical perspectives. The norms concerning appropriate responses are important for preventing the possibility of a party hijacking the discussion by repeating objections that have already been addressed adequately—the possibility that both Kitcher and Longino consider an important threat (see, e.g., Kitcher 2011c, 221–222; Longino 2002a, 132–133). These norms may be (and should be) open to change—for example, the relevance of certain perspectives may only become clear in the course of the debate. Such openness, in particular, is important for avoiding the re-emergence of elitism in the situation where participation is rigidly limited. Nevertheless, the recognition of some norms of this kind as the starting point is likely to be necessary for making an inclusive discussion work well.

The necessity of a shared basis of relevant knowledge and norms, however, means that this approach leads to numerous local solutions—numerous local Millian arenas, one could say—rather than one general solution. This limitation of scale is due to the fact that a complex of shared knowledge and norms rich enough to support productive discussion is likely to be non-universal and specific to a particular context, issue or group.

Kitcher himself discusses some examples of well-functioning local Millian arenas and stresses their non-universal character. As Kitcher suggests (Kitcher 2011c, 204), discussion among scientists “behind closed doors” may on many occasions provide the benefits that are traditionally expected from free discussion—improved knowledge of truth and its justifications. This, however, is only possible because scientists possess the extensive shared knowledge required. Kitcher writes in terms of “understand[ing] the substance of the debate” but one could also add the understanding of the common aims and standards of this debate. Elsewhere Kitcher (2011a, 371–372) mentions local “ethical conversations” where people can participate in the discussion of local problems with immediacy and equality reminding of the original ethical project. Again, one may suggest that the shared context and existing shared norms and aims are crucial for the success of these conversations.

Thus, there are important elements of the local approach in Kitcher’s account. Some of the improvements he proposes are also local. For example, Kitcher stresses (Kitcher 2011c, 216) that it is not possible to decide what kinds of diversity in scientific community would be beneficial, in abstracto—one has to attend to local cases. More generally, Kitcher suggests that looking for improvements in particular cases could be the task for the “twenty-first century social philosopher” (Kitcher 2006, 1221). Discussing the use of models in biology as

[73] Kusch (2013, November) criticises Kitcher’s approach to the organisation of science as an attempt to present a context-independent account of the aims of science; the opposite of it is contextualism that Kusch defends. There are some important common targets for Kusch’s criticisms and mine: in particular, Kitcher’s mistrust of the public and unregulated public discussion. For my argument, I prefer the contrast “universal-local”, because I stress the difference between organising science as a whole and organising specific local communities.

an example for philosophy of science, Kitcher (2002b, 267) proclaims that the ambition to envisage the universally optimal form of organisation of science is inadequate in principle:

> Just as the actual work of biologists concentrates on local understanding of the traits of particular groups, so too the theory I'm proposing would proceed piecemeal, looking at particular problem contexts that strike us as offering opportunities either for understanding or reform, trying to discover the best ways of achieving our goals and appraising the merits of various social institutions that can be envisaged.

In the subsequent paragraphs I argue that, even so, Kitcher's approach to solving the general problem of science in democracy with the help of the model of well-ordered science reflects a very different method.[74] In Kitcher's panhuman conversation, ensuring orderliness does not rely on local context-specific norms. Instead, it is made possible by carefully restricting what participants in discussion are expected to contribute. In well-ordered science, experts are to provide tutoring in an objective and disinterested way. Representatives of the public are to give information about their deep needs, subject to the cognitive and affective conditions. As a result, the variety of initial spontaneous wishes is reduced and idiosyncratic wishes are filtered out. In the process, the relevant information is decoupled from local contexts or particular human agents. Ultimately, what deliberators have to know is the "atlas of scientific significance" and the "index of human needs": both objective, unambiguous, universally valid and it principle detachable from those compiling them. Participants in deliberative conversation are subject to norms that are as universal and detached from local contexts—the general requirement of disinterestedness for experts and the cognitive and affective conditions for deliberators. As a result of this remove from the local and the careful regulations for contributors, the number of interactions between the parties—potentially "imperfect and costly"—and the possibility of disorder during interactions are greatly reduced, making discussion more orderly and tractable. These interactions may even be reduced to zero if the hypothetical reconstruction of real needs is involved. Such a universal approach also avoids the problem of potential conflicts between locally well-functioning but not integrated Millian arenas that the local approach faces.

The cost of this universal solution, however, is the remoteness of contributions allowed in the discussion from what actual participations could offer, caused by the strict criteria these inputs are to satisfy. Deliberators' inputs in this model are not what actual laypersons could contribute but rather a highly idealised and abstract reconstruction of their interests subject to the constraints of the ideal conversation. In Kitcher's account, the criteria for contributions of laypersons become more stringent as the scope of his proposal increases. In *Science, Truth, and Democracy*, the target audience is limited to the citizens of a particular democracy and the problems to address are whatever is important for the citizenry. In this model, the wishes of the citizens are subject to tutoring and limits imposed by the requirement to take others' wishes into account, but there are no other more general requirements. In *Science in a Democratic Society*, the audience grows to include the entire humankind and the problems to address are those that are relevant for ensuring everyone's chances to live a worthwhile life. The wishes to be taken into account in this truly global conversation should be such as to be defensible in the ideal conversation with its demanding conditions. The more general the discussion, the more carefully potential inputs are filtered.

Another cost of the solution is the problematically demanding visions of both deliberators and experts. Deliberators are supposed to be able to satisfy the conditions of the ideal conversation. Experts are expected to possess, or to be able to discover, all knowledge

[74] Besides that, the models meant in the quote above are the models discussed in *The Advancement* rather than the model of well-ordered science. The quote summarises the considerations that played a role in the abandonment of the game-theoretical approach and the switch to well-ordered science.

relevant for the realisation of the aims of well-ordered science and to be able to present it in an objective and disinterested way. In the previous sections I argued that this approach presents a conception of experts and deliberators that is implausible in light of Kitcher's general view of epistemic agents and his constructivist view of inquiry and ethical conversation. I also showed how it may undermine both the successfulness and the democratic spirit of well-ordered science.

Given these problems and the possibility to avoid cacophony in a more local way, one may ask why Kitcher chooses the approach that he does. I suggest that in addition to the aim of avoiding the dangers associated with vulgar democracy, Kitcher's approach reflects another desideratum that I tentatively call "universality"—the wish to propose one universal form of organisation of science.

Kitcher does not discuss the generality, or universality, of the proposed solution as a separate issue. One indication of the importance of the theme may be the mentioning of "dangers of local optimisation" (Kitcher 2003, 114), when discussing the change from the approach of *The Advancement*, which attempts to optimise the system of incentives in particular decision-making situations, to the more general approach of *Science, Truth, and Democracy*. As Kitcher stresses elsewhere (Kitcher 2002b, 267), "an arrangement fruitful in one context (or set of contexts) may be deleterious elsewhere". Improving the local situation is thus not enough.

More generally, I believe that the historical development of Kitcher's account suggests a motivation for Kitcher's universal approach. I have previously suggested that Kitcher's accounts of *Science, Truth, and Democracy* and *Science in a Democratic Society* can be seen as the fulfilment of the aims Kitcher first set in *The Advancement*. In *The Advancement*, Kitcher focused on cognitive aims of science, justifying this choice by their tractability. Not only there are fewer fundamental epistemic aims, but there is also less variation in the way they are weighed against each other. There may be considerable disagreements about the weights of practical aims and about the way they are weighed against epistemic aims—but all of those committed to epistemic aims mostly agree how to weigh them with respect to each other (Kitcher 1995a, 93, fn). Epistemic aims thus lend themselves to a systematic exploration and practical aims do not. The discussion of practical aims of science has to await the creation of "a very general account of human flourishing" (Kitcher 1995a, 92) that would in turn serve as the starting point for solving "a very general problem of optimisation" (Kitcher 1995a, 391).

The way particular human wishes are pared down by increasingly strict conditions from Kitcher (2003) to Kitcher (2011c) and the absorption of these wishes into the general framework of the ethical project can be seen as an attempt to achieve this greater tractability for non-epistemic aims. In particular, the idea of participation in the ethical conversation as a part of the human condition helps to introduce some agreement about weights of different practical aims. Participants of the ethical conversation cannot but agree that addressing currently neglected basic needs of large groups of people and global problems that threaten everyone has the highest priority. Thus, the commitment to the ethical project provides the basis for discovering the natural hierarchy of aims to pursue—the hierarchy all participants who satisfy the conditions of the conversation would agree about. Once the practical aims are organised in this more tractable way, the "very general problem" can be approached. This task of providing a systematic solution for the optimisation of the organisation of science with respect to a single well-defined and tractable set of aims can thus be seen as something that was first formulated in *The Advancement* and finally achieved in *Science in a Democratic Society,* albeit with many important changes in Kitcher's approach.

The ambition of well-ordered science to give chances to live a worthwhile life to the entire humankind naturally supports the approach that aims for a universal system rather than a patchwork of local solutions. Even more strongly, such an approach is supported by the desire to address global problems that call for a solution on the global scale—above all, the problem of the global climate change. This problem plays a prominent role in the presentation of Kitcher's (2011c) account, from the description of the social context characterised by ignorance and resistance to the scientific consensus concerning the climate change (Kitcher 2011c, 25–31), to the discussion of the well-ordered science's approach to addressing this issue (Kitcher 2011c, 243–248). For Kitcher, the climate change is a global problem not only because it defines life conditions for everyone but because it touches what is one of everyone's fundamental real wishes:

> Most people, including most of those who oppose, or are indifferent to, any policies for addressing the problems that will be generated for our descendants by our continued excessive use of fossil fuels, care deeply about the opportunities their descendants will have. (Kitcher 2011c, 243)

Therefore, it may seem appropriate to reject approaches that would prioritise more local interests (such as those identified by members of particular democratic societies in Kitcher 2003) or distract from the most streamlined development, dissemination and use of scientific knowledge relevant for addressing this issue.

Once the aim of offering a universal solution is set, the rejection of the local approach for the universal one follows naturally. Kitcher recognises keenly limitations of the possibility to widen a well-functioning local Millian arena. On the one hand, the problem is similar to that of direct democracy: a discussion that requires actual participation of many parties cannot be scaled up without losing its immediacy (for democracy, Kitcher demonstrates this point with the help of convincing calculations in Kitcher 2011c, 78–80). On the other hand, to the degree productive discussion depends on substantial knowledge relevant for the issue, educating everyone to the same standard is likely to be impractically slow and impractically burdensome. Kitcher understands this kind of education in terms of teaching science to non-scientists. He believes that educating everyone about technical issues would likely be a very slow process (Kitcher 2011c, 186) that is unlikely to go sufficiently far to make free public debate of scientific matters productive (Kitcher 2011c, 176). The same point, however, would apply to making what non-certified ("lay") experts know universally known. A different approach is necessary, and Kitcher's model of carefully regulated well-ordered science where aims for science are a part of the ethical project makes its entrance.

The arguments of the previous sections had the aim to demonstrate that Kitcher's approach runs into serious problems. The approach to experts and deliberators in well-ordered science has such a high epistemic and democratic cost that it threatens the achievement of the aims of well-ordered science and the inner consistency and plausibility of Kitcher's account. Now I have suggested that these restrictions can be explained as following from a particular approach to managing public discussion that reflects Kitcher's desire to provide a universal account free from the limitations of local solutions. I have also shown how the desire to avoid cacophony in democratised science can be achieved by different means. Pursuing an alternative solution incurs certain losses—in particular, the loss of attractive economy and harmony that a general solution promises. Instead, it will be necessary to work out particular solutions for local contexts and these local solutions may be incompatible. The challenge of addressing global problems also looms large for such a local approach. Nevertheless, I suggest that the problems of Kitcher's account that I have discussed are serious enough to justify giving up the dream of universality. The aim of the final chapters of my thesis is to describe such a local alternative to Kitcher's account and to argue that it can be viable.

4.4 Conclusion

The aim of this chapter was to provide a critical examination of Kitcher's well-ordered science. At the beginning of the chapter I summarised a number of features of Kitcher's account that I find congenial, including the integration of various aspects of the social in science, the attention to the consequential character of scientific research and the balance of different consequences, and the interest in collaboration with political science. It is against this background of agreement that I developed my criticism.

My criticism focused on three large groups of issues. The first of them concerns Kitcher's vision of the expert with its ideal of disinterestedness and tensions between the roles of the expert as a member of scientific community and a participant in the deliberative conversation of well-ordered science. The second discusses the potential limitations of expert knowledge due to the value-laden character of concepts and the context-sensitive nature of applicable solutions, and the resulting necessity to supplement experts' knowledge with critical lay perspectives and local knowledge. The third concerns the elements in Kitcher's account of public participation that undermine the motivation to involve the public and promote reliance on the hypothetical reconstruction of the public's needs instead. The issues related to expertise are crucial for the realisation of aims of well-ordered science and they are interconnected with other problems of Kitcher's approach. The focus on the ideal of disinterestedness masks the possible incompleteness of experts' knowledge—the problem that cannot be solved by a better adherence to the ideal. The move away from actual public participation precludes the possibility of lay knowledge transfer during interactions between experts and laypersons.

In addition to the problems for the successfulness of well-ordered science, I discussed tensions between the demands imposed on experts and deliberators and Kitcher's usual approach to imperfect human agents as a threat to the consistency of Kitcher's account. I suggested that Kitcher's view of experts and the public reintroduces the vision of them that Mark Brown connects with the traditional view of political and scientific representation. This view does not mix easily with the development of Kitcher' constructivist and value-laden views of scientific significance and constructivist view of ethics. Finally, I pointed out how Kitcher's approach to strictly limited public participation contradicts the democratic orientation of his proposal, which fails, as a result, to realise the benefits usually associated with democratisation.

In the concluding part of the chapter, I discussed the rationale for the form that Kitcher's proposal takes. I suggested that the problem of vulgar democracy can be addressed in two different ways. The local approach focuses on local norms and local knowledge but cannot be easily scaled up. The universal approach avoids limitations of scale at the cost of restricting the number of potential interactions and severing their connection with actual laypersons' contributions. I suggested that Kitcher's preference for the universal approach can be explained by his desire to provide one systematic solution for the social organisation of science. As I showed in this chapter, this solution runs into serious problems that undermine both the promise of well-ordered science and the internal consistency of Kitcher's account. Accordingly, I suggested that an alternative approach to the socialisation and democratisation of science and science policy is necessary. In the following chapter I show how such an approach is possible on the basis of Longino's account.

CHAPTER 5. APPROACHING THE SOCIAL ORGANISATION OF SCIENCE WITH LONGINO'S IDEAS

5.1 Introduction

In the first two chapters of the thesis, as well as when discussing the points of agreement with Kitcher, I suggested that an adequate philosophical proposal for the social organisation of science should address a number of issues. Among them are the integration of various aspects of the sociality of science; the recognition of the consequential character of research and related questions of fairness and justice; and the attention to the political side of the proposals made.[75] In the previous chapter I argued that there are some important problems in Kitcher's approach to achieving these desiderata. There are tensions between Kitcher's requirements for experts and deliberators in well-ordered science and his analysis of human knowers that cause inconsistencies in Kitcher's social account. There are failures to take into account possible limitations of experts' knowledge—the possibility that there are unexplicated values in experts' concepts and gaps in their knowledge when it comes to local contexts. These failures are problematic in light of Kitcher's own view of knowledge and threaten the successful achievement of the aims of well-ordered science. Finally, there are tendencies in the discussion of democratic participation that weaken the motivation to actually realise it. They undermine the characterisation of Kitcher's model as democratic and social and raise questions about the relevance of empirical research on democratic participation.

Presenting those criticisms, I began to outline an alternative that would avoid these problems. At its heart is the idea of discussion that involves a variety of perspectives and various forms of lay expertise. Ideally, such a discussion would simultaneously address the problem of limitations of experts' knowledge and encourage democratic involvement of the public with science. The aim of this chapter is to begin to describe such an alternative in more detail. In the chapter, I show how Longino's account of science can support this kind of epistemic and democratic improvement of science. In order to defend this use of Longino's account, the chapter begins with addressing some important objections against it. Following that, I outline the possibilities of a Longino-inspired approach to science and the place of science in society. I show how it offers a systematic account of social aspects of science, enables to attend to practical consequences of science and opens possibilities for a close contact with political developments and their analyses. Discussion of political aspects of Longino's account allows outlining how the realisation of Longino's ideas in practice may be possible. These possibilities are discussed in the next chapter. There, a contact between Longino's account and political science analyses is established and ways to realise local improvements in science are discussed.

In the following section of this chapter I discuss what can be considered an important point of disagreement between Kitcher's and Longino's approaches—the question of underdetermination. I argue that Kitcher's arguments against the possibility of underdetermination do not threaten Longino's approach in which underdetermination takes centre stage. After that I discuss another potential objection against preferring Longino's account. As I have argued, one of the contradictions of Kitcher's account is the possibility to realise the supposedly social and democratic approach without actual public participation. A similar argument has been made against Longino's approach. Biddle (2009) argues that due to a particular conception of individual in Longino's account, community is not really necessary for objectivity as she describes it. If this is so, Longino's account is subject to the same irony I

[75] The remaining point of agreement—the necessity to discuss trade-offs between epistemic and practical consequences of a form of organisation—is not discussed in this chapter. I return to it at the end of the next chapter.

have discussed in Kitcher's case: an account that supposedly puts sociality at the centre of science turns out to be individualistic. To address this concern, in the third section I argue, contra Biddle, that objectivity in Longino's account is necessarily social. Doing so, I add further arguments against the possibility to avoid actual discussion involving various publics by doing a reconstruction of the publics' positions.

After removing these obstacles, I describe in the fourth section how Longino's account can be used to approach the question of the social organisation of science. I argue that Longino's account supports a close integration of the approaches to the organisation of science and the organisation of science policy that helps to avoid the problems of Kitcher's model. In the final section of the chapter, I describe how a connection with developments in wider political context of science is crucial for the possibility of realising Longino's ideas in practice. Accordingly, establishing contact between philosophy of science and analyses of political developments is necessary. An approach to establishing this contact and some examples of its application to specific cases are presented in the next chapter.

5.2 Underdetermination

Compared with remarkably different positions in Longino's (1990b) *Science as Social Knowledge* and Kitcher's (1995a) *Advancement of Science*, there had been a considerable convergence between their positions by the time Kitcher's (2003; first published 2001) *Science, Truth, and Democracy* and Longino's (2002a) *Fate of Knowledge* appeared.[76] A number of similarities in their positions will become evident, as I describe how Longino's account can be used to address some of the questions Kitcher's well-ordered science addresses. The issue of underdetermination, however, seems to make their accounts incompatible, given the central role underdetermination plays in Longino account and the opposition to the possibility of ubiquitous underdetermination in science that has persisted throughout Kitcher's books.[77] Thus, doubts can be raised about the plausibility of Longino's account as the basis for an alternative to Kitcher's. The aim of this section is argue that Kitcher's approach to rejecting underdetermination as a serious possibility does not undermine Longino's account.

Introducing her account, Longino (1990b; 2002a) stresses that it brings together two senses of the social in science: the inevitable role of social values and the indispensable role of social interactions for maintaining objectivity of reasoning in science. This approach hinges on a specific account of underdetermination. As Longino describes it, there is no one-to-one relation between evidence and hypothesis and some background assumptions have to be involved in order to connect the former and the latter. It is because of this underdetermination that evidential reasoning cannot be insulated from social values in principle, as social values may be a part of these inevitably involved background assumptions. This, in turn, threatens to introduce subjective preferences and biases into hypothesis acceptance. Longino argues that this threat is prevented by social interactions in community. As individuals' claims only become a part of recognised public knowledge as a result of collective processes of criticism, subjective biases may be exposed and their modification or

[76] Longino (2002b, 573) herself comments on that. There are other authors who discuss Longino's and (the newer) Kitcher's accounts together; one example of an explicit commentary of their convergence is Leuschner (2012).

[77] In the 2002 discussion between Kitcher and Longino—Longino's (2002c) review of *Science, Truth, and Democracy* and Kitcher's (2002a) response and Kitcher's (2002c) review of *The Fate of Knowledge* and Longino's (2002b) response—the interpretation of pluralism is one of the central point of disagreement (this theme is further discussed in Kellert, Longino and Waters 2006). The question is whether all representations that conform to nature are ultimately consistent (Kitcher) or whether there may be conforming mutually inconsistent systems (Longino). I will not attempt to enter this discussion because this question, while important, does not form the basis of Longino's approach to the sociality of science the way the issue of underdetermination does.

rejection required. The importance of the social character of science is thus demonstrated in connection with ubiquitous and inescapable underdetermination in evidential reasoning.

The existence of such underdetermination, however, is what Kitcher has steadfastly denied.[78] When discussing underdetermination, Kitcher (1995a, 247–263; 2003, ch. 3) focuses on a specific understanding of the issue. On this view, as hypotheses are underdetermined by evidence and any hypothesis can be "saved" by adjusting some element in the complex "hypothesis–observations–auxiliary statements", it is not possible to decide upon hypothesis acceptance on the basis of evidence alone. It is always possible to propose alternative hypotheses on the basis of the same evidence or to hold on to a hypothesis no matter what evidence.[79] Kitcher's main argument against the possibility of widespread and persistent underdetermination in science points out that science in fact is not the way these arguments describe it. Science is not usually characterised by a multiplicity of alternative theories. Instead, in many cases there is only one real option; it seems impossible to offer alternatives without the help of "philosophical devices". Similarly, even if giving up existing possibilities and related constraints on alternatives may be conceivable, it may be associated with such epistemic losses elsewhere as to make the option inadmissible in practice. In the words of an imagined scientist (inspired by Kitcher's conversations with Stephen Jay Gould),

> It's hard enough … to find *one* way of accommodating experience, let alone many. And these supposed ways of modifying the network of beliefs are changes that no reasonable—sane?—person would make. There may be a *logical* point here, but it has little to do with science. (Kitcher 1995a, 247, italics in the original)

I suggest that juxtaposing this view of underdetermination with Longino's approach to characterising it makes it possible to see that Longino and Kitcher talk about two different issues. As a result, Kitcher's arguments against the Duhemian underdetermination fail to demonstrate that the kind of logical underdetermination Longino describes does not pose a problem either for the scientist or the philosopher.

In one of her papers, Longino specifically addresses the issue of differences between Kitcher's understanding of underdetermination and her own. As she points out,

> There is another way to see underdetermination than Kitcher does: not as a matter of empirically undecidable conflicts between two or more theories but as a matter of relations between theories and the evidence available for them. (Longino 2006, 170)

This is the way of seeing underdetermination that Longino adopts in her discussion of the role of background assumptions. In a number of expositions, Longino offers several formulations of the issue of these relations between theories and evidence. The underdetermination she is concerned with stems from "the discontinuity of language" (Longino 1990a, 151) between descriptions of observational data and theoretical claims. Elsewhere she characterises the issue in terms of "semantic independence of theoretical and observational language" (Longino 1992c, 329) or the notion of "semantic gap" (Longino 2004, 132) between hypotheses and data. Hypotheses talk about processes or objects (for example, sub-atomic particles) using as evidence observational data that does not describe these processes and objects directly (for

---

[78] As noted earlier, Kitcher recognises the existence of transient underdetermination but considers it trivial. Biddle (2013b) argues that the consequences of transient underdetermination cannot be dismissed so easily. I will not go into this argument because my own discussion focuses on Kitcher's general definition of underdetermination rather than its subtypes.

[79] In *The Advancement*, Kitcher (1995a, 160–169) discusses another argument that is related to the issue of underdetermination: the thought experiment of research groups identical in all their social features and given different samples to analyse. According to Kitcher, our intuition is to expect different results, contrary to the idea that beliefs are not (fully or even at all) determined by evidence. As Longino does not embrace this view of strong constructivism, this argument does not apply to her account. (Longino also suggests (Longino 2002a, 56–59) that this argument of Kitcher's is based on a problematic interpretation of sociological laboratory studies.)

example, what the data describes is traces in the bubble chamber). Background assumptions are necessary to relate the two. With the stress on this role of background assumptions in evidential reasoning, what is at issue is not the possibility of alternative hypotheses, but rather the possibility of seeing the evidence as relevant for a hypothesis at all.

No variety of empirically equivalent theories is necessary to support the logical point that Longino makes with her account of underdetermination. Many factors may play a role in the fact that only one option is taken to be acceptable (and many of these factors may be perfectly respectable epistemically).[80] In one of her papers Longino discusses one type of these factors—epistemic values—explaining why "idealised underdetermination situations" (Longino 2008a, 80) are rare in science. These values act as heuristics, guiding choices of questions, data, methods, interpretations etc. throughout the course of inquiry and helping to close the underdetermination gap imperceptibly. The observation that only one acceptable hypothesis exists for a given body of evidence thus does not resolve the philosophical issue, because the real issue is located on a different level. The fact that the semantic gap is usually closed should not offer comfort for the philosopher. One should still ask which background assumptions are involved and whether they are defensible, empirically, conceptually or, possibly, ethically. As Longino stresses when criticising Kitcher's approach to underdetermination,

> The relevance of the logical fact of underdetermination is, then, not that scientists must make, for the most part, arbitrary or venal decisions, but that analysts of particular episodes must attend to *how* the logical gap is bridged—for example, to what assumptions are used to confer evidential relevance to data. (Longino 2002a, 63, italics in the original)

As Longino's (2013) analysis of research programmes in biology of behaviour shows, demonstrating the role of specific background assumptions in different research programmes can be much more than making a "logical point" that "has little to do with science". Analysing background assumptions helps to see the limits of programmes based on different assumptions, the kind of understanding that can be achieved with their help, and the possibilities of interactions between them.

Thus, I have argued that by distinguishing different senses of underdetermination it is possible to show that Kitcher's arguments against the possibility of ubiquitous underdetermination fail to be applicable to Longino's argument. They also fail to establish that the kind of underdetermination that Longino describes does not offer philosophical interest. Accordingly, I conclude that there is no reason to reject Longino's approach on the grounds of the thesis of underdetermination that Kitcher considers improbable. However, there may be other conceptual objections to preferring it to Kitcher's. In Longino's account, the notion of transformative criticism takes centre stage when explaining the social nature of science. If this criticism is ultimately individualistic, as Biddle (2009) argues, Longino's account cannot be called social. In this case, it does not offer a better alternative to Kitcher's account whose social nature I have questioned. Addressing this possibility constitutes the subject of the next section.

[80] Criticising Kitcher's arguments against underdetermination Biddle (2006) shows that Duhem, of the underdetermination version that Kitcher targets, himself acknowledged that a variety of alternatives is not typical for science. This state of affairs can be explained, once it is recognised that the development of hypotheses happens within a particular research tradition. Such a tradition provides important constraints—in terms of experimental possibilities, methods, desiderata, norms etc.—on this development. Given these constraints, it is not surprising that typically there are no numerous well-developed alternatives.

5.3 Is Longino's account of objectivity social?

5.3.1 Introduction

In his paper, Biddle (2009) presents a powerful argument against the characterisation of Longino's account as social. He argues that implicit in Longino's account is a particular conception of individual. According to this conception, the individual has capabilities that make social interactions unnecessary for maintaining objectivity—such individuals can in principle do so on their own. Thus, Longino's claim about the fundamentally social nature of her account is refuted. Social interactions may be useful in practice but are not strictly necessary in principle. Biddle suggests that social epistemology should employ a different conception of individual that is compatible with a fully social account of science. He also describes briefly how such an account could look like.

The aim of this section is to argue for the fundamentally social nature of Longino's approach to objectivity. In the first part of my argument, I discuss whether individuals that according to Biddle are presupposed by Longino's account would be able to achieve objectivity in isolation. I argue that they would not: community would be required for objectivity even in the case of such individuals. In the second part, I engage with Biddle's interpretation of Longino's conception of individual directly and argue that the textual and contextual support for it is not unambiguous. Instead, there is evidence for a different conception and this alternative conception supports the characterisation of Longino's account as social. I conclude that both arguments support the claim that sociality is necessary for objectivity in Longino's approach; objectivity in the sense that Longino discusses cannot be a-social. While responding to Biddle's argument is the primary aim of the section, doing so I make a more general suggestion. With my argument, I show that assessing one's and others' claims in light of intersubjective norms and alternative perspectives as a part of being objective is impossible without community and in this sense it is essentially social.

In the following subsection, I summarise Biddle's argument against Longino's claim that objectivity in her account is social and can only be fully realised in community. In the third part, I argue that two crucial aspects of objectivity, as Longino and Biddle discuss it, require socialisation and belonging to community even if the individual is understood in the way that Biddle ascribes to Longino. Developing this argument I draw on Harry Collins's (2010) account of strong collective tacit knowledge and Kusch's (2004) account of rule-following. In the concluding part I discuss the evidence for an alternative conception of individual in Longino's account. Doing so, I suggest that some aspects of Longino's conception that Biddle overlooks share important similarities with the conception Biddle himself defends as appropriate for social epistemology.

5.3.2 Biddle's criticism

Before discussing Longino's account of objectivity and Biddle's criticism it is helpful to highlight the specific understanding of objectivity in Longino's account. (Biddle does not discuss the meaning of objectivity as a separate issue; I assume that his usage follows Longino's.) In her discussion of objectivity, Longino (1990b, 62–66) distinguishes between its two senses. In one sense, objectivity is connected with the notion of truth: to be objective is to represent facts of the world truthfully. In another sense, objectivity is understood as the opposite of subjectivity: to be objective is to rely on non-subjective or non-arbitrary criteria. Elsewhere, Longino defines the latter sense of objectivity as "independence from subjective bias" (Longino 1990b, 75) and freedom from "the intrusion of individual subjective preference" (Longino 1990b, 76). It is this sense of objectivity that Longino is concerned with.

Starting from her account of evidential reasoning and the inescapable role of background assumptions there, Longino argues that objectivity in this sense is essentially social. Objectivity emerges on the basis of social—interactive—practices in community that is supposed to follow certain social norms (availability of venues; shared norms; responsiveness to criticism, or uptake; tempered equality of intellectual authority). These social interactions make it possible to block influence of subjective biases in a way that is unachievable for an individual. Objectivity is thus seen as a property of communities rather than of individuals. This is the main sense in which Longino's account of objectivity is social.

In his 2009 paper, Biddle challenges this view and argues that the conception of individual that Longino's account presupposes conflicts with Longino's characterisation of objectivity as necessarily social.[81]

Biddle suggests that Longino's criteria depict objective community as realising Mill's ideal of the "free marketplace of ideas" (Biddle 2009, 613 and 614–615) and that Longino's account is deeply embedded in Mill's political liberalism (Biddle 2009, 615–616). In turn, this connection is crucial for Biddle's interpretation of Longino's criterion of uptake (responsiveness to criticism). Biddle (2009, 616) suggests that this criterion could be interpreted in two different ways. In what Biddle calls the individualistic interpretation, it is applied on the level of individuals. For community to satisfy the requirement of uptake, most of its members must satisfy it. In the social interpretation of uptake, it operates on the level of community. For community to satisfy this requirement, community as a whole must be responsive to criticism, without each individual member necessarily being so responsive.

Biddle argues that while Longino does not specify how uptake should be interpreted, there are reasons to think that her account requires the individualistic interpretation. First, Biddle reads Longino's account as stressing the importance of individuals' actions, which supports the interpretation of uptake as demanding responsiveness on the individual level. As an example, he quotes Longino's statement that "[w]hat is required is that community members pay attention to the critical discussion taking place and that the assumptions that govern their group activities remain logically sensitive to it" (Longino 1990b, 78 quoted in Biddle 2009, 617). Second, Biddle argues that Longino's account shares the conception of individual with Mill's. In turn, Biddle reads Mill's argument for free speech as describing the individual who is very responsive to criticism as the ideal. It is an individual that "*listen[s] to all that could be said against him*" and for each of his opinions studies "*all modes in which it can be looked at by every character of mind*" (Mill 1978, 19 quoted with italics added in Biddle 2009, 617). Biddle concludes that Mill's ideal presupposes individuals that are open to any criticism from any source and are able to question any of their beliefs. According to Biddle, this is also what Longino's criterion of uptake requires. A community that satisfies the criterion of uptake must mostly consist of individuals that are open to everything (Biddle 2009, 617–618).

In the decisive step of his argument, Biddle points out that such radically open individuals are in principle able to criticise their beliefs exhaustively and impartially on their own. Social interactions may facilitate this process but are not fundamentally necessary. There is nothing in such individuals (whom Biddle likens to "unencumbered selves" (Biddle 2009, 618) discussed in moral and political philosophy) that could prevent the same effective criticism from being fully realised by the isolated individual. As Biddle writes (2009, 619),

> But if individuals, in the ideal, are completely open-minded—if they are capable of questioning all their beliefs, of examining evenhandedly all potential weaknesses in their views, and of adjudicating between opposing beliefs, methodologies, evaluative criteria, and so on, in a fair and evenhanded manner—then it is false that a community is

[81] Biddle (2007) similarly reads Longino's account as ultimately individualistic; see, e.g., Biddle (2007, 35).

> necessary for the justification of beliefs. For such individuals are perfectly capable, at least in principle, of evaluating lines of argument in an objective fashion—and of doing so on their own, qua individuals.

Accordingly, Biddle concludes that Longino's claim about objectivity being necessarily social is proven wrong, as the individual her account presupposes can fully achieve objectivity independently from community. He suggests that developing a truly social account of knowledge would require abandoning the conception of "unencumbered self" and the free marketplace of ideas as the ideal for scientific community (Biddle 2009, 620).

I challenge Biddle's conclusion by presenting two separate arguments. For the sake of the first of them, I acknowledge that it is possible to interpret Longino's account in such a way that the satisfaction of the criterion of uptake would ideally require each individual respond to criticism with the complete openness of the "unencumbered self". This possibility is prima facie plausible even without assuming that Longino's conception of individual is the same as Mill's (and that Biddle interprets the latter correctly). On both interpretations of the criterion of uptake, for a community to be responsive to criticism, at least some of its members must be responsive to it. Presumably, the more there are such individuals and the more each of them is open to criticism, the greater community's responsiveness is. One can thus reach the conclusion that the ideal community would entirely consist of individuals that are completely open to any criticism. Nevertheless, I intend to argue that even for such "unencumbered selves" that can impartially evaluate all of their beliefs, the kind of criticism Longino describes is only possible to the full extent in community. Doing so, I go beyond Longino's text and the authors she draws upon and turn to works on tacit knowledge and rule-following. Accordingly, the next subsection is best seen as a discussion of the general question whether the "unencumbered self" on one's own is capable of objectivity as Longino describes it. Biddle's argument presupposes the response in the affirmative. I argue against this possibility and through that against Biddle's conclusion. I postpone the questioning of Biddle's interpretation of Longino's conception of individual until the penultimate subsection where I develop the second line of argumentation.

5.3.3 Objectivity, collective tacit knowledge and rule-following

The argument I propose in this part of the paper focuses on two aspects of criticism for maintaining objectivity that Longino and Biddle discuss. This criticism, as Longino describes it, involves, first, the evaluation of beliefs in light of some norms, in order to ensure that one is accountable to something beyond one's subjective preferences. This is the issue that Longino's criterion of availability of shared norms addresses.[82] Second, it involves the evaluation of beliefs and points of view in light of alternative beliefs and perspectives, in order to ensure that one's problematic assumptions may become visible. This is the issue that Longino's requirement of diversity of perspectives in community addresses. Discussing the kind of criticism that the "unencumbered self" supposedly can develop in isolation, Biddle describes similarly that such criticism would involve evaluating one's beliefs by taking into account both alternative criteria and alternative beliefs (Biddle 2009, 619). The aim of this part is to argue that both varieties of criticism require socialisation in community and further participation in community's life in order to acquire relevant knowledge and to continue to use it correctly. Developing this argument, I draw on accounts of tacit knowledge (when

[82] Longino's idea of norms is wide—she describes them as "encompass[ing] everything discussed as methodology by philosophers of science and more" (Longino 2002a, 145). Following this wide usage, the notion of norms that I use in the section includes, along with general norms of reasoning, more local norms, standards of evidence, argumentation and analysis, use of particular methods, and rules of good practice that characterise a particular area of inquiry at a particular time. While the general norms are unlikely to change in the individual's lifetime, more local standards evolve faster. For example, individuals who entered biomedical community before and after randomised trials became the standard would acquire different norms.

discussing learning of rules and perspectives) and rule-following (when discussing their application). I conclude that the kind of criticism possible in community is not possible for an isolated individual.

There is a long tradition of discussing knowledge of rules as necessarily involving tacit knowledge. On the more abstract level there is the idea that "rules of action do not contain the rules for their application", as Collins (2010, 2) characterises the approach to tacit knowledge he connects with Ludwig Wittgenstein's ideas. Even after the rule has been stated in an explicit form, one has to know how to apply it, how to adapt its use to a specific context, how to judge the application of the rule as acceptable or unacceptable, both in known and novel situations. In short, one has to know what the rule means in each particular case.[83] As an attempt to further explicate the rule would create the same problem on the next level, one has to acknowledge that knowing and applying rules successfully involves knowledge that goes beyond their verbal formulation. On a less abstract level, the classical discussion of tacit knowledge by Polanyi stresses how learning the language of a particular discipline (including, I suggest, its norms and standards of performance), is impossible without learning "what is meant" by it, without acquiring relevant tacit knowledge. (Polanyi's famous example (Polanyi 2002, 101) is that of a student, observing chest X-rays and listening to experts' discussions, and gradually acquiring the ability to see in X-rays what competent radiologists say they see in them.)

Knowledge of shared norms that constitutes a precondition of objectivity can thus be understood—like the case of knowing rules in general—as involving some tacit knowledge. In order to make clearer what type of tacit knowledge is involved in learning rules and norms, I propose using Collins's (2010) account. With its help, it is possible to demonstrate that community is necessary for learning rules, without which criticism for sustaining objectivity is impossible. It is important to stress that I do not assume full compatibility between Longino's and Collins's accounts (particularly their conceptions of individual as an epistemic subject). Instead, the point I want to make is that, insofar as the knowledge of shared norms plays a role in Longino's approach to objectivity, discussion of tacit knowledge is relevant and Collins's account provides some helpful distinctions.

According to Collins, several kinds of tacit knowledge can be distinguished depending on different senses of explication and different kinds of obstacles for explicating such knowledge (Collins 2010, 1). An element of tacit knowledge can be explicated by elaboration or transformation, by creating a mechanism that imitates a tacit skill or process, or by providing a scientific explanation for it (Collins 2010, 81). In the case of weak (relational) and medium (somatic) tacit knowledge explication of at least some elements of knowledge is possible in one or several senses of "explication". At least in principle, one can explicate elements of relational tacit knowledge (explicable knowledge that remains tacit for contingent reasons) one possesses. For instance, one can explain step by step how one handles a piece of laboratory equipment in a particular way (see Collins 2010, 91–98 for the discussion of relational tacit knowledge). At least in principle, one can explicate elements of somatic tacit knowledge. For instance, one can provide a scientific explanation of the process of maintaining one's balance on a bicycle or create a mechanism that imitates this process (see Collins 2010, ch. 5 for the discussion of somatic tacit knowledge).

Collins argues that in the case of strong (collective) tacit knowledge no explication is possible (see Collins 2010, ch. 6 for the discussion of collective tacit knowledge). This kind of tacit knowledge characterises human actions in social context. For instance, riding a

[83] In the case of Longino's account, knowing a rule can be additionally explained as knowing the difference between an idiosyncratic interpretation of a rule and the interpretation that can be intersubjectively recognised as proper.

bicycle in a street, in addition to maintaining balance, requires the knowledge of relevant social rules and the ability to interpret them according to the situation, to interact with others and to respond adequately to their actions. Acquisition and use of collective tacit knowledge requires the ability to develop fluency in the language and culture of community, to understand and apply rules in a way that is context-sensitive and responsive to changes. According to Collins, such fluency can only be developed in the process of socialisation and subsequent participation in the social life of community. Collective knowledge cannot be acquired once and for all. With developments in life of community, collective knowledge changes constantly and unpredictably. Accordingly, keeping this knowledge up to date requires constant contact with community (Collins 2010, 30–31).

Collins's classification suggests that tacit knowledge involved in learning norms, standards and rules of practice may contain different elements. Some of its parts may be relational—they are explicable verbally and only remain tacit because no one has the need, or the willingness, to explicate them. Some of its parts may involve skilful bodily action—somatic tacit knowledge.[84] In addition to those potentially explicable elements, I suggest that it is what Collins calls collective tacit knowledge that forms the necessary basis for the ability to learn norms.

Collins's account attracts attention to two crucial aspects of this learning. First, rules may be interpreted and applied appropriately or inappropriately, and this appropriateness depends on the context of application. Rules in abstraction can be interpreted in many possible ways. In order for rules to play their role in critical practices for blocking subjective biases, a (somewhat) stable intersubjective understanding of the appropriate interpretation is required. Accordingly, in order to be able to use rules one has to acquire with them the knowledge of what counts as the appropriate performance. This knowledge, in turn, has to be acquired by being immersed in community of those who already possess these norms and can make judgements about the appropriateness of their use. Without familiarity with the social context where the norms are applied and the correctness of the application judged, there is no possibility to learn what the correct (and intersubjectively recognisable as such) application of rules means. Thus, a necessary precondition for one's learning to apply norms in order to evaluate one's own, and others', claims is learning them in the context of their application by being socialised in the respective community.

One could concede as much and yet defend Biddle's conclusion by arguing that this learning process has an end. The agent who has once learnt community's norms would supposedly have no further need in community. One could thus argue that once the "unencumbered self" has completed scientific education, the self could be said to know the norms and be able to apply them for maintaining one's objectivity by self-criticism. However, I suggest that by taking into account the second aspect of collective tacit knowledge that Collins stresses—its changeability—one can argue against this possibility. Norms and accepted ways of their interpretation—what counts as the appropriate or correct application—may change, as community changes. Any attempt to lay down community's norms permanently for future independent use is bound to fail, as these fixed rules will become outdated. As Collins (2010, 132–133) writes,

[84] Discussing whether it is possible to criticise tacit knowledge—for example, that one should pour liquid in a particular way—Longino writes that some of this knowledge can be made explicit and the rest can be demonstrated (Longino 2002a, 104, fn). So, it seems that Longino thinks of tacit knowledge as either relational or somatic.

> The competence shown by the isolated speaker will not last indefinitely; the individual is a temporary and leaky repository of collective knowledge. Kept apart from society for any length of time and the context sensitivity and currency of the individual's abilities will fade.[85]

Standards of scientific practice and critical discussion are not different in this respect. In order for the individual's knowledge to reflect changes in these standards, the individual requires continuous contact with community. Without such contact, knowledge claims that the individual considers objective in light of (once learnt) norms may no longer be acceptable as objective for others, if community's norms or accepted ways of their interpretation have meanwhile changed (The previously mentioned example of medical community before and after randomised trials became the standard is relevant here.) The way objectivity and intersubjective acceptability are related reinforces the need for such a contact. As noted in the discussion of Longino's account of objectivity, community's norms at any given moment may be effective for blocking individual biases. However, if some biases are shared by the entire community, they may remain hidden. They would not be exposed in critical dialogue and may remain embodied in community's claims, practices and norms. Discovering such biases and modifying norms accordingly constitutes an improvement in community's objectivity and raises the bar for claims that can be accepted by community as objective. Proposals for such modifications and acceptance of such proposals by community, however, are not something that can be predicted in advance. Thus, in order to keep one's knowledge of norms up to community's standard one has to maintain contact with community.

One might still suggest that an individual could apply once learnt rules in isolation indefinitely and the resulting claims would satisfy community's standards *at the moment when one's knowledge of rules was up to date.* If some of the rules and the ways to interpret them have not meanwhile changed, one's claims can be potentially recognised as up to the current standard. Even if the norms have changed, one can still be recognised as following objectively an outdated standard. In order to exclude this possibility, I turn to Kusch's (2004) argument on rule-following.

The starting point for Kusch's argument is the insight (that Kusch 2004, 97 traces back to Wittgenstein) that it is impossible to talk about rightness (e.g., following a rule in the right way) unless one can distinguish *being right* and *seeming to be right*. Developing this Wittgensteinian argument, Kusch argues that sociality is the only resource available for sustaining this distinction. In other words, what Kusch calls "private rule-following" (Kusch 2004, 176) is not possible. In order to demonstrate that *I am right* as opposed to the situation where *it seems to me that I am right*, I have to appeal to a standard that is independent from me. Kusch suggests that only continuing participation in community provides such a standard. If it seems to me that I am right but I am not, others can correct and criticise me. On the other hand, others' agreement that I am right supports the conclusion that I am indeed right and it is not the case that it only seems to me. Self-criticism, for example, relying on the memory about one's past self (or imagining the self as a community of self's "slices") cannot provide a basis comparable to that provided by criticism from community. As Kusch (2004, 190–191) points out, in interactions with others one can be surprised by their criticisms and encounter resistance in a way that one's yesterday's self cannot surprise or resist today's.

Being able to use already learnt rules correctly is thus impossible without participating in community, because community's reaction to one's performance is the only basis for judging whether rule-following is being done correctly. Without community, it is impossible

[85] In their account of expertise that they approach as resting on the basis of tacit knowledge, Collins and Evans (2007, 3, italics mine) make a similar point: "Acquiring expertise is, therefore, a social process—a matter of socialisation into the practices of an expert group—and *expertise can be lost if time is spent away from the group*."

to control whether one's performance in applying the rule is not slipping. It may seem to the individual that one continues to do what one did when following the rule correctly. However, in isolation there is no way to establish that one is right about it. Accordingly, an individual in isolation would not be able to follow once acquired standards in the same way that members of community can thanks to checks on the correctness of performance they receive in community.

A similar argument for the necessity of continuing participation in community in order to learn about various perspectives and to be able to check whether one applies them correctly can be made in the case of the evaluation of claims in light of different perspectives as a precondition of objectivity. Longino's argument for the necessity of social inclusiveness makes it easy to see this precondition in terms of tacit knowledge, although she does not use the term herself. Discussing the damage to objectivity caused by exclusion of certain social groups from scientific community, Longino talks about perspectives that are based on belonging to a particular group rather than on any explicitly formulated and adopted body of knowledge. The requirement of inclusiveness means that the kind of knowledge in question cannot be separated from its bearer and fully presented in an explicated form. Were it otherwise, it would not be necessary to demand actual presence of bearers of different perspectives in scientific community. Thus, according to my interpretation of this requirement of Longino's, inclusion of various social groups benefits scientific community, because it gives each member possibility to learn about perspectives based, among other factors, on different tacit knowledge.

The knowledge involved may, in turn, be analysed in Collins's terms as containing different types of tacit knowledge. Bringing a particular perspective into discussion may include different elements, including what is, or can be, explicitly stated. However, such a perspective is ultimately grounded in the social experience its bearer has. Accordingly, it can only be fully acquired by having this kind of social experience, by being socialised in the respective community. This means that to the degree that a perspective depends on particular social experience one cannot imitate it if one does not possess this experience. Accordingly, individuals in isolation from communal dialogue may not be able to generate some of relevant perspectives on their own. One may still be able to think of some alternatives; the point is, their variety would be limited compared to that potentially available in an inclusive community. The difference between the self-generated challenge and the challenge that comes from others that Kusch mentions is also relevant here. It seems plausible that one can be genuinely surprised by criticism originating from someone else's perspective in a way one cannot surprise oneself. Thus, the only way to benefit fully from a variety of perspectives is to take part in communal dialogue where persons with different social experience and collective knowledge participate.[86] An attempt to lay down perspectives for future individual reference—for instance, by laying down all points of view that are currently recognised in communal discussion as relevant—would encounter the already mentioned problem of becoming obsolete. Collective knowledge that members of different social groups possess evolves constantly and the perspectives they bring to communal dialogue evolve with it.

Learning about the necessary variety of perspectives thus requires continuing contact with community just like in the case of norms. Similarly, applying a perspective correctly

[86] Sharyn Clough (2013) criticises Longino for the failure to take into account issues related to embodiment, in particular, the failure to argue that not only marginalised *views* have to be present in community but also the marginalised *persons* themselves. Clough argues that Intemann's (2010) standpoint-theory-based approach addresses this issue better (Intemann and Melo-Martín (2014) make a case for inclusion as well). Clough then presents her own pragmatist approach as further strengthening the case for actual inclusion of marginalised persons. My reading of Longino supplemented by Collins's and Kusch's arguments shows how it is possible to argue for the indispensability of actual inclusion on the basis of Longino's account.

requires continuing participation in community in order to ensure that one's performance can be checked and corrected. Without this kind of community's control, the isolated individual runs into the familiar impossibility of distinguishing being right (I use this perspective and generate criticisms on its basis correctly) and seeming right (it seems to me that I use this perspective for criticising my claims correctly). Again, I conclude that the individual in isolation would not be able to do what an inclusive community can—to evaluate claims in light of as large a variety of alternative perspectives and to have means for distinguishing the correct and a seemingly correct use of these perspectives.

To sum up, I have discussed certain preconditions of criticism for maintaining objectivity, using Collins's and Kusch's arguments. I have concluded that objectivity cannot be fully realised by individuals in isolation from community even if they are completely impartial and open-minded. Thus, I reject Biddle's claim that "unencumbered selves" would not need community for achieving objectivity.

It is important to discuss how this conclusion should be understood in the context of my response to Biddle. I have argued that the work on tacit knowledge and rule-following supports the conclusion that to the degree objectivity involves evaluation of one's position in light of intersubjective norms and alternative perspectives, it is impossible without community even for "unencumbered selves". Therefore, I reject Biddle's claim that Longino's account is not necessarily social if it presupposes "unencumbered selves". According to my argument, such an account would still be fundamentally social.

At the same time, it is important to keep in mind that I argue for the social nature of such an account on a basis that Longino might not accept and that is not a part of her argument in the writings that Biddle discusses.[87] The aim of the discussion of the current section is to resist the conclusion of Biddle's argument. I have argued that even if Biddle's reading of Longino is correct, there are independent grounds to argue that "unencumbered selves" are not able to achieve objectivity, as Longino describes it, in isolation. However, it does not touch Biddle's claim that Longino's account as it is presupposes the problematic notion of "unencumbered self". The aim of the second line of my argumentation is to address this issue directly. In the next subsection, I argue that in Longino's text there is evidence for a different conception of knowing subject and that the "unencumbered self" is neither strictly necessary nor desirable for the realisation of Longino's idea of objectivity.[88]

5.3.4 Objectivity for "encumbered selves"

Biddle's argument about the conception of individual in Longino's account is based first on the interpretation of Longino's own text and second on seeing her account as closely related to Mill's and sharing Mill's conception of individual. My argument is similarly based on reading Longino's text and looking at her account through the lens of Mill's argument.

In order to question Biddle's interpretation of Longino's conception of individual in the most straightforward way, I turn to Longino's own discussion of the conception of individual in her epistemology. Describing her account, Longino (2002a, 9) states that her ambition is to develop an epistemology for non-idealised subjects—"epistemology for living

---

[87] In one of her papers, Longino makes an argument that can be taken as supporting the approach I present here. Arguing that knowledge-productive practices are social, Longino discusses the potential objection—the possibility of Crusoe producing knowledge in isolation. In response, Longino argues that

> Crusoe's conversation with herself is parasitic on her past and potential interactions with others. She must rely on meanings and practices developed in the social setting from which she has been set adrift; and she must regard the results of her cognitive activity as tentative, awaiting ratification by the community of which she is still, in intention, a part. (Longino 1994a, 143)

Both the idea of the knowledge of rules as depending on community and the idea of the necessity of community for checking the application of the rules are present here.

[88] Previously, I briefly addressed this issue in Eigi (2012, 59–61).

science, produced by real, empirical subjects". These empirical subjects, in turn, are understood as limited in their ability to be open and impartial. Longino (2002a, 107) remarks that an important lesson of sociological studies of science is the realisation that epistemic subjects are historically, geographically and socially located, and their locatedness matters for knowledge they produce. Thus, the acknowledgement that individuals are influenced by a particular historical, social and cultural context and their openness is therefore inevitably limited is an integral part of Longino's account. Elsewhere, discussing various conceptions of individual in social epistemology, Longino (2002a, 147) mentions Foucault's idea of "the death of the subject". She interprets it as applicable to the idealised subject discussed in traditional epistemology—the subject that is "capable of the view from nowhere", the "unconditioned (or universally conditioned) Subject". Longino does not defend this conception. Instead, Longino argues that giving it up leaves open the question about the non-idealised subject—the "embodied, socially located, and culturally conditioned" (Longino 2002a, 147) individual. This is the individual that epistemology has to be developed for. Thus, for Longino the absolutely open self is neither an adequate description of real subjects nor the model object for epistemology.

Another confirmation of the limited openness of individuals can be seen in Longino's justification for the need to include various social groups in scientific community. Longino stresses that it is not that marginalisation creates an objective perspective. Rather, it is the representation of different perspectives that is crucial for objectivity (Longino 1990b, 82, fn). As there are no fully impartial knowers, objectivity has to be achieved on the basis of a variety of partialities.

In this idea of "located" selves there can be seen important similarities with the conception of individual Biddle himself defends. Biddle contrasts the conception of "unencumbered self" that is open to any criticism and does not have connections with any communities, ideas or ways of life, with the notion of "advocate" (Biddle 2009, 620–622). Due to socialisation in a particular scientific community and deep connections with a particular research programme, scientists as advocates have less openness to criticism and more persistence in pursuing the line of research they are committed to. Biddle argues that this kind of "advocacy" is ultimately beneficial for the advancement of science. I suggest that in discussing the inevitable social and cultural locatedness of the knowing subject Longino's account similarly acknowledges this inescapable "advocacy" aspect of the individual.

So, in Longino's text there is evidence that she explicitly rejects the conception of "unencumbered self", which Longino calls the "unconditioned self", for a conception of individual whose openness to criticism is inevitably limited. Despite that, one might suggest that this ideal of the individual is implicit in Longino's account because this is what the full realisation of the criterion of uptake presupposes. If Longino's criterion of uptake is to be understood individualistically, as Biddle argues, the ideal subject to realise this criterion is the one who is capable to heed to all possible criticism, "unencumbered" by any commitments. The following part of my argument aims to challenge this individualistic interpretation of uptake drawing on certain statements Longino makes about the functioning of the ideal of objectivity.

Crucial for my argument is Longino's explanation that in her account objectivity does not presuppose a particular "objective" attitude on the part of each individual. Instead, it relies on the operation of community practices:

> The objectivity of individuals in this scheme consists in their participation in the collective give-and-take of critical discussion and not in some special relation (of detachment, hardheadedness) they may bear to their observations. (Longino 1990b, 79)

The ability for full detachment from one's beliefs that Biddle describes as the crucial attribute of the "unencumbered self" is thus not a necessary condition for taking part in community dialogue and by doing so helping to sustain objectivity of community. However, if each individual is not required to be responsive in this way, it is the community level where responsiveness should be ensured. Elsewhere Longino confirms that her account focuses on the social rather than the individual level, as she describes her norms as directed at community structures and processes rather than individuals (Longino 2002a, 145). This statement similarly supports the social interpretation of uptake, rather than the individualistic sense Biddle suggests. In light of Longino's insistence on the primacy of the communal level, Longino's requirement that "community members pay attention to the critical discussion" that Biddle (2009, 617) quotes in support of his interpretation, can also be read as requiring responsiveness on the community level. This collective interpretation is further supported by Longino's response to Solomon who criticises Longino's approach to rationality as individualistic. Rejecting this interpretation of her approach, Longino writes that

> The role of social interaction in my view is to enable both the scientific community and the larger community to scrutinise evidence, reasoning, and hypotheses, prior to including them in the accepted corpus. *Individuals may or may not change their beliefs in response to criticism; what matters is what the community does. (*Longino 2008a, 242, italics mine)

A community may remain objective and prevent individuals' claims that fail to satisfy its standards from acquiring the status of public knowledge, even if the individual behind a particular claim does not acknowledge relevant criticism and continues to uphold the claim. Longino describes the production of scientific knowledge as a collective process. Modification of the initial claim does not have to be made by its author. As Longino writes, "[i]f the original proponent does not [modify claims and assumptions in response to criticism], someone else may do so as a way of entering into the discourse" (Longino 1990b, 73). Whether a particular claim becomes a part of accepted public knowledge is not determined solely by its author but depends on collective activities in community.

Longino mentions peer-review as an obvious example of such critical activity (Longino 1990b, 68–69). Extending her example, I suggest seeing members of scientific community as continuously taking part in acts of criticism and response. By subjecting claims to peer-review and responding to criticism received, by providing criticism as peer-reviewers and subsequently judging the adequacy of the author's responses, by choosing to cite or not to cite a particular claim, to use or not to use particular data, to attempt to replicate someone's experiment or to take part in discussion over a published paper they all contribute to the collective practice as a result of which individuals' claims are reworked into collective knowledge.[89]

Given this ongoing process of criticism, some responsiveness on the part of each member is necessary (one cannot be a member of contemporary scientific community without being ready to subject one's work to peer-review). At the same time, each particular critical

[89] Highlighting this widely distributed character of critical interactions in scientific community offers a partial response to Solomon's (2006) criticism of approaches centred on rational deliberation, including Longino's. According to Solomon, such approaches are undermined by studies of deliberation in groups that demonstrate "groupthink" and related phenomena. Due to these phenomena, results of deliberation are often worse than those achieved without deliberation. However, the group dynamics that lead to groupthink clearly fail to be relevant in the situation where the critical interaction consists in responding to the written comments of an anonymous reviewer. (Rolin 2011, 32, fn makes a similar point talking about dispersed scientific communities.) Some of the critical interactions that Longino's account describes are thus exempt from Solomon's criticism. This response is nevertheless partial because other critical interactions also covered by Longino's approach—for example, discussions within a research group—are not so exempt. In her response to Solomon's paper, Deborah Tollefsen (2006) discusses how the effect of groupthink may be mitigated within research groups; this may offer another part of the response. (Tollefsen herself mentions the relevance of her argument for Longino's approach.)

challenge may allow a variety of responses. For example, one may choose to retract one's claim or to change it as requested by the reviewer; to provide additional arguments to support the initial claim; to take the paper elsewhere where it would not raise similar criticism; to incorporate response to previous criticisms into the next paper etc. In doing so, each individual may demonstrate both openness to criticism in some respects and unwillingness or inability to change in response to criticism in others. However, as individuals' claims do not become a part of community's knowledge automatically, this inability needs not threaten community's ability to be objective. The ability of the individual for the detachment and control over one's subjective biases is less important than the organisation of communal practices. Therefore, as complete openness of individuals is not necessary for successful critical dialogue in Longino's account, there is no need to suggest that an "implicit" conception of individual capable of such openness is necessary in order to make her account work.

I develop this argument further suggesting that limited openness to criticism is not only non-threatening but can also be to some degree desirable for maintaining objectivity in Longino's account. In order to do so, I turn to some aspects of Mill's argument that Biddle overlooks.

As Biddle justly stresses, Mill's ideal individual—the "wise man" (1978, 19)—is someone capable of listening to all possible objections and potentially ready to change any of beliefs in response to them. This, as Biddle argues, presupposes the ability to distance oneself from one's most central beliefs in the way that is associated with "unencumbered selves". Acknowledging that, I nevertheless suggest that there are other strands in Mill's writings. I interpret them as supporting the view that not everyone is required to show this degree of detachment and community can live with, and benefit from, having less responsive members. In other words, I argue that some aspects of Mill's argument can be interpreted as supporting the social interpretation of responsiveness.

The aim of Mill's defence of freedom of discussion is to show that it is community that benefits from the toleration of dissenting opinions—silencing an opinion is not "simply a private injury", but an act of "robbing the human race" (Mill 1978, 16). It is above all the majority, those holding the currently predominating opinions that enjoy the benefits that Mill associates with free discussion. These benefits are the replacement of false opinions with true ones, the completion of incomplete true opinions, and the better appreciation of the meaning and justification of complete truths.

The benefit, in turn, is the greater the stronger the dissenter's opinions are and the more vigorous their defence. As Mill stresses, someone holding the predominating opinion "must be able to hear [arguments] from persons who actually believe them, who defend them in earnest and do their very utmost for them" (Mill 1978, 35). While Mill welcomes the ultimate convergence of opinions on truth, he also stresses that losing the benefit of having persistent and eager opponents is "no trifling drawback" (Mill 1978, 42) of this process. As long as it is not completed, community should welcome those defending deeply held dissenting views. The latter should be seen as a fortunate "spontaneous" opportunity to test the majority's views against the most serious objections. The community should "rejoice that there is someone to do for us what we otherwise ought … to do with much greater labour for ourselves" (Mill 1978, 43). Neither are limitations of openness on the part of those defending alternative opinions necessarily problematic, particularly given the imperfect state of community: "so long as popular truth is one-sided, it is more desirable than otherwise that unpopular truth should have one-sided assertors, too" (Mill 1978, 44).

Thus, the presence of those unwilling or unable to question certain of their opinions—"advocates", "conditioned selves"—does not necessarily threaten community's pursuit of truth. Rather, it may benefit community as long as there is enough of those who are capable of questioning and improving their opinions in light of opposing views. What is required is that there is enough of those whom Mill characterises as "the calmer and more disinterested bystander" on whom "this collision of opinions works its salutary effect" (Mill 1978, 49). So, while disinterestedness remains the ideal for the individual, the community as a whole may operate successfully as a "free marketplace of ideas" even if not every of its members realises this ideal. In other words, there are reasons to talk about the collective, rather than the individualistic interpretation of the requirements posed by the ideal. Looking at Longino's account as embedded in Mill's may thus be compatible with interpreting the criterion of uptake socially.

Again, it is important to consider the impact of my conclusions on Biddle's argument. Biddle argues that both Longino's and Mill's texts point in the direction of the individualistic interpretation of uptake. In this section, I argue that this interpretation fails to take into account other important claims made by Longino and Mill. Longino's repeated denial of the possibility of the "unconditioned self" and her insistence that the discussion of objectivity be focused on the community-level processes contradict the reading Biddle proposes. Even Mill, who sees openness to criticism as the only possible basis for "wisdom", may be read as supporting the social interpretation of this openness. Mill shows that community as a whole may benefit from passionate and persistent proponents of alternative views. I conclude that the reading Biddle proposes is ultimately inadequate, missing important strands of Mill's and Longino's thought.

At the same time, Biddle's argument attracts attention to an important tension in Longino's account. For a community to be responsive, at least some of its members must be. Accordingly, even for a proponent of the social interpretation of uptake, improvement of the responsiveness requires the growth of the number of responsive individuals and the degree of their responsiveness. The openness of the "unencumbered self" may be seen as the ultimate stage of this development. I have shown that Longino approaches knowing subjects as situated and thus incapable of complete openness as a matter of fact. I have also discussed how Mill describes the way community could benefit from such an inability. Nevertheless, there remains an important ambiguity. What I have attempted to show is that this ambiguity does not undermine the social character of Longino's account in the way that Biddle argues it does.

5.3.5 Conclusion

With my argument, I challenged Biddle's denial of the social nature of Longino's account in two different ways. First, I argued against the general idea at the centre of Biddle's argument—the idea that the "unencumbered self" that Biddle sees as the conception of individual underlying Longino's account would alone be able to be objective in the same way as community is. Drawing on Collins's and Kusch's ideas I showed that a crucial element of objectivity—being able to evaluate one's and others' claims in light of non-subjective norms and alternative perspectives—can be acquired, kept up to date and applied correctly only through socialisation and continuing participation in community. Second, I argued that Biddle's interpretation of Longino's account fails to acknowedge important aspects of her position. In her text, Longino explicitly adopts the conception of located and conditioned self and stresses the importance of approaching objectivity on the level of community. Moreover, it is possible to make an argument that community as a whole can benefit from "encumbered selves" and thus that requiring the fulfilment of the ideal of openness from everyone is not strictly necessary.

I conclude that contrary to Biddle's claim, there are strong reasons to characterise Longino's account of objectivity as social, both in seeing community as necessary for objectivity and in seeing the knowing subject as socially located and conditioned. More generally, the necessity of community for critical evaluation of one's and others' claims means that any account that focuses on criticism has to be social. In Longino's account of objectivity there is no irony of the kind I have shown in the case of Kitcher's approach.

5.4 Using Longino's account to discuss the social organisation of science

Discussing proposals about the social organisation of science in the first two chapters, I outlined some considerations that such a proposal should take into account. Later, the overview of the aspects of Kitcher's approach with which I am in agreement provided an example of one way these desiderata may be satisfied. The aim of this section is to show how Longino's account can also provide the basis for developing an account that satisfies them. Using Kitcher's social account of science as a foil, I describe how Longino's approach enables to treat the social aspects of science that Kitcher analyses, in a systematic way. I also show that Longino's account may in principle support proposals that cover the same aspects of organisation that Kitcher's well-ordered science, addressing the issues raised by consequences of research and the question of fairness with regard to them. I then argue how Longino's approach allows one to go beyond that and to develop an account of the social organisation of science that avoids the problems I have identified in Kitcher's approach. Building on Longino's ideas, it is possible to address the issues related to experts' objectivity, the utilisation of local knowledge, and public participation. Such a Longino-inspired approach, however, will inevitably be local compared to Kitcher's universal approach. I conclude the section by discussing some aspects of such a local approach.

Longino's argument about the role of background assumptions in evidential reasoning shows the potentially inescapable presence of values in the central practices of science and thus the thoroughly value-laden character of science. I suggest that as a result her account can be used to attract attention to the same social aspects of science that Kitcher has come to discuss in the most recent version of his account.

Introducing her account of evidential reasoning, Longino stresses that states of affairs neither carry labels identifying what they are evidence for nor dictate unambiguously how they should be described (Longino 1990b, 40–43). In both cases, some background assumptions have to be involved. Given different background assumptions, different classifications and conceptualisations of data are possible. Longino's account thus shows how different descriptions of the world are possible reflecting different interests and values. The commitment to this kind of pluralism is reaffirmed on the level of the products of scientific research—the point that both Longino and Kitcher make with the help of the map metaphor. Multiple scientific representations may all adequately depict their object just as different maps may do so for the terrain (although, as noted before, Longino and Kitcher disagree about the proper extent of this pluralist commitment). This pluralism of languages and maps is what Kitcher uses as the starting point in *Science, Truth, and Democracy* in order to open the way for discussing well-ordering of science.

Similarly, according to Longino, states of affairs do not by themselves dictate whether they are worth researching or under what description and with what methods they should be investigated. Discussing the constitution of the object of inquiry, Longino (1990b, 98–102) shows how social interests and values are in continuous interplay with cognitive values. As she stresses, defining the object of inquiry is "a matter of *decision, choice, and values as much as of discovery*" (Longino 1990b, 100, italics in the original). The selection of objects about which knowledge is sought and the kind of knowledge sought reflects particular social values and interests. Simultaneously, the kind of knowledge sought defines what will count as a good explanation and it is on this basis that cognitive values and their interpretations take shape. Longino's discussion of the intertwining of contextual and constitutive values, as well as her discussion of socio-political "valence" of particular cognitive values (discussed in chapter 2), attracts attention to the same issues that Kitcher discusses when talking about epistemic significance. This understanding of significance as interest- and value-laden, in turn, is what puts the stage of research-planning into the sphere of democratic deliberation and decision-making. (In Longino's account, the discussion of the role of values opens possibility for feminist and other alternative forms of science, characterised, just like Kitcher's well-ordered science, by a better conformity to the values and needs of communities practicing them.)

The recognition of the role of values also supports attention to the consequences of particular approaches and the way they interact with common value-laden assumptions, including those about particular social groups. This is the issue that Longino discusses in connection with feminist criticisms and other social concerns about science (see e.g., Longino 1987b and 1989; 1992c; 1994b; 2001). As previously described, concerns about the consequences of research also play a prominent role in the emergence of the model of well-ordered science.

Finally, constitutive values, related both by origin and by constant interaction to contextual values, influence a variety of decisions made throughout the research process. The potential involvement of values in background assumptions is one way values enter epistemic practices of science. In addition to that, in her discussion of alternative sets of epistemic values with their different valences, Longino proposes to understand these values as heuristics. They play a role in shaping questions, guiding the selection and representation of data, the choice of methods and the assessment of models and hypotheses (Longino 2008b, 79–80). Attracting attention to the role of values mediated by particular background assumptions and heuristics helps to see the thoroughly value-laden character of scientific inquiry. In *Science in a Democratic Society,* Kitcher argues for a similarly ubiquitous role of values, from the interplay of various kinds of values in the development of a scientific debate to the issue of inductive risk. The recognition of the role of values, in turn, enables Kitcher to extend the model of democratic involvement in research planning to cover also the stages of certification and decision-making in the situation of a controversy in science.

In Kitcher's approach, the account of the social dimensions of science has ultimately led to the two-level model that I discussed in the preceding chapter. The recognition of the involvement of social values in inquiry and the social character of scientific practices encourages attention to mechanisms that can ensure epistemic effectiveness of these practices and the integrity of their results. The recognition of the involvement of values in judgements about research directions, acceptance of results and their application supports the requirement to explicate the value judgements involved and to discuss them from the point of view of consequences of the decisions made and their fairness. So, in Kitcher's account, there are recommendations for improvements within scientific community and there is the level of well-ordered science that is to address the issues of science in society.

Kitcher's (2002c) review of *The Fate of Knowledge* can be read as recommending the same two-level approach for Longino. In the review, Kitcher argues that for democratisation of science two problems can be distinguished. One of them is the Millian Problem, or the failure to include members of certain social groups in scientific communities. As a result, epistemically better alternatives are never proposed. The second is the Interest Problem, or the failure of the accepted alternatives to address concerns of certain social groups. Kitcher stresses that the problems are distinct and require different approaches: "the ways of articulating the democratic ideal will differ" (Kitcher 2002c, 557) depending on the problem. From this point of view, Longino's account with its discussion of the criteria for scientific community and the requirement of inclusiveness can be seen as addressing only one of the two problems, the Millian Problem. On Kitcher's view, Longino's account needs to be developed in order to address the Interest Problem as well.

I believe that Longino's account has the potential for tackling both problems as Kitcher sets them. The brief overview given in the previous paragraphs shows that Longino's account exposes the social nature of the aspects of science that support Kitcher's argument for well-ordered science. It is thus in principle possible to use Longino's account to support the same changes in the organisation of science in society that Kitcher proposes. The role of values as heuristics throughout the process of research, the possibility of values' involvement in background assumptions and the interplay between these values and features of wider society makes decisions about research directions and certification and application of research results thoroughly value-laden. Accordingly, one may propose that the involvement of the public is called for in order to manage these value- and interest-laden aspects of science appropriately.

In the preceding chapter I discussed the problems into which Kitcher's two-level approach runs. In the subsequent paragraphs I show how Longino's account provides further arguments against the model of well-ordered science. I then describe how the themes of the social organisation of science and of science in society are interconnected and how Longino's account allows addressing both.

Longino's discussion of the limitations of objectivity secured through intersubjective criticism serves as the basis for the integrated approach that I outline. As Longino shows, intersubjective criticism in community may be effective for exposing individual biases. Biases shared by the entire community, however, may survive criticism undetected. The probability of that is the higher, the more homogenous the community is.[90] Once the possibility of shared experts' biases is recognised, some of the central elements of the model of well-ordered science are put into doubt. In Kitcher's model, deliberators are tutored about existing research possibilities before decisions about future research directions are made. However, the very language used when describing current research possibilities may contain problematic values that remain undetected in experts' shared assumptions. This problem may be especially serious in the areas where the language may be expected to be strongly value-laden. (It is plausible that a considerable part of knowledge that is the most relevant for public needs belongs to this category.) Possible gaps in experts' knowledge when it comes to local contexts is another important limitation of experts' shared knowledge. These are the problems I identified for well-ordered science in the previous chapter, using Kitcher's discussions of classification and application in context and drawing on Lacey's and Wynne's analyses. I also showed how this approach to the role of the expert introduces tensions with Kitcher's account of the epistemic agent and Kitcher's non-correspondent account of knowledge.

I suggest that thanks to the recognition of the possibility of unrecognised shared assumptions, Longino's account can be used to support a different model of experts' interactions with the public. This model requires the involvement of a wide variety of perspectives and substantial criticism from various points of view and a rethinking of the relations between insiders and outsiders in community.

In her writings, Longino discusses a number of options for introducing alternative perspectives. First, Longino recommends communication between different communities:

[90] As noted before, Kourany (2010, 60–62) argues that this problem may not be fully resolved by a greater inclusiveness. Scientists from newly included groups may fail to produce a different kind of knowledge because they have to undergo socialisation in scientific community and thus risk acquiring and reproducing shared biases. This is why I believe that the proposals to increase the variety within scientific community only, such as James Brown's (2004) alternative to Kitcher's well-ordered science and K. Brad Wray's (2001) suggestion for preventing detrimental influence of biases, as well as Kitcher's own (2011c) are important but insufficient.

“One obvious solution is to require interaction across communities, or at least to require openness to criticism both from within and from outside the community” (Longino 2002a, 135). This solution places a more stringent duty of openness on scientific community. However, it leaves unspecified whether other communities in question are scientific communities or may include lay communities too.

Longino’s second line of argumentations offers a more inclusive view, albeit presented tentatively. In *Science as Social Knowledge*, Longino writes that

> The precise extension of “scientific community” is here left unspecified. *If* it includes those interested in and affected by scientific inquiry, *then* it is much broader than the class of those professionally engaged in scientific research. (Longino 1990b, 69, fn; italics mine)

In *The Fate of Knowledge*, Longino is more definite but still treats the issue as opening a number of unresolved questions:

> The tempered equality condition also raises complex questions of community membership. It requires both that scientific communities be inclusive of relevant subgroups within the society supporting those communities and that communities attend to criticism originating from “outsiders”. It makes us ask who constitutes the “we” for any given group, and what the criteria are for providing an answer. Are “we” those actively engaged in producing knowledge of a certain kind for a certain aim, as members of a laboratory group are, or should “we” encompass also all those potentially affected by that knowledge? (Longino 2002a, 133–134)

I suggest that the questions Longino poses should be approached on a case-by-case basis. This reading is supported by some remarks of Longino’s. On the one hand, she stresses that perspectives included should be “relevant” (Longino 2002a, 131, fn). On other hand, Longino points out “criticism may originate from an indeterminate number of points of view, none of which may be excluded from the community’s interactions without cognitive impairment” (Longino 2002a, 133). So, what points of view are relevant and who, for the issue under discussion, belongs to community may be an open question to be answered depending on the issue at hand.

My previous discussion of relevant lay perspectives and local knowledge supports the possibility that in some cases the requirement for a greater inclusion of the lay public can be expected from such an issue-specific analysis. Discussing the possibility of value-laden classifications when criticising Kitcher, I suggested that the inclusion of those affected by the knowledge produced may be important for uncovering those values. So, there are reasons to maintain that the knowledge-productive “we” may in some cases—those that revolve around such potentially value-laden notions—involve, at least “part-time”, those affected. What counts as the grounds for inclusiveness in science policy in order to address the Interest Problem may thus sometimes be relevant for improving research community, or addressing what is thought to be the Millian Problem.

Besides that, a strong case can be made on the basis of Longino’s account, with its focus on inclusive critical interactions, for the involvement of those capable of contributing substantial information, criticisms, and perspectives. As a result, those who are not a given community’s members “full-time” and do not participate in the production of knowledge as their main activity may be treated as community members for the sake of addressing a specific issue. In such cases, they bring in knowledge, expertise and perspectives they have acquired as full-time members of other communities.[91]

[91] One may ask whether “part-time members” have the same duties of responsiveness to criticism as the core community. If not, can they still be considered community members? In her discussion of participatory projects in cultural research, Inkeri Koskinen (2014) argues that there is a tension between the long-standing tradition of avoiding appraisal of alternative knowledge systems and the growing trend of treating informants as collaborators. If there is no critical appraisal, lay informants cannot be real collaborators in a knowledge-productive community understood in Longino’s terms. As my choice of words shows, I think that individuals

Longino's discussion of knowledge production as crucially depending on community's control over background assumptions can thus be used to support inclusive critical involvement of various perspectives, both expert and lay, with different aspects of scientific inquiry. I suggest that approaching the organisation of the relations between the expert community and the public from this perspective has a number of advantages compared to Kitcher's well-ordered science. First, it recognises the limitations in the knowledge-producing practices of the expert community that may have an impact on the possibility of creating socially relevant and practically applicable knowledge. It then offers a possibility for addressing these limitations by encouraging the use of knowledge and perspectives of "outsiders", including knowledgeable laypersons. Second, it avoids the tension between the characterisation of scientists as members of scientific community on the one hand and as experts in the interactions with laypersons on the other. In Longino's approach, objectivity is in both cases secured through critical interactions. It is achieved on the community level rather than through the individual's ability to realise the ideal of the impartial expert or the epistemically and ethically impeccable deliberator.[92] More generally, it avoids the tension between the way knowledge and its production are described in the context of scientific community (contested and value-laden) and of science policy (represented in an objective "atlas of scientific significance). Previously, I criticised the inconsistency of Kitcher's account in this respect, drawing on Mark Brown's discussion of representation and correspondence. Finally, the approach based on Longino's ideas invites actual public involvement with science, avoiding the inconsistency of attempting democratisation without democratic participation.

So, on the basis of Longino's arguments it is possible to offer an approach to the social organisation of science that addresses many of the issues Kitcher's well-ordered science covers, without running into the problems I have identified in connection with Kitcher's approach. In the thesis so far, I have argued that knowing the consequential and value-laden character of science, we have grounds to make proposals for a more socially relevant, responsible and trust-inducing science. I have also argued that this kind of science requires a wider involvement of relevant perspectives in science in order to address potentially value-laden concepts and frameworks and possible gaps in experts' knowledge. The more potentially value-laden inquiry, the more such an involvement is necessary. This kind of inclusiveness, in turn, is what Longino's account of objectivity requires.[93] The advantage of approach based on this account is that it enables seeing how making science democratic in the sense of a better harmony with democratic society may sometimes require opening it to public participation in the production of knowledge, in addition to the public participation in planning, certification and application of research.[94]

In one important respect, however, Longino's approach does not allow achieving what Kitcher attempts to achieve with the model of well-ordered science. In the terms of the

---

may be involved in community to a different degree and with respect to different issues. So, it is likely to be a case-specific matter how much responsiveness to criticism may be expected.

[92] I argued for this community-level understanding of objectivity when discussing Biddle's argument. I also defended the necessity of actual participation in community for the possibility of objectivity, which is relevant here as well.

[93] The keywords for laypersons' input are thus the same as in Douglas (2005): framings, local knowledge and values. In my argument, however, the issue of local knowledge plays a vastly more important role, supported by arguments such as Cartwright's and case studies such as Wynne's. Values are also approached differently: in terms of value-laden concepts and assumptions rather than value judgements.

[94] This distinction is similar to the distinction between democratising science and democratising science policy that Longino makes in her response to Kitcher's (2002c) criticism. Longino (2002b) suggests that well-ordered science attempts to democratise science policy, rather than science. According to Longino, an account that shows underdetermination and plurality of science supports the necessity to democratise both.

distinction that I introduced in the previous chapter when discussing the possible approaches to solving the problems of cacophony, it is a local-level, rather than a universal approach. In the previous chapter, I argued that Kitcher's attempt to develop a universal approach fails and that a local approach may be the only option. In the reminder of the section I continue to argue why the approach I outline is inescapably local and I respond to some arguments about the problems of such an approach on the example of Longino's local epistemology.

In the preceding chapter, I suggested that the approach that focuses on the conditions for critical dialogue involving a wide variety of perspectives is likely to be local because the basis of shared standards and norms is likely to be community-, context- or issue-specific. Longino (Longino and Lennon 1997, 28–30 and 33–34; Longino 2002a, 173–174 and 184–189) makes this point when she argues that a normative epistemology is local. Particular standards are only binding for those who share the aims to which these standards are related and which they are supposed to advance. Someone who does not share these aims has no reason to follow the standards. Successful critical communities thus require some shared aims that are specific enough to guide their critical activities. However, the more specific the norms are, the less widely held they are likely to be.

Another reason why such an approach is bound to be local is related to the notion of relevant perspective. Being able to offer such a relevant perspective requires the possession of relevant experience or specialist knowledge and some familiarity with others' perspectives and knowledge. Both are likely to be context- or issue-specific. This is the point that Collins and Evans (2002) make in their interpretation of Wynne's sheep farmers case study. Collins and Evans argue that the study does not support indiscriminate involvement of the public in technical issues. What made the farmers' perspectives relevant was not that they were laypersons, but that they had non-certified specialist knowledge, acquired thanks to the deep familiarity with the location and their occupation. Making this kind of expert knowledge visible and integrating it with other relevant kinds of expertise—like that of science—would in turn require the kind of familiarity with both that Collins and Evans call "interactional expertise": the ability to interact meaningfully with (other) experts. This kind of familiarity is also likely to be context-specific, requiring learning and immersion in the context.[95]

Thinking in terms of local knowledge and particular contexts and issues helps to address two related problems for Longino's account of local epistemologies. Longino indicates one of the problems herself. The existence of shared norms can be taken for granted in the case of a specific community because having such shared norms is a part of being a community. In the case of interactions between several communities (or the introduction of "outsiders" into community and other developments that blur community's borders) existence of shared norms and standards cannot be taken for granted. Such norms may have to be worked out. The process of working out these norms, however, cannot be guaranteed to be easy or always successful. As Longino (2002a, 130, fn) writes,

> For intercommunity criticism, effort is required to identify commonalities and produce agreement or disagreement. This effort is subject to all the vicissitudes of cross-cultural communication and translation.

Shared aims and standards are thus something to be worked out. I suggest that a shared concern or shared location may provide a chance to do that. One can imagine the endless regress where members of a diverse group have to agree on some shared norms and aims in order to start to work out shared norms and aims for the discussion. Before that, the members

[95] Longino makes a similar point when she writes about the possibility of feminist science: in order to have a chance to succeed (and be taken seriously in the first place) the alternatives will have to be "local, that is, specific to a particular area of research" and "in some way continuous with existing scientific work" (Longino 1990b, 193–194).

of the group have to agree upon shared aims with respect to this agreement etc. Having some shared context, for example, around a particular common concern, may in practice stop such a regress and enable the community to avoid the problem of cacophony. Douglas's (2009, 164–167) discussion of the joint assessment in Valdez, mentioned in the discussion of her proposal in chapter 2, may be an example of such a productive cooperation of several communities that otherwise have different aims and practices.

Thinking in terms of specific concerns that may go beyond the usual aims of a community helps to address another problem for Longino's account of local epistemology. Several critics point out that thoroughly local epistemology precludes the possibility of intercommunity criticism. If communities do not share aims and standards, their criticisms against each other do not have normative power (see, e.g., Longino and Lennon 1997, 37–54; Büter 2010). The solution I offer to this problem is to focus on working some shared norms that would unite otherwise different communities in a particular context for the sake of cooperative knowledge production and action in this context. So, the solution is not to make one community fully accept criticisms reflecting the aims and standards of the other but to explicate points of agreement and to supplement them with situation-specific shared aims. Longino (1997, 29) writes about the "areas of overlap or intersection [that make] possible critical interaction among as well as within communities". To the identification of such areas, the creation of case-specific shared aims and norms may be added. Such norms are especially important if the communities are so diverse as to make the possible areas of overlap minimal. Shared aims, in turn, provide the possibility to criticise a particular community's norms and practices to the degree they are relevant for the achievement of these aims. Intercommunity criticism may not be effective when it goes beyond those shared commitments. If a community does not recognise the relevance of criticism for its inner aims, the criticism is likely to remain unheeded. However, to the degree "overlaps" are identified and novel norms are established in connection with a particular issue, criticism may proceed where it matters most for the joint knowledge production and action. What the notion of different local epistemologies helps to see is that such a result may require a dedicated effort. Simply bringing different communities in contact does not by itself guarantee a productive critical exchange.

In this section, I argued that Longino's account of values in science and scientific objectivity enables one to offer an approach to the social organisation of science that satisfies some of the desiderata I had previously proposed. Such a Longino-inspired alternative to Kitcher's approach requires that the issues related to the internal organisation of scientific community and the place of science in democracy be approached on the same basis. Ensuring both objectivity of knowledge produced and its social usefulness and responsibility relies on the involvement of a variety of perspectives, including those of certain "outsiders". Longino's account of local epistemologies can then be used to discuss how such communication across communities may become possible. The solution is achieved, however, at the cost of universality. If Longino's approach is to be successful at all, it will be successful locally.

The aim of the next section is to argue further for the advantages of Longino's approach by showing how it attracts attention to the philosophical importance of the wider political context of science and changes in it.

5.5 Using Longino's account to make philosophy of science political: why?

In chapter 1, discussing James Brown's proposal about the socialisation of biomedical research, I pointed out one way in which such a proposal may have political connections. Making a new form of organisation of science possible may require political action. The aim of this section is to show how the possibility of the form of the social organisation of science recommended on the basis of Longino's account is related to social and political issues that may not be solvable by a single decisive political action. Seeing these connections allows one to perceive more clearly what is required for a specific form of the social reorganisation of science to be possible in practice.

I begin exposing these connections with addressing the question of the critical and meliorative potential of Longino's approach. In the previous section, I argued that Longino's account can be used to support proposals for the organisation of science that involve public participation in the production of knowledge. One may, however, ask how strong the meliorative orientation of Longino's account is. The argument I propose is based primarily on Longino's account of objectivity. Longino characterises it as an "explication of 'objectivity', or a partial explication of 'knowledge'" (Longino 2002a, 173). One may suggest that if it is so, communities that we recognise as unproblematically knowledge-productive are objective almost by definition, especially if we add to this that objectivity is meant to be a matter of degree (Longino 1990b, 76). With the exception of clearly pathological cases, such as science under the Nazis, actual scientific communities may be expected to demonstrate objectivity to at least some degree. If they are already good enough (as technological successfulness may give reasons to think), is there a reason to make proposals to reorganise these communities in order to improve them?

One part of the response is to point out the meliorative proposals that Longino actually makes, such as the argument about the possibility of feminist science. I suggest that besides that, throughout Longino's arguments a strong critical position with respect to actual scientific communities is expressed. These communities are indeed objective to some degree but they also fail to realise the ideal of objectivity more fully. As Longino points out,

> Several conditions can limit the extent of criticism and hence diminish a scientific community's objectivity without resulting in a completely or intentionally closed society (for example, such as characterised Soviet science under Stalin or some areas of Nazi science). (Longino 1990b, 79)

Thus, it is possible to recognise that objectivity allows a variation by degree without becoming too sanguine about the objectivity of actual knowledge-producing communities.

On Longino's view, mechanisms ensuring objectivity of knowledge claims can only be social. It is intersubjective criticism in community that prevents uncontrollable influence of subjective biases on knowledge claims made and accepted in community. Given the importance of criticism, Longino discusses conditions that could enable effective criticism in community, from establishing channels for presenting criticism to ensuring the equality of intellectual authority among those presenting it. These conditions are summarised in Longino's criteria for transformative criticism and the related discussion of the necessity of a variety of perspectives in the critical dialogue. Thinking about these conditions for objectivity within scientific community, in turn, allows seeing the relevance of several features of the wider social context of science.

In this section, I focus on three large groups of issues. These are the issues related to the equality in community and pluralism of perspectives; those related to privatisation and commercialisation of knowledge production; and those related to the global problems that scientific community and wider society face. Doing so, I discuss both Longino's own remarks that show recognition of the relevance of these issues for her vision of objective scientific community and arguments made by her critics. I describe how the possibilities for realising the conditions for objectivity depend on the developments in the wider social and political context of science. Accordingly, doing philosophy of science requires an understanding of these developments and a way to establish a connection with them. In conclusion, I discuss the implications of this contact with the political sphere for the local character of the approach

I have been advocating. I suggest that despite some important complications, "politisation" of philosophy of science may provide further support for such a local approach. I develop this suggestion in detail in the final chapter of the thesis.

In my summary of Longino's account I showed that for her the question of the organisation of scientific community has a political dimension. Community's failure to include members of certain social groups reflects certain features of the wider political context, which means, in turn, that rectifying it would require political action. This action may be inseparable from conflict. As Longino stresses,

> in a power stratified society, the inclusion of the less powerful, and hence of models that could serve as a resource for criticism of the received wisdom in the community of science, will always be a matter of conflict. (Longino 1991, 674)

Longino's account thus attracts attention to the fact that failures of objectivity within scientific community are connected with inequalities in wider society. Improving objectivity of scientific communities would require addressing them. Longino mentions several aspects of the problem of inequality that go beyond the exclusion of members of some social groups from scientific community. First, even if members of these groups are in principle admitted, they may not be in a position to offer well-developed alternatives because the development of such alternatives requires intellectual and material resources (Longino 1987a; 1991 and 1993a). These resources, however, are currently distributed very unequally. Creating "niches" where alternatives could be developed would require changes in the context where science is practiced: "[t]he practice of science is too materially dependent on its socio-political context for significant change to be possible without changes in that context" (Longino 1995, 396). Second, even if members of marginalised groups are included, there may be systematic factors that diminish their credibility for other members of community (Longino 2008b, 83–84 refers in this connection to the notion of "epistemic injustice" developed by Miranda Fricker).

A number of philosophers discussing Longino's work also bring attention to the issues related to inequality. One example is Biddle's (2007) argument that Longino's approach to the social organisation of science (as well as Kitcher's) is inadequate because it presupposes an "equal playing field" (Biddle 2007, 33). This assumption ignores the large power inequalities that characterise contemporary science, especially the ability of financially powerful private companies to shape the research process in their interests. Another example is Clough's (2013) discussion of the marginalisation of knowers, which in turn draws on Intemann (2010). Historically, some knowers (Clough writes in terms of knowers embodied in a particular way) have been seen as incapable of joining knowledge-producing communities regardless of their actual competence.

Objectivity of scientific communities as Longino describes it thus depends on the way phenomena of exclusion, marginalisation, and various kinds of inequality are addressed in wider society. The arguments summarised so far attract attention to persistent problems. The discussion of obstacles for realising the conditions of objectivity, however, can also be used to attract attention to societal developments that may help to improve the situation. These developments may take a variety of forms. They may include very specific intentional interventions, such as education and science policy initiatives to get more women and members of minority groups into science and technology education and to improve the retention of female and minority scientists in academia. They may also include more general developments towards a greater equality, more equal access to various kinds of resources and less biased perceptions of ability and credibility. For understanding the possibility to improve epistemic community in the spirit of Longino's account, understanding such developments is necessary.

Biddle discusses the problem of power inequality in the context of privatisation and commercialisation of research. This issue poses another important problem for maintaining and improving objectivity of communities. As noted earlier, Longino has commented on the influence of the wider social context of scientific community on its practices. Commercialisation and privatisation of science is a prominent example of such an influence (military secrecy is another example Longino (1984; 1986) previously discussed). Most significantly for Longino's account of objectivity, privatisation undermines the traditional practices of scientific communication. Dissemination and discussion of results is limited due to various forms of secrecy. The detrimental influence of these developments goes beyond the doubt cast over the credibility of "privatised" results. Secrecy and the subversion of peer-review impoverish the pool of knowledge claims that can be used in critical interactions and therefore have a negative impact on critical practices of science in general. The possibility of intersubjective control over individual biases is thus weakened just as the threat of bias due to the financial stakes in the results grows.

The failure to take into account the ever-increasing privatisation of science and its influence on the process of research is one of the criticisms Longino (2002c; 2006) raises against Kitcher's well-ordered science.[96] Longino herself, however, has been the target of similar criticism. As noted earlier, Biddle (2007) criticises Longino's approach, arguing that Longino is unable to show how contemporary institutional arrangements of privatised science can be made to approximate the philosophical ideal proposed. Jukola (2016) is more sympathetic to Longino's ideas—she too defends a version of the social approach to objectivity, arguing that the individualistic approach is inadequate. However, Jukola is concerned with the numerous ways commercialisation of research undermines social practices of objectivity. Among them are the influence of financial interests on the choice of approaches (as a result, the diversity of approaches is limited), methodological choices in the course of research (and so the results are skewed in the direction preferred by the private companies) and the publication of results (negative results do not get published). Pinto (2014) similarly discusses aspects of contemporary commercialised science that pose problems for Longino's norms of critical contextual empiricism. Pinto lists issues such as the changes in publication practices (the emergence of private peer-review services and pharmaceutical companies' publication planning teams) and the rise of tactics for abusing the requirement of uptake by presenting numerous criticisms with the sole aim of muddling the debate.[97]

Also in this case, the relevance and applicability of Longino's criteria depend on the larger features of the social organisation of science. The possibility of productive transformative criticism may depend on relatively specific factors such as the journal policies to prevent incomplete publication of data. It is also influenced by more general aspects of research and development policy such as the predominating funding regimes, laws concerning the protection of intellectual property, regulations for licensing of various commercial products etc. Again, I suggest that realising Longino's ideas requires understanding developments in science policy and wider society. Some of these developments may threaten objectivity of scientific communities. Others may open possibilities for improving it, if only locally or in some respects.

[96] Kitcher's (2002a) response to Longino is that the commitment to the ideal of well-ordered science requires resistance to privatisation of science as incompatible with it. (Reflecting the differences in their approaches, Kitcher focuses on the issues related to the research agenda rather than the influence on practices of science that Longino is primarily concerned about.) Kitcher does not specify, however, how the trend of privatisation can be reversed.

[97] Pinto (2015) argues that Kitcher's well-ordered science seems similarly unable to deal with commercialisation of science, and discusses some of these examples, including publication planning.

Discussion of the privatisation of science often touches another aspect of its contemporary organisation—its globalised character (for example, Pinto (2014) writes about "globalised privatisation"). Longino herself comments on globalisation in her criticism of Kitcher: "Modern science is not only privately sponsored, but it has gone global. How can individual societies or communities maintain the control envisaged?" (Longino 2002c, 568). A similar question, however, applies to Longino's account. Privatisation of science poses problems for the kind of objective community Longino envisages. The global character of this process adds layers of further problems. For example, there may be possibilities for bypassing the epistemically consequential regulations I mentioned in the previous paragraph, by outsourcing research to other countries.

There is another sense of globalisation that Longino briefly discusses in connection with her account—the global character of problems that science is expected to solve:

> The various degradations of the environment, from the destruction of the world's rain forests to the evaporation of the ozone layer, pose technical and political problems. So does the need for clean and renewable energy sources. These are not problems from whose consideration we can excuse ourselves on political grounds, for we will all be affected by whatever actions (including inaction) are taken. (Longino 1990b, 213–214)

Familiarly, the possibility of increasing objectivity in scientific community depends on what happens in its wider (in this case, the widest possible) context. Both possibilities to avoid the problems identified in connection with privatisation of science, this time on the global scale, and to coordinate efforts to address global problems depend on international laws and regulations and institutions. Among the relevant factors may be international intellectual property agreements, regulations for organising clinical trials abroad, international agreements for environment protections etc.

So far I have discussed several features of the wider social context of science on which the possibilities for improving objectivity of scientific community or counteracting developments that threaten it may depend. Certain features of the context, both long-standing (such as the marginalisation of certain knowers) and relatively recent (such as privatisation of research), contribute to making scientific communities less objective than they could be. On the other hand, specific developments in the sphere of laws, regulations and institutions may create possibilities, even if niche ones, for realising Longino's ideal of objectivity more fully. Longino's account can thus be used to provide a lens through which to explore various aspects of science policy and the organisation of science in society. Some of these aspects are already discussed by philosophers—for example, under the heading of commercialisation of science. I suggest that Longino's account has the advantage of enabling a more focused discussion, as her account of the conditions for maintaining objectivity and through that knowledge production allows identifying specific obstacles and opportunities in the social context of science. In particular, in the context of my argument thinking in terms of specific conditions for successful critical dialogue may help to see obstacles for the improvement of the current state of scientific community and to formulate steps required for overcoming these obstacles clearer than Kitcher's discussion in terms of ideal experts and ideal deliberators.

There may be two ways to think about the role of the philosopher with respect to these political developments.[98] One may treat the identification of these obstacles for a better form of the social organisation of science as a call for new actions to overcome them. The philosopher is thus seen as someone who may demand the political action required. Another possibility is to focus on the developments that are already happening and their potential epistemic impact. The contemporary political context for science is already characterised by some willingness to act, for example, in order to improve the position of women in academia

[98] I return to the discussion of this theme in the section on the roles for the philosopher of science.

or to regulate the publication of clinical trials data. In the preceding paragraphs I proposed to think about these developments as opening possibilities for improving scientific communities in Longino's spirit. In the next chapter I discuss the ways to connect philosophy of science and such ongoing developments. Before that I outline a potential objection to the discussion of a Longino-inspired approach in the political context.

In the previous section, describing my approach to using Longino's ideas, I argued that it is strongly local. Now, I place it into the world of globalised politics. This may raise concerns about the possibility to preserve the local character of the approach. There is an obvious tension between my insistence that most productive participatory practices are likely to be bound to particular local communities and the global character of contemporary political developments. Longino writes about a related problem when discussing the public involvement in making decisions about the locations for hazardous experiments:

> The difficulty with this model is that it seems to work best for real communities, towns such as Cambridge, Massachusetts, and Berkeley, California, in which ordinary citizens have meaningful access to decision-making. If the hazards of rDNA research are real, however, it is not clear that they will respect city or state or national boundaries. (Longino 1984, 58)

The social and political developments I have described may similarly transcend local borders. I have thus identified a predicament for my interpretation of Longino's ideas: it simultaneously stresses the local character of knowledge-productive communities and describes their dependence on the aspects of contemporary globalised democratic politics. One of the aims of the following chapter is to address this predicament. There, I argue that democracy itself is in an important sense local and thus its discussion itself supports a local approach.

The main aim of the next chapter is to substantiate the claim that political developments may support the emergence of local conditions for improving objectivity of science and science policy with the help of greater public inclusiveness. Doing so, I demonstrate that Longino's account provides one possible basis for productive cooperation between philosophy of science and political science, realising the ambition that I have praised in the case of Kitcher's account.

5.6 Conclusion

The aim of this chapter was to show that Longino's account can successfully serve as the basis for approaching the question of the social organisation of science in accordance with the desiderata I have been discussing. Preparing the ground for my argument, I first addressed one major disagreement between Kitcher's and Longino's gradually converging accounts—underdetermination. I showed that Kitcher's arguments against the thesis of underdetermination understood as the possibility of multiple empirically equivalent hypotheses are not applicable to the version of logical underdetermination on which Longino's account is built. Second, I responded to the concern that Longino's account does not constitute a suitable alternative to Kitcher's, as it is not, as Biddle argues, really social. In my response, I demonstrated the necessarily social character of objectivity as Longino describes it with the help of the arguments about tacit knowledge and rule-following. I also argued that Longino's account recognises the social character of knowers for whom actual participation in community is the only possible basis for objectivity.

Having disposed of these objections, I described how Longino account can be used to attract attention to various aspects of the sociality of science and to support proposals about its social organisation. I suggested that it is able to cover the same aspects of science that Kitcher's well-ordered science covers. Even more importantly, Longino's account can go beyond that, as it can be used to support wide public involvement in both research and science policy in order to address gaps and blind spots in experts' knowledge and maintain its objectivity. This allows it to avoid the problems I have identified in connection with Kitcher's solution. A Longino-inspired approach avoids the contradictions in the concept of expert and the problematic approach to public participation that characterise Kitcher's approach. At the same time this approach is strongly local. Productive public cooperation depends on the creation of shared standards for highly diverse communities and this process is likely to be context-specific.

In the concluding section I began to address the question about the possibility to realise such an approach to the social organisation of science, discussing the influence of the features of wider social context of science. I suggested that for the social organisation of science on the basis of Longino's ideas, many of developments in science policy and wider democratic policy may be highly relevant. The realisation of this approach to the organisation of science may depend on the possibilities created by specific laws and institutions. The need to understand these developments and establish a contact with them encourages cooperation between philosophy of science and political science. The aim of the final chapter is to discuss the form such cooperation may take.

CHAPTER 6. PHILOSOPHY GONE POLITICAL

6.1 Introduction

The aim of the previous chapter was to discuss how Longino's account of the social nature of science can serve as the basis for approaching the question of the social organisation of science. A prominent feature of the form of organisation I described is the involvement of laypersons possessing relevant perspectives and local knowledge in the production of knowledge. At the end of that chapter I showed how thinking in terms of Longino's criteria for objective communities directs one's attention to features of the wider political and social context that may enable, or threaten, the kind of objectivity understood as inclusiveness. I suggested that this makes developments in science policy highly relevant for philosophy of science. The possibility of inclusive epistemic communities and productive critical dialogue may depend on both very specific laws and institutions and wider trends, such as developments towards a greater social equality. Understanding possibilities to realise a philosophical proposal for the social organisation of science requires understanding these ongoing developments and establishing a connection with them.

Such a connection may be established in a number of ways. One possibility is to offer analyses that show epistemic impact (especially the potentially negative influence) of various institutions and practices that touch the production of knowledge. This is what, for example, James Brown, Biddle, Jukola, Pinto and others do when they discuss the epistemic impact of commercialisation and privatisation of research.

I believe that this approach produces valuable results and that pursuing it constitutes an important task for philosophy of science. In this chapter, however, I outline a different way to establishing contact between philosophy of science and analyses of the social and political context of science. Over the recent decades, there have been numerous attempts to democratise science policy by increasing public participation in deliberation and decision-making. I suggest that Longino's account of objectivity as based on inclusive critical discussion allows a connection with these attempts and related theoretical reflections in political science. Despite having primarily political, ethical or pragmatic rather than explicitly epistemic motivations, these science policy developments may share, due to the common ideas about objectivity, important similarities with what a philosophical proposal would recommend. The wealth of empirical information about these developments in political science and science policy analyses can therefore be used to assess the practicability of philosophical proposals in light of attempts to democratise science policy made in practice. Two case studies derived from science policy analyses are presented in this chapter to substantiate this proposal. Additionally, I draw on these case studies in order to defend my position from a number of criticisms and to show its advantages compared to the model of well-ordered science. From the argument of this chapter and the case studies, a particular view of proposals for the organisation of science and the role of the philosopher emerges.

In the next section I discuss how philosophy of science can be brought into contact with political analyses, using some common ideas about objectivity as described in philosophy of science by Longino and in political theory by Mark Brown (2009). I describe the status of Longino's ideas in the approach I propose and I show how it avoids the criticism Biddle (2007) offers against Longino's account as abstract and practically inapplicable. I also discuss the relations between my approach and several other arguments that connect philosophical arguments and political analyses.

Showing the application of the approach described, in the third section I introduce the first of the case studies: an analysis of the attempt to democratise some aspects of German science policy in the early 1990s. I discuss this development in the context of Jasanoff's

(2005) analysis of civic epistemologies and I show how Jasanoff's approach demonstrates the local character of both science and democracy. I close the section by discussing some important implications that the German case has for arguments in philosophy of science. In particular, I discuss Kitcher's well-ordered science in general and his view of the relation between philosophy of science and research in political science specifically in light of the analysis of this case. I also defend the use of Longino's approach against the circularity objection raised by Anna Leuschner (2012).

In section four I discuss Wylie's work on some developments in the practices of American archaeology. The second case study continues to explore the possibility of niches for greater objectivity enabled by political developments—the possibility for which I have been arguing in these two chapters. In addition to that, it is meant to address some questions raised by the German case, above all, the question about the possibility of successful intercommunity communication. In the conclusion, I argue that also in this case there are important problems for an attempt to analyse this development in the framework of Kitcher's well-ordered science.

In the final section I discuss some lessons for philosophy of science that these examples provide. I also show how my position is related to several existing models for the philosopher's involvement with science policy or policy-relevant science issues.

6.2 Using Longino's account to make philosophy of science political: how?

In the previous chapter I argued that the existence of inclusive communities and productive dialogue as Longino describes them depends on various features of the social context of these communities. This makes understanding the relevant aspects of developmental processes that characterise this context an important task for my proposal.

There are many phenomena that can be analysed from the point of view of their epistemic influence on knowledge-producing communities. Given the central role of public participation in my argument, I focus on a loosely defined trend that has constituted an important feature of contemporary science policy. There, democratisation, understood in terms of wider public participation, has been an important development. Over the recent decades, there have been numerous attempts to make science and science policy more socially relevant, accountable, democratic and legitimate or to improve its epistemic quality by involving in different aspects of science policy, in addition to the traditional participants (experts and decision-makers), various representatives of the public. Numerous experimental forms of democratisation—technology assessments, citizen juries, polls, public consultations, consensus conferences etc.—have been tested during this time.[99] This raises an obvious question about the appropriate relation between the attempts to make science more democratic in science policy and in philosophical proposals such as Kitcher's and Longino's.

One attempt to bring the two in touch is Maxence Gaillard's (2013) case study that compares the recommendations of Kitcher's well-ordered science with the actual development of French public debates on nanotechnology. In this section, I similarly bring into contact a philosophical proposal and a political science analysis. However, I do not compare specific policy initiatives with a fully developed philosophical model. Instead, I

[99] Pleas for public participation in science policy have been around since at least J. D. Bernal. They greatly intensified—together with actual public participation—in the late 1960s and 1970s and have by now become an important feature of science policy in many countries, especially in Europe. Mark Brown (2009, 17–18 and 219–221) gives a brief overview of this development. An overview of the state of the field is given by Simon Joss (1999) in the introduction to a special issue of *Science and Public Policy*. Martin Lengwiler (2008) provides another overview, including a history of science–public relations beginning with the second half of the 19th century, in the introduction to a special issue of *Science, Technology, & Human Values*.

suggest going down to a more general level and discussing certain shared ideas—in my case, the ideas about the nature of objectivity—that underlie both the philosophical proposals and the developments in science policy.

I suggest that these similarities allow one to approach specific developments in the political sphere *as if* one of their aims were epistemic improvement that can be recommended on the basis of the philosophical proposal. Given the existence of important conceptual and motivational differences and given the political, institutional and cultural context that defines the development of a science policy initiative in practice, one should not expect a complete realisation of the philosophical ideal. Gaillard's analysis demonstrates that this is what happened in the French nanotechnology debate. Both the development and the results of the French experiment were different from what Kitcher's model of well-ordered science presupposes. To avoid this kind of disappointment, I suggest that philosophical proposals should be approached as a set of general ideas that are to be applied and developed into a more definite form in a specific context.

Approaching philosophical ideas in this way allows responding to the criticism about the abstract and practically inapplicable character of Longino's approach that Biddle (2007) presents. I argue that it is the general character of Longino's ideas that allows for a productive connection with the approaches developing similar intuitions in a different sphere. In order to be applicable, however, these general ideas are to be developed into a more specific form in accordance with what a particular case requires. For learning about such specific contexts and the obstacles and possibilities inherent in them, science policy analyses are, in turn, indispensable. I discuss two examples in the subsequent sections.

As shown throughout the discussion of Longino's account of objectivity, at its centre is the essential connection between the absence of subjective bias and the ability to withstand intersubjective criticism. Ultimately, openness to vigorous criticism is the only way a community can justify its claims to objective knowledge:

> For the only non-question-begging response to challenge must be: "We are open to criticism, we do change in response to it, and while we may not have included all possible perspectives … we've included as many as we have encountered (or more than others have)". (Longino 2002a, 174)

Longino remarks that objectivity of science in this sense is not in principle different from objectivity in other spheres, such as philosophy or literary and art criticism (Longino 1990b, 75). Thus, although her discussion focuses on scientific objectivity, Longino's approach to objectivity is supposed to be applicable to a wider range of areas. Elsewhere, Longino makes an even more general claim, characterising her account of objectivity with its criteria for transformative criticism as "an explication" of what objectivity means (Longino 2002a, 173–174).

While I will subsequently argue that Longino's conception of objectivity captures certain widespread ideas about it, it is indeed "an" explication rather than the full explication of the meaning of objectivity. For example, Douglas (2004; 2009) proposes to distinguish eight different senses of objectivity (although one of them is shown to be problematic) that describe different aspects of individual's thought processes, human–world, and human–human interactions. While some of these senses, in particular interactive objectivity, overlap with what Longino's account describes, Douglas argues that these senses are interconnected but ultimately irreducible. And Longino herself recognises that besides objectivity as non-subjectivity, there is also the traditional notion of objectivity as truthfulness to the fact (Longino 1990b, 62–64). Similarly, the notion of objectivity that Longino describes may not be timeless. For example, Lorraine Daston (1992) describes how the understanding of objectivity as escape from a particular perspective and elimination of idiosyncrasies only emerged in the late 18th century in moral philosophy and aesthetics. It gradually came to dominate natural sciences in the middle of the 19th century, as important changes in the social organisation of science were happening.[100]

Longino's account of objectivity thus only attracts attention to one, albeit important (according to Douglas) or even currently prevailing (according to Daston) understanding of objectivity. I suggest that despite incompleteness, the focus on just this aspect of objectivity may be helpful for establishing a contact between proposals based on Longino's account and some developments in science policy. If it is possible to show that Longino's associations between objectivity, absence of subjective bias and inclusive critical discussion are also recognised in the political sphere, developments in the political sphere based on these ideas may have important implications for philosophical ideas about improving objectivity. In particular, if the ideas about the epistemic importance of inclusive discussion constitute one of the reasons to recommend a wider public involvement in science and science policy, developments attempting to increase public participation may be discussed from the point of view of their epistemic consequences even if it is recognised that these developments are also associated with different, non-epistemic motivations. Accordingly, analysing these developments may be highly relevant for understanding how an attempt of epistemic improvement would fare in practice. Such an analysis helps to address the questions whether there may be political and public willingness to initiate such a change, whether it can be epistemically and politically successful and whether such a change can be stable long-term.

In their review of Longino's work, Solomon and Alan Richardson (2005, 217–218) point out the general moral and political appeal of the open society that Longino's norms of objectivity can be taken to describe. One could also mention the previously discussed connection of Longino's ideas with Mill's political liberalism. In this section, I draw on the already cited work of Mark Brown (2009) on democratic representation in order to provide a more focused characterisation of these commonalities. My aim is to show how certain common ideas about objectivity underlie a discussion of issues stretching from epistemic quality to questions of democratic legitimacy. So, epistemic considerations may be among motivations for certain science policy developments and their analysis may therefore be relevant for philosophy of science.

Discussing democratic representation, Mark Brown (2009, ch. 9) distinguishes five crucial elements of representation and discusses practices and institutions that enable realisation of these different senses of representation. The elements in question are authorisation and accountability of the representatives, public participation and deliberation, and resemblance between representatives and those being represented. It is the fifth sense—representation as resemblance—that introduces the themes related to the idea of objectivity as achieved through critical discussion from multiple points of view.

Mark Brown proposes to think about resemblance in terms of "social perspectives", using Iris Young's term. Making representatives resemble their constituents better can be understood in terms of making social perspectives present among the public also present among those representing the public—for example, among experts and policy-makers working out science policy decisions. A social perspective grows out of certain shared experience and provides a basis for shared concerns and questions, knowledge and worldviews, although it does not necessarily result in one particular shared interest or opinion. This experience may be common for some social group—for example, women or a racial minority group—but there may be different perspectives within a group and no perspective

[100] The argument about the historically changing character of objectivity is developed in rich detail in Daston and Peter Galison's (2010) *Objectivity*.

common to all its members. Such a perspective is essentially open—specifying what it consists of, its relevance to the issue at hand and attributing it to a particular group or particular individuals is always open to challenge. Neither is such a perspective fixed—it may change in the process of deliberation and this very changeability is important for the possibility of productive deliberation.[101] Nevertheless, such perspectives provide something deliberation can start from.

Mark Brown argues that there are a number of benefits associated with the increase in resemblance and the involvement of a wider spectrum of social perspectives in deliberation. First, it may improve what Brown calls "deliberative validity" (Mark Brown 2009, 230). Understanding of a problem may be expected to improve when it is approached from different perspectives. As Mark Brown writes in the passage strongly reminiscent of Longino's approach to objectivity, "[t]he more perspectives involved, the more likely that errors and biases will be identified and corrected" (Mark Brown 2009, 230–231). Elsewhere he adds, writing this time in terms of "epistemic quality", that persons who have been professionally involved with particular issues "tend to develop blind spots that may be remedied by including laypeople with relevant knowledge and experience" (Mark Brown 2009, 235). This echoes Longino's argument about the invisibility of shared assumptions. Second, the inclusion of a greater number of perspectives may improve public credibility and acceptability of the decisions made, as it makes them more responsive to the concerns of all groups potentially affected by them. Third, it may help to diminish the sense of alienation with respect to politics, including science policy. To the degree members of a social group identify with a perspective, its representation in various institutions helps members of this group to feel symbolically represented, reducing the alienation they may feel.

Mark Brown offers a theoretical analysis of representation in science policy but there are also analyses of actual institutions and policies that show the existence of a similar understanding of objectivity in science policy. In her comparative analysis of politics of biotechnology in several countries, Jasanoff (2005) shows that there exist several understandings of the way to ensure objectivity, or to enable knowledge claims and decisions that are "untainted by bias and independent of the claimant's subjective preferences" (Jasanoff 2005, 264). In particular, Jasanoff distinguishes the approach that relies on the application of quantitative analysis for demonstrating objectivity; the approach that sees the individual's qualifications—the "capacity to discern the truth" (Jasanoff 2005, 266) as an essential element for making objective decisions; and, finally, the approach that stresses the crucial role of inclusiveness and interaction of different perspectives. In the latter, decisions made are expected to be objective "not only by virtue of the participants' individual qualifications, but even more so by the incorporation of all relevant viewpoints into the output that the collective produces" (Jasanoff, 2005, 267). Jasanoff's argues that different political cultures tend to have relatively persistent preferences for a way to ensure objectivity. In particular, the collective approach to objectivity where "[t]he appearance of a view from nowhere is achieved by resolutely embracing ... the views from everywhere (or everywhere that matters for the issue at hand)" (Jasanoff 2005, 267) is prominent in Germany. Still, first, I believe it important to have a confirmation that there exist successfully functioning institutions and practices that embody this view of objectivity. Second, Jasanoff also stresses that these preferences should not be seen as too rigid or changeless. So, it may be possible to create new institutions on this model of objectivity (although I later discuss Jasanoff's reservations about the possibilities of transferring institutions and practices).

[101] This approach to social perspectives as open and „unfinished" and the impossibility to specify them once and for all can be taken as support for my argument about the necessity to maintain connection with community in order to be able to present up-to-date knowledge of a perspective in the critical discussion.

I thus intend to build an approach to establishing a connection between philosophy of science and political science on certain ideas that both share. Also in this case, there may be several possibilities for approaching such shared ideas. For example, Loren King, Brandon Morgan-Olsen and James Wong (2016) focus on the notion of deliberation. The possibility of democratising science, as Longino and Kitcher propose, depends on understanding biases and social and institutional factors (the authors call them mediating structures) that may influence deliberation. King et al. mention a variety of pernicious mediating structures such as the tendency to polarisation and the disadvantaging of certain groups due to biases. They are particularly interested in disagreements, especially the ways a potentially resolvable disagreement may be framed as involving irresolvable antagonism of values and worldviews. The possibility of productive deliberation depends on the possibility to counteract such misidentification of emerging disagreement.

Another example is Van Bouwel's (2015) paper, centred on the notion of scientific pluralism. In the paper, Van Bouwel argues that in order to understand different versions of philosophical pluralism it is helpful to use different models of democracy in democratic theory. Van Bouwel describes Chantal Mouffe's agonistic pluralism that is contrasted with consensual pluralism on the one hand and antagonistic pluralism on the other. Van Bouwel then shows how each of the three versions of pluralism in philosophy of science that he discusses resembles one of these models when it comes to the attitudes towards dissent, diversity and non-mainstream parties in the debate. Democratic theory can thus be used for the clarification of work done in philosophy of science, including the possibility to spell out philosophical proposals in greater detail. For example, Van Bouwel suggests that different models of deliberation and their approaches to deciding who participates and on what conditions may help Longino's norms of objectivity to gain in specificity. Another possibility is that studies of actual deliberative processes may help to improve interactions within scientific community.

I am sympathetic to both approaches (although I am going to mention some reservations at the end of the next section). Indeed, discussion of factors that may prevent the realisation of Longino's criteria of objectivity can easily be connected with the discussion of "mediating structures". My own approach in the next two sections, however, is somewhat different. As I have suggested, Longino's account and some arguments in political theory share certain ideas about objectivity. The aim of the next two sections is to discuss particular cases in science policy that can be seen as attempts to reorganise institutions and practices on the basis of these ideas about epistemic and political benefits of inclusive discussion. As described at the beginning of the section, attempts to make science policy more inclusive and democratic have been a noticeable trend in many countries. So, in line with the local approach that I have been advocating, I focus on analyses of particular developments in science policy and the opportunities and obstacles that a local political context may offer for changes in the organisation of science policy and scientific community. I discuss more theoretical levels of political science only to the degree they are relevant for working out tools for analysing local differences. My approach is still similar to Van Bouwel's and King et al.'s, insofar as I believe that empirical information on factors behind success or failure of actual deliberative processes is highly relevant for a philosophical proposal that recommends inclusive discussion.

One potential objection to such a reliance on actual cases in science policy is the possibility of fundamental differences between the spheres. Discussing Mark Brown's arguments I have shown that there is the recognition in political theory that increasing the number of perspectives involved in deliberation and decision-making in science policy may have epistemically beneficial consequences. These are the consequences that understandably are at the centre of attention for the philosopher of science. This recognition is what allows me to approach political developments that attempt this increase as relevant for Longino's ideas. At the same time, political theorists, and those who initiate political change, may be more interested in the possibility that this increase is associated with other consequences seen as beneficial for democratic society. As Mark Brown writes,

> Efforts to increase the diversity of social perspectives in public deliberation aim in part to remedy long histories of systemic discrimination against socially disadvantaged groups. They also seek to provide symbolic representation of these groups, in part to encourage political engagement by group members. These justifications for the representation of diverse social perspectives do not apply to scientific disciplines. (Mark Brown 2009, 235)

I acknowledge the importance of this concern. I have argued that changes in science policy may lead to an increased number of perspectives and improved critical discussion that the philosopher of science would recommend. However, in science policy these changes are likely to be initiated for non-epistemic considerations. These considerations may, in turn, push in directions different from those philosophers would prioritise. As a result, it may be possible, for example, that a perspective whose inclusion is deemed necessary in order to address the most pressing cases of alienation would not be most epistemically fruitful.

Nevertheless, I maintain that the developments Mark Brown describes may still be relevant for Longino's ideas. Previously, I argued that efforts to fight discrimination and marginalisation of certain social groups are important for the realisation of inclusive and productive critical discussion that Longino describes. In order to improve this kind of discussion, there should be inclusive opportunities for participation in knowledge production, unhampered by lack of resources or epistemic injustice. Accordingly, the efforts that Mark Brown mentions may have epistemically relevant consequences for scientific communities too. More generally, my argument is based on the suggestion that broadly the same recommendations for greater inclusiveness can be supported by a mix (in different proportions) of epistemic and political justifications. Accordingly, primarily politically motivated changes in science policy may still be close enough to what an epistemically motivated proposal, such as that inspired by Longino's criteria, would recommend. In this case, one may approach certain developments in science policy *as if* one of their aims were epistemic improvement that can be recommended on the basis of the philosophical account. I thus suggest that these differences may not be threatening for a productive contact between philosophy of science and science policy analyses. The following two sections can be seen as an attempt to substantiate this suggestion. I return to the issue of possible divergence in the concluding section, where I argue that these differences in motivation may even be an important resource for philosophy of science.

Longino's ideas and especially her conditions of objectivity are thus to serve as a lens through which to read analyses of specific political developments. Read in this way, these political developments can be used as a kind of test case for such a philosophical proposal: what happens when science and science policy are changed so as to involve a wider variety of perspectives? Detailed, context-sensitive science policy analyses can be helpful for understanding how an attempt to increase inclusiveness and encourage criticism fares in a particular social and political context. What does it take for such a change to work? What aspects of the local political and cultural contexts are relevant for its success or failure? How do different motivations behind it interact? This, in turn, helps to address a crucial question a proposal for the social organisation of science faces—could this proposal be successful in practice and does it constitute a good basis for approaching the organisation of science? Solomon and Richardson (2005) (as well as Solomon on her own—Solomon 2001, 143–145), for example, argue that Longino has not succeeded in demonstrating that the norms she proposes lead to better science. Discussing case studies in science policy does not constitute a direct response to Solomon and Richardson—they expect case studies of scientific research, especially canonical historical cases such as the Scientific Revolution. Nevertheless, my argument has relevance for the more general concern about practicability and successfulness of Longino's norms, of which Solomon and Richardson's criticism is an example.

Discussing how general ideas of objectivity play out in the context of specific attempts to democratise science policy helps to address another concern about the usefulness of Longino's approach. As discussed earlier, Biddle (2007) criticises Longino for the inability to respond adequately to the problem of commercialisation. He also presents several criticisms concerning Longino's approach more generally. According to Biddle (2007, 23–24 and 32–35), Longino's account is, first, too general, prescribing the same ideal to all scientific communities. Different areas of research, however, face different challenges and are likely to require different forms of organisation. Second, Longino's account is too abstract, building on the intuitions about knowledge or justification instead of analysing the actual organisation of scientific communities. Third, Biddle is concerned with the practical attainability of Longino's criteria. His concerns are in this case somewhat different from Solomon and Richardson's. Biddle is interested in institutions that can help to realise these criteria:

> if Longino cannot specify organisational arrangements that result in the fulfilment of her four conditions—if she cannot tell us, in a very specific way, how we are to achieve communities that meet these conditions—then we have little reason to believe that her ideal is achievable, even in principle. (Biddle 2007, 33)

In response, I suggest thinking about Longino's criteria as spelling the minimal necessary conditions for maintaining objectivity; they are necessary but not sufficient.[102] In practice, these conditions are always realised in the form of specific institutions, practices and rules. One could say that Longino's criteria in their general form are underdetermined; their determination occurs in those local contexts. Such an approach recognises the possibility of important differences between communities. This, in turn, is the point that Longino makes with her local epistemology and the stress on locally binding norms. There is also other evidence that Longino acknowledges the relevance of the characteristic features of a field. In her early writings on commercialisation and militarisation of research, Longino (1984; 1986) argues that given the differences between research projects in terms of the subject matter, aims and procedures, and political relevance, addressing the problems raised by them is likely to require different institutional mechanisms.

Responding to the criticism concerning institutions necessary for realising these criteria requires a related, but somewhat different answer. As stressed when introducing Longino's account, objectivity for Longino is a matter of degree. The criteria of objectivity are meant to capture some crucial intuitions about knowledge production, so that all knowledge-producing communities may be expected to satisfy them to some degree. Accordingly, the question is not how *to achieve* such communities, as Biddle suggests, but how to analyse the degree of compliance of actual communities and to improve it. Again, this interpretation of Longino's position encourages the use of her criteria as the starting point for analysing specific cases.

Longino's account is thus better understood not as a complete characterisation of all scientific communities but as the basis for understanding specific communities. Building on this basis it is possible to develop the kind of philosophy that Biddle advocates—philosophical accounts producing recommendations that are appropriate for actual communities. I suggest that when approaching Longino's criteria in this way, their intuition-based character turns out to be an advantage. As I have argued, these general ideas of objectivity allow establishing a connection with the developments in science policy based on

[102] In a different context, Jukola (2016) defends Longino's account in a similar way.

similar intuitions. This, in turn, opens a variety of developments in science policy for the kind of analysis that Biddle recommends.

The aim of the following two sections is to apply the approach just envisaged to some specific cases in science policy. I discuss examples derived from Jasanoff's comparative analyses of biotechnology policy and Wylie's study of the development of professional ethics in American archaeology. My aim is to show how developments initiated in science policy and the organisation of science for a variety of heterogeneous reasons can be usefully seen in the framework for understanding objectivity that Longino offers. Their analyses, in turn, help to identify both constraints and possibilities that such developments offer for a philosophical proposal. Doing this study of specific local contexts realises my vision of a possible role for the philosopher of science, which I spell out in the concluding section.

6.3 "Knowing things in common": Jasanoff on civic epistemologies

6.3.1 Introduction

In the previous section I outlined an approach to bringing philosophy of science and policy analyses in contact. I proposed to start from certain common ideas about objectivity and to discuss specific cases of democratisation of science policy that can be seen as realisation of these ideas. In this section, I discuss one such case—the German attempt to make inclusive public hearings a part of decision-making in biotechnology policy—that Jasanoff (2005) describes as a part of her comparative analysis of biotechnology politics in several democratic countries. I believe such specific case studies to be a source of important information about the local contexts where the general ideas about objectivity have to take specific form. I also believe that the general ideas such as Longino's can in turn be helpful for understanding these specific cases. So, in this section I provide an example how thanks to the approach I envisage both philosophy of science and analyses of science policy can benefit from cooperation.

In the first part of my argument I introduce Jasanoff's comparative analysis of politics of biotechnology. In order to analyse and compare the varieties of knowledge production in different countries, Jasanoff (2005, 9) introduces the notion of "civic epistemology" and develops a set of categories for describing civic epistemologies. With my argument, I suggest that public knowledge-making practices as Jasanoff analyses them can be seen as a kind of critical dialogue that Longino's criteria are meant to describe. Accordingly, to the degree this dialogue satisfies Longino's criteria, it can be called conducive to objectivity. Combining Jasanoff's and Longino's approaches thus allows one to show that *politically acceptable as objective* and *objective in the normative sense* can be brought closer together. As a result, experiments in the sphere of civic epistemologies can be used in order to understand opportunities and constraints an attempt to change some aspects of science policy towards greater objectivity may face in a specific local context.

In the second part, I substantiate this proposal by analysing one of the controversies in biotechnology policy that Jasanoff discusses—what I call the petunia controversy that developed in the wake of the acceptance of the German Genetic Engineering Law of 1990. I outline briefly the context and the development of the controversy and I suggest that Longino's criteria of objectivity, understood as the minimal conditions for successful transformative dialogue, can be helpful for understanding its development. In particular, they help to identify important obstacles that would have had to be overcome in order for the case under discussion to succeed as knowledge-producing dialogue. The inability to overcome these obstacles may result, as it did in the German case, in a failure that is simultaneously epistemic and a failure of democratic policy.

In the third part, I discuss implications of this case for a number of philosophical arguments, including my own. I suggest that despite the ultimate failure, the German case shows how specific laws and practices may create possibilities for greater objectivity and bring ongoing threats to it, such as commercialisation, under some degree of control. I argue that building on Jasanoff's empirical analysis enables a more adequate picture of this controversy than the approach based on Kitcher's well-ordered science could. The adoption of Jasanoff's framework, in turn, supports a strongly local approach to cooperation with political science, as opposed to more general approaches, such as Kitcher (2011c). Finally, I defend the use of Longino's criteria in my account, contra the rejection of these norms by Leuschner (2012). This section thus leaves the norms of objectivity to stand but opens the question how these norms could be realised in practice, overcoming the problems my discussion of the petunia controversy brings to the fore. In order to address this question, the second case study of this chapter is offered in the next section.

6.3.2 Knowledge society and "knowing things in common"

In her *Designs on Nature*, Jasanoff (2005) analyses the politics of biotechnology in three countries—Germany, the United Kingdom, and the United States—and on the level of the European Union. Her analysis serves the wider aim of understanding the changing relations between science and democracy during the transition to "knowledge society". In knowledge society, knowledge, and individuals possessing it, become the most important element of wealth. Jasanoff is interested in the consequences of this transition for democracy in the world that is increasingly shaped by science and technology (Jasanoff 2005, 4–5). As Jasanoff stresses at the beginning of her analysis, it is impossible to understand modern democracy without exploring the politics of science and technology—the ways knowledge is created, disputed and put to use in society.

The impossibility of understanding contemporary democracy without understanding the sphere of knowledge production means that knowledge production is deeply intertwined with wider issues of democracy. As the starting point for her discussion, Jasanoff specifies a number of general themes that constitute the wider framework for the development of biotechnology policy in each of the countries analysed. These themes run the gamut from the specific task of finding ways to manage new technologies and cope with the changes they bring to the task of nation-building, or finding a way to maintain and update the national identity in the context of the late 20th and early 21st century (Jasanoff 2005, 7–8) and to maintain what Jasanoff elsewhere calls "national narratives" (Jasanoff 2005, 201). They also include the most general task of maintaining ontological and ethical borders around what is seen as the natural categories of objects and persons (Jasanoff 2005, 26–27).

Yet despite the common issues, numerous case studies of the national reactions to technologies from GMO foods to reproductive technologies show that political responses to biotechnology are different in these three countries. One of the main tasks for Jasanoff's analysis is to explain the differences in reaction to the same aspects of biotechnology among the highly developed democratic countries under discussion (Jasanoff 2005, 8–10 and 29–31). Accordingly, throughout the book Jasanoff discusses the ways these common themes are shaped by each nation's particular historical and cultural context and political culture, which is defined as "systematic means by which a political community makes binding collective choices" (Jasanoff 2005, 21). These established ways of political life have a profound influence on the way particular issues in science policy are framed and addressed. These initial framings, in turn, possess what Jasanoff calls "stickiness" (Jasanoff 2005, 274). An attempt of change may face both the inertia of the existing political culture and the lasting influence of the initial solutions in the sphere of science and biotechnology policy.

Jasanoff stresses that political cultures provide both constraints and opportunities. In particular, Jasanoff turns to the part of the political culture she calls "civic epistemology" in order to show how it enables political communities to produce, disseminate and put to use knowledge. As Jasanoff defines them, civic epistemologies are culturally specific ways of knowing that form a part of the local political culture. Civic epistemologies provide the shared understanding what reliable knowledge is and how knowledge claims should be presented and defended (Jasanoff 2005, ch. 10). Jasanoff's discussion thus allows one to understand epistemic practices that already exist in the public sphere.

An important aspect of these practices that Jasanoff's approach helps to uncover is the role of the public. Jasanoff begins her account of civic epistemologies with the observation that in analyses of the increasingly close relations between science and the state the role of the public in knowledge production often remains unnoticed. This absence is puzzling regardless of one's views concerning activity or passivity of the public in these practices. In either case, practices of establishing and disseminating knowledge claims need an audience and this is what the public provides. As Jasanoff points out, "[c]itizens after all are the primary audience for whom the state enacts its scientific and technological demonstrations" and these demonstrations would be impossible without ensuring the attention of the public just "[a]s a play could not exist without spectators" (Jasanoff 2005, 248). Civic epistemologies are Jasanoff's attempt to repair this failure to acknowledge the role(s) of the public. In particular, they attract attention to the fact that the public has some role to play even before intentional efforts are made to involve it in science policy or even before its role, potential or actual, is recognised at all.

The acknowledgement of the "spectator" function of the public, however, may be compatible with seeing the public as essentially passive, lacking knowledge and in need of repairing this deficit. Such a vision of the public underlies the public understanding of science (PUS) approach that is used routinely when discussing science policy.

Jasanoff decisively distances her notion of civic epistemology from PUS (Jasanoff 2005, 249–255). She points out that PUS relies on the picture of universally valid science the understanding of which among members of the public can be measured—for instance, with the help of questionnaires. If members of the public are not able to give satisfactory answers, it is interpreted as an indicator of insufficient understanding, or ignorance, on the part of the public. Ignorance is in turn understood as the cause of public scepticism or hostility towards science, and attempts are made to fight this ignorance by spreading popularised scientific information and through that to ensure acceptance of science and technology.

Jasanoff's constructivist approach means that the conception of science that underlies the PUS approach is no longer taken as self-evidently true. Jasanoff approaches the authority of science and the presumed universal validity of its claims as something that requires an explanation in the first place. Another important change concerns the way the public is conceptualised. A shift of focus happens from the individual level to the social, communal level. In Jasanoff's own words, the notion of civic epistemology shifts attention from the individual who knows or does not know particular facts to the ways political communities "know things in common" (Jasanoff 2005, 250). According to Jasanoff, the individualistic approach that stresses the presumed scientific ignorance of the public is not able to explain the active role of the public in the politics of knowledge and different reactions of the public in different countries (Jasanoff 2005, 270–271).

The variety and vigour of these reactions also mean the rejection of the vision of essentially passive and "lacking" public as a whole. Instead, as Jasanoff writes, attempting to understand science policy in different countries has to come to terms with the

> collective that neither passively takes up nor fearfully rejects all scientific advances, but instead (as real publics are doing all over the world) shapes, crafts, reflects on, writes about, experiments and plays with, tests, and resists science and technology. (Jasanoff 2005, 255)

These activities of the public, in turn, play out against the background of the local civic epistemology.

The normative expectations embodied in a civic epistemology define how knowledge claims made in the public sphere are appraised. Claims have to conform to these norms in order to be accepted as objective and reliable knowledge. Civic epistemologies need not take form of explicit rules. They usually exist in the form of systematic institutionalised practices (Jasanoff 2005, 255). Jasanoff suggested distinguishing six aspects of such practices (Jasanoff 2005, 258–269):

1. Participatory styles of public knowledge-making: who is involved in the creation and appraisal of knowledge?
2. Public accountability: how is trustworthiness of experts and policy-makers before the public ensured?
3. Public demonstration practices: how is credibility of knowledge claims demonstrated to the public?
4. Registers of objectivity: what are the methods for demonstrating objectivity of public decisions?
5. Foundations of expertise: what is the basis of experts' credibility?
6. Visibility of expert bodies: how open are the workings of expert bodies for the public?

With the help of this framework, Jasanoff describes the recurring tendencies in civic epistemologies of the three countries that she analyses. (Simultaneously Jasanoff stresses that civic epistemologies should not be taken as uniform, changeless or immune to challenges.) From the point of view of public participation the first two dimensions—the styles of knowledge-making and the basis for trust—are the most relevant. In particular, Jasanoff's analysis helps to see that the border between those involved in knowledge creation and those who are not does not necessarily coincide with the border between certified experts and the public.

Jasanoff shows that in different countries different groups of the public may be involved and the legitimacy of their involvement in knowledge creation may be established on different grounds. For example, the civic epistemology of the United States is described as contentious. The style of knowledge making is pluralist and interest-based. It is the interested parties, including environmentalists, public activists, consumers and other members of the public, that are expected to produce knowledge. Litigation serves as the basis for checking these knowledge claims. In Britain the civic epistemology is, according to Jasanoff, communitarian. The style of knowledge making is embodied and service-based, relying on trustworthiness of governmental bodies. Credibility is earned through a record of service in public interest. Germany's civic epistemology is characterised as consensus-seeking; its knowledge-making style tends to be corporatist and institution-based. Trustworthiness is defined through institutional affiliation. Participants in the production of knowledge are invited because they represent some relevant institution, from a church to a patients' group.

How members of the public may be involved in the production of knowledge in a way that elicits trust thus varies depending on the country: from entering this sphere through litigation to defend one's interest to being chosen to represent a group whose participation is considered important. The way the public is involved is in turn intertwined with other aspects of the local civic epistemology with its ideas about expertise, trust and justification. As a result, an attempt to involve the public in policy-making inevitably happens against the background of the existing civic epistemology.

On the basis of these six aspects it is possible to analyse other dimensions of practices of public knowledge production in different countries. In accordance with the aims of

Jasanoff's analysis, they allow one to demonstrate differences between the countries and to show the interconnectedness of the politics of biotechnology in these countries with their peculiar political cultures. Jasanoff's approach to explicating systematically different aspects of knowledge-producing practices can thus provide an important tool for understanding the social context where the realisation of a philosophical proposal would have to take place. A nuanced understanding of the political and social context, from the general features of the political culture to what Jasanoff calls "a particular reservoir of national memories, experiences, and political activism" (Jasanoff 2005, 58), helps to realise that no meliorative attempt, be it epistemically or politically motivated, develops in vacuum. Such an attempt is always intertwined with wider political developments and develops against the background of existing political culture. Understanding this background is a crucial precondition for successful changes in the social organisation of science.

Thus, there are important benefits for the philosopher of science provided by this contact between a political analysis and a philosophical argument. I suggest that a philosophical argument, such as Longino's may also bring a helpful perspective on the political analysis. Longino's account of objectivity may be used to provide a kind of normative epistemic legitimisation for the civic epistemologies Jasanoff describes. One might suppose that civic epistemologies inevitably are epistemically suspicious. As civic epistemologies are intimately connected with local political cultures, one might suspect that they produce what only counts as knowledge for the given political community and cannot be called knowledge in the normative sense of the word. To counter such a suspicion, I suggest that civic epistemologies can be seen in the spirit of Longino's account of the social nature of objectivity. Longino shows how the emergence of objective knowledge is possible on the basis of social interactions. Civic epistemologies as described by Jasanoff are such interactive practices for public presentation, criticism and justification of knowledge claims. Thus, they can in principle support the kind of critical dialogue Longino describes. To the degree civic epistemologies actually enable transformative criticism in accordance with Longino's criteria, their results may count as objective knowledge in the sense that Longino discusses in her normative philosophical account. This prevents writing them off as pure politics by someone interested, as the philosopher of science would be, in the normative standing of practices under consideration.

So, one can apply Longino's analysis to civic epistemologies Jasanoff describes in order to argue that they can in principle produce knowledge in the normative philosophical sense of the word. It is also possible to make a normative judgement how well a specific practice manages to produce such knowledge. At the same time, Jasanoff's analysis allows one to demonstrate how these social processes of knowledge production are realised in the context of a particular political culture so that their results can be recognised in that political community as valid knowledge. Juxtaposing the two accounts thus allows for a new perspective on each of them, helping to understand attempts to improve objectivity in practice. The next subsection shows how the application of the two approaches is helpful for understanding a specific case in science policy.

6.3.3 Petunias and public participation

Jasanoff's and Longino's arguments help one to begin to recognise what it may take for an attempt to change existing knowledge-producing practices to succeed. Jasanoff attracts attention to the persistence of political cultures and civic epistemologies as their part, and stickiness of framings. Longino's criteria of transformative criticism help to see what conditions need to be fulfilled for productive critical dialogue in community. It is thus instructive to analyse an attempt to establish new practices for the creation of knowledge in light of both. I do so using as an example one of the biotechnological controversies Jasanoff (2005, 103–106, 114–117) describes.

The German Genetic Engineering Law of 1990 contained the requirement to involve the public in the discussion of questions related to genetic technology. According to the law, public hearing was a necessary precondition for making decisions about the establishment of facilities for genetic engineering and about the release of genetically modified organisms into the environment.

The first project to undergo such a public hearing, submitted by a research group from the Max-Planck-Institute for Plant Breeding Research in Cologne, involved planting some genetically modified petunias in an open field. As it turned out, the proponents of the project were not prepared for the way the public hearing developed. Instead of focusing on the scientific significance of the project and particular risks and benefits—the issues the researchers were ready to discuss—environmental activists seemed to use every opportunity to prolong the discussion and to prevent the approval of the project. Nevertheless, the petunia project was approved and went on.

The requirement of public hearings was withdrawn altogether just three years later. According to the critics, the experience of these hearings had demonstrated that the public primarily used them for trying to block projects under discussion. From the point of view of the critics, the public used the law improperly.

Jasanoff's analysis of the development of politics of biotechnology in Germany provides a perspective on the factors that first made this experiment with public participation possible and later led to its abandonment. An explanation from this perspective demonstrates the previously mentioned "stickiness", or lasting influence of earlier choices, and the persistence of important features of political cultures on the one hand, and the importance of opportunities offered by ongoing political developments on the other.

An important element of Jasanoff's account of national differences in biotechnology policy is the explication of different ways to frame biotechnology (Jasanoff 2005, ch. 2). Biotechnology can be understood as a source of products: it is the specific products that have to be evaluated for safety, while the process of their creation is considered inessential. Biotechnology can be understood as a process that due to its unusual character, a great degree of uncertainty involved and potentially dramatic consequences has to be closely controlled. On this view, evaluating individual products is insufficient. Finally, biotechnology can be understood as a programme that potentially allows the state to control (and remake) the nature and the citizenry in unprecedented ways. On this understanding, even stricter oversight from citizens is required. The products-oriented approach has mostly defined the politics of biotechnology in the United States. In Europe, the understanding of biotechnology as a process has predominated. In Germany it has been additionally coloured by concerns about the emergence of a disturbing programme for governance and control.

The presence of the understanding of biotechnology as a potentially dangerous political and technological programme offers an explanation why the demand for new forms of control over it—including public participation—could emerge in the German political arena. Jasanoff (2005, 59–61) describes how in the decade preceding the Genetic Engineering Law, the Green Party in Germany adopted the approach to biotechnology as a programme and stressed possible risks of such a programme for the environment, society and individuals. The Greens' position found its expression, in particular, in the statement of dissent the Greens presented in the parliamentary Commission of Enquiry on genetic engineering. While the comparatively moderate report of the Commission recommended the creation of new legislation to control biotechnology, the Greens demanded that alternatives be thoroughly explored before genetic engineering is allowed to be considered at all. Wide public discussion as a part of the exploration of desirability and permissibility of genetic engineering was another of their demands. While Greens' opposition to biotechnology did not lead to its complete abandonment, the regulations of the Genetic Engineering Law were quite stringent (more so than in the United States at that time): "[e]vidently, the Greens' aggressive political dissent had made a difference" (Jasanoff 2005, 61). The public hearing procedure can be seen as a part of this political difference.

However, as Jasanoff shows, these provisions were a compromise. Although it appeared to be "workable, if tense" (Jasanoff 2005, 106) initially, it turned out to be unstable. All sides involved in public hearings had reasons to be unsatisfied with the possibilities they offered and the results they could achieve. The environmental activists did not abide by the expectations that the proponents of genetic engineering had about the proper development of the discussion. For the activists, these expectations were not acceptable. They did not allow for the questions that were crucial for the activists—the necessity and justifiability of genetic engineering in principle (see Jasanoff (2005, 116) for the summary of one analysis of the controversy that did recognise these needs of the activists).[103] For the proponents of genetic engineering, public hearings were not a place to express principled opposition to biotechnological research. For them, the aim of the discussion was weighing the risks and benefits of particular projects. Accordingly, they perceived the environmental activists as sabotaging the debate rather than contributing to it in a rational way (see Jasanoff (2005, 104–105, 115) for some examples of the attitudes of the proponents).[104] In this situation, the perspective of those who assessed the procedure from the point of view of "the state's interest in scientific freedom, industrial productivity, or governmental efficiency" (Jasanoff 2005, 116) won. Under the influence of complaints from industry, researchers, and experts responsible for assessing genetic engineering projects within the government, the Genetic Engineering Law was amended. The requirement of public hearings was abandoned. Instead of this experimental form of public participation, Germany fell back on the well-established forms of expert decision-making.

Jasanoff's analysis of the German civic epistemology and the creation of the Genetic Engineering Law demonstrates what I have previously called possibilities and obstacles for a change inherent in the local political and cultural context. The recognition of the possibility and desirability of public hearings can be traced back to some important aspects of the German context, from the distrust of the State due to the particular historical experience (above all, the legacy of the Nazi Germany) to the emergence of the Green party as an influential political power. Similarly, the abandonment of this requirement can be discussed in the context of another set of general developments, such as the pressure to ensure the ability of the nation to compete internationally in the sphere of science and technology. Traditional forms of expertise for decision-making, sanctioned by the local political epistemology, similarly worked against the experiment. Different factors may thus pull in opposite

[103] In the analysis of an earlier German controversy over the genetic engineering facility for producing insulin (which mostly unfolded before the 1990 law), Rosemary Robins (2001) argues that employing official legal mechanisms for expressing the opposition to the facility drew local activists into technical debate concerning particular risks. This was the very kind of approach to genetic technology they wanted to challenge in order to discuss more general social and ethical concerns about the project. If taking part in public hearings "by the rules" of the 1990 law similarly worked against the activists' aims, it may be understandable why they often chose the tactic of resistance during public hearings.

[104] In the analysis of the legal rationale for public participation, Alfons Bora (1998, 124–127) describes the way hearings under the Genetic Engineering Law often unfolded, with activists trying to swamp them with lists of objections and complaints about the procedure. In such a situation, it may be understandable why someone who saw the aim of public hearings as a substantial discussion over particular risks of particular projects did not perceive the opposing side as rational partners in the dialogue.

directions, and as a result a solution once achieved may break down.[105] The factors that enabled the emergence of the practice of public hearings in the first place were not sufficient to keep it or to force the rethinking of public participation in terms other than the utilitarian terms of its contribution to productivity and efficiency.

Jasanoff's analysis thus helps one to realise how precarious an attempt to increase objectivity of science policy via enhancing inclusiveness may be, how dependent its very possibility is on the political and cultural context, and how easily such a development may be reversed under the influence of competing factors. I suggest that in order to further develop the understanding of the controversy, it is helpful to use Longino's analysis of objectivity. The aim of doing so is to attract attention to some crucial points where problems could arise.

This German initiative to increase public participation in biotechnology policy can be seen as an attempt to create possibilities for inclusive critical dialogue in society. As public hearings were a requirement of the law, their introduction created an official venue for presenting criticism and reactions to it. The law also made it a duty for those initiating genetic engineering projects to be responsive to public perspectives. (Presumably, there was the symmetrical expectation that the public should be responsive to scientific information.) The conditions of public hearings thus conformed to two of Longino's criteria for effective dialogue. However, the petunia controversy demonstrated some fundamental failures of communication between the researchers and the public. I suggest that these failures can be explained with the help of remaining Longino's criteria. They concern some of the central issues for the social process of knowledge production—the questions of shared standards, the membership in community, and the authority of members.

The question of shared standards is intertwined with the question of membership. On the one hand, as Longino argues, the more relevant points of view are involved in the dialogue, the better it may be expected to be at sustaining objectivity. An attempt to involve the public in science and technology policy can be seen as an attempt to widen the dialogue. In addition to researchers, representatives of the industry, and policy-makers, members of the public are also seen as belonging to the relevant community and capable of providing relevant perspectives. On the other hand, an effective dialogue is not possible without some shared standards. In order to be recognised as relevant, criticisms have to conform to some norms that all participants acknowledge. In the passage I have previously cited, Longino (2002a, 130, fn) points out that such norms are usually available when criticisms are exchanged within a community (e.g., a particular scientific subcommunity). The existence of shared norms is one of the characteristics that define a community. However, if there are several communities involved in the discussion, there may be no common norms. An additional effort may be required to create the shared basis for communication.

The petunia debate demonstrated this lack of common ground in the dialogue of the researchers and the public. There was no agreement about the suitable framework for the discussion. As a result, one of the parties (activists) attempted to break the framework they felt was imposed on them; the other party regarded these attempts as sabotage of the dialogue. Given the radical differences of the initial positions and the lack of a shared basis for compromise, simply bringing the opposing sides together in the situation of public hearings was not enough for the emergence of mutually acceptable knowledge claims and policy decisions.

[105] Jasanoff discusses the German experiment together with the attempts made to control biotechnology and public responses in Britain and the United States. These attempts, although made on a very different basis in each of the countries, all ultimately unravelled, showing the difficulty of creating a stable solution.

Thus, there exists a considerable tension between the requirement of inclusiveness and the requirement of shared norms. Realising the former without addressing the problems connected with the latter threatens the success of dialogue. A similar tension characterises the relation between inclusiveness and the ways of establishing authority. Even if members of previously excluded groups are included in community, they may be seen as having less authority and their contributions to the dialogue as deserving less attention. This situation endangers the critical potential of the dialogue.

In order to address this problem, Longino (2002a, 133, fn) suggests distinguishing between cognitive and intellectual authority. While cognitive authority depends on the individual's specialised knowledge, intellectual authority reflects the individual's general abilities for analysis and rational discussion. Longino's criterion of tempered equality is meant to describe this general intellectual authority. Accordingly, community is not supposed to distribute authority on the basis of technical expertise alone. Longino points out, however, that technical expertise remains highly relevant and that finding the balance between the two kinds of authority is a complex issue.

From the point of view of Longino's distinction, the German attempt to involve the public in biotechnology policy can be seen as an attempt to raise the importance of intellectual authority in the public sphere. The cognitive authority—the specialised scientific and technical knowledge—of the public on this particular issue may be expected to be considerably smaller than that of experts. Nevertheless, the requirement of public hearings implies that the intellectual authority of members of the public is also relevant for decision-making. However, the petunia debate and the subsequent abandonment of public hearings demonstrated that the recognition of the public as a part of the relevant community and the acknowledgement of its intellectual authority were neither unambiguous nor permanent. The contribution of the public was seen as a desirable, but not a necessary, element that could be abandoned, if its effectiveness was considered lower and its cost higher than expected. Yet, according to Longino, inclusion of all relevant perspectives is a requisite for the creation of objective knowledge. The "residual traces of illegitimacy" (Jasanoff 2005, 106) that according to Jasanoff characterised the closure of the petunia debate thus signalled a failure to create publicly acceptable knowledge both in the political and the epistemological sense.

Jasanoff's analysis of civic epistemologies helps to understand the context that is relevant for the success of an attempt to make science policy more inclusive—to understand the importance of the wider political and social developments and the persistence of existing civic epistemologies. Longino's criteria, in turn, allow for a helpful perspective on the causes of the failure to initiate a knowledge-productive public discussion. These criteria provide a lens through which to identify the problems that can prevent this kind of dialogue from functioning, epistemically and politically, as planned. In particular, these criteria bring to the fore the issues of the establishment of shared standards (and the willingness of participants to do so), the roles of the public and the experts in the debate, and the assignment of different kinds of authority between them.

The identification of these problems raises an important question for attempts to open the production of knowledge to the public. As Longino herself recognises, establishing a shared basis of norms and aims may require an effort. Accordingly, one may worry whether one can expect participants of the dialogue enabled by a political decision to accept this burden in addition to other duties. The aim of the second case study is to discuss how this problem may be resolved in practice. Before turning to it, I discuss some implications of adopting Jasanoff's analysis for the argument I am making.

6.3.4 Philosophical implications

Previously, I described how improvements in the social organisation of science on the model offered by Longino's account may depend crucially on the features of wider social and political context. I also suggested that alongside developments that threaten the existence of the conditions for objectivity, such as commercialisation of research, there may be developments that create niches of opportunity for realising these conditions more fully. In particular, I outlined the possibility that there may exist opportunities for improving objectivity via wider public participation that are enabled by science policy initiatives, undertaken for mostly political and ethical reasons.

The German Genetic Engineering Law of 1990 can be seen as creating such a niche opportunity. Some possibilities for public involvement in a critical discussion were created, despite the state interest in the national scientific and technological competitiveness that pushed towards the rapid adoption of genetic engineering. This pressure eventually contributed to the abandonment of this participatory experiment. Nevertheless, for a while it was functioning, bringing a degree of public involvement into decisions on science and technology, including, potentially, privately funded research and development. The possibility of such laws may offer a response to the question that Longino poses for well-ordered science, but that also applies to her account—on what grounds could be privatised science regulated? The German case shows that privately funded science may sometimes be brought under public control, to the degree that it is subject to laws and regulations that apply to both public- and private funded research. The ideas about the proper control over science and science-based technologies can thus be realised in the form of such regulations.

The German case study can be taken as an illustration of the possibility that I have described as an important opening for philosophical proposals about the social organisation of science. In addition to substantiating my proposal, the case study and the adoption of the framework of Jasanoff's analysis have a number of implications for the argument I have been making. In this section, I discuss three groups of issues. The first of them concerns the support that I take the case study to provide for my criticism of Kitcher's well-ordered science. Second, I discuss the implications for the vision of the appropriate relationship between philosophy of science and political science. Specifically, I argue that Jasanoff's analysis supports the local approach I advocate and puts into doubt the vision Kitcher (2011c) offers. Finally, I defend the relevance of Longino's criteria for the kind of political solutions I have discussed. Contra Leuschner (2012), I argue that public participation and shared norms may be indispensable for pluralistic research communities.

The aim of this and the preceding chapters is to show that using Longino's account as the basis enables developing a less problematic approach to the social organisation of science than Kitcher's model of well-ordered science. In the following paragraphs I provide further support for the approach I have been developing by showing that the petunia controversy cannot be adequately interpreted in the framework of well-ordered science. I suggest that my interpretation and an interpretation on the basis of well-ordered science may agree on some points. Nevertheless, I believe well-ordered science to be ultimately inadequate for understanding the development Jasanoff describes. This, in turn, has important implications for the viability of the model of well-ordered science and Kitcher's approach to cooperation with political theory. I suggest that analyses such as Jasanoff's undermine Kitcher's optimism that political analysts may offer a simple instruction "How?" to the philosopher of science. I conclude that these analyses support my local approach to the organisation of science.

Thinking in terms of well-ordered science, one would probably agree that the German experiment with public hearings ended with a political and epistemic failure. However, given the position of well-ordered science on unregulated free speech, this experiment can easily be seen as an example of cacophony that followed from an attempt to start a free public discussion without tutoring the participants first. Were it to end with the prohibition of the planned research, it would have been a case of tyranny of the ignorant as well. This is where my disagreement with well-ordered science begins.

I suggest that Jasanoff's analysis allows for a more nuanced understanding of the situation that shows limitations of the assessment in terms of ignorance. As Jasanoff demonstrates, one of the ways to interpret the dynamics of public hearings is to see them as an attempt of critics of genetic engineering to change the framework that was imposed on them. Instead of discussing benefits and risks of particular projects (the products-centred framing), they attempted to shift the discussion to wider implications of the process and the programme that underlies them. As Jasanoff has shown, different framings for managing biotechnology have emerged in different countries. Their development has relied on considerable scientific expertise of the respective countries. Classifying a particular framing as an expression of the tyranny of the ignorant out of hand is thus problematic.

Ironically, Kitcher even cites (Kitcher 2011c, 191 and 238) Jasanoff's (2005) book when he mentions the high number of Europeans who believe that non-GMO organisms do not contain genes. At the same time, Kitcher seems to fail to realise that his position with respect to genetically modifies organisms—"[t]here is nothing special, or especially risky, about *genetic* modification of organisms" (Kitcher 2011c, 238, italics in the original)—exemplifies what Jasanoff describes as the American framing that focuses on specific products. Indeed, Kitcher stresses that the opposition to GMOs is very much a European phenomenon. There is no recognition that there are other framings possible. Taking the American products-centred approach for granted misses the variety of policies that can be proposed and supported by scientific expertise—and have indeed been proposed—in the context of different countries.

Thinking about the opposition to genetic engineering among many groups in German society in terms of ignorance that is to be remedied may also lead to inadequate organisational arrangements. In Kitcher's well-ordered science and its practical approximations the problem of ignorance is to be addressed by tutoring. During tutoring, representatives of the public are given all the relevant scientific information and are educated about the methods of its creation and certification, until all their doubts are put to rest. Kitcher's well-ordered science has been criticised (Wilholt 2014, 170–171) for the failure to take into account the framing issues—the ways a specific framing of a question may influence its public discussion. The German case may be taken as an example of the situation where it is the framing of an issue that it is at stake. The researchers were prepared to provide the public with the relevant information on risks and benefits of their projects. For their opponents, however, the question was about the very desirability and permissibility of genetic engineering. Talking about specific risks and benefits meant that the affirmative answer had already been presupposed, and this is what the activists fought against. If the controversy is seen in this light, it is not ultimately about the inability of the researchers to tutor or the inability of the public to learn. Rather, it is about the limitations of the model that assumes the possibility of objective and neutral tutoring about issues that are taken as defined naturally and unproblematically. As Kitcher's well-ordered science is based on this model, it can be expected to be inadequate for issues where it is the definition of the issue that is challenged. In this case, finding less restrictive ways of public participation that would allow addressing problems related to framing seems warranted.

Recognising the variety of framings for regulating biotechnology issues in different countries has another important consequence for the relationship between the process of deliberation and its results in well-ordered science. In the initial version of well-ordered science (Kitcher 2003), the focus was on the appropriate results; the procedure was whatever could approximate these right results. In the reworked version (Kitcher 2011c), it is the process of ethical conversation that takes centre stage. Kitcher stresses the open-ended nature of this conversation. No one is in the position to produce definitive judgements; the authoritative judgement can only be a product of conversation (see, e.g., Kitcher 2011c, 248). Previously, I argued that there is an inconsistency in Kitcher's approach. Kitcher simultaneously stresses the authority of the conversation and makes some confident predictions about the positions that well-ordered science would take with respect to a number of questions. In the end Kitcher seems to be committed to both the procedure and the definite results: deliberative conversation or its real-life approximation is expected to deliver the results specified.

When discussing this issue for the first time, I was concerned with its impact on the motivation to actually involve the public in deliberation. There may be other concerns. One may doubt whether the institutions Kitcher envisages would reach the conclusions that Kitcher postulates, as Dale Jamieson (2002) does, or whether either the results or the institutions are specified enough to be achievable, as Pinto (2015) does. I suggest that the analysis such as Jasanoff's attracts attention to another aspect of the problem of institutions and results in well-ordered science. Not only there may be doubts whether actual democratic deliberation would reach the positions close enough to those described by Kitcher; it may also happen that different groups of deliberators in different local contexts would reach different conclusions that would not converge on the position Kitcher specifies. As a result, well-ordered science faces a dilemma: either to prioritise the outcomes and forgo the democratic process, or to prioritise the process with the possibility that different deliberative exercises might reach different results neither of which might coincide with those specified by Kitcher. At the very least, recognising the differences Jasanoff's analysis uncovers should preclude one from taking the convergence of deliberative exercises on the same solution for granted.[106]

Whichever horn of the dilemma is chosen, Jasanoff's analysis demonstrates that different democracies do reach different outcomes and show persistent differences in the practices and institutions that lead to them. Jasanoff's analysis thus can be taken as support for the local approach to bringing together philosophy of science and political science. In the previous chapter I argued that solutions for inclusive knowledge-productive communities are likely to be local due to the local character of binding norms. However, once the necessity to discuss these local developments in their political context was introduced, the concern about losing the local character of this approach arose. I now use Jasanoff's analyses in order to support the suggestion I mentioned then—the suggestion that democratic politics are also local. Possibilities for improving objectivity opened by specific laws and institutions emerge in specific political contexts with their histories and political cultures. Similarly, attempts to work out shared norms for intercommunity communication are likely to be influenced by existing practices of public knowledge production, which in turn are a part of specific political cultures. These connections may offer some opportunities—for example, attempts to establish shared norms may draw on a rich pool of already existing common ideas about knowledge and authority. At the same time, they introduce a strong element of locality, supporting the local approach to the organisation of science.

[106] In addition to the analyses of 2005, Jasanoff (2011) discusses responses of different countries—Germany, the United Kingdom, and the United States—to climate change, which is an especially important example for Kitcher (2011c). Jasanoff shows how despite strong scientific agreement on climate change, there are important differences in the ways this knowledge is taken up and put to use in the political sphere. She traces these differences back to different civic epistemologies. Enabling concerted international action against climate change requires recognition of these differences and conscious efforts to make "cosmopolitan knowledge" possible. While Jasanoff outlines briefly some possibilities for doing that, it remains something to be attempted, not something that can be taken for granted.

This, in turn, has important implications for conceiving the relationship between philosophy of science and political science. As discussed earlier, in *Science in a Democratic Society*, Kitcher demonstrates some interest in experiments with public deliberation in the political sphere (Kitcher 2011c, 222–226). According to Kitcher, research in political science could provide tools to improve democratic deliberation while philosophical proposals such as Kitcher's well-ordered science show where to employ them. In Kitcher's words,

> Deliberative polling, as well as the citizen juries of the Jefferson Project, endeavours to find structures for improving democratic decision making. My attempt to understand the relations between scientific expertise and democratic values identifies places at which those structures might be put to use. If you like, Fishkin's question is "How?" and mine is "Where? (Kitcher 2011c, 225)

So, Kitcher seems to believe that deliberative technologies such as those realised in citizen juries and deliberative polling can be detached from their original context and apply wherever it is necessary. I suggest that Jasanoff's comparative analyses make Kitcher's assumption about the possibility of universal political know-how problematic. These analyses put into doubt the idea of the universal character of democracy and the context-independent and transferable character of forms of democratic deliberation. Taking these considerations into account, I propose that building a relationship between philosophy of science and political analysis should take this locality and context-specificity into account.[107]

Jasanoff's comparative analysis shows that democracy is not uniform. Countries with similarly well-established democratic traditions may have remarkably different political cultures, including civic epistemologies, and different forms of public participation in science. As a result, they may (and do) end up with remarkably different science and technology policies. Attempts to transfer political solutions from one context to another without taking into account these differences may therefore be expected to fail.

One problem for the possibility of transfer is a potential mismatch between the principles of the local epistemology embedded in a specific institution or practice in the place of origin and the local epistemology of the place where the transfer is attempted. For example, one may doubt whether the mechanisms of public participation that have emerged in the environment where it is the private interest, demonstrated, if necessary, through litigation, that serves as the basis for entering the public sphere of knowledge production, would gain enough credibility to get off the ground in a different environment where it is customary to staff public bodies with persons selected on the basis of their record of public service.

A different problem arises for experimental bodies that are based on a novel approach to establishing legitimacy. For example, deliberative polling relies on representative random sampling for forming the deliberating body. As it is orthogonal to any of the traditional approaches, it may be a promising candidate for a transferable solution. (Tests of deliberative polling have been run in more than 22 locations across the world so far.[108]) I suggest that a problem for such a potentially universal solution is the integration of the exercise with the existing norms of a civic epistemology. As I have stressed, one important lesson from Jasanoff's analyses is that any experimentation of this kind happens in the context where there already exist norms for appropriate knowledge claims and institutions for knowledge production and appraisal. Without some kind of integration the deliberative exercise may remain an isolated experiment with no policy-making impact.

---

[107] Similar considerations apply to other attempts to bring philosophy of science and political science together that focus on higher-level models or general factors that may interfere with deliberation. There may be important differences in the local contexts where these models are realised or where these factors are to be overcome and the results to be integrated with other aspects of the local political culture.

[108] Centre for Deliberative Democracy, http://cdd.stanford.edu/what-is-deliberative-polling/, accessed 18.11.2015.

Analyses of both specific developments and general trends in public participation show that this is what happens regularly in the case of public participation. For example, Gaillard shows that the French debate on nanotechnology might have been perceived as "hollow" (Gaillard 2013, 253) because research proposals were already being submitted and pursued during the public participation procedure. The French case is not exceptional in this respect. In a review of the "Science, Technology and Governance in Europe" (2001–2005) project that analysed public participation in science governance in Europe, Rob Hagendijk and Alan Irwin (2006, 174) conclude that public deliberation is often seen as "one-off hurdle to be cleared at a time judged appropriate by government, and often quite late in the process of decision-making". Often, public deliberation fails to have a political impact. Experiments with public participation are typically organised on an ad hoc basis and they do not become a lasting feature of the political infrastructure. They are also mostly "kept at arm's length from formal decision-making" (Hagendijk and Irwin 2006, 176): their results are not necessarily taken seriously.

I do not intend to claim that experiments with public participation may never be successful or that novel approaches to involving the public in discussion are never relevant. I have been arguing that for a philosophical proposal about the social organisation of science, developments in wider social and political context of science may be highly relevant, as they create possibilities for approximating philosophical ideas. Such an argument implies some degree of optimism about the possibility of successful experiments. Indeed, I discuss what I consider a success story in the next section. The point I want to stress, however, is that developing a form of public discussion that improves deliberative quality (and Fishkin (2011) argues that the improvements are consistent and considerable) and putting it into the service of a philosophical proposal is likely to be insufficient on its own.[109] The experience of public participation, as summarised by Hagendijk and Irwin, shows that making a deliberative public experiment a systematic and consequential element of science policy poses a challenge. Its success cannot be taken for granted.

Jasanoff is also doubtful about the possibility of easily transferable solutions. She associates the belief in them with the early period of comparative studies. The development of the field makes this belief increasingly problematic:

> With growing awareness of the culturally embedded character of both knowledge and policy, there are reasons to be sceptical of unproblematic learning from others' experiences. The insights gained from comparative analysis suggest, indeed, that neglecting cultural specificities in policymaking may be an invitation to failure within any political community's own terms of reference. (Jasanoff 2005, 15)

Moreover, even given the improvement of knowledge about "cultural specificities" since this early period, a transfer of specific institutions is likely to be costly due to the necessity of remaking other relevant aspects of the local epistemology:

> The distinctive features of political culture are not so easily transferable. They are embedded in a rich matrix of experience and practice, and efforts to graft them onto other settings may fail or entail a higher price than enthusiasts for such transfer would find acceptable. (Jasanoff 2005, 291)

Ultimately, it may be possible to learn about these specificities—and analyses such as Jasanoff's contribute to that—and it may even be possible to transfer specific institutions if

[109] In the context of my criticism of well-ordered science is also important to stress that deliberative polls do not have the aim of producing knowledge. As Fishkin writes, the poll participants are not asked "for an expert judgement. It is rather a question of collective political will or public judgement" (Fishkin 2011, 120). For knowledge, the polls rely on briefing materials that experts prepare. So, deliberative polls do not have the aim to solve the problems of experts' objectivity, or gaps in experts' knowledge, or framing issues. (They do try to mitigate them by involving a variety of experts and balancing the briefing materials.)

one is willing to accept this “higher price”. Yet even in this case well-ordered science would face a problem. As stressed repeatedly, Kitcher’s well-ordered science has a strong universalist element. The potential public is the entire humankind and the decisions made are supposed to be binding for the entire humankind. The successful transfer of some country’s solutions for science policy or the successful integration of some experimental form of deliberation, however, are likely to be as different as the different civic epistemologies at the place of origin and the place of application. As a result, it cannot be taken for granted that such a successful transfer results in the decisions that Kitcher expects from well-ordered science.

I conclude that there are reasons to think that analysing experiments with public participation is unlikely to result in universally applicable instructions “How”. Accordingly, learning from analyses of developments in science policy should not be expected to deliver a straightforward know-how for reorganising science policy so as to approximate the philosopher’s specifications. Rather, what one could hope for is an improved ability to understand the differences and to recognise the difficulties an attempt of change may face.

In this and the preceding chapter I have been arguing for a close contact between philosophy of science and developments in science policy. In the approach I propose, Longino’s account of objectivity serves as the basis for connecting philosophically motivated proposals and political developments. Longino’s criteria of objectivity also play an important role. In particular, I show how they can be useful for identifying obstacles for the realisation of inclusive public debate. The problem of shared norms and the related questions of authority have emerged as one of the central problems for the organisation of such a debate. In the concluding part of this section I discuss a proposal that attempts to avoid problems related to Longino’s criteria—Leuschner’s (2012) argument about the political solution for the problem of objectivity. Leuschner’s solution is limited to deliberative bodies that consist of experts only. In my response, I argue that the reasons Leuschner gives for requiring pluralism among experts similarly support the inclusion of the public. In the case of public participation, however, a purely political solution may be insufficient and the creation of shared norms may still be required. I conclude that Longino’s norms retain their importance and I devise a different solution for the problems Leuschner is concerned with.

Leuschner is sympathetic to the basic idea of Longino’s account—the connection between plurality of perspectives and the possibility to improve objectivity by exposing biases. For Leuschner, the main problem of Longino’s criteria is circularity. If there is a requirement to include only relevant perspectives, some objective standards are required in order to evaluate the relevance. So, objective community standards have to precede the inclusion of perspectives that are necessary for making the community objective. In Leuschner’s (2012, 193) words, “That way, objectivity (of the standards) is premised for a process (pluralism) that is to generate that very objectivity”. If some standards are taken to be objective in this way, contributions from those not sharing them are excluded, undermining pluralism of perspectives. Longino’s approach is thus ultimately detrimental for pluralism.

Leuschner argues that this problem cannot be solved analytically, on the level of Longino’s ideas. As an alternative, she proposes the solution in the form of political regulations for critical debate, widely understood. These regulations may encourage some perspectives (for example, with the help of hiring quotas for specific social groups) and limit others (for example, with the help of regulations for the use of specific technologies). The membership and the norms of pluralistic expert bodies are to be established by a political decision that reflects relevant epistemic and ethical considerations. In this way, successful critical discussion that would not run into the problem of circularity becomes possible. Leuschner discusses the Intergovernmental Panel on Climate Change (IPCC) as a successful example of such a politically established, experts-staffed, pluralistic body. She sees these politically established bodies as realising the deliberative ideal of Kitcher's well-ordered science; unlike Kitcher's, however, Leuschner's ideal deliberators only include experts.

There are some important commonalities between my approach and Leuschner's. We both suggest that possibilities for a productive inclusive (or pluralistic) discussion can be created by specific political measures. However, unlike Leuschner, I maintain that the successful realisation of these possibilities may still require the establishment of certain shared norms. I suggest that Leuschner can avoid discussing them because she focuses on experts-only deliberative bodies. As stressed earlier, the existence of certain shared standards and aims can be presumed to exist in the case of a specific research community. In the subsequent paragraphs I argue that the reasons Leuschner gives for pluralism in expert bodies may also support public involvement. In this case, there is the possibility of conflict due to the lack of shared norms in the newly created hybrid community. The necessity of shared norms cannot therefore be sidestepped.

Discussing deliberative expert bodies, such as the IPCC, Leuschner brings out both epistemic and political reasons to require plurality there. Inclusion of different perspectives helps to expose biases and utilise local knowledge; simultaneously, it helps to achieve agreements that all parties involved can recognise as fair. As my earlier summary of Mark Brown's arguments shows, both reasons may also be used to support the inclusion of lay participants in such deliberative bodies. Given that Leuschner's argument mostly discusses epistemic considerations, I also focus in my response on the epistemic reasons for the involvement of the public.

In chapter 4, I argued that the participation of lay public in knowledge production may be necessary for exposing experts' shared assumptions and for supplementing experts' knowledge with relevant local knowledge. There are analyses of this kind of relevant lay knowledge in the field of climate change science that Leuschner discusses. In her analysis of representations in the sphere of climate change, Marybeth Long Martello (2008) describes how Arctic indigenous peoples are gradually coming to play an important role in research and politics of climate change (Martello 2004 also discusses some of these issues).[110] These peoples are a distinctive group under threat from climate change. As such, they act as spokespersons for themselves and other groups in a similar position. In addition to that, Martello shows that they are increasingly seen as capable of important epistemic contributions:

> Arctic indigenous peoples are becoming recognised as holders of specialised knowledge, which is crucial for identifying and understanding local manifestations of global environmental change and attendant nature–society interactions. (Martello 2008, 353)

Martello describes one of the research projects that have received important contributions from these groups—the 2004 Arctic Climate Impact Assessment (ACIA), prepared by the Arctic Council and the International Arctic Science Committee.[111] Throughout the preparation of the document, the participation of indigenous peoples was encouraged and they took part in reviewing and writing some parts of the report. Their ability to contribute different kinds of knowledge (which can be called experience-based in Collins and Evans's terms or, attracting attention to another of its aspects, "environmentally rooted" knowledge—Martello 2008, 366) was thus recognised and utilised:

---

[110] In addition to the relevance for my response to Leuschner, Martello's analysis is important as it provides an example of successful participation of the local public(s) in research on global problems. It thus somewhat assuages the worry that the local approach I advocate cannot help to approach global problems.

[111] The report and the related materials are available at http://www.amap.no/arctic-climate-impact-assessment-acia; accessed 8.12.2015.

> ACIA portrays the knowledges and experiences of these peoples as helpful for understanding the changes that have taken place, how indigenous peoples have coped with those changes in the past, and how they and others might address them in the future. (Martello 2008, 360)

The knowledge of ongoing changes may include the experience of phenomena such as the weather predictability, snow and ice characteristics and seasonal weather patterns as well as the consequences of these changes for the interactions between indigenous peoples and their environment. In the discussion of connections between the environment and human activities and identities, the contribution of indigenous peoples may also go beyond what Martello (2004, 112) calls "witnessing", or providing facts that scientists deem significant. The traditional knowledge indigenous peoples offer may be approached more holistically, recognising its connections with values, practices and worldviews. These connections, in turn, may have important implications for what is an acceptable solution to the problems caused by climate change. (Unlike the specific projects discussed in Martello (2008), however, this kind of cooperation that challenges experts' assumptions is envisaged in Martello (2004) as a possibility.)

Even if the discussion is confined to "witnessing" of facts, Martello's analysis demonstrates the epistemic grounds for including representatives of these publics in institutions attempting to address climate change. The wider inclusion of laypersons, however, may be expected to bring problems related to the lack of shared basis for communication—the basis whose existence can be assumed in the case of communication within experts' groups. As the discussion of the petunia controversy shows, a purely political decision—the decision that specifies who and on which conditions takes part in deliberation—may not be sufficient. Bringing together groups with very different frameworks, aims and standards of authority may not be enough for initiating a productive dialogue. A more substantial basis of shared norms and aims may be necessary.

I thus suggest that Leuschner's solution fails for cases where there are grounds to think that the inclusion of lay perspectives is desirable. Leuschner's argument, however, attracts attention to an important problem for Longino's criteria: how can we make norms objective if we use them in the very process for enabling objective communities?

I suggest that while a perfect solution is not possible, it is possible to break the circularity by treating norms as themselves subject to criticism and open to change. Political decisions such as those Leuschner describes, may be one way to initiate this change. They may do so directly (for example, by appointing the members of an expert body) or indirectly (for example, through social change that allows resources for the development of a previously neglected perspective). However, as I have been arguing, sometimes the political solution alone may not sufficient. Another way a change in norms may happen is when the necessity to work out new norms for a specific situation that requires collaboration is recognised and serves as the starting point for action. This is the possibility I proposed when discussing how intercommunity criticism may become possible. Finally, the norms within a community may also change. Longino herself stresses that the norms can be criticised on the basis of other elements of community's epistemology. It is not possible to criticise every element at once but no element is shielded from criticism in principle. As Longino writes,

> standards are not a static set but may themselves be criticised and transformed, in reference to other standards, goals, or values held temporarily constant. Indeed, as in the case of observation and the assumptions underlying justificatory reasoning, the presupposition of reliance on such standards is that they have survived similar critical scrutiny. (Longino 2002a, 131)

Similarly, the ideas about relevant perspectives may be open to change. The failure of objectivity on the community's part is greater if alternative perspectives are readily available and community members are aware of them and can recognise their relevance for the discussion. The more evident this ignoring of available perspectives, the greater the failure of objectivity is and the more pressing the community members' duty to improve the situation. In order to do so, the community's members who recognise some marginalised perspective as relevant could attempt to influence the community's judgements by making this perspective more visible and presenting it in a way that helps other community members to see its relevance. Such decisions can in turn influence the community's future decisions and the norms that form the basis for decision-making. As a result, objectivity of the community's standards, while remaining a matter of degree, may be improved. There is no inescapable circle.

I conclude that there are epistemic reasons not to limit pluralistic bodies to experts only. The wider inclusion, however, may make the creation of a shared basis for communication necessary. In practice this task may be laborious and complex; yet, there is no irresolvable conceptual circularity in Longino's approach to norms.

6.3.5 Conclusion

The discussion of this section exemplified the approach I propose for bringing philosophy of science in contact with science policy. I showed how a political development may lead to institutions and practices that could be recommended on epistemic grounds, how political analyses may offer valuable information about opportunities and obstacles for these developments and how philosophical ideas may be helpful for analysing them.

I began by summarising Jasanoff's discussion of civic epistemologies and connections between science policy and democracy in contemporary societies. Jasanoff's analysis allows putting into focus existing practices for the creation and evaluation of knowledge, against the background of which an epistemic meliorative attempt would have to unfold. It demonstrates both limits that local civic epistemologies, specific issues and initials solutions to them impose on experiments in the sphere of public knowledge production and possibilities they may offer. I also suggested that Longino's analysis of objectivity may be helpful for identifying some crucial obstacles for the emergence of an epistemically successful, as well as politically successful, dialogue in such a local context.

After setting this framework, I discussed one specific experiment that attempted to increase public participation in science policy—the German Genetic Engineering Law of 1990. I suggested that the failure of this experiment demonstrates the importance of shared norms and ideas about authority, as well as shared frameworks for approaching a specific issue. The political decision about the institution of specific deliberative bodies or practices may be a step for creating possibilities for more inclusive knowledge-productive practices. However, simply bringing different parties in a politically mandated situation of communication may not be enough for productive dialogue.

In the final part of this section I discussed a number of implications of adopting Jasanoff's analysis. I suggested that it supports the local alternative I have been offering to Kitcher's approach. Attention to specifics of local epistemologies allows seeing phenomena that remain hidden in the model of well-ordered science, in particular the issues related to framing. It also supports the conclusion that there is unlikely to be straightforward political know-how that can be applied universally without taking into account local civic epistemologies. Finally, I defended the necessity of shared norms in this approach against Leuschner's proposal to dispense with Longino's requirement of norms as circular. I argued that there are epistemic reasons to include laypersons in inclusive knowledge-producing communities and as a result the necessity of finding some shared basis for communication cannot be escaped. I also described how the threat of circularity can be prevented if norms are seen as themselves open to criticism. I thus reaffirmed the plausibility and usefulness of Longino's ideas for approaching matters of science and politics. The question of the possibility of shared norms for communication between diverse groups, however, remains crucial for attempts to make changes in practice. In the next section I describe how the problem of shared norms can sometimes be successfully addressed as the disciplinary identity of a field is changing.

6.4 Ought scientists to be translators? Wylie on archaeological ethics

6.4.1 Introduction

The aim of the previous section was to demonstrate how specific laws may create possibilities for public involvement in science policy and to discuss obstacles that may arise for the realisation of these possibilities. This discussion brought to the fore the question of shared norms and ways to attribute authority. Jasanoff's analysis of civic epistemologies, in turn, gives reasons to think that any attempt to address these issues has to take into account existing shared understandings about the norms according to which reliable knowledge is to be produced. The unfolding of any such attempt is therefore likely to be context-dependent.

In addition to context-sensitivity, such an attempt may be expected to require for its success a great degree of good will and effort to enable productive communication on the part of different groups involved: the public, the decision-makers, and the experts. To the degree objectivity depends on such an effort, making it can be considered an inseparable part of striving to be objective. One may doubt, however, whether there is evidence of willingness to make this effort in science policy developments that I discuss. Controversies such as the petunia case reinforce the concern about the relation between requirements of Longino's approach and realities of science policy and communication. Another concern is whether the duty of enabling communication is compatible with realising other aspects of objectivity. So, Longino's approach to objectivity may be thought to be either inapplicable in practice or undesirable.

In this section, I have two complementary aims when I discuss Wylie's (1996; 1999; 2000 and 2005) analyses of the development of a new approach to professional ethics in American archaeology. The primary aim is to address the concerns outlined in the previous paragraphs. I use Wylie's analyses to show that changes in science policy and science practices may sometimes bring with them changes in communication norms, aims and standards of authority. As a result, productive communication between experts and the public concerning both the matters of research planning and the epistemic content of research becomes possible. Wylie's analyses thus demonstrate that the duty of communication may become a part of individual scientists' and the discipline's identity without necessarily threatening their other epistemic duties. The second aim is to provide another example of the approach I advocate. I use Wylie's analysis of the social and epistemic context where the new disciplinary identity for archaeology has emerged to substantiate further my suggestion that local political and cultural developments may result in changes that are similar enough to what can be recommended on epistemic grounds. In this sense, Wylie's analysis can be said to provide a "success story" to balance the story of the failed German public participation experiment.

In the following subsection I describe how Longino's ideas of objectivity, which may require a community's members to take part in communication with those outside the community, may also support the duty to participate in the creation of a shared basis for this communication. There may be doubts, however, whether this duty is sustainable in practice or epistemically desirable. In order to address these doubts and to defend Longino's account as the basis for approaching the social organisation of science, I present in the third subsection Wylie's discussion of the changes in archaeologists' ethics and self-understanding and their consequences for archaeologists' practice, including epistemic practices. I treat Wylie's analysis as another example of epistemically consequential change that has been brought about by a variety of predominantly non-epistemic factors. In the concluding subsection I discuss the implications of this example for my argument. I suggest that also in this case the analysis Wylie offers is more adequate than approaching the same events in the framework of well-ordered science would be. Together with the previous section, this discussion serves as the basis for discussing, in the final section of this chapter, some general implications of my approach and my vision of the role of the philosopher of science.

6.4.2 The problem: ought scientists to be translators?

I have suggested that one of the lessons of the petunia controversy is the importance of shared norms and authority evaluations. In the situation where there is no shared basis for communication and the very framing of the debate is a matter of disagreement, simply establishing an official forum for communication may not be enough. Making the changes for improving objectivity work in practice may thus require additional effort for establishing such a shared basis of communication. Going beyond specific cases, one may suggest that being objective demands being ready to do this kind of work. The aim of this subsection is to outline the implications that this approach to objectivity has for what may be expected from scientific communities and to describe the concerns that this interpretation raises.

As previously described, introducing her criteria for transformative criticism, Longino discusses a number of problems related to their realisation, both within community and in its wider social, cultural and political context. One of these problems is the tension between the requirement of inclusiveness and the necessity of shared standards to keep criticism relevant. For example, writing about the necessity of inclusiveness and the simultaneous necessity of standards to filter out "crackpot" perspectives, Longino (1991, 674) points out that "[t]here is always a danger that the politically marginal will be conflated with the crackpot." The requirement of shared norms is meant to address this problem. However, there is the danger that the rules either fail to exclude problematic perspectives or, on the contrary, exclude relevant ones (as Leuschner (2012) warns). Longino herself admits the complexity of the problem:

> While one function of public and common standards is to remind us of that distinction [between the marginal and the crackpot], and to help us draw it in particular cases, I do not know of any simple or formulaic solution to this problem. (Longino 1991, 674)

In my own argument, "this problem" has emerged as a crucial issue for successful public participation.

The distinction that Longino makes between intellectual and cognitive authority, stressing the importance of the former, may be seen as one attempt to address this problem. It opens the possibility that reasonable and relevant perspectives may be admitted even if they do not share all the standards that characterise experts' knowledge-producing practices. The openness of norms themselves to criticism and the creation of new case-specific norms offer another possibility to widen the circle of potential participants in the dialogue. The third class of possibilities for reducing the negative epistemic consequences of social marginalisation is opened, albeit less directly, thanks to the wider political and social changes towards greater equality.

The discussion of openness to criticism, creation of norms and the social change, however, brings attention to another aspect of this problem—the issue of effort. As the petunia controversy shows, such norms may not emerge automatically on contact between different perspectives. As already discussed, in the case of different communities, there may be no such shared norms and the success of critical interactions would then depend on their creation. This creation, in turn, may be expected to be both laborious and uncertain: "subject to all the vicissitudes of cross-cultural communication and translation" in Longino's (2002a, 130, fn) already quoted words. It is this idea of the necessity of intercultural translation that gives the title to the current section.

Longino thus stresses that creating a shared basis for communication requires effort. If scientists and scientific communities have the duty to strive towards objectivity, it follows that they also have the associated duty to make this effort in order to make objectivity possible. Accordingly, to the degree it is desirable to increase objectivity in science and science policy through greater public inclusiveness, it is also desirable that participants in this process make efforts in order to work out a shared basis for communication all participants in the dialogue may accept. This work may require the kind of translation Longino mentions. A group's aims, concepts and standards need to be translated so as to be understood by the outsiders and so that potential "overlaps" and mutually acceptable elements could become clear. This kind of translation may be required from all groups taking part in the discussion. However, it seems especially important in the case of groups whose standards and aims may be expected to dominate the debate (as experts may be expected to dominate science-related debates in knowledge societies) and who get to decide the fate of the contributions of more marginalised participants. As the petunia controversy showed, it is the public whose participation is precarious. There is no doubt that scientific experts, policy-makers and industry representatives have to be listened to. This is why I formulate the question from the point of view of one of those potentially predominating parties—ought scientists to be translators?

This discussion of such an approach to objectivity has certain far-reaching implications for individual scientists and professional organisations. It places on them some additional and possibly onerous duties—as Longino's quote shows, she recognises that this kind of effort is not easy. What would it mean for the scientist to live by these prescriptions? Would it be possible? Specifically, would it be possible to initiate this change of attitude and behaviour with the help of the politically motivated developments I have been describing? Would this duty be compatible with other duties that are currently valued—the duties related to epistemic significance and quality? The additional burden of ”intercultural translation” may be seen as compatible with more traditional epistemic duties but distracting from them. As a result, scientists burdened with the duty to translate and communicate would do science less effectively. More seriously, one may worry that something epistemically significant may go lost in translation. The concern in this case is that creating the shared basis for communication would mean watering down epistemic standards of scientific community or giving up some of scientific aims. If so, scientists made translators would not only do less of science but it would also be worse science.

One may thus worry that the duties implied by Longino's criteria are not realistic, because they may place on scientists an excessively heavy burden, or that they are epistemically undesirable, because they threaten the epistemic integrity of the research. If this worry is justified, the desirability and the promise of practical applicability of Longino's account are undermined. The aim of the following subsection is to show, on the example of the developments in American archaeology, that this worry does not necessarily materialise in practice.

6.4.3 American archaeology, "ethics of stewardship" and epistemic pluralism

In a series of papers (Wylie 1996; 1999; 2000; 2005; these themes are also discussed in Nicholas and Wylie 2009), Wylie analyses the development of professional ethics in American archaeology in the 20th century, with a particular focus on its last three decades. Discussion of this ethical development is presented against the background of extensive changes in archaeology's social environment and work conditions. Wylie's analysis shows how some consequences of this development can be evaluated as epistemically positive, which makes her analysis especially fitting for the argument I am making.

Setting the background for her argument, Wylie describes how for most of the 20th century at the core of American archaeology's self-understanding was the commitment to scientific aims. Archaeology was understood as a scientific enterprise whose interest in archaeological objects is due to their evidential value for research, setting archaeologists apart from those who value these objects for other reasons (aesthetical, commercial, emotional etc.). This understanding was a part of archaeological practice and education. It was also reflected in the regulations and ethical guidelines of the professional organisations such as the Society for American Archaeology (SAA).

Wylie describes the changes that have happened in this self-conception, as reflected in archaeologists' professional ethics, between the early 1970s and the present day. These changes can be understood as a response to a series of challenges, or pressures, that made the previous self-understanding and practice based on it increasingly problematic:

> the SAA's position on a range of ethics issues has evolved incrementally in response to pressures for accountability that have arisen and have themselves evolved, both in content and in urgency, over the last thirty years. (Wylie 1999, 332)

Wylie's analysis of these challenges provides a fascinating insight how policy changes that may later be positively evaluated from the epistemic point of view could be born as a response to various changes in the social, cultural and political context of a research field and be initially motivated by considerations that are not primarily epistemic.

One important factor behind this development was the pressure to professionalise archaeology, which previously defined itself by the commitment to certain aims rather than any certification procedure. The demands to establish the minimal professional standards and ensure compliance with them had been made by various parties for several decades. Their urgency grew as more archaeologists were employed in cultural resource management or as consultants for various industries and governmental agencies. (By that time the majority of archaeologists were in fact so employed instead of pursuing purely academic research.) New professional obligations of archaeologists and the potential tension between the commitment to scientific aims and other professional duties and responsibilities made informal self-regulation seem insufficient.

Concerns about the preservation of archaeological record were also growing. It was threatened by development projects such as urban extension, road building, agriculture and industry on the one hand and large-scale looting and trade in antiquities on the other.[112] As the commitment to conservation among archaeologists grew, it could not but have an effect on the commitment to scientific aims of archaeology. (The commitment to conservation, however, might simply express the conviction that by conserving the archaeological record now one ensures better chances for gaining scientific knowledge later. In this case, scientific aims were still given primacy.)

Besides that, the archaeologists' right to work with archaeological material was increasingly challenged by groups who had a different claim on it. Indigenous peoples, descendant communities and other ethnic, religious and cultural groups saw this material as a part of their living cultural tradition, objected to its treatment as material for scientific inquiry and questioned the universal value of scientific understanding achieved. Challenges from

[112] Looting posed a separate set of challenges for keeping scientific aims of archaeology free from commercialism. While archaeologists and their professional organisations had strongly opposed looting and trade in antiquities, the dilemma whether it is ever admissible to work with materials of problematic provenance if it allows salvaging valuable scientific information and the worry that scientific publications may be used by looters were gaining prominence for archaeologists.

these parties might take a variety of forms, legal, moral and political. They might include direct activism and intertwine with other forms of these groups' struggle for their rights. The right of these groups to control their heritage was also gradually recognised by the law. For example, the Native American Grave Protection and Repatriation Act (NAGPRA) was enacted in the United States in 1990. On the archaeologists' side there was also the growing recognition that the interests of descendant communities and indigenous peoples should be taken into account in ethically appropriate archaeological research. As a result of these developments, archaeologists had to deal with a variety of new demands. For example, representatives of descendant groups might require that a burial ground or a sacred site be respected as such rather than approached as an archaeological resource. This, in turn, might limit or block archaeologists' possibilities to excavate it, to undertake certain kinds of investigations or to exhibit and publish the resulting material. Requirements of organising consultations, obtaining informed consent and communicating results might be imposed on archaeologists.

This is the context in which Wylie discusses the emergence a new self-understanding for the discipline of archaeology. From one point of view the response of archaeologists can be seen as a mostly pragmatic reaction. Its aim may be understood as securing space for free research in the situation where the possibilities for it became more and more restricted. In one of her overviews of the historical development of ethics codes in archaeology, Wylie (1999) refers to the report on ethics activities of member organisations prepared in 1980 by the American Association for the Advancement of Science. One of the central messages of the report was the need to establish clear, enforceable and socially responsive ethical guidelines. The report warned that otherwise there was a serious risk of inviting external control and regulation, which would threaten research autonomy. As Wylie points out, the guidelines that archaeologist have since adopted "respond to a sea-change in the conditions of archaeological practice that realise some of the worst fears" (Wylie 1999, 321) expressed in the report. So, the changes can be seen as a defensive move on the part of archaeologists.

At the same time, it is important to point out other, less self-centred and pragmatic considerations behind the change. Wylie, who with Mark Lynott chaired the ethics committee of the SAA at that time, offers a first-person account of the 1993 meeting at the University of Nevada-Reno, where the new approach to disciplinary ethics began to take shape (Wylie 2005). The conversation started with issues related to looting and commercialisation. Later, the problem of assessing archaeological objects in monetary terms (for restitution, compensation or insurance purposes) was raised. Such an assessment was often at odds with the way descendant communities approached these objects and with what was at stake for them. Museum curators and archaeologists could share a similar feeling of discontent with monetary evaluation of artefacts. The notion of stewardship emerged in the conversation—the role of the archaeologist as the keeper of archaeological material for wider society. This notion provided the starting point for working out new guidelines that were to address the complex of issues American archaeology faced. Thus, it is possible to suggest that the changes that have since happened in archaeological practice were not fully defined by external pressures and the desire to deflect them. They can also be seen as inspired by the will to change one's practice, motivated by ethical considerations growing out of one's professional experience. The subsequent reception of the principles first formulated at the University of Nevada-Reno meeting shows that they captured concerns and perspectives of many researchers in the field.

The process of working out new principles for archaeological practice culminated in 1996 by the adoption by the Society for American Archaeology of the "Principles of Archaeological Ethics" (Society for American Archaeology 1996). In these principles, the notion of stewardship takes centre stage. The change it brings is not limited to the ethical side of archaeologists' practice. Rather, it implies a profound change in the ways aims of archaeology are understood. The ethics of stewardship shifts the focus from the archaeologists' duty to advance scientific aims of the discipline to a wider set of responsibilities for the material they work with and before the persons and groups that have a relation to this material. According to the first principle,

> It is the responsibility of all archaeologists to work for the long-term conservation and protection of the archaeological record by practicing and promoting stewardship of the archaeological record. Stewards are both caretakers of and advocates for the archaeological record for the benefit of all people; as they investigate and interpret the record, they should use the specialised knowledge they gain to promote public understanding and support for its long-term preservation.

As Wylie points out, this understanding of archaeologists as caretakers rather than the privileged (or even the sole) users of archaeological record shows the readiness to take into account other interests in archaeological record in addition to those of scientific archaeology. Accordingly, the duty to communicate and to find compromises acquires a key role. It is reflected in the second principle:

> Responsible archaeological research, including all levels of professional activity, requires an acknowledgment of public accountability and a commitment to make every reasonable effort, in good faith, to consult actively with affected group(s), with the goal of establishing a working relationship that can be beneficial to all parties involved.

The duty of what I have called being a translator in intercommunity dialogue has thus been inscribed in the central ethical principles of a scientific field.

Wylie discusses some examples of the reaction to these changes. Some archaeologists oppose them, seeing them as a capitulation to the assault on proper scientific aims of archaeology. This group demands these traditional aims be upheld. Others criticise them precisely for an insufficient departure from the traditional aims. As Wylie points out, there are several possible interpretations of the notion of stewardship. Some of them may allow one to continue the traditional archaeological practice in a mostly unchanged way. Nevertheless, Wylie argues that American archaeologists nowadays do generally recognise the duty of accountability to other groups and try to communicate and collaborate with them, often going beyond the legal requirements. (As an example of this lasting change of attitude on the part of archaeologists Wylie refers to the series "Working Together" published in the *SAA Bulletin* and documenting archaeologists experience with this more collaborative practice.) The general principles of professional ethics find their expression in specific arrangements for collaboration, negotiations with particular stakeholders and case-specific solutions to demands of a particular situation. The assumption about the primacy of scientific aims is given up. Instead, there is the commitment to develop an intercommunity understanding and to reach an agreement about aims and values to be taken into account. Thus, one can conclude that there is a successful example of making the duty to communicate and establish a shared understanding a part of both individual and disciplinary practice and self-understanding.

However, one may continue to worry that compromises involved in collaborative research endanger the achievement of epistemic aims of archaeology. I suggest that Wylie's analyses help to address this concern as well. Her earlier articles already mention the positive epistemic experience some of the archaeologists describe (see, e.g., Wylie 1996, 181 and 1999, 329–330). In Wylie's recent papers (Wylie 2014; 2015), the discussion of the epistemic aspects of collaborative research, with a special focus on the issues of pluralism, takes centre stage.

Wylie discusses several kinds of pluralism that emerge from collaboration between archaeologists and descendant communities. On the one hand, there is what Wylie calls "syncretic pluralism" (Wylie 2015, 195). Wylie describes it as tolerant but non-interactive.

This kind of pluralism reflects ideas about responsibility before descendant communities and the commitment to seek their consent, to proceed in a culturally sensitive way and to reciprocate (for example, by explaining the results of a research project to the descendant community). However, in the case of syncretic pluralism there needs not be any epistemic contact with the contents of alternative points of view. Epistemic practice, standards and aims of archaeology may mostly remain the same. In contrast to that, Wylie discusses forms of pluralism where collaboration does influence the substance of archaeological practice, potentially changing its presuppositions and its content.

One example of a fruitful collaboration between archaeologists and descendant communities that Wylie discusses is the study of the frozen remains of a man found in 1999 in British Columbia on the traditional territory of the Champagne and Aishihic First Nations and named Kwäday Dän Ts'ìnchí (Long Ago Person Found) by them.[113] The aims of the study and the methods to be applied to the remains were set in cooperation by the representatives of the First Nations, local authorities, and researchers. Wylie describes how some of the questions addressed during this collaborative research reflected the interests of the descendant community that did not belong to the traditional interests of the archaeologists. For example, community members wanted to know what family and clan the man belonged to, so as to be able to organise a proper burial and memorial. Motivated with this concern, they agreed to the DNA testing of the remains and a considerable number of volunteers submitted their samples for comparison.

The answers to these questions, initially addressed out of respect for the descendant community, turned out to be relevant to traditional archaeological concerns as well. For example, the results of the comparison between the DNA samples indicated living relatives both in the coastal and the inland parts of the region. Together with other findings, such as the changes in the lifetime dietary profile of the man (the switch during the last year of life from coastal to inland foods) and the mix of coastal and inland materials in his clothing and tools, these results bring to the fore the importance of family connection stretching across the coast-inland divide. With that, the previously predominating assumptions about the archaeology of the region, which stressed the geographical localisation, were put into doubt. Thus, the collaborative project resulted in a challenge to the background assumptions in archaeological knowledge, inspiring further research to address new questions.

This collaboration shows that certain compromises involved in collaborative practice do not exclude the possibility of obtaining results that are relevant for scientific aims of archaeology. They may even have the potential to transform some long-standing assumptions of the discipline. Wylie discusses possibilities to develop this kind of collaboration further, creating the form of pluralism she calls "dynamic" (Wylie 2015, 198). Dynamic pluralism opens the traditional archaeological understanding for interactions with other communities' understandings and viewpoints.

One of the worries I have mentioned in connection with Longino's criteria is the concern that scientists as translators may do epistemically worse science. Wylie's analysis shows that it needs not be so. It may be in certain respects a different science, addressing different questions and approaching the material from a different position. Even so, it may provide novel and relevant inputs for traditional research projects. Wylie also argues that ultimately collaborative science helps to improve scientific practice by opening it to new criticism. She calls the readiness to subject one's assumptions and methods to criticism one of the core commitments of scientific inquiry. Cooperating with other communities is thus beneficial in principle, as it allows realising this commitment to criticism more fully.

[113] For information about the project see, e.g., British Columbia Ministry of Forests (n.d.) and especially Hebda et al. (2011).

This discussion brings up themes that are central for Longino's argument about the essential connection between criticism, objectivity and knowledge. Wylie herself refers to Longino's criteria when analysing her examples. In particular, Wylie sees them as relevant for addressing the issues raised by the fourth of Longino's criteria, that of tempered intellectual authority. Wylie argues that this criterion should be extended so as to require the inclusion of non-scientific communities. Members of scientific communities should develop collaborations with other communities whose knowledge and perspectives have the potential to challenge fruitfully scientific community's assumptions and practice—not only the knowledge of specific facts but more general frameworks and practices as well. As Wylie (2014, 80) writes,

> If well-functioning epistemic communities are to counteract the risks of dysfunctional group dynamics that insulate their established standards of justification from critical scrutiny and revision, they must seek out critical, collaborative engagement with those communities that are most likely to have the resources—not only to fill lacunae and correct specific errors in their substantive beliefs but also to generate a critical standpoint on their own knowledge-making and ratifying practices.[114]

The position I defend with respect to lay knowledge, arguing that it may be relevant for both supplanting experts' factual knowledge and helping to expose their community's assumptions, is very close to Wylie's. (Unlike Wylie, however, I also stress the continuity of my position with Longino's, pointing out the elements of Longino's account that support such intercommunity critical contacts). The development of ethics in American archaeology, as Wylie describes it, provides a reassuring example in support of this position. As a result of what was to a considerable degree a pragmatic response to pressures interfering with archaeology practiced as usually, the possibility of a more inclusive practice where lay perspectives and knowledge may be taken seriously has emerged. Some of the results of this new practice may be judged as an epistemic improvement also from the point of view of the traditional epistemic practices.

6.4.4 Philosophical implications

In my thesis, I use Wylie's analyses of the changes in the practices of American archaeology to support a number of proposals I have made. First, it is another example, along with the German case previously described, of the possibility that I consider crucial for establishing a contact between philosophy of science and developments in science policy. I have been arguing that developments in the social organisation of science, even if caused by primarily ethical and political considerations, may result in the creation of niches of opportunity for realising an inclusive critical community. The changes in the conditions for American archaeology, from the laws such as the NAGPRA to the ethical guidelines for the discipline and to the infrastructure for collaboration in specific locations can be seen as enabling such niches. (Wylie remarks that a crucial condition for successful collaboration with respect to unanticipated archaeological finds such as Kwäday Dän Ts'ìnchí has been the already existing political and legal infrastructure—Wylie 2014, 72–73 and 74.) Wylie's analyses are a particularly important example for my argument because one of the points Wylie makes with her analyses is the epistemically consequential character of the developments she describes:

> While the impetus for these collaborations is often, in the first instance, moral and political—they arise from demands for respect, reciprocity, consultation—increasingly they are also robustly epistemic. (Wylie 2015, 189)

[114] In 2015, Wylie is even closer to Longino's terminology as she formulates this principle in terms of "collaborations with external communities whose epistemic goals, practices, and beliefs differ from their own in ways that have the potential to mobilise *transformative criticism*" (Wylie 2015, 207, italics mine).

So, the case of archaeological ethics may be used to support my suggestion that a development in science policy initiated by ethical and political reasons may also lead to epistemic benefits. These are the benefits that Longino stresses when she writes about the importance of inclusive criticism for uncovering background assumptions or that Mark Brown refers to when he mentions the possibility to fill in experts' blind spots with the help of a wider representation of perspectives. Such a convergence of results is especially important given the potential differences between epistemically motivated and politically motivated changes that I discussed at the beginning of the chapter.

The epistemic potential of the developments that Wylie analyses is also important in another respect. In these two chapters I have argued, with Longino and against Kitcher, that democratisation of science policy, as opposed to the democratisation of science, is not alone sufficient. There are strong reasons to call for more inclusive production of knowledge. I have also suggested that there are ongoing developments in science policy that may be similar enough to what can be recommended for the creation of such inclusive science practice. However, as Lengwiler (2008, 187) points out, most initiatives for democratising science policy limit public involvement to the level of science policy. Public involvement in actual research practices is rarely attempted or even called for. Wylie's analysis shows that sometimes the former may lead to the latter. The involvement of the public in science policy decisions (e.g., whether to pursue a specific project) may sometimes be successfully combined with the participation of the public in research-related questions (e.g., how to proceed with the project).

Second, the introduction of Wylie's analyses helps to address some questions raised by the German case. The most important among them is the question whether the difficulties of intercommunity communication may ever be overcome in practice and whether working to overcome them may become a part of researchers' professional practice without unsustainable epistemic loss. The example of American archaeology shows that sometimes it may be possible. Relatedly, Wylie's discussion of collaboration with the parties that have traditionally been marginalised politically and epistemically is important for addressing the issue of inequalities of power that arises inevitably for an approach that demands inclusive discussion. In their discussion of possibilities for collaborative practice, Wylie and Nicholas identify "crucial disparities in power among stakeholders" (Wylie and Nicholas 2009, 39) as one of the central issues. They also discuss several approaches, already tested in various collaborations (within and outside of archaeology), for addressing such disparities. Analysing these collaborations may thus be helpful for understanding how productive inclusive discussion may be possible in society characterised by inequalities.

As in the previous case, however, one may ask whether the same developments cannot be adequately described in the framework of well-ordered science. In the remainder of the subsection I argue that the analysis of the developments in archaeology that Wylie offers provides in this case a more adequate understanding than thinking in terms of well-ordered science could. I also suggest that this analysis further supports the local approach to the social organisation of science. I conclude by outlining some questions that successful collaborations such as those in archaeology may nevertheless raise.

Discussing the implications of Jasanoff's analysis, I argued that the interpretation of the petunia controversy in terms of well-ordered science could miss important aspects of the case. Now I suggest that a similar argument can be made about the case of the ethics of stewardship in American archaeology. In principle, an interpretation within the framework of well-ordered science is possible. One could say that as a result of the adoption of the new ethical principles, American archaeology has become better ordered. The indigenous peoples and descendant communities are now able to provide input about their wishes—what it is in their interest to know about the archaeological material in question. Archaeologists can then use their expertise to address these needs (simultaneously tutoring these communities about research possibilities and significance).

This reading, however, may be problematic due to the limitations on wishes to be taken into account in well-ordered science when it comes to research directions and ethical limitations on research. As previously described, in the latest version of well-ordered science deliberators' wishes are subject to the stringent epistemic and affective conditions. One of the consequences of these epistemic conditions is the exclusion of religious arguments:

> the epistemic conditions rule out many of the firm pronouncements that actually disrupt conversations about values, including the familiar assertions that particular things are required because they are commanded by one's preferred deity. (Kitcher 2011c, 51)

If this strong secular orientation is taken into account, the evaluation of the changes in American archaeology becomes ambiguous. On the one hand, these changes can be seen as giving members of the public their appropriate say in the matters of research planning. This is a development that can be characterised as democratically desirable, especially giving the unfairness that this specific public has historically faced. On the other hand, religious reasons have played a prominent role in these developments. Respect towards their religious beliefs has been what many of the indigenous and descendant groups have demanded and what many archaeologists working in the more collaborative projects have demonstrated. This may support an altogether different interpretation of these developments. Instead of a successful case of democratisation, they may be seen as a capitulation of science before the tyranny of the ignorant.

The second interpretation ignores the epistemic and political benefits of collaborative practice that Wylie discusses. To the degree it misses these benefits, it misses an important example how science has in fact been made more democratic in practice. However, even if the interpretation that sees the recent developments in American archaeology as an example of well-ordering is upheld, it may be less adequate than Wylie's analysis. In my criticism of Kitcher's argument, I showed how Kitcher fails to recognise the possibility of substantial epistemic contributions from the public, both in terms of factual information and challenges to experts' frameworks. Wylie's discussion of epistemic benefits of these collaborations, however, stresses precisely this possibility of substantial expertise, including standpoints that allow critical examination of experts' knowledge-producing practices. In her discussion of dynamic pluralism, Wylie argues that archaeologists' lay partners do possess various forms of expertise and that realising the fundamental commitment to open and critical research community requires recognition of this expertise (see, e.g., Wylie 2015, 198–202). So, what may happen (and what is highly desirable) in collaborative projects is that not only the priorities experts assign to existing research aims change. Experts' understanding of their practice, its standards and its aims and the content of their knowledge may change too. In this case, limiting the discussion to well-ordering misses what is at stake in these developments according to Wylie—and what makes them an important example from which to learn for my argument.

Another reason to reject the universal model of well-ordered science is the distinctly non-universal character of the developments that Wylie analyses. Ethics of stewardship is bound with the particular challenges American archaeology faced between the 1970s and 1990s. While it can be read, as I do in the context of my thesis, as open to interpretation in terms of some general ideas about objectivity, the specific expression of these ideas is context-specific. It cannot be expected to persist unchangeably even within archaeology itself, not to mention to be transferable easily to other disciplines. As Wylie summarises this inevitably contextual character of the SAA principles,

> One lesson reinforced by our review of existing statements on archaeological ethics was that they were always responses to specific problems; however much they were intended to reach beyond the particularities of current practice, they were deeply structured by the conditions of their production. (Wylie 2005, 55)

Moreover, even within archaeology itself the principles of archaeological ethics are, in effect, underdetermined: specific forms of productive collaboration can only be worked out on a case-specific basis, in interactions with specific communities. Throughout her analyses, Wylie stresses that there are unlikely to be universal solutions in the sphere of professional ethics:

> ethics deliberation in archaeology must be responsive to context and circumstance. … [T]he principles developed by archaeological societies need to be flexible enough to allow for and to support local negotiations. (Wylie 2000, 155)

So, just as the comparative study of different civic epistemologies, Wylie's analysis of the development of professional ethics undermines the hope for a universal solution that would enable a productive relation between researchers and the public. Also in this case, the hope to understand specific developments is better supported than the hope to acquire universal instruments for realising an epistemically and democratically better science.

In this section, I suggested that the developments Wylie describes can be read as a "success story". Changes in the professional ethics have made possible novel, democratically and epistemically beneficial relations between experts and the public. While I do consider archaeology a mostly successful example, it prompts a return to one of the problems for proposals about the social organisation of science that I first discussed in chapter 1.

Discussing philosophical arguments about the social organisation of science, I argued that one of the aspects that require explicit discussion is the balance of practical and epistemic consequences that may be expected if the proposed solutions are realised. I argued that there are no grounds for excluding research from the appraisal in terms of both types of consequences. There is no way to escape the fact that any form of organisation comes with a certain trade-off between gains and losses of different kinds. I suggested that an appropriate reaction is to try to be explicit about the balance that can be expected from the proposal in question. The attention to the possibility of epistemic losses is also one of the points I lauded in the case of Kitcher's discussion of well-ordered science.

Throughout the last two chapters, I have been arguing that making both science policy and research more inclusive and increasing in this way its objectivity may bring important epistemic and democratic benefits. Showing that these benefits are possible in practice has been one of the motivations beyond introducing Wylie's analysis. Yet Wylie's analysis also reintroduces the concern about possible losses. It is possible that the greater power laypersons have in interactions with experts and the new duties imposed on experts may in specific situations lead to epistemic losses. Some aims we currently consider epistemically valuable and achievable may no longer be possible under the new regime. Both the external forms of accountability such as the NAGPRA and the archaeologists' growing commitment to respective practice mean that research projects that once might have been archaeology as usual may no longer be possible. (One illustration of a specific loss in the context of an overall productive collaboration is the case of the archaeologist who left unexcavated a section of the archaeological site that might have been a sacred dance floor. The decision reflected the acknowledgement that the sacred dance floor, which is an object of intense interest for the archaeologist, is also a sacred space not to be disturbed for the indigenous community—see, e.g., Wylie 1996, 181 and Nicholas and Wylie 2009, 33–34.)

Noting such specific losses brought by changes in the organisation of science, as Wylie does, should constitute a part of the analysis of such developments. The fact of such losses, however, should not be taken as an automatic argument for the preservation of the status quo in the social organisation of science. As I have argued, the status quo too entails a specific trade-off between gains and losses. One of the aims of my thesis, as well as of many other arguments about the social organisation of science, is to show that the current situation may be improved, both epistemically and democratically.

6.4.5 Conclusion

The case study of the previous section and reflections on Longino's criteria of objectivity brought to the fore the central importance of a shared basis for communication for enabling productive inclusive discussion. Taking part in communication and "translation" in order to work out such a shared basis may thus be a necessary part of striving to be objective.

In this section, I drew on Wylie's analyses of the emergence of ethics of stewardship in American archaeology in order to provide an example that it is possible to recognise this duty and to integrate it into practice. Wylie shows how the new ethical principles emerged in response to a number of pressures on traditional practices of archaeology, above all the demand for a more ethically responsible and respectful research practice. At the same time, they have changed and in some cases benefited epistemic practices of archaeology, enabling a more pluralistic approach to knowledge production in collaborative projects. Thus, this example illustrates the possibility that I have been arguing for—the possibility that epistemically beneficial changes may become possible as a result of politically and ethically motivated developments. It also reinforces the point about the relevance of detailed policy analyses in philosophy of science.

In the concluding subsection I compared my use of Wylie's analyses with an interpretation that may be proposed on the basis of well-ordered science. I showed that, of the two interpretations possible, one misses the positive political and epistemic developments that Wylie describes. The other, while recognising their democratic success, misses their important epistemic aspects. I concluded the section by returning to the problem of explicating the balance of epistemic and practical gains and losses brought by a change in the social organisation of science. This question remains important also for the cases of change that can be deemed overall successful.

6.5 Lessons and roles for the philosopher of science

At the beginning of this chapter I suggested that Longino's approach to objectivity can serve as the starting point for bringing into contact philosophical proposals and ongoing "democratising" developments in science policy. As long as there are science policy initiatives that understand objectivity in a similar way and attempt to increase it by initiating, or improving, inclusive critical discussion, they can be used in order to study possibilities and obstacles for realising a philosophical proposal in practice. In order to understand them, detailed analyses of science policy are necessary. I included two such analyses in order to offer a glimpse of the variety of information they may provide.

The aim of this section is to discuss what lessons the philosopher of science could take from such analyses and what the role of the philosopher could be in these developments. I first outline some general lessons that can be formulated on the basis of the examples I have discussed. They are sobering, because policy analyses show that there is no simple way to realise a proposal for change. Any such attempt would take place in a complex context with numerous stakeholders involved. At the same time, however, this very complexity of context and variety of motivations may be a source of hope. It may be taken as an indication of robust interest in democratisation of science in society. I suggest that this interest may offer an important opening for philosophy of science. In the concluding part of the section I discuss some forms the philosopher's involvement with science policy could take, showing the relation of my position to a number of arguments about more applied forms of philosophy of science.

I suggest that the single most important lesson of analyses such as Jasanoff's and Wylie's is the multiplicity of factors contributing to the emergence and development of a particular initiative that attempts to increase inclusive public debate in science policy. Different parties, different forms of action and different levels of policy may be involved. This involvement may in turn play out against the background characterised by different political and cultural trends and persistent institutions, norms and practices that comprise the local political culture.

In these democratising developments, political activism and traditional forms of political action, such as a political party representing a specific programme in the parliament, may play a role. For example, a political party may act to introduce new legislation opening science policy to a wider public involvement and imposing new duties on scientists, research sponsors and decision-makers. This is what the German Greens did when their dissenting position played an important role in the formulation of the 1990 Genetic Engineering Law. Or, activists may act attempting to influence the way the new law is applied in practice, like the environmentalist activists in Germany did when they used this law in attempts to redefine the framework of discussion about genetic engineering. At the same time, both formal and informal political action is constrained by the persistent features of the political culture such as its civic epistemology and by actions of other interested parties, from industry and researchers to international bodies such as the European Union. So, in the German case public hearings with their potential to initiate transformative critical dialogue were a compromise that did not fully reflect what any of the parties involved desired. After a relatively short period of time they were abandoned for more traditional forms of German civic epistemology that were better suited to the interests of the stakeholders whose perspective prevailed in the discussion.

In the case of the changes in American archaeology, political activism on different levels also played a considerable role. In addition to that, changes in the attitudes of researchers, characterised by the growing acknowledgement of the necessity of a more respective practice were crucial for this development. However, the readiness of researchers to work together with the public, important as it is, may not always be sufficient. For example, it was not in the German case. While the researchers might have been ready to discuss specific risks and benefits, there was a dramatic mismatch of understandings about the appropriate framing of discussion between the researchers and the opposing activists.

The introduction of a law (such as the German Genetic Engineering Law or the American NAGPRA) may play an important role in creating niches of opportunity for inclusive dialogue. However, laws are not necessarily capable of fully defining the further development of the situation. As the German case demonstrated, the way the new possibilities offered by the law are used may defy expectations about its functioning. Similarly, in the American case the laws protecting the interests of descendant communities were just one of the factors archaeologists had to take into account when modifying their practice. A variety of responses was in principle possible. Binding decisions of researchers' professional bodies (such as the SAA) may have a more immediate relevance than laws, as they may influence researchers' practice more directly. However, even they do not necessarily predetermine the ways individual practices would change. As Wylie shows, there remain different positions among archaeologists even after the official adoption of the "Principles of Archaeological Ethics". Moreover, as Wylie argues, such professional ethics principles are essentially open for interpretation and adaptation to particular circumstances. In effect, formal regulations on different levels are underdetermined. Their determination depends on a variety of factors, including the behaviours of the parties involved and the conditions of their interactions.

So, in a particular development, different parties (researchers, political parties with particular programmes, activists for a particular cause etc.) and different levels of decision-making (laws, professional ethics codes etc.) may be involved. Jasanoff's analysis also attracts attention to less tangible and less open to direct intervention but nevertheless influential aspects of the context where these changes take place, such as political cultures, civic epistemologies and "sticky" framings. Similarly influential and difficult to modify directly are political and cultural trends in the background. In the cases discussed they include phenomena such as the growth of importance of biotechnology for international technological competitiveness, the emergence of the "green" concerns in the public sphere, the recognition of the political and cultural rights of the indigenous peoples, changing patterns of employment in academic disciplines etc.

This complexity simultaneously gives grounds for pessimism and optimism with regard to the philosopher's possibilities for analysis and action. Pessimistically, one has to conclude that there can be no single handle for putting a philosophical proposal into practice. Given the variety of factors that can influence the success of a change in the organisation of knowledge production and related policies, neither persuading researchers to adopt new responsibilities nor mobilising the public or introducing novel legislation guarantees success. The complexity of the situation is such as to seem to discourage any active position for the philosopher interested in the change in the social organisation of science. An analysis may help to understand why a particular attempt developed the way it did but it seems impossible to predict its development in advance or to influence it with any certainty.

Optimistically, one can see grounds for a (cautiously) more active position for the philosopher. The very variety of parties that may be acting in order to increase public participation in research and science policy, creating new venues, demanding (or showing readiness to offer) new forms of accountability or redefining its previously existing forms etc., show that there exists a certain recognition of the benefits of a greater involvement of different perspectives in science and science policy and there is some will to act in order to make this more inclusive approach a reality. This, in turn, may offer an important opening for philosophy of science.

I have previously pointed out that despite the similarities between the ideas of objectivity in scientific community and in democratic society, there may be important differences in the perspectives deemed relevant and, as a result, in the outcomes of the changes. My discussion of the case studies, particularly Wylie's analyses, aimed to show that, even given the potential divergence, politically motivated developments may nevertheless bring epistemic benefit. Now I suggest that the very domination of non-epistemic motivations may help to address an important concern about philosophical proposals: the doubt whether there exists political will to implement changes proposed on the epistemic grounds. Are there in fact politicians, researchers and members of the public willing to initiate a switch to well-ordered science, or socially responsible science, or science that realises the potential of transformative criticism?

The discussion of the motivation to realise proposals about the social organisation of science is conspicuous by its absence in many of the arguments that are analysed in my thesis. The constatation that "a solution … will also involve a social reorganisation of scientific research, achieved through political action" (James Brown 2008a, 190) is not followed by a discussion of the parties ready to deliver the political action required. Arguments such as James Brown's identify an important problem and show a way to solve it. One may acknowledge that we as society would be better off if we addressed the problem and applied the solution. This, however, fails to show by itself that there is willingness in society to actually do so.[115] Similarly, Kitcher's discussion "Where" and "How" to improve democratic deliberation in science identifies a predicament and proposes a way out. There is no discussion, however, whether there are parties—politicians, decision-makers, popular movements—in position to act on these proposals and recommendations and actually interested and motivated to do so. The question of motivation arises especially sharply, given Kitcher's gloomy view of society's attitude towards science.

With the discussion of recent developments in science policy I have shown that there are in fact different parties interested for different reasons in making some aspects of research and science policy more democratic. Accordingly, philosophers could attempt to work with ongoing developments, attracting attention to their epistemic potential. By focusing on the processes already happening, the philosopher may then act to demonstrate epistemic benefits that these developments have a potential to bring, to discuss how to increase these benefits or to warn against potentially troublesome developments. In this way, it is possible to work with existing motivations and interested parties instead of trying to initiate an epistemic change from the ground up. Returning to Kitcher's (2011c) formulation, instead of pointing "where" to begin, the philosopher could be looking "where" relevant developments are already happening. The existence, and the variety, of politically and ethically motivated attempts to increase objectivity in the political sphere can in this way be understood as a resource for philosophy of science.

What form could such a local cooperation with possibilities opened by non-epistemic developments take? Over the recent years there have been a number of proposals for a more applied or engaged philosophy of science.[116] The approach I have been advocating can be seen as related to several strands in this development.

One possibility is the application of philosophy of science's conceptual tools and analytical skills in the context of specific socially relevant issues. Douglas (2010) recommends such an approach with her "applied philosophy of science in context". She argues that instead of trying to apply "ready-made philosophy of science" (Douglas 2010, 318), philosophers could do socially relevant philosophy of science by getting involved with practitioners of a scientific discipline in a particular local context and using philosophical analysis as a tool to address issues arising in that context. Outlining one possibility to do so, Douglas draws on her experience of working in the areas related to science used in policy-making, in particular her cooperation with the Society for Risk Analysis.

There are several philosophical arguments that can be interpreted as a kind of local application of Longino's ideas to specific cases of reasoning in sciences. For example, Soemini Kasanmoentalib (1996) uses Longino's account of the role of contextual values in order to clarify values involved in risk assessments before the deliberate release of genetically modified organisms. Rachel Ankeny Majeske (1996) proposes using Longino's account of objectivity as a better basis for thinking about the kind of objectivity that can be achieved in decisions about the candidate selection for organ transplantation. Tabatha J. Wallington and Susan A. Moore (2005) use the framework of critical contextual empiricism to analyse the

[115] Approaches to the social organisation of science that are related to movements that have experience of political action and political activism, such as feminist approaches, may be an exception. There may not be society-wide willingness to act, but some of it may be expected among those involved with the movement.

[116] In the text I only discuss several examples to which my own position is the closest. The field of "socially relevant" or "socially engaged" philosophy of science is not exhausted by these examples. For an overview of themes, examples and rationales see the introduction to a special number of *Synthese* by Carla Fehr and Kathryn S. Plaisance (2010) and the introduction to a collection of papers in *Erkenntnis* by Francis Cartieri and Angela Potochnik (2014).

reasoning of a group of ecologists as exhibited in the Delphi-process the authors have organised.

My approach shares with these examples the application of Longino's ideas to specific issues with the aim of achieving a greater clarity. However, my focus lies on a different plane from analyses of reasoning and decision-making in specific scientific fields. Instead, I am interested in the social conditions for inclusive critical discussion as a part of knowledge production and science policy decision-making. So, I use Longino's criteria of objectivity in order to show how specific forms of organisation of science that have emerged as a result of novel professional, institutional and legal arrangements may be understood *as if* they were an attempted improvement of objectivity and to analyse possible obstacles for emergence of productive inclusive discussion in these developments. Nevertheless, my position is similar to what Douglas's argument recommends in two crucial respects. The form of philosophy I propose is intended to be local and particular, focusing on specific developments. It is also intended to begin in the middle of things, so to say, following the interests and motivations of participants.

The interest in specific forms of organisation and their wider contexts connects my approach with another strand of thinking about a more applied philosophy of science. One example of such an approach is the previously discussed paper by Leuschner (2012). In her paper, Leuschner proposes that the creation of a pluralistic community may be enabled by political measures on a case-by-case basis. A task of philosophy of science can then be to analyse how these "ideal deliberative instances" (Leuschner 2012, 197) may be established in different fields of research. Despite the reliance on some general ideas, such as the (modified) framework of well-ordered science, the approach that Leuschner proposes is local: "even if it is not possible to find a satisfying solution for all cases, this does not mean that it is not possible to find a satisfying solution for every case" (Leuschner 2012, 197).

Another position, similar in its focus on specific instances but different in its abandonment of any ideal framework is the "non-ideal systems design" that Biddle (2014, 15) describes in one of his papers. This approach starts with analysing an aspect of an actual form of organisation of research in a specific field and showing its implications for the production of knowledge. A specific proposal for change is then made and its consequences analysed. After that, the process can be repeated. The non-ideal systems design thus attempts to improve the social organisation of research "in a piecemeal, iterative, and empirically-based manner" (Biddle 2014, 15).[117]

On the face of it, my own position is closer to Leuschner's, as we both share the focus on the political measures for the creation of more objective and pluralistic communities and rely on a general philosophical model. In the preceding chapters, I argued that Longino's approach is preferable to Kitcher's as such a model; my argument also included a response to Leuschner's criticism of Longino. However, I also have considerable sympathy for Biddle's approach.[118] As I argued previously in response to Biddle's criticism of Longino, I see no contradiction between adopting the framework of Longino's approach to objectivity and exploring specific forms of the social organisation of science. Longino's norms of objectivity are essentially underdetermined: they take a definite form in specific organisational arrangements and specific knowledge-producing communities. Analysing these arrangements

[117] A similar piecemeal, iterative approach that tests the consequences of each step empirically is the "adaptive management" that Reiss (2010, 441–442) describes.

[118] In a series of projects led by Endla Lõhkivi, I was one of the co-authors in a kind of empirically-based and piecemeal analysis. We analysed working conditions and work experiences in the Estonian humanities, showing their epistemic implications and discussing steps that could mitigate their negative influence on knowledge production (see Lõhkivi et al. 2012; Eigi et al. 2014).

and their potential problems can then be a task for philosophy of science. I subsequently described how Longino's criteria may be useful for such analysis, helping to see arrangements that have emerged as a result of political decisions in terms of niches enabling greater objectivity, and to understand obstacles for the realisation of their promise. In the iterative process that Biddle describes these criteria can play a role both when assessing the epistemic consequences of the current form of organisation of research and envisaging the directions in which it may be developed in future.

Wylie's work in ethics of archaeology, in the dual role of the co-chair of the committee responsible for the new ethical guidelines and the philosopher analysing the epistemic consequences of these guidelines can be taken as an example of yet another model of a more socially relevant philosophy of science. Kourany outlines such an ethics-centred approach in a number of papers.[119] In response to Giere's (2003) criticism that political activism takes one outside of the current professional philosophy of science, Kourany (2003b) argues that what she expects from philosophers is not different from what members of other disciplines studying science are already providing. Philosophers could similarly act as expert witnesses or advisers to governmental bodies, civil right organisations etc., contributing their specific expertise to the ongoing process of working out particular science policies. One specific form of such a contribution that takes centre stage in Kourany's (e.g., 2008a and 2010, ch. 5) approach is the participation in the development of scientific ethics codes, helping to make them more adequate, both ethically and epistemically. (As Kourany points out, the issues ethics codes are meant to address (such as fraud) have both ethical and epistemic dimensions.) While I have not attempted to develop this kind of philosophy in the thesis, my discussion of Wylie's work and my argument for cooperation with ongoing developments show that I consider it an important opportunity for philosophy of science.

Returning to the concern about the philosopher's passivity, I conclude that there exists the growing recognition of the possibility for a philosopher to do a more socially relevant philosophy of science and there already are a number of examples of doing this kind of philosophy. These approaches to the social organisation of science do not promise a universal solution, an ideal solution or a possibility to rebuild science and science policy from the ground up to the philosopher's specifications. Nevertheless, one may hope them to be philosophically satisfying—and hopefully also impactful.

6.6 Conclusion

In the previous chapter I brought attention to the importance of the wider social and political context for the possibility of objective communities as understood by Longino and I suggested that some developments in this context may open possibilities for realising such communities. In this chapter, I discussed how to connect philosophical ideas and relevant political developments.

In the first section of the chapter I suggested that it is Longino's notion of objectivity that can serve as the basis for bringing philosophy of science and science policy developments in contact. I drew on Mark Brown's discussion of political representation, especially representation as resemblance, to show that the understanding of objectivity as achieved through inclusive discussion from multiple points of view is also present in the political sphere. There, both its epistemic and political benefits are recognised. Attempts to democratise science and science policy can be seen as reflecting the recognition of these benefits. I suggested that due to this shared notion of objectivity, analyses of these political

[119] Kourany (2010, 119) actually mentions Wylie as an example of a philosopher's contribution to the ethics code of a discipline. Kourany (2013, 97–98) discusses the example of the American archaeologists' ethics code and Wylie's work with a slightly different emphasis—as a response to the concern that attempts to formulate ethics codes will result either in an irresolvable disagreement or the exclusion of some perspectives.

developments can be useful for understanding the possibilities and obstacles for realising a Longino-inspired approach to the organisation of scientific communities in practice.

In the second section I presented the first of the two case studies I use to substantiate my proposal. I summarised Jasanoff's discussion of civic epistemologies as the framework for understanding the context where attempts to create possibilities for greater objectivity unfold. I then discussed one of such attempts that Jasanoff describes—the introduction of public hearings as a part of the 1990 German Genetic Engineering Law and the controversy that the application of this law provoked. I suggested that this development can be usefully seen in terms of Longino's criteria of objectivity. In particular, these criteria allow identifying obstacles for productive inclusive dialogue. The issues related to shared norms and ways of assigning authority emerged as central for the failure of the German public participation experiment.

The importance of shared norms for the possibility of improving objectivity means the duty to take part in working such norms out as a part of the duty to strive towards objectivity. In the second case study I addressed the concern that this duty is either unsustainably onerous or epistemically undesirable, drawing on Wylie's analyses of the emergence of new professional ethics in American archaeology. Wylie's analysis of the pressures in response to which the new ethics principles were formulated contributes to the understanding of the role of the context for changes in the social organisation of science. Wylie's discussion of the modified archaeological practice also enables responding to the concerns outlined above. It shows that it is possible that the duty of intercommunity communication becomes a part of the disciplinary identity and that the new more inclusive practice may be epistemically productive.

Throughout the two sections I discussed the relation of the case studies to the position I defend in the thesis. I argued that such case-specific studies offer a more adequate understanding of science policy developments than the model of well-ordered science, which may miss certain important aspects of these developments. I suggested that the examples discussed support a local approach to the social organisation of science because they show that no universally applicable solutions or convergence of local solutions can be expected. I also defended Longino's ideas as a helpful framework for discussing these political developments against Biddle's and Leuschner's objections.

Finally, I suggested that political analyses, showing the complexity of the context where attempts of change unfold, undermine the hope of realising philosophical proposals in a simple or straightforward way. At the same time, the very variety of stakeholders potentially interested in these changes and the robustness of political reasons for them offer hope for the realisation of philosophical proposals in practice. Instead of attempting a change in the social organisation of science single-handedly, the philosopher may hope, by becoming involved with these developments and making visible their epistemic aspects, to add epistemic motivations to existing political ones. I closed the chapter by discussing several recent proposals about more socially relevant philosophy of science. I showed how my approach can be related to the proposals of applying philosophical tools to specific issues in specific contexts on the one hand and analysing specific political arrangements and possibilities for their gradual improvement on the other.

CONCLUSION

Philosophical accounts of the social aspects of science and proposals about the social organisation of science constitute a fascinating and increasingly noticeable development in philosophy of science. The aim of the thesis was to discuss a number of themes in connection with these proposals. This discussion resulted in three interconnected arguments. First, I discussed what considerations a philosophical proposal about the social organisation of science should take into account. I argued that both epistemic and non-epistemic consequences of a form of organisation of science demand discussion and justification. Second, I analysed Philip Kitcher's argument about the social organisation of science as a prominent example of proposals about the social organisation of science in philosophy of science. Alongside other important virtues, Kitcher's argument demonstrates the profound recognition of the consequential character of science. Nevertheless, I argued that Kitcher's approach has important problems that undermine its internal consistency and practical applicability. Finally, drawing on Helen Longino's ideas I proposed an approach to the social organisation of science that avoids the problems of Kitcher's well-ordered science. I showed that this approach enables a fruitful connection between philosophy of science and some developments in science policy and that it may serve as the basis for doing a more socially relevant philosophy of science.

In this section, I summarise the main points of these arguments and reflect upon their significance. I then discuss some questions that these arguments prompt to address in future.

In the thesis, the first two chapters introduced the arguments that form the subject of the discussion. These are the arguments that show the epistemic relevance of social aspects of science and on this basis make proposals about the social organisation of science. Setting the stage for the discussion of such arguments, I described some historical context for them and some factors that have prompted the renewed interest in the social aspects of science. I proposed to think about these factors as challenges to the traditional picture of science, brought about, on the one hand, by novel analyses of science and, on the other hand, by changes in science itself. I also discussed what a social account of science may mean. I summarised some arguments that show the role of social values in science, challenging the value-free ideal, or the role of social relations and structures, rejecting assumptions about the individualistic character of cognitive practices. I then showed how specific arguments open possibilities for proposals about the organisation of science and discussed the limitations of approaches focusing on one dimension of the sociality exclusively.

The main argument developed in this introductory part of the thesis had the aim to establish what kinds of considerations a proposal about the social organisation of science should take into account. Using James Robert Brown's argument about the socialisation of biomedical research as an example, I argued that a purely epistemological approach is not sustainable. James Brown states that his proposal is methodological: no social values need be involved for the acceptance of the recommendations he outlines. This position presupposes that once the epistemic rationale of the proposal is convincingly demonstrated, it is screened from a discussion in terms of social values. In my argument, I discussed three strategies to preclude such a discussion. These strategies include distinguishing pure research and application; maintaining that the value of knowledge trumps any other values; and arguing that science is value-neutral and can support all values equally well. I showed that none of these strategies cohere with James Brown's argument. In his argument, there is the recognition that biomedical research has practical consequences; that certain other values are at least as important as the advancement of knowledge; and that socialised biomedical research is better compatible with some values than others. I also discussed some more general objections to these strategies. The growing prominence of applied research blurs the line between research and application. The recognition of ethical limitations on research shows the wide acknowledgement of non-epistemic values in science. Finally, Hugh Lacey argues convincingly that science as practiced now is not value-neutral. Most generally, separating epistemic and practical consequences of research is not sustainable after the practice turn, or the shift of philosophical attention to scientific practices. On the practice-centred view advocated, for example, by Joseph Rouse, science is about experimentation and intervention in the world. The advancement of knowledge is thus always consequential for the world.

I concluded that making philosophical proposals about the social organisation of science one should explicate and weigh both the epistemic and the practical consequences the proposed change may be expected to bring. This position reflects a more adequate understanding of science that is attentive to both its practical character most generally and the consequential character of contemporary science specifically. It is also more responsible ethically, as it precludes the possibility to ignore the issues of responsibility and justice in connection with the question of the social organisation of science.

The third and fourth chapters described the development of Kitcher's account and presented my criticisms of it. Kitcher proposes a far-reaching reform of the social organisation of science based on an account that shows the fundamental importance of both social values and social interactions in science. Kitcher's account recognises the potential of science to influence society and the necessity to attend to the related questions of justice and fairness. Nevertheless, I showed that there are some important conflicts between Kitcher's account of science and his model of well-ordered science. These conflicts undermine the plausibility of Kitcher's approach and the promise well-ordered science may have for fulfilling the social functions of science in democratic society.

My first point of criticism concerned the role of the expert in well-ordered science. I showed how Kitcher's assumption of disinterested and objective experts contradicts his account on several levels. The role of the disinterested expert is in tension with the role of the deliberator presenting one's central interests—the role that experts are also expected to play. The assumption of disinterestedness is incompatible both with Kitcher's usual approach to modelling epistemic agents and his characterisation of actual scientists. In the case of Kitcher's arguments about the division of cognitive labour, it is shown how "sullied" agents may achieve an epistemically optimal result on the community level; there is no comparable social-level explanation for experts' disinterestedness in well-ordered science. Finally, the image of the interests-free experts points back to a very traditional view of science and society (the view Mark Brown connects with the traditional understanding of representation in science). It does not mix easily with Kitcher's account of thoroughly value-laden science.

My second point of criticism demonstrated a similar tension between the account of science and the proposal for its organisation when it comes to experts' frameworks and knowledge. Kitcher's discussion of classifications and epistemic significance stresses the role of interests and values. These values may, however, remain invisible, if the entire community shares them and if it fails to include those for whom these values and interests may be problematic. The involvement of outsiders' perspectives in order to control the assumptions involved in experts' concepts and frameworks is thus warranted. Yet, there are no provisions for that in well-ordered science. Similarly, Kitcher's discussion of solutions that are applicable in specific contexts may be used to attract attention to the importance of the knowledge of local conditions that experts do not necessarily possess. However, there is no expectation that laypersons familiar with this context may have relevant substantial knowledge; there are also no institutional arrangements to utilise it. I used Lacey's analysis of value-laden concepts in biotechnology and Brian Wynne's study that demonstrates the relevance of the local knowledge for problem-solving, in order to show that these issues have important practical implications.

Finally, I showed that the possibilities for experts–laypersons' interaction in well-ordered science are limited more generally due to a number of factors that undermine the motivation for actual public involvement. Kitcher does stress the necessity of public participation for learning about the public's needs and values and justifying values in the ethical conversation. Other elements of Kitcher's account, however, undermine motivation to widen public participation in science and science policy. On the one hand, the epistemic agents as Kitcher conceptualises them in general and the actual public as Kitcher describes it in particular seem unable to satisfy the demanding cognitive and moral conditions of the ideal conversation that serves as the basis for decision-making in well-ordered science. On the other hand, Kitcher stresses that the needs to be taken into account must be the real needs rather than the superficial needs that the public may be immediately aware of. This opens the possibility that it may be more effective to reconstruct real needs theoretically instead of learning about them from the public. Indeed, there are provisions for such a reconstruction in well-ordered science, for example, in order to have the interests of the future generations represented. The impression of dispensability of public participation is further reinforced when Kitcher outlines some definite positions that he expects well-ordered science to take. As a result, despite its democratic ethos, well-ordered science may not involve any democratic participation at all. This drastically weakens its appeal as an approach to the democratisation of science.

The three arguments I developed show important tensions within Kitcher's approach. Kitcher's social approach to science opens it to the possibility of extensive social reorganisation. Yet, the institutions and procedures of well-ordered science fail to take into account the nature of the relevant aspects of science—the nature of epistemic agents, the importance of participation, the value-laden character of concepts and frameworks, the relevance of the local context of application—that Kitcher's analysis of science convincingly demonstrates. Given the philosophical ambition and the influence of Kitcher's account, showing these problems is an important result. This result also has some further-reaching implications. As many of the social aspects of science that Kitcher describes are widely discussed in the socially oriented philosophy of science, showing what forms of the organisation of science they can or cannot support constitutes an important contribution. Most generally, the arguments I developed provide grounds to think that the kind of science *for* the people in democratic society that Kitcher envisages may not be possible without some participation *by* the people in the production of relevant knowledge.[120] This is an important result for the discussion of proposals that share Kitcher's aims.

The fifth and the sixth chapter discussed Longino's ideas as the basis for approaching the social organisation of science. I presented responses to some criticisms of Longino's account; most important among them is my defence of the social character of the interactions-based approach to objectivity against Justin Biddle's criticism. The central aim of the chapters was to outline an approach to the social organisation of science that avoids the problems I had identified in connection with Kitcher's account. The approach I defended is based on Longino's account of the social character of objectivity. Longino argues that objectivity is a community's characteristic. Communal interactive processes of criticism help to expose problematic values and biases that may be inherent in individuals' evidential claims and judgements about hypothesis acceptance. In this process of collective criticism, however,

[120] This formulation is inspired by Mark Brown's observation that

> [d]espite Kitcher's rejection of elite governance by scientists and politicians, and despite his persuasive argument against "objectivist" ethics, Kitcher appears more concerned with what government can do *for* people than with what people can do *by* government. (Mark Brown 2004, 83, italics in the original)

biases shared by everyone in community may remain invisible. Longino's account of objectivity thus attracts attention to the importance of inviting outsiders' perspectives into community dialogue. I suggested that it can therefore be used to support the participation of laypersons possessing relevant perspectives or local knowledge, in the processes of knowledge production in community that is built on Longino's principles of objectivity. This allows addressing the problems of Kitcher's model. First, this approach creates opportunities for utilising laypersons' perspectives and local knowledge. Second, it invites actual public participation not only in science policy decisions but also in research. Third, this approach does not set impossibly demanding requirements on the individual level, as it relies on social interactive processes for ensuring objectivity of community's judgements.

The connection between inclusive dialogue and objectivity is thus at the centre of Longino's approach. I suggested that it allows establishing connections with developments in science policy that are premised on a similar understanding of objectivity. As long as they are similar enough in this respect, experiments with public participation and democratisation in science policy can be used for learning how attempts to create more inclusive knowledge-producing practices may fare in practice. Longino's criteria of successful critical dialogue can be used in this discussion for understanding conditions for the possibility of productive inclusive knowledge-producing practices. Simultaneously, discussing particular scientific communities and science policy initiatives enables learning about forms that the general principles described by Longino may take in specific local contexts. Two cases studies developed on the basis of analyses of science policy developments demonstrated the possibilities of this approach for posing and answering questions about participatory scientific practices. The most significant result of these chapters was showing how an important development trend in science policy can be a source of material for philosophical analysis. Relatedly, I suggested that establishing a connection with these developments may be an important opening for bringing philosophical ideas into practice.

The approach I proposed can thus be seen as an invitation to philosophical exploration of specific developments in science policy that can be construed as reflecting certain ideas about inclusiveness and epistemic quality. In the thesis, I relied on analyses of past cases. The crucial task for the approach I propose is to demonstrate how it is possible to analyse ongoing science policy initiatives and, equally importantly, how it is possible to get involved with such initiatives at the stages of planning and development. The former is necessary in order to show that the programme I propose can be a productive way to advance knowledge about the social organisation of specific communities. The latter is important in order to show how the philosopher of science could play a contributory role in these developments, advancing a more socially applied philosophy of science.

In addition to this programme for exploring local initiatives and local communities I find it necessary to point out a more general question. This question concerns the ability of various science policy initiatives to sustain the kind of substantial public participation that makes them close enough to philosophical proposals. As noted before, such initiatives have been an important feature of contemporary science policy in many countries. Yet there may be concerns about the proportion of genuinely participatory initiatives and more or less formal exercises. As Hagendijk and Irwin (2006, 176) point out, the analysis of a number of initiatives shows that in this sphere "rhetoric is running well ahead of practice". If this is so, the perspectives of the approach I propose depend on whether there is willingness to make the practice catch up with the rhetoric. This is a crucial issue both for developments in the national context of a country and the international developments such as the participatory climate change research projects Marybeth Long Martello discusses. I do not believe that philosophy may be able to bring the necessary change in science and science policy on its own. As I previously pointed out, the identification of a predicament and the proposal of a solution are not guaranteed to prompt action. Nevertheless, I would like to hope that promoting what we have reasons to consider a more adequate understanding of science—an understanding according to which science is social in many important intertwined senses—may play some role in changing attitudes towards the question of the social organisation of science and science policy.

REFERENCES

Ackoff, Russell L. (1968). Operational research and national science policy. In: De Reuck, Anthony; Maurice Goldsmith and Julie Knight (Eds.), *Decision Making in National Science Policy*: A *Ciba Foundation and Science of Science Foundation Symposium,* pp. 84–91. Boston: Little, Brown and Company.

Adam, Matthias (2008). Promoting disinterestedness or making use of bias? Interests and moral obligation in commercialized research. In: Carrier, Martin; Don Howard and Janet Kourany (Eds.), *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited,* pp. 235–254. Pittsburgh: University of Pittsburgh Press.

Adam, Matthias; Martin Carrier and Torsten Wilholt (2006). How to serve the customer and still be truthful: Methodological characteristics of applied research. *Science and Public Policy*, Vol. 33, No. 6, pp. 435–444.

Aronson, Jay (2003). *Science, Democracy, and Truth* [sic] by Philip Kitcher; *Science, Technology, and Democracy* by Daniel Lee Kleinman. *Science, Technology, & Human Values,* Vol. 28, No.1, pp. 162–168.

Barad, Karen (2007). *Meeting the Universe Halfway: Quantum Physics and the Entanglement of Matter and Meaning.* Durham; London: Duke University Press.

Bereijo, Antonio (2011). The category of "applied science": An analysis of its justification from "information science" as design science. In: Gonzalez, Wenceslao J. (Ed.), *Scientific Realism and Democratic Society: The Philosophy of Philip Kitcher (Poznań Studies in the Philosophy of the Sciences and the Humanities* 101), pp. 327–350. Amsterdam; New York: Rodopi.

Bernal, J. D. (1967). *The Social Function of Science.* Cambridge (Mass.); London: The M.I.T. Press (first edition 1939).

Biddle, Justin B. (2006). *Socializing Science: On the Epistemic Significance of the Institutional Context of Science*. PhD Dissertation defended at the University of Notre Dame.

Biddle, Justin B. (2007). Lessons from the Vioxx debacle: What the privatization of science can teach us about social epistemology. *Social Epistemology: A Journal of Knowledge, Culture and Policy,* Vol. 21, No. 1, pp. 21–39.

Biddle, Justin. B. (2009). Advocates or unencumbered selves? On the role of Mill's political liberalism in Longino's contextual empiricism. *Philosophy of Science*, Vol. 76, No. 5, pp. 612–623.

Biddle, Justin B. (2012). Tragedy of the anticommons? Intellectual property and the sharing of scientific information. *Philosophy of Science.* Vol. 79, No. 5, pp. 821–832.

Biddle, Justin B. (2013a). Institutionalizing dissent: A proposal for an adversarial system of pharmaceutical research. *Kennedy Institute of Ethics Journal,* Vol. 23, No. 4, pp. 325–353.

Biddle, Justin B. (2013b). State of the field: Transient underdetermination and values in science. *Studies in History and Philosophy of Science Part A*, Vol. 44, No. 1, pp. 124–133.

Biddle, Justin B. (2014). Can patents prohibit research? On the social epistemology of patenting and licensing in science. *Studies in History and Philosophy of Science Part A,* Vol. 45, pp. 14–23.

Bluhm, Robyn (2012). Book review: Philip Kitcher's *Science in a Democratic Society*. *The Journal of Philosophy, Science & Law,* Vol. 12 (July 17, 2012). Available online at http://jpsl.org/archives/book-review-philip-kitchers-science-democratic-society/, accessed 17.02.2016.

Bora, Alfons (1998). Legal procedure and participation by the public: Germany's 1990 Genetic Engineering Act. *Law & Policy*, Vol. 20, No. 1, pp. 113–133.

British Columbia Ministry of Forests, Lands and Natural Resource Operations: Archaeology (n.d.). *Kwäday Dän Ts'ìnchi*. Available online at http://www.for.gov.bc.ca/archaeology/kwaday_dan_tsinchi/, accessed 12.09.2014.

Brown, James Robert (2000). Privatizing the university—the new tragedy of the commons. *Science,* Vol. 290, No. 5497, pp. 1701–1702.

Brown, James Robert (2002). Funding, objectivity and the socialization of medical research. *Science and Engineering Ethics,* Vol. 8, No. 3, pp. 295–308.

Brown, James Robert (2004). *Science, Truth, and Democracy* by Philip Kitcher. *The Journal of Philosophy,* Vol. 101, No. 11, pp. 599–606.

Brown, James Robert (2008a). The community of science®. In: Carrier, Martin; Don Howard and Janet Kourany (Eds.), *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*, pp. 189–216. Pittsburgh: University of Pittsburgh Press.

Brown, James Robert (2008b). Politics, method, and medical research. *Philosophy of Science,* Vol. 75, No. 5: Proceedings of the 2006 Biennial Meeting of the Philosophy of Science Association. Part II: Symposia Papers, pp. 756–766.

Brown, Mark B. (2004). The political philosophy of science policy. *Minerva: A Review of Science, Learning and Policy,* Vol. 42, No. 1, pp. 77–95.

Brown, Mark B. (2009). *Science in Democracy: Expertise, Institutions, and Representation.* Cambridge (Mass.); London: The MIT Press.

Brown, Mark B. (2013). Philip Kitcher, *Science in a Democratic Society*. *Minerva: A Review of Science, Learning and Policy*, Vol. 51, No. 3, pp. 389–397.

Brown, Matthew J. (2012). Philip Kitcher: *Science in a Democratic Society*. *Notre Dame Philosophical Reviews: An Electronic Journal,* 2012.03.07. Available online at http://ndpr.nd.edu/news/29284-science-in-a-democratic-society/, accessed 17.02.2016.

Bukharin, N. I. et al. (1971). *Science at the Cross Roads: Papers Presented to the International Congress of the History of Science and Technology Held in London from June 29th to July 3rd, 1931 by the Delegates of the U.S.S.R*. London: Frank Cass and Co LTD (first published 1931).

Büter, Anke (2010). Social objectivity and the problem of local epistemologies. *Analyse & Kritik: Zeitschrift für Sozialtheorie*, Vol. 32, No. 2, pp. 213–230.

Carrier, Martin (2004). Knowledge and control: On the bearing of epistemic values in applied science. In: Machamer, Peter and Gereon Wolters (Eds.), *Science, Values, and Objectivity,* pp. 275–293. Pittsburgh: University of Pittsburgh Press; Konstanz: Universitätsverlag Konstanz.

Carrier, Martin (2008a). Introduction: Science and the social. In: Carrier, Martin; Don Howard and Janet Kourany (Eds.), *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*, pp. 1–13. Pittsburgh: University of Pittsburgh Press.

Carrier, Martin (2008b). Science in the grip of the economy: On the epistemic impact of the commercialization of research. In: Carrier, Martin; Don Howard and Janet Kourany (Eds.), *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*, pp. 217–234. Pittsburgh: University of Pittsburgh Press.

Carrier, Martin (2012). Historical epistemology: On the diversity and change of epistemic values in science. *Berichte zur Wissenschaftsgeschichte,* Vol. 35, No. 3, pp. 239–251.

Cartieri, Francis and Angela Potochnik (2014). Toward philosophy of science's social engagement. *Erkenntnis: An International Journal of Scientific Philosophy*, Vol. 79, No. 5 (Supplement), pp. 901–916.

Cartwright, Nancy (2006). Well-ordered science: Evidence for use. *Philosophy of Science*, Vol. 73, No. 5: Proceedings of the 2004 Biennial Meeting of The Philosophy of Science Association. Part II: Symposia Papers, pp. 981–990.

Cartwright, Nancy (2012). Will this policy work for you? Predicting effectiveness better: How philosophy helps. *Philosophy of Science*, Vol. 79, No. 5, pp. 973–989.

Cartwright, Nancy (2013). Knowing what we are talking about: Why evidence doesn't always travel. *Evidence and Policy: A Journal of Research, Debate and Practice*, Vol. 9, No. 1, pp. 97–112.

Chang, Hasok (2011). The philosophical grammar of scientific practice. *International Studies in the Philosophy of Science*, Vol. 25, No. 3, pp. 205–221.

Churchman, C. West (1948). Statistics, pragmatics, induction. *Philosophy of Science,* Vol. 15, No. 3, pp. 249–268.

Clough, Sharyn (2013). Pragmatism and embodiment as resources for feminist interventions in science. *Contemporary Pragmatism*, Vol. 10, No. 2, pp. 121–134.

Collins, Harry M. (2010). *Tacit and Explicit Knowledge*, Chicago; London: University of Chicago Press.

Collins, Harry M. and Robert Evans (2002). The third wave of science studies: Studies of expertise and experience. *Social Studies of Science*, Vol. 32, No. 2, pp. 235–296.

Collins, Harry M. and Robert Evans (2003). King Canute meets the Beach Boys: Responses to "The third wave". *Social Studies of Science*, Vol. 33, No. 3, pp. 435–452.

Collins, Harry M. and Robert Evans (2007). *Rethinking Expertise*. Chicago; London: University of Chicago Press.

Daston, Lorraine (1992). Objectivity and the escape from perspective. *Social Studies of Science,* Vol. 22, No. 4, pp. 597–618.

Daston, Lorraine and Peter Galison (2010). *Objectivity*. New York: Zone Books (first published 2007).

Diéguez, Antonio (2011). Kitcher's modest realism: The reconceptualization of scientific objectivity. In: Gonzalez, Wenceslao J. (Ed.), *Scientific Realism and Democratic Society: The Philosophy of Philip Kitcher (Poznań Studies in the Philosophy of the Sciences and the Humanities* 101), pp. 141–169. Amsterdam; New York: Rodopi.

Douglas, Heather E. (2004). The irreducible complexity of objectivity. *Synthese: An International Journal for Epistemology, Methodology and Philosophy of Science,* Vol. 138, No. 3, pp. 453–473.

Douglas, Heather E. (2005). Inserting the public into science. In: Maasen, Sabine and Peter Weingart (Eds.), *Democratization of Expertise? Exploring Novel Forms of Scientific Advice in Political Decision-Making* (*Sociology of the Sciences Yearbook 24*), pp. 153–169. Dordrecht: Springer.

Douglas, Heather E. (2009). *Science, Policy, and the Value-Free Ideal.* Pittsburgh: University of Pittsburgh Press.

Douglas, Heather E. (2010). Engagement for progress: Applied philosophy of science in context. *Synthese: An International Journal for Epistemology, Methodology and Philosophy of Science,* Vol. 177, No. 3, pp. 317–335.

Douglas, Heather E. (2013a). Philip Kitcher: *Science in a Democratic Society*. *The British Journal for the Philosophy of Science,* Vol. 64, No. 4, pp. 901–905.

Douglas, Heather E. (2013b). The value of cognitive values. *Philosophy of Science*, Vol. 80, No. 5, pp. 796–806.

Downes, Stephen M. (1993). Socializing naturalized philosophy of science. *Philosophy of Science*, Vol. 60, No. 3, pp. 452–468.

Dupré, John (2004). Science and values and values in science: Comments on Philip Kitcher's *Science, Truth, and Democracy*. *Inquiry: An Interdisciplinary Journal of Philosophy,* Vol. 47, No. 5, pp. 505–514.

Eigi, Jaana (2012). Two Millian arguments: Using Helen Longino's approach to solve the problems Philip Kitcher targeted with his argument on freedom of inquiry. *Studia Philosophica Estonica*, Vol. 5, No. 1, pp. 44–63.

Eigi, Jaana; Pille Põiklik; Endla Lõhkivi and Katrin Velbaum (2014). Supervision and early career work experiences of Estonian humanities researchers under the conditions of project-based funding. *Higher Education Policy*, Vol. 27, No. 4, pp. 453–468.

Elliott, Kevin C. (2008). Scientific judgment and the limits of conflict-of-interest policies. *Accountability in Research: Policies and Quality Assurance*, Vol. 15, No. 1, pp. 1–29.

Fehr, Carla and Kathryn S. Plaisance (2010). Socially relevant philosophy of science: An introduction. *Synthese: An International Journal for Epistemology, Methodology and Philosophy of Science*, Vol. 177, No. 3, pp. 301–316.

Feyerabend, Paul (2002). *Against Method*. London; New York: Verso (first published 1975).

Fishkin, James S. (2011). *When the People Speak: Deliberative Democracy and Public Consultation*. New York: Oxford University Press (first published 2009).

Flory, James H. and Philip Kitcher (2004). Global health and the scientific research agenda. *Philosophy & Public Affairs,* Vol. 32, No. 1, pp. 36–65.

Fuller, Steve (1994). Mortgaging the farm to save the (sacred) cow. *Studies in History and Philosophy of Science Part A*, Vol. 25, No. 2, pp. 251–261.

Gaillard, Maxence (2013). The governance of "well-ordered science", from ideal conversation to public debate. *THEORIA: An International Journal For Theory, History And Foundations Of Science,* Vol. 28, No. 2, pp. 245–256.

Giere, Ronald N. (2003). A new program for philosophy of science? *Philosophy of Science,* Vol. 70, No. 1, pp. 15–21.

Goldman, Alvin I. (2002). *Pathways to Knowledge: Private and Public*. Oxford; New York: Oxford University Press.

Goldman, Alvin I. (2003). *Knowledge in a Social World.* Oxford: Clarendon Press (first published 1999).

Goldman, Alvin I. and James C. Cox (1996). Speech, truth, and the free market for ideas. *Legal Theory,* Vol. 2, No. 1, pp. 1–32.

Gonzalez, Wenceslao J. (2011a). From mathematics to social concern about science: Kitcher's philosophical approach. In: Gonzalez, Wenceslao J. (Ed.), *Scientific Realism and Democratic Society: The Philosophy of Philip Kitcher (Poznań Studies in the Philosophy of the Sciences and the Humanities 101*), pp. 11–93. Amsterdam; New York: Rodopi.

Gonzalez, Wenceslao J. (Ed.) (2011b). *Scientific Realism and Democratic Society: The Philosophy of Philip Kitcher (Poznań Studies in the Philosophy of the Sciences and the Humanities 101*). Amsterdam; New York: Rodopi.

Haack, Susan (1996). Science as social?—yes and no. In: Nelson, Lynn Hankinson and Jack Nelson (Eds.), *Feminism, Science, and the Philosophy of Science (Synthese Library: Studies in Epistemology, Logic, Methodology, and Philosophy of Science 256),* pp. 79–93. Dordrecht; Boston; London: Kluwer Academic Publishers.

Hacking, Ian (1994). *The Advancement of Science: Science Without Legend, Objectivity without Illusion* [sic] by Philip Kitcher. *Journal of Philosophy,* Vol. 91, No. 4, pp. 212–215.

Hagendijk, Rob and Alan Irwin (2006). Public deliberation and governance: Engaging with science and technology in contemporary Europe. *Minerva: A Review of Science, Learning and Policy,* Vol. 44, No. 2, pp. 167–184.

Hands, D. Wade (1995). Social epistemology meets the invisible hand: Kitcher on the advancement of science. *Dialogue: Canadian Philosophical Review/Revue canadienne de philosophie,* Vol. 34, No. 3, pp. 605–622.

Hands, D. Wade (1997). Caveat emptor: Economics and contemporary philosophy of science. *Philosophy of Science*. Vol. 64, Supplement: Proceedings of the 1996 Biennial Meetings of the Philosophy of Science Association. Part II: Symposia Papers, pp. S107–S116.

Hardwig, John (1985). Epistemic dependence. *The Journal of Philosophy,* Vol. 82, No. 7, pp. 335–349.

Hebda, Richard J.; Sheila Greer and Alexander Mackie (2011). *Teachings From Long Ago Person Found: Highlights from the Kwädąy Dän Ts'inchį Project: A Joint Project of the Royal BC Museum, Champagne and Aishihic First Nations and BC Archaeology Branch*. Available online at http://issuu.com/royalbcmuseum/docs/kdt_highlights/1?e=3254148/2399432, accessed 17.02.2016.

Howard, Don (2003). Two left turns make a right: On the curious political career of North American philosophy of science at midcentury. In: Hardcastle, Gary L. and Alan W. Richardson (Eds.), *Logical Empiricism in North America (Minnesota Studies in the Philosophy of Science 18),* pp. 25–93. Minneapolis; London: University of Minnesota Press.

Howard, Don (2009). Better red than dead—putting an end to the social irrelevance of postwar philosophy of science. *Science & Education: Contributions from History, Philosophy and Sociology of Science and Mathematics*, Vol. 18, No. 2, pp. 199–220.

Intemann, Kristen (2010). 25 years of feminist empiricism and standpoint theory: Where are we now? *Hypatia: A Journal of Feminist Philosophy,* Vol. 25, No. 4, pp. 778–796.

Intemann, Kristen and Inmaculada de Melo-Martín (2014). Addressing problems in profit-driven research: How can feminist conceptions of objectivity help? *European Journal for Philosophy of Science,* Vol. 4, No. 2, pp. 135–151.

Irzik, Gürol (2010). Why should philosophers of science pay attention to the commercialization of academic science? In: Suárez, Mauricio; Mauro Dorato and Miklós Rédei (Eds.), *EPSA Epistemology and Methodology of Science: Launch of the European Philosophy of Science Association*, pp. 129–138. Dordrecht: Springer.

Jamieson, Dale (2002). *Science, Truth, and Democracy* by Philip Kitcher. *Issues in Science and Technology,* Fall 2002, pp. 90–92.

Jasanoff, Sheila (2004). What inquiring minds *should* want to know. *Studies in History and Philosophy of Science Part A,* Vol. 35, No. 1, pp. 149–157.

Jasanoff, Sheila (2005). *Designs on Nature: Science and Democracy in Europe and the United States*. Princeton; Oxford: Princeton University Press.

Jasanoff, Sheila (2011). Cosmopolitan knowledge: Climate science and global civic epistemology. In: Dryzek, John S.; Richard B. Norgaard and David Schlosberg (Eds.), *The Oxford Handbook of Climate Change and Society,* pp. 129–143. Oxford; New York: Oxford University Press.

Joss, Simon (1999). Public participation in science and technology policy- and decision-making—ephemeral phenomenon or lasting change? *Science and Public Policy*, Vol. 26, No. 5, pp. 290–293.

Jukola, Saana (2016). The commercialization of research and the quest for the objectivity of science. *Foundations of Science*, Vol. 21, No. 1, pp. 89–103.

Kasanmoentalib, Soemini (1996). Science and values in risk assessment: The case of deliberate release of genetically engineered organisms. *Journal of Agricultural and Environmental Ethics*, Vol. 9, No. 1, pp. 42–60.

Kellert, Stephen H.; Helen E. Longino and C. Kenneth Waters (2006). Introduction: The pluralist stance. In: Kellert, Stephen H.; Helen E. Longino and C. Kenneth Waters (Eds.), *Scientific Pluralism (Minnesota Studies in the Philosophy of Science 19)*, pp. vii–xxix. Minneapolis; London: University of Minnesota Press.

Kincaid, Harold; John Dupré and Alison Wylie (2007a). Introduction. In: Kincaid, Harold; John Dupré and Alison Wylie (Eds.), *Value-Free Science? Ideals and Illusions,* pp. 3–24. Oxford; New York: Oxford University Press.

Kincaid, Harold; John Dupré and Alison Wylie (Eds.) (2007b). *Value-Free Science? Ideals and Illusions*. Oxford; New York: Oxford University Press.

King, Loren; Brandon Morgan-Olsen and James Wong (2016). Identifying difference, engaging dissent: What is at stake in democratizing knowledge? *Foundations of Science*, Vol. 21, No. 1, pp. 69–88.

Kitcher, Philip (1982). Genes. *The British Journal for the Philosophy of Science,* Vol. 33, No. 4, pp. 337–359.

Kitcher, Philip (1984a). Against the monism of the moment: A reply to Elliott Sober. *Philosophy of Science*, Vol. 51, No. 4, pp. 616–630.

Kitcher, Philip (1984b). Species. *Philosophy of Science*, Vol. 51, No. 2, pp. 308–333.

Kitcher, Philip (1985). *Vaulting Ambition: Sociobiology and the Quest for Human Nature.* Cambridge (Mass.); London: The MIT Press.

Kitcher, Philip (1989). Some puzzles about species. In: Ruse, Michael (Ed.), *What the Philosophy of Biology Is: Essays Dedicated to David Hull (Nijhoff International Philosophy Series 32),* pp. 183–208. Dordrecht: Reidel.

Kitcher, Philip (1990). The division of cognitive labor. *The Journal of Philosophy,* Vol. 87, No. 1, pp. 5–22.

Kitcher, Philip (1995a). *The Advancement of Science: Science Without Legend, Objectivity Without Illusions*. New York; Oxford: Oxford University Press (first published 1993).

Kitcher, Philip (1995b). Author's response. *Philosophy and Phenomenological Research,* Vol. 55, No. 3, pp. 653–673.

Kitcher, Philip (1995c). Précis of *The Advancement of Science*. *Philosophy and Phenomenological Research,* Vol. 55, No. 3, pp. 611–617.

Kitcher, Philip (1995d). Who's afraid of the Human Genome Project? *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association,* Vol. 1994, Volume Two: Symposia and Invited Papers, pp. 313–321.

Kitcher, Philip (1996). *Abusing Science: The Case Against Creationism.* Cambridge (Mass.); London: The MIT Press (first published in 1982).

Kitcher, Philip (1997a). An argument about free inquiry. *Noûs,* Vol. 31, No. 3, pp. 279–306.

Kitcher, Philip (1997b). *The Lives to Come: The Genetic Revolution and Human Possibilities*. New York: Simon and Schuster (first published 1996).

Kitcher, Philip (1999). Unification as a regulative ideal. *Perspectives on Science,* Vol. 7, No. 3, pp. 337–348.

Kitcher, Philip (2000a). A plea for science studies. In: Koertge, Noretta (Ed.), *A House Built on Sand: Exposing Postmodernist Myths about Science*, pp. 32–56. New York; Oxford: Oxford University Press (first published 1998).

Kitcher, Philip (2000b). Reviving the sociology of science. *Philosophy of Science*, Vol. 67, Supplement: Proceedings of the 1998 Biennial Meetings of the Philosophy of Science Association. Part II: Symposia Papers, pp. S33–S44.

Kitcher, Philip (2002a). Reply to Helen Longino. *Philosophy of Science,* Vol. 69, No. 4, pp. 569–572.

Kitcher, Philip (2002b). Social psychology and the theory of science. In: Carruthers, Peter; Stephen Stich and Michael Siegal (Eds.), *The Cognitive Basis of Science*, pp. 263–281. Cambridge: Cambridge University Press.

Kitcher, Philip (2002c). The third way: Reflections on Helen Longino's *The Fate of Knowledge*. *Philosophy of Science*, Vol. 69, No. 4, pp. 549–559.

Kitcher, Philip (2002d). Veritistic value and the project of social epistemology. *Philosophy and Phenomenological Research,* Vol. 64, No. 1, pp. 191–198.

Kitcher, Philip (2003). *Science, Truth, and Democracy*. Oxford; New York: Oxford University Press (first published 2001).

Kitcher, Philip (2006). Public knowledge and the difficulties of democracy. *Social Research: An International Quarterly,* Vol. 73, No. 4, pp. 1205–1224.

Kitcher, Philip (2007a). Does "race" have a future? *Philosophy & Public Affairs*, Vol. 35, No. 4, pp. 293–317.

Kitcher, Philip (2007b). Scientific research—who should govern? *NanoEthics: Studies of New and Emerging Technologies,* Vol. 1, No. 3, pp. 177–184.

Kitcher, Philip (2010). Two forms of blindness: On the need for both cultures. *Technology in Society: An International Journal*, Vol. 32, No. 1, pp. 40–48.

Kitcher, Philip (2011a). *The Ethical Project.* Cambridge (Mass.); London: Harvard University Press.

Kitcher, Philip (2011b). Public knowledge and its discontents. *Theory and Research in Education*, Vol. 9, No. 2, pp. 103–124.

Kitcher, Philip (2011c). *Science in a Democratic Society*. New York: Prometheus Books.

Kitcher, Philip and Nancy Cartwright (1996). Science and ethics: Reclaiming some neglected questions. *Perspectives on Science*, Vol. 4, No. 2, pp. 145–153.

Koskinen, Inkeri (2014). Critical subjects: Participatory research needs to make room for debate. *Philosophy of the Social Sciences,* Vol. 44, No. 6, pp. 733–751.

Kourany, Janet A. (2003a). A philosophy of science for the twenty-first century. *Philosophy of Science*, Vol. 70, No. 1, pp. 1–14.

Kourany, Janet A. (2003b). Reply to Giere. *Philosophy of Science*, Vol. 70, No. 1, pp. 22–26.

Kourany, Janet A. (2008a). Philosophy of science: A subject with a great future. *Philosophy of Science*, Vol. 75, No. 5: Proceedings of the 2006 Biennial Meeting of the Philosophy of Science Association. Part II: Symposia Papers, pp. 767–778.

Kourany, Janet A. (2008b). Replacing the ideal of value-free science. In: Carrier, Martin; Don Howard and Janet Kourany (Eds.), *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*, pp. 87–111. Pittsburgh: University of Pittsburgh Press.

Kourany, Janet A. (2010). *Philosophy of Science After Feminism*. Oxford; New York: Oxford University Press.

Kourany, Janet A. (2013). Meeting the challenges to socially responsible science: Reply to Brown, Lacey, and Potter. *Philosophical Studies: An International Journal for Philosophy in the Analytic Tradition*, Vol. 163, No. 1, pp. 93–103.

Kuhn, Thomas S. (1977). Objectivity, value judgement, and theory choice. In: Kuhn, Thomas S. *The Essential Tension: Selected Studies in Scientific Tradition and Change,* pp. 320–339. Chicago; London: University of Chicago Press.

Kuhn, Thomas S. (1996). *The Structure of Scientific Revolutions*. Chicago; London: University of Chicago Press (first edition published 1962).

Kukla, Rebecca (2012). "Author TBD": Radical collaboration in contemporary biomedical research. *Philosophy of Science,* Vol. 79, No. 5, pp. 845–858.

Kusch, Martin (2004). *Knowledge by Agreement: The Programme of Communitarian Epistemology*. Oxford; New York: Oxford University Press (first published 2002).

Kusch, Martin (2013, November). What are the functions of the social sciences in liberal democracy? Paper presented at the International Conference on The Special Role of Science in Liberal Democracy, November 21.–22., 2013, University of Copenhagen, Denmark.

Lacey, Hugh (1997). The dialectic of science and advanced technology: An alternative? *Democracy and Nature: The International Journal of Inclusive Democracy*, Vol. 4, No.1 pp. 34–53.

Lacey, Hugh (1999). *Is Science Value Free? Values and Scientific Understanding*. London; New York: Routledge.

Lacey, Hugh (2000). Seeds and the knowledge they embody. *Peace Review: A Journal of Social Justice*, Vol. 12, No. 4, pp. 563–569.

Lacey, Hugh (2003). Seeds and their sociocultural nexus. In: Figueroa, Robert and Sandra Harding (Eds.), *Science and Other Cultures: Issues in Philosophies of Science and Technology,* pp. 91–105. New York; London: Routledge.

Laudan, Larry (1984). *Science and Values: The Aims of Science and Their Role in Scientific Debate*. Berkeley: University of California Press.

Laudan, Larry (2004). The epistemic, the cognitive, and the social. In: Machamer, Peter and Gereon Wolters (Eds.), *Science, Values, and Objectivity,* pp. 14–23. Pittsburgh: University of Pittsburgh Press; Konstanz: Universitätsverlag Konstanz.

Lengwiler, Martin (2008). Participatory approaches in science and technology: Historical origins and current practices in critical perspective. *Science, Technology, & Human Values*, Vol. 33, No. 2, pp. 186–200.

Leplin, Jarrett (1994). Critical notice: Philip Kitcher's *The Advancement of Science: Science without Legend, Objectivity without Illusion* [sic]. *Philosophy of Science,* Vol. 61, No. 4, pp. 666–671.

Leuschner, Anna (2012). Pluralism and objectivity: Exposing and breaking a circle. *Studies in History and Philosophy of Science Part A,* Vol. 43, No. 1, pp. 191–198.

Longino, Helen E. (1984). Runaway science or science overrun? *Politics and the Life Sciences*, Vol. 3, No. 1, pp. 57–59.

Longino, Helen E. (1986). *Science Overrun: Threats to Freedom from External Control*. University of Houston Institute for Higher Education Law and Governance: Institute Monograph Series, Monograph 85–10. Available online at https://www.law.uh.edu/ihelg/monograph/85-10.pdf, accessed 18.02.2016.

Longino, Helen E. (1987a). Can there be a feminist science? *Hypatia: A Journal of Feminist Philosophy,* Vol. 2, No. 3, pp. 51–64.

Longino, Helen E. (1987b). What's really wrong with quantitative risk assessment? *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association,* Vol. 1986, Volume Two: Symposia and Invited Papers, pp. 376–383.

Longino, Helen E. (1989). Biological effects of low-level radiation: Values, dose–response models, risk estimates. *Synthese: An International Journal for Epistemology, Methodology and Philosophy of Science*, Vol. 81, No. 3, pp. 391–404.

Longino, Helen E. **(**1990a). Feminism and philosophy of science. *Journal of Social Philosophy*,

Vol. 21, No. 2–3, pp. 150–159.

Longino, Helen E. (1990b). *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*. Princeton: Princeton University Press.

Longino, Helen E. (1991). Multiplying subjects and the diffusion of power. *The Journal of Philosophy,* Vol. 88, No. 11, pp. 666–674.

Longino, Helen E. (1992a). Essential tensions—phase two: Feminist, philosophical, and social studies of science. In: McMullin, Ernan (Ed.), *The Social Dimensions of Science (Studies in Science and the Humanities from the Reilly Center for Science, Technology, and Values 3)*, pp. 198–216. Notre Dame (Ind.): University of Notre Dame Press.

Longino, Helen E. (1992b). Hard, soft, or satisfying. *Social Epistemology: A Journal of Knowledge, Culture and Policy*, Vol. 6, No. 3, pp. 281–287.

Longino, Helen E. (1992c). Knowledge, bodies, and values: Reproductive technologies and their scientific context. *Inquiry: An Interdisciplinary Journal of Philosophy*, Vol. 35, No. 3–4, pp. 323–340.

Longino, Helen E. (1993a). Subjects, power, and knowledge: Description and prescription in feminist philosophies of science. In: Alcoff, Linda and Elizabeth Potter (Eds.), *Feminist Epistemologies*, pp. 101–120. New York; London: Routledge.

Longino, Helen E. (1993b). Taking gender seriously in philosophy of science. In *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association,* Vol. 1992, Volume Two: Symposia and Invited Papers, pp. 333–340.

Longino, Helen E. (1994a). The fate of knowledge in social theories of science. In: Schmitt, Frederick F. (Ed.), *Socializing Epistemology: The Social Dimensions of Knowledge,* pp. 135–158. Lanham (Md.): Rowman and Littlefield Publishers.

Longino, Helen E. (1994b). Gender, sexuality research, and the flight from complexity. *Metaphilosophy*, Vol. 25, No. 4, pp. 285–292.

Longino, Helen E. (1994c). In search of feminist epistemology. *The Monist: An International Journal of General Philosophical Inquiry*, Vol. 77, No. 4, pp. 472–485.

Longino, Helen E. (1995). Gender, politics, and the theoretical virtues. *Synthese: An International Journal for Epistemology, Methodology and Philosophy of Science*, Vol. 104, No. 3, pp. 383–397.

Longino, Helen E. (1996). Cognitive and non-cognitive values in science: Rethinking the dichotomy. In: Nelson, Lynn Hankinson and Jack Nelson (Eds.), *Feminism, Science, and the Philosophy of Science (Synthese Library: Studies in Epistemology, Logic, Methodology, and Philosophy of Science 256),* pp. 39–58. Dordrecht; Boston; London: Kluwer Academic Publishers.

Longino, Helen E. (1997). Interpretation versus explanation in the critique of science. *Science in Context*, Vol. 10, No. 1, pp. 113–128.

Longino, Helen E. (2001). What do we measure when we measure aggression? *Studies in History and Philosophy of Science Part A*, Vol. 32, No. 4, pp. 685–704.

Longino, Helen E. (2002a). *The Fate of Knowledge*. Princeton; Oxford: Princeton University Press.

Longino, Helen E. (2002b). Reply to Philip Kitcher. *Philosophy of Science,* Vol. 69, No. 4, pp. 573–577.

Longino, Helen E. (2002c). Science and the common good: Thoughts on Philip Kitcher's *Science, Truth, and Democracy*. *Philosophy of Science,* Vol. 69, No. 4, pp. 560–568.

Longino, Helen E. (2003). Does *The Structure of Scientific Revolutions* permit a feminist revolution in science? In: Nickles, Thomas (Ed.), *Thomas Kuhn*, pp. 261–281. Cambridge; New York: Cambridge University Press.

Longino, Helen E. (2004). How values can be good for science. In: Machamer, Peter and Gereon Wolters (Eds.), *Science, Values, and Objectivity,* pp. 127–142. Pittsburgh: University of Pittsburgh Press; Konstanz: Universitätsverlag Konstanz.

Longino, Helen E. (2006). Philosophy of science after the social turn. In: Galavotti, Maria C. (Ed.), *Cambridge and Vienna: Frank P. Ramsey and the Vienna Circle (Vienna Circle Institute Yearbook 12),* pp. 167–177. Dordrecht: Springer.

Longino, Helen E. (2008a). Norms and naturalism: Comments on Miriam Solomon's *Social Empiricism*. *Perspectives on Science,* Vol. 16, No. 3, pp. 241–245.

Longino, Helen E. (2008b). Values, heuristics, and the politics of knowledge. In: Carrier, Martin; Don Howard and Janet Kourany (Eds.), *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*, pp. 68–86. Pittsburgh: University of Pittsburgh Press.

Longino, Helen E. (2013). *Studying Human Behavior: How Scientists Investigate Aggression & Sexuality*. Chicago; London: Chicago University Press.

Longino, Helen E. (2015). The social dimensions of scientific knowledge. In: Zalta, Edward N. (Ed.), *The Stanford Encyclopedia of Philosophy (Spring 2015 Edition),* http://plato.stanford.edu/archives/spr2015/entries/scientific-knowledge-social/, accessed 17.12.2015.

Longino, Helen E. and Kathleen Lennon (1997). Feminist epistemology as a local epistemology. *Aristotelian Society Supplementary Volume,* Vol. 71, pp. 19–54.

Lõhkivi, Endla; Katrin Velbaum and Jaana Eigi (2012). Epistemic injustice in research evaluation: A cultural analysis of the humanities and physics in Estonia. *Studia Philosophica Estonica,* Vol. 5. No. 2, pp. 108–132.

Machamer, Peter and Gereon Wolters (2004). Introduction: Science, values, and objectivity. In: Machamer, Peter and Gereon Wolters (Eds.), *Science, Values, and Objectivity,* pp.

1–13. Pittsburgh: University of Pittsburgh Press; Konstanz: Universitätsverlag Konstanz.

Majeske, Rachel Ankeny (1996). Transforming objectivity to promote equity in transplant candidate selection. *Theoretical Medicine and Bioethics: Philosophy of Medical Research and Practice,* Vol. 17, No. 1, pp. 45–59.

Martello, Marybeth Long (2004). Global change science and the Arctic citizen. *Science and Public Policy,* Vol. 31, No. 2, pp. 107–115.

Martello, Marybeth Long (2008). Arctic indigenous peoples as representations and representatives of climate change. *Social Studies of Science,* Vol. 38, No. 3, pp. 351–376.

Matheson, Carl (1996). Critical notice: Philip Kitcher, *The Advancement of Science: Science without Legend, Objectivity without Illusions*. *Canadian Journal of Philosophy,* Vol. 26, No. 3, pp. 463–489.

McMullin, Ernan (1983). Values in science. *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, Vol. 1982, Volume Two: Symposia and Invited Papers, pp. 3–28.

McMullin, Ernan (1992). Introduction: The social dimensions of science. In: McMullin, Ernan (Ed.), *The Social Dimensions of Science (Studies in Science and the Humanities from the Reilly Center for Science, Technology, and Values 3)*, pp. 1–26. Notre Dame (Ind.): University of Notre Dame Press.

Mill, John Stuart (1978). *On Liberty*. Indianapolis: Hackett Publishing Company, Inc. (first published 1859).

Mirowski, Philip (1995). Philip Kitcher's *Advancement of Science*: A review article. *Review of Political Economy,* Vol. 7, No. 2, pp. 227–241.

Mirowski, Philip (1996). The economic consequences of Philip Kitcher. *Social Epistemology: A Journal of Knowledge, Culture and Policy,* Vol. 10, No. 2, pp. 153–169.

Mirowski, Philip (2004). The scientific dimensions of social knowledge and their distant echoes in 20th-century American philosophy of science. *Studies in History and Philosophy of Science Part A*, Vol. 35, No. 2, pp. 283–326.

Morrison, Margaret (2008). Values and their intersection: Reduction as methodology and ideology. In: Carrier, Martin, Don Howard and Janet Kourany (Eds.), *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*, pp. 45–67. Pittsburgh: University of Pittsburgh Press.

Nicholas, George P. and Alison Wylie (2009). Archaeological finds: Legacies of appropriation, modes of response. In: Young, James O. and Conrad G. Brunk (Eds.), *The Ethics of Cultural Appropriation*, pp. 11–54. Malden (Mass.); Oxford; Chichester: Wiley-Blackwell.

Niiniluoto, Ilkka (1984). Finalization, applied science, and science policy. In: Niiniluoto, Ilkka, *Is Science Progressive? (Synthese Library: Studies in Epistemology, Logic, Methodology, and Philosophy of Science 177),* pp. 226–243. Dordrecht: Springer: Science+Business Media B.V.

Perdomo, Inmaculada (2011). The characterization of epistemology in Philip Kitcher: A critical reflection from new empiricism. In: Gonzalez, Wenceslao J. (Ed.), *Scientific Realism and Democratic Society: The Philosophy of Philip Kitcher (Poznań Studies in the Philosophy of the Sciences and the Humanities 101*), pp. 113–138. Amsterdam; New York: Rodopi.

Pfetsch, Frank R. (1979). The “finalization” debate in Germany: Some comments and explanations. *Social Studies of Science,* Vol. 9, No. 1, pp. 115–124.

Pinto, Manuela Fernández (2014). Philosophy of science for globalized privatization: Uncovering some limitations of critical contextual empiricism. *Studies in History and Philosophy of Science Part A*, Vol. 47, pp. 10–17.

Pinto, Manuela Fernández (2015). Commercialization and the limits of well-ordered science. *Perspectives on Science*, Vol. 23, No. 2, pp. 173–191.

Polanyi, Michael (2002). *Personal Knowledge: Towards a Post-Critical Philosophy*. London; New York: Routledge (first published 1958).

Radder, Hans (2013). Exploring philosophical issues in the patenting of scientific and technological inventions. *Philosophy & Technology*, Vol. 26, No. 3, pp. 283–300.

Reiss, Julian (2010). In favour of a Millian proposal to reform biomedical research. *Synthese: An International Journal for Epistemology, Methodology and Philosophy of Science*, Vol. 177, No. 3, pp. 427–447.

Reiss, Julian and Philip Kitcher (2009). Biomedical research, neglected diseases, and well-ordered science. *THEORIA: An International Journal for Theory, History and Foundations of Science*, Vol. 24, No. 3, pp. 263–282.

Richardson, Alan W. (2002). Engineering philosophy of science: American pragmatism and logical empiricism in the 1930s. *Philosophy of Science*, Vol. 69, No. S3 (Supplementary Volume), pp. S36–S47.

Richardson, Henry S. (2014). Review: Philip Kitcher, *Science in a Democratic Society*. *Kennedy Institute of Ethics Journal*, Vol. 24, No. 1, pp. E106–E109.

Richardson, Sarah S. (2010). Feminist philosophy of science: History, contributions, and challenges. *Synthese: An International Journal for Epistemology, Methodology and Philosophy of Science*, Vol. 177, No. 3, pp. 337–362.

Robins, Rosemary (2001). Overburdening risk: Policy frameworks and the public uptake of genetic technology. *Public Understanding of Science*, Vol. 10, No. 1, pp. 19–36.

Rolin, Kristina (2011). Contextualism in feminist epistemology and philosophy of science. In: Grasswick, Heidi (Ed.), *Feminist Epistemology and Philosophy of Science: Power in Knowledge*, pp. 25–44. Dordrecht: Springer.

Rolin, Kristina (2015). Values in science: The case of scientific collaboration. *Philosophy of Science*, Vol. 82, No. 2, pp. 157–177.

Rolin, Kristina (2016). Values, standpoints, and scientific/intellectual movements. *Studies in History and Philosophy of Science Part A*, Vol. 56, pp. 11–19.

Rouse, Joseph (1990). *Knowledge and Power: Toward a Political Philosophy of Science*. Ithaka; London: Cornell University Press (first published 1987).

Rouse, Joseph (1996). *Engaging Science: How to Understand Its Practices Philosophically*. Ithaka; London: Cornell University Press.

Rouse, Joseph (1998). New philosophies of science in North America—twenty years later. *Journal for General Philosophy of Science*, Vol. 29, No. 1, pp. 71–122.

Rouse, Joseph (1999). Understanding scientific practices: Cultural studies of science as a philosophical program. In: Biagioli, Mario (Ed.), *Science Studies Reader*, pp. 442–456. New York; London: Routledge.

Rouse, Joseph (2014). Scientific practice and the scientific image. In: Soler, Léna; Sjoerd Zwart; Michael Lynch and Vincent Israel-Jost (Eds.), *Science After the Practice Turn* *in the Philosophy, History, and Social Studies of Science*. pp. 277–294. New York; London: Routledge.

Rudner, Richard (1953). The scientist *qua* scientist makes value judgments. *Philosophy of Science,* Vol. 20, No. 1, pp. 1–6.

Schmitt, Frederick F. (1994). Socializing epistemology: An introduction through two sample issues. In: Schmitt, Frederick F. (Ed.), *Socializing Epistemology: The Social Dimensions of Knowledge,* pp. 1–28. Lanham (Md): Rowman and Littlefield.

Shapin, Steven (1995). Here and everywhere: Sociology of scientific knowledge. *Annual Review of Sociology,* Vol. 21, pp. 289–321.

Sheehan, Helena (2007). Marxism and science studies: A sweep through the decades. *International Studies in the Philosophy of Science*, Vol. 21, No. 2, pp. 197–210.

Shils, Edward (Ed.) (1968). *Criteria for Scientific development: Public Policy and National Goals: A Selection of Articles from Minerva*. Cambridge (Mass.); London: The MIT Press.

Society for American Archaeology (1996). *Principles of Archaeological Ethics*. Available online at http://www.saa.org/AbouttheSociety/PrinciplesofArchaeologicalEthics/tabid/203/Default.aspx, accessed 12.09.2014.

Soler, Léna; Sjoerd Zwart; Vincent Israel-Jost and Michael Lynch (2014). Introduction. In: Soler, Léna; Sjoerd Zwart; Michael Lynch and Vincent Israel-Jost (Eds.), *Science After the Practice Turn in the Philosophy, History, and Social Studies of Science,* pp. 1–43. New York; London: Routledge.

Solomon, Miriam (1995). Legend naturalism and scientific progress: An essay on Philip Kitcher's *The Advancement of Science*. *Studies in History and Philosophy of Science Part A,* Vol. 26, No. 2, pp. 205–218.

Solomon, Miriam (2001). *Social Empiricism.* Cambridge (Mass.); London: The MIT Press.

Solomon, Miriam (2006). *Groupthink* versus *The Wisdom of Crowds*: The social epistemology of deliberation and dissent. *The Southern Journal of Philosophy*, Vol. 44, No. S1 (Supplement), pp. 28–42.

Solomon, Miriam and Alan W. Richardson (2005). A critical context for Longino's critical contextual empiricism. *Studies in History and Philosophy of Science Part A,* Vol. 36, No. 1, pp. 211–222.

Tollefsen, Deborah Perron (2006). Group deliberation, social cohesion, and scientific teamwork: Is there room for dissent? *Episteme: A Journal of Individual and Social Epistemology*, Vol. 3, No. 1–2, pp. 37–51.

Turner, Stephen (2003). The third science war. *Social Studies of Science,* Vol. 33, No. 4, pp. 581–611.

Uebel, Thomas (2005). Political philosophy of science in logical empiricism: The left Vienna Circle. *Studies in History and Philosophy of Science Part A*, Vol. 36, No. 4, pp. 754–773.

Van Bouwel, Jeroen (2012). What is there beyond Mertonian and dollar green science? Exploring the contours of epistemic democracy. In: Vanderbeeken, Robrecht; Frederik Le Roy; Christel Stalpaert and Diederik Aerts (Eds.), *Drunk on Capitalism: An Interdisciplinary Reflection on Market Economy, Art and Science (Einstein Meets Magritte: An Interdisciplinary Reflection on Science, Nature, Art, Human Action and Society 11),* pp. 35–48. Dordrecht: Springer.

Van Bouwel, Jeroen (2015). Towards democratic models of science: Exploring the case of scientific pluralism. *Perspectives on Science,* Vol. 23, No. 2, pp. 149–172.

Vihalemm, Rein (2011). Towards a practical realist philosophy of science. *Baltic Journal of European Studies: Journal of Tallinn University of Technology*, Vol. 1, No. 1, pp. 46–60.

Vihalemm, Rein (2012). Practical realism: Against standard scientific realism and anti-realism. *Studia Philosophica Estonica*, Vol. 5, No. 2, pp. 7–22.

Wallington, Tabatha J. and Susan A Moore (2005). Ecology, values, and objectivity: Advancing the debate. *BioScience*, Vol. 55, No. 10, pp. 873–878.

Whyte, Kyle Powys and Robert P. Crease (2010). Trust, expertise, and the philosophy of science. *Synthese: An International Journal for Epistemology, Methodology and Philosophy of Science*, Vol. 177, No. 3, pp. 411–425.

Wilholt, Torsten (2006). Design rules: Industrial research and epistemic merit. *Philosophy of Science*, Vol. 73, No. 1, pp. 66–89.

Wilholt, Torsten (2009). Bias and values in scientific research. *Studies in History and Philosophy of Science Part A*, Vol. 40, No. 1, pp. 92–101.

Wilholt, Torsten (2014). Philip Kitcher: *Science in a Democratic Society*. *Philosophy of Science*, Vol. 81, No. 1, pp. 165–171.

Winsberg, Eric; Bryce Huebner and Rebecca Kukla (2014). Accountability and values in radically collaborative research. *Studies in History and Philosophy of Science Part A,* Vol. 46, pp. 16–23.

Wray, K. Brad (2001). Science, biases, and the threat of global pessimism. *Philosophy of Science,* Vol. 68, No. S3: Supplement: Proceedings of the 2000 Biennial Meeting of the Philosophy of Science Association. Part I: Contributed Papers, pp. S467–S478.

Wylie, Alison (1996). Ethical dilemmas in archaeological practice: Looting, repatriation, stewardship, and the (trans)formation of disciplinary identity. *Perspectives on Science,* Vol. 4, No. 2, pp. 154–194.

Wylie, Alison (1999). Science, conservation, and stewardship: Evolving codes of conduct in archaeology. *Science and Engineering Ethics,* Vol. 5, No. 3, pp. 319–336.

Wylie, Alison (2000). Some reflections on the work of the SAA Committee for Ethics in Archaeology. *Canadian Journal of Archaeology,* Vol. 24, No. 2, pp. 151–156.

Wylie, Alison (2005). The promise and perils of an ethic of stewardship. In: Meskell, Lynn and Peter Pells (Eds.), *Embedding Ethics,* pp. 47–68. Oxford; New York: Berg.

Wylie, Alison (2014). Community-based collaborative archaeology. In: Cartwright, Nancy and Eleonora Montuschi (Eds.), *Philosophy of Social Science: A New Introduction*, pp. 68–82. Oxford: Oxford University Press.

Wylie, Alison (2015). A plurality of pluralisms: Collaborative practice in archaeology. In: Padovani, Flavia; Alan Richardson and Jonathan Y. Tsou (Eds.), *Objectivity in Science: New Perspectives from Science and Technology Studies (Boston Studies in the Philosophy and History of Science 310)*, pp. 189–210. Cham: Springer.

Wynne, Brian (1989). Sheepfarming after Chernobyl: A case study in communicating scientific information. *Environment: Science and Policy for Sustainable Development,* Vol. 31, No. 2, pp. 10–39.

Wynne, Brian (1992). Misunderstood misunderstanding: Social identities and public uptake of science. *Public Understanding of Science*, Vol. 1, No. 3, pp. 281–304.

Wynne, Brian (1996). May the sheep safely graze? A reflexive view of the expert–lay knowledge divide. In: Lash, Scott; Bronislaw Szerszynski and Brian Wynne (Eds.), *Risk, Environment and Modernity: Towards a New Ecology,* pp. 44–83. London: Sage.

Wynne, Brian (2003). Seasick on the third wave? Subverting the hegemony of propositionalism: Response to Collins & Evans (2002). *Social Studies of Science,* Vol. 33, No. 3, pp. 401–417.

Yoshida, Kei (2012). Re-politicising philosophy of science: A continuing challenge for social epistemology. *Social Epistemology: A Journal of Knowledge, Culture and Policy*, Vol. 26, No. 3–4, pp. 365–378.

VÄITEKIRJA EESTIKEELNE KOKKUVÕTE

Teaduse sotsiaalne korraldus kui teadusfilosoofiline probleem

Märkimisväärset nähtust viimaste aastakümnete teadusfilosoofias kujutavad endast kirjutised teaduse sotsiaalse korralduse kohta. Sel teemal kirjutavaid filosoofe ühendab see, et nad esmalt näitavad, kuidas teadus on mingis olulises mõttes sotsiaalne. Säärast teaduse sotsiaalset käsitlust kasutatakse omakorda alusena, et esitada tähelepanekuid selle kohta, milline peaks olema teadlaskonna sotsiaalne korraldus või kuidas teadus peaks olema ühiskonnas korraldatud. Väitekirja eesmärk on vaadelda mitmeid küsimusi, mis on seotud selle teema kohta käivate argumentidega, kriitiliselt analüüsida ühte mõjukat argumenti sellel alal – Philip Kitcheri hästikorraldatud teaduse mudelit – ning kirjeldada ja kaitsta alternatiivset lähenemist teaduse sotsiaalse korralduse küsimusele.

Väitekirja põhilisteks tulemusteks on kolm üksteisega seotud argumenti. Kõigepealt arutan ma, milliseid faktoreid peaks arvesse võtma säärase teaduse sotsiaalset korraldust käsitleva argumendi arendamisel. Ma väidan, et selle juures ei saa lähtuda üksnes episteemilistest kaalutlustest – strateegiad mitte-episteemiliste kaalutluste kui mittevajalike üle arutlemise välistamiseks on problemaatilised. Ma teen järelduse, et tuleb arvestada ka teaduse korraldamise vormi praktilist mõju ühiskonnale. Kitcheri hästikorraldatud teaduse mudel – üks tähtsamaid teaduse korraldamisele pühendatud käsitlusi – on minu teise argumendi teema. Ma kirjeldan Kitcheri argumendi arengut ja näitan, kuidas selle uusim versioon tõepoolest arvestab teaduse eri sotsiaalseid aspekte ja selle võimet mõjutada ühiskonda. Vaatamata sellele väidan ma, et hästikorraldatud teaduse mudel on mõnes suhtes problemaatiline. Ma toon esile probleemid, mis on seotud ebarealistlike ootustega ekspertidele, ekspertide teadmiste võimalike puudujääkidega ja avalikkuse puuduliku kaasamisega eeldatavasti demokraatlikus hästikorraldatud teaduses. Ma näitan, et need probleemid õõnestavad Kitcheri lähenemise sisemist kooskõla ning lootust, et see saab täita oma funktsioone demokraatliku ühiskonna teenistuses. Kolmanda argumendi eesmärk on kirjeldada alternatiivset lähenemist teaduse korraldamisele. Selle lähenemise aluseks on Helen Longino argument mitmekesiseid relevantseid perspektiive kaasavast dialoogist kui objektiivsuse alusest. Kõigepealt näitan ma, et Longino argumendil põhinev lähenemine võimaldab vältida Kitcheri mudeli probleeme – see toetab avalikkuse esindajate (*laypersons*) relevantsete perspektiivide kaasamist teadmise loomisel ja võimaldab selle kaudu täita võimalikke lünki ekspertide teadmistes ning soodustada nende objektiivsust. Longino ideedel põhineva lähenemise teine tähtis eelis on võimalus luua seoseid oluliste arengutendentsidega nüüdisaegses teaduspoliitikas – teadusfilosoofia ja teaduspoliitika suhted pole veel filosoofias piisavat tähelepanu saanud. Ma näitan, et teaduspoliitikas eksisteerib Longino omaga sarnane arusaam objektiivsuse ja kaasava dialoogi seostest. Praegu toimuvaid teaduspoliitika demokratiseerimise katseid võib vaadelda kui sellest arusaamast inspireerituid. See omakorda võimaldab sääraste katsete analüüside kasutamist mõistmaks, kuidas teaduskorralduse muutmisega eksperimenteerimine võib praktikas areneda ja millised faktorid võivad seda protsessi mõjutada.

Väitekirja **esimene peatükk** tutvustab teaduse korraldamise teemalisi kirjutisi, kirjeldab nende konteksti ja arutab, milliseid kaalutlusi peaks arvesse võtma selle teema üle arutlemisel. Käsitlused, mis lisaks teaduse sotsiaalsete aspektide analüüsile pakuvad ka tähelepanekuid selle sotsiaalse korralduse kohta, on tänapäevase teadusfilosoofia üha nähtavam osa. Sääraste argumentide tutvustamisel kirjeldatakse neid tihti kui uuenduslikke ja hädavajalikke. Üheks sellise tendentsi põhjuseks võib pidada teadusfilosoofia ajaloolist arengut. Kuigi on võimalik osutada ka varasematele teaduse sotsiaalsetele aspektidele keskenduvatele argumentidele (algupärase Viini Ringi huvi teaduse sotsiaalse rolli vastu on neist kõige tähtsam näide), on valdav osa teadusfilosoofilist uurimistööd pärast Teist

Maailmasõda olnud mittesotsiaalne, puhtalt epistemoloogilistele teemadele pühendatud. Seda, miks pööre sotsiaalsete teemade poole on tänapäeval hädavajalik, võib omakorda seletada teadusfilosoofia probleemsituatsiooniga. Traditsiooniline teaduspilt seisab silmitsi oluliste väljakutsetega, mille ma jagan peatükis kahte rühma. Üks neist ühendab teaduse mittefilosoofilisi käsitlusi, mis näitavad, et teadus on sotsiaalne, – teadusajalugu, teaduse sotsiaalsed uuringud ning teaduse feministlikud uuringud on mõned näited. Teist liiki väljakutsed on seotud oluliste muutustega teaduses endas – rakendusteadus, teaduseksperdid riigiaparaadi osana ning teaduse privatiseerimine ja kommertsialiseerumine on üha märgatavamad arengutendentsid. Teaduse filosoofilised käsitlused, mis on mõeldud vastuseks neile väljakutsetele, võivad omakorda toetada tähelepanekuid teaduse sotsiaalse korralduse kohta – kui on kindel, et teaduse teatud sotsiaalsed aspektid on olulised, on teadusfilosoofia loomulik ülesanne näidata, milline lähenemine neile aspektidele oleks teaduse ja teadmise loomise jaoks optimaalne.

Peatüki teises osas näitan ma aga, et üksnes episteemiliste kaalutluste arvessevõtmine pole piisav. James Robert Brown väidab, et tema argument patentide ning erarahastuse kaotamise kohta biomeditsiinis on puhtmetodoloogiline ning sotsiaalsete väärtuste kaalumine on selle omaksvõtmise jaoks irrelevantne. Ma vaatlen kolme strateegiat, mida saab kasutada, et põhjendada sotsiaalsete väärtuste üle arutlemise mittevajalikkust – teaduse ja tehnoloogia vastandamist, teadmise edendamise ülima väärtuse kuulutamist ning teadusliku teadmise eeldatavat neutraalsust väärtuste suhtes – ning näitan, et need on sügavalt problemaatilised. Teadusfilosoofia pööre teaduse materiaalsete ja eksperimentaalsete praktikate uurimisele (Joseph Rouse'i käsitlus on üks näide) seab samuti kahtluse alla teaduse episteemilise ja praktilise poole eristuse. Ma teen järelduse, et teaduse sotsiaalset korraldust arutades on vajalik kaaluda nii selle tagajärgi teadmise loomisele kui selle poliitilisi, eetilisi ja muid tagajärgi ühiskonnale. See omakorda tõstatab olulise probleemi eri liiki tagajärgede võrdlemisest ja kaalumisest, mida ma tutvustan Janet Kourany sotsiaalselt vastutustundliku teadusfilosoofia mudeli näitel.

Väitekirja **teise peatüki** eesmärk on kirjeldada, mida võib tähendada teaduse sotsiaalne käsitlus ja kuidas erinevad käsitlused võivad omakorda toetada erinevaid väiteid teaduse sotsiaalse korralduse kohta. Peatükis jagan ma need käsitlused kaheks – need, mis keskenduvad sotsiaalsete väärtuste rollile teaduses, ning need, mille tähelepanu keskpunktis on sotsiaalsed suhted teadlaskonnas ning teaduse sotsiaalsed institutsioonid. Teaduse sotsiaalsete käsitluste esitamisel alustan ma nende vastandi kirjeldamisest, ehk sellest, mida võib pidada traditsiooniliseks mittesotsiaalseks käsitluseks. Väärtuste puhul on see väärtustevabaduse ideaal, mille järgi mängivad teaduse episteemilistes praktikates rolli üksnes episteemilised väärtused; sotsiaalsete väärtuste roll on piiratud teaduse perifeeriaga, näiteks uurimistöö planeerimise või teaduseetika küsimustega. Väärtustevabaduse ideaali vastu võivad sotsiaalselt meelestatud teadusfilosoofid väita, et episteemilised väärtused on episteemiliste valikute jaoks ebapiisavad ning sotsiaalsed faktorid võivad nende kõrval olulised olla (Martin Carrier; Hugh Lacey) või et episteemilised väärtused ise peegeldavad teatud sotsiaalseid eelistusi (Longino). Teine võimalus väärtustevabaduse ideaali kummutamiseks on näidata, et ka teaduse sisemistes episteemilistes praktikates on sotsiaalsetel väärtustel oma osa – näiteks tõendusmaterjali piisava hulga üle otsustamine sõltub sellest, kui kõrge on võimaliku vea hind (Heather Douglas). Sotsiaalsete suhete ja institutsioonide puhul on traditsiooniliseks mittesotsiaalseks positsiooniks see, et kognitiivsed praktikad toimivad põhimõtteliselt indiviidi tasemel. Vastuväitena sellele võivad teaduse sotsiaalset loomust pooldavad filosoofid juhtida tähelepanu kollektiivsetele kognitiivsetele praktikatele (Alvin Goldman) või näidata teaduse kollektiivse, koostööl ja konkurentsil põhineva loomuse episteemilist tähtsust (Susan Haack). On võimalik ka radikaalsem positsioon, mille järgi on teaduse teatud kesksed omadused, nagu ratsionaalsus, ise sotsiaalsed ja neist peaks rääkima kogukonna tasemel (Miriam Solomon).

Argumente, mis näitavad sotsiaalsete väärtuste rolli teaduses, saab omakorda kasutada väitmaks, et on võimalik teistsugustel väärtustel põhinev teaduse sotsiaalne korraldus (Lacey). Neid argumente võib kasutada ka selleks, et nõuda teaduslike institutsioonide tegevuses väärtustevabaduse ideaalist loobumist ja võimaluste loomist aruteluks väärtuste üle (Douglas). Argumendid, mis näitavad sotsiaalsete praktikate tähtsust teaduses, juhivad tähelepanu neid praktikaid võimaldavatele tingimustele. Neist argumentidest lähtudes võidakse arutada näiteks teaduse rahastamise, tulemuste patenteerimise jms mõju üle uurimistöö suundade valikutele teadlaskogukonnas või omavahelisele koostööle ja infovahetusele ning pakkuda välja episteemiliselt paremaid teaduskorralduse vorme. Nende võimaluste vaatlemise tulemusena väidan ma aga, et keskendumine üksnes episteemiliste eesmärkide saavutamise optimeerimisele teaduse sotsiaalse ümberkorraldamise kaudu võib olla problemaatiline, kui ignoreeritakse seda, kuidas sotsiaalsed väärtused võivad nende eesmärkide konstitueerimist mõjutada. Seega rõhutan ma, et oluline on arvesse võtta teaduse sotsiaalsuse mõlemat dimensiooni.

Peatüki lõpus tutvustan ma Longino käsitlust kui säärase tervikliku lähenemise näidet. Longino argumendi lähtepunktiks on alamääratuse probleem – loogiline lünk tõendusmaterjali ja hüpoteesi vahel (tõendusmaterjal kirjeldab üksikuid vaadeldavaid sündmusi; hüpoteesi eesmärk on kirjeldada nende sündmuste aluseks olevaid varjatud struktuure või seadusi). Tõendusmaterjali ja hüpoteesi suhestamisel peavad paratamatult mängima rolli teatud taustaeeldused, mille sekka võivad kuuluda ka isiklikud eelistused ja väärtused. Longino näitab, et sääraste sotsiaalsete faktorite problemaatilist mõju tõendamise objektiivsusele on võimalik ennetada tänu teaduse sotsiaalsele – interaktiivsele – loomusele. Indiviidi panus saab teaduskogukonna tunnustuse alles pärast seda, kui kogukond tervikuna on saanud tänu avalikule diskussioonile võimaluse tuua välja tema töö problemaatilised eeldused. Selline transformatiivne kriitika on teadlaskonna objektiivsuse tagatis. Longino argument seob seega kokku sotsiaalsuse eri tähendused – see võimaldab näha nii sotsiaalsete väärtuste rolli kui sotsiaalseid tingimusi, mis võimaldavad väärtusi kontrolli all hoida.

Kolmandas peatükis analüüsin ma Kitcheri teaduskäsitluse arengut ja näitan, kuidas sotsiaalsete faktorite mõju tunnistamine tema töös on süvenenud, võimaldades üha kaugemale ulatavaid tähelepanekuid teaduse korraldamise kohta. Nagu eelminegi, demonstreerib ka see peatükk, kuidas rõhuasetus erinevatele teaduse sotsiaalsuse aspektidele võimaldab erinevaid tähelepanekuid teaduse sotsiaalse korralduse kohta. Kitcheri esimene üldteadusfilosoofia raamat *The Advancement of Science: Science Without Legend, Objectivity Without Illusions* (esimene trükk 1993) seab eesmärgiks arendada välja teaduse käsitlus, mis on samaaegselt realistlik ja ratsionalistlik ning teaduse sotsiaalset mõõdet arvestav. Sotsiaalsete faktorite roll, mida Kitcher tunnistab, on aga piiratud sotsiaalsete interaktsioonide ning tööjaotusega teadlaskonnas. Kitcher näitab, kuidas indiviidid, keda iseloomustab episteemiliste ja mitte-episteemiliste motiivide segu, võivad kogukonna tasemel jõuda parema tööjaotuseni eri meetodite vahel, kui suudaks ainult episteemilistest kaalutlustest lähtuv kogukond. Samal ajal eitab Kitcher sotsiaalsete väärtuste rolli teadlaste otsustes või teaduse eesmärkide formuleerimisel: teaduse eesmärk on oluliste tõdede avastamine ja need tõed on looduse poolt ette määratud.

Loobumine sellisest olulise tõe mõistmisest on kõige tähtsam muutus Kitcheri käsitluses tema järgmises raamatus *Science, Truth, and Democracy* (esimene trükk 2001). Kitcher väidab, et maailma klassifitseerimisel ja kirjeldamisel ning episteemiliselt oluliste eesmärkide valimisel tehtavad otsused ei ole ette antud – need peegeldavad inimeste praktilisi huvisid ja väärtusi. See käsitlus võimaldab väita, et praegu teaduses valdavad otsused pole ainuvõimalikud – on võimalik teadlikult teha otsuseid, mis kannavad demokraatliku ühiskonna huvisid ja ideaale. Kitcher paneb ette modelleerida neid otsuseid "hästikorraldatud teaduse" ideaali abil. Teadus on hästi korraldatud, kui otsused uurimissuundade valiku ning uurimistöö tulemuste rakendamise kohta langevad kokku otsustega, mida representatiivne grupp saavutaks demokraatliku arutluse käigus, kui selle liikmed oleksid teadlikud teaduse praegusest seisust ("koolitatud") ja ühiskonna eri rühmade huvidest.

Seda mudelit on edasi arendatud Kitcheri hilisemas raamatus *Science in a Democratic Society* (2011). Seal on tema arusaamine sotsiaalsete faktorite rollist taas laienenud. Kitcher rõhutab nüüd väärtusotsuste paratamatut rolli teaduspraktikates – tõendusmaterjali piisavuse ning teadusliku debati arengu kohta tehtavad otsused on väärtustest läbinisti mõjutatud. See tähendab, et ka need teaduse aspektid on põhimõtteliselt avatud demokraatlikule arutelule, ja Kitcher laiendab hästikorraldatud teaduse mudelit, et katta ka teaduslike tulemuste sertifitseerimine ning teaduslike konfliktide lahendamine. Otsuste mõõdupuuks olev idealiseeritud arutelu on omakorda modifitseeritud vastavalt Kitcheri väärtustekäsitlusele. Kitcheri järgi on väärtuste põhjendamise aluseks ettekujutus ideaalsest eetilisest vestlusest, kus töötatakse välja väärtusi, mis toetavad kõikide inimkonna liikmete võrdseid šansse väärikale elule. Hästikorraldatud teadus eeldab seega, et otsused uurimistöö planeerimise, tulemuste tunnustamise ja rakendamise kohta on samasugused, nagu teeksid kogu inimkonna esindajad sellises väärtuste-teemalises vestluses. Teadlaste (ekspertide) roll selles protsessis on „koolitada" vestluses osalejaid, andes neile adekvaatse ettekujutuse teaduse hetkeseisust ja perspektiividest. Kitcher arutab ka seda, kuidas võiks praktikas selle ideaalse mudeli poole püüelda. Ta paneb ette koostada eri sotsiaalsete rühmade esindajatest koosnevad testgrupid. Need grupid saavad teadlastelt koolituse ja on pärast seda võimelised mõjutama teaduspoliitikat ja suhtumist teadusse, nii et need oleksid hästikorraldatud teadusega rohkem sarnased. Kitcheri argumendi viimane versioon hõlmab esiteks teaduse käsitlust, mis näitab eri sotsiaalsete faktorite sügavat mõju teadusele, teiseks visandeid teaduskorralduse ulatuslikust reformist ning viimaks mõningaid ettepanekuid nende visandite elluviimiseks.

Neljanda peatüki eesmärk on esitada Kitcheri hästikorraldatud teaduse kriitikat. Ma hindan tema käsitlust kõrgelt, kuna see võtab arvesse teaduse sotsiaalsuse eri tahke. See, et Kitcher pöörab tähelepanu teaduse ühiskondlikule mõjule ning õigluse küsimustele, on samuti kiiduväärne. Oma argumentidega näitan ma aga, et nii hästikorraldatud teaduse ideaalil kui sellest innustatud väikestel testgruppidel on olulisi probleeme, mis ohustavad Kitcheri käsitluse sisemist kooskõla, hästikorraldatud teaduse võimet täita oma sotsiaalseid funktsioone ning selle demokraatlikku vaimu. Minu kriitilised argumendid keskenduvad Kitcheri käsitluse kolmele aspektile: ootused ekspertidele, ekspertide teadmistega seotud probleemid ning avalikkuse kaasamisega seotud küsimused.

Hästikorraldatud teaduses eeldatakse ekspertidelt võimet anda mitteteadlastele objektiivne ettekujutus teaduse hetkeseisust; eeldatakse, et eksperdid on objektiivsed ja erapooletud. Selline ootus on aga vastuolus Kitcheri käsitluse mitme aspektiga. Hästikorraldatud teadus nõuab, et igaühe – ka teadlase – ettekujutusi väärikast elust võetaks arvesse. See aga tekitab vastuolu erapooletu eksperdi ning omaenda huvisid kaitsva teadlaskonna esindaja rollide vahel. Erapooletus ja objektiivsus on vastuolus ka sellega, kuidas Kitcher on alati käsitlenud episteemilisi agente – tema tööjaotusele pühendatud argumendid lähtuvad sellest, et teadlastel on ka teisi huvisid peale puhtepisteemiliste. Traditsiooniline erahuvidest vaba eksperdi kuvand satub vastuollu Kitcheri teaduskäsitluse viimase versiooniga, mis rõhutab huvide ja väärtuste rolli teaduses.

Argumendi teises osas osutan ma ekspertide teadmistega seotud probleemidele, mis ohustaksid hästikorraldatud teadust ka juhul, kui erapooletud eksperdid oleksid võimalikud. Kui Kitcher hakkas rõhutama, et teadus on huvidest ja väärtustest mõjutatud, oli üks tema keskseid näiteid teaduslikud klassifikatsioonid. Mõistetega võivad varjatult kaasneda huvid ja väärtused. Mõned neist võivad olla problemaatilised, kuid see ei pruugi olla märgatav ekspertkogukonnale, mis jagab samu väärtusi. Lisaks Kitcheri enda käsitlusele kasutan ma Lacey näidet viljaseemne mõistest nüüdisaegses biotehnoloogias, modernse ühiskonna väärtuste osast selles mõistes ning sellest, kuidas need väärtused pole ühitatavad paljude vaesemate agraarsete kogukondade väärtustega. Ekspertide väärtuste eksplitseerimiseks oleks kasulik võtta arvesse nende kogukondade perspektiive. Hästikorraldatud teaduses ei eeldata, et avalikkuse esindajad võiksid pakkuda selliseid perspektiive, ja puuduvad institutsionaalsed mehhanismid nende esitamiseks. Võimalus, et ekspertide väärtustest mõjutatud mõisted ja lähenemised ei luba adekvaatselt lahendada inimkonna eri rühmade probleeme, on seega üks oht. Teine potentsiaalne oht on seotud sellega, et hästikorraldatud teaduselt oodatakse lahendusi praktilistele probleemidele, adekvaatsed lahendused võivad aga nõuda kohalike tingimuste tundmist. Toon siin näiteks Brian Wynne'i analüüsid ekspertide katsetest inglise lambakasvatajate aitamiseks pärast Tšernobõli katastroofi. Need katsed luhtusid ekspertide suutmatuse tõttu võtta arvesse talunike teadmisi kohalikest tingimustest ja lambakasvatuse praktikatest. Avalikkuse esindajate teadmised võivad seega olla hästikorraldatud teaduse edukaks funktsioneerimiseks hädavajalikud. Hästikorraldatud teaduse mudel ei võta aga seda võimalust arvesse ning selles puuduvad tingimused ekspertide teadmiste täiendamiseks relevantseid teadmisi ja kogemusi valdavate mitteteadlaste abiga.

Oma argumendi kolmandas osas näitan, et hästikorraldatud teaduses on motivatsioon avalikkuse üleüldse kaasamiseks vastuolude poolt nõrgendatud. Ühelt poolt väidab Kitcher, et inimestel peaks olema võimalus ise oma huvisid esindada, ning tema lähenemine väärtustele nõuab nende põhjendamiseks laia kaasavat vestlust. Teiselt poolt ei tohi otsuste langetamise aluseks olla suvalised soovid – Kitcher rõhutab, et arvesse võetakse sügavaid huvisid, mis sobivad kokku eesmärgiga tagada igaühele väärikas elu. Nendest sügavatest huvidest kirjutab ta aga mõnikord nii, nagu oleks neid võimalik sõnastada ilma inimestelt küsimata. *Science in a Democratic Society* lõpeb peatükiga, mis kirjeldab hästikorraldatud teaduse positsiooni mitme olulise uurimissuuna suhtes. Lisaks on Kitcher üldiselt skeptiline sõnavabaduse osas – ta hoiatab korduvalt "kakofoonia" eest. Ideaalse eetilise vestluse osapooltelt eeldatakse, et nad on hästi informeeritud ja tundlikud üksteise huvide suhtes; avalikud diskussioonid tegelikus elus, nagu neid kirjeldab Kitcher, on väga kaugel nendele ootustele vastamisest. Kui avalikkuse esindajate tegelikku kaasamist nähakse ebaproduktiivsena ning eeldatakse, et relevantseid vajadusi on võimalik identifitseerida ka ilma selleta, langeb motivatsioon kaasamiseks. See aga muudab küsitavaks Kitcheri mudeli demokraatlikkuse.

Peatüki lõpus pakun ma võimaliku seletuse Kitcheri lähenemisviisi valikule. Kitcheri lähenemine on globaalne – ta püüab teaduse ja demokraatia suhete probleeme lahendada kõige üldisemas vormis, mis muuseas võimaldaks ka efektiivselt tegeleda globaalprobleemidega nagu kliima soojenemine. Võimaliku "kakofoonia" ja kaose vältimine sellel skaalal nõuab väga ranget reguleerimist – huvid ja soovid, mida võetakse tõsiselt, on kitsalt defineeritud, teadlaste ja mitteteadlaste rollid selgelt ette kirjutatud, vastuolude ja konfliktide tõenäosust on kujutatud minimaalsena. Minu kriitika on aga näidanud, et selline lahendus toob kaasa väga olulisi probleeme.

Viienda peatüki eesmärk on pakkuda alternatiivi Kitcheri lähenemisele. Minu lähenemise aluseks on Longino objektiivsuse käsitlus. Kõigepealt vastan ma mõnedele üldistele vastuväidetele. Esiteks näitan ma, et Kitcheri kriitika alamääratuse teesi vastu, mille järgi on probleemiks mitme võrdväärse hüpoteesi olemasolu – ja sellist hüpoteeside paljusust teaduses tüüpiliselt ei esine – pole rakendatav Longino loogilise alamääratuse mõistmisele, mille keskmes on tõendusmaterjali ja hüpoteesi seos. Teiseks kaitsen ma Longino käsitlust Justin Biddle'i kriitika eest. Biddle'i järgi on indiviidid, nagu neid kirjeldab Longino, võimelised oma väiteid ise kriitiliselt analüüsima ja seega pole kogukond objektiivsuse saavutamiseks vajalik. Järelikult pole Longino objektiivsuse-käsitlus sotsiaalne. Mina väidan, et selline kriitika, mis on vajalik objektiivsuse saavutamiseks, on siiski võimalik ainult kogukonnas – selleks on vaja tunda kogukonna standardeid ja olla tuttav kogukonnas esindatud perspektiividega. Ma toetun Harry Collinsi vaikiva teadmise käsitlusele ning Martin Kuschi reeglite järgimise argumendile, näitamaks et standardite äraõppimine ning korrektne järgimine nõuab osalemist kogukonnas. Lisaks näitan ma, et see, kuidas Biddle mõistab Longino indiviidikäsitlust, on problemaatiline – Longino kirjeldatud indiviidide kriitilise analüüsi võimed on piiratud ning nad vajavad objektiivsuse saavutamiseks kogukonda.

Peatüki teises osas näitan ma, kuidas Longino lähenemine võimaldab käsitleda teaduse sotsiaalseid aspekte, mida katab Kitcheri teadusekäsitlus. Longino lähenemist võib põhimõtteliselt kasutada ka selleks, et nõuda avalikkuse väärtuste ning huvide arvessevõtmist uurimistöö planeerimisel, sertifitseerimisel ja rakendamisel, nagu seda eeldab hästikorraldatud teadus. Ma näitan aga, et Longino käsitlus saab tegelikult toetada ka avalikkuse esindajate sisukamat osalemist teadmise loomises. Longino rõhutab kogukondliku kriitika tähtsust ning osutab, et kogukonnas jagatud taustaeeldused võivad jääda nähtamatuks. Kogukonna objektiivsuse tagamiseks on seega vajalik relevantsete väliste perspektiivide kaasamine. Kasutan seda argumenti, et toetada relevantsete teadmiste ning perspektiividega mitteteadlaste kaasamist teadmise loomisesse eesmärgiga kritiseerida ekspertide perspektiive ja täita lünki nende teadmistes. Selles mudelis ei oodata ekspertidelt erapooletust, sest objektiivsus tagatakse kogukonna tasemel. Longino ideedel põhinev lahendus väldib seega Kitcheri lähenemise probleeme. See osutub aga võimalikuks üksnes universaalsuse arvelt. Selleks, et Longino ideedel põhinev kaasav diskussioon oleks produktiivne, on vajalikud teatud ühised normid ja eesmärgid. Sääraste ühiste normide väljatöötamine on aga lokaalne ja kontekstispetsiifiline protsess.

Peatüki lõpus kirjeldan ma, kuidas Longino käsitlus juhib tähelepanu laiema konteksti tähtsusele objektiivse kogukonna võimaldamisel. Efektiivne kriitiline diskussioon eeldab mitmekesiste perspektiivide võrdsetel tingimustel kaasamist. Ebavõrdsuse erinevad liigid, näiteks teatud sotsiaalsete rühmade ja nende perspektiivide marginaliseerimine (naiste vähesus teadlaskonnas) ja ebavõrdne ligipääs ressurssidele erinevate uurimissuundade arendamisel (teaduse kommertsialiseerumisest tingitud erinevused), ohustavad seega objektiivsust. Protsessid, mis vähendavad sellist ebavõrdsust või loovad tingimused kaasavaks diskussiooniks, võivad samas kaasamise kaudu aidata parandada kogukondade objektiivsust. Longino käsitlus aitab näha, mil viisil on säärased sotsiaalsed protsessid teadusfilosoofia jaoks relevantsed.

Kuuenda peatüki eesmärk on näidata, kuidas Longino ideedel põhinev käsitlus võimaldab teadusfilosoofiale produktiivseid lähenemisviise säärastele teaduspoliitilistele protsessidele. Demokratiseerimine ja avalikkuse kaasamine teaduspoliitikasse on olnud viimaste aastakümnete teaduspoliitikas märkimisväärne nähtus. Toetudes Mark Browni demokraatia ja teaduse suhete analüüsile, näitan ma, et nende eksperimentide taga võib näha eri motiivide segu – ühelt poolt poliitilised motiivid langetavatele otsustele legitiimsuse andmiseks ja teiselt poolt episteemilised motiivid otsuste episteemilise kvaliteedi parandamiseks ja objektiivsuse suurendamiseks. Tänu ühisele arusaamale objektiivsusest ja kaasavast diskussioonist nii Longino käsitluses kui teaduspoliitika demokratiseerimise eksperimentides, panen ma ette kasutada nende eksperimentide empiirilist analüüsi, mõistmaks, kuidas kaasavate teaduspraktikate võimaldamise katse võib kulgeda ja milliseid võimalusi või takistusi seab sellele lokaalne poliitiline ja sotsiaalne olukord.

Selle lähenemisviisi illustreerimiseks esitan ma kaks juhtumianalüüsi. Esimese kirjeldamisel toetun ma Sheila Jasanoffi biotehnoloogia poliitika analüüsile. Saksa Demokraatliku Vabariigi 1990. aasta geenitehnoloogia seadus nõudis avalikkuse kaasamist geenitehnoloogiaga seotud otsuste langetamisel. See lõi ühiskonnas tingimused kaasavaks diskussiooniks. Avalikud arutelud kaldusid aga olema ebaproduktiivsed ja kõikide osapoolte jaoks frustreerivad; hiljem neist loobuti. Toetudes Longino ideedele produktiivsest diskussioonist pakun ma selle katse läbikukkumise üheks seletuseks ühiste standardite ja eesmärkide puudumist teadlaste ja avalikkuse esindajate diskussioonis. Eduka kaasava diskussiooni loomisel on seega võtmeküsimuseks sääraste ühiste standardite tekitamise võimalikkus. Näitamaks, et see mõnikord õnnestub, esitan ma teise juhtumianalüüsi, mis põhineb Alison Wylie arheoloogia eetika uuringutel. Wylie demonstreerib, kuidas Ameerika arheoloogias on kujunenud uus ettekujutus arheoloogi kui pärandi hoidja kohustustest teiste sotsiaalsete rühmade ees. Kommunikatsioonist huvigruppidega, kelle nimel arheoloogilist materjali hoitakse, on saanud üks arheoloogi keskseid kohustusi. Wylie toob näiteid sellest, kuidas kommunikatsioon ja avalikkuse esindajate kaasamine on võimalikuks teinud episteemiliselt produktiivse arheoloogilise praktika. Episteemiliselt ja poliitiliselt edukas kaasav teaduspraktika on seega teatud tingimustel võimalik.

Ma kasutan nende juhtumite analüüsi, et toetada Longino ideedel põhineva lähenemise eelistamist võrreldes Kitcheri hästikorraldatud teadusega. Ma näitan, kuidas need analüüsid demonstreerivad lokaalsete tingimuste tähtsust, rääkides seega vastu globaalse lähenemise võimalusele. Kohalike kontekstide eripära tunnustamine sunnib kahtlema ka Kitcheri kirjeldatud erinevate testgruppide võimes jõuda samade teaduspoliitiliste otsusteni, nagu hästikorraldatud teaduse globaalne iseloom eeldaks. Lisaks juhivad esitatud analüüsid tähelepanu kirjeldatud protsesside sellistele aspektidele, mida hästikorraldatud teaduse ekspertide objektiivsuse ja mitteteadlaste ignorantsuse eeldused varjavad. Longino käsitluse rakendamine konkreetsetele teaduspoliitilistele olukordadele võimaldab vastata kriitikutele, kes süüdistavad teda abstraktsuses (Biddle) või tsirkulaarsuses (Anna Leuschner). Peatüki lõpus panen ma ette lugeda sääraste lokaalsete teaduspoliitiliste initsiatiivide analüüsi ja nende soodustamist üheks võimalikuks viisiks teha sotsiaalselt relevantsemat teadusfilosoofiat. Koostöö olemasolevate teaduspoliitiliste arenguinitsiatiivide elluviimisel võib anda võimaluse filosoofiliste tähelepanekute realiseerimiseks.

Sellise lähenemisviisi arendamine avab võimalusi edasiseks uurimistööks: teadusfilosoofia ülesandeks saaks siis järgnevate demokratiseerimiskatsete analüüsimine ja koostöö nende läbiviijatega. Keskne küsimus oleks sealjuures, kui suur osa teaduspoliitika demokratiseerimise katsetest on võimelised toetama avalikkuse relevantsete teadmiste ja perspektiividega esindajate sisulist osalemist mitte üksnes teaduspoliitikas, vaid ka teadmise loomises.

CURRICULUM VITAE

Name: Jaana Eigi
Date and place of birth: 19.08.1985, Tallinn
E-mail: jaana.eigi@gmail.com

Education:

University of Tartu, Department of Philosophy, PhD programme (2011–2016), title of the thesis "The Social Organisation of Science as a Question for Philosophy of Science", supervisor Dr Endla Lõhkivi

University of Vienna, Department of Philosophy, visiting PhD student (10.2014–01.2015)

University of Tartu, Department of Philosophy, MA programme (2007–2009), MA *cum laude,* title of the thesis "Public Understanding of Science: A Case Study of Rind et al. Controversy" (in Estonian), supervisor Dr Endla Lõhkivi

University of Tartu, Department of Philosophy, BA programme (2004–2007), BA *cum laude,* title of the thesis „Philip Kitcher's Argument on Freedom of Inquiry" (in Estonian), supervisor Dr Endla Lõhkivi

Research interests:

Philosophy of science, social aspects of science, science policy, public understanding of science

Publications:

Peer-reviewed articles:

1. Eigi, Jaana (2016). Different motivations, similar proposals: Objectivity in scientific community and democratic science policy. *Synthese: An International Journal for Epistemology, Methodology and Philosophy of Science*, DOI 10.1007/s11229-016-1077-1.
2. Eigi, Jaana (2015). On the social nature of objectivity: Helen Longino and Justin Biddle. *THEORIA: An International Journal for Theory, History and Foundations of Science*, Vol. 30, No. 3, pp. 449–463.
3. Eigi, Jaana; Pille Põiklik; Endla Lõhkivi and Katrin Velbaum (2014). Supervision and early career work experiences of Estonian humanities researchers under the conditions of project-based funding. *Higher Education Policy*, Vol. 27, No. 4, pp. 453–468.
4. Eigi, Jaana (2013). "Knowing things in common": Sheila Jasanoff and Helen Longino on the social nature of knowledge. *Acta Baltica Historiae et Philosophiae Scientiarum*, Vol. 1, No. 2, pp. 26–37.
5. Lõhkivi, Endla; Katrin Velbaum and Jaana Eigi (2012). Epistemic injustice in research evaluation: A cultural analysis of the humanities and physics in Estonia. *Studia Philosophica Estonica*, Vol. 5, No. 2, pp. 108–132.
6. Eigi, Jaana (2012). Two Millian arguments: Using Helen Longino's approach to solve the problems Philip Kitcher targeted with his argument on freedom of inquiry. *Studia Philosophica Estonica*, Vol. 5, No. 1, pp. 44–63.

Other publications:

Eigi, Jaana (2014). Ühiselt asjade tundmine: objektiivne teadmine ja avalik diskussioon. *Sirp. Eesti Kultuurileht*, No. 13(3483), pp. 18–18.

Courses taught:

John Zimani teadusfilosoofia (Valitud peatükke teadusfilosoofiast)

Teadusfilosoofia ja -metodoloogia

Disagreements in Science (Research seminar) (taught with Endla Lõhkivi, Ave Mets and Edit Talpsepp)

Professional Memberships:

Member of the European Philosophy of Science Association (EPSA) since 2013

Member of the Estonian Association for the History and Philosophy of Science (TTEÜ) since 2012

ELULOOKIRJELDUS

Nimi:	Jaana Eigi
Sünniaeg ja -koht:	19.08.1985, Tallinn
E-post:	jaana.eigi@gmail.com

Hariduskäik:

Tartu Ülikool, filosoofia osakond, doktoriõpe (2011–2016), väitekirja teema "Teaduse sotsiaalne korraldus kui teadusfilosoofiline problem" (inglise keeles), juhendaja Endla Lõhkivi (PhD)

Viini Ülikool, filosoofia osakond, külalisdoktorant (10.2014–01.2015)

Tartu Ülikool, filosoofia osakond, magistriõpe (2007–2009), MA cum laude, magistritöö teema "Ühiskonna ettekujutused 'heast teadusest' (Rind-Tromovitch-Bausermani artikli juhtumi analüüsi näitel)", juhendaja Endla Lõhkivi (PhD)

Tartu Ülikool, filosoofia osakond, bakalaureuseõpe (2004–2007), BA cum laude, bakalaureusetöö teema "Philip Kitcheri argument uurimisvabadusest", juhendaja Endla Lõhkivi (PhD)

Peamised uurimisvaldkonnad:

Teadusfilosoofia, teaduse sotsiaalsed aspektid, teaduspoliitika, teaduse mõistmine ühiskonnas

Publikatsioonid:

Publikatsioonid eelretsenseeritavates ajakirjades:

1. Eigi, Jaana (2016). Different motivations, similar proposals: Objectivity in scientific community and democratic science policy. *Synthese: An International Journal for Epistemology, Methodology and Philosophy of Science*, DOI 10.1007/s11229-016-1077-1.
2. Eigi, Jaana (2015). On the social nature of objectivity: Helen Longino and Justin Biddle. *THEORIA: An International Journal for Theory, History and Foundations of Science* 30(3), lk. 449–463.
3. Eigi, Jaana; Pille Põiklik; Endla Lõhkivi and Katrin Velbaum (2014). Supervision and early career work experiences of Estonian humanities researchers under the conditions of project-based funding. *Higher Education Policy* 27(4), lk. 453–468.
4. Eigi, Jaana (2013). "Knowing things in common": Sheila Jasanoff and Helen Longino on the social nature of knowledge. *Acta Baltica Historiae et Philosophiae Scientiarum* 1(2), lk. 26–37.
5. Lõhkivi, Endla; Katrin Velbaum and Jaana Eigi (2012). Epistemic injustice in research evaluation: A cultural analysis of the humanities and physics in Estonia. *Studia Philosophica Estonica* 5(2), lk. 108–132.
6. Eigi, Jaana (2012). Two Millian arguments: Using Helen Longino's approach to solve the problems Philip Kitcher targeted with his argument on freedom of inquiry. *Studia Philosophica Estonica* 5(1), lk. 44–63.

Muud publikatsioonid:

Eigi, Jaana (2014). Ühiselt asjade tundmine: objektiivne teadmine ja avalik diskussioon. *Sirp. Eesti Kultuurileht*, 13(3483), lk. 18–18.

Õpetatud kursused:

John Zimani teadusfilosoofia (Valitud peatükke teadusfilosoofiast)

Teadusfilosoofia ja -metodoloogia

Disagreements in Science (Uurijaseminar; inglise keeles) (õpetatud koos Endla Lõhkivi, Ave Metsa ja Edit Talpsepaga)

Osalemine erialastes organisatsioonides:

Euroopa Teadusfilosoofia Assotsiatsiooni (European Philosophy of Science Association, EPSA) liige alates 2013. aastast

Teadusajaloo ja Teadusfilosoofia Eesti Ühenduse liige alates 2012. aastast