universität
wien

DISSERTATION

Titel der Dissertation

“Ethics is like a book that one reads when one has time”

Exploring lay ‘ethical’ knowledge in a public engagement setting

Verfasser

Mag.phil. Michael Strassnig

angestrebter akademischer Grad

Doktor der Philosophie (Dr. phil.)

Wien, November 2008

Studienkennzahl lt. Studienblatt:	A 092 122
Dissertationsgebiet lt. Studienblatt:	Soziologie, geistewiss. Studienzweig
Betreuerin:	Univ.Prof. Dr. Ulrike Felt

»Ethics is like a book that one reads when one has time«
Exploring lay ‘ethical’ knowledge in a public engagement setting

---

Table of Content

Acknowledgements

I have been occupied with writing this thesis, and with the preceding research, for four and a half years. In the course of the work, many people and institutions contributed in a number of different ways to its production. Thus, I would like to thank them here.

First and foremost, I would like to thank Ulrike Felt for supervising and supporting me when I was encountering the challenges of such an endeavor. She also led the project on which this thesis is based. The same debt of gratitude I also owe to Maximilian Fochler and Annina Müller, my collaborators in the project at the Department of Social Studies of Science, University of Vienna, for providing a constructive environment; I'm deeply indebted to them for discussing a multitude of issues with me, and much more, for their friendship.

My thanks also go to Sandra Karner, Bernhard Wieser, Astrid Mager and Michaela Glanz, for many contributions to the "Let's talk about GOLD!" project, and to Priska Gisler, Silke Schicktanz and Brian Wynne, for their contributions as members of the project's advisory board.

Rob Hagendijk, Alan Irwin, Martina Merz, Helga Nowotny, Jane Summerton and Sally Wyatt provided valuable comments on different versions of this thesis as a work in progress at the Department's yearly Summer School in Raach, Austria.

I'm also grateful to Michael Nentwich, as the second assessor and examiner.

I would also like to thank the GEN-AU ELSA program of the Austrian Federal Ministry of Science and Research, which provided the financial means for carrying out the "Let's talk about GOLD!" project, and funding my research stay abroad at CESAGEN (Lancaster and Cardiff University). I would like to thank also the people there, and especially Ruth Chadwick and Paul Atkinson for generously hosting me during a research stay in 2004 and 2005.

My thanks also go to Brian Conn and Carrie Kovacs for proofreading this text and correcting my English.

I would like to express my great thanks to the researchers, lay participants and the invited experts of the Round Table discussions who so generously shared their thoughts with us.

Finally, my thanks and my love goes to Annina—for all the support she gave.

All remaining errors are mine.

1. Introduction

> *People of Earth, your attention, please. As you will no doubt be aware, plans for the redevelopment of personhood, family, and sexuality and morality require the building of a hyper spatial express route through your values, and regrettably they have been scheduled for demolition. An independent ethics committee has given its approval. The process will take slightly less than two of your Earth minutes. Thank you.*
>
> (Cook 2002)

1.1. Public participation and ethics: Antagonisms in science governance

The call for public engagement with science and the call for ethics have both become commonplaces in contemporary public policy around science, innovation and emerging technologies. Public engagement and ethics go beyond more classical notions of the state in decision-making processes, as well as of the role of science and citizens in society, and have largely replaced traditional risk concepts of assessing the probability of harm arising from determinable causes (Jasanoff 1999). The rise of these new "technologies" of governance has been triggered to a large degree by advances in new genetics that pose new and challenging questions about their social and ethical impacts, creating a source of new public controversies—for example regarding GM food. However, there are huge differences in the way science, politics and citizens are enacted in public participation and by ethics. While public participation aims to extend the range of knowledge, experiences and interests relevant to political decision-making, ethics, in particular in its dominant practice as institutionalized expert deliberations, is mostly characterized by a rather ambivalent relationship to the public. On one hand, it rests on the exclusion of lay people from ethical opinion making. On the other hand, ethical authorities cannot deny that public legitimacy is required for politics today.

A nice example for this ambivalent relationship with the public is the *European Group on Ethics in Science and New Technologies* (EGE), which advises the European Commission. The EGE is a pure expert committee of recognized academics from different fields, whose deliberations, its mandate notes, "shall not be open to the public". The EGE members are appointed as persons and not as representatives of social institutions, groups or interests, in order to provide "independent" expertise, and thus are responsible only to their own consciences. Hence, their institutional practice rests on a broad exclusion of what is called the "public"; however, their reports (so-called "Opinions") emphasize that "Public participation is of vital concern in democratic states", and ask "How can we ensure that the public participates not only in discussions associated with nanotechnology and nanomedicine but in the overall design of research and development policy?" (European Group on Ethics in Science and New Technologies to the European Commission (EGE) 2007, 44). Here, the EGE calls for public participation not only in concrete issues, but much more basically in the way current innovation regimes are constructed. The question is how this call for opening up deliberations about innovation processes in science and research to more bottom-up approaches relates to the EGE's own practice of predetermining relevant ethical issues in the form of recommendations which aim to define what is an ethical issues for a particular subject in a very exclusive way. And what is the role of the EGE in governing the innovation processes which it aims to democratize?

This and other striking antagonisms and ambivalences in the current governance of science and technology were among the central motives for writing this dissertation. The question is, if public engagement is politically legitimized and already practiced for a wide range of issues—be they scientific or not—why is ethics still a nearly exclusive domain for experts? Should public participation not be possible for ethics too, in particular if ethics means how current and future technologies interact with our moral imaginations and everyday practices?[1] Ethics experts deliver some answers to these questions: A recurrent theme in their claims that the public cannot participate in ethical issues is that it lacks the ability to articulate "reasoned" arguments. In this claim, "ethics" is seen as the "reflective" and "theoretically informed" way of engaging with morality with the end of producing normative judgments. Ethicists often argue that ethical decision-making has to be based on both informedness on the matter itself and the ability to provide well-argued, "sophisticated" ethical reasons. Hence, the call for "reason" in public ethical debates is often a means to exclude certain positions from the discussion and to maintain expert authority over certain issues.

The way science governance deals with public participation is characterized, too, by a range of ambivalences that become especially visible in EU policy statements. There—as well as in national contexts implementing these policies—public participation is celebrated as a new and more stable way of political decision-making, able to restore lost public confidence in institutions and authorities as well as to overcome the crisis of legitimacy in political decision-making. However, if science is understood in terms of "innovation" within an economic rationale, the role of citizens in decision-making processes is often quickly marginalized. If citizens do not place enough value on the economic benefits they might gain from an increased investment in R&D, the public has to be made "aware" of this "fact"—thus

---

[1] These questions were in the focus of an article my colleagues Ulrike Felt, Maximilian Fochler, Annina Müller and I wrote together and submitted to *Public Understanding of Science* in 2006, and which will be published in 2009 (Felt et al. 2009). Taking this article's basic questions as a starting point, I aim to analyze these questions more broadly and in more detail in this dissertation.

constructing the scientific citizen as a consumer, worshipper or follower whose task is to provide an innovation-friendly environment for businesses. Here, the citizen loses the role of a "sovereign" of political decision-making that is assigned to him or her in other contexts.

These different narratives stand side by side, often in the same documents (Hagendijk 2004). The question, however, is how these different visions of the "scientific citizen" relate to each other as well as to more technocratic governance regimes that highlight the role of experts in political decision-making processes. How are the more technocratic visions of science governance in ethics politics and innovation-framed science governance compatible with the more democratic tenets of political decision-making embodied by "public participation", which rest neither on informedness nor on the ability to articulate sophisticated opinions? So far, answers have not been provided.

Despite the striking ambivalences and unanswered questions inherent in public participation and ethics, both have become "business" in many contexts. Public participation events are organized routinely on a wide range of issues; methods and models are imported and exported from one country to another. The call for standardization and "best practices" becomes louder (e.g., Österreichisches Lebensministerium and Bundeskanzleramt der Republik Österreich 2008), and increasingly the organization of public participation events is "outsourced" to professional Public Relations agencies "discovering" science communication as a new business area. More and more, public participation has be-come "business as usual", and governments and policymakers quickly return to their daily routines after such events end (Hagendijk and Irwin 2006; Irwin 2006). Thus, some observers perceive a danger that participation is treated as a one-size-fits-all solution for all kind of problems, thus engendering a "technocracy of participation" (Chilvers 2008) or even a "tyranny of participation" (Cooke and Kothari 2001).

Ethics, too, has gone business, for example in biotech companies that operate their own ethics committees to morally "approve" their businesses. National ethics committees have become "political machines" that produce moral opinions on a wide range of issues and then inform national as well as international policies, often bypassing wider debates as well as the usual legislation processes by installing some kind of "soft law" (Tallacchini 2006). For the EU's Framework Programme research proposals, enterprises offer so-called "Ethical Review Red Teaming"[2] in order to test research proposals for the worst case and thus to produce the "ethically safe" research proposal which will make it through the review process. Ethics and participation, while seeking to remedy shortcomings of more traditional concepts of science governance, open up a series of questions which science policy has not even attempted to address, and thus also challenge assumptions about democracy.

---

[2] See http://www.efpconsulting.com (accessed July 7, 2008).

A further feature of both the participation "business" as well as the ethics "business" is that they have become increasingly more internationalized and globalized. With regard to public participation, nationally successful models such as the Danish consensus conferences have been exported and introduced in different national contexts and cultures. They also serve to create a "European society" and public, though often with problematic outcomes and visions. A paramount example is the *Meeting of Minds* consensus conference on brain research,[3] which assembled representatives of the "public" from many different EU member states. However, a danger may be that a conception of a European society or public is created that does not correspond to the heterogeneity of the public's cultural and national concerns towards emerging technologies. Nevertheless, the emphasis on a single European society is reiterated in recent European science policy documents (European Commission 2004a, 2005a, 2007c). Ethics, too, has become important in global politics of science and technology, suggesting that morality regarding science and technology has a common global basis. For example, the UNESCO alone operates three ethics committees on science and technology: the International Bioethics Committee (IBC),[4] the Intergovernmental Bioethics Committee (IGBC),[5] and the World Commission on the Ethics of Scientific Knowledge and Technology (COMEST).[6] The increasing institutionalization of such committees contributes to the assumption that values are tradable between cultures, and thus creates what Brian Salter has termed a "global moral economy" (Salter and Salter 2007).

In my personal experiences and encounters with "ethics", these ambivalences have also become visible. In a research project which is not part of this dissertation, we engaged with the question of informed consent—one of the core issues of ethics—at a hospital that used skin tissue remaining from plastic surgery in basic research. Since we had no experience with practices and routines in hospitals hospital before the beginning of the project, much of our knowledge of informed consent came from ethicists' accounts of the issue, as well as guidelines for the appropriate informed consent procedure. What was surprising was the distance between bioethical ideals accounts and what happened in the hospital between patients, researchers, doctors, administration and us as social scientists. How informed consent was performed in these complex relations had not much to do with the way informed consent is framed in the ethical literature. While ethics frames this issues rather narrowly, as a problem of doctor-patient relations often based on a few principles such as "autonomy" or "benevolence", in fact, in the moment of giving consent, there is much more at play than the rational provision of information and the free consent of patients. In our interviews, patients, researchers and other

---

[3] http://www.meetingmindseurope.org/europe_default_site.aspx?SGREF=14 (accessed May 27, 2008).

[4] http://portal.unesco.org/shs/en/ev.php-URL_ID=1879&URL_DO=DO_TOPIC&URL_SECTION=201.html (accessed May 27, 2008).

[5] http://portal.unesco.org/shs/en/ev.php-URL_ID=1878&URL_DO=DO_TOPIC&URL_SECTION=201.html (accessed May 27, 2008).

[6] http://portal.unesco.org/shs/en/ev.php-URL_ID=6193&URL_DO=DO_TOPIC&URL_SECTION=201.html (accessed May 27, 2008).

hospital professionals accounted for the possibilities and limits of trust in the medical system as such, the way research is entangled with the production of therapies that might be useful to patients, the imagined roles of patients as citizens in this system and so forth (Bister et al. 2009). Much more interestingly, many ethicists seem fully aware of this gap between theory and practice, but it doesn't make a difference for them in their professional engagement with the issue. This has to do with the meta-ethical assumption that what "is" cannot inform what "ought to be". Ethics provides a normative vision of human relations which do not exist and probably never will. Of course, what ought to be cannot necessarily be justified by what is. However, the "is" could indicate the complexities of social relations, and thus should caution against simple normative recipes. For example, the very practice of informed consent in biomedical contexts follows textbook visions, and thus imposes a certain framework on both patients and doctors that they are unable to comply with in most of the cases. Thus, informed consent presented itself to me as a "technology of disillusion", where all parties involved never believe that the explicit content of informed consent is meant seriously. While ethics provides fixed solutions based on norms that guide the relationship between doctors and patients, it is open what the question actually is in the first place. Is "informed consent" the problem (and the solution), or are there other concerns that are much more important to all involved actors?

The case of informed consent hints at the fact that ethics has become a powerful means of moral ordering in society, permeating society in more and more contexts but often bypassing "official" and democratic ways of rule-making. While both ethics and public participation could be powerful instruments to challenge established regimes that govern science-society relations, both have been incorporated into bureaucracy to a large degree. Despite the aim of both approaches to address the shortcomings of traditional regimes that emphasize the quasi-natural authority of science to provide solutions to society's problems, ethics and participation have been treated so far rather separately. Experts in institutionalized bodies handle ethics, in which the public has no voice, and where in most cases there is no transparency. Public engagement, on the other hand, at least rhetorically, opens up a range of legitimate experiences that could contribute to a more "socially robust" decision-making process. The question is, what happens if the public more directly engages with ethical issues? Will this fail, as ethicists sometimes suggest, because non-professionals in ethics are not able to provide "reasoned" accounts of their moral judgments? Or will the public prove to be better "ethicists" because their ethical judgments are based on experience? The answers, however, are—as I will show in this thesis—far more complex.

1.2. Empirical setting and research questions

In this thesis I will address the possibilities and limits of discussing "ethics" in a participatory setting where ethical non-experts—lay people and researchers from a genomics project—met in order to discuss ethical and social aspects of genomics as well as science and research in general. I will argue that the participants were able to discuss ethical issues in an advanced way, but often by framing the problems rather differently than professional ethicists would do. These accounts are not simply "other" than in "official ethics", but respond in a particular way to existing regimes.

The data for this thesis stem from the research project *Let's talk about GOLD! Analyzing the interactions between genome-research(ers) and the public as a learning process,* carried out at the Department of Social Studies of Science at the University of Vienna.[7] In this project we staged an experiment in public participation wherein citizens with different backgrounds met with researchers on a particular genomics research project for six whole-day "Round Table" discussions where ethical and social aspects of (genome) research were discussed (see Chapter 6 for a more detailed account of the setting). When designing the project, we assumed that a different kind of ethical debate would take place when ethics experts were not involved. Furthermore, through the symmetrical involvement of citizens and researchers, not only would the public have the opportunity to "talk back to science", but researchers would also be able to respond directly to the citizens and their concerns, as well as raise the own concerns.

Because "public participation in ethics" has rarely been done empirically and seldom been addressed in social science analysis,[8] I will provide a broad picture of the ethical debates at the Round Table discussions. First, I will ask what kind of ethical issues were given importance by the participants. As such, this question relates to the context of ethics in wider society: On one hand, ethics often justifies expert authority over ethical issues by assuming that the public is ignorant of "reasoned ethical judgments" and "scientific facts" (Levitt 2003). On the other hand, "official ethics" is criticized because of its narrow framing of ethical issues and its ignorance of public concerns (Wynne 2001). Thus, the questions are, what ethical issues do the participants emphasize, and how are these issues framed?

Second, we regarded the Round Table much more as a developing process and learning setting than a mechanism to produce certain outcomes—as is often the case in a range of public engagement exercises. The discursive processes that take place in such settings are widely neglected both by policy makers—who are primarily interested in a "digestible" output—and by social scientists (Rowe and Frewer 2000, 2004) evaluating such exercises (Harvey 2008). Thus, I will ask what kind of discursive processes are at work in the discussion around ethical issues of genomics, and science and technology in general. As ethics sometimes describes

---

[7] Project leader: Ulrike Felt; research collaborators: Maximilian Fochler, Annina Müller (Department of Social Studies of Science), Sandra Karner, and Bernhard Wieser (IFZ Graz).

[8] My colleagues Ulrike Felt, Maximilian Fochler, Annina Müller and I aimed to fill this gap in an article forthcoming in *Public Understanding of Science* (Felt et al. 2009). Only a few other article have also engaged with this question, see Levitt (2003) and Banks, Leach Scully, and Shakespeare (2006).

itself as the struggle for the best argument, I will analyze the micro-politics that shape the course of the debate in such a setting.

Third, public engagement exercises produce not only material outputs, such as consensus papers, but also “citizens” (Elam and Bertilsson 2003; Irwin 2001; Michael and Brown 2005) who had particular experiences with the setting itself as well as with the content of the debates. Thus, it is crucial to ask how the participants evaluate their engagement with ethics and ethical issues afterwards. How did the lay people assess the way the researchers dealt with ethics, and what relevance was given to ethics in science governance in general? This is intended as a contribution to a critical “public understanding of *ethics*” as many parallels can be drawn to earlier conceptions of “public understanding *of science*” (The Royal Society 1985), where the public was characterized as “ignorant” and thus in need of proper education. This classical public understanding of science and ethics revolved around the idea that science and ethics can be mediated and perceived by lay people as a sum of factual knowledge and textbook methodologies. However, the work of critical public understanding of science showed that people perceive science predominantly in its institutional dimension (Wynne 1993, 1996a) and in relation to their social identities and relations (Michael 1992, 1996, 2006). The question is, then, how did the Round Table participants perceive ethics after having engaged with it for a rather long time? How did they contextualize their own engagement with ethical questions within the institutional landscape?

1.3. How to trace “ethics” in a public participation event? Practical, conceptual and methodological challenges

As I will discuss later, the participants in our setting did not talk about ethics by explicitly using this term and referring to approaches and theories common in professional ethics. Thus, tracing ethics at the Round Table did not simply work by importing definitions from this context. What is called “ethics” here by the participants often has little in common with the way the term is used by ethical experts. However, I decided to use the term “ethics” when making value-based arguments because I understand “ethics” not only as an epistemological term used by a specific academic discipline, but also in its political dimension. Indeed, my use of the term “ethics” for the discussion at the Round Table has a political impetus, as it aims to pose the question of who should participate in the definition of what counts as an “ethical” issue.

Following Thomas Gieryn (1999; 1995), the work of ethicists and their institutions can be seen as “boundary-work” by attributing selected characteristics to what it means to account for ethics (being educated as ethicist; forms of institutionalization and thus public legitimacy; methods used, etc.) (Kelly 2003). Through “boundary-work”, ethics is segregated from other societal domains—for example, from the public ways of thinking about ethical issues, or from science as a domain concerned with the fabrication of facts—and thus a social boundary is constructed that distinguishes professionalized ethics from other cultural and intellectual activities that are regarded as non-ethics. The main feature of ethical boundary-work is the constitution of the distinction between “ethics” and “morality”, which creates two separated spheres for dealing with value questions. The former is theoretically informed and said to be rational, systematic and reflective, as well as detached from social contexts; the latter is said to be informed by social experiences, affected by emotions, bound to the context in which it is produced and partly “irrational”. This can frequently be observed in public discussions on highly controversial issues such as embryonic stem cell research or euthanasia, where ethical experts criticize the debate as too emotional and call for a more informed and more rational debate. As I do not want to re-enact cultural boundaries between ethics and the rest of society, I abandon the ethicists’ distinction between “ethics” and “morality” for my analysis of the discussions at the Round Table, but keep in it well in mind as an actual practice by institutionalized ethics. This is crucial, as the participants in the Round Table implicitly and explicitly re-introduced this distinction by denying their own ability to engage in a “real” ethical debate.

While institutionalized ethics, as the dominant form of “ethics” in public policy, implies the exclusion of a wide range of actors from an ethical discourse and a rather top-down definition and framing of what counts as an ethical issue, our aim in the project was to open up this question to a wider range of actors and allow for an alternative framing of ethical issues. A common objection of bioethicists to the inclusion of “the public” is that “normal” people only hold moral attitudes and do not reflect on and scrutinize the basis on which these attitudes are built. Although this may be true for particular “mechanisms of voicing” (Michael and Brown 2005) such as quantitative surveys, the Round Table discussions demonstrated that the participants were able to reflect critically on their own and others’ value assumptions. It is also true that the ethical discussion at the Round Table was not as “sophisticated” as academic ethical discussions; however, the question remains, who defines which arguments are sophisticated and theoretically informed enough to really be ethics?

While institutionalized ethics frames issues rather narrowly (Evans 2002; Wynne 2001) by a given set of predefined principles and a particular way of thinking, I will strive for a more open meaning of ethics which seeks to take up how different actors—in my case the lay participants and the researchers—deal with value questions related to science and technology. In particular, in order to get “bottom-up” definitions, I will not assume a predefined way in which issues must be addressed to count as “ethics”. I will use a modest working definition of ethics as all negotiations on values that allow for an exploration of the *reasons why the participants have promoted these values,* which provides insights into the underlying narratives that guide personal and collective opinion-making. In many cases no single person provided such a narratives; hence, I will focus rather on *discourses between the participants*. By focusing on discourses and the distributed manufacture of ethical argumentation, I will circumvent an objection against lay participation in ethics that is raised by professional ethicists. Furthermore, I will also take into consideration the *discursive politics* the participants employed when they talked about ethical issues as an important dimension of an ethical discussion. This is, however, contrary to the assumption of ethicists that the "best reasoned argument" will and should prevail. To be taken "seriously" in a public discourse, much more than a rational argument is required. It is more the context than the argument itself that defines the "validity" and acceptability of a moral argument (Jasanoff 2005, 171-202; Fox and DeVries 1998; Fox and Swazey 1984).

1.4. Outline of the thesis

I start by describing changes in the relationship between science and society along different aspects which are the background for the thesis's main theme of public participation in ethics (Chapter 2.1). I then analyze the notion of the responsibility of science and scientists, as "responsibility" has been one of the main buzzwords that accompanies transformations in science-society relations, and involves a new aspect of the governance of science (Chapter 2.2).

In Chapter 3, I describe public participation and ethics as two approaches of science governance that aim to address particular problems in science-society relations. I focus first on the different narratives of public participation that have been high on the political agenda during recent years, and engage in particular with the kinds of conceptions and values that are promoted through different visions of public participation (Chapter 3.1). In Chapter 3.2, I turn to ethics and identify its dominant form, which aims to manage value questions of science and technology. The dominance of a particular form of ethics that rests on the expertise of professionals excludes alternative forms of addressing the moral questions of technosciences, particularly those present in citizens' imaginations. I explain why more attention should be paid to researching lay ethics. I then describe some basic characteristics of the dominant form of ethics and its political context (Chapter 3.3), and ask how ethics and public engagement can be brought together (Chapter 3.4).

As this dissertation engages with ethics empirically, I analyze other attempts to do so. Here, the field of "empirical ethics" is of particular relevance, and is discussed in Chapter 4. I analyze the basic assumptions of empirical ethics, critically engage with it from a social science perspective and aim to situate it in a wider political context. In order to contextualize the ethical debates of the Round Table, I draw on the Austrian discourses and institutional practices of public participation and ethics in Chapter 5.

In Chapter 6, I engage with the methodological setting of the Round Table, and explain in more detail what assumptions were built into the empirical setting in order to investigate how ethical lay deal with ethics beyond expert discourses. Furthermore, I explain how I analyzed the material produced at the Round Table

(Chapter 6), and then go into the main research questions that guided my empirical analysis (Chapter 7).

The first of my empirical chapters is dedicated to the analysis of four main ethical issues and topics that were discussed at the Round Table (Chapter 8), which, in different subchapters, problematizes different aspects of the ethical discussions. Chapter 8.1 engages with the question of what can be a "problem" and its associated "solutions" in the first place, in the way the researchers and the lay participants define them. Chapter 8.2 focuses on the issue of responsibility, that is, the question of how and for what can and should science and researchers be responsible. Both subchapters are engaged with ethical question in science-society relations, while the next two subchapters elaborate on ethical issues in the means of knowledge production around the debates on animal experimentation. Chapter 8.3 asks how the ethical issue of animal experimentation contributed to the construction of the individual and collective identities of the researchers. Chapter 8.4 elaborates on how animal experimentation shaped mutual trust relations between researchers and lay people in which "transparency" and "authenticity" was the focus. Chapter 9 engages with the discursive processes that shaped the debate on ethical issues. The last empirical part analyses what changes and "learning processes" on ethics took place in course of the Round Table (Chapter 10).

Building on the empirical analysis of the Round Table discussions, I offer three main conclusions in Chapter 11, which address in what ways lay ethical knowledge might be useful in critically questioning implicit assumptions of science governance, as well as what implications for ethics might emerge from the engagement of the public with ethical issues.

2. From science *and* society to research *in* society?

Currently, we are witnessing wide debates about the role of science/research in society, with special emphasis placed on “change” and “transformation”. In these multilayered debates, concepts that are presented as “new” confront visions of science and research which are thought of as more traditional. As such, normative expectations and political visions meet with more realistic accounts of current scientific practices, which make it difficult to sort things out in these debates. In this section, I will not engage with the question of whether the transformation from the traditional conceptualization of what science means towards new conceptualizations is “real” or is only due to political rhetoric. I will address this issue primarily as a *passage of perspectives*—that is, as changes in the way in we look at “scientific” knowledge production. Thus, the question is, what aspects, features and characteristics of “science”—a term that is itself at stake in this debate—are emphasized in current discussions? What societal role is ascribed to science/research—which is an important feature of this debate, as the social role of science defines science as much as accounts that remain purely self-referential. However, when looking at the passage of perspectives of science/research, this does not mean that these necessarily remain on the level of language while scientific practice goes on unaltered. The new imaginations of what “science” today is in relation to society provide powerful narratives and frames, which science policy on all levels—from supranational governance down to the management of scientific institutions—makes use of, and which thus also shape the “realities” of research practices. However, these links and impacts are less causal and less direct than often imagined by science policy, and what these narratives actually do in research practices must not necessarily be coherent with larger science policy concepts.

Why do I engage with these “changes” and “shifts” in our views on science/research, particularly when this thesis is on an ethical discussion between lay participants and researchers in an engagement setting? Centrally, as addressed already in the introduction, public engagement and ethics are two different and rather new ways in which society aims to deal with the impacts of science/research on society. However, participation and ethics are not only compensation mechanisms for the consequences science/research have on society. On the contrary, both public engagement and ethics do something with science/research. For example, a new way of talking has to be appropriated by those in research who take care of science communication; research proposals have to anticipate ethical concerns and at least rhetorically deal with them; and, often, certain research fields, such as human embryonic stem cells or GMOs, are avoided if public opinion does not seem to favor these issues. However, the question is if and how the new regimes of science in society penetrate what is thought to be the core of scientific practice—the work in the laboratory. A second reason that these changing concepts are of importance is an empirical argument: It was addressed by both the lay participants and the researchers at the Round Table very extensively. More general narratives of what science/research are, and of their role in society, turned out to be crucial resources for both groups of participants to evaluate particular incidents and cases, and provided a narrative framework for assessing their normative dimensions.

In this chapter, I will engage with the broad lines of the debates of the transformation of science/research in society. In short, the shifting roles of science in society are described as a confrontation between two models of science/research: The somewhat "old" or "classic" model of a "basic science", which is only concerned with knowledge production for its own sake, which seems not immediately concerned with "application", which is committed to internal rules and norms and which primarily takes place in academic contexts (mode 1 science); and a "new" model of research, which is concerned with the solution of "practical" problems by embracing the technological aspects of knowledge, which seeks to be more inclusive with regard to values coming from society and which transgresses academic contexts by the fact that it can take place virtually everywhere (mode 2 knowledge production). While these two models of science/research are often seen as supplementing each other, and thus are not seen in a relation of competition, many accounts of this issue suggest that the new model is replacing or should replace the old one. I aim to make visible major ambiguities in they way we view science, which are often a source of conflict within the science community as well as between science policy and the public. The consideration of these ambiguities is crucial for understanding the debates at the Round Table, as many debates and conflicts emerged from different understandings of what "science" is. While I will not specifically engage with each of these dimensions in the following empirical sections, the participants have addressed them all in one or another. I will then discuss one of the latest accounts in European science policy—*Frontier Research* (European Commission 2005a; see also European Commission 2004a). The term is an interesting case, as it seeks to reconcile many of the differences between mode 1 and mode 2 knowledge production.

The second part of this chapter will engage with the question of "responsibility" in and of science. The concept of responsibility allows the tracing of transformations in science-society relations in particular instances. I engage with this term in more detail because it is one of the key buzzwords in science policy to account for changes in science-society relations. Furthermore, the participants at the Round Table placed the issue of "responsibility" high on the agenda, and thus a closer look at how responsibility is discussed in policy seems rewarding.

2.1 Two different visions of science/research: From mode 1 science towards mode 2 knowledge production?

The first tension between mode 1 and 2 is on **outcome vs. process**. Mode 1 science puts special emphasis on the *outcome* of research, while the production process often remains obscure and black-boxed. Typically, the focus is on textbook knowledge and ready-made facts that account for “science”. As the focus is on the outcome, the knowledge is purified of its local production conditions and claims universal validity. The other perspective on “science” highlights its procedural character, as *research*: Science is a special kind of craft that takes place at particular sites. It needs people, instruments, money, buildings, experiences and much more, all of which must be brought together in a particular and carefully managed way, so that in the end, after long negotiations, a more or less stable “fact” is produced that the scientific community considers as true—at least for a certain time (Latour 1987). In mode 1 science, the social acceptability of true knowledge is not of importance. It makes no difference if the public acknowledges a scientific fact or not. While the provision of facts is still a major feature of science communication, for example in the media, there is also increasing interest in looking behind the closed doors of the laboratory, as the ideal-typical site of knowledge production, in order to make more transparent what researchers actually do when they produce knowledge. This renewed interest in the process of knowledge production, however, is partly triggered by science policy, as it is hoped that trust in science can be (re-)established by making science more transparent to the public.

Another difference between mode 1 and 2 science regards the **venue of science** that is prioritized. In mode 1 science, the place of “scientific” knowledge production is primarily academia, and, even more ideal-typical, the university. For example, in Vannevar Bush’s seminal and influential report to the US president, which outlined the US research policy for many centuries after the Second World War, Bush stated that “The responsibility for basic research in medicine and the underlying sciences, so essential to progress in the war against disease, falls primarily upon the medical schools and universities” (Bush 1945). It was clear to him that the “natural” space of basic knowledge production was universities, colleges and research institutes. Industry, in his eyes, only made use of scientific knowledge, and was not seen as a genuine space of scientific knowledge production. In mode 2 science, the contexts in which knowledge is produced are much more diverse. Scientific knowledge production is not seen as limited to a particular domain, but can take place virtually everywhere. While the universities lose their monopoly in producing “scientific” knowledge, they still have a dominant role in mode 2 science. However, universities have to change their roles accordingly, through different forms of cooperation with non-academic institutions such as industry. Clear demarcations between inside and outside become blurred (Nowotny, Scott, and

Gibbons 2001). Another change of perspective with regard to the question *where* science takes place comes from the notion of *Realexperimente.* These are experiments that leave the laboratory and are carried out in "society". *Realexperimente* intermingle intervention and knowledge production. Thus, in mode 2 the traditional innovation regime, from laboratory experiments to field tests to societal application, is shortcut as the laboratory is expanded to the whole society (Groß, Hoffmann-Riem, and Krohn 2005).

The **innovation regimes** between mode 1 and 2 science differ in several ways. The innovation concept of mode 1 science rests on a strict separation between "basic science" and application. For the outline of the mode 1 innovation concept, I will refer to the already classic account of Robert K. Merton. He wrote his analysis of the social and normative order of science during the approach of World War II and the rise of the racial politics of Nazi Germany, where he observed that scientific significance was subordinated to the utility of knowledge, "with its depreciation of the theorist and its glorification of the man of action" (Merton 1938, 323). He observed anti-science movements that only valued "beneficial" applications and devalued knowledge production for its own sake. However, he argued that what he called "pure science" was an important source of societal advancement. He regarded knowledge production for its own sake not as a dispensable cultural activity, but rather as the absolute necessary condition for the possibility of later technological applications, because "[e]xperience has shown that the most esoteric researches have found important applications" (Merton 1938, 324). In a similar fashion, Vannevar Bush argues that "Basic research is performed without thought of practical ends"; however,

> It creates the fund from which the practical applications of knowledge must be drawn. New products and new processes do not appear full-grown. They are founded on new principles and new conceptions, which in turn are painstakingly developed by research in the purest realms of science. (Bush 1945)

Bush and other relevant actors established a linear model of innovation by linking applied research with basic science and introducing the concept of innovation as a linear flow: Basic science provides a pool of knowledge, concepts and ideas, which are taken up by applied research by working on a concrete "solution", and which are then further exploited in development for commercial use and societal diffusion. This model was a commonly shared fiction (Godin 2006), which serves the interest of basic researchers as it attributes social meaning to their way of knowledge production without contesting the researchers' authority and autonomy over their domain. The claim behind the importance of "basic science" in the innovation process is that it is so useful *because* of its rather strict renunciation of applications and technologies.

The perspective of mode 2 science on innovation is a different one, as the clear distinction between basic knowledge production and successive steps towards application is given up in favor of a more *integrated innovation* model. In this, the context of application is present from the very beginning—in fact it is seen as the main driving force that initiates the research process, and knowledge is not produced until all interests are accounted for. Thus, knowledge in mode 2 is produced in the *context of application* (Gibbons 1994; Nowotny, Scott, and Gibbons 2001). Research in mode 2 always has an agenda towards society, and must be "marketable" in some way (to industry, governments or defined user groups). In particular, research and economy are seen as more integrated in mode 2 science. It is not only that research has to adapt itself to a market; the economy must also be more oriented towards "knowledge" and research. This is suggested in the EU's Lisbon Agenda by the concept of the "knowledge-*based* economy" (Godin 2003) which has developed further, becoming a "knowledge-*driven* economy", "emphasizing the fact that the current contribution of knowledge is very much as the dynamo of our economy" (European Commission 2004b, 5). While the linear model is seen as in need of change towards an integrated model of innovation, the question is who is integrated in the innovation process, and under which conditions; and who is excluded from this process?

The two modes of knowledge production also have different stances regarding **uncertainty.** In mode 1, scientific knowledge production is seen as a remedy and panacea for all kinds of uncertainties that are "externally" imposed on society or caused by it. Within this regime of uncertainty, science is seen to "insure our health, prosperity, and security as a nation in the modern world" within the overall narrative of progress (Bush 1945). While this citation of Bush's *Science—The Endless Frontier* of 1945 suggests that this is an outdated narrative which was valid in historical times, the idea that science as innovation is the appropriate means to address social uncertainty is still frequently encountered in strategic papers on science policy (see for example Rat für Forschung und Technologieentwicklung 2001). Especially in the Austrian context, but also on the European level, R&D and innovation are only marginally identified as a source of uncertainty but rather as its remedies. In mode 2 science, uncertainty is a narrative to describe the overall state of society (Beck 1992) and is not limited to society as distinct from science. Much more, "science" is regarded as a central source of uncertainty. For example, while Chernobyl, BSE and GMOs still represent a very small proportion of "science", these perceived crises have contributed to an increase the general sense of uncertainty related to science and technology, and facilitated skeptical and critical positions towards technosciences and scientific expertise. Hence, it is concluded that the management of uncertainties must not be left to scientists alone; rather, new social innovations are needed to enable individuals and groups to cope with the uncertainties which are expected to arise in *mode 2 society* (Nowotny, Scott, and Gibbons 2001; Gibbons 2000). However, uncertainty is not a state society can overcome; the tendency towards uncertainty and complexity is regarded as irreversible.

Mode 2 knowledge production, then, does not simply mean that science is or must be rethought, but also that society has undergone changes towards a mode 2 society. Both science and society are not thought of in separated categories, but are "transgressive" (Nowotny 2000; Nowotny, Scott, and Gibbons 2001). Thus, mode 1 and 2 science and society imply different **conceptions of citizens and the public**. As this is discussed in a later section (see chapter 3.1.), I will be brief here. In mode 1 science, because of its exclusive authority to speak truth to society, the public is seen as being constantly in need of "proper" education, that is, in a "deficit" of adequate information. For example in Merton's (1973) concept of science, because of the complexities of research, scientific theories seem "esoteric" to lay people. Thus, Merton imputed the tendency to follow totalitarian spokespersons to the people, as they are seducible by the more common-sense explanations of political ideologies. In the mode 1 imagination of citizenship, the public is not seen as having its own capacity to deal with science in a rational way. In mode 2 science and society, the public is on the one hand regarded as capable of dealing with science and technology (even if they are "ignorant", see Michael 1996) in a reasonable way based on their experiences with science, and, on the other hand, lay are discovered as valuable resources of a kind of knowledge that contributes to a greater social robustness of scientific knowledge (Epstein 1995). The transgressive nature of mode 2 knowledge production is demonstrated by its transdisciplinary character, in which society participates in the production process (Nowotny, Scott, and Gibbons 2001). Citizens have the chance, or even the responsibility, to participate in the production, implementation and governance of knowledge, and thus contribute to a more "socially robust" knowledge. However, modes 1 and 2 each have their own normative visions of how citizens should deal with science/research.

Further differences in the perspectives on "science" in mode 1 and mode 2 can be identified in the **moral governance** of science, that is, what values are employed to govern individual scientists, the scientific community and relations with society. Mode 1 science is concerned nearly exclusively with ethical self-regulation that does not consider outside moral values. The seminal example for this moral governance of science is the *scientific ethos* as described by Merton (1973). In his sociological analysis, Merton provided four norms which are internally shared by the community and which guide the behavior of researchers. These norms are: (1) universalism: science has an impersonal character because validity claims of scientific knowledge do not depend on individual and social characters of its advocates. Thus, science demands access to research beyond race and class thinking. (2) Communism: science is seen as a collaborative endeavor, and knowledge is regarded as public domain. Scientists can claim symbolic acknowledgement of the efforts, but not ownership of financial rewards. The norm of communism also demands open access to all knowledge. (3) Disinterestedness: the quest for new knowledge does not rest on individual interests. (4) Finally, organized skepticism: the "enlightenment"-function of science. This methodological as well as institutional command requires that the scientific community question claims on the basis of shared methods. The ethos as such is concerned with the question of what ethical principles scientists (should) follow in order to ensure that true knowledge prevails and is not compromised by personal attributes of the scientists involved. The ethos, however, is not concerned with what happens with knowledge in societal contexts and how it is possibly (mis)used there. Hence, responsibility is only attributed to the knowledge production itself and not to the societal consequences of this knowledge. In mode 2 science, moral governance is more broadly conceptualized. In particular, the *institutionalization of ethics* (discussed in a later chapter) on many different levels reflects that an ethos that ensures the objectivity of knowledge does not suffice. In mode 2, knowledge must not only be "true", it also must not violate existing societal moral norms, either in the production of knowledge (animal experiments, the use of embryos in research, etc.) or in its later application (reproductive human cloning, etc.). To account for this changed perspective, a wide range of institutions that consider both social interests and scientific standards in the moral governance of science are created. Additionally, scientists are themselves "responsibilized" to critically engage with societal values and the consequences their knowledge might bear. Thus, in mode 2 science, responsibility is much more socially distributed between science and society. A clear division of moral labor is not foreseen in mode 2, while in mode 1 scientists only have to follow the scientific ethos while society is concerned with coping with the ethical implications that emerge from technologically applied knowledge. The emphasis of "basic research" in mode 1 knowledge production, and the clear demarcation from applied knowledge, imply that responsibility for technological application can be attributed to "society". As mode 2 claims a transdisciplinary and thus more socially inclusive way of knowledge production, research has to be socially accountable from the start. However, what does it mean that "society" is better represented in mode 2 knowledge production? While science increasingly makes reference to "society" to legitimate research, in most of the cases only a few interests are actually represented. This raises serious questions of power and hierarchy with regard to who can be represented when scientists aim to be "social accountable" (Nowotny, Scott, and Gibbons 2001). Often, economic interests are better "represented" than wider public concerns of groups that lack the ability of being heard.

In mode 1 and 2, different **models of success and failure** apply, and thus the means of constructing, assessing, justifying and managing "quality" differ. In mode 1 science, the most important criterion for quality is acknowledgement by the scientific community for instance in peer review. The assumption is that only peers have the appropriate expertise to allow for an assessment of the quality of a scientific contribution. "Success" means having a number of recognized publications that have been approved by the community as contributing new insights to the already existing body of scientific knowledge. In mode 2 knowledge production, models of success and failure often become far more complex and even contradictory. While peer review remains a relevant criterion—for example with the talk of "scientific excellence" that has become seminal over recent years—the knowledge produced must also be "socially accepted" and "socially robust" in the context of its application. Hence, not only "peers" decide on the success and failure of a contribution, but also a range of other stakeholders that follow different rationales. For example, "successful" research is increasingly required to communicate with the public or produce marketable products. One can be quite successful with regard to these two aspects without gaining acknowledgement by the scientific community in the peer review processes. Moreover, success in the media and in public communication is sometimes regarded as negative for the scientific credibility of a researcher (Weingart and Pansegrau 1999). The same holds for those who are perceived to sell out science to commercial interests. On the other hand, particularly in biology, relations with industry are crucial in order to acquire funding. Hence, researchers are required to balance carefully between selling themselves and selling themselves *out*.

As models of success and failure are different between mode 1 and 2, so is the **occupational image** researchers have. The perspective of mode 1 emphasizes doing research as a *vocation*, while in the mode 2 perspective, conducting research is regarded as a *profession*. The tendency towards science as a profession can be traced in the increasing orientation of university curricula towards labor markets and not only towards the production of academic scientists or intellectuals. Hence, in mode 2, career perspectives are regarded as much more heterogeneous and diverse, while mode 1 has a rather restricted idea of career trajectories, namely those of academic scientists. Of course, there are huge cultural differences in what is regarded as the ideal model of a scientific career, as Max Weber already mentioned (Weber 1995); however, there are shared codes within cultures for ideal-typical career patterns. This, however, does not suggest that in a mode 2 perspective career trajectories are less normative than in mode 1. The “inward calling for science”, as Max Weber termed it, is a narrative that has become less desirable and rewarding in a mode 2 environment. The places where science can be practiced as a profession have diversified, which is both an opportunity for researchers to realize individual careers and also an imposition, as “flexibility” is required, as a new norm, in order to be able to switch between different work environments. Thus, a range of additional abilities have to be learned by researchers. It does not suffice to be excellent at the bench; researchers are expected to appropriate managerial abilities, as “excellent” researchers are expected to assume leading positions. In mode 2, researchers have to reconcile diverse or even contradictory demands, for example, being both an “excellent” researcher and a charismatic manager. Researchers have to become “entrepreneurial scientists” (Etzkowitz 1998; see also Latour 1996).

The relation between mode 1 and 2 knowledge production is often seen as a change and transition from mode 1 towards mode 2 knowledge production, implying a historical order. A series of authors have described the transition processes and the emergence of a new kind of knowledge production with different labels. For example, Silvio Funtowicz and Jerome Ravetz described the emergence of a *post-normal science* that no longer ensures certainty but rather has to manage uncertainty (Ravetz 1999; Funtowicz and Ravetz 1993). Bruno Latour (1998) argues that we have two distinct traditions of accounting for science/research, and that we currently witness a transition process from the culture of science—to which he attributes certainty, being cold, straight and detached, and objectivity—towards a *culture of research*—which stands for uncertainty, being warm, involving and risky.

His argument is that the old culture of science can no longer be upheld. John Ziman claims that we are witnessing a transition from academic towards *post-academic science* (Ziman 1998, 2003). Hence, besides Nowotny and co-authors and the concept of “mode 2 knowledge production”, there is a series of other authors who claim that science is undergoing fundamental changes in the way “scientific” knowledge is produced.

As the number of advocates for a mode 2 knowledge production is large, so is the number of its critics. Their main argument is that the assumed “change” and “transformation” takes place only at a rhetorical level while existing “real” structures of how science operates are barely touched by these changes—and, if at all, only in a small segment of science. For example, Lenhard et al. (2006) argue that the demand for transdisciplinary knowledge production does not weaken disciplinary structures in science. Transdisciplinary research fields are seen as only emerging in “policy-related” fields such as Technology Assessment or climate research. While research projects appear transdisciplinary to the outside, they maintain disciplinary boundaries within the internal organization (Weingart 1997, 1999); hence, being “transdisciplinary” is primarily a political stance to attract funding.

Another critique of mode 2 is that it is not clear about its writing impetus; that is, do they provide a “realistic” description or a normative program? Terry Shinn argues that mode 2 advocates “work actively in its favour and seek to persuade others to think likewise” (Shinn 2002, 604), and, in particular, that *The New Production of Knowledge,* by Gibbons et al., (1994) “can be likened to political manifestos, whose expository form is rhetoric” (Shinn 2002, 610). Benoît Godin (1998, 467) states that this text “is first and foremost a political plea, mixing descriptive and normative perspectives”. He concludes that mode 2 is a “performative discourse” with which it is too easy to criticize the “old” system with the claims of the new modes of knowledge production that can easily be translated into normative policies. While I agree that the idea of change, as well as the assertion of newness of certain elements of mode 2, is exaggerated, particularly in the work of Nowotny, Gibbons and co-authors, to locate mode 2 only on a rhetorical level while the “real” processes go on unaltered falls short and underestimates the potential of “talk” to change practices in research. Mode 2 provides a set of norms which—taking each element alone—are of course not new. But in sum, they provide a convincing program that is subsequently implemented by science policy. Thus, mode 2 is not a description of what contemporary knowledge production is “really” about, but a rhetorical device through which decision makers in science policy increasingly learn to see science/research. This does not necessarily mean that institutions and researchers mechanically incorporate these norms and act accordingly, but that these perspectives are subject to a wide range of translation processes that may ultimately trigger changes in the practices of scientists. On the other hand, they may only adapt to the rhetoric, using it as an additional resource to justify what they are doing.

Empirically, based on our experiences in the "Let's talk about GOLD!" project, the researchers did not follow one particular mode of knowledge production. What "mode" they employed was highly dependent on the actual context of the discussion. When the issue was responsibility for the outcomes of the research, for example, the researchers tended towards the "pure science for its own sake" narrative in order to deflect ethical objections that might come from society. When the issue was on the legitimacy of public funding, the researchers often employed mode 2 arguments that hint at the potential societal utility of their knowledge. Different researchers held different priorities, but one particular researcher could also easily subscribe to both narratives. The same holds for the evaluation of the new forms of knowledge production: In particular, the more established researchers made a clear plea for mode 1 science, and regarded it as an ideal form of doing research. However, they did not hesitate to employ arguments that are attributed to mode 2 if they were urged to legitimate their research. The way the researchers at the Round Table accounted for "science" raises some question regarding mode 2. In particular, it is difficult to ascribe a particular chronology to different modes of knowledge production, that is, to say that mode 1 precedes mode 2. Both perspectives on scientific knowledge production may be present simultaneously. Rather than one replacing the other, different narratives of knowledge production were important resources for the researchers to legitimate their research to society, as well as to their own community and to themselves. Another characteristic is that the norm of mode 2 does not determine how these norms actually translate into practice. For example, the new production of knowledge holds "social accountability" as a key feature. My claim is that science today is more socially accountable by virtue of being more *aware* that "accountability", "responsibility" and "reflexivity" are important societal narratives towards which researchers have to position themselves in some way. However, there is a wide spectrum of ways for researchers to meet these demands. They may be met by giving up certain methods in animal experimentation. Or they may be met by ticking boxes in forms for ethics committees during the proposal phase of the research and then forgetting about it. Hence, it is—seen from a macro perspective—impossible to decide if researchers really have become more "reflexive" or if they have learned to respond to society in the language that is expected from them. Like "transparency" (Power 1994), "social accountability" has become an ambivalent term.

That the line between mode 1 and mode 2 knowledge is not as clear as some literature suggests can be observed not only in the particular engagement setting which was the basis for our research project, but also in research policy. A seminal example is the recent invention of **frontier research** in European research policy. This concept was introduced in 2005 by a report that sought to outline future research policy in the European Research Area, and is one of the key concepts of the European Research Council (ERC), a newly founded institution that funds research under the Seventh Framework Programme. The key feature of the ERC is that the EU now supports bottom-up research that hitherto has been solely under the authority of the member states. Frontier research takes up the two perspectives—mode 1 and mode 2—and aims to merge them into a single narrative. On one hand, frontier research takes up the notion of "basic research"; that is, it "stands at the forefront of creating new knowledge and developing new understanding", and possibly "achieving the occasional revolutionary breakthrough that completely changes our knowledge of the world". On the other hand, the "traditional distinction" between basic and applied research is abandoned: "With frontier research researchers may well be concerned with both new knowledge about the world and with generating potentially useful knowledge at the same time" (European Commission 2005a, 18).

The question is whether the label of "frontier research" is a rhetorical means to "sell" basic science to society by connecting it to its potential utility for technological development, or whether it is an attempt to import values that come from "applied science" into basic research, and thus to redefine basic research. However, interestingly, frontier research seeks to reconcile different notions of research rather than to replace one with the other. In my opinion, the creation of the ERC and its notion of frontier research further suggests that mode 1 and mode 2 cannot be ordered along a chronological timeline, but describe two distinct perspectives—each with its own politics—which are deployed depending on the context .

What is also interesting is that, with the introduction of "frontier research", a discussion has been started within European policy that has not arisen explicitly before. In prior European policy discourses on "science and/in society", no attempt was made to define "science" in a detailed way; "science" and "research" were mainly used in a generic sense, and their meanings were taken for granted. However, the introduction of the term "frontier research", along with changed funding practices, introduced a new debate into the European policy discourse; that is, about the distinction between "basic science" and "applied science", and thus the relation between "knowledge for its own sake" or as "cultural good" and the orientation towards the applicability and marketability of knowledge. The advocates are fully aware of this problem, as they hope to bypass it by the use of the term "frontier research". In a way, "frontier research" is used to shortcut wide-ranging and long debates between "basic research" and "applied research" which have characterized post-war science and which science policy was not able to reconcile in a sufficient way.

Frontier research also aims to compensate for the supposed shortcomings of European innovation policy, the so-called "European paradox". This is the assumption that EU countries play a leading role in terms of "excellent" scientific output, while lagging behind the ability to use scientific knowledge for industrial exploitation compared to the US (Dosi, Llerena, and Labini 2006). While seeking to reconcile economic and scientific demands, the question is how this may happen, beyond fashionable labels. On one hand, it is claimed that research must be socially and economic relevant. On the other hand, ERC only funds research that is "based exclusively on scientific excellence" … "The pursuit of excellence needs an autonomous space, where curiosity is the driving force, pursued by individual creative minds" (Nowotny 2006). As between mode 1 and mode 2, there are different models of success and failure that come together in the notion of “frontier research”. What might be successful and excellent research in purely “scientific” terms—a number of highly-rated publications—does not sufficiently provide the grounds for technological and economic success on the market. However, such differences are neglected in the paradigm of “frontier research”, and the question is what alternative rationales will be used in the evaluation of frontier research in the future.

There is a long learned tradition of having at least two different perspectives on research, which we have learned to regard as opposing. The latest attempt by the ERC to reconcile these different visions by introducing the notion of frontier research neglects that behind these different perspectives on research are diverse social interests which are not easy to reconcile. In recent years, increased attention has been paid to knowledge production that transgresses the notion of “basic research”; however, this does not necessarily mean that mode 2 knowledge production has replaced (or will replace) “science” in its classical sense. The ERC president, Fotis C. Kafatos, argued that “frontier research” is needed because “this is an essential part of our civilisation” and culture, and it is “a matter of competitiveness” (Kafatos 2007). Here, two notions of what science is—a cultural good and an economic resource—are brought together in the notion of “frontier research” by not even acknowledging that these two notions have been a source of conflict in many instances. The most problematic feature of frontier research is that “social” interests seem to be equated with economic interests. However, it remains to be seen if the European vision of “society” in the idea of “frontier research” is sufficiently accounted for. There may be alternative visions of “research in society” that have a different vision of a socially accountable research, beyond its economic impetus. These contain enough potential to produce wide-ranging conflicts in the European Union. It also remains to be seen if new models, such as the recently employed “frontier research”, which is high on the political agenda throughout Europe, are able to account for “research *in* society”. The question is whether it provides a new quality of research practice that is able to absorb the “speaking back of society”. Currently, only a few voices of “society” seem to be heard, in particular those of industry and economy.

2.2. Making science “responsible”: The moral governance of research

“Responsibility” has become one of the key buzzwords coined by science policy to describe and enforce transformations in science-society relations. I will engage with this concept for two reasons: First, with “responsibility”, science-society relations are problematized, and thus the transformation of the perspectives from mode 1 to mode 2, or, put differently, from “science” to “research”, are further illuminated (Latour 1998). Second, for the participants at the Round Table, the issue of responsibility was a prime token to debate science-society relations by putting forward mutual expectations, hopes and concerns. The term is flexible and vague enough to absorb a multitude of different meanings.

Theoretically, the term refers to a shift in governance regimes that seeks to move top-down regulation of science towards an uncoerced commitment of science with regard to societal values and interests that are not codified by legal regulations. Nikolas Rose (1999b) has described techniques of "responsibilization" as a key feature of advanced liberal democracies in general—thus, the call for responsibility is not a particular feature of science policy as such. Rose argues that with responsibilization "Politics is to be returned to citizens themselves, in the form of individual morality and community responsibility" (Rose 1999a, 11). Responsibility as "moral governance" aims to "replace top-down regulation and juridical accountability" (Shamir 2008, 13), and implies that social actors are expected to assume a reflexive moral capacity that goes beyond mere compliance with legal rules and "presupposes one's care for one's duties and one's un-coerced application of certain values as a root motivation for action" (Shamir 2008, 7). Hence, the call for science to be more responsible transcends the legal liability that is superimposed by state policy, and seeks to cause collectives to develop internal and informal codes of conduct that govern their relation to society in terms of a collective welfare.

However, the call for more responsibility in and of science must not be understood in purely moral or ethical terms, but as "ethopolitics" which "works through the values, beliefs, and sentiments thought to underpin the techniques of responsible self-government and the management of one's obligations to others" (Rose 2000). The demand that science has to act more responsibly towards society goes beyond the ethicization of science, that is, making scientific research subject to ethical concerns. Responsibilization has a clear political goal of *making communities*. With regard to science, one can observe a transformed perception from science as a "system" with certain "functions" in society (producing true knowledge, for example in Luhmann's system theory (Luhmann 1992) towards a "community" with "responsibilities" towards an imaged collective (society) that shares the same values. In ethopolitics, a simple orientation along the code true/false is replaced by a concept of science as a citizen in civil society (Irwin 1995).

An important feature of the responsibilization process is that the current call for responsibility goes beyond the notion of being penalized if negative consequences for society occur. This is typically the scheme of science understood as mode 1: The question is whether or not science can be held responsible for producing certain knowledge that may have harmful impacts on society *when turned into technologies*. Of course, this scheme is still present in contemporary narratives—and also in the accounts of the participants at the Round Table (see chapter 8.2). However, responsibility has been recast as a "positive" concept that seeks to align the interests of science with societal values. Hence, "responsibility" is a genuine political term that creates science as a civil "community" where "responsibilization" and "communitization"[9] go hand in hand.

This shift in the understanding of "responsibility" can be traced, for example, in the 2001 *Eurobarometer*[10] (European Commission 2001a)—a regular survey commissioned by the EU which aims to measure Europeans' attitudes towards diverse aspects of European policy—where people were asked whether scientists were responsible for the potential negative consequences of their research. The question was asked in two different ways: First, "As *members of society*, scientists *share* in the responsibility of any use—whether good or bad—of their discoveries" (emphasis added). The other version was: "Scientists are responsible for the misuse of their discoveries *by others*" (emphasis added). While the respondents were undecided about the second statement, nearly 70% agreed with the first. The agreement of more than two-thirds of the respondents with the first statement could be interpreted to mean that scientists are regarded as citizens who should "share" responsibilities with other members of society. The second statement, which highlights a more direct and individualistic version of responsibility, found fewer acceptances. However, "responsibility" was regarded as both an individual and a collective issue.

The inclusion of the topic in *Eurobarometer,* as well as the emphasis on social responsibility in other EU policy documents, shows that it has become an important science policy issue, particularly on the European level. For example the EU's *Science and Society Action Plan* states that "Because of their knowledge, researchers, research organizations and industry now have a particular responsibility vis-à-vis society in terms of providing scientific and technological information to Europe's citizens" (European Commission 2002, 11). Science is no longer regarded as an unproblematic field of knowledge production, but has become subject of a range of ethical and social concerns which include not only the products of science but also the very methods of conducting research. The question of the social responsibility of science and scientists can be read as manifestations of more general (public) concerns regarding the relations between science and society.

One place in research were we can witness responsibilization is the emergence of the language of (social) responsibility within codes of conduct for research that hitherto have been dominated by science-internal norms that mostly ignored relations to the wider societal environment in which research takes place.[11] "Good sci-

---

[9] But note that "communitization" here has a different meaning than in EU speech, namely, political means to make "communities" with certain rights and duties.

[10] The *Eurobarometer* surveys have been carried out by the EU since the 1970s. Science and technology were subject to a series of *Special Eurobarometer* surveys. For more detailed information, see http://ec.europa.eu/public_opinion/index_en.htm. The *Eurobarometer* reports can be downloaded at http://www.gesis.org/en/data_service/eurobarometer/

[11] For example, the German fraud case of the physicist Jan Henrik Schön (see, for example, Felt 2005), where the *Deutsche Forschungsgemeinschaft* (DFG) (1998) reacted with recommendations for good scientific practice. These codes of conduct, however, defined responsibility only as internal norms and did not relate the conduct of scientists to their responsibilities towards society.

entific practice" is now often seen not only as a prerequisite to scientific progress but also as a precondition for a trustworthy relationship with society.[12] Scientific misconduct is seen as violating a responsibility towards society,[13] and thus as endangering a fragile trust relationship between science and society.

While in codes of conduct the compliance with internal scientific norms itself is seen as a way to act responsible towards society, the *European Charter for Researchers* issued by the European Commission requires more direct feedback to society in the conduct of research. Under the heading of "Professional responsibility", the charter states that "Researchers should make every effort to ensure that their research is *relevant to society*" (European Commission 2005b, 11, emphasis added). The charter no longer regards research as a self-legitimizing activity of basic knowledge production, i.e., of "science", but rather as an enterprise which has to align its activities with societal demands and interests. The Charter further reminds researchers that they are accountable not only to those who provide the financial means but to the whole society: "Researchers need to be aware that they are accountable towards their employers, funders or other related public or private bodies as well as, on more ethical grounds, towards society as a whole" (European Commission 2005b, 13). However, one could argue that the orientation towards societal relevance is an intervention in the autonomy of research to define research subjects. Though, in another EU report entitled *Codes of conduct. Standards for ethics in research,* the author emphasizes the need for more social responsibility by scientists in order to re-establish a more stable trust relationship between science and society, increasing the autonomy and authority of science:

> It seems to me that scientists' acceptance of social responsibility … can serve to increase their power and support their autonomy. To the extent that a relationship of mutual dependence exists between science and society, science benefits from accepting accountability and the need to contribute, e.g. by receiving in return increased political, financial, and public support and trust. The latter strengthens science and increases its capacity for autonomous pursuits. Lack of support and—not least—lack of trust could be correspondingly harmful. (Evers 2003, 15)

"Social responsibility" is related to the establishment of a trust relationship between science and society. Public trust in science is seen as a prerequisite for conducting research in contemporary society, and thus the enactment of social responsibility by science establishes a new kind of trust relationship.

The new language of responsibility that increasingly becomes an integral part of the descriptions and accounts of how science and research (should) work, however, creates great challenges for those who have to practice responsibility in concrete situations. Research still faces an increasing differentiation of disciplines and sub-fields, so that "Individual scientists increasingly 'know more and more about less and less,' and thus can hardly foresee the consequences of their discoveries for

---

[12] Principles for good scientific practice, Universität für Bodenkultur (2004).

[13] Principles for good scientific practice, Karl-Franzens-Universität Graz (2004).

related fields, let alone the possible applications that could result from interactions with other fields" (von Schomberg 2007, 6). The particularization of the knowledge production process makes it difficult for researchers to see the "big picture", that is, the overall societal development to which the respective knowledge contributes. In a similar fashion, the *International Council of Science*[14] states:

> Knowledge and awareness about the consequences of scientific and technological development are now widely distributed in society. Scientists have no monopoly on evaluating the ethical implications of their work. Nor do scientists necessarily have the knowledge or capacity to forecast the full social implications of innovation. Given the distributed character of expertise on science and society, where does the social responsibility of science end, and what is the role of other actors in assessing the impacts and consequences of scientific and technological change? (International Council for Science (ICSU) 2005, 20)

This report argues, too, that scientists do not have the capacity to foresee the full range of consequences the knowledge produced might yield. Furthermore, the argument is that the boundaries of "science" are less clear, and thus it is difficult to assign responsibility. It is argued that knowledge is now more distributed in science and society. Thus, science alone should not have the role of assessing the consequences; other actors must be involved too. The latter argument is related to those put forward by Helga Nowotny, Michael Gibbons and co-authors on the co-evolution of science and society and new forms of knowledge production (Gibbons et al. 1994; Nowotny, Scott, and Gibbons 2001). In mode 2 knowledge production, research is increasingly interwoven with the context of application, so that responsibility, or, as Nowotny and co-authors term it, social accountability, is an intrinsic element of these new kind of knowledge production. The "new norm" of social accountability should remedy the shortcomings of the peer review system, and thus ensure the quality of the knowledge production process (Nowotny, Scott, and Gibbons 2001, 61). It is, for example, reflected in publication policies where authors are required to take responsibility for the full content of their papers, or in the requirement that all research involving human subjects needs to go through an ethical review process. The increasing number of dedicated research programs that often focus on issues regarded as socially relevant, too, exemplifies the intrusion of the responsibility and accountability narrative in science policy.

The growing awareness of the impacts of scientific and technological advances is also reflected by the composition of research teams working under mode 2 science regimes. Here, different academic disciplines and non-scientific professions work together on a commonly defined problem. These new kinds of cooperation, as well as other characteristics of mode 2 science, "increase the sensitivity of scientists and technologists to the broader implications of what they are doing" (Gibbons et

---

[14] The *International Council for Science* is a non-governmental organization representing a global membership that includes both national scientific bodies and international scientific unions. See http://www.icsu.org

al. 1994, 7). New forms of collaboration, for example with industry or self-help groups, introduce different values in the knowledge production process that have to be integrated into the daily research practice. In mode 2 science, notions of "social responsibility" are already incorporated into the knowledge production process, as the problem definition and the research process are carried out in the context of application. Within this knowledge production regime, scientists are no longer accountable only to their own community and its norms, but have to cope with and adapt to multiple value systems. Thus, in mode 2 the consequences of knowledge are much more directly enacted—and experienced—than in mode 1 science.

Mode 2 science is also described as a "socially distributed knowledge production system". Hence, responsibility and accountability also take place within this new institutional framework. This creates great opportunities as well as dangers. As the Mertonian idea of science neglects all responsibilities for social consequences, casting "science" as true and thus indisputable knowledge, the inclusion of a wider set of actors, and thus of interests, values, and preferences, creates the opportunity to consider possible consequences in the knowledge production process—that is, "upstream". However, the clarity of mode 1 with regard to the institutional structures and the actors and interests involved also makes it easier for the public to attribute responsibility. Mode 2, on the other hand, removes strict boundaries between public and private, university and industry, producer and user, etc. Sometimes, anticipated resistance is already incorporated in the knowledge production process, making it harder for opponents to reject applications later or to attribute responsibility to identifiable actors.

Different forms of knowledge production elicit different ideas about the "social responsibility" of science/research and researchers. In mode 1, the question of social responsibility is dominantly posed ex post. As discussed above, mode 1 science is characterized by a strict separation between pure and academic knowledge production and its application, as well as a linear process of innovation from early basic research to technologies applied in social contexts. In this innovation regime, responsibility only comes into play if socially applied technologies turn out to challenge current social values. If, on the contrary, knowledge remains in the realm of "pure science" and does not lead to any application, responsibility questions cannot be posed without contesting the strict boundaries between basic research and technological application as well as the autonomy of research. Thus, in mode 1, the question of social responsibility leaves untouched the epistemic core of basic knowledge production.

In mode 2 knowledge production, which is characterized by an interwovenness of research and contexts of application from the very beginning, the whole research process is accompanied by different aspects of "social responsibility". Researchers and societal actors alike who are involved in the transdisciplinary research process have to reflect continually on the impacts of their work on the societal contexts they work in. "Did we define our problem according to the social environment of its intended use? What are the impacts of our solutions for the societal contexts we imagine? Did we sufficiently acknowledge the feedback of our users and did we meet their concerns?" These are questions that accompany the ideal transdisciplinary research process as typically depicted in mode 2. Thus, the "social responsibility" of research has rather different faces in different regimes that govern science-society interactions. One need not even go far into the claims of a true transdisciplinary research process to observe the changing face of responsibility of science and scientists. For example, in branches of science that are considered central to national economies, fraud cases are often depicted as catastrophic for whole branches of industry in national economies, having the potential to weaken national welfare and competitiveness. See, for example, the Hwang fraud case in South Korea involving human embryonic stems cells. While on one hand attributed to the failure of the scientific peer review system and to the criminal energy of Hwang, the case moves beyond the science system as such by creating an uncertain environment for investors in South Korea's growing biotech industry (Gottweis and Triendl 2006). Here, it seems that scientists are not only regarded as responsible to their own community, but also—in particular branches—as committed to national economy and prosperity.

In line with the transformations science has undergone with the increasing importance of new modes of knowledge production, the "role" of the scientist has become multiple. Still, science as a vocation (Weber 1995) plays an important role in the self-descriptions of researchers. However, today they have to fulfill a range of additional roles. They have to align their actions according to the multitude of different interests involved, for example, those of the funding agency, those of the narrower scientific community, those of themselves as citizens, those of the representatives and advocates of science to the public, etc. This raises the question of the (in)coherence of the image the researchers present to themselves as well as to the public, and it raises some questions about researchers' identities that are not easy to answer. Claire Waterton argues that researchers are "learning to live with multiple versions of actively negotiated science-policy boundaries, many of which seem to have different qualities and make different demands on them as scientists" (Waterton 2005, 443). Coherence of self-image and role may be a desirable goal for researchers. On the other hand, "coherence" of one's own self-image might impede the advancement of one's career, as the current environment often demands "flexibility", the ability to switch between multiple repertoires in front of different "audiences". University researchers are increasingly asked to raise third-party funding and to cooperate with industry to create additional symbolic and financial value. The question is how researchers, both individually and collectively, can deal with new demands that question their traditional roles.

The different accounts that emphasize responsibility are part of what Rose (1999b) has termed process or "technologies of responsibilization". Researchers become "responsibilized" by becoming alert to the fragile economic setting they work in or to the danger of losing public trust and legitimacy. It is not the state anymore that has to take care of a stable science-society relation; scientists are asked to contribute a substantial part. This is also true for certain public participation and science communication exercises that allow scientists to engage directly with the public. Here, both scientists and citizens are made responsible for creating a mutual and more stable relationship.

Science policy, thus, increasingly demands from researchers that they take on responsibility for their relationship to the public. As the authority of "science" has been challenged over crises and controversies in recent years (BSE, GMOs, etc.), a more direct engagement of scientists with the public promises a more "authentic" picture of what science is, and may permit a more trustworthy relationship with society to be re-established (Brown and Michael 2002). Science in a situation of crisis, however, must be placed against the background of the 99.99 % of science and technology that goes on without such crises. However, society has become alert to science and its potential to harm—regardless of whether the crises are widespread or only a few exceptions. And science, too, has become sensitive to societal sentiments. However, the way, responsibility is performed by science policy has remained quite ambivalent so far. The rhetoric of responsibility in policy documents suggests that responsibility should be integrated into the mindset of researchers as an authentic motivation that does not need external control. On the other hand, science and scientists are *made* responsible by a range of technologies of accountability which have been implemented over the last decades; for example, ethics committees. Furthermore, the question is whether the social responsibility of research is a "language-game" (Hoeyer and Tutton 2005) to appease the public, or whether it necessitates changes in the production of knowledge itself.

3. Public engagement and ethics: Two approaches—one problem?

The starting point for many changes in the relationship between science and society, and the move away from "science" towards a more socially embedded knowledge production, has been a series of incidents that have gained a certain prominence in public debate, so that many observers—including those coming from STS—speak about a veritable "crisis" of public trust in science. The BSE outbreaks and conflicts over GMOs in agriculture are the outstanding examples of this crisis. What is interesting is that events that result in a crisis of public trust only concern a remarkably small proportion of "science"; that is, most of research and technological developments are done without being criticized. Furthermore, BSE and agricultural GMOs have often been interpreted as a crisis of public trust towards "science" in a more general sense, while in fact they have been a failure of political institutions to adequately deal with the risks. Hence, it is interesting that BSE or GMOs were not prominent incidents in the crisis of public confidence in politics, as politics is much more used to dealing with a lack of public trust. For "science", the few incidents assumed a kind of warning role. It was feared that particular and often legitimate concerns towards certain technologies ("science" in the sense of basic research was only marginally at stake in these crises) spread over "science" in general. Critical concerns have been interpreted in terms of a general crisis of public trust in "science".[15] On the other hand, events such as BSE and GMOs have been "seeds" around which other concerns could crystallize in the public discourse. Hence, when science politics and institutions talk about the "crisis of public trust in science", the issue may be something else which is obscured by the crisis narrative.

In this chapter, I will analyze two different approaches that aim to counter the assumed public crises over "science" and new technologies. These are "public engagement" and "ethics". From a distance, both seem to address the same problems and issues: What is the relationship of (public) values and concerns to emerging technologies? How should society deal with new technologies, particularly if they are in conflict with existing value systems? "Public engagement" and "ethics" both address these questions implicitly and explicitly. However, despite some similarities with regard to the more general aims of establishing a socially and ethically more robust knowledge politics, "public engagement" and "ethics" are quite different ways to address science-society relations.

---

[15] See for example the statement of the British House of Lords Select Committee Science and Technology (Third report, 2000), cited and discussed by A. Irwin (2001): "Society's relationship with science is in a critical phase. Science today is exciting, and full of opportunities. Yet public confidence in scientific advice to Government has been rocked by BSE; and many people are uneasy about the rapid advance of areas such as biotechnology and IT—even though for everyday purposes they take science and technology for granted. This crisis of confidence is of great importance both to British society and to British science."

I will first briefly discuss the discovery and construction of the public in the mid-1980s with the idea of *public understanding of science* and its critique. This framework, however, has lost its dominance in recent times, in favor of ascribing more active roles to citizens. This “participatory turn” is characterized by the acknowledgement of forms of knowledge that fall outside traditional forms of expertise, for example as highlighted by the term “lay expertise”. Another important feature of this participatory turn is that “participation” is regarded as a good as such in democratic societies, and thus to be promoted. I will then turn to the particularities of “ethics” in contemporary society, depicting an interesting contrast to lay involvement. While public engagement underlines the importance of citizens’ way of framing issues and promotes participation as a democratic virtue, both aspects seem absent in the current dominant form of ethics. Finally, I will inquire into the possibilities of ethics beyond its dominant expert-oriented form by drawing it together with “public engagement”.

3.1. From deficit to dialogue? Constructing citizens in science governance

The last two decades have seen increasing attempts to include the public in technoscientific decision-making. In the 1980s, the public was discovered as a relevant actor in the public policy of science and technology, although in a quite passive role (Felt 2003). This conceptualization took place under the label of *public understanding of science* (PUS). A seminal report that triggered a series of initiatives and framed the debate for a rather long time was a report commissioned by the Royal Society in the mid-1980s (The Royal Society 1985) that was also the eponym of this strand in science policy. The report concluded that science and technology play a major role in many aspects of daily life. Thus, the assumption of the report was that a "proper" understanding of science and technology was needed for every single citizen in order to cope with the challenges of modern society. While the report stated that public attitudes regarding science showed that the public had "considerable interest" in science, the "understanding" of science was seen as quite low in the general public. The poor understanding of science and its methods was seen as a danger on many different levels: First, it was seen as an obstacle for people in coping with their everyday lives. Second, poor scientific literacy was seen as endangering national economic welfare. And third, it was seen as problematic in terms of participation in democratic processes.

In the Royal Society report and the political activities that followed, the "public" was conceptualized in a very passive role. It was seen as in need of education in order to meet the requirements of modern democratic knowledge-society. The public was characterized by having a deficit of proper scientific knowledge, which prevented them from recognizing the benefits of science and technology. Termed as the "deficit model" of PUS, the process of communication between science and society was thought of as a one-directional flow of information from the scientific experts to lay people: Scientists, as the producers of genuine knowledge, make use of "translators", such as media, which popularize scientific knowledge. At the other end of the pipe, the public, as recipient, receives this knowledge, and, if the knowledge is appropriately incorporated, is then able to better handle everyday issues, as well as to better contribute to a political decision-making process.

On a political level, the deficit model became the dominant model of how politics addressed the relationship between science and society for quite some time. On the European level, the public understanding of science was subject to a range of large-scale comparative surveys, within the established instrument of the *Eurobarometer,* that initially aimed to measure the attitudes of the population towards European policy. In these quantitative surveys, people were asked about their factual scientific knowledge as well as their attitudes towards science and technology in general. This, then, is taken as an account for the "scientific literacy" of Europeans. The bottom line that runs through all *Eurobarometer* surveys on science and technology is that people indicate that they are rather interested in science, but feel poorly-informed regarding the matter. This is further confirmed by questions on scientific facts that the surveys posed, where people were quite often unable to provide the "correct" answer in scientific terms. The *Eurobarometer* surveys, then, assumed a strong relation between a high degree of understanding of science and a generally positive attitude towards science (Durant, Evans, and Thomas 1989).

In the wake of the deficit model of public understanding of science, which increasingly pervaded the way politics addressed science-society relations, a series of critics raised their voices and challenged many assumptions embedded in the classical notion of PUS. The Royal Society report, as well as the *Eurobarometer* surveys, contained specific, however often implicit, normative assumptions about "the public", "understanding" and "science". "The public" was mostly depicted as an amorphous and de-situated mass of people. "Understanding" mainly referred to a sender-receiver model of communication where "to understand" means to reproduce facts in a scientist's fashion. And "science", too, was perceived as a more or less uniform institution with a univocal message. Among the critics of the assumptions embedded in traditional PUS, Mike Michael (1992) analyzed how people perceived "science" within their everyday lives, and came to the conclusion that laypersons develop quite complex understandings of what science is, depending on the particular context. People often hold two different repertoires of what counts as "science" for them: "Science-in-general" is an entity of abstract knowledge, to which people see themselves in distinction, while "science-in-particular" is seen as open for mutual interaction and participation. People simultaneously are able to state that they are ignorant of science-in-general while having particular knowledge of a science-in-particular. "Ignorance" of science and scientific knowledge, however, must not be read as simply a deficit of knowledge and understanding that has to be compensated for by educational means—as political measures in PUS often suggest—but rather as a legitimate resource for people to cope with the complexities of a knowledge society (Michael 1996; see also Henwood et al. 2003). For many people, science seems like a distant entity that is barely experienced as such in everyday life.

This points to the fact that scientific knowledge cannot be regarded as an abstract and universal body of uncontestable facts, but is rather a situated assemblage of different experiences. Helen Lambert and Hilary Rose (1996) described, in a case study of people affected with familial hyperlipidaemia, how people develop situated understandings of (medical) science. In contrast to what the deficit model of public understanding of science suggests, to "understand" is an active process that aims to reconcile prior experiences with the knowledge provided in direct encounters with scientists. Brian Wynne (1996b) suggests that what is more important to people than a correct factual provision of scientific knowledge is the "institutional body language" of science and scientists when engaging with the public. Studying the encounters of Cumbrian sheep farmers with nuclear scientists and other experts from government institutions after the Chernobyl disaster, he showed that trust-relations between lay persons and experts cannot simply taken for granted, but trust-relations are far more complex and more continually negotiated than previously assumed: “trust and credibility are contingent variables which depend upon evolving relationships and identities” (Wynne 1996b, 20). Trust and credibility in expert systems do have a strong social dimension, which depends on the particular conduct of experts in their interactions with the lay public. Thus, Wynne opposes the rather simplistic notions of public trust in science present in the *Eurobarometer* surveys, where trust is often measured in a quantitative way. He shows that trust is not a question of numbers. but one of the quality of experiential relationships and local encounters between science and the public.

While the particular notions within the traditional concept “public understanding of science” were thoroughly questioned and juxtaposed with experiences in everyday life with a range of empirical studies, they were also criticized for their inherent assumptions of democratic politics. Beginning with the Royal Society report on PUS, up to recent governance papers on national and supranational levels, there is an assumption that true participation of citizens in democratic governance rests on appropriate knowledge of scientific and technological issues. For example, the EU’s *Science and Society Action Plan* states that “In a knowledge society, democracy requires citizens to have a certain scientific and technical knowledge as part of their basic skills” (European Commission 2002, 11). This approach, which became intrinsic to many science policy regimes after the public was discovered as a relevant actor in science policy in the 1980s, is criticized by Lévy-Leblond:

> For the requirement ... that people should be experts, or at least fluent, in science and medicine before giving their view about it, after all, is contrary to the basic tenet of our democratic societies. Democracy is a *bet*: the bet that conscience should take precedence over competence. We do *not* require an expert, nor even an ‘amateur’ level of knowledge in constitutional or criminal law before allowing citizens to use their voting rights or participate in a jury. Why should we be more demanding concerning technical and scientific matters? In other words, the problem we face is not so much that of a knowledge gap which separates laypeople from scientists, but that of the power gap which puts scientific and technical developments outside of democratic control. (Lévy-Leblond 1992, 20)

Lévy-Leblond argues that science—for reasons not provided—enjoys different criteria for participation than other domains. However, expert-shaped citizens contradict the basic principles of democratic societies, and he wonders why participation in science and technology requires special skills that are not required in equally important social affairs. However, in the practice of traditional public understanding of science, the expert-shaped citizen has never been realized. Despite the many measures through which science policy sought to “educate” the public to act and think like scientists, little has changed in the Europeans’ knowledge and attitudes towards science and technology. The assumptions of traditional public understanding of science, as well as the its enactment in practice, help to constitute the public as an “ignorant mass” (Bensaude-Vincent 2001), which in turn justifies leaving the governance of science and technology in the hands of experts and technocrats.

Despite the criticism traditional PUS model received over the years, this particular model of science-society interaction remained important in science communication. The linear model of science communication reinforced the authority of science as well as the role of a passive public as mere receivers of scientific knowledge. However, in recent times, science policy—beyond the recurrent emphasis on the scientifically literate citizen as a prerequisite for democratic participation and economic prosperity—has begun to experiment with different roles for the public, depicting them in a more active way. To a certain degree, the discourse of the citizen in need of education before being able to participate in democratic governance has been sidelined by alternative visions of the role of citizens that emphasize “dialogue” rather than “education”. In light of a series of critical incidents in public science policy, such as GMOs and BSE, the re-establishment of public confidence in science governance was a central goal for policy makers (Irwin 2001).

This language is particularly present on the European level, where the relationship between science and society is increasingly characterized by references to “dialogue” with and “participation” of the public, suggesting a “new partnership” between science and society (European Commission 2002). This new language is triggered by a more general crisis of governance that the European Union and its nation states are facing. The need for changes in the governance regimes are, for example, reflected in the EU’s *White Paper on Governance* (European Commission 2001b), where it is concluded that “many Europeans feel alienated from the Union’s work”. This alienation is seen as rooted in the ineffective action of the EU on a series of issues, and thus in a crisis of trust, credibility and the democratic legitimacy of its institutions. As a consequence, changes in the style of governance of the EU are seen as needed, in which “participation” and “involvement” of the citizen are important factors. Among other issues, the call for participation and involvement should also be reflected in the way the EU deals with issues of science and technology: “The EU’s multi-disciplinary expert system will be opened up to greater public scrutiny and debate. This is needed to manage the challenges, risks and ethical questions thrown up by science and technology” (European Commission 2001b, 33).

The call for dialogue and participation has manifested in a series of public engagement exercises throughout Europe. These have been carried out on local, national and supranational (e.g., “Meeting of Minds”[16]) levels. They can be categorized by the intensity of citizen participation; process or output orientation; overall embedding in a political process (“communication”, “consultation”, “participation”); the composition of the citizen group (“representativeness”); and many more parameters. As accounting for the full range of public participation methods that have been developed and deployed over recent years is far beyond the scope of

---

[16] See http://www.meetingmindseurope.org (accessed July 8, 2008).

this dissertation, I have to refer to the work of others who have more fully engaged with the issue (e.g., Felt, Fochler, and Müller 2003b; Rowe and Frewer 2005; Joss and Bellucci 2002; Steyaert and Lisoir 2005). In what comes next, I will highlight some central aspects of the so-called "participatory turn" in science policy and the issue of public participation in technosciences that represent a departure from the traditional models of public understanding of science that conceptualized the citizen as a lay person lacking expertise and thus unable to participate more in political decision-making processes on technoscientific issues. I will focus on two aspects of this new regime of science governance, because they contrast nicely with the dominant form of how ethics deals with the governance of science and technology—which I will discuss afterwards. First, the participatory turn brought forward *alternative resources of knowledge,* in particular those of citizens, increasingly considered as valuable and legitimate resources of knowledge and experiences. Second, not only the knowledge but also the *processes* of how a society comes to decisions on technoscientific issues were increasingly regarded as pertinent to the legitimacy of decision-making.

In traditional PUS, lay people were not recognized as equal partners in science communication. They were regarded as mere receivers and consumers of scientific knowledge provided by scientists and experts. The idea that lay people could hold particular knowledge that could be relevant in decision-making processes on technoscientific issues was completely absent. The case of the Cumbrian sheep farmers after the Chernobyl fallout demonstrated experts' ignorance of local and experiential knowledge, as well as the farmers' resourcefulness in dealing with the contamination problem in their own way (Wynne 1996a). This and other instances where lay knowledge and experiences collided with epistemic cultures of scientific experts led to the increasing contestation of scientific expertise. The UK (and elsewhere) BSE crisis is a pivotal example of the crisis of public trust in the ability of governments and experts to cope with problems. During the crisis, regulations were imposed "in haste and secrecy", as the UK BSE Inquiry Report noted. The public was not informed of the huge disagreement between experts about what to do. Many experts believed for a rather long time that BSE posed no threat to human health. Once additional data indicated the health risks of BSE, the authorities' assumption was that the public would react in an "irrational" way; thus, the officials in charge exhibited considerable mistrust towards the public. This in turn triggered the people's mistrust in the ability of experts and officials to tell the truth and inform them about the full extent of the crisis.[17] In the wake of the BSE crisis and in light of the upcoming GM controversy, the UK *House of Lords Select Committee on Science and Technology* reconsidered established modes of science-society relations. They concluded that "public confidence in scientific advice to Government has been rocked by a series of events." Public trust in science could no longer be taken for granted, but must be established in the first place by a greater "openness" of scientific institutions. The committee identified a "new mood for dia-

---

[17] http://www.bseinquiry.gov.uk/ (accessed July 8, 2008).

logue": "Today's public expects not merely to know what is going on, but to be consulted; science is beginning to see the wisdom of this, and to move 'out of the laboratory and into the community' to engage in dialogue aimed at mutual understanding." In order to practice the new mood for dialogue, the committee suggested a series of public engagement events, from focus groups to citizens' juries, to remedy the crisis of trust and the shortcomings of the then-current regime of scientific governance (all quotes UK House of Lords Select Committee on Science and Technology 2000).

The policy statements in the aftermath of the BSE crisis suggest that the assumed crisis of public trust was not ascribed to public ignorance of scientific knowledge, but to the very method of experts' knowledge production, and that the way it was entangled with decision-making processes was under heavy scrutiny. Expertise and experts were increasingly contested. Science was not longer seen as having one clear voice which speaks truth to power, and the image of the disinterested expert was shattered. The report suggests also a series of measures that involve the public more closely in decision-making processes on technoscientific issues. To a certain degree, lay and stakeholder knowledge was acknowledged as an important factor in dealing with large-scale technoscientific crises, and this new way of dealing—at least as it is suggested in the Committee's report—marked an important difference from the Chernobyl crisis, with quite different roles for lay people and experts. Also on the European level, different voices called for a "democratization of expertise." In an EU report (Gerold and Liberatore 2001) that is associated with the central *White Paper on Governance,* the authors also refer to the BSE crisis and the GM controversy, and state that expertise is increasingly contested. In order to counter this development, they argue for a pluralism of expertise, no longer to be seen as a weakness of scientific authority, but rather as a strength. They state that efforts to re-establish public confidence in governance "cannot be confined to 'educating the public': the very process of developing and using expertise needs to be made more transparent and accountable, and sustained dialogue between experts, public and policy makers needs to be pursued" (Gerold and Liberatore 2001, 2). In sum, the BSE crisis and the anticipation of the GM debate triggered a series of changes in the rhetoric and language of science policy. Expert knowledge and public concerns were no longer regarded as opposing each other, but had to be reconciled in some way. As such, the paradigm of the knowledge deficit was largely dismissed from the official language.

Apart from the discussion of the role of public knowledge and experience in large-scale crises and controversies, there has been another area where lay knowledge has been acknowledged, and in some instances has deeply changed the way research is conducted. In particular, patient movements around certain diseases have managed to put forward their concerns, experience and knowledge. For example, lay participation of AIDS activists has changed epistemic practices in biomedical research (Epstein 1995). Another case of participatory knowledge-making in the biomedical domain is the case of the *Association Française contre les Myopathies* (AFM, French Association to Fight Muscular Dystrophy), which "played a key part in the orientation of research, that is, in the definition of a research and innovation policy in the full sense of the term"(Callon and Rabeharisoa 2008, 239). Especially in biomedicine, there are a number of examples where organized lay citizens managed to challenge existing regimes of scientific expertise. These forms of lay knowledge, within their particular concerns and with regard to very specific domains of science, namely biomedical research for drugs, were able to exercise a great deal of influence.

Policy-makers, however, also see the activist character of lay participation as a problem with regard to the engagement of the wider public with science and technology. One of the bigger public participation exercises that followed the House of Lords' call for greater openness and dialogue has been *GM Nation?* in the UK. It has been the UK's largest experiment with public consultation, with numerous national and local events attended by more than 1000 people (Horlick-Jones et al. 2004). Alan Irwin (2006) has aptly discussed this case in light of the assumptions implicit in public participation. In the case of *GM Nation?,* the organizers have been concerned about the "capture" of the events by "special interests" and activists. They enacted special measures to avoid the participation of stakeholders who have already been actively involved in the discussion. Afterwards, the event was criticized for failing to address a "wider array of people": "The suggestion is that by circumnavigating the usual stakeholders, it is possible to tap into a less prejudiced (and more 'representative') public opinion" (Irwin 2006, 312).

There is a general acknowledgement in science policy that citizens should be involved in debates around science and technology. However, what remains rather unclear are the expectations of citizen involvement regarding the question of what the citizens can contribute to the deliberation process. If lay knowledge is sought for and appreciated in public engagement exercises, whose knowledge is meant? In the case of *GM Nation?,* organized citizens with a clear opinion and stake were excluded from the discussion process, which aimed instead to address citizens who were "innocent" and more "representative". The marginalization of "activists" with already existing views, and the prioritization of the "open-minded" citizen, suggest that science policy-makers assume that the open-minded citizen is rather susceptible to the views of experts and thus that conflicts can be avoided. However, there is no clear boundary between "activist" and "disinterested" citizens, and the question is, who can legitimately decide whether specific citizens are "allowed" to participate in public engagement exercises?[18] Thus, on one hand the "participatory turn" abandoned the old paradigm of citizens as mere consumers of scientific knowledge and facts, and suggested that lay people hold knowledge, experiences and values relevant for the governance of science. On the other hand, the public

---

[18] In STS, Alan Irwin (2006) has particularly critiqued such a regime of participation. However, Robert Evans (Evans and Plows 2007), based on his and H.M. Collins's model of expertise (Collins and Evans 2002), advocates the advantages of the involvement of "disinterested", that is "non-expert" and "non-activist" citizen in public engagement exercises. By "disinterested" the authors mean "lack of engagement and detailed knowledge". The question is how to enroll these citizens and demand some commitment to the engagement process if the requirement is complete absence of engagement.

cannot be considered as a mass of “average” people sharing the same attitudes, and it has not yet been determined how different forms of knowledge in the public relate to each other and to scientific expertise.

Another idea of many organizers of public engagement events is that these settings are politically neutral spaces in which different forms of knowledge and experience can mutually engage with each other. Thus, the setting itself would not influence what kind of knowledge would prevail, but rather allow each argument to stand on its own. Brian Wynne frequently emphasizes that “invited” public involvement often imposes a normative frame on public issues, and thus influences what knowledge is more acknowledged in debates and what falls outside the scope of the debate (Wynne 2001, 2003, 2007). An example of this “framing work” of experts is the framing of ethical aspects of emerging technologies with accepted formal principles that have been already enacted in a wide range of issues; for example, “informed consent” or “privacy” as the standard repertoire of ethicists’ responses. The prevalence of formal argumentation in ethical debates leads to an exclusion of other ways of knowing which are not germane to this kind of thinking. In this regard, Sheila Jasanoff (2003a, 397-398) argues that “public engagement is needed in order to test and contest the framing of the issues that experts are asked to resolve. Without such critical supervision, experts have often found themselves offering irrelevant advice on wrong or misguided questions.” Thus, lay involvement contributes to the legitimacy of expert advice as well as to the social robustness of expertise. What is most important in public participation exercises is thus not that citizens “contribute” to the expertise of experts, as suggested for example by Harry Collins and Robert Evans (2002), but that they challenge the basic assumptions that guide experts’ way of thinking; for example; the role of science and research in society; how innovation processes work and should work; the role of governance, regulation and politics in the conduct of science and so forth. The task of citizens to challenge expert framings, thus, goes well beyond the idea that citizens should contribute to “technical decision making” as suggested by Collins and Evans, because this stance is already a narrowly framed concept of what is at stake when lay people and experts meet.

The second aspect of the “participatory turn” is that public participation can be regarded as an “exercise in democracy” as such. Beyond the discussion of what knowledge and experience lay people may contribute to the governance of science and technology—however closely connected to it—is another narrative of public participation, present in the discourse as well as enacted in practice. Public participation is often justified not with reference to the matters at stake, but as an intrinsic value of democratic orders. Put bluntly, the emphasis of a more participatory culture of governance aims to construct citizens, with regard to their participation in science and technology, as “scientific citizens” (Horst 2007; Irwin 2001; Michael and Brown 2005; Michael 1998). Thus, public participation events can be understood not only as engaging with science and technology as such, but also as “exercises in democracy”.

The exercise character of public participation becomes visible in the way public policy makers deal with it, especially on the European level. On one hand, public participation is held high in a number of EU policy documents. The *White Paper on Governance*, for example, asserts that the "quality, relevance and effectiveness of EU policies depend on ensuring wide participation throughout the policy chain – from conception to implementation" because "legitimacy today depends on involvement and participation. This means that the linear model of dispensing policies from above must be replaced by a virtuous circle, based on feedback, networks and involvement from policy creation to implementation at all levels" (European Commission 2001b, 10-11). This vision of the role of participation in governance is all-embracing, and concerns not only matters of (dis)agreeing with ready-made decisions but, in particular, "upstream" involvement in the conceptualization of issues and problems. The EU's *Science and Society Action Plan* (European Commission 2002) argues in a similar fashion, and suggests a greater involvement of the public at all stages of science and technology.

This narrative of public participation is, however, in contrast to the role of the public in light of innovation policies. For example, the central *Lisbon Agenda* of the European Union clearly aims to make the EU the "most dynamic and competitive knowledge-based economy in the world". The so-called Aho Report further specifies European innovation policy by emphasizing "the need for Europe to provide an innovation-friendly market for its businesses", because our economic "way of life is under threat" (Aho 2006, VII). Within the regime of economic innovation, the role of citizens in science governance has quite a different tone than for example in the *Science and Society Action Plan.* Public participation and dialogue are not addressed anymore; rather, what is needed is "a cultural shift which celebrates innovation, using the media and other means to *encourage citizens to embrace innovative goods and services*" (Aho 2006, 24, emphasis added). The envisioned "true dialogue" in the *Science and Society Action Plan* is abandoned in favor of the promotion of innovation to the public as a prerequisite for a market economy. The question is how the envisioned creation of "an innovation-friendly environment", in which public debate and controversies hardly have a place, relates to the tenets of "dialogue" and "participation". This dilemma, however, has not been addressed so far, and often became obvious when these conflicting demands are translated into national policies. Particularly in the Austrian case, where the governance paradigm of the provision of an innovation-friendly environment is comparatively strong, participatory events often have the character of a staged exercise in democracy. One might argue that the innovation regime advocated by the Lisbon Agenda and the Aho Report only concerns applied technology production, and thus leaves untouched a possible "dialogue" with what we may call "basic research". However, the EU recently redefined their R&D policies, and is now also funding research, formerly known as "basic research" and now labeled "frontier research", which abstains from the

> traditional distinction between 'basic' and 'applied' research [which] implies that research can be either one or the other but not both. With frontier re-

> search researchers may well be concerned with both new knowledge about the world and with generating potentially useful knowledge at the same time. Therefore, there is a much closer and more intimate connection between the resulting science and technology, with few of the barriers that arise when basic research and applied research are carried out separately. (European Commission 2005a, 18)

While this report takes into account the transformation of science and research in light of new ways of knowledge production—for example, its transdisciplinary character and the more prominent links between what we used to understand as “basic” and “applied” research—the economic imperative of the new European research landscape is obvious. Consequently, the role of citizens—if addressed at all—is limited to that of followers:

> Rolemodel researchers created by a highly visible ERC grants system should contribute to *making science more attractive to the general public* as well as to students deciding whether to study science or engineering or pursue careers in research. Enhanced visibility will *thus raise the status of research* itself among policy-makers, politicians and the public. (European Commission 2005a, 36, emphasis added)

Particularly with regard to the more recent policy statements on innovation, the role of citizens has been redefined in terms of their contributions to building an innovation-friendly environment. How this model of governance relates to those which aim to be more “inclusive” with regard to the concerns of the public remains unclear. It is as if “two voices are struggling to be heard” (Hagendijk 2004, 46). Interestingly, the description of the public as suffering severe knowledge deficits, so present in traditional PUS regimes, has largely vanished in policy discussions. It has been replaced either by the language of “dialogue”, “involvement” and “participation”, or by the description of the public as in need of an increased “awareness” of the benefits of research and innovation for the competitivity of Europe in a globalized market economy. Thus, the wider educational impetus of science communication has lost its dominance and been replaced by other motives for the public to engage with science, that is, to exercise democratic virtues and to allow technology-oriented businesses to flourish for the sake of economic prosperity.

The exercise character of public participation also becomes visible in the way it deals with different methods of public participation. There is often thought to be a “neutral” means that can be easily displaced from one cultural setting to another, as well as be applied on a range of issues (Fochler 2007). For example, when Austria organized its first consensus conference in the field of biomedicine (2003, on genetic data), there were barely any local experiences of this method available. Despite the missing public responses in this exercise (Bogner, Puchrucker, and Zimmer 2004), the head of the organizing institution celebrated the event as a great success and advocated “to make the citizen conference to a fixed instrument in the treatment of explosive issues” (ORF ON Science 2003)[i] . The participants were provided with the vague promise that the organizer would try to promote the outcome of the citizen conference to political decision-makers. There were several features of this story that gave the impression that public engagement was understood as a mere exercise in "democracy". First, the participants were left in the dark about how their deliberations would relate to an overall political process—that is, where did the outcomes feed into, and what was the overall political purpose of this exercise? Second, the organizers were quick to note that this standardized way of practicing democratic virtues could be applied to a range of different issues, without considering that other subjects might necessitate different forms of engagement depending on the respective technology discussed (see also Felt et al. 2008). Third, despite the commitments, this event did not have a successor, giving the impression that citizen conferences in particular and public participation more generally are ticked off without joining a serious debate on what matters, in what ways, and with the participation of whom public engagement in science would make sense.

Summing up, I analyzed "public participation" along two dimensions: First, lay publics have been discovered as alternative sources of knowledge to those of traditional expert decision-making. However, the public does not share a common body of knowledge of and interest in science. The knowledge of lay activists in biomedicine might be quite different than the knowledge and experience envisioned by organizers of public deliberation events. These large-scale "invited" public participation exercises enact a specific vision of the scientific citizen that prefers the "open-minded" rather than the activist. The conflicting visions of the contributions of lay knowledge to the technoscientific decision process reveal the unanswered question of how lay experiences and knowledge relate to more expert-oriented deliberations. While some suggest that lay knowledge may supplement expert knowledge ("contributory expertise"), others foreground the role of lay publics in challenging expert framings that often neglect public meaning in debates. In my dissertation, through analysis of the discussion at the Round Table between lay members and genomics researchers, I opt for the latter view, following Brian Wynne and Sheila Jasanoff.

Second, the inclusion of lay publics in the technoscientific decision-making process (and beyond) may be grounded in the transformation in classical statehood over the last decades, marking a transition from state-oriented government towards "governance". In this new paradigm, participation is often celebrated as such in "exercises in democracy". While there are surely good reasons to welcome more open political decision-making, the question remains how these new forms relate to more top-down decision-making processes in particular areas. As discussed above, the current priority of research and innovation in order to render the EU the most competitive "knowledge-economy" in the world is seen as outside the scope of broader participation processes. Here, the public must be made aware of the importance of innovation in order to facilitate an innovation-friendly market. The question is who decides what issues allow public participation and what issues are not subject to public debate. The character of public participation as mere "exercises in democracy" has been further emphasized by the assumption that methods can be easily imported and exported, and thus that they are neutral means. My assumption for this thesis is, following my colleagues' work (Felt et al. 2008; Felt, Fochler, and Müller 2003b), that public participation must be sensitive to its contexts—the political cultures in which it emerges and is carried out, as well as the matters and problems at stake.

As I will show in the next section, "ethics" has provided a quite different answer to the way society should deal with the governance of science and society. The many questions that emerged around the discussion of public participation and the role of citizens in this process seem answered rather definitely by ethics.

3.2. Ethics in the public sphere: Moral expertise and lay 'ethical' knowledge

Alongside the increasing importance of public engagement in the policy process, "ethics" has been developed as a means to respond to societal challenges of emerging technologies, resulting in an "increasingly moralized politics of science policy" (Kelly 2003, 340). The moralization and ethicization of science policy hint at the fact that technological progress, especially with regard to the life sciences, is no longer regarded as unproblematic in terms of societal values. Former science policy regimes focused on technical risk management, but gradually the governance of innovation was foregrounded (Felt and Wynne 2007). Here, the point is that new technologies have to be assessed regarding their impact and consequences on society and its values, and that these impacts can no longer be treated only with regard to their technical risk. Technological issues are increasingly interwoven with moral issues, so that the one cannot be answered without engaging with the other. In part, blind belief in technological progress has become suspect. In anticipation of public conflict and rejection of emerging technologies, "ethics" as a means to govern societal values in relation to technoscientific innovation has been introduced on local and national levels of governance, as well as on the European level. Thus, today we are confronted with a range of ethical practices that intervene in and govern our interactions with technologies.

While ethics is regarded by politics as a panacea for a wide range of problems related to technosciences, and the "ethics talk" is widely spread in societal areas, the term "ethics" is also a cause of confusion, because it summarizes many different practices. Thus, I will first attempt to classify *different ways of ethical knowing* in society in order to more sharply accentuate the basic problem this dissertation is addressing. For this purpose I will distinguish between two different forms of ethical knowing in society: On one hand, *official or institutionalized ethics,* and, in contrast, *public or lay 'ethical' knowledge.* [19]

---

[19] A more specific definition of my understanding of ethics in analyzing the empirical data is provided in a later chapter (6.2). I also talk about "ethics" rather than about "bioethics", because "bio" as a term delimiting the application of ethics to a specific subject is difficult to de-

In *official or institutionalized ethics*[20] I include moral knowledge and normative practices produced by institutionalized bodies that are publicly authorized to speak ethically, and that therefore set norms that are authorative in some way. Official ethics can be broadly arranged according to its main functions: ethics as a provider of *advice* to politics in matters of science and technology that are regarded as ethically sensitive; ethics as a means of (self)-*regulation* in research, for example in institutional review boards; and *academic* ethics as a way of authorative knowledge production that aims to theorize moral behavior with the end of making normative statements of what "ought" to be. The boundaries between these different roles—providing advice, regulation, production of normative knowledge—are permeable, especially with regard to the actors that are involved in these three forms. However, these actors need not necessarily be academically trained ethicists, but may have a broad range of disciplinary backgrounds. "Institutionalized ethics" refers to the form of the discourse and the institutional structures in which this discourse is produced and made sense of.

Furthermore, the umbrella term of "official" ethics does not mean that there are no conflicts and differences within this area, and that it forms a homogenous epistemological and institutional body. Rather, conflicts are quite common; for example academic bioethicists often view political ethics committees critically. However, their common ground is the production of an authorative body of knowledge on moral issues that claims to transcend individual moral experiences. Institutionalized and professionalized ethics can be understood as an "epistemic community" following Peter Haas:

> An epistemic community is a network of professionals with recognized expertise and competence in a particular domain and an authoritative claim to policy-relevant knowledge within that domain or issue-area. Although an epistemic community may consist of professionals from a variety of disciplines and backgrounds, they have (1) a shared set of normative and principled beliefs, which provide a value-based rationale for the social action of community members; (2) shared causal beliefs, which are derived from their analysis of practices leading or contributing to a central set of problems in their domain and which then serve as the basis for elucidating the multiple linkages between possible policy actions and desired outcomes; (3) shared notions of validity that is, intersubjective, internally defined criteria for weighing and validating knowledge in the domain of their expertise; and (4) a common policy enterprise—that is, a set of common practices associated with a set of problems to which their professional competence is directed,

---

fine. In practice, ethics bodies sometimes explicitly abstain from using the term "bio" to describe their field of action in order to be able to address a wider range of issues.

[20] The term "official ethics" is derived from Sheila Jasanoff (2005). Susan Kelly (2003) and John H. Evans (2002) use the term "public bioethics" for the same matter. I rather prefer to use "official ethics", since "public bioethics" could be misleading in terms of the aim of this dissertation to analyze how members of the *public*—laypersons and scientists—deal with ethics in a public engagement setting.

> presumably out of the conviction that human welfare will be enhanced as a consequence. (Haas 1992, 3)

The claim of official ethics is that its knowledge follows a certain kind of epistemic rationality that often serves to exclude other forms of ethical thinking. Thus, official ethics claims that the engagement with morality necessitates a special kind of expertise that is not accessible to those lacking a certain kind of professional education. Everyday moral experience is seen as insufficient in order to be a member of the epistemic community of official ethics. It demands a special kind of training and education, either in ethical theory (moral philosophy, theology) or in a discipline to which ethics refers regarding its subject (biomedicine, genetics, etc.). Thus, official ethics constitutes an interdisciplinary field of experts that shares a common language and rationality (Evans 2002). Of course, the involvement of experts and the use of expertise in modern governance is nothing new, and the relations between expertise and democratic orders have been widely discussed in social science and politics (e.g., Wynne 1992, 1996a, 2001, 2003; Collins and Evans 2002; Gerold and Liberatore 2001). The newness of official ethics lies in its claim that societal values that govern everyday practices cannot be appropriately addressed and promoted, negotiated and defended by those who hold these values, but need the authority and guidance of experts in order to legitimately subscribe to certain moral principles. While morality is seen as a common property of all human beings, the reflection on morality is seen as demanding a special kind of expertise. Official ethics gets its justification from the belief that there is something like *"moral expertise"* that provides "better" knowledge and arguments than a "normal" citizen could provide.

The assumption of moral expertise is that moral experts not only provide moral opinions ("X is wrong") but also give reasoned arguments that are superior to subjective opinions; that is, "moral judgement is a reason-governed activity" (Crosthwaite 1995, 370). In ethics, the reason-guided engagement with ethical issues is often juxtaposed with a moral decision-making process that is based on "counting" different moral opinions: "Moral decision-making at the social level could be no more than counting votes. … I want to reject the idea that moral decisions should be reached by counting heads, even given democratic values" (Crosthwaite 1995, 370, 378). Thus, "moral expertise" derives its value from the strict boundary between those who are regarded as simply holding certain attitudes towards bioethical issues and those who are seen as able to provide reasoned arguments for their positions. While such a difference may make sense, it becomes problematic when reasoned reflection is only ascribed to professionals in ethics, while members of the public are seen as only holding moral positions without being able to provide reasons for them: "there is surely a difference between what the general public is able to say about complex moral issues and what professional philosophers are able to say (as is evident in the professional journals)" (Crosthwaite 1995, 371). Here, the ways of ethical knowing of the public are subordinated to the body of knowledge of learned ethicists, and thus, a hierarchical difference is constructed between these two forms of knowledge. It is rather easy to play this game, because institutionalized ethics has established mechanisms of communication within and outside the community. By referring to journals, ethicists can easily "prove" that their knowledge is much more sophisticated than that of "the general public". The latter only exists here as an undifferentiated mass that has no voice of its own but is only heard through opinion polls. This frames the responses of the public in a particular way. Of course, the account of the cited ethicist is not representative of the opinions of all ethicists on this issue. Some of them are rather critical regarding the claim of moral expertise of professionals (Powers 2005). However, the practice of official ethics as the dominant mode of how ethics is performed in society hints that the presumption of the existence of moral expertise is widely institutionalized, as for example the provenience of members of ethics commissions demonstrates.

It is far beyond my scope to analyze the full range of reasons why today the existence and legitimacy of moral expertise is widely accepted, and is the dominant way ethics is enacted in society. However, it is relevant to highlight the fact that, in spite of the contestations expertise is facing today, ethics has been established as a largely uncontested field of expertise over the last decade. The move towards the democratization of expertise as addressed in the previous section does not seem to have touched "official ethics". The dominance of official ethics led to the failure to consider that a wider range of members of society, such as citizens and stakeholders, could hold ethical arguments that are relevant to policy-making. The subtext of official ethics is that "the public" is not sufficiently able to provide a reasoned articulation of "societal" values. Considering this argumentation, it seems interesting to ask to what official ethics is referring to when it talks about "common" or "societal" values, because seemingly they do not mean the moral attitudes of citizens. Thus, investigating to what extent members of the wider public are able to provide substantial arguments on ethical issues has not been on the agenda of official ethics. Even in social sciences such analyses have been rare.

In order to consider the possibility that the public has a repertoire of well-reasoned articulations of the ethical issues of technosciences—and not just moral opinions that only discriminate between right and wrong—I will use the notion of *public or lay 'ethical' knowledge.* This means the way ethical non-professionals—citizens and scientists alike—articulate value judgments based on explicit reasons and justifications for moral assumptions. My basic assumption is that non-professionals in ethics are able to provide reasoned articulations of ethical problems that concern technoscientific developments. Its "reasoning", however, often remains invisible, as lay 'ethical' knowledge is mostly a matter of non-verbal practices and ad hoc talk that is not made manifest in the form of texts as official ethics is. That official ethics is considered "more reasoned" than lay engagement with ethics is the effect of particular practices of official ethics that strongly focus on the production of texts (be it guidelines, "opinions" or academic papers). Official ethics, thus, has established the means to make visible its way of reasoning, while public ethics has not. However, in ethicists' judgments, the lack of technologies for making visible public ethical reasoning is equated with the incapability of members of the public to contribute to an advanced ethical debate. The ethical practices of public ethics often remain unarticulated because they take place outside institutions that allow for the visibility and communication of moral reasons.

Thus, public ethical knowledge would require “mechanisms of voicing” (Michael and Brown 2005) that reveal how certain moral positions are argued and reasoned. Usually, official ethics refers only to a caricaturist version of public ethics, such as “counting heads” (Crosthwaite 1995). Willingly or not, public involvement is imagined in the form of public-opinion polls in which people are asked if they for or against something. This attitude towards public involvement is sometimes accompanied by a fear of demagoguism and the presumption of a malleable public (Gmeiner and Körtner 2002). Such comments suggest that representatives of institutionalized ethics regard ethical issues of science and technology as exempt from the usual political processes and in need of special treatment in expert bodies. However, the question is why, for example, ethical aspects of human embryonic stem cells should be handled differently than other public policy issues in terms of democratic processes.

Furthermore, there is a range of other mechanisms of public involvement that do not rest on “counting heads”; however, these often seem to lie beyond the scope of the social imagination of institutionalized ethics, as they are addressed only marginally in expert debates. The questions is, then, what comes out when alternative mechanisms of voicing are employed, providing an appropriate space for “lay ethicists” to articulate their reasons in a much deeper way? The way ethical issues are debated and argued by non-experts in ethics is the subject of this dissertation. This question is discussed in the empirical chapters of this dissertation, based on a public engagement setting that seeks to find alternative ways of engaging ethical lay with value questions of science and technology. For now, it is important to note that, considering the criteria of involvement, there are two versions of ethics in society —official/institutionalized and lay ‘ethical’ knowledge. The former is the dominant mode of the ethical debate in society, and it is led by experts and professionals. The latter remains mostly invisible and unarticulated, because so far society has not institutionalized the appropriate means to give voice to this way of ethical knowing. Thus, my hypothesis is that there is no pre-existing and “natural” knowledge hierarchy between lay and expert ethics. Any difference is, inter alia, a result of asymmetrical access to “mechanisms of voicing” in public debates. As such, approaches that seek, in the fashion of classical public understanding of science, to compensate for an assumed lack of knowledge by providing the “right” information, are rather pointless, as they would not remedy the lack of possibilities for ethical lay to raise their concerns and be taken seriously.

3.3. Trading zones of values: The moral economy of institutionalized ethics

In this section I will explore in more detail the features and characteristics of institutionalized ethics by analyzing institutionalized ethics as a “trading zone” of values within a “moral economy”. Peter Galison (1999)—however in the entirely different context of physics—has outlined the concept of “trading zones” as spaces where action and belief are coordinated:

> Like two cultures, distinct but living nearby enough to trade, they can share some activities while diverging on many others. In particular, the two cultures may bring to what I will call the trading zone objects that carry radically different significance for the donor and recipient. What is crucial is that in the highly local context of the trading zone, despite the differences in classification, significance, and standards of demonstration, the two groups can collaborate. They can come to a consensus about the procedure of' exchange, about the mechanisms to determine when the goods are “equal” to one another. They can even both understand that the continuation of exchange is a prerequisite to the survival of the larger culture of which they are part. (Galison 1999, 146)

The different sites where institutionalized ethics takes place can be understood as such “trading zones” in which action (of researchers in the labs, of politicians working out a law, etc.) is balanced and coordinated with “belief”, that is, values and morals. In institutionalized ethics, scientific practitioners often meet with moral experts in order to negotiate what scientists should do with regard to human, societal and cultural values. However, these different “trading zones”, such as ethics committees, IRB (Institutional Review Board) meetings or conferences and journals on ethics, are not isolated instances, but contribute to what Brian and Charlotte Salter have termed a (global) “moral economy”. They understand bioethics as a “political means” to create “a global moral economy where the trading and exchange of values is normalized and legitimated” (Salter and Salter 2007, 555) between conflicting cultural positions.

The necessity to establish a moral economy of ethics emerges from the political need to reconcile the promises and expectations of emerging technologies with the cultural costs of scientific advance; that is, when economic values meet more cultural values. Thus, a political technology is necessary to adapt technological and economic regimes to public values and vice versa. Institutionalized ethics has been introduced to fill this gap and to remedy the loss of public confidence in the authority of science and politics to govern knowledge production and innovation in relation to given cultural values. The increasing demand for the social accountability of science in a mode 2 science and society demanded that knowledge production and technological development commit not only to their own ethos but also to “social values”. While on one hand politics put great emphasis on “public participation”, accounting for societal values was put in the hands of a new type of expert—ethicists with expertise in morality. Ethics was regarded as an appropriate means to provide a rather friction-free environment for the development of innovation, which was seen as endangered by a largely assumed but partially real public resistance towards technology. Institutionalized ethics is a means to account for "social values" without involving citizen in this deliberation process, and thus also serves to legitimate decision-making on science and technology to the public by referring to ethics. Institutionalized ethics—and this is one of its prime functions—makes the outcomes of balancing processes between technological and economic progress and given cultural values *calculable*. With institutionalized ethics, politics can define the parameters of the calculation process and thus define, to a certain degree, desirable outcomes. In short, it is clear to politics how ethical experts work, how they think and what kind of suggestions they will produce, while the transfer of an ethical debate to the public is regarded as unpredictable from a political perspective. Referring to Salter and Salter (2007), the public might introduce a "currency" in the global moral economy that is inappropriate as cash because it cannot be exchanged with other currencies in the moral economy, thus fragmenting the global moral economy and providing a less favorable environment for businesses and research.

In what follows, I will briefly discuss four main characteristics of official ethics that contribute to the creation of a wider moral economy where values—and, in their wake, knowledge—can be traded across cultural boundaries. I will sum up by highlighting some impacts of ethics research, the governance of innovation and the role of ethics in society.

First, in order to imagine a space where values can be traded across multiple boundaries (be they cultural, legal or technological), it is assumed that we inhabit a moral space that at least shares basic ethical principles. Such a *common morality* plays a prominent role in some of the most influential theories of biomedical ethics (Rauprich 2008; Turner 2003). The assumption of a common morality is particularly pivotal for the influential *Principles of Biomedical Ethics* by Beauchamp and Childress (1994). While of course the claim of common morality has been challenged often within ethics, the very practices of ethics committees hint that the assumption of a common morality is a guiding principle for the work of ethics committees. Common morality has become a self-fulfilling prophecy, and based on the assumption that there is a common morality, the recommendations of ethical authorities suggest norms that claim a wide-ranging validity. This becomes evident in virtually every ethical opinion provided by these committees, where "the" ethical issues for a given technology are defined with the assumption that these are based on a societal consensus about "socially approved norms of human conduct" (Beauchamp and Childress 1994, 6). Ethical judgments from ethics bodies as well as academic ethicists are mostly deprived of their cultural context of production (Hoffmaster 2001), and thus seem to reflect a common morality that is widely shared. This belief in a common morality is crucial to allow for ethics to become a trading zone of values across cultural boundaries. It is also useful to innovation regimes that aim to create a free market for scientific knowledge and technologies, as a common moral economy allows for the free exchange of research.

Second, the authority a common morality can wield is bound to the question of how it is institutionalized on a political level. Here, recent years have seen an *increasing institutionalization of ethics on a political level*. All Western states now have a least one ethics committee that advises the government on ethical issues of science and technologies. In the meantime, national ethics committees have also started networking activities. The *Forum of National Ethics Councils (NEC Forum),* for example, consists of the chairpersons and secretaries of the national ethics councils of member countries of the EU, and understands itself as an open coordinator of the activities of national ethics committees.[21] The *European Conference of National Ethics Committees (COMETH)* is composed of representatives of national ethics committees (or equivalent bodies) in member states of the Council of Europe.[22] The EU itself also operates an ethics body, the *European Group on Ethics (EGE).* Additionally, there are a number of further ethics bodies on an international level, for example the *UNESCO's International Bioethics Committee.*[23] While these bodies have been created in order to advise national as well as EU policies, they increasingly go beyond their role of counseling and deliberation towards becoming powerful political players in legislation processes on science and technology,; for example, Yesley (2005, 8) notes that we currently "witness several ongoing transitions in bioethics: from a philosophical to a legal orientation, from national to international standards, and from professional to political policymaking." It is important to note that the intensification of exchange between single ethics bodies establishes a (global) moral economy where values can be traded, and at the same time constitutes these ethics bodies as serious political players by making them an "obligatory passage point" (Callon 1986) for national and supranational legislation on science and technology.

Third, alongside the institutionalization of ethics in the political decision-making process on science and technology, it also is increasingly *"integrated" in the very processes of research*. Ethics has become a substantial part of the current innovation regime in Europe. The term "integrated ethics" (European Commission 2007b) was used by the European Commission in its latest Framework Programme 7. The practice of ethics in FP7 is closely related to the goals formulated in the Lisbon Agenda, with its clear economic imperative for research. While ethical clearance is required for research involving human subjects and human material, the EU regards ethics not as "hindering scientific progress", but as trying to be "collaborative and constructive". Ethics is not seen as in opposition to the free conduct of research; rather, "By considering ethical issues from the conceptual stage of a

---

[21] http://ec.europa.eu/research/science-society/index.cfm?fuseaction=public.topic&id=75 (accessed July 8, 2008).

[22] http://www.coe.int/T/E/Legal_Affairs/Legal_co-operation/Bioethics/COMETH/ (accessed July 8, 2008).

[23] http://portal.unesco.org/shs/en/ev.php-URL_ID=1879&URL_DO=DO_TOPIC&URL_SECTION=201.html (accessed July 8, 2008).

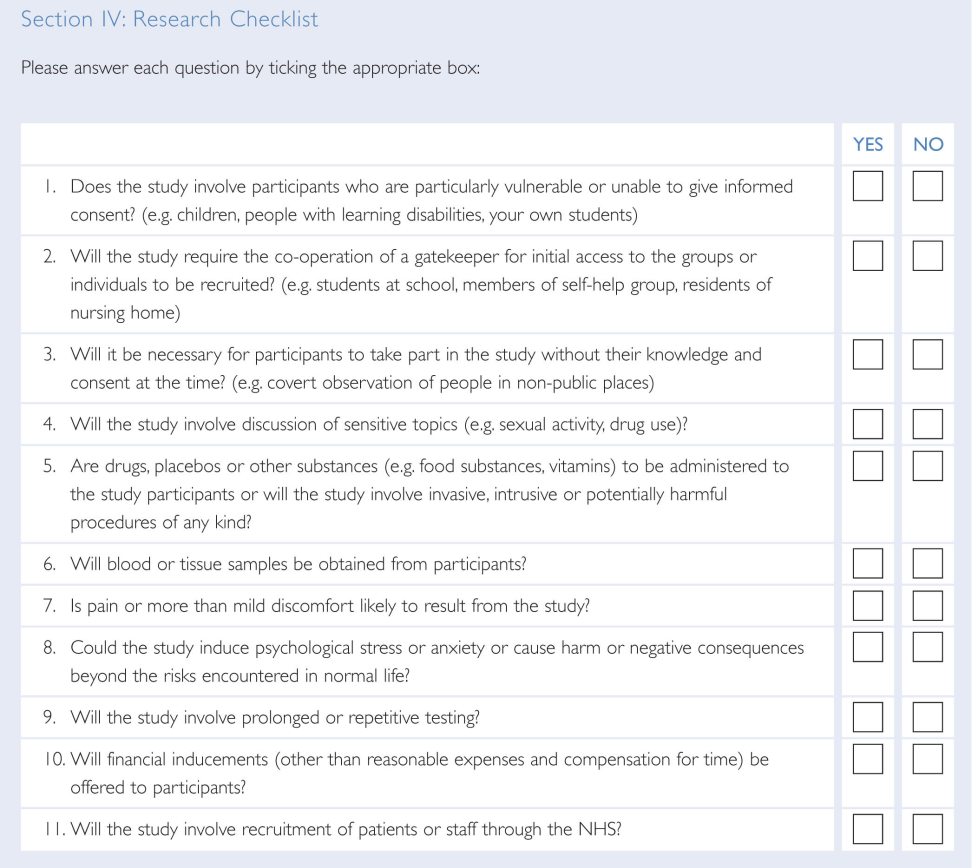
proposal, the quality of research is enhanced" (European Commission 2007a, 8). While one could argue that this is evidence for a new mode of knowledge production that much more strongly integrates social values into the conduct of science and does not regard science and society as antagonistic, the ethical review process is strongly shaped by the narrative of scientific and economic progress (European Commission 2007a), as public concerns beyond the limited set of criteria in the ethical review are not considered. Ethical review processes, thus, publicly signal that if research has been approved, everything is in order.

Ethics has become inevitable for researchers. It accompanies research from the writing of the proposal to the final publication of an article, when for example journal editors ask for information on the ethical approval of the research. The term "integrated ethics" reflects that, in the parlance of European politics, "ethics" is seen not as alien to research but as an integral part of it. Increasingly, ethics is also taught at universities as part of the normal curriculum, thus promoting a specific frame within which researchers have to deal with the moral questions of their research. Suggestions to develop a "European core curriculum for teaching research ethics" have already been made (Lanzerath 2006), pointing again to the first point I made on the assumption of a "common morality".[24] The integration of ethics into research signals a multiplication and expansion of "moral trading zones" that are increasingly linked through common sets of ethical standards and thus constitute a wider moral market on which "values" are traded.

Finally, ethics is increasingly practiced in a *bureaucratic and formalized way.* The tendency towards bureaucratization can be observed both in research ethics and in the political ethics which guides legislation activities. For the seventh Framework Programme, the European Commission has designed a particular procedure that every research proposal must go through, termed "management of ethics". The proposal must first discuss foreseeable ethical issues and how these are to be "managed". After scientific evaluation, the scientific panel decides if the proposal has to go through an ethical assessment process. Similar standard procedures can be found in other funding agencies. What is interesting is the standardization of the process of "ethics", as well as the language associated with it, such as the phrase "management of ethics". Furthermore, what counts as an ethical issue is predefined in the form of a "checklist" (European Commission 2007b), where single ethical issues can be ticked off one by one. These formalized processes contribute to the exclusion of ethical issues that are not within the scope of the checklist. Here is a vivid example of this "checkbox ethics" taken from the ethical review process of the UK's ESRC (Economic and Social Research Council):

---

[24] Another question that can be raised concerning ethics curricula for scientists is what understanding of ethics is promoted in the courses and how do the students perceive and deal with ethics in relation to what they understand as the „epistemic core“ of knowledge production.



Section IV: Research Checklist

Please answer each question by ticking the appropriate box:

	YES	NO
1. Does the study involve participants who are particularly vulnerable or unable to give informed consent? (e.g. children, people with learning disabilities, your own students)	☐	☐
2. Will the study require the co-operation of a gatekeeper for initial access to the groups or individuals to be recruited? (e.g. students at school, members of self-help group, residents of nursing home)	☐	☐
3. Will it be necessary for participants to take part in the study without their knowledge and consent at the time? (e.g. covert observation of people in non-public places)	☐	☐
4. Will the study involve discussion of sensitive topics (e.g. sexual activity, drug use)?	☐	☐
5. Are drugs, placebos or other substances (e.g. food substances, vitamins) to be administered to the study participants or will the study involve invasive, intrusive or potentially harmful procedures of any kind?	☐	☐
6. Will blood or tissue samples be obtained from participants?	☐	☐
7. Is pain or more than mild discomfort likely to result from the study?	☐	☐
8. Could the study induce psychological stress or anxiety or cause harm or negative consequences beyond the risks encountered in normal life?	☐	☐
9. Will the study involve prolonged or repetitive testing?	☐	☐
10. Will financial inducements (other than reasonable expenses and compensation for time) be offered to participants?	☐	☐
11. Will the study involve recruitment of patients or staff through the NHS?	☐	☐

(Economic and Social Research Council (ESRC) 2005, 34)

The ESRC Research Ethics Framework from which the above figure is taken states that “The study should not begin until all boxes are ticked” (Economic and Social Research Council (ESRC) 2005, 34). “Ethics” here is performed in a particular way which displaces moral reflection from the minds and practices of researchers into a form and the ticking off of possible ethical issues. It suggests that ethical questions can be treated as yes or no questions, providing no indication, however, of what it actually means—in a moral as well as practical sense.

The formalization of ethics in research is also demonstrated by the pre-definition of what counts as an ethical issue. As such, a quasi-standard repertoire of ethical issues in research has been developed. These are, in particular, “informed consent”, “privacy and data protection”, “animal use” and “dual use” (European Commission 2008). These issues provide a fixed repertoire of responses to ethical concerns and are regarded as a panacea for moral concerns about research. Moreover, these “solutions” are often not only seen as possible responses to ethical concerns (where maybe others are possible) but actually taken as ethical concerns themselves. These issues can be regarded as a “currency” in the moral economy of ethics, as they appear in most of the ethical guidelines for research and codes of conduct. This standard repertoire of ethical issues in research was developed in the past in the context of certain technologies. This, however, possibly led to a blindness to ethical aspects that come up in new technologies. An example is how the Austrian *Bioethikkommission beim Bundeskanzleramt* assessed the ethical issues of nanotechnology. After having discussed nanotechnology using the standard repertoire of ethical responses, it stated that "Nanotechnology as a new technology does not raise fundamentally new ethical questions"[ii] (Bioethikkommission beim Bundeskanlzeramt 2007, 4). A certain framing starts to shape perceptions, and thus promotes a systematic blindness to alternative views. The frame of the fixed repertoire of ethical responses to technologies may impede the introduction of different ethical issues into the debate. The assumption of a common morality is further demonstrated by the standardized responses to ethical concerns, which help to establish a common "currency" in the moral economy, making it is easier to trade values across cultural boundaries. Such formalization processes often contradict the claim of ethics to be a space for advanced reflection, as the "ethical machine" is processing research from different domains and in different cultures using a limited set of formal criteria such as "informed consent" and "privacy". However, it makes the outcome of ethical assessments calculable for research applicants as well as policy makers. The formalization of ethical assessments guarantees that no new—and hence possibly conflict-triggering—ethical issues emerge, and it allows researchers and politics to anticipate and respond in advance to ethical concerns.

What are the consequences of the moral economy of ethics based on the idea of a common morality, its increasing institutionalization and integration into research, its formalization and its bureaucratization? I will briefly discuss four levels on which the impacts of the dominant way of performing ethics are articulated.

First, the "legitimate" space where ethics has to be negotiated is displaced to ethics committees. This narrative was also prominent in the discussions at the Round Tables, where the researchers and lay participants often shifted authorative ethical expertise to institutional bodies, although for different reasons. The researchers insisted on a division of labor between ethics and science in order to ensure their autonomy in knowledge production. The lay participants, buying into the idea that ethics needs a kind of expertise, and thus seeing themselves as poorly-equipped for a real engagement in an ethical debate, regarded ethics committees as a counterweight to the values promoted by progress and advancement in science. Not only at the Round Table but more generally, ethics committees are imagined to be exclusive checkpoints for controlling the social and moral implications of knowledge production in research. Responsibility for the overall trajectories of progress is displaced hence to a few institutions.

Second, on the level of research and researchers' practices, institutionalized ethics appears to be an "ironic" dealing with ethical questions of research—"ironic" because ethics is taken rather seriously while simultaneously it is not. It has to be taken very seriously by researchers, because to engage with it is crucial when applying for funding, which has increasingly become the dominant regime of doing research. Ethical engagement has become an obligatory passage point for conducting research in many disciplines. On the other hand, researchers are fully aware of the formal and bureaucratic nature of ethics in research proposals. Ethics has become a routine feature of research, and thus often impedes a more reflective dealing with one's own practice (Bister et al. 2009). Laurel Smith-Doerr (2004, 2008) has argued that the wider deployment of ethics education of scientists helps to deflect ethical concerns and public discourses from science, because the formalized methods of ethics provide quick answers rather than permitting a discussion of what the problem might be. Thus, "ethics", like a Janus head, communicates two different messages to researchers, which are characterized by certain ambivalence. On one hand, the very practice of ethics as ticking off boxes in forms strongly suggests that it is nothing but an administrative feature. On the other hand, ethics in a more open sense is perceived as a reflective way to deal with research and its consequences that cannot be easily formalized.[25]

Third, the institutionalization processes of ethics also lead to the constitution of a space of "moral expertise" (Crosthwaite 1995; Powers 2005). While many domains of expertise of public science policy have been contested over recent decades, ethics has managed to create a field of expertise that has hitherto gone rather uncontested in the public. While the internal discussions of ethicists often address the limitations of moral expertise, or whether the possibility of moral expertise exists in principle, the practices of the dominant form of ethics in the public sphere prove that there is a fairly powerful field of expertise on moral questions of science and technology. The moral experts have gained authority over the framing of issues in public debate, and often practice boundary work of what counts as an "ethical" issue, thus defining to a large degree the way social concerns about science and technology must be addressed. What "moral expertise" further does is to maintain and reinforce the distinction between "fact" and "values", or as Brian Wynne (2001) expressed it, "maintaining a distinction between science and its ethical consequences". Dominant institutionalized ethics is preoccupied with the ethical consequences that emerge from (existing or anticipated) applications of scientific knowledge. Its focus on what technologies do when applied in the human domain, however, contributes to a careful separation of the scientific knowledge production process from the domain of values. Thus, dominant ethical regimes can be described as an ethics of implementation, or as an ethics of compensation, as Levidow and Carr (1997) term it—that is, as compensating for past value-choices. Hence, current innovation regimes are barely challenged, because ethics is integral to securing this way of governing science. Because of this dominant form of ethics, "commitments and assumptions are protected from critical collective public examination including critical self-reflection on the part of those institutions defining and dominating the policy agenda" (Wynne 2001, 453). The quasi-naturalized view of the fact/value distinction has turned out to be a powerful politics to guarantee the autonomy and free conduct of science as well as linear models of innovation. Steven Shapin (1995, 403) noted that "Our technical knowledge is only as secure as the moral economy in which it is produced. The 'scientific portion' of any ethical

---

[25] I will discuss the ambivalent approach to ethics in a later chapter, suggesting that the researchers' way of dealing with ethics is much more complex than guidelines on research ethics often suggest.

decision contains institutionalized moral judgements, and the fact that we do not recognize them as such is itself an aspect of the modern condition." Taking seriously the critique by these scholars, ethics needs to challenge more strongly the assumptions that are built into current innovation regimes, the concepts we have of how research is and should be conducted and how science and society are co-produced, and regard these as fundamental ethical questions, that is, as open to collective decision-making, and not as given facts about how the world works.[26] On a more general political level, state-sponsored institutionalized ethics "separated 'risk' from 'ethics', while reducing both realms to specialist tasks. The risk/ethics boundary was designed to gain public deference to the expert assessment of both safety regulators and professional ethicists" (Levidow 2001, 76; see also Levidow and Carr 1997; Wynne 2001). This separation of technical risks and societal values is central to the dominant regime of science governance in Europe, as it helps to facilitate an innovation-friendly policy by isolating value questions from technical questions. It suggests that technology can advance (and be safe) while traditional societal values are preserved. Hence, it provides the ground for separating technical and social innovation.

The last point I want to raise here is that "ethics" is increasingly associated with political rule-making in the governance of science and technology. Tallacchini (2006) argues that ethics has become a self-legitimating way of making politics and law, but without the usual guarantees of the legal system to protect citizens from state power or particular private interests. Ethics, thus, often serves as a means to introduce norms beyond those of traditional political rule-making. The political assumption is that ethics is a more flexible version of law, able to speed up as well as simplify legislation processes. Expressions like "ethical legislation" in EU policy documents reflect the institutional confusion that often renders ethics and legal norms hard to differentiate in practice. The use of IRBs as a form of self-regulation of science exemplifies the entanglement of ethics and law. Fleetwood and Unger (1994) argue that IRBs have been partly established as "alternative courts", lacking, however, the democratic legitimacy as well as the procedural rigor and transparency of official law systems. While the normative impetus of ethics is nothing new, it becomes problematic because of its institutionalization on the political level, its integration into the conduct of research and its increasingly formalistic nature. Thus, ethics has become a trading zone of what future norms should govern science and society, mostly without the opportunity for democratic participation in the decision-making processes.

---

[26] We will encounter this question again in the empirical chapter in how both scientists and lay people discussed on ethical issues of "genomics". Rather than remaining within the dominant framework of official ethics—e.g. around issues of informed consent and data protection—the debate revolved much more around fundamental questions of the relations between science and society.

3.4. Public engagement with ethics: Going beyond expert-framed ethics

In this chapter, I have discussed two approaches to the governance of science—public participation and ethics—that try to remedy the shortcomings of classical centralized state politics. While both have in common the orientation towards “values”, and thus abstain from pure technical decision-making, there are a number of differences between the approaches. First, public participation aims to extend the range of opinions, attitudes and values to be included in deliberation and decision-making processes. Thus, new kinds of knowledge and framings are brought into the debate. Institutionalized ethics, on the other hand, pursues a politics of exclusion; that is, only a few experts—particularly those with a background in ethics, theology, philosophy, biomedical science and law—are entitled to define what counts as “common morality” and thus how technoscientific issues are addressed. Second, while bioethics began as a open dialogue between different disciplines concerning the moral challenges of new medical technologies, it has dominantly become a bureaucratic and formalized instrument to “manage ethics” efficiently—that is, to guarantee the basic rights of individuals but give high priority to the free conduct of research within the regime of a knowledge-based economy. Thus, ethics is more amenable to “progressive” innovation regimes than to societal concerns about the consequences of research and emerging technologies. Public engagement is able—at least in principle, though often used otherwise (Irwin 2006)—to challenge the way issues are framed by experts and thus open up new and more socially robust ways of dealing with emerging technologies (Jasanoff 2003c). Third, there is a misunderstanding about the “values” over which ethics and public participation deliberate. While in politics “ethics” is (mis-)taken for public concerns, the terms do not refer to the same object. Ethics uses the concept of a “common morality”, which is assumed as a given ethical consensus, while in public participation “public concerns” refers to practically articulated moral positions of citizens. Because it is not the same object, the latter plays virtually no role in mainstream ethics. Furthermore, it is often emphasized in ethics that the opinion of the public on ethical issues must not inform what is good or bad in normative ethical reasoning.

While I basically advocate a wider public participation in ethical issues of science and technology, I have also hinted at the open questions and problems that are created by the “participatory turn”; in particular, the often problematic construction of the “public”—and thus who is entitled to participate and who not—as well as the relation between public participation and other governance regimes, especially those of technocratic expertise and “commitology”. Furthermore, while some issues are regarded as open for a wider involvement of the public, others seem strictly excluded; for example, the objectives defined in the EU’s Lisbon Agenda, which gave high priority to knowledge-driven economic growth. Under this agenda, the public is not seen as able to participate in decision-making processes—and thus possibly to challenge some of the developments—but has to be made "aware" of the benefits, in a top-down process, in order to provide an "innovation-friendly" environment for businesses and the free flow of knowledge. Thus, what is needed is a more general reflection on the relation between citizens, democracy, science, innovation and politics, which defines more explicitly the role of citizen participation in governance beyond being a politics of talk (Irwin 2006).

Only a few studies have hitherto—practically and theoretically—engaged with the question of public engagement with ethics, and those have been dominantly from the social sciences. In ethics—although it is in part occupied with "empirical ethics", which addresses the role of social science research in relation to normatively informed ethical reasoning—public participation itself has not been an issue. Mairi Levitt, herself a social scientist, reports from a European project where medical scientists and philosophers discussed the role of the public with regard to bioethics:

> The view was expressed that there was not much point in public consultation about ethical issues in science and technology. The scientists agreed that the public are ignorant about science and that they need more information presented in a clear and simple way. The philosophers accepted this and talked about the need for science to present the facts while they (the ethicists) highlight and discuss the ethical issues. For them finding out what people think is not furthering ethical research which, instead, aims to clarify what ought to be done. However, scientific 'facts' were seen as relevant background. (Levitt 2003, 15-16)

The reliance of ethics on scientific facts, as well as the disregard of public moral concerns for ethical reasoning and decision-making, is akin to rather narrow technical risk-assessment, in which public fears have been ignored as "irrational". The assumption of the "ignorant public" by expert ethics, however, rests on prejudices that have never been explored in detail, as well as on the way the "public" is constructed through large-scale quantitative surveys like the *Eurobarometer*. The absence of "factual knowledge" of a scientific type is interpreted as an exclusion criterion for wider public participation in ethical deliberation. However, some studies have engaged with the question of public engagement with ethics and come to a rather different conclusion:

> The issues that lay people find important, and the ways they express and develop their opinions and arguments, constitute an important dimension in policy decisions about the use of new genetic technologies. The richness of this lay contribution is best captured not by opinion surveys … but rather by deliberative processes that take place in groups—for example, focus groups, citizen's juries or Socratic dialogue. Such deliberations can generate a sophisticated discussion that goes beyond mere unreflecting prejudice and adds texture to often abstract and principle-based philosophical debates. (Banks, Leach Scully, and Shakespeare 2006, 300)

What Banks and co-authors suggest is that the "ignorance" of the public—often taken by professional ethics as a justification for its exclusion—is a product of the absence of a mechanism of voicing that would reveal a more differentiated level of public engagement with ethics. The public is *made* ill-informed because of the dominant ways through which it is given voice, that is, quantitative surveys. The experts' disregard of public ethics also implicitly assumes that there is only one way to address ethical questions of science and technology. Mike Burgess (2004, 6), however, argues that the "expert-driven approach to ethics, like that found in risk assessment and science, neglects the fact that ethical analysis of practical and policy matters is far from a univocal or uncontroversial practice."

Brian Wynne (2001) has argued that institutionalized ethics frames moral issues of science and technology assumed to "represent public concerns" in a way that can easily be digested and domesticated within the framework of the existing institutional culture of science governance. Instead, public engagement in ethics could reveal wider issues at stake. This is particularly important as the existence of ethics is legitimated by public concerns and public interest. Mike Burgess argues that "policy decisions governing what options are available to health care providers and patients ultimately presume some perspective on what is in the interest of the 'public' … Since public policy in health and all other areas is inevitably justified in terms of the public interest, what is the relevance of personal experiences of injustice to fair policy?" (Burgess 2004, 5). Because expert ethics argues about "public interest", citizens should be involved in the process of ethical deliberation. Burgess goes even further by arguing that involvement in ethics is not only a right of citizens, but also a civic duty, because

> Overemphasis on the need to become "expert" in a particular application neglects the expertise and responsibility we all have as citizens to consider the effects of our actions on others, and to participate and respect the stakes of others in the kind of society we become. (Burgess 2003, 15)

Only a few studies so far have engaged with the possibilities and limits of public engagement with ethics in a more intense way that would shed light on how citizens construct moral aspects of science and technology, in particular with regard to the way they provide reasons for certain moral positions. This is because official ethics has assumed that they are not able to do so. This is particularly astonishing as ethics often argues that judgments should be based on facts and evidence. With regard to the possibilities and limits of public participation, these "facts" are largely missing, and thus the neglect of the public as having a role in ethical deliberation is mostly based on pure guessing.

Engaging the public with ethics and allowing a greater role for citizens in the shaping of how ethical questions of science and technology are addressed seems important in light of the participatory turn science governance has undergone in recent years. However, it is also necessary to consider carefully how the public's engagement with ethics, and thus the knowledge and assessments produced in these settings, relates to professional ethics in its institutionalized forms. In order that it not be a mere “politics of talk”, the role of expert ethics must be reconsidered. Rather than “supplementing” expert advice on ethical issues, public involvement in ethics could have the task of challenging expert framings of ethical issues in the governance of science.

4. Ethics goes empirical? On the relationship between social sciences and ethics

I have so far discussed "ethics" and "public engagement" as separate strands in public policy, as well as in academic discussions, that have been barely brought together so far. However, there exists a certain area in ethics that has the potential to reconcile these different strands: "empirical ethics". In recent years, there has been an increasing engagement of academic ethics with the "empirical" in general and the possible links between ethics and social science in particular. At the same time, the social sciences have engaged more intensely with issues formally assigned to the domain of ethics (e.g., Hedgecoe 2006; Corrigan 2003; Henwood et al. 2003). To a certain degree the debate is a "virtual" discussion, as it often revolves around assumptions of "what would happen if…", while in fact the empirical ethics studies are rather scarce (Borry, Schotsmans, and Dierickx 2006). The mutual involvement of social scientists and ethicists, however, presents tremendous challenges to both with regard to basic methodologies and epistemologies, as well as with regard to "politics". As this dissertation is also engaging with ethical issues—how lay participants and scientists negotiate on ethical issues of genomics and science/research in general—it is relevant to engage with this discourse in order to shed some light on my position in this debate and to situate this work within the debate. Contrary to some other social scientists who have engaged with the issue of collaborative work between social scientists and ethicists (Haimes and Williams 2007; Hedgecoe 2004), I do not straightforwardly argue for a closer and improved collaboration or dialogue between these two disciplines. My argument here is rather, If ethics is addressing the relation to social science (methods) in order to enhance its ethical reasoning, is this really the problem we should discuss? Put differently, is the problem one of interdisciplinary cooperation, or does this debate raise much deeper questions about the status of experts in society, their legitimacy and the role of expertise in democratic society, as well as leading to power struggles regarding who has the right to "represent" the public and society and its values?

In ethicists' accounts of the relation between the social sciences and ethics, the relation between the disciplines is framed as a methodological debate. A definition of "empirical ethics" often cited in the literature comes from Sugarman, Faden and Weinstein (2001), who define "empirical ethics" as the *application of research methods in the social sciences to the direct examination of bioethical issues*. They call attention to contexts of morality, and the way the inclusion of social science methods in bioethics aims to enhance and enrich ethical analysis.

The arguments that ethics should engage with empirical research dominantly note the traditional inter- and multidisciplinary nature of ethics, and thus the question, Why not include social science methods and findings? However, while empirical ethics has existed for a rather long time, it has become subject to a wider debate only with the last decade. Since then, the social sciences have begun to engage more intensively with issues that were dominantly in the domain of ethics, partly because of the ELSA/ELSI programs of the Human Genome Project, which sequenced the entire human genome in the period between 1990 and 2003. The large-scale genome programs made biomedical research an issue of public policy, in particular by incorporating ideas for future applications from the very beginning. Further, universities have been increasingly subject to evaluation processes that in part demand greater social relevance and accountability of teaching and research. Thus, there is in fact a competition between humanities and the social sciences over resources and for public legitimacy, to which ethics too must increasingly pay attention in order not to loose its financial basis. Hence the question is, Is "empirical ethics" a site of the struggle for public and political legitimacy and authority? While the inclusion of empirical data in ethics journals is still sparse, the debates around "empirical ethics" have produced some disturbances in ethics. This hints at the fact that much more is at stake in this debate than simply methodological questions.

While there have been increased calls for "dialogue" and cooperation between the social sciences and ethics from both sides, the relationship between these two disciplines is also characterized by a series of misunderstandings, especially regarding the renewed interest in empirical methods on the side of ethics. As Zussman (2000) has argued, many (if not most) ethical propositions are based on empirical claims. These empirical "facts" mostly stem from the (natural) sciences, which hitherto have served as a basis for ethical reasoning without reflection. Thus, the recent interest in empirical research from the social sciences as a source for "empirical data" needs additional explanation. Here the question is whether the renewed interest of ethics is in accessing an additional reservoir of "empirical data" or is due to a genuine interest in social science.

The most striking feature of these misunderstandings is that, in the perceptions of ethics, the social sciences are often equated with "empirical methods". There is also an assumption about intrinsic linkages between "social science", "empirical research" and "facts", where social science (discipline) conducts empirical research (method) and is thus able to provide facts (ontology of the knowledge produced) to provide a basis for ethical reasoning (hierarchy between disciplines). Here, the social sciences are often seen as identical to "empirical research", as, for example, is suggested by this quote: "We will use the terms, 'sociology', 'social sciences', and 'empirical approaches' in a broadly interchangeable fashion" (Borry, Schotsmans, and Dierickx 2005, 52). There is also the assumption that "empirical" research results in "facts": "The common picture of the relationship between bioethics and the social sciences assigns responsibility for accurately gathering the pertinent facts to epidemiologists, sociologists, anthropologists, and their kin, and for assessing those facts to bioethicists wielding explicitly normative techniques" (Nelson 2000, 13).

Seeing the social sciences with the "facts" glasses is rooted in the metaethical distinction between "is" and "ought", that is, the idea that the factual can never determine what could be regarded as good. However, while normative reasoning clearly draws a boundary around "facts", the assumption is that "Good ethics starts with good facts" (Loewy 2003, 174); however, "Yet facts alone … will not produce good judgments since these are two different modes of thought. … It is an age-old fact/value argument about the division of labor between those who carry the empirical burdens and those who toil the normative fields: moral choices require information, but they also involve values and mores that escape descriptive analysis." (Dzur 2002, 199-200).

The role of social science as a provider of "facts" can be challenged in many ways. In particular, "facts" never speak on their own, but are inherently linked to the theoretical and epistemological frameworks in which these "facts" are produced. What is called "facts" is the product of technologies that make the social sciences see the things they see, and these means are neither neutral nor objective. The call of ethics for social facts demonstrates the wish to define what "is" the case in order to be able to carry out an ethical analysis in a proper way. However, not all scientists using empirical methods are social scientists; and social sciences often employ non-empirical methods of knowledge production. Furthermore, it is often difficult to precisely define whether a study is "empirical" or not—for example, a discourse analysis where texts that have been produced by others are analyzed. The metaethical distinction between "is" and "ought" assigns a special role to the social sciences, that is, as "merely empirical auxiliaries" (Borry, Schotsmans, and Dierickx 2005, 61) or as a "handmaiden" of ethics (Haimes 2002). A stronger "integration", beyond a supplementary role for the social sciences, is accompanied by fears that ethics may loose its normativity and thus its relevance (van der Scheer and Widdershoven 2004). While on one hand the social sciences produce facts through empirical research, they are also associated with relativism: "Most bioethicists depicted sociological studies as irrelevant to their discipline because they feared being too strongly influenced by historical and sociological contextualization, which could bog them down in cultural and ethical relativism“ (Borry, Schotsmans, and Dierickx 2004, 1). The fear expressed is that the context would dictate what is right or wrong thus making ethics depend on cultural relativism. Another fear is that the inclusion of empirical research in ethical reflection would lead to "mediocre studies of little interest or significance" (Hope 1999, 219). However, the question is to whom such studies are and should be significant: To other ethicists or to politics?

While there are misconceptions on the side of ethics about what social science is and what it does, the misunderstanding often also rests on the side of social science. Klaus Hoeyer (2006a) identifies three dominant ways in which social sciences encounter bioethics: They either employ a "deficit model" (bioethics lacks a sense of context), a "replacement model" (social science is the better way to conduct bioethics) and a "dismissal model" (bioethics should be abandoned as a misconstrued veil of power). He goes on to suggest a dialogue with bioethics.

However, misunderstandings seem unavoidable in light of the heterogeneity of both disciplines. Even for social scientists, it is impossible to provide a comprehensive account of their own discipline, its procedures and tasks. The "ethics wars", as Hoeyer (2006a) has termed them, are not only a *Methodenstreit* between two disciplines about the best way to produce knowledge. Rather, both disciplines have different "epistemic cultures" (Knorr Cetina 1999) that also define the way the knowledge produced relates to politics and society. Scientific ways of knowing are always associated with political cultures as "civic epistemologies" (Jasanoff 2005). Focusing on the context of value production, as social science does, is not merely a methodological question, but methods always contain a performative dimension that enact certain politics (Mol 2002; Law 2004). This is true for both social science and ethics, which both carry implicit and explicit assumption about the political nature of their knowledge. Thus, these misunderstandings reveal different knowledge politics.

In politics, involving "ethics" is often equated with addressing "public concerns" in decision-making. Debates and controversies are expected to be ended by referring the treatment of the issue at stake to an institutionalized ethics body. The assumption is that institutional ethics is able to "represent" a common morality and thus to balance the questions posed by new technological developments and innovation with given values in a society. The discussions around "empirical ethics" in part reflect this increasing struggle for "representativeness" and public legitimacy. However, as outlined above, it is strongly contested whether public attitudes towards ethical issues should inform ethical opinion making: "If it [a survey] is sufficiently probing, it can determine the reasons why people have the beliefs and preferences that they do, as well as the causes for their beliefs and preferences that may not function as reasons. But judgments, unlike preferences, are correct or incorrect; and determining what people believe *does not determine whether their beliefs are correct*" (Hausman 2004, 244). This constitutes a striking ambivalence between allowing for public values and completely ignoring them, which helps to maintain ethics as a field of expertise that has been hitherto largely unchallenged in public debate.

"Public concerns", in this version, do not include the values of people who are and will be confronted with new technologies. Public is replaced by "common", which abstracts from concrete people and refers rather to an abstract reason or idea of values in democratic regimes that count as "accepted". The social sciences—on the other hand—do have quite different technologies of representation. The "public" is rather understood as consisting of people with different attitudes and preferences.

Of course, the misunderstanding is also on my side. Throughout the text, I have used the term "ethics" rather than "bioethics" or "medical ethics". I have also focused on "ethics" in its normative version, rather than "descriptive ethics" or pragmatist approaches. This is because I have aimed to describe ethics as a larger "epistemic community" (Haas 1992) and its *dominant*[27] practices in societal, and in particular political, contexts. My focus has been the institutional practices and their relation to public policy, as well as the governance of science; in short, I have aimed to analyze ethics as a technology of power (without the intention to disqualify "power" in principle) to construct communities that are held together by the "free" commitment to a certain reasoned morality (Rose 1999b).

When describing these dominant institutional practices of ethics, I have had no intention of speculating on the individual motives of ethicists regarding their work in this profession, be it academics or members of ethics bodies. I regard ethics as a means to order and regulate how humans deal with themselves as a collective in relation to scientific knowledge and technologies. Ethics as a means to co-produce moral orders of humans and technologies, however, is not particularly dependent on the direct involvement of trained ethicists, but is rather a wider discourse that shapes the way we deal with values in relation to emerging technologies. Ethics could be analyzed as a certain kind of power (Hoeyer 2006b; López 2004) that is manifested in institutional practices. Thus, there is of course a wide range of misunderstandings on the part of the social sciences with regard to how ethicists produce knowledge, and about the disciplinary cultures of ethics. However, the struggle between ethics and social science is not so much about "method", but rather about "political authority", as particular epistemic cultures and methodologies construct certain visions of the social.

While an increased dialogue between social science and ethics, rather than mutual ignorance, is surely to be welcomed, in particular as it leads to the questioning of implicit assumptions both disciplines hold—that is, with regard to ethics, assumptions about the nature of the social and the nature of science and research; and, with regard to the social sciences, their own, often non-reflected, normativity—it also seem crucial to further pursue the social study of ethics as a societal phenomenon, and thus contribute to a critique of ethics as an authorative institution. The basic problem of ethics is not that its evaluations are too weak and thus need additional evidence or "facts" from the social sciences in order to gain more legitimacy and representativity. Rather, this is just an isolated dimension of issues that concern the institutional context in which ethics is carried out today, and that characterize ethics as a political endeavor. Empirical ethics, however—with its current appeal to social science as a new supplier of "evidence" and "facts"—aims to render ethics *a*political again, thus re-enforcing the distinction between fact and values or is and ought, something which Bruno Latour described as "(political) epistemology", which "claims to be limited to Science, whereas its aim is really just to humiliate politics" and to "short-circuit any and all questioning … through the invocation of Science as the salvation from the prison of the social world" (Latour 2004, 13). (Political) epistemology is not treating science and politics with equal interest, but seeks to do politics without politics by referring to facts and reason

---

[27] For a similar approach of describing dominant discourses of ethics in the public domain, see Wynne (2001).

"without respecting the procedures for coordination either of the sciences or politics" (Latour 2004, 15). Certainly, Latour had in mind natural sciences and their invocation of facts and "nature" when doing politics. However, many claims made in the debate over "empirical ethics", as well as in institutionalized ethics, strongly suggest that in fact a certain kind of (political) epistemology is enacted.

Hence, engaging with ethical issues—from whatever disciplinary background—means performing public meaning. Institutionalized ethics so far has not reflected on the constructive role it is playing in the performance of public meaning—and thus the role the public has with regard to deliberations on ethical issues. The inclusion of empirical methods in order to enhance ethical analysis and to be able to provide more valid evaluations does not cover the full scope of the problem. Rather, it hides and displaces the problem of the societal legitimacy of ethics. Thus, rather than simply including social science analysis in ethical reasoning, a much more fundamental reflection seems required. What is the role of ethics in society? How do the norms produced in ethical reasoning relate to public meanings? How does ethics implicitly and explicitly frame public debates on ethical issues? What are the fundamental presumptions of ethical methodologies? How are ethical issues generated in the first place (why those and not others)?

In my opinion, ethics and the social sciences do not share enough common ground at this time to opt for a more integrated collaboration. Both methodologies and epistemologies seem rather distant; for example, consider the fact/value and is/ought distinction. While for ethics this distinction is quasi-naturalized and serves to legitimate the whole enterprise of ethical expertise, in STS what counts as facts and what counts as a value is the product of negotiation processes that are constantly being remade. There is nothing basic in this distinction (see Latour 1993, 2004). Social science has expressed some quite fundamental critiques towards ethics, for example that professional ethics is "thinning out" the debate on ethical issues (Evans 2002), or that bioethics has established a "global moral economy" that aims to normalize the trading of values across cultural boundaries (Salter and Salter 2007), or that ethics has become a political technology that has introduced a kind of side-law beyond the traditional mechanisms of democratic rule-making (Tallacchini 2006). This critique targets the institutional and political dimensions of ethics in society, and it is hard to see how a closer collaboration (or integration) with social science methods, as is often suggested in "empirical ethics", can remedy the often problematic nature of institutionalized ethics. In my opinion there is a fundamental misunderstanding of social sciences involved, that social science methods and empirical work is somehow neutral and serves to create facts about the nature of the social. However, methods as deeply performative have a normative dimension that cannot be separated from what is called the "facts". On the other hand, sociological inquiries into ethical issues are sometimes rejected by ethicists because of their (implicit) normative statements (Herrera 2008). Thus, the way ethicists deal with the social sciences sometimes suggests that in their view social science research can be separated into two branches: the relatively neutral gathering of data (which is seen as the domain of social scientists), and the interpretation of this data afterwards through which normative assumptions are injected into the data.

The discourse of empirical ethics is dominated by the question of how social science methods can enhance the quality of ethical assessments. This implicitly touches on the question of the societal legitimacy and representativity of ethics—although this is seldom explicitly addressed in the debates. However, ethical assessments and problem solving always take place in a societal environment that is characterized by a certain distribution of power and authority. While it may be true (and it certainly is) that particular ethicists just want to help in difficult moral situations, the power of ethical advice is determined much more by the context than by the virtue of the best argument. Thus, the call for dialogue and cooperation must not compromise the social sciences' task of institutional critique.

5. Hesitant latecomer and expert-orientation: The Austrian context of participation and ethics

In order to contextualize the following empirical chapter, I will now shed some light on the wider context of public engagement and ethics in the political culture of Austria. This context was mirrored in the discussions at the Round Table; that is, the participants not only reacted to each other, but also reflected—implicitly and explicitly—the wider civic and citizens' epistemologies (Jasanoff 2005; Felt and Fochler 2007) common in the cultural context they live in. By referring to the contexts in which our experiment in public engagement took place, I aim to make an argument to take seriously the localities of citizens' encounters with science, a dimension often neglected in public participation exercises (Felt et al. 2006).

In general, the science policy engagement with public participation set in rather late in the Austrian context compared to the European context. Public participation exercises have been implemented rather poorly and in a hesitant way (Felt, Fochler, and Müller 2003a), and science policy has not managed so far to install sustainable measures in this regard. In the meantime, the priorities of science policy have shifted again, away from the attempt to let the public participate in technoscientific decision-making and towards "awareness" campaigns with a clear economic impetus. Thus, science communication in Austria is characterized by a "prosperity by consensus" narrative (Fochler 2007), where the public is largely and deliberately bypassed in contributing to opinion-making in innovation policies. This also holds for ethics in the public sphere, where citizens are completely absent as relevant actors. On an institutional level, Austria is in line with other European countries. However, while Austria has been able to implement ethics on administrative level, it has failed to initiate a wider public debate on this issue, with the result that ethics is seen as a matter for specialized experts.

In the following, I will outline four features of public participation and ethics in Austria that possibly informed the discussions at the Round Table.

The first attribute of the Austrian discourse is its particular cultural *understanding of consensus*. Austria has a long tradition of corporatist consensus politics, the so-called *Sozialpartnerschaft (social partnership)*, which has led to a rather informal system of politics, where collective decisions are made outside of parliamentary structures, which would warrant a certain degree of formal procedures and transparency. The policy domains of the *Sozialpartnerschaft* are not limited to the direct concerns of employers and employees, but comprise a wide range of economic and social issues. The corporatist culture has had the result that conflicts and dissent are rarely brought into a public debate, but decisions are made behind closed doors without the deliberative participation of the wider public. This culture of corporatist consensus is also reflected in public understanding of science policies.

The main aim is to raise the "public awareness of research and innovation", because innovation "is the motor of economic prosperity and provides a solid basis for the social and cultural advancement of this society. The public is too little aware of this fact, and it should be made clear to the population through a number of means" (Rat für Forschung und Technologieentwicklung 2001).[iii] Hence, a "broad acceptance in the public", by aiming at a "societal consensus", is needed. The societal consensus is not one that is struggled with by engaging and involving the public, but a predefined one that has to be manufactured. The Austrian innovation policy regime is located in the triangle of economic prosperity, public awareness and consensus, thus, rather closing down than opening up opportunities for civic participation in research and technology development. The local cultural understanding of consensus was also present at the Round Table discussions, and was articulated in manifold ways. For example, dissent—e.g., in the form of fundamental opposition to particular practices—was not articulated in the plenum sessions of the Round Table, but in peer group discussions, in order not to disrupt the social setting of the Round Table perceived as fragile. Thus, in a way, the cultural importance of consensus and the avoidance of public conflict led to a taming of the discourse.

Great confidence in the status of expert authority is a second feature of the Austrian context that comes into play in ethics and in science communication. Based on the central agenda of Austrian science policy to raise "public awareness" for innovation, the campaign *Innovatives Österreich* (Innovative Austria), for example, initially relied on classic means of advertising and PR. Later on, the campaign was relaunched, aimed at a more "interactive" involvement of citizens via a website where they could pose questions to "experts" who then provided their expertise on the questions of the lay public. Hence, while claiming to be more interactive and open to user involvement, the initiative also reinforced the hierarchical divide between experts and citizens.

Experiments with more open notions of lay involvement and participation have been scarce, and were also characterized by a hierarchical relation between lay participants and experts. In 2002, a so-called *Diskurstag* (Discourse Day) on genetic diagnosis was organized by the GEN-AU program.[28] Despite its aim to create a more open debate on genetic issues, the organization was rather unspecific with regard to the overall aims of the event, especially with regard to its notion of "dialogue", where, again, experts responded to questions of lay people (Felt, Fochler, and Strassnig 2003). As there is no long-learned tradition of participation in Austria, it was rather hard for the people to integrate the *Diskurstag* into their experiences with the existing political culture. 2003, a citizen conference on genetic data was organized within the framework of the *Innovatives Österreich* awareness campaign. The Danish consensus conferences served as a role model for this event, in which a panel of citizens was supposed to ask questions of experts and then de-

---

[28] The GEN-AU program is a dedicated research program funding research in the field of genomics. See http://www.gen-au.at. The research this dissertation is based on also took place within GEN-AU's ELSA branch.

liver a consensus report. While this exercise was celebrated as a participatory event by the organizers, the citizen conference did not find any resonance in politics—and not even in media reporting. Its basic logic was that of a participatory event—however, it remained unclear to all involved actors in what it participated (Bogner, Puchrucker, and Zimmer 2004; Felt, Fochler, and Müller 2003b; Fochler and Müller 2006). Furthermore, this citizen conference followed the model of lay asking questions and experts responding. The lay participants had not been empowered by the organizers in such a way that they were able to challenge and critically question the expert advice provided to them. Hence, a main feature of the consensus paper of the citizens was that they often recommended further expert engagement with issues, and thus re-affirmed expert dominance over technoscientific issues (Rat für Forschung und Technologieentwicklung 2003).

Later public engagement events of the GEN-AU program focused on attracting pupils to careers in research. This is also the focus of recent initiatives of the Austrian Federal Ministry of Science and Research (bmwf). Under the heading "Sparkling Science"[29], projects are funded that are explicitly directed to pupils, aiming at a "true partnership" between research and schools. The long-term goals are to raise the proportion of scientists in the population, especially with regard to technical and natural science.

The recent initiatives that focus on younger people further emphasize the educational impetus of the Austrian science policy paradigm. The larger public is seen as in need of education regarding the benefits of innovation for economic prosperity, and younger people must get into science in order to raise the number of scientists in this country, which is seen as an important prerequisite for innovation. The tone of these initiatives also suggests that the obligation to engage with science is with the public. The reason for the public's reservations towards science is seen as an informational deficit. The assumption is that if the people were better informed they would appreciate science and subscribe to a career in science. The hesitant beginning of public engagement initiatives directed to a broader public, such as the *Diskurstag* or the *BürgerInnenkonferenz,* has not been continued in a sustainable and more institutionalized way that would facilitate a sustainable institutional learning process. There are also hardly any institutions that have experience and special competences in organizing such events, different exercises often seem uncoordinated, and their relations to the more general democratic culture in Austria often remain unarticulated. This hints at the inability of Austria to develop its own culture of public engagement in science and technology, because most of the exercises are imported models applied in the Austrian context without adaptation to more local cultures of participation. This is also true for the uptake of EU policy discussions. They remain mostly in the background, and policies are implemented in a very selective way. In particular, the economic narrative of "public awareness" had a huge impact, while other facets of the European discussion, such as the de-

---

[29] http://www.sparklingscience.at/ (accessed July 8, 2008).

mocratic impetus of participation, are often completely missing in the Austrian discussion.

The strong role of experts also holds for the bioethical discourse in Austria, which is largely limited to the *Bioethikkommission* and its members. The *Bioethikkommission* is an expert panel with representatives from medicine, genetics, law, philosophy, social sciences and theology. A representative of the pharmaceutical industry (Novartis) was on the panel in previous years, but has now been replaced by a representative from disability organizations as a concession to critics. The majority of the members are from the field of biomedicine. All current members except two are affiliated with a university. The bioethical discourse in Austria is mostly carried out by these people via contributions to a small range of high-quality media. While the *Bioethikkommission* is supposed to initiate and facilitate a public debate on bioethical issues, the limited financial means of the institution do not admit the organization of particular events in this regard. Besides the *Bioethikkommission* and the individual contributions of a small number of its members, an engagement with bioethical issues is completely absent. In politics, the government has had no explicit programs on biomedical issues for years, and the political programs of the parties represented in the parliament do not contain bioethical issues. Thus, biopolitics and bioethics are not very high on the political agenda in Austria. For example, Austria has not been able so far to pass a law that regulates the production and use of human embryonic stem cells. The Reproductive Medicine Act indirectly prohibits production, but the exact legal status of imported stem cell lines is contested among experts. Opponents and advocates of stem cell research are reluctant to regulate this domain: The advocates fear that the legal rules would constrict research too much, while opponents assume that they would lead to further liberalization. Thus, a grey area has been established where everyone seems happy that the matter is not formalized in legal norms (Körtner 2008a). This situation also holds for other biomedical issues. In general, politics seems reluctant to explicitly address biomedical issues, because these issues are seen to lack political relevance compared to others. Furthermore, it is feared that the debate produces “unnecessary” conflicts that impede the innovation paradigm proclaimed by science policy. This policy of non-conflict supports the role of ethical experts, as the issue is not publicly discussed and expert opinions are not be in danger of being challenged in public debates.

The confidence that is put in experts and their authority was reflected in the discussions at the Round Table. The first two to three Round Tables were often characterized by one-directional communication: Researchers provided factual knowledge while the lay participants asked some questions for clarification and further explanation. This puts the researchers in the role of experts and the lay participants in the role of consumers of knowledge. People were hesitant to bring in their own experiences, as this knowledge was initially not regarded as a legitimate form of discourse in such a setting, thus maintaining the hierarchy between experts and lay. That is, both researchers and lay participants regarded the “expert” model as the ideal-typical way in which to debate about knowledge in the public domain.

Third, the *ethical discussion in Austria is largely monopolized* by one institution and its members, namely the national ethics committee, Bioethikkommission beim Bundeskanzleramt (founded 2001). A large proportion of the public discussion of ethical issues of science and technology is carried out either by this institution or by a rather small number of its members. Furthermore, the public debate over ethical issues takes place via certain media; that is, in a few “high-quality” print media and in a few branches (such as the religion department) of the public broadcasting corporation, ORF. The domination of ethics by the national ethics committee is complicated by a high degree of opaqueness. In particular, the *Bioethikkommission* has been criticized for its opaqueness with regard to its process and the appointment of its members (Gottweis 2001). The criteria for the members’ appointments, as well as their legitimacy in representing societal values, are not transparent—an issue which has been often criticized in STS (Jasanoff 2003b). The members are not asked to publicly reveal conflicts of interest, which is particularly a problem for the members who come from biomedical disciplines. In the past, a possible conflict of interest have been a source for some public debate which led to the resignation of the last chairman of the *Bioethikkommission*—however, not because he had such relations, however, but because he promoted a medically controversial cancer therapy.

With the *Bioethikkommission* as the dominant actor in ethics in Austria, it is seen as the “natural” and legitimate place for ethical deliberations, allowing for a delegating—or displacing—of ethical questions to the committee. The concentration of “ethics” in a single institution and a handful of actors also leads to the framing of ethical issues in a particular way. In particular, the Austrian bioethical discourse has subscribed to a deontological ethics and has focused on the preservation of human dignity—rather similar as in the German “civic epistemology” of ethics (Jasanoff 2005). Furthermore, representatives of ethics often have a Christian background, and thus promote a certain framing of ethical issues dictated by ontological understandings of human nature, barely leaving room for other framings of the value debate.

The monopolized character of ethics in Austria was to a certain degree reflected in the discussions at the Round Table in the assumption of many participants that ethics is best handled in an institutionalized committee consisting of experts and representatives of societal interest groups.[30] The monopolistic character of the ethics committee further influenced the Round Table discussions in the lay participants’ desire for a central agent able to assemble and reconcile heterogeneous values by providing collectively binding moral advice. Because ethics has been monopolized by a few actors, the participants at the Round Table were also hesitant to label their engagement as “ethical”, calling it “moral” instead. What also became visible in the discussions at the Round Table was the desire to delegate the responsibility for ethical decision-making to certain authorities that were regarded as

---

[30] On the other hand, the lay participants struggled to find a composition of the committee that was “representative” enough in order to legitimate its decisions.

better equipped for engaging in the discussion. Of course, participants also called for public deliberations on ethical issues, which involves the public to a larger degree; however, in the end, institutions were seen as the legitimate space for collective decision-making.

The fourth aspect of the Austrian ethical and participatory culture concerns the particular understanding of the "public" and its role in society in the presence of the *conception of a potentially malleable and technology-adversarial public*. A common assumption about the Austrian public is that it is hostile towards technology in general (Torgersen and Seifert 1997). This assumption, widely shared in science policy circles, stems from a few instances where the public heatedly rejected technologies, in particular the nuclear power plant in Zwentendorf, which was rejected in a referendum in 1978 (see, for example, Hirsch and Nowotny 1977; Nowotny 1979), and the public resistance against a hydroelectric power plant in the Hainburg floodplain forest in 1984. These instances, supported by the outcomes of the EU's *Eurobarometer* surveys, led to the idea that the Austrian population suffers from a general aversion towards technology. Zwentendorf and Hainburg were both instances where, for the first time since the Second World War, the formation of a civil society resulted in a large mobilization and in changes in the authorative methods of official politics. However, it was a form of "uninvited participation" which was not very well appreciated by politics. This results in a political paradox: the public should engage and participate in (technological) matters, but if the outcome is not in favor of the political elites, the idea of a malleable and ignorant public is invoked. A second paradox arises between the idea of the aversion of the public towards technology in general, and the political encomium of the innovative potential of Austria, where it is praised as a leading innovator, particular in high technology. The assumed general "aversion" of the population towards new technologies must be related to the fact that, in nearly all cases, research and technological application goes on without any controversies. Wider public rejection focuses on only a few technologies, namely nuclear technologies and agricultural GMOs. Hence, it is interesting to observe in what instances politics invokes the narrative of a public that suffers technology aversion. Often, the "public" is drawn on when it is feared that a particular interest group may object to certain innovations.

The idea of a malleable public finds its continuation in the discussion around (bio)ethics in Austria. The public is seen as subject to demagogic manipulations, and thus as holding unreasoned opinions. Here is a quote from an article by two members of the *Bioethikkommission* in which they discuss the relation between ethics and the public:

> Instruments of participatory democracy are certainly not a convincing alternative [to expert panels]. They refer to "the population", which is constantly re-invented in plebiscitary campaigns to stage politics as a medial and emotionalized event. … The request for increased involvement of the public in bioethical and biopolitical debates is emphatically to be supported with regard to democratic politics, but encounters considerable difficulties in its re-

> alization. In pluralistic society, "the" public does not exist, but only different publics which partially perceive each other and which seldom can be assembled into the one great public. (Gmeiner and Körtner 2002, 170)[iv]

The public is regarded as an artifact that is often created by populist conceptions and thus not to be trusted: "What 'the' public thinks is hard to grasp. The attempt to penetrate this 'black box' is the domain of opinion research, which of course can only deliver constructs of the public"[v] (Körtner 2008b). In these accounts, two conflicting notions of the "public" are put forward: On one hand, the national ethics committees are calling for more public debates on ethical matters. What is meant is, however, more "published" debates among legitimate actors in this domain, that is, "experts". That such a "public" debate does not exist is attributed to a lack of appropriate (print) media in the Austrian context, and the German discussions are often pointed out as a role model. The other legitimate place of a "public" debate is seen as the institutions of representative democracy, that is, the parliament. On the other hand, there is also a negative notion of "public" debate. In this notion, the public is thought of as "emotional", "irrational" and subject to populist and demagogic seductions. These two notions can be played off against each other; that is, one can call for a "public" debate without including the wider public in the ethical debate.

The ethical discussion dominated by the *Bioethikkommission* is a rather elitist discourse, which is in need of drawing boundaries around lay opinions—based on an "elitist" (Jasanoff 2003b) or "technocratic" (Weingart 2001) understanding of expertise, where high entry barriers are rhetorically erected. In principle, everyone is allowed to talk about ethics; "however, it is important that the systematic approach does not get lost in the reflection. Ethical discussion should not be carried out at the level of the 'regulars' table'"[vi] (Österreichischer Forschungsdialog 2008). Hence, for ethics specific criteria are defined by experts that allow ordinary people to participate in the discussion, something which Lévy-Leblond (1992) identified some time ago, with regard to early PUS activities, as against the basic principle of democracy. The official ethical discourse in Austria regards the public as malleable by forces that cannot be controlled by the experts in power. Thus, criteria are sought that allow for an exclusion of these voices.

The idea of a public which opposes technology in principle and which is malleable was also present among the arguments of the Round Table participants—both the lay participants and the researchers. This is reflected by the fact that the lay participants welcomed public participation on the level of "talk", but rejected the idea that people "like them" could be put in a position where they could decide for the whole society. While the existing ethics commission was perceived as problematic with regard to transparency and membership, this institutional form was nevertheless the role model for good ethical governance if the wider public was represented—not exclusively, but in balance with scientific experts from different disciplines.

6. Tracing “ethics” in a public engagement setting: Empirical setting and methodological approaches

This dissertation is based on a research project, “Let’s talk about GOLD! Analyzing the interactions between genome-research(ers) and the public as a learning process”,[31] of which the main element was the organization of a public engagement event—the Round Tables—where a group of lay people and genome researchers met over a longer period of time to discuss ethical and social aspects of genome research. The Round Table, as well as interviews with all participants, provided the empirical data for my dissertation.

The central idea of the project was to stage a “collective experiment” in public participation in ethical and social aspects of genomics. “Experiment” refers to our aim to deliberately modify key parameters of public engagement settings in order to test implicit and explicit assumptions of the relations between “the public”, “science” and public engagement. “Collective” means that many elements of our engagement setting were subject to changes emerging from the discussion process itself, and therefore the project was open to input from the participants. We modified the public participation method of the “Round Table” that was developed by the Swiss foundation *Science et Cité*. Its basic principle is to let a group of lay people accompany a bigger research project/topic over a longer period of time. Experiences for setting up the project came from another project that analyzed and compared public engagement exercises and their role in a “socially robust politics of knowledge” in several European countries (Felt, Fochler, and Müller 2003b), as well as from evaluations of public understanding of science activities (Felt, Fochler, and Strassnig 2003).

6.1. The “Round Table” as a “collective experiment” of public engagement with science

The idea of a “collective experiment” was central to the design of the setting. We aimed to set up a rather different “trading zone” for the negotiation of values by taking up several aspects and problematizations of public engagement and ethics. What are the basic characteristics of the “Round Tables” that we aimed to modify for our public engagement setting?

---

[31] The research project was carried out at the Department of Social Studies of Science (University of Vienna) and funded by the GEN-AU ELSA program of the Austrian Federal Ministry of Education, Science and Culture. The principle investigator was Ulrike Felt. Annina Müller, Astrid Mager, Maximilian Fochler and I worked at the project at the Department of Social Studies of Science, University of Vienna, and Sandra Karner and Bernhard Wieser at the IFZ Graz. The project ran from 2004 to 2007.

Upstream engagement: In order to allow the participants to address a wide range of ethical and social questions, the question was in what kind of research the lay people should be involved. The selection of the genomics project was an attempt to choose a project that on the one hand understood itself as "basic research", but on the other hand was related to contemporary issues in public health and thus might have implications for a wide range of actors including members from the public. Hence, the lay participants in particular would have the possibility to discuss more fundamental values underlying innovation processes as well as imagined outcomes and their consequences for society. While other models of science communication and public engagement step in at a rather late point in time, when many institutional commitments have already been made and a wide range of issues is no longer open for debate, upstream engagement rather poses questions about the innovation regime as such, and thus seeks "to force some of these questions back on to the negotiating table" (Wilsdon and Willis 2004, 29).

Symmetrical participation: In order to recover the shortcomings of science communication within a deficit model of public understanding of science, recent public engagement events had taken the idea of the public's "speaking back to science" too far, so that there was little room for those who spoke in the name of science to articulate their expectations, hopes and concerns apart from delivering the "facts". Thus, we aimed at involving both lay participants and researchers in a symmetrical way. Symmetry concerned both the number of members invited to the Round Table and the discussion procedure. For example, we abstained from using academic titles when addressing each other, in order not to further facilitate already existing hierarchies between lay participants and researchers. All members were free to provide input at any time during the discussions; the facilitator was instructed to take special care of those who had less trust in their rhetorical abilities, and thus were rather silent.

Long-term engagement: Our aim was to allow for a continued debate that could refer to prior discussions and thus elaborate on some issues in more detail. The long-term engagement allowed for the participants to reflect on the discussion and re-problematize certain issues later on. While in short-term interactions participants tend rather to "sell" their "messages", an ongoing engagement may open up the space for discursive ambivalences and complexities that we think are a feature of current technosciences which needs more reflection. This is of crucial importance for the discussion of ethical and social aspects, because the Round Table had to be able to provide enough time to allow for the development of complex arguments and continued discourses. Long-term engagement also allowed for developing (negative and positive) social relations between the participants, and thus the articulation of dis/trust relationships between researchers and lay participants based on concrete experiences.

Open dialogue: Our aim was to create an open space with as few rules as possible. Our facilitator was briefed to intervene as little as possible, but to take care that a fair discussion took place. Of course, our setting was far from being "neutral", and, moreover, had a strong performative dimension that is included in our participatory setting as well as in social science method (Law 2004). Our interventions took place during the design of the setting, and we aimed for little input as possible during the discussion process. The open space idea was continued over coffee and lunch breaks, where the researchers and lay participants had the chance to discuss outside of recorded plenum sessions, and thus mutually engage in a rather personal way. The open dialogue aim was supported by the confidentiality of the discussions, because the Round Tables did not take place in public. "Open dialogue" also meant that we as organizers never explicitly or implicitly expressed any expectations about what would be the desired outcomes of the process.

Process as outcome: We explicitly did not demand a concrete output such as a consensus paper from our participants, but regarded the process itself as the main "product" of our project. We assumed that a pre-defined output would narrow the discussion process too quickly and demand the entire attention of the participants. Furthermore, we decided that the discussion would not directly feed into policy, in order not to trigger the participants' expectations with regard to a direct political input. However, our focus on the process itself left some of the lay participants quite puzzled about what the aim of the Round Table had been.

These aspects were communicated—as well as our intentions for the project—at the beginning of the Round Table.

In our version of the Round Tables, a group of 14 lay people met with a group of genome researchers working together on a specific research project on the genomics of lipid-associated disorders ("GOLD"). The number of Round Tables organized was six, and they took place over a period of 8 months from September 2004 to May 2005 in Graz, Austria, where the bulk of the GOLD researchers were working.

The lay participants were selected via a nationwide call through posters and leaflets in public institutions with an educational mission (public high schools, museums, etc.). This strategy was chosen on one hand to reach a quite broad range of the Austrian population, and on the other hand to specifically target audiences relevant to the project goals (affected people, people related to the issue of gender and health). Additionally, we advertised in a local newspaper and sent bulk mail to the local population in Graz, where the Round Tables took place. The applicants were asked to write a short paragraph on their motivations for participating in the activity, and to provide some basic personal data. This information was used to select the participants. We aimed at a balanced selection regarding sex, age, education and personal motivations. However, our goal was not "representativity" of the Austrian population, but heterogeneity with regard to personal motivations while maintaining a balanced distribution of sex, age, and education. While we achieved this with the former two, the actual selection of education had a strong bias towards higher education. We can only speculate about the absence of applications from people with no formal education, but it was surely due to the issue of genomics in the domain of lipid disorders, as well as to the large time investments we demanded from the participants.

We recruited researches with an internal call within the GOLD project. We presented our project personally at an internal meeting of the GOLD project, and then made a call for participation via email. In the end, 13 different researchers contributed to the discussions at the Round Table, eight of them on a regular basis. All hierarchical levels were represented at the Round Table, including two PhD candidates, two post-doc researchers, the project manager, two subproject leaders and the project leader. Additionally, specifically selected researchers contributed at different Round Tables. We were unable to establish a balanced group regarding sex, age and academic position, as all project leaders were male while the young researchers were female. The given hierarchies in the GOLD project also fed back into the Round Table discussion, as the researchers implicitly assumed that the project leaders were the legitimate spokespersons for the researchers' positions with regard to social and ethical aspects. Thus, the young researchers barely contributed to the discussions, and provided insights only when directly addressed.

The six Round Table meetings were full-day discussions, usually taking place on Saturday in a seminar room of the university department where one subproject of GOLD was located. A facilitator, who was not part of the social science research team, moderated the discussions, in order to provide the participants an independent arbitrator for their concerns. The first three Round Tables were devoted to the presentation of the GOLD project and various sub-projects (including lab visits) as a preparation for the following discussions. There, the participants defined the following topics for Round Tables 3-6: science and the media; ethical issues of genomics; and regulation and governance of genome research. To each of these thematic Round Tables, a specific expert was invited (a science journalist, an academic ethicist, a representative of a state regulatory body for genome research) to introduce the topic and to discuss the issue with the participants. The Round Tables mainly took place as plenary discussions with all participants sitting around the table; however, small group discussions (mixed or with researchers and lay participants only) were also organized in order to allow for a different mode of discussion and to provide input for plenary sessions. Additionally, we organized a seventh Round Table with the lay participants only, in order to let them reflect on the whole process of the Round Table without the presence of the researchers. Because of the researchers' time commitments, an additional Round Table for their reflection was not possible.

We conducted interviews with all regular participants (14 lay people, 8 researchers) before and after the Round Table discussion in order to trace changes in the participants' positions. The aim of the *ex ante* interviews was to document the participants' attitudes towards several dimensions of science and research and expectations for the upcoming Round Tables. The aim of the *ex post* interviews was to document the experiences on a more individual level, as one of the goals of the project was to grasp the learning effects for the participants.

All interviews and Round Tables were recorded and transcribed for further analysis in the "Let's talk about GOLD!" project, and provide the data basis for my dissertation.

6.2. Why choose the "Round Table" as a setting to discuss ethical and social aspects of genomics?

One of the basic aims of the "Let's talk about GOLD!" project was to contribute to an enlarged vision of social and ethical issues linked to the field of genome research, which goes beyond the classical, purely expert-defined perceptions. Usually, ethics is discussed between "experts" in specially established bodies. Its members have to prove their expertise in order to be assigned to an ethics body. In light of critical public understanding of science and the "participatory turn" in science governance, we aimed to set up an experiment where ethically non-trained persons—lay participants and researchers—could discuss ethical issues, in order to think about whether their contributions could provide an alternative vision of how ethical issues can be addressed. The Round Table provided a platform where those who can be regarded as *"producers" and "users" of the knowledge meet* and discuss their visions of the ethical and social aspects of genomics.

Official discourses on ethical issues in technosciences are characterized by a strong hierarchy of knowledge. The expertise provided by ethics bodies is considered more authorative than opinions held by the public with regard to ethical aspects of science and technology. Thus, the role of the public in this discourse is rather marginalized, particularly in the Austrian context. Though members of expert committees often claim that they want to facilitate "public discussion" on bioethical issues, it remains unclear how precisely such a discussion is imagined, beyond the published opinions of experts for consumption by the public. The Round Table provided a setting were it should be possible to debate ethical issues without a predefined hierarchy of knowledge on ethical and social aspects of genomics. The Round Table, however, had no intention of replacing ethics committees and other forms of institutionalized ethics. It also did not aim to provide a definite decision or recommendation for ethical issues related to genomics; that is, it did not assuming the framing of institutionalized ethics. The aim was to *provide different perspectives on ethics,* which could possibly nourish professional ethics with experiences from other more open settings, as well as to generate accounts of ethical issues able to challenge framings of issues by institutionalized ethics (Jasanoff 2003c, 2003a)

In contrast to ethics committees that provide expertise for governmental decision-making on bioethical issues, the Round Table was *linked to a concrete research project* and to the work of the participating researchers. This does not mean that ethical issues only had a narrow, project-related focus. In fact, the participants addressed ethical questions mostly in a more general way, oscillating between "ethics in general" and "ethics in particular". However, the presence of a concrete genomics project and the people involved in it provided an "anchor" for the discussion that allowed it both to abstract from the particular research and to return to it when needed. This should have allowed for a debate over ethical questions "in here" by people who personally have to bear the consequences of both the scientific research and of rule making based on recommendations by professional ethics. Thus, I think it makes a huge difference in the framing of ethical issues whether one gives general recommendations on wide-sweeping bioethical issues or addresses ethical concerns in the context of a concrete research project.

The Round Table, thus, *goes well beyond the notion of "empirical ethics",* which is a very narrow one with regard to the role of the social sciences and the abilities of non-experts in ethics (in our case, both lay participants and researchers). First, ethics in social contexts was not just observed for theoretical reflection, but the participants were actively asked to engage with ethics in the field of biomedicine. Thus, the ethical discourse at the Round Table was not another source of data in the way that scientific facts provide data for ethicists' analysis. The participants were well aware that they were engaging in and contributing to a discussion of ethical issues of biomedicine. Second, I aim to analyze what ethical concepts the participants used themselves, and in what kind of wider conceptions their ethical reasoning was embedded. Hence, my theoretization of the ethical issues discussed at the Round Table is strongly linked to the discourses of the participants, and does not take place independently from them, as it is often the case in ethics based on the metaethical distinction of facts/values or "is" and "ought". Third, "empirical ethics" is framed as a problem of interdisciplinary co-operation between social sciences and ethics. Thus, the discussion and negotiation remains among experts. We, however, regarded the participants as "experts" on debating ethical issues.

6.3. Data Analysis

The analysis of ethics at the Round Table was made using the Grounded Theory method (Strauss and Corbin 2000). The Grounded Theory approach was chosen because of its synoptic consideration of theorizing and data analysis, which allows oscillating between both levels, and because it does not predetermine empirical analysis by *ex ante* imposed theoretical frameworks. While I did not follow Grounded Theory in a formulaic way, it provided the guiding perspective for reaching an inclusive consideration of theoretical, contextual and concrete empirical elements.

All data were encoded with Atlas.ti software using iterative processes of global coding in order to develop a code set appropriate for the data. Global coding also served to identify the main themes and discursive lines of the ethical debates. Then, relevant ethical issues at the Round Table were selected for analysis in detail. The relevance of issues was, on one hand, determined by the participants themselves through the emphasis they put on certain issues in the discussions, and by identifying crucial ethical issues in the *ex post* interviews. One the other hand, the selection of ethical issues was informed by larger theoretical discussions in STS, as well as in relation to wider societal debates. The relevant ethical issues and the discursive processes of the discussion on ethics are discussed in the following empirical chapters.

As the Round Table was designed to trigger a "learning process" between lay participants and researchers, the discussion had a clear timeline, according to which arguments made later at the Round Table built on earlier experiences. For example, based on the encounters with the researchers, the lay people first had to form and perceive themselves as a coherent group, which then helped to articulate positions in response to the arguments of the researchers with regard to certain topics, such as the responsibility for societal consequences of genome research. Therefore, it is crucial to take into consideration for data interpretation that the Round Table was a process; on the other hand, it is also important to identify recurrent themes and topics to pinpoint what narratives and issues were important to the participants. Therefore, data interpretation is a "discourse" analysis, in the sense that it makes arguments based on a multitude of statements by the participants, while, however, taking strongly into consideration that the Round Table was also a process that developed and changed over the course of the discussion.

7. Research questions

The general purpose of this thesis is to illuminate basic aspects of a discussion on ethics in a "bottom-up" public engagement setting, beyond the usual expert-oriented settings, and thus to inquire into the possibilities and limits of citizens' (researchers' and lay people's) engagement with ethics. Such an analysis has barely been done in detail before. However, such work is crucial for understanding the relations between science, the public and the role of expert ethics in our contemporary society, because the current division of labor between these actors is built on a wide range of assumptions that have not been scrutinized in detail before. Ethics today is a substantial feature of innovation regimes that include visions and expectations of technosocial futures. Thus, it seems crucial to juxtapose dominant policy regimes with the responses of those "affected" by their policies, in order to analyze how these link up (or not).

I posed three broad research questions to the material:

1. What issues counting as ethical have the participants discussed?
2. How did the participants discursively deal with ethics?
3. How did the participants evaluate their participation in a discussion about ethics, and what did they "learn" from it?

The first research question seeks to analyze the main ethical issues that were raised by the participants. This question is related to the fact that institutionalized ethics frames ethics in a particular way and thus gives priority to very specific issues. Thus, my research questions seek to shed light on *researchers' and lay-people's framings of ethics*. In more detail, this question asks

- What ethical issues do the participants define as crucial with regard to genomics, and also with regard to science and research on a more general level?
- Who should be concerned with these questions and who should be involved in a debate about them?
- Where are the legitimate and appropriate sites where these questions can and should be asked?
- What kind of innovation models and models of science and society relations are suggested that are linked to how science and society can deal with ethical questions? How do the concepts of ethics relate to wider notions of the relationship between science and society?
- When should an ethical deliberation take place in the so-called innovation process?
- What models and "lay theories" of ethics do the participants enact?

- What are the differences in the way researchers and laypeople address ethical issues?
- How should researchers deal with ethical issues, both in the eyes of the lay participants and according to the researchers themselves?

The second general research question considers the Round Table as a trading zone for values. Institutionalized ethics widely neglects the fact that their ethical opinion-making takes place within a certain social context. Thus, I aim to analyze the *discursive mechanisms and strategies at work* in the discussions that are an integral part of every ethical talk.

- What discursive processes take place at the Round Table that shape the discourse on ethics? Are there hierarchies and power structures that shape the ethics debate? What different "micropolitics" are performed at the Round Table?
- How does the participants' engagement with ethics contribute to the construction of individual and collective identities at the Round Table (and beyond)?
- How do the participants relate knowledge to the ability to make ethical judgments?

The third main research question analyses the researchers' and lay people's self-evaluations of their involvement in a discussion on ethics. This set of question takes seriously the Round Table as a kind of *learning process,* and asks what difference it makes that both researchers and lay people took part in the Round Table.

- What changes take place in the perception of ethics during the discussion process?
- What do the participants "learn" from their participation in the Round Tables, with regard to ethics as well as with regard to their perceptions of each other?
- After participants have engaged with ethics for a rather long period, what role do they attribute to ethics in the governance of science more generally?
- How do the lay people perceive the researchers' dealing with ethics in relation to their societal status and role?
- Given the strong expert orientation of institutionalized ethics, how do the participants perceive the relation of their ideas to those of ethics experts?

These research questions aim to describe the participants' dealing and experience with ethics as complex, and thus to provide a multi-faceted picture of public participation in ethics. Because little research has yet been conducted in this field, I aim to provide a broad picture of the discussions. I will conclude by providing a range of conclusions about the possibilities and limits of a public engagement in ethics, and its possible wider consequences for the contemporary innovation regime.

8. Exploring lay ‘ethical’ knowledge: Negotiating values of science-society relations and knowledge production

In the following chapter, I aim to shed light on the participants’ *lay ‘ethical’ knowledge* as a bottom-up perspective on “ethics”. I will do so by analyzing how the main “ethical” issues were discussed at the Round Table. My analysis focuses on two different strands of the discussion. First, value questions concerning the *relations between research and societal values*. In this domain, I identified two relevant ethical issues around which values were negotiated:

- I will start (chapter 8.1) by analyzing the discussion of what counts as a problem and its solution, focusing on the issue of “obesity”: What is the nature of the “obesity problem” and what kind of “solutions” should be drawn upon in order to solve the problem? What are the values the participants draw upon when making their arguments?
- In chapter 8.2, I analyze the discussions around the issue of “responsibility” of research: What different positions were debated at the Round Table on the question of who should and can take responsibility for the consequences of the knowledge produced by research? For what can responsibility be taken and for what can it not? What are the different understandings of how responsibility should be enacted?

The second strand of the ethical discussion at the Round Table focused on the *means of knowledge production*. This discussion primarily revolved around the issue of animal experimentation, but quickly left the terrain of a pure moral question—“is animal experimentation right or wrong?”—and revealed aspects that are not typically associated with animal ethics in academic ethical debates on this issue:

- First, I investigate how the drawing of ethical boundaries in animal experimentation served to (co-)construct the individual and collective identities of the researchers (chapter 8.3).
- Second, I analyze the relations between the way the researchers articulated their position towards animal experimentation and the lay participants’ trust in the researchers (chapter 8.4).

The term “lay” in “lay ethical knowledge”, refers to “expertise” on ethical issues—thus, both researchers’ and lay participants’ discourses of ethics fall under the category of lay ‘ethical’ knowledge. I will use the term “knowledge” rather that “epistemologies” because the latter term by Jasanoff (2005) would need the clarification of dimensions such as objectivity strategies, styles of public accountability, demonstration practices and so forth. I assume that in the public there are ethical epistemologies present; however, to analyze these would require me to go beyond the available data and conduct further empirical research. Thus, I will use the more modest term “knowledge”, which also allows for incoherent and fragmented pieces of the participants’ engagement with ethics to be included in the analysis. The notion of lay “ethical” knowledge is useful to express that the participants’ understanding of “ethics” was not necessarily be based on a coherent set of principles in the sense of academic ethics. Furthermore, I put the term “ethical” in quotes, as it was not always certain whether the participants saw themselves in an “ethical” discussion or in one on “values” or in a “political” discussion.[32] However, this is not (only) a definitional or analytical problem, but a substantial part of the discussion itself, reflecting wider societal issues with regard to ethics.

---

[32] This problem is addressed in chapter 9.

8.1. Defining problems and solutions as ethical questions

In this chapter, I will analyze the “ethical” issue at the Round Table around the question of *defining “problems” and solutions*. Concretely, the debates revolved around “obesity” as a central subject in the work of the researchers, which they focused on in their descriptions of their work from the beginning. The issue of “obesity” was an interface which linked their basic research on the genetics of fat metabolism to general societal issues such as public health. Around this issue of obesity, a debate emerged over what kind of problem “obesity” actually is. What is the nature of this “problem”, and what are its “causes”? On what different values did the participants draw to enforce their arguments? In the second section of this chapter, I will engage with the question of what kinds of solutions are proposed to “solve” this problem. How should the problem of “obesity” best be addressed, and what values are drawn upon in order to justify the proposed solutions?

The attribution of the word “ethics” to this part of the Round Table discussions is in need of some explanation. Indeed, it is a problem as such (see chapter 9), as the participants often did not explicitly label their discussions as “ethical”. However, assuming that “ethics” is more than an issue of labeling, the discussion of problems and solutions was a debate about different and often opposing values that were called into play by different definitions of problems and solutions. Because the participants provided underlying reasons why they opted for certain problem definitions as well as solutions, the discussion can be seen as an “ethical” one in the sense of the definition of ethics I provided in chapter 1.3. I will take up this question again in the concluding remarks to this chapter, and relate it to the wider context of innovation policy.

Defining obesity as a problem: Between fact and contingency

How did the researchers define the “problem” they were working on? And what were the lay participants’ readings of this definition, as well as their own views on what the “problem” was? In analyzing this, my hypothesis is that, behind the debates around “problems”, different values are negotiated, which are linked to different presumptions about the social and political, thus linking “ethics” to political and social contexts. In the following, I will analyze value tensions in the debates that characterized the discussion of the question of what is a “problem”.

In these debates about what counts as a problem, a particular tension was included which could be—on a general level—described as follows: The researchers’ claim was that obesity is a “disease” that exists independently from what the researchers do—and as such in two different ways: On one hand, obesity is a disease associated with genes, thus it is determined by nature. On the other hand, the problem is also determined by society. Thus, they argue, they only “pick up” the “problem” that has already been defined by others in society. Many of the lay participants indeed agreed that the problem was a given one. However, in their view, *this is* the "problem". While the researchers used "society" as a justification and as a moral resource to legitimate research, "society" was the subject to be explained for the lay participants; that is, it could not be taken as a justification for research. On the contrary, they argued, what is socially given must be questioned in terms of possible alternatives. That is, one basic tension at the discussion at the Round Table was between *"problems" as given fact vs. as contingent*. I will start with the way the researchers defined the "problem" they were working on:

> The problem we are working on is the new plague, namely obesity. And the reason why we know so little about it is that it is hard to grasp: it is a very complex disease. (S6/RT1/2/13)[vii]

In this quote, the researcher makes a direct link between obesity and "disease" in a biomedical sense, that is, as rooted in the genetic makeup of persons. She also relates obesity to public health issues by describing it as "the new plague" and thus defining it as a problem for the whole society. Hence, the "problem" is defined twofold: On one hand, the "problem" is regarded as a matter of "nature", of the genetic makeup of the body. As such, it is of interest for "basic research" regardless of its relevance to society. In the discussions, the researchers often invoked the narrative of explaining nature as driving their interests, as this researcher stated: "As a basic researcher … I'm primarily interested in gaining new knowledge. I want to know how the fat metabolism functions" (S8/RT1/2/69).[viii] Defining themselves as "basic researchers", they argued that nature poses challenges and riddles which they aim to solve. On the other hand, they strongly related their research to problems that have been defined as such by society:

> we have, so to speak, received the offer by the public through these programs: 'we as the public offer you a lot of money for something that is of interest for us'. And we have engaged with it. (S7/RT5/2/476)[ix]

Here, the researchers argue that it has not been they who have defined obesity as a problem that should be tackled, but that "society" has defined it as such and then turned to science to help solve the problem. This "offer" is, so they argued, the consequence and outcome of more general cultural need in society, namely of being slim:

> So … if you look at how many people buy the Brigitte [German women's magazine] diet or such things, or at 'Slim Fast', to some it is obviously a need. If you … walk by the shelf with the women's magazines, there is some new diet on each cover page. On the non-fiction shortlist, the South Beach diet is on top and the Atkins diet on second place, and that for months. (S6/RT5/2/389)[x]

Acting as a "lay sociologist", this researcher explains that there "is" a societal demand for means to counter obesity based on a particular cultural attitude. By providing “evidence”, the researchers sought to “prove” this demand as a “sociological fact” that could not be denied by the lay participants.

Hence, in the researchers’ view, it is both “nature” and “society” that tell research what “is” a problem. By taking into consideration both contexts as the “cause” of the “problem”, the researchers were able to establish a double narrative by which different values could be promoted simultaneously—on one hand focusing on research based on the riddles posed by nature and the value of knowledge production uninfluenced by subjective or social interests, while on the other hand taking up interests that are shared by society and hence showing “solidarity” with society and its problems. Hence, in justifying their research by means of the problem definition, the researchers aimed to reconcile internal and external interests and values; that is, to adjust the internal values of science—the quest for new knowledge—to the external values of society—improving public as well as individual health. With this scheme, the researchers aimed to uphold the relevance of their research to the scientific community as well as to society; however, they also confirmed boundaries between science and society. However, the researchers put themselves in a rather passive position with respect to their research objects, as it is not they themselves who actively define the problem they are engaged with—it is either “nature” or “society”. This can be explained by the fact that the researchers saw themselves in an “ethical” discussion during the Round Table, which was generally perceived as potentially challenging their own “cultural” practices, in particular when the discussion engaged with research itself and not just its outcomes. Hence, they aimed to defend research by attributing the forces that define “problems” to the outside—be it “nature” or “society”. In doing so, they believed that they were in a position that could not easily be ethically contested by the moral concerns of the lay people.

However, in their critique, the lay participants touched upon many aspects of the researchers’ “problem” narrative by attempting to open up value questions as contingent. In particular, the lay participants targeted the researchers’ definition of obesity as a form of *medicalization*:

> the definition of disease is questionable for me, and then next came ‘Oh, well! We know that adiposity is a genetic disease’. If I now say a large proportion of the population is adipose, and the whole thing is a genetic disease, then I define the majority of the population suddenly as ill by definition. (L6/RT1/2/164)[xi]

In this quote, the lay argue that the researchers are medicalizing the population by defining a condition and behavior as a “disease” and thus making it subject to medical and genetic interventions. This was seen to contradict their basic research narrative, in which they highlighted that they were only interested in gaining new knowledge. Thus, the lay discovered a contradiction between the researchers as non-intervening according to the narrative of basic research, and as interventionalist in defining obesity as a “disease” in need of medical treatment. Behind the critique of the lay participants was the presumption, which was widely shared among the lay participants even after the Round Table, that the researchers were not “authentic” regarding their “real” goals of research. As such, many lay participants assumed economic motives behind the research on obesity, as one lay argued that only an extremely small proportion of the population could be regarded as adipose and thus as ill. For him, such a small target group could not justify the deployment of high amounts of money; thus, there must be other aims behind the research, namely to address people “with a small beer belly” (L1/RT1/2/39), that is, a great number of people. He argued that people are medicalized because of economic interests.

Second, some of the lay participants identified the “problem” as one that is not caused by genes and thus by “nature”, but rather solely by society and cultural habits:

> Actually, in an affluent society where there is a huge offer of many things, [where] ‘I, being overweight, have to abstain from something that tastes good to me’, I think that this psychological domain plays a role too. What tastes good, I have to abstain from to reduce my weight. Thus, on one hand I’m confronted with the difficulty of abstaining from something, and on the other hand we have this huge offer—the seductive force of the offer. And where can I find the middle ground? And this surely has to do with the psyche. (L14/RT1/2/67)[xii]

This lay argues that obesity is not due to genetic causes but to the particular culture people live in, which overburdens people with consumption and where renunciation is not a quality that is favored. Thus, her explanation of obesity is a psychological and social one, that is, that individuals cannot resist the temptations of the affluent society. To a certain degree, the researchers were also sympathetic to this analysis; however, they regarded changes in lifestyle as only a “trigger” of the “obesity epidemic”, as its true cause was located in the genetic disposition of individuals. In doing so, they frequently referred to cases in which mice with a special makeup could eat as much as they wanted but did not become fat. For the lay participants, it was clear that the cause for the obesity epidemic must be located in changes of lifestyle, as other societies and other times did not suffer from it. Thus, as the “cause” for this problem was a social one, it had to be solved in this domain. As such, discussing obesity served as a way to express a wider cultural *critique of modern society,* in particular in its economic dimension, where the “real” needs and nature of humans are imposed by modern culture and its globalized structures, which seem out of the scope of governability. The argument of the lay people was that the ability of individuals to contribute to wider societal and cultural changes is rather limited.

Third, the discussion of the cause of “obesity” provoked debates over whether “obesity” was a “problem” at all, which revealed another tension: If something is a problem, for whom is it? As the researchers aimed to the define the problem of obesity as one not only for “basic research” but also for society, the discussions at the Round Table shifted to a debate over the question of *what counts as a “good* *life".* The particular tension that became visible here was between a *collective consensus and individual autonomy in decision-making.*

The researchers argued that they only "picked up" a societal problem in their research. In doing so, they argued that the problem was based on a collective consensus rather than on the particular interests of a small group of actors. They argued, "Namely I believe that not all, no question, but a large proportion of the population has a rather similar—not the same but a rather similar—attitude concerning a good life." (S6/RT5/3/350).[xiii] As they justified their research by "social problems" that are widespread, they had to define what counted as a "good life". The assumption was that obesity meant a "reduced quality of life" for most people, and many were unsatisfied with their situation. They assumed that there was a "collective consensus" of what counts as a "good life". The "good life" defined by the researchers was a life that did not suffer the burdens of obesity; thus, they assumed that there was a collective consensus that being slim was considered good. In their account, the greatest number defined the greatest good.

For some of the lay participants, however, what counts as a "good life" could not be defined for the whole society. They rather held personal autonomy in choosing lifestyle as a value preference. In his critique, one lay argued that the societal dominance of the slimness narrative is also a problem as such:

> On the other hand there is [the] argument—people or so who say there is nothing worse than this youth, slimness, reducing weight delusion … And there is the saying "The fat are jovial." Hence, are these, so to speak, not arguments that can be taken in contrast, that say 'I do not want to reduce weight or I do not want to get slim.' They do not want to lose weight at all. (L4/RT5/2/379)[xiv]

The lay participant argues that a collective consensus of a "good life" has to be assessed regarding its downsides because the enforcement of dominant values may endanger the capability of individuals to enact their own vision of a "good life". Thus, the lay participant juxtaposes the collective consensus that is based on the greatest number of people with the value of individual autonomy in decision-making. Individual autonomy in choosing lifestyles was the value some lay participants advocated, a value which they saw as endangered by the normative visions of the researchers.

While for the lay participants the "problem" of obesity was characterized by social contingency in terms of its status as a "disease", its "causes" and its relations to dominant cultural norms and individual autonomy, the researchers were rather convinced of the "fact" nature of the problem they were addressing with their research. It was both a "problem" that was rooted in the genes and thus in nature and a problem that was rooted in societal "facts", that is, its costs for public health, individual burdens and a culture in which obesity is seen as a problem. The tension revealed in this debate was, in a more general sense, that the lay participants regarded obesity as a problem that is socially negotiable. For the researchers, the problem as such was barely negotiable, since many dimensions of it were considered "facts." This is not to say that the majority of the lay participants did not consider obesity a problem; however, they considered it one the terms and conditions of which were subject to debate. However, there was dissent about the nature of the problem: the researchers predominantly characterized obesity as a disease in a biomedical sense, while the lay people saw it as a certain lifestyle. While some lay argued that the choice of lifestyle should be an individual decision, others clearly regarded this lifestyle as having large consequences for individual and public health, and thus agreed to a certain degree with the researchers' assumptions. However, there were huge differences in the way researchers and lay thought the obesity problem should be addressed. In the following, I will analyze how "solutions" and their values were negotiated at the Round Table.

Defining solutions: Between "social" and "technical"

In the debates around the question of what kind of solutions might be appropriate to address the obesity problem, different tensions came to the fore: First, the *tension between "human and social" and "technical" solutions,* which was debated with reference to the "fat pill". A second tension concerned the question of what values should govern the allocation of research money, and was a *confrontation between solidarity in terms of distributive justice and the internal values of science,* such as scientific "excellence" and "quality". The third tension was around the role of citizens and society in the enforcement of "solutions" in which *collective responsibility and individual freedom* were juxtaposed. I will discuss them in turn.

Based on their definition of obesity as a disease rooted in genetics, the researchers made a strong argument for biomedical solutions to the problem. However, given their strong motivation as "basic researchers" and their prime interest in gaining knowledge, they did not aim to be too concrete in their suggested solutions, as this would have weakened their position as basic researchers and hence the "exceptional" moral position which the assumed for themselves as basic researchers. Thus, in a balancing act, they managed to rule out solutions that were not based on a genetic understanding of the problem one by one, more or less implicitly suggesting their particular kind of solution without being too definite about it.

> Now, there are several possibilities for how I can resolve this. The easiest is … yes, I go to the gym, do a little bit of sports and eat a bit less, one apple a day would arguably be enough, wouldn't it. … that obviously doesn't work, otherwise we wouldn't have the problem, if it were so easy. A short-term change is reachable in most cases, but in the long run it simply doesn't work, That means, it would be the healthiest, it is the best, it is above all the cheapest, and nevertheless it doesn't work. This means, there must be other possibilities. (S6/RT1/2/14)[xv]

These other possibilities were medical interventions, such as drugs that prevent the absorption of fats from the diet or surgical interventions such as bariatric surgery to reduce the volume of the stomach. Both had been identified as problematic by the researchers in terms of the kind of intervention into the body. However, a medication based on the genetic understanding of obesity was never explicitly mentioned by the researchers. While discussing solutions for obesity that would not work, they were rather vague about their own contribution to possible solutions, emphasizing their role as "basic researchers"—"We aim to find the adipolytic genes that we do not know" (S6/RT1/2/14)[xvi] — and then mentioning the patenting of outcomes as a goal of the project. While the researchers did not explicitly mention a drug, therapy or the like in their introduction to their research, the lay participants assumed such a goal right after the researchers had made their statement. The assumption that the researchers were "secretly" working on that application was held by the participants throughout the Round Table. The symbol for the "technical" solutions of the obesity problem was the so-called "fat pill", that is, a drug or other simple-to-apply medication that would remedy obesity. The development of the "fat pill", however, was not welcomed by the lay participants, but subject to great critique throughout the Round Table. In the lay participants' critique of the "fat pill" three dimensions are of importance:

First, the "fat pill" was subject to criticism by the lay participants because it accounted for their assumption that the researchers had *not been honest about the "real" goals* "behind" their research. The assumption that they hid their real aims raised concerns that the researchers' work might entail consequences that were ethically questionable, and that the researchers did not openly want to talk about. Hence, the double narrative of the researchers, in which they aimed to subscribe both to basic research and to the social relevance of their research—which is understandable considering the transformations of science-society and the high demands on research(ers)—did obviously result in a great degree of mistrust by the lay participants, which could not be resolved until the end of the Round Tables.

Second, the "fat pill" was a *symbol for a particular technology-driven innovation politics* that puts great emphasis on "technological" solutions the dominance of which leads to the "ignorance" of other means that might address the problem in a "better" way. The assumption was that "technology" increasingly shapes and intervenes in society and in individual behavior, introduced and enforced by actors who benefit from these technologies. The "fat pill" was understood as a "technical fix" of problems that are rooted in cultural changes in lifestyle. As such, the "fat pill" was seen as a promoter of a technological culture in which all kinds of problems are increasingly addressed solely by technical means and through which "societal" values that are not seen as represented in the technology are reshaped.

> And then one could not quickly quit it. And I think with the fat pill it would be the same, because not everybody wants to be slim of course, yes. I mean, [in] some cultures women aren't supposed to be slim at all. And if the fat pill were to be thrown on the market there together with Western beauty ideals, it would probably have fatal consequences. (L1/eP/275)[xvii]

In this quote, the “fat pill”, as a symbol for technology-driven social change, was seen as endangering cultures and their particular values.

Third, “technical” solutions were perceived as *endangering individual freedom* with regard to public health issues. Genomics was seen as being part of a development that is increasingly diminishing individual autonomy, as in the future the population might be “forced” to take the “fat pill” in the name of public health. One lay participant argued:

> Now, one does gene research on fat metabolism disorders, one finds a medication that remedies [obesity], or another procedure—it does not necessarily have to be a medication, but a procedure where one can correct or control it, and the people get slim—and this has the outcome that public health insurance is saving a lot of money. And then they could—and now we get to the ethics—then they could come across the idea that one must use it. (L9/RT5/3/45)[xviii]

In this quote, technology, in the form of the “fat pill”, and the autonomy of the individual to choose a lifestyle are juxtaposed. As the “fat pill” was seen as a goal of the genomics researchers at the Round Table, their research was not regarded as “neutral” in terms of gaining new knowledge, but as contributing to a particular politics that eventually could endanger values of importance to the participating lay people. The “fat pill” was regarded as politics by other means, and as something that might be imposed on the public without its permission and justified by the savings to the public health system.

While the lay participants, in their critique of the fat pill, aimed to discuss different values that may be in conflict with a society that is governed by technological innovation and “technical fix” approaches, the researchers responded by arguing that they personally did not work on the “fat pill”, and that such a drug would be technically barely be possible even in the future.

> Most likely, it will never be that way … that everyone gulps down his fat pill in the morning and we all run around with ideal proportions. THAT will most likely [never be the case], that it works totally without side effects … and I take this prophylactic life-long. Such drugs almost do not exist. If I intervene in a metabolic pathway then I disturb a lot, always. I can try to minimize it and to reduce my side effects, but to have a prophylactic remedy that is really so efficient—that is more than unlikely. (S6/RT1/2/125)[xix]

The researcher responds to the concerns of the lay participants by negating the possibility of a “fat pill” and by alluding to the technical problems in realizing it. Thus, the discussion of the “fat pill” is an example of a case in which both groups used rather different, incommensurable frames in the discussion. The researchers did not engage with the value arguments made by the lay participants, but only with the technical aspects of a possible “fat pill”, and thus were not able to respond in such a way that the lay participants could see that they were engaging with their concerns. Such arguments are very common in many debates on emerging technologies, where science policy or institutionalized ethics is responding to public concerns by referring to technical risk assessment, but ignoring underlying value questions (Levidow and Carr 1997; Wynne 2001).

Such public concerns, however, often go beyond the issue and seek to widen the context in which a particular problem is discussed. An example of this can be found at the Round Table, where the lay participants aimed to link research agendas to societal expectations of science. In doing so, the lay participants situated the "obesity problem" in the wider context of research policy and raised the question of what values the allocation of research money should be governed by. In this, the tension between *distributive justice and the self-governance of science under the norm of "excellence"* became visible.

Based on the researchers' narrations, a question that puzzled the lay participants throughout the Round Table was the researchers' relation to basic knowledge production on one hand, and acting in pursuance with economic goals on the other: "Does one rather consider insights and knowledge, or is it economic interests that are in the foreground?" (L2/RT5/3/131)[xx] was the question one lay participant posed to the researchers. The lay participants' presumption was that it was not the quest for new knowledge that was driving the researchers' interests, but the potential marketability of products. To illustrate their argument they coined two examples: First, research on malaria, which they saw as poorly represented in the research landscape. The malaria example was brought up because "so many people die because of malaria" because they "cannot afford to buy medication", and the "society that provides the money does not suffer from malaria" (L12/RT5/sgP/519).[xxi] Second, the lay introduced the example of rare diseases—"where there are, I don't know, only 500 diseased in Europe. In this case, the pharmaceutical industry doesn't spend anything for research because one cannot make a profit there" (L9/RT5/3/147).[xxii] With these two examples of orphan diseases and those predominantly afflicting poor and developing countries, the lay participants raised the issue of *distributive justice in the allocation of research money*. While they accused science of following the norm of maximizing profits on large market, they also coined the value of solidarity with those who are excluded by market-guided norms in research policy. In the eyes of the lay participants, it was dominantly economic values which governed the allocation of research money. As an alternative, they suggested that the value of solidarity in terms of distributive justice should be considered to a greater degree. Distributive justice was addressed both as an issue *in* Western societies, to balance funding of mass and orphan disease research; and *between* Western societies and poor or developing countries, to balance the needs of domestic and foreign populations. Thus, they argued that it is not science alone and its alleged economic impetus that should define research agendas.

The researchers, on the other hand, did not straightforwardly argue that economic norms should guide the allocation of research money. On the contrary, the were quite critical, for example regarding "scientific fashions" or dedicated research programs, because these, especially the latter, were seen as endangering bottom-up basic research, for which they made a clear plea in the way research money should be allocated. They argued that potential applications should not be a criterion for or against the funding of basic research: “To make differences here … in basic research, this is not the case and should not be the case either, for god’s sake” (S8/RT6/2/151).[xxiii] Thus, the researchers argued that what should be funded or not must not be subject to social and moral values. In their view, *“scientific quality” and “excellence” should govern the allocation of research money*. As such, it is only science itself that defines the criteria for what research should be funded, by means of peer review. In this regime, social values such as solidarity and “just” distribution of the research money played no role for the researchers. Basically, the researchers’ rejection of moral issues as playing a role in their work was based on their particular self-understanding as “basic researchers” and the resulting “non-responsibility” for social concerns. I will address this matter in more detail in chapter 8.2. While the lay participants rejected the idea of a profit-oriented research policy, they were equally critically of science’s orientation along pure internal norms. They assumed a kind of “social contract” between science and society, where society provides the money and science should deliver solutions in return: “I somehow have the feeling as a taxpayer, I finance a huge apparatus [science] and I expect something from it. There must be outcomes that make my life better, because it’s my money, yes” (L13/RT2/3/116).[xxiv]

Since “technical” solutions were generally rejected by the lay participants, *what kind of “solutions” for the obesity problem did they suggest instead?* In the discussions, the lay participants suggested a rather wide range of solutions through which they aimed to provide an alternative to the alleged “technical fix” approach of the researchers. In a general sense, they self-labeled their solutions as “more social” and “more humane” compared to those of the researchers. However, the question is, what do “social” and “humane” in this context mean concretely?

Overall, two different narratives that describe a particular tension were argued during the Round Table discussions. One narrative seeks to “responsibilize”[33] citizens with regard to the collective. This narrative emphasizes the “duty” of citizens to take on responsibility within a community where shared achievements need to be protected. The other narrative highlights the “right” of individual autonomy and freedom to choose a particular lifestyle. In each narrative, a different set of values is promoted, and different ideas about the relation between the individual and the collective are put forward.

The first narrative on the question of what kind of solutions are to be favored with regard to the obesity problem emphasized and defended the *individual’s right to choose a lifestyle*—that is, with regard to the problems discussed at the Round Table, to be obese:

---

[33] I briefly discussed the idiom of “responsibilization” in chapter 2.2 as a crucial feature of advanced liberal democracies with regard to the governance of science. Here, responsibilization takes on another facet when citizens discuss how to “responsibilize” citizens.

> Public health wants to have a pill so that metabolism disorder goes down, so that the costs go down. That is good so far. Then they go down, only ... we would compelled to take this pill everyday. That is coerced obligation ... you have to swallow two *Sortis*[34] every day because and so forth. And this is in my opinion an ethical problem. Do I have to force him not to become fat, or is it his free decision to be fat? If he perhaps has to pay certain deductibles to the health system or to the social insurance is perhaps worth a discussion, but where is freedom? (L9/RT5/sgP/181)[xxv]

In this quote, the lay participant is expecting that the individual's freedom to choose a lifestyle will be endangered in the future by the increased cost pressure in public health systems together with "technical" solutions provided by biomedical research. The value on which he bases his argument is individual freedom. His suggestion for a solution to the obesity "problem" is to leave it to individual preference if someone wants to be fat or not. The obesity problem, he argues, should not be subject to decision by the whole society. In balancing the financial costs for the welfare state with the basic right of free decision-making, the latter clearly prevails in his view, because "if he eats much, he pays a lot of value-added taxes too" (L9/eP/366),[xxvi] and thus public costs and expenses are balanced. In that case, the individual's rights are of higher ethical import that the interests of the collective.

At a first glance, the researchers seemed to agree with this narrative, as they too mentioned the opportunity of people to choose freely whether they take this or that drug against obesity or whether they eat fatty food: "Yes, you can decide that you don't eat fast food or no fatty food in the same way as you can decide later, whatever, not to eat the fat pill" (S2/RT5/sgS/185).[xxvii] However, there are substantial differences in the conceptualization of individual freedom and the discursive contexts in which these narratives are brought up. First, the emphasis on individual freedom to choose must be contextualized by the researchers' argument that obesity is not "caused" by individual behavior (this is only the "trigger") but by physiological properties defined by genes. That is, they argued that the individual cannot really choose if he or she wants to be slim or fat: "that is, changes in behavior are a very problematic matter, and therefore it will be absolutely necessary to intervene here in a different manner" (S8/RT1/2/69),[xxviii] that is by biomedical means such as surgery. Thus, there is a certain ambivalence in the arguments of the researchers, who depict the individual as free and not free simultaneously depending on the context. Second, the researchers did not refer to individual freedom in an emancipatory-democratic sense, as the lay participants did, but rather in the sense of a market-democracy, where individual preferences in markets decide what has to be considered as right. Third, for the researchers the *principle possibility* of individual freedom was the crucial dimension in their argument: In the above quote they argue that "in principle" no one can "really" be forced to adopt a par-

---

[34] A drug lowering cholesterol. *Sortis* is the market name in Germany, Austria and Switzerland. In other countries it is known as *Lipitor*.

ticular behavior. The “fat pill”, for example, cannot exert “existential” force on persons, and thus the principle of individual autonomy remains untouched. The lay participant, on the contrary, focuses on the societal conditions that increase or decrease the chances that citizens can act freely and choose their lifestyles. In his argument, the “fat pill” entails “social coercion”; that is, the “social costs” of not following a normative behavior would be very high. In this view, individual freedom is conceived in practical contexts—and not as an ethical principle—and thus appears as a “right” to be defended.

However, arguments in favor of the “right” of individual freedom were only marginally represented among the lay participants. The majority of those who explicitly positioned themselves emphasized rather the *“duty” of the individual in a larger collective* for which the citizen should take on responsibility. As such, the lay participants proposed two different yet interwoven ways to solve the obesity problem. First, they argued that *citizens should be “responsibilized”* in terms of their dealing with health:

> Yes, this is the beginning actually, not to look for responsibility at the doctor, and not at the researcher, but to take responsibility for oneself. Why did I go there to call on a doctor? That is the problem. Why did I gain weight? Because there is such a huge offer of this and that? No, because, perhaps, something does not work with my own discipline. The other is not to blame. (L14/RT5/3/302)[xxix]

This lay argues that the responsibility for one’s own health must not be displaced “elsewhere”, but the individual must admit and confess that he or she is the only one who is responsible. The lay aims to bring back “responsibility” to the citizens themselves, and not have them rely on the collective to take care of them. This narrative strongly emphasizes the “duty” of citizens to take care of themselves, but does not mention the “rights” of the individual as the lay participants above did. This self-responsibility, however, was not only concerned with individuals living side by side but remaining unconnected:

> The responsibility for oneself. … To learn that to bear self-responsibility, first of all in its smallest form for myself—then for the family and society comes later, it’s significantly further, isn’t it? Thus, for myself, for the siblings, family, friends, and then somewhere comes society. But the whole, the source is self-responsibility. (L8/RT5/sgP/514)[xxx]

In this quote, the lay participant argues that self-responsibility is the birthplace and necessary condition for taking on responsibility for the whole society. (Self-)responsibility calls upon the duties of the citizens to a greater collective. The welfare of the whole is put in an inextricable relation with the individual’s well-being. Here, a particular concept of “solidarity” is used: While the lay participant who emphasized individual freedom called upon the welfare system to be in solidarity with the individual citizen regarding his or her right to choose his or her lifestyle, solidarity is here depicted from its flip-side. It is the individual who has to be in solidarity with the whole, and the citizen who has obligations towards the community, rather than the other way around: Taking care of oneself is taking care of the community.

The lay participants' ideas for how to "govern" obesity can be related to the wider political context: In fact, the lay participants' ideas for solving the obesity problem are close to Nikolas Rose's idea of "responsibilization", which "works through the values, beliefs, and sentiments thought to underpin the techniques of responsible self-government and the management of one's obligations to others" (Rose 2000). As such, he argues elsewhere, "Politics is to be returned to citizens themselves, in the form of individual morality and community responsibility" (Rose 1999a, 11). This fits nicely with some of the lay participants' attitudes toward solutions for the obesity problem.

Second, the argument of "responsibilization" for solving the problem of obesity was supplemented by solutions in a rather Foucauldian manner, that is, by *disciplining citizens*. Two particular mechanisms of disciplining were suggested by the lay participants. On one hand, some lay participants put great emphasis on education, in order that people learn to deal with nutrition in a more reasoned way. For example, it was proposed that basic schools should educate pupils regarding nutrition. On the other hand, the lay people suggested that people could be governed regarding their nutrition behavior by disciplining them through money:

> We thought it that way. For example, McDonalds, on every burger—or other firms that just produce unhealthy products so to speak—so the costumer just pays more, whatever, 50 cents more for one burger, and all the rest of it. And the 50 cents of this fat tax go of course to useful things, for example to the promotion of [health], prevention in society, funding of research or change of behavior. (L2/RT5/3/175)[xxxi]

The lay participants developed the idea of a "fat tax", through which individual behavior could be governed, and the financial gain of which would be used to sponsor further activities of health precaution. Beyond this argument, there was the widespread opinion among the lay participants that people are best governed by systems of rewards and penalties.

Interestingly, those who argued for the individual's right to choose a lifestyle did not perceive the responsibilization and disciplining of citizens as a kind of coercion as they did with "technical" solutions. It is difficult to answer why this was the case. One reason may lie in the fact that "technology" was perceived as something alien to "society", and thus often conflicting with cultural values, while social technologies of responsibilization were perceived as "internal". Responsibilization brings back politics—as Rose argues—to the citizens themselves, and leaves it in their hands to enforce certain values by their own means, so that people feel more involved in political processes. Another reason could be in the social dynamics of the lay group, which was characterized by boundary work with respect to the researchers in order to constitute themselves as a more coherent community with shared moral and epistemic assumptions. Internal arguments would have disrupted these efforts; thus, they put emphasis on the boundary work with respect to the researchers and their concepts of "technical" solutions rather than criticizing each other.

Lay 'ethical' knowledge and the challenge to push ethics upstream

In summing up, I again take up the question of why I introduced these issues as an "ethical" discussion. In discourses of institutionalized ethics, defining a "problem" and its associated "solutions" is usually not regarded as an issue that deserves ethical consideration. Problem definitions created by science are generally taken for granted in the context of institutionalized ethics. However, as the discussions at the Round Table showed, what is a "problem" cannot be assumed to be a matter of consensus; different accounts of "problems" engage different values. However, if a problem is defined in a certain way, decisions are made about institutional and financial commitments to future technoscientific trajectories, making it rather difficult to open up these questions again if contested later. I have demonstrated that, in the discussions of problems and solutions, a wide range of concerns regarding "science" and innovation were communicated, especially by the lay participants.

The debate around problems and solutions particularly poses a question about the "when" of an ethical debate. The lay participants' discussion of these questions indicates the wish that a debate over what values are involved when "society" decides that something is a problem and that this problem should addressed by certain means should take place before too many future commitments are already made, impeding a more open debate. In short, it suggests that ethics should take place more "upstream", which is also suggested by the literature on this issue, in order to be able to pose questions like "Do we need this technology at all?" (Wilsdon and Willis 2004). This literature argues that such a discussion cannot be resolved in terms of classical PUS—that is, "the public does not know the facts of 'obesity' but once informed persisting and critical questions will quickly disappear by the common epistemic ground of 'science'". The discussions at the Round Table revealed that the gap is not between knowledge and ignorance, but between different cultural values. Upstream engagement is necessarily connected to a discussion on values, because it seeks to forego discussion about risks in favor of a debate of underlying values and interests.

However, the call for "upstream engagement" also deserves closer consideration. Three questions can be posed: First, "ethical" debates do not emerge in a "purified" manner, but involve a range of aspects, some of which are problematized "upstream"—that is, as open to change—while others are employed as "downstream"—that is, as given facts. As a matter of fact, discussion of which are "upstream" in its full content is hard to conduct, as the participants in the discussions—regardless of whether "experts" or "lay"—need resources that they can take for granted and that serve as an anchor in the discussions. The question is *how to balance between upstream and downstream discussion*? How far can a debate move upstream and not end up as idle talk? Second, the question is *"upstream" for whom*? For the researchers, the issues involved in the obesity problem have already been clarified. For them, many elements were "facts" not in need of any further debate. For the lay participants, however, problems and solutions were far from being considered consensus facts. In the mutual engagement at the Round Table, different "frames" collided, and it often seemed that a kind of "meta-language" was necessary so that the participants would be able to talk about the same issue in the same framework. However, such a meta-language will be hard to find, and probably will result in further conflicts. Third, the question remains, *what is "upstream" enough,* and who defines this? This question was also raised by the way the participants discussed the question of problems and solutions. In debating what was to be considered a problem and what might be possible solutions for it, the participants drew upon different ideas of the social and the political, in particular with regard to the relation of the individual to the collective. These narratives were not as explicitly debated as the question of problems and solutions. While one could argue that the Round Table discussions were rather upstream with regard to the question of what a problem is and what might be solutions for it—in particular when considering the way institutionalized ethics deals with the issue—the discussion stopped at the point of the problem definition, while simultaneously calling into play visions of the social and the political that were not explicitly made a subject of the debate. These ideas were not only mere "context", but were strongly linked with what was suggested as a particular problem definition as well as a solution.

It is in particular the social ideas of lay 'ethical' knowledge that deserve closer attention, as different visions of the social have different "ethical" implications in a narrow sense. The dominant narrative in the lay group, especially, was able to pose challenging questions as they redrew the relations between the individual and the collective in terms of rights and duties. These can be labeled *"neo-social" political epistemologies,* in which the welfare state is coupled with citizens' responsibilities in such a way that only those who are ready to take on responsibility for themselves are to be supported by the welfare state. The "active" citizen is reconstructed in a moral way. The discussion of problems and solutions, thus, reflects the wider political context and the transformation of the state over recent years. As such, this discussion is not limited to the Austrian context, but mirrors developments in all Western liberal democracies in which the relation between the individual, the collective and the state is conceived in new ways and political questions are increasingly declared to be "debates on common values".

8.2. Responsibility of research as an ethical question

The issue of "responsibility" was one of the main ethical questions and challenges for both the researchers and the lay participants at the Round Table, as it was debated throughout the whole Round Table—initially in a rather implicit way, but towards the end of the discussion it was brought forward in particular by the lay people in a rather explicit manner. Generally, two different ways for addressing the responsibility of research came to the fore in the discussions. Both have in common that they "struggled" with responsibility, but they did so in rather different ways. The researchers subscribed predominantly to a model of a *limited responsibility of research,* which narrowed down their responsibility to the means of knowledge production only, while the responsibility for the consequences and impacts of the knowledge produced was largely rejected. Hence, their idea of responsibility was largely one of non-responsibility. By and large, the researchers went into the discussions with this narrative and also left the Round Table with it. For the lay participants, the issue was far more complex, as they did not go in with a ready-made concept of the responsibility of research and researchers, but had to develop their visions in the engagement with the researchers' narratives, which they predominantly rejected. However, it was extremely difficult for them to find a positive counter-narration, and they did not leave the Round Table with a clear idea of how the responsibility of research could be governed in a better way.

Before I analyze the different narratives on responsibility, the term itself is in need of a few words of clarification. It is overloaded with a multitude of meanings, as it is used in many different contexts (philosophy, ethics, law, everyday conversations, etc.). The common ground around which all participants positioned their narratives at the Round Tables was that responsibility implies an attribution of consequences to one's actions, and thus suggests a more or less direct causal relation between an action and its subsequent impacts and consequences. At the Round Table, however, the issue of responsibility was not discussed as a theoretical concept of ethics as such, but as a means to articulate science-society relations and to express mutual expectations between "science"—as represented by the researchers—and "society"—represented by the lay participants.

I will start with the predominant narrative of responsibility held by the researchers, as they went into the Round Table with it and thus started to promulgate it from the very beginning. Then I will turn to the narrative of the lay participants; afterwards, I will briefly discuss narratives of responsibility present rather marginally, and make some conclusions

Limiting the responsibility of research(ers)

Is science responsible for its societal consequences? The researchers' answer to this question was clearly "no"—if "science" is understood in a particular way. How must "science" be understood in order not to be responsible for the outcomes? The researchers' image of science rested on a clear-cut separation between basic research and knowledge production in the context of application, together with a linear model of innovation. "Basic research" was understood as the practice of basic knowledge production in the laboratory, which remains—and ought to remain—untouched by the expectations of society, as this researcher explained:

> As a researcher, I strongly distinguish between the things I do in the laboratory, where I'm forced by law to let nothing escape—to take care that my mutants really stay in the lab. And as they are not able to do harm, I'm in a very different position than a plant physiologist who plants [genetically modified] corn; or any company, whatever, that releases genetically modified potatoes, tomatoes or whatever to the open field. ... But as a natural, as a laboratory scientist ... I'm in an entirely different position. (S7/RT5/2/263).[xxxii]

I this quote, the researcher argues that a clear distinction can be made between research that aims for practical ends and societal application, and research that remains in the lab, aiming only for new knowledge. As basic and applied research are different matters, the ethical framework for each must also be different. The researchers' ideal vision of science contained the idea of an "epistemic core" of knowledge production, which is committed not to an "outside" but to an internal ethos of science that warrants that the knowledge produced is primarily genuine and of relevance to the community of scientific peers in a Mertonian (1973) fashion according to the scientific ethos. Other forms of knowledge production, in particular those which are concerned with applications and technology development, must, following the researchers, be carefully distinguished from "basic research". The researchers at the Round Table situated themselves in the area of "basic research", with gaining knowledge as their prime motivation, as this researcher argues: "I want to know how the fat metabolism functions, and I am frantically interested in how this fat metabolism is connected with diabetes" (S6/RT1/2/69).[xxxiii]

The researchers' interest in gaining new knowledge as their basic motivation came up in many contexts at the Round Table, in particular when the discussion concerned issues of the societal consequences of research as well as the expectations of society towards research. What, then, was the researchers' vision of the relation between what they called "basic research" and knowledge production that strives for technologies and application and finally towards marketable goods and services? To describe this relation, they introduced a linear model of innovation:

> Within our research aims, we have classified it like this: we have direct aims, which are ours in the laboratory. Our direct aim is to identify genes and to clarify metabolic pathways ... Then, there are indirect and long-term aims. Indirect aims, one could say, somebody takes this up to research whether one can develop certain substances with which this gene or its product can be manipulated so that somebody becomes fat or slim, or to remedy a certain disease. ... And the long-term aims would be to reduce obesity, to reduce

> arteriosclerosis, heart attacks, cancers and so forth. But these indirect and long-term aims are not the aims of GOLD. These are only societal aims, which are realized by others. We do not do them ourselves. We cannot even do them. (S6/RT5/2/314)[xxxiv]

This innovation model is particularly interesting because it suggests a linear flow from basic research to technological applications in society. The basics of fat metabolism are clarified, then substances are developed *by others* to target particular diseases, and finally drugs are introduced into society in order to remedy diseases. However, they also introduce different stages in the knowledge production process that are separated by clear boundaries. Each of these stages describes idiosyncratic social and moral worlds in which different norms apply. In "basic research", working towards practical ends and solutions plays no role. Here, the consideration of societal values would be quite harmful. In "applied research", as well as technology development and implementation, the orientation towards societal needs and expectations is central. The need for "basic research" is explained by arguing that it provides a reservoir of knowledge that must not be compromised by a short-term orientation towards applications, so that one cannot bypass these different stages of innovation and move directly to technology development. They explained this, for instance, with the example of the laser:

> the laser was not invented so that I could have a laser beamer … and to scan my eyes. ... These are applications which were totally unforeseeable. That [the laser] was not invented because of an application ... Per se, I can assess then what all I could have done with it: That, I believe, is asking too much. (S6/RT5/2/25)[xxxv]

The researcher here introduces the notion of "serendipity" (Merton and Barber 2004) as a crucial property of the innovation process, which assigns high importance to basic knowledge production. She argues that it is impossible to foresee what kind of technology comes out in the end when starting with basic research. As mentioned above, responsibility implies some kind of causal relation between what one does in the laboratory and the consequences that appear later in society. The researchers' narrative of what science is aims to remove these causal relations between basic knowledge production and later technological applications, describing strict and quasi-natural boundaries between basic research and applied forms of knowledge production by introducing different stages of innovation and by the narrative of serendipity. The researchers' argument is that if there are no direct causal relations between gaining basic knowledge and later applications, "we" as researchers cannot be held responsible for what "others" "elsewhere" do with this knowledge.

That basic researchers cannot be held "responsible" for applications was also implied by another characteristics of "basic knowledge", as this quote demonstrates: "there always will be possibilities to apply things negatively. The question is, then, does it have to be research that is restricted, or shouldn't it be negative application which I try to restrict" (S6/RT5/2/65-67).[xxxvi] For the researchers, knowledge as

Erkenntnis has no moral properties, but has to be regarded as neutral and given by nature. Hence, they argue that it is its societal use that decides if something is "good" or "bad", and not the fact that this knowledge exists through basic research.

Yet, this quote offers another feature of the responsibility narrative of the researchers. Responsibility is associated particularly with *negative outcomes.* For the researchers, making a link between "basic research" and "negative consequences in society" would have negative impacts on research itself, namely the restriction of the autonomy of research to define its research agenda. Their assumption is, if "basic research" could be held responsible for later applications because there is a direct causal relation, society would quickly start to interfere in and restrict basic research. Hence, "responsibility" was perceived as the attribution of "blame" or "guilt" in case of negative impacts on society, which would have the consequence—in the eyes of the researchers—that science would no longer be entitled to autonomously define its research fields by its internal criteria. As basic research and application are entirely different, regulation must be concerned with applications and not interfere in research agendas.

The researchers' perception of responsibility as potentially being blamed for negative impacts on society had another interesting facet. While basic research was predominantly conceptualized as a collective enterprise of a community sharing particular norms that attribute meaning to the knowledge produced through shared models of success and failure, they often individualized responsibility in order to oppose the arguments of the lay people. They attached "responsibility" to the capacity of individual researchers to assess the full range of the developments certain scientific knowledge might trigger in the future by saying, for example, "Is Darwin to blame for euthanasia?" (S6/RT5/2/200).[xxxvii] The researcher rhetorically overstates the responsibility of the individual researcher in order to "prove" that the collective of science cannot held be responsible for the misuse of knowledge ascribed to society. For the researchers, applications that later turn out to be good or bad are not foreseeable, because of the nature of scientific knowledge, the innovation process and the eventuality of individual misconduct. As such, they did not exclude the last as a possibility for how individuals, even when they are researchers, might make use of scientific knowledge:

> Somebody who wants to have war gas will not wait until others develop it and then take it as a byproduct. He will do it himself in the worst case. Somebody who assumes negative motives, I cannot prevent him from this because it is forbidden. (S6/RT5/2/41)[xxxviii]

Constructing the figure of the "black sheep" scientist, the researchers personalized scientific misconduct and misuse of knowledge. They argued that potential misuse is a pitfall of all knowledge. This, however, must not be attributed to the knowledge production process itself, as the consequence would be to stop basic research altogether and technological progress would come to an end. They argued that if society wants to reap the rewards of technoscientific progress, it has to accept the fact that misuse is possible. However, only individual misconduct is responsible for this misuse.

In order to underline that black sheep are deviations from the mainstream norms of basic research, these researchers were always located “elsewhere”. “Elsewhere” was meant both in a geographical as well as in a cognitive sense. They are distant in place, for example in “China”, as well as distant to the internal norms of science. Individual misconduct does not harm the integrity of basic research, insofar as it is not the product of a missing general responsibility of science but one of individual criminal motivations. The “black sheep” remained a singular exception in the eyes of the researchers.

One can see that the researcher cast a rather black and white picture of responsibility, with no shades of grey. Responsibility was regarded as a matter a researcher could only fully assume when breaching the internal norms of science based on criminal intentions, or which could not be assumed at all due to the nature of knowledge and the serendipity of the innovation process.

So far, I have analyzed why the researchers did not see themselves as able to take responsibility for the consequences scientific knowledge might have in society. However, for what did the researchers regard themselves responsible? Their assumption was that they could only be responsible for the *means of knowledge production*. However, responsibility in this regard was meant less in moral than in legal terms. Based on the assumption that the regulatory system in place covers the full range of the activities of the researchers’ research practice, responsibility is satisfied if the researchers stick to the law and other formal rules. Following the rules was often perceived as the fulfillment of responsibility, as this researcher indicates in the interview made after the Round Tables in which she was directly asked to what extent she would apply responsibility in a project proposal: “It is actually required everywhere, when one applies for a project, that one fills out a form which precisely contains what ethically relevant things one intends and so forth. After all, it is actually very strictly controlled” (S2/eP/219).[xxxix] In this quote, and in particular with regard to the issue of animal experimentation, the researchers frequently referred to the law when the lay participants brought up the issue of responsibility. This was a rhetorical strategy of the researchers, by which moral questions were displaced into the legal domain, thus again shifting responsibility outside science.[35] Researchers are only responsible for following the rules, not for making them. The delegation of responsibility from one area to another—in particular from science to politics as lawgiver—was subject to serious critique by the lay participants, as I will analyze in the next section.

So far, I have provided a rather clear-cut image of the researchers’ attitude towards responsibility by showing that, because of their role as “basic researchers” and the clear demarcation between “inside” and “outside”, they could not be held responsible for the consequences of the knowledge they produced. However, this

---

[35] I will discuss this in more detail in chapter 9.

picture assumed more shades of grey in the discussions, as there were several instances at the Round Table where *"society" interfered with the "inside",* and thus created a slightly modified concept of responsibility.

In the course of the discussions, the lay participants increasingly challenged the researchers regarding their idea of responsibility. Being heavily criticized with regard to the societal "use" of basic research, the researchers reacted by arguing that not only does research produce knowledge that is science-internally relevant, but scientific knowledge and its production must be understood as a "cultural good" ("*Kulturgut"*) similar to art and other cultural traditions. In particular, one senior researcher felt "assigned by the public" to produce knowledge as a cultural good. Here, social meaning is imported to the knowledge production process, which originally was described as having no practical ends. The arguments of the researchers were that in society the cultural value of art is usually uncontested, that it is appreciated on its own terms and that society is investing a lot of money in it without demanding a clear orientation towards practical application and utility. With the idea of the cultural good of science, the researchers sought to maintain the autonomy of research and thus to deflect the lay people's demand for a more responsible science. However, the researchers also introduced different social criteria for appraising science; that is, as one of the lay asked in the interview after the Round Tables, is society willing to pay for genome research as for the state opera?

The narrative of being solely basic researchers and thus being unable to assume responsibility for the social consequences of research was also disrupted by the researchers themselves. When ethical issues were discussed at the Round Table, the researchers relied on the model of basic research to describe their work, and they were thus largely able to deflect the ethical concerns of the lay participants. However, in other contexts of the discussion, they emphasized the close relations of their research with society. In particular, when the issue was the legitimacy of public funding for research, they were able to leave the terrain of pure basic research and highlight the potential usefulness of their research by linking it to "societal" problems such as obesity. Here, they argued that they have been "approached" by society to help to solve its problems. One researcher states "that is virtually morally demanded … to research fat metabolism. It is simply a problem. A majority of people die [from diseases related to disorders of fat metabolism]. The most frequent cause of death worldwide … is simply the impact of obesity" (S6/RT5/2/324).[xl] Here the researcher argues that if research aims to take on responsibility, it must do research.

Desiring a different kind of responsibility of science

These ambivalences and tensions in the narratives of the researchers made the lay participants increasingly alert regarding their notion of limited responsibility. How did the lay participants respond to the way the researchers dealt with responsibility? What kind of ideas of the responsibility of science did they develop? From the very beginning, the majority of the lay participants did not agree with the researchers' narrative of the limited responsibility of research. Basically, the lay did not perceive their narratives as coherent and authentic; in particular, the attribution of "basic research" to the kind of research the participating researchers conducted was not plausible to the lay participants:

> And now for me somehow the dialectics is a bit difficult: on one hand wanting to gain knowledge in the sense of basic research, and on the other hand a certain sense of enthusiasm: there [is] the application in the form of drugs, … to talk about diverse firms, to have the idea that there will be a pill. Thus, … somehow I have difficulty accepting that it is really only about knowledge, only about academic research. (L6/RT1/2/164).[xli]

In this quote, the lay participant addresses the ambivalences and tensions in the narratives of the researchers, where they argue both in terms of potential future remedies for obesity and in terms of "basic research" that is not oriented towards applications. This does not mean that the lay participants believed that "basic research" does not exist at all, but they expressed concerns that the "basic research" narrative was used to cover up the researchers' "real" motivation, namely to develop drugs to remedy obesity, as a kind of technical fix to a problem that the lay participants did not necessarily consider as such and where alternative means would be ruled out without public debate in the name of "basic research" (see chapter 8.1). Hence, for the lay, the researchers were not "authentic" about their motives. The question of the authenticity of researchers in direct interaction with the public—their "body language"—was central to the lay participants in establishing trust relations, as I will show in more detail in a following chapter (8.4) on the issue of animal experimentation.

The lay also questioned the researchers' model of innovation, which on one hand suggested a seamless flow from knowledge produced in basic research towards applied technologies, but also posits clearly separable stages of innovation that "shield" basic research from the need to take on responsibility for its outcomes. The lay challenged the researchers' innovation model, as they did not believe that "basic research" was done without thinking and being able to think about later applications. For example, they did not buy into this because they questioned whether huge amounts of money would be spent if there were not an expectation of application from the research. For many lay participants, the orientation towards application is present from the very beginning—in particular because the researchers themselves communicated it right from the beginning of the Round Table. This and a series of other tensions in the discourse of the researchers provided a quite incoherent picture of what "research" is about and of what researchers do to the lay participants. This incoherency was attributed to the researchers' not being authentic about their motives.

The lay also expressed quite some unease about the researchers' shifting of responsibility "downstream" in the innovation process, towards the moment when ready-made technology becomes available to be implemented in society, thus, away from science towards "society". Research imposes responsibility on society regarding the use of knowledge, while research only takes care of its production. Being quite unhappy with this solution of the responsibility problem, the lay participants aimed to challenge the boundary work of the researchers. However, they struggled throughout the discussions to find responses to the researchers' idea of responsibility. In defense of the model of responsibility, the researchers could primarily rely on two aspects: First, they brought their model along when entering the Round Table, and they could rely on the fact that the other researchers would share this notion and thus speak with one voice. The lay participants, however, first had to form an "epistemic community" (Haas 1992) that shared the same concepts and values, and thus had to negotiate among themselves what kind of position they would develop towards the researchers. Second, the linear model of innovation, as well as the separation between basic and applied research, are dominant societal models that have structured science policy as well as research itself (Godin 2006) for a rather long time. It is the dominant idea of how innovation works, and that which alternative models need to challenge in order to be accepted. The researchers' model was the norm, and thus need not be legitimized. Hence, it was easy for the researchers to refer to funding institutions such as the Austrian Science Fund FWF, which funds predominantly bottom-up basic research and thus supported the researchers' model. They could also provide anecdotal evidence from the history of technology, such as the above-mentioned laser, to "prove" that their concept of basic research, and thus their model of responsibility, was "true".

The dominant model of innovation, with its particular implications for the responsibility of research, made it quite difficult for the lay people to develop their own ideas of responsibility. In fact, their engagement with this issue was foremost characterized by a desire or an expectation that a "different" way of dealing with responsibility in research was required, but an inability to find a coherent counter-narrative to the researchers' approach to the issue.

The discussion in a small group the lay participants had among themselves is a pivotal example of their struggle to find a "viable" response to the narratives of the researchers. In particular, they did not aim to find a merely normative position of what ought to be the case, regardless of its practical problems, but also reflected upon the possibilities of how alternative visions of responsibility would work out in a social reality they considered given. In this small group discussion, they first discussed the shortcomings of the researchers' approach to responsibility. The main focus of the critique was that the researchers always "delegated" responsibility to someone else, which was identified as a "very simple means to make oneself invulnerable"; however

> if I teach at the university or something, then I am somehow in an outstanding position, and then I have to somehow take on responsibility or make a commitment or, yes, simply stand above things. In that I cannot reflect upon

> things somewhere, maybe quietly at home, but I have to take up a stance, even towards basic questions. (L5/RT5/sgR/164)[xlii]

The lay participant identifies researchers as public actors who are, therefore, obliged to assume responsibility in a quite wide sense. That science should be a public good was a narrative to which both lay and researchers agreed in the discussions. While for the researchers the public good character of research was satisfied by the production of knowledge, the lay people explicitly demanded that the researchers themselves must be present as public actors and articulate moral positions. Because of the researchers' deflection of ethical concerns by referring to "basic research", the lay people regarded them not as willing and able to take on such responsibility.

The *delegation of responsibility* was identified not only as a way the researchers dealt with responsibility, but also as a more common, society-wide characteristic. In further discussion in the small group, the lay aimed to identify promising candidates to take care of responsible development in science. In a first round, they discussed whether responsibility for research could be assigned to specific societal domains, such as science itself, ethics, politics, the economy or religion, but they failed to do so, as "in reality" those actors always delegate responsibility somewhere else. The lay's primary example of this particular approach to responsibility was "politics": "What does politics do? It puts together a commission and is advised [by it], isn't it? What do politicians really decide themselves?" (L5/RT5/sgR/258).[xliii] Politics is described here as unwilling to make binding collective decisions, but also as malleable to economic interests, because "Politics is finally an instrument too, or is also influenced by the economy, I believe" (L5/RT5/sgR/758).[xliv] The lay participants assumed that responsibility circulates in society but cannot be pinned down to a particular actor: "we are again approaching this circle, which consists of politics, the media, the society as such, the researchers, the economy, which, yes, which always shift responsibility towards each other" (L13/RT5/sgR/284).[xlv] The lay participants criticized the "specialization" of society, its splitting into sub-domains, as the main cause for this "delegation of responsibility", because every societal domain now aims "to optimize its own parameters" (L13/RT5/2/11),[xlvi] for example, in the interaction between science and politics:

> Because the politician, ... because what does he do? He is just like us, he is no expert, and in order to be able to make a decision he invites scientists who advise him. And here we are again in this circle, aren't we? The politician is creating the legal framework for the scientist who is in turn advising the politician in order that he is able to create the framework. And there it goes in circles and finally no one is responsible anymore. (L13/RT2/3/183)[xlvii]

Hence, his argument is, politics is turning to science while the "responsibility" for decision-making formally remains with politics. Politics is not assuming its role, while experts are seen as transgressing their competences by too strong an involvement in political decision-making processes. Politics is regarded as not assuming its control function, because it transfers governance to those who should be controlled. As such, the politics perceived as state-centered politics was perceived as too weak to exert control over technoscientific developments.

The second point of critique by the lay participants was the question of what it means to take on responsibility: “the question of responsibility is only posed in the moment of catastrophe” (L13/RT5/sgV/310),[xlviii] thus permitting only the notions of “blame” and “guilt”, which are attributed *ex post*. However, they asked if there were shared norms to determine what is a negative event for which someone can be blamed. This critique, too, was a response to the researchers’ conceptualization of responsibility, because the researchers imagined responsibility solely as “guilt” for negative consequences. However, the lay suggested that technoscientific developments such as stem cell research are much more ambivalent, so that black and white attributions of responsibility are not possible. While stem cells can be ethically challenged in different ways, the lay asked who was responsible for missing economic opportunities. Hence, they argued that there was a great need to discuss the question of what “our” shared norms are that allow us to assess whether something is right or wrong, which could then serve as a starting point for the attribution of responsibility.

“Sharing” responsibility: Who and how?

What was the lay participants’ alternative vision of responsibility, after they had discussed the shortcomings of the researchers’ narratives and the problems they would face in society when trying to find a different way to deal with responsibility? Based on this critique, the lay participants aimed to find a *viable* alternative; however, their idea was less clear than the researchers’ model, in terms of who is and who is not responsible for what, because they abstained from using such strict boundaries as the researchers did.

Reflecting on their difficulties in finding a response, the speaker of the small group stated, in his presentation of the outcomes in front of the plenary with all participants present, that “we have discussed this question of responsibility and we too did not reach an unambiguous outcome” (L13/RT5/2/7).[xlix] In the following presentation, they aimed to sketch out their method of discussion, seemingly struggling to come to terms with what an alternative model of responsibility might look like. Based on their critique of the delegation of responsibility caused by “specialization” of scientific knowledge production and ethical reflection, they first concluded that “in principle we all are responsible” (L13/RT5/2/11).[l] However, they increasingly distanced themselves from this argument, because “we all” also included the researchers. Here, they raised concerns about the researchers’ capability to critically challenge their own practice, because the researchers followed particular interests and were not regarded as being able to transcend their involvement in research and “objectively” reflect upon the consequences of their research. Finally, they presented their alternative view on the responsibility of research, which consisted of two main arguments:

Based on the critique that responsibility only becomes subject to debate if something goes wrong, they argued that

> this question about responsibility, that is, the question of preventing something really bad from happening, perhaps it can best be prevented by that along the path one walks consequently asking if what one does is right or wrong. That means that the society takes care in a sense that the path that the society takes is be permanently critically challenged. (L13/RT5/2/11)[li]

This quote shows that the lay participants wished to discuss responsibility during the whole innovation process, from the definition of public research agendas for basic research to applications and their implementation in society. For the researchers, responsibility was instead a question of a once-and-for-all decision at the moment when technologies are ready and waiting to be introduced to the market. Only at this point does society have the possibility to decide. For the lay participants, decision-making on technosciences could not be reduced to a single moment, as many institutional commitments would have been made already before that moment. In particular, large amounts of money would already have been invested in the research. Instead, course corrections have to be made along the whole process of knowledge production.

In the view of the lay participants, who should take care of responsibility as permanent reflection during the innovation process? On one hand, the lay people demanded "continuous thought-provoking impulses from the outside—namely, that there be certain persons or organizations which simply critically question if what happens would be right (L13/RT5/2/11).[lii] Pushes from the "outside" should be supplemented by "self-responsibility" by facilitating critical reflection by the researchers on their own practices.

What are the particular features of the lay participants' vision of responsibility? First, the definition of "who" should take on responsibility remains extremely vague, as idioms such as "one should", "the society", "certain persons and organizations" or "the outside" demonstrate. Even in the later accounts of the lay people, the question of "who" was not answered in a more detailed way. Hence, they struggled to identify particular actors who would be responsible for technoscientific governance. In a certain sense, the lay participants' answer to the question of who is responsible is similar to that of the researchers. Both had a rather vague concept that "society" should be responsible. However, the researchers shifted responsibility to the technology end of the innovation process, and imposed the full responsibility on "society" so that basic research would not be touched by these concerns. The lay participants, on the other hand, did not make a general distinction between science and society with regard to the attribution of responsibility.

Second, in the lay people's visions, a tension becomes evident. On one hand, the call for "self-responsibility", primarily directed towards the researchers, hints at notions of liberal governance (Rose 1999b), putting great emphasis both on imposing responsibility on citizens and on thoughts about reflexive modernization

(Beck 1992; Beck, Giddens, and Lash 1994), which considers possible side-effects of R&D early in the innovation process using ideas of, for example, “sustainability” and the “precautionary principle”. On the other hand, by referring to persons and organizations from the “outside”, the lay people express a certain desire for a central moral authority that is listened to in order to trigger moral reflection “inside” science. This does not mean that they were not in favor of wider public deliberation over ethical issues, but that they thought “the public” should be consulted rather than pushed into the role of decision-making. This also has to do not only with an assumed lack of knowledge in the general public, but also with the authority that is ascribed to professional critics of science. It was assumed that the researchers would rather listen to them than to “ordinary citizens”. This assumption was partly based on the experiences at the Round Table, where the lay participants failed to influence the assumptions of the researcher for quite some time. At the fifth Round Table, the invited ethicist was able to question several basic assumptions of the researchers, something that the lay ascribed to the ethicist’s background as a trained academic scientist who was able to meet the researchers at eye level, and thus whose authority to speak in the name of ethics could not be challenged so easily. As an outcome of the mixed experiences at the Round Table, the lay participants called for both wider public deliberation and central actors to guide this deliberation process.

Third, analyzing the sum of the discussions in the small group, the presentation in front of the plenary and other accounts during the Round Table reveals that the lay implicitly had a certain normative hierarchy regarding the question of who should take on responsibility. On top, as the ideal form of how responsibility should be enacted, was the “responsible citizen” who takes care of himself or herself as well as showing solidarity with values considered shared and common. Researchers were also regarded here as “citizens” who equally share responsibility. However, this form of governance was seen as not workable, as everyone “optimizes his own parameters”; that is, different societal actors pursue their respective particular interests. In particular, the researchers were not seen as able to reflect upon the consequences of their actions in an “objective” way. Thus, in the view of the lay participants, a central moral authority was needed that would be able to reconcile these conflicting social interests and provide a moral basis for society. However, the lay were unable to find such a central actor: The state was considered too weak; the economy is only interested in profit; and religion itself consists of too many different moral concepts. Hence, what remained was the demand of the lay people that “the society” should take responsibility. This, however, is much more a question than an answer—a blank position waiting to be filled.

The researchers’ model of the limited responsibility of research, and the lay people’s search for and struggle with a different kind of responsibility, dominated the discussion on responsibility. However, it is interesting to note that other ideas of responsibility were also put forward, but played only a minor role at the Round Table. This is in particular true of the idea that the *state* should rebuild its past role as the central body of collective decision-making—an idea that was promoted by only one lay person. Similar to the other lay people, he regarded the self-governance of science as not able to impose boundaries on research. While the majority of the lay people in search of a different kind of responsibility politics put emphasis on public deliberation and involving "the society", the lay advocating the state as a central actor regarded deliberative political processes as also too weak to be able to control science because the public was not seen as well-informed enough to be able to decide on these issues—an assumptions which he thought also held for politicians themselves. Thus, he suggested the increased institutionalization of expert bodies that take on a leading role in political decision-making processes. In his view, pubic deliberation is not able to exert enough control over science. By and large, state-oriented control of and responsibility for research was rejected by both the researchers and the other lay people. The researchers did not want to give up their authority in the self-governance of science. The rest of the lay people argued in favor of a stronger role for civil society in the governance of science, rather than state control and expert ruling, because the state was seen as too weak an actor and incapable of controlling science in general and genomics in particular, based on the assumption that even strict national regulation is futile as science policy is made on a global level. More liberal regulations elsewhere—frequent examples were the United States or China—will produce knowledge that will be available also in Austria. The global knowledge economy implies that knowledge and innovations produced elsewhere will sooner or later be introduced also in Austria.

By the same token, the idea of a *radical liberal market democracy* that gravitates around the individual citizen as the sole unit of responsibility was abandoned by the majority of the lay participants. At the Round Table, a series of different versions of individualized responsibility existed, in which individual preferences in "consumption" would govern technoscientific pathways. The version suggested by one lay participant was characterized by the assumption that governance has to focus on the individual citizen to increase his or her capacity to take responsibility for his or her own life. This would alter consumption behavior in the market, and people would acquire a "healthier" lifestyle. This change in lifestyle would make drugs generated in genomics dispensable, and thus would exert indirect control over scientific developments through consumer markets. This idea found its counterpart in some ideas promoted by the researchers, which were meant as a supplement to the limited responsibility of research. Here, as basic research cannot take on responsibility, it is the individual citizen who has to take responsibility. The argument of the researchers was that whatever outcomes are produced in basic research, it is still the individual who decides what he or she does. Based on their assumptions that genetics-based "solutions" for the obesity problem are the only workable ones, the displacement of responsibility to the individual citizen in a market should work in favor of their research. At the same time, it also shifts responsibility away from basic research towards the use of technologies "downstream".

Debating responsibility—ethicizing innovation

Drawing together these different ideas of responsibility—in particular the dominant debate between researchers and the majority of the lay participants—what does this mean with regard to the wider context of “ethics” and “public participation” and the transformation in science-society relations?

All ideas had in common that they reflected the wider societal context as well as perceived transformations in science and society. Regarding the researchers, their way of engaging with the issue of responsibility can be seen as a way to defend the autonomy of research in an environment that increasingly interferes with knowledge-production processes in the laboratory. As such, to define oneself as a “basic researcher” is a way to limit expectations from “outside” and create a space for autonomous action. It is also a way to reduce the complexity of the heterogeneous expectations that are seen as increasingly imposed on science, from industry relations over ethics to communication with the public. To draw these clear boundaries, as the researchers did, can be seen as a response to an environment where such clear demarcations are increasingly removed and new kinds of relations are demanded. That the researchers were well aware of this dissolution of boundaries was demonstrated by a different discursive repertoire on other issues, for example on the justification of funding. In terms of what Nowotny and co-authors have described as a transition from mode 1 to mode 2 knowledge production, the choice of the researchers was naturally to employ both modes in their narratives as it suited their interests.

For the lay participants, experiences beyond the Round Table, as well as their direct interaction with the researchers, made clear to them that such strict distinctions between basic research and application can no longer be made. Science increasingly turns out to be an opaque assemblage of a multitude of interests where responsibilities are difficult to attribute. For them, the references to possible obesity drug development made by the researchers demonstrated that science today follows different rules than they had imagined before the Round Table started. They interpreted the incoherence in the narratives of some researchers as non-authenticity of their motives; that is, they assumed that these researchers pretended to be basic researchers while in fact looking ahead to commercial exploitation of their findings. It was not imaginable to the lay participants that the researchers could hold a multitude of narratives which were all “authentic”. The lay participants’ own ideas on responsibility reflected both the experiences at the Round Table and the particular political context in Austria. In this context, there is no discussion about the governance of innovation as such. The dominant narrative is that the population must be made “more aware” of the economic benefits innovation brings (see chapter 5)—criticism of certain applications is interpreted by science policy as a more general aversion of Austrians towards technology and innovation. Thus, “consensus” must be reached that innovation is “good for all of us”.

The quest for a consensual way of governing innovation can also be traced in the narratives of the lay participants, in which they looked for ways to reconcile conflicting interests and provide a consensual moral basis for decision-making on technoscience. The Austrian context is also reflected by the lay participants' way of seeing the role of non-experts and citizens in this process. With regard to responsibility, they argued that "the society" should have a greater role. However, "society", although remaining mostly unspecified, did mean professional representatives of society rather than members of the wider public. This reflects the weak role of public participation in the Austrian context, where civic participation has no tradition, but interests are taken care of by professional representatives. Traditionally, social interests are balanced between the social partnership behind closed doors, represented within the political parties without being explicitly negotiated or seen as already decided "outside", in particular in the EU. Thus, state-oriented politics is perceived as weak, which was manifested in the way the lay participants perceived the potential of the state to govern developments in genomics. Hence, with one exception, the state was not considered a potential central actor for enforcing responsibility by the lay participants. The state was not trusted anymore to have the capacity to control research.

What conclusion can be drawn from the discussion on responsibility for the wider debate over ethical issues of science and technology? The central argument would be that the way the lay people approached this issue indicated a desire *to make the dominant innovation processes and regimes as such subject to a wider ethical debate*. In the analysis above, I showed that the researchers embedded a series of value assumptions in their idea of the innovation process. To a large extent, it is not only our researchers at the Round Table who subscribe to these assumptions, but they are also a feature of how society as a whole deals with innovation. This can be traced in national innovation policies as well as on the European level. In these innovation regimes, a particular role is attributed to institutionalized ethics, namely to oversee a smooth implementation of emerging technologies with respect to cultural values. However, in these ethical evaluations, innovation processes as such are never opened to debate. Lay 'ethical' knowledge would provide an additional resource for decision-making processes on innovation, as it poses questions like the following: What kinds of values are embedded in dominant knowledge-production processes? Should science alone define these values or should society have a say in the process? What does it mean to leave the fabrication of "facts" to science while society is concerned with decision-making on "values"? What commitments have already been made in the innovation process before something becomes subject to societal debates in the form of technologies? Can these commitments be reconsidered? Are there other options for how to govern innovation processes? The lay participants and their idea of a shared responsibility re-define innovation processes as such as ethical questions. Responsibility, they argue, should not concern only technologies applied and implemented in society; rather, responsibility as the continued awareness of the possible impacts of even pure knowledge should be present during the whole innovation process.

They argue that it might already be too late if ethical reflection is delayed until applications are already at the front door, and thus that innovation itself must be regarded as an ethical issue. As such, the Round Table provided no definitive answers to these questions, but produced some suggestions about what directions questions about innovation regimes could take.

8.3. “Working with mice”: Ethical boundary work and the construction of individual and collective identities in the debates around animal experimentation

In this and the following chapter, I will turn to a second aspect of ethics that was important at the Round Table, that is, ethical questions concerning the *means of knowledge production*. The discussion’s focus on the moral dimensions of the means by which knowledge is produced, however, does not mean that science-society relations were not at stake. This question was predominantly discussed in terms of the issue of animal experimentation, which, for both groups of participants, was very central. In many ways, the discussion of animal experimentation was constitutive for the Round Table as an interactive setting, for the mutual perception of the participants and their identities and for the way trust relationships were enacted between the participants.

In the first part of this chapter, I will ask, “What are mice in the laboratory?” to show that animals are not simply technical constructs or natural beings, but moral agents in the laboratory shaping the identity of the involved researchers. I will describe five aspects of animals that are strongly related to the different roles, interests, and identities of the involved participants. In the second part of this chapter, I will describe with what further means the researchers constructed their individual and collective identities by referring to animal experimentation, but also how differences between the researchers with regard to the ethics of animal experiments became evident.

What are mice in the laboratory? Animals as epistemic and moral agents

Today, the use of animals in science is seen as an essential factor for the advancement of knowledge and progress in medicine. The development of biomedical science and the use of animals are inextricably tied together (Rader 2004; Birke, Arluke, and Michael 2007; White 2005). However, the meaning of animals in biomedicine goes well beyond their instrumental relevance. They have been active agents rather than passive objects in shaping material laboratory practice. On the other hand, the use of animals in science often demands the harming or killing of the animals; thus, the use of animals in the scientific laboratory is characterized by the ambivalence of the involved researchers, as well as public controversy, as in both contexts the way the animal are conceptualized in the laboratory collides with other cultural notions of animals. This does not mean that in society, animals have a unanimously meaning. Here, they are encountered with ambivalence too, for example, as pets, as pest or as suppliers of food. As I will describe the researchers narratives of animals as ambivalent at the Round Table, their ambivalences are nothing special but reflect wider cultural ambivalences.

Many of the ambivalences the researchers at the Round Table were confronted with when conducting animal experiments were suggested by the phrase "working with mice", which they frequently used. On one hand, mice were seen as collaborators, as subjects or as "workers in labs" (Haraway 2008, 71), because the laboratory research was highly dependent on the mice's cooperation, their willingness to contribute substantially to research. On the other hand, "working with" also indicates their character as objects and their instrumental use. Here, mice are used as tools in order to reach another goal, that is, the production of knowledge. Thus, as Birke, Arluke and Michael (2007, 53-54) note, the "animal model" is both a technical construct for conducting biological research and a moral category in which different values are brought together and negotiated.

These cultural tensions around experimentation with animals were present in the discussions at the Round Table. The researchers anticipated the issue as a possible highly controversial issue at the Round Table, while most of the lay participants did not identify animal experimentation as an ethical topic that would come up at the Round Table. This picture at the beginning of the Round Table was somehow inverted at the end: Many of the lay participants now claimed that animal experimentation was the most important ethical issue for them at the Round Table. The researchers, on the other hand, barely mentioned the lay participants' ideas on the issue, but rather expressed surprise at their colleagues' attitudes.

Both groups of participants, however, did share some common assumptions regarding the use of animals in research. It was in principle uncontested that the knowledge gained from animal experiments allows for better understanding of human conditions by science in general. However, there were voices among the lay people that raised the question whether the researchers at the Round Table were legitimized in their use of animals, as they often claimed to be interested in the production of "pure" and "application-free" knowledge. That is, the lay asked for the careful balancing of possible benefits against the means of reaching them, and also questioned whether the possible benefits, if agreed on, could be reached by other means. The lay further questioned whether the way the researchers treated animals was the best one, and whether the use of animals for scientific experiments could not be more efficient in terms of reducing the number of lab animals. But it was acknowledged that *animal models yield outcomes providing valid knowledge of human physiology*—something that is strongly contested by the anti-vivisectionist movement (Festing 2005). For most of the lay participants, animal experimentation was a question of balancing right and wrong, risks and benefits. The researchers shared this rationale; however, they had different arguments to add to the balance, and therefore different opinions of where to define limits in the dealing with animals in research. For the researchers, the use of mice in research is a trade-off between the greatest possible proximity to human physiology, in order to yield knowledge that is valid for human application, and the moral and technical "usability" of these model systems, which decreases the nearer they are to human physiology:

> We have mice as model animals, which can be handled very well because they are easy, relatively easy, to keep, and because they are rather close to humans; and yeast. The problem that I always have is: The more simple my system is, the easier I can work with it, but the more distant it is of course from the human. (S6/RT1/2-14)[liii]

Animals in the laboratory, however, are not simply "natural" beings in another context. Biology's obsession with animals has also reshaped animals in many ways, not only in a material sense by changing their genetic make-up. Besides their meaning as "natural" animals, mice, for example, are also considered "data" in the laboratory as well as in scientific articles (Turner 1998; Lynch 1988). As modern biology has seemingly multiplied the meanings of animals, it is interesting to investigate how the mice were discussed at the Round Table, especially because the Round Table was a forum for negotiations with members of the public. Going beyond the dichotomy between animals as "natural" and as "data" put forward by Mike Lynch (1988), I will discuss five such articulations of mice at the Round Table that defined the involved researchers even as they defined the animals themselves. The identities of the animals and of the researchers, both as individuals and as a collective, were co-constructed. As the Round Table brought together researchers with different roles and functions from different hierarchical levels in the research consortium with members of the public, it is interesting to observe who subscribed to what kind of articulation. Such specific articulations, then, contributed to the identity work of different "communities" at the Round Table, be they the young researchers, the project management, the project leaders or the lay participants.

I start with the *epistemic animal*. In this articulation, the mice—and animals in general—were constructed as a source of information/knowledge and as "data" (Lynch 1988) only. The animal was relevant only as a carrier of relevant information that helped to shed light on the puzzles posed by nature the researchers aimed to solve:

> The genes that code these protein substances can knock out an animal. What's more, we can induce the cells of the animal to over-express the protein; that means the cell now produces much more protein than it would produce otherwise. That is, one gets basic biological information, and on the other hand one gets information on what roles enzymes play if the fat metabolism does not work right. (S3/eA/50)[liv]

In this context, the mouse was not considered as a living being with a material body. While the animal is addressed in the singular ("the animal"), no specific animal is meant. Using the rhetorical trope of synecdoche, "the animal" refers to the whole species, and, even wider, to the organic functioning of all living beings. This abstract idea of biological functions misses the possibility of suffering.

This way of seeing animals was mainly enacted by senior researchers at the Round Table who barely did bench work themselves. Their distance from laboratory work was mirrored by the distance in their narratives of the role of mice. The mouse is a carrier of information that contributes to knowledge production. This view, which describes mice as “produced”, was therefore subject to criticism from the lay participants. While the senior researchers saw the mice as a pure epistemic entity most of the time, the junior researchers also partly talked about the mice in this way; but they also had different articulations of mice, which rather pointed to a “naturalistic” understanding. This switching between different meanings of mice suggested their moral struggle, ambivalences in their relationships with animals (see also Birke, Arluke, and Michael 2007, 59) and a habituation and learning process they have to undergo in their scientific careers. The articulation of the epistemic animal, however, made ethical reasoning largely absent, as ethics was seen to require the involvement of living beings. Constructing animals as carriers of information necessary for research was a way to bypass and temporarily suspend moral questions about animal experimentation.

The second articulation was the *instrumental animal,* which means that the use and killing—or “sacrifice” in the language of the science—of animals is necessary in order to reach a particular goal; that is, in the context of the researchers at the Round Table, to gain knowledge of human conditions that may lead to remedies in the future. Mice were not killed for the sake of producing new knowledge alone. Here, the death and the suffering of animals were mentioned, but were balanced with the greater good of improving human health. The animal’s body was pulled between current suffering and potential future benefits for human health. This balancing of benefits was particularly evident in the statements of the young researchers, who often linked animal experimentation to societal problems and goals:

> Yes, of course on the one hand to show what we are actually doing, why we are doing it, what the goals are. That is, that we do not do it because we have fun killing animals, which is absolutely not the case, but that we are really pursuing a goal that we regard as useful of course, and which of course also transfers to others … finding drugs against arteriosclerosis eventually in the future, I think, is a goal that everybody probably finds very interesting, and they want to have it, and we should hurry up. Which finally will come, then. And then simply to demonstrate that genome research too can go in this direction, that it is also bringing positive things for people. (S1/eA/183)[lv]

This articulation manifests the young researchers’ struggle between seeing the animal as “natural” and as a provider of information that may lead—in the future—to remedies for human illnesses. This rationale is based on the idea of a linear progress of scientific innovation (Godin 2006), where basic research on mouse models finally yields applications useful in societal contexts. Belief and trust in this model justifies the use of mice for a greater good. The instrumental animal also was used to justify research in public contexts, however, further highlighting that the end (fighting a wide range of diseases) justifies the means (animal experiments).

The third articulation of animals at the Round Table was the mouse as an *economic good*. They were seen as part of the scientific capital of the research group. It was not the individual animal that was valued, but the mouse as carrier of genetic attributes:

> The mice are our primary capital, aren't they? You can imagine if a Post-doc has to work three years so that such a mouse is produced—it has an enormous value in the laboratory. It is not something with which we [deal] carelessly … It cost a gigantic amount of money. The [mice] are, thus, the real gold that we finally own. That means they are normally better taken care of than our doctoral students or other members of the laboratory. (S8/RT1/3/93-97)[lvi]

The assumption is that the mouse's economic value for the research group guarantees that the animal is treated ethically appropriately. Therefore, in responding to a layperson's question, the researchers rejected the allegation that the mice were "tormented". To emphasize that mice are valued—both economically and thus ethically—the researcher above compares the treatment of mice with the way doctoral candidates are taken care of. Seeing the mice as an economic good was a position that was held in particular by those researchers who had to fulfill managerial tasks in the group and who had to take care of the overall financial resources of the consortium.

At the Round Table there was also a range of articulations that constructed the animal as a *suffering, natural and living being*. The notion of the "natural" animal comprised a wide spectrum of understandings—from the animal in its natural environment to pets. The articulation of mice as "suffering and natural" animals was shared by most of the lay participants, but also sometimes by the young researchers who struggled with that notion when killing them for research purposes—that is, in a context where they had to see the mice other in epistemic or instrumental terms. However, more experienced researchers also sometimes talked about mice as "natural" animals, by linking them to their own emotions. What is interesting in this understanding of the animal is that an emotional relation with the mice as natural beings was supposed to ensure an ethically correct dealing with the mice.

> In principle they are a bit like family. Yes, the mice bear around five to ten offspring. And I am of course very curious, and I have already looked into the mice stable, and I go in nearly every day, because in the beginning the mice are without hair, totally meaty, little, blind bulbs. And as soon as they have hair it gets exciting for me, because if the mice are black, then it is bad for me and bad for the black mice. (S9/RT2/2/84)[lvii]

In this quote, the researcher emphasizes his great care for the mice, his empathetic curiosity. However, empathetic caretaking is not enough in the context of research, and thus in the quote he quickly turns his attention to the selection process that is necessary for his research. As the mice carry a genetic marker through which they develop black fur if they do not have the desired genetic properties, the researcher is then forced to kill these mice instantly. Interestingly, such a way of dealing with the mice was not perceived as unjustified cruelty by the lay people, but, on the contrary, the inclusion of his own emotions led to the fact that this researcher was perceived as “authentic” and resulted in greater trust from the lay people.

Including emotions when talking about animals was also present in the narratives of the junior researchers, but in quite a different way. They particularly expressed their unease when killing individual mice, and they addressed the relation of killing practices and the personal attitude towards killing. Thus, different visions of “care” were articulated. The above-cited senior researcher expresses the care a breeder has for his or her animals when selection is a necessary strategy in order to maintain the value of the whole animal population. The lay people perceived the junior researchers’ uneasiness regarding the killing of the mice not so much as “care” for the animals but rather as care for themselves. My hypothesis is that the lay participants could accept the breeder account because this was familiar from agricultural contexts where farmers are able to express care for their animals but later have to slaughter them. However, killing and the practices associated with it—as reported by the young researchers—are cultural experiences almost no one has had. As a result, they acknowledged the narrative of the senior researcher as more authentic then the accounts of the junior researchers. This may be linked also to the anthropomorphization of the mice in the account of the senior researchers, a rhetorical practice also frequently performed by the lay participants at the Round Table, for example when one lay participant suggested that mice should be “healed” after experiments are conducted “because with regard to humans I act in the same way” (L4/RT5/4/187)[lviii].

Finally, the *social animal* was conceived as an entity shaping the social relations between the researchers in the research group; therefore, the treatment of animals had to be regulated, in order to govern the research group:

> In principle, then, not everyone is really working with the mice. But one should be in principle prepared to do so. … Oh well, it [having no such principle] would always lead to there being do-gooders in the laboratory and mice murderers. And that is, with regard to group dynamics, surely very unfavorable. … in principle it should not be like that, that there are some who consider themselves morally purer than others. Such tensions surely have no place in a laboratory. (S8/eP/172-180)[lix]

This statement, which was repeated several times at the Round Table, emphasizes the great importance the animals have in the context of experimental biology. They are not only a passive means for producing valid knowledge, but also an active agent for shaping the relations among the researchers. While the norm suggests that everyone was “equal” within the group, different researchers interpreted this code of conduct according to their context. The young researcher saw it rather as a kind of personal choice whether or not to kill mice with their own hands. On the other hand, senior researchers and project management were only marginally involved in animal experimentation as a concrete activity.

Starting from the multiple articulations of mice, one can see that the mice and the researchers' identities are co-constructed. Thus, the mice are active moral agents in shaping the collective of the research group as well as individual roles and positions in the group. However, the different articulations of mice must not be understood as standing side by side on the same level, as the researchers also provided a story in which these different articulations were arranged in a chronological order, using the narrative of an individual development: overcoming personal disgust when dissecting animals was a habituation process, where in the end the notion of the animal as an epistemic and instrumental entity should prevail. This, in turn, does not mean that ethical boundaries as shifted further and further, but that they are constantly re-negotiated.

Constructing individual and collective identities of researchers: Ethical boundary work and animal experimentation

I will now analyze how the collective and individual identities of researchers were constructed at the Round Table by the ethical issue of animal experimentation. This was done in two ways. On one hand, boundaries were drawn between the collective of researchers and society. On the other hand, the issue of animal experimentation also constituted differences inside science. To analyze this, I will use the concept of "ethical boundary work" that has been developed by Wainwright and co-authors (2006) using Thomas Gieryn's (1999; 1995) concept of boundary work. Gieryn's concept seeks to explain the discursive work done by scientists and their respective institutions to demarcate science from non-science in order to maintain authority over a certain domain. Wainwright and co-authors, however, see their concept of "ethical boundary work" in a certain opposition to Gieryn's notion, because they try to show "that non-science, in the form of 'ethics', is becoming an integral part of maintaining the image of science" (Wainwright et al. 2006, 735). Ethical boundary work means the practices and discourses that demarcate a certain social domain by assigning it its own morals and ethical norms that distinguish this area and its actors from other cultural endeavors. These norms, *inter alia,* can even mean that ethics in a wider sense (for example, reflection on the social consequences of science) does not apply to science. Socio-ethical domains must be understood as flexible ones that may change their boundaries from one context to another. For example, ethical boundary work in science does include the possibility of excluding other scientists if the way they produce knowledge is regarded as "unethical" (discussed in this chapter as "creation of negative others"). Therefore, "science" as a whole can be a specific socio-ethical domain defined by boundary work in one context; however, other collectives are also possible as socio-ethical domains, for example a specific research group or a cohort group within this research group. Ethical boundary work, then, serves to construct a socio-ethical domain within which certain actions are considered morally unproblematic.

Ethical boundary work has a crucial impact on the construction of identities for scientists as individuals, but also for scientific collectives, be it a research group or "the" scientific community" as an imaged community that all scientists are part of. The negotiation of norms and values associated with animal experimentation contribute to the way science and scientists are understood by others and to the way they perceive themselves. Negotiations over moral questions, however, must not be understood as impeding the process of becoming a full-fledged scientist, but rather as being an integral part of this socialization process. Ethics and scientific practice mutually shape each other. Scientists always enact both ethical and epistemic arguments simultaneously. In doing so, they perform an integral part of their identity work aiming at the establishment of moral-epistemic communities. Their identity is made in the course of this ethical boundary work. It not only defines them as scientists in general, representing science at large, but also defines the researchers' specific positions within a smaller research collective.

Collective identities of researchers and the ethical demarcation of the "outside"

Concretely at the Round Table discussion, ethical boundary work towards an "outside" with regard to animal experimentation employed two prominent mechanisms. First, the work with mice was described as a *habituation process*, necessary for enculturation as a researcher in modern biology, as a learning process that every researcher had to go through. This allowed for an individualization of researchers in the group, and accounted for hierarchies within the group of researchers. The idea behind the narrative of the habituation process was that one cannot assume that all researchers naturally share the same values with regard to animal experimentation; however, through a learning process, all researchers would become part of the same moral-epistemic community in the end. Second, in light of an increased public attention to ethically sensitive research, the researchers were eager also to emphasize differences between their ethical conduct and that of other scientists who were considered immoral in their dealing with animal experimentation. These *negative "others"* were created together with a positive image of themselves.

The first narrative of a *habituation and familiarization process* focused on the individual researcher and his or her engagement with animal experimentation as a learning process. While expressing too much empathy for the animals was not seen as a good way of handling the work with mice, it was also unwelcome if one liked to kill animals:

> I say, if somebody reacts like "when can I finally [kill] my first mouse", he needs a doctor. He is misplaced in a laboratory. I know no one who with great lust and love is thrilled when she walks into the mice stables the first time to kill a mouse. That is by all means a step that takes getting used to, but is also necessary. Similarly—I always say that too to people who eat many chickens in course of their life but have trouble killing a chicken: Theoreti-

> cally, one must say if I eat it then I should be also prepared to kill it. (S8/RT1/2/103)[lx]

While the work with mice in the laboratory entails the killing of mice and this is sometimes perceived as “cruelty” by animal rights activists, this researcher puts great emphasizes to not include ongoing researchers with a cruel attitude in research. However, to practice the killing of mice is seen as an important learning process that should not be bypassed, as the work with mice is a necessary and central element in modern life sciences. Working in the life sciences means that researchers benefit from the knowledge gained from animal experimentation; therefore, one should—in principle—be able to kill the animals her- or himself, and accept that being a researcher in the life sciences entails “sacrificing” animals for “higher purposes” as a process of enculturation. Thus, a careful balancing process between habituation and sensitization is pivotal to the researchers.

The process of enculturation was seen as a gradual process of habituation to animal experiments in which personal ethical boundaries have to be transgressed little by little so that originally undoable work becomes more and more accepted:

> I can only talk from my experience: … I have always worked with animals, and my field was for a time transgenic animals. … And in the beginning I also had not overcome this hurdle. [I] thus told myself: “… you cannot do it. Do you really have to genetically modify mice?” And then I only started with it in the years ’91 or ’92. Hence, you can see, one of course is making up one’s mind, and not everything that later becomes a matter of course is a matter of course on the first day. (S8/eA/402-28)[lxi]

This senior researcher describes a gradual enculturation process in which moral thresholds move towards performing the work with animals as a matter of course. The young researchers also held the narrative of boundaries transgressed in the course of a scientific career, “because one simply has to, because it belongs to the work, which one must deal with” (S1/eP/223).[lxii] This process was seen as one that would never be complete, as scientific practice and progress continues to demand that ethical barriers are re-negotiated: “And that is simply that which … from the beginning of my study until now and probably also in the future always will be there, I think” (S1/eP/223).[lxiii] According to the habituation narrative, researchers conform to the work environment they live in rather than drawing clear ethical boundaries that are never transgressed. Being a researcher in the life sciences means, therefore, a constant re-negotiation process of personal values in order to prevail over the course of a career.

Having said that, re-negotiating personal ethical boundaries do not mean that the animal experimentation should be done without any unease. The demand was to balance carefully between gradual habituation and maintaining sensitivity to moral questions in animal experimentation. Furthermore, the process of habituation must no be seen as a general one in science or biology. What counts as acceptable habituation to animal experimentation, and thus the moral limit, is defined rather by the smaller collective of the research group than by legal regulations or by general scientific standards.

To a certain degree, the lay participants shared this assumption that a habituation process is necessary and unavoidable in science by comparing it with experiences from their professional or personal lives: "I have for example many friends who work in prison. There is a different way of speaking…. That is, in my opinion, normal; that goes through all domains [of professional life]: One is numbed to that with which one is working" (L10 RT1/4/77).[lxiv] While habituation was mostly accepted as a normal way of dealing with ethically difficult situations, this position was also subject to criticism. Gradual habituation was also perceived as a danger by the lay participants, as it gets harder and harder to see and define ethical boundaries.

What has also been present in the researchers' discussions on animal experimentation was the myth that scientists share a special position to science. Concretely, this meant that the researchers quasi naturalized their way into research as one that was already laid out in childhood and the way they developed interests in knowing things beyond the usual. This allowed performing a collective identity work through the narration of a commonly shared history of how one becomes a researcher by telling stories about the origin of scientific curiosity. Such stories were told each other in a small group discussion where the members of the public were not present. They did not share these narrations with the lay participants, as they possibly considered them "outsiders" who would not share the same understanding. By telling such stories, the researchers did identity work by placing the origin of scientific curiosity in childhood. As such, they mirror the wider societal ambivalences with animals—on one hand great emphasis is put on the protection of animals; on the other hand, animals are killed for human purposes.

The *creation of negative "others"* (Michael and Birke 1994), and simultaneous sanitization of their own practice of and reasons for animal experimentation, was the second important rhetorical strategy of the researchers to manage their ethical unease and also to foster the collective identity of the research group. In the discussions with the lay people, the researchers tried to display their own way of dealing with animals in a good light by hinting at practices elsewhere that they did not regard as ethically sound. Distance was a crucial resource, as spatial distance also meant a cultural and cognitive distance for them. Thus, very often, these negative "others" were located abroad, for example in countries that did not comply with "our" standards.

One example was the use of hedgehogs in basic research, because this species has a protein that only exists in humans and hedgehogs and is thus an interesting model organism. The project manager explained that the hedgehog was a protected species in Austria, but "In China there exists no such regulation. And in China this experiment was conducted" (S8/RT6/3/156).[lxv] However, as publication in Western journals requires the approval of ethics committees, "the Chinese researcher who according to our standards is doing a prohibited animal experiment really has theoretically only one way out: he's publishing it in a Chinese journal. Only, in practice, it has no meaning today, because … no one reads these journals" (S3/RT6/3/188).[lxvi]

"Chinese research" was one example of a negative "other"; and, simultaneously, the Western internal control systems of peer review and ethics committees were displayed as institutions that guaranteed that published knowledge was ethically sound. Michael and Birke (1994, 201) concluded that the depiction of foreigners as morally questionable "appeals to nationalistic propensities in the would-be audience." Besides the slightly nationalistic implications of the creation of the negative "other" abroad, the negative account of foreigners worked to create trust in national regulatory regimes, and thereby in research itself.

Other examples of cases where the researchers at the Round Table would draw ethical boundaries were experiments on living animals, which were described by the researchers as "disgusting", or on primates. These negative "others" comprised a wide spectrum, including research on animals done in non-Western countries, research conducted on animals classified as evolutionarily "higher" than mice, research for purposes which were not regarded as ethical (e.g., for the cosmetics industry) and experimenting on living animals and thereby causing pain.

The above arguments were directed against other research methods, and were made in the presence of the lay participants. The researchers, however, also enrolled a different type of negative "other" when discussing among themselves in a small group. Here, "society" appeared as the negative "other". In a quite emotional discussion, the researchers listed a number of societal practices that could count as "unethical" by the standards of animal experiments. They looked for resources through which they would be able to counter the arguments of the lay people in the following discussion. The consumption of meat and the killing of mice as pests in agriculture were such examples for the researchers, through which they aimed to display the double standard society employs with regard to research: "So, I think somebody who eats meat is not allowed to be upset about animal experimentation, which is on the best possible level" (S6/ RT5/sgS/427).[lxvii] For them, medical research was a higher purpose than eating meat. Referring to "meat" allowed for a balancing between animal experimentation and other commonly accepted cultural practices in which the killing of animals for reasonable purposes is accepted.

While on one side creating negative "others", the researchers also *aimed to create a positive self-image* with regard to animal experimentation. The researchers often emphasized, even when among themselves, that they handled animals in research according to high ethical standards, for example, "that they are always anesthetized and that they are always killed immediately so that they do not even squeak, it goes so fast that they do not notice it at all" (S6/RT5/sgS/419)[lxviii] in order to "make it as convenient as possible for the animals" (S6/eA/438).[lxix] The creation of a positive self-image also included hinting at ethical boundaries in the research practice. In a particular story, they emphasized that at one point in time they had voluntary abstained from mice experiments that they could in principle have conducted according to legislation, but where the suffering of the animals was perceived as too high:

> one can justify a certain degree of suffering that is short-term of course perhaps to oneself—only if the animal dies instantly. With regard to this experiment, however, it was a longitudinal experiment that went over 24 hours. … The experiments are absolutely internationally accepted. It isn't something that would be forbidden, but we have internally agreed that we will not do it. (S8/RT6/3/148)[lxx]

The researcher emphasizes that, although the experiments are common internationally, the law would allow them and there would be new knowledge produced, the researchers' personal impressions of and experiences with the mice made them abstain from these experiments. Thus, while mostly arguing that following legal regulations as well as Western standards of scientific conduct was sufficient to be on the ethically safe side, there were instances where the ethical conscience of the research group was more pivotal. The central role of this story, however, was to shed a positive light on their own practice with mice in front of a public perceived as critical. More important than the individual conscience of the research group, however, were institutional structures and mechanisms that ensure ethical treatment of animals in the lab. Particular emphasis was put on peer review, which was seen as able to limit unethical conduct:

> [It] is becoming increasingly common that the publisher requires the approval number of the animal experiment showing that the experiments are permitted to be conducted. So, in the meantime, it is also common in publications, similarly it has been the case with studies on humans for a long time already, so that the ethics committee was demonstrably concerned with it. (S8/RT6/3/122)[lxxi]

The positive display of the researchers' own practice with mice in scientific experiments complements the depiction of negative "others". The aim is to foster the moral-epistemic position of the research group and contribute to the identity work of the researchers. Institutional as well as individual ethical limits take care that animals in the laboratory are treated in a good way. Such narratives served to absorb ethical protests from the public and to legitimize the researchers' own research in a societal environment perceived as potentially adversarial to animal experimentation.

Ethical boundary work between researchers

The discourse of the researchers analyzed in the section above aimed at the construction of the identity of the participating group of researchers by drawing boundaries between them and society as well as "negative others". This helped to create the image of a collective that shares common values, distinguishing them from non-scientists as well as from immoral scientists. However, the image of a coherent moral-epistemic group was opposed by narratives that emphasized ethical differences with regard to animal experimentation within the participating group of researchers.

The Round Tables assembled researchers with quite different research backgrounds—some worked directly with mice, some only used mouse tissue prepared by others, some did work on yeast but created their experiments based on the knowledge gained by research on mice. Furthermore, the involved researchers were at different stages in their careers, ranging from doctoral candidates to principal investigators. Also, different functions of the research group were represented at the Round Table, from people working only in the laboratory to the project manager, who was not directly involved in animal experimentation. This heterogeneity of the group led to the fact that the "negative others" were not only situated outside the research group, but also within. Thus, who counted as a "negative other" was highly dependent on the context: When speaking of animal experimentation from the perspective of the whole collective, the negative "other" might be in located abroad; when speaking of the work of individual researchers in the group, the negative "other" might be sitting just next door. Hence, some participating researchers themselves became negative "others" if they did not appear to meet the standards of the speaker. I will draw here on two examples: First, researchers who were not part of the subproject working with mice directly often emphasized how relieved they were that they did not need to kill animals themselves. This is a form of ethical boundary work, however, without blaming someone directly. Second, there was been a debate between the project manager and the young researchers during the fifth Round Table over whether the principle that everyone should be ready to kill mice should apply to them and whether the method of handling the animal experimentation issue should be subject to a collective norm or to personal attitudes.

Seeing oneself in the ethically "fortunate position" of not being concerned with ethical issues of animal experimentation was an argument that was frequently employed by the researchers at the Round Table. In doing so, they did not actively create a negative "other" within the research consortium, but often mentioned that their work did not include the killing of animals in a direct way, and therefore that they did not have to bear such moral burdens as those conducting animal experiments. This was an important part of the ethical boundary work, especially in front of a public that may have had reservations about animal experimentation:

> It was actually this way then, in the discussion in the breaks over coffee where they [the lay people] asked if one actually works with mice. ... Thank God I always said, no, I receive them ready-minced [laughs]. (S2/eP/155)[lxxii]

Because this post-doc received mouse tissue from another research group and did not have to kill them, she saw herself in a "fortunate position", a narrative which was also employed by other researchers. While they saw the necessity of animal experimentation, they would prefer not being directly involved in it. This "fortunate" position was sometimes ascribed to a personal decision during education. I described above how getting used to the killing of animals was seen as a process of moral and epistemic enculturation in science and a necessary process in becoming a scientist. However, the narrative of habituation was sometimes contrasted with a narrative of "sensitization" during the study, as one researcher describes it:

> I actually once, in animal physiology training, killed a frog, and then said, I will never do this again. That's enough. I only thought I have to try it one time to see how I do with it, and then I decided, for me, okay, I do not want [to work] with higher animals. (S7/eP/27)[lxxiii]

This narrative is opposed by the project management's norm that working with animals is mandatory, and by the narrative of a habituation process necessary for becoming a full-fledged life scientist. Of course, there is a strong power dimension in those concepts. Often, researchers who did not have to kill animals themselves were in a hierarchical position where they were able to opt out. Thus, the specific role and position in the research consortium is linked to the ethical standpoint one holds and the way animal experimentation is addressed.

Besides statements of an ethically fortunate position, two different ethical cultures—that of a *collective norm and of a personal choice*—within the group of researchers explicitly collided in the debates at the Round Table. The project management set up a rule that everyone in the subproject must in principle be ready to kill mice and prepare mouse tissue for further analysis. The aim was to create a "moral-epistemic community", with the intention of avoiding social conflicts in the group, because without such a principle of governance those who killed the mice would be considered morally "bad" by others in the group. This principle was supposed to strengthen the social coherence of the research group, but also to prevent conflicts in wider society over animal experimentation from being carried into the laboratory. In that sense, such a governance principle is a kind of ethical boundary work, as it draws a clear line between those who are involved in the "work with mice"—the researchers in the laboratory—and those who are not—the society outside.

The project management stated this norm at Round Table one, directed to the public to stress that the participating researchers should be regarded as a coherent group with regard to animal experimentation in order to protect those who actually kill mice in the laboratory practice. Other researchers did not comment on the statement, so the lay participants could assume that all the researchers would share this norm. During the fifth Round Table, finally, after the young researchers had been asked directly why they went into research and what ethical aspects they saw in their daily practice, a young researcher responded:

> after I learned that in this department mouse research, animal experiments, are conducted, it was clear to me [that] I certainly will kill no mice. So, I do research with them … and I vaccinate them also, but I certainly kill no mice. So, that [is] actually a personal attitude of mine … there is a separate mouse stable. Anyway you have seen how it works. There are people there who do

> this, and if I do not have to do it—I do not want to do it. So, that is actually a personal attitude of mine. (S5/RT5/4/88)[lxxiv]

Despite her awareness that a common norm in the other subprojects exists, she clearly states that killing mice is not an option for her. As there are people whose primary task is to kill mice, the young researcher does not see the necessity for doing the killing herself. In the following discussion, a rather emotional debate between the project management and the young researchers developed. The project manager asked what the difference was between standing aside and observing how a colleague superextends the mouse's neck and doing it herself, because she was "actually the executioner, who is responsible anyway" (S6/RT5/4/88-173).[lxxv] The young researcher, supported by a more experienced post-doc, answered that this would be a question of "personal attitude if you have to consciously kill a living being".

Here, two ethical cultures collided: The project management aimed at the comprehensive principle for the whole group in order to avoid moral discrimination within the research group. The personal emotional level of the involved researchers played no role in the project management's rationale. Ethics, here, were conceptualized as concrete guidelines providing orientation for individual actions from the top down. The young researchers, however, located ethics on a very personal level as a question of individual choice. Ethics meant that one was free to decide individually in morally problematic situations and not along law-like rules that ignore personal attitudes.

The importance of the animal killing issue and the debate between the project management and the young researchers was highlighted by the prominence the involved researchers gave this debate in the interviews we conducted with them after the Round Table:

> What was surprising was also the reaction of a certain colleague who was surprised when I said that we as young scientists do ethically reflect on this. … Because it is true that we have to deal with it. And because of what it is, it is hard for us to deal with it. … One month ago or so we had a test series that was relatively challenging with the animals, where three of us worked together, where we really some days simply sat together and talked about it again and again. Others could not even join us, simply because we had to deal with it on our own. And I was surprised then that the colleague was so astonished there [at the Round Table]. Because I think, for me it is also important that we reflect on it ethically, also regarding the handling that we do there, and not simply go to work unreflectively. (S1/eP/11)[lxxvi]

The young researcher argues that such a space for ethical reflection is strongly needed to cope with the difficult work with animals. Yet, they preferred to keep their discussions among themselves, as the issue discussed was regarded as too personal to be able to share it with colleagues who had different interests and a different position in the research group. For the young researchers, ethical debate was more a means for personal reflection, and not for developing principles to guide daily practice in laboratory.

The case of animal experimentation at the Round Table is an example of how ethical and epistemological aspects cannot separated from each other, as the mice in the lab are both moral and epistemic agents. Beyond the role mice had in the development of life science as a discipline (White 2005; Birke, Arluke, and Michael 2007; Kohler 1994), they are pivotal for the construction of the individual and collective identities of researchers. The discussion of animal experimentation revealed the ethical boundaries the researchers draw not only between themselves and society, but also between researchers of the same research group. While they on one hand formed a moral-epistemic collective, there were also differences in the way ethical questions were handled. These differences are related to the position in the research group and the career stage.

Personal struggle and ambivalence with animal experimentation could be seen as "productive", in the sense that they allowed the young researchers to reflect on their ethical attitudes and on the moral implications of a career in the life sciences. That is, a certain degree of ethical concern does not impede a scientific career, but was seen in this group of researchers as an obligatory enculturation process. Different moral cultures—the collective ethical behavior of the research group and other ways of dealing with animals, be it in society or in science, as well as the ethical boundary work performed within the group—contributed to the creation of identity both for the group and for the individual researchers.

8.4. Animal ethics, authenticity and public trust in science

In the following pages, I analyze the roles trust plays in ethical debates—an issue to which little attention has been paid by bioethics so far (De Vries and Kim 2008). In this relation, the performance of "authenticity" at the Round Table is relevant regarding two dimensions. The first one involves science in its non-personal, institutional aspect. For the lay participants, the question was whether they were allowed to see "authentic" science taking place, that is, to look behind the assumed closed doors of everyday scientific practice. The concrete touchstone, here, was the lay people's request to see the mice stables. The second dimension concerns the way the researchers expressed how they coped with the necessity to kill animals for the production of knowledge. My assumption is that those researchers who were able to make visible their "conscience" and "unease" about animal experimentation were the most authentic and the most trustworthy. "Authentic" in this context means that the lay participants considered the articulations of the researchers "true"; that is, they did not think that there were other, hidden motives and interests behind the statements.

For both researchers and lay participants, attitudes towards animal experimentation were a touchstone for mutual trustworthiness. For the lay participants, the question was whether the researchers treated the animals according to moral convictions that they thought should be a standard in a society—that is, that they did not regard animals in purely instrumental terms as a mere means to create pure knowledge. On the other hand, the researchers also expected to be appreciated and trusted by the lay people. The researchers implicitly assumed that the lay participants' trust in the researchers' work, and, much more importantly, *in the researchers as persons,* were a necessary and important condition of robust science-society relations. This indicates changes in the relation between science and society: non-scientific criteria, such as public trust, increased their importance for science, but where particular dimensions of scientific practice, for example the way scientists act as "persons" when engaging with the public, also received increased prominence in the public's understanding of science.

For more than twenty years, science has perceived a lack of public trust in science in many different dimensions, be it the legitimization of its outcomes or its means of producing knowledge. As a "countermeasure", science and science policy introduced a range of changes aiming at a democratization of science, for example "gestures of transparency" (Brown and Michael 2002, 270) and participation exercises. Indeed, opening science to societal concerns also implies that the authority of scientists as experts has been widely challenged. In that light, Brown and Michael (2002, 259) observe a "transition from the demonstration of expert authority to that of public authenticity".

Such changes in the image of science and scientists could also be observed at the Round Table discussions. Of course, rather classical notions of scientists often prevailed in the discussions between researchers and lay participants. The researchers' epistemic expertise was seldom challenged when they were explaining the scientific background of their research. However, when the lay participants perceived a clear "societal" dimension in the researchers' statements, their authority as experts was called into question. Most obviously, this was the case in the discussions on animal experimentation, where the lay participants did perceive themselves as legitimate advocates for animals where no expert knowledge was needed in order to participate in an informed discussion. The researchers responded by lending their narrations an authentic tone, that is, resigning from authoritarian gestures as experts and emphasizing the "emotional" dimensions when dealing with experimental animals. This micro-politics of emotions was supposed to re-establish what science qua authoritarian experts failed to do, that is, to regain trust in the eyes of the public.

Looking inside the mice stables: See-through science and public trust

The lay participants at the Round Table perceived genomics as a complex network that was entangled with society in a range of dimensions, creating tremendous repercussions on cultures values. Because of its perceived complexity, it was rather difficult for the lay participants to come up with concrete future scenarios with a high potential to be realized. On one hand, there were vague ideas that a genomics-informed health system would increase the pressure on the individual to subscribe to this health paradigm. Genomics invoked many uncertainties and unknowns that were considered difficult to deal with. On the other hand, the researchers were often able to draw boundaries between their present research and its potential realization in the form of applications in society. Hence, the question for the lay participants was what criteria—in the absence of clear technoscientific trajectories—they should draw upon in their judgments of the work of the present researchers and genomics in general. In answering this, researchers' "body language" regarding animal experimentation can be seen as a cognitive resource and anchor for the lay people in dealing with complex genomics-related matters. Looking at how the knowledge was produced in the laboratory was regarded as a token for trustworthiness and the controllability of science.

One dimension in the assessment of genomics was to look at the technology as such. For the lay people, genomics was characterized by a lack of the clear material artifacts that they were used to from other technologies, and that would have served as anchors to position themselves in relationship to this technology. One lay participant expressed his ambivalence as follows: "What would worry me just now, like a huge laser canon, I did not see. They [the researchers] really did not have them." (L12/eP/191).[lxxvii] He talked about a material artifact that would have signaled clearly to him that this technology was dangerous. This he could not find. However, what does this absence mean? Is the technology therefore harmless, or is it just playing tricks and deluding the public? The lay participants made clear that the perception of genomics as an opaque network constituted a danger, because the technology's repercussions on society only become visible when it is already too late. In this situation of high uncertainty and ambivalence, the researchers' discursive behavior with regard to animal experimentation served as a touchstone and reference point to address these wider concerns. Their rationale was, if I can trust the researchers to really conduct research in an ethical way, it would be a criterion to evaluate what genomics does and could do in society. On the other hand, the formation of trust is not a unidirectional movement. It was also crucial for the researchers to trust that what they told and showed the lay people would not result in premature conviction or rejection of the research or the researchers as persons. Thus, the researchers did not quickly open up their minds and laboratories, as they feared to be misunderstood.

A crucial episode at the Round Table with regard to the question of mutual trust began with the question whether or not the lay participants should be allowed to visit the mice stables. After a visit to some laboratories of a subproject during Round Table 2, some of the lay participants expressed their wish to see the mice stables, because some of them had the impression that, while certain things had been shown to them, others had been deliberately hidden. The researchers responded that so many people would endanger the mouse populations by bringing in infectious microorganisms. However, the lay participants insisted on seeing the mice stables, and asked if it would be possible to film the mice in the stables. Even then, the researchers were very hesitant to show the mice stables; therefore, the lay participants suspected that something wrong was going on in there. As the researchers themselves did not regard the mice stables as a "nice" place to show to outsiders, and because they feared that the lay people's impression of the mice stables would have a negative effect on their relationship to the researchers, they employed a range of arguments for why it was "better" for the lay people not to see the mice stables. When one lay said that she had never been in "such a laboratory", the project manager responded by saying that the mice stable is a room "where racks and cages are disposed" (S6/RT2/2/121).[lxxviii] The researcher described the mice stables as mundane objects familiar to everyone, and thus not worth seeing. On the other hand, the researchers also emphasized that they wanted to spare the lay participants the sight, because of the unpleasant smell of the mice's excrements.

Later at this Round Table, when reflecting, one lay said that she felt "that they want to hide something" (L5/RT2/3/418).[lxxix] This reluctance of the researchers to open up the stables made it even more interesting for the lay participants to get access to the mice stables, because "something becomes more interesting if you are not allowed to do it" (L2/RT2/3/402).[lxxx] Because of some interventions from us as organizers of the Round Table, the lay participants finally were able to see the mice stables. What was interesting with regard to this episode was that after the lay participants had visited the mice stables and had seen with their own eyes how the mice were held, the mice stables as an object of discussion largely disappeared.

This episode at the Round Table can be regarded as an instructive lesson about how mutual trust relations between science and society are negotiated: The way the researchers dealt with animals was a touchstone for the lay participants for dealing with the complexities of genomics in general. While it was impossible for the lay to control or to oversee genomics in general, they could themselves visit the mice stables and personally assess how the mice were kept as living beings. Because of the idea that "transparency" is a crucial criterion for the trustworthiness of science, getting access to the mice stables was a test site for the lay participants' trust. Their question was whether the researchers trusted them enough to show those elements of their research practice that were not considered "clean" and "nice", and which were usually hidden from the public, as to a certain degree the lay participants had the impression that only the positive aspects of research were shown to them:

> We have surely, then, seen the thousand really good mice... [the] awful looking mice we have of course not seen. Yes, well, for me this also fits so to speak into the image that it was displayed very, very positively. But as I said, I certainly understand it. So, I would probably do it exactly the same way if somebody came to me and wanted to see everything. Then I would also show and tell him only the nice things. (L4/eP/31)[lxxxi]

While still thinking that the researchers had not shown the true face of their practice to the lay participants, and even that the laboratory visits had been "staged", even this lay admitted "that animal experimentation also comes with it" and that "it was totally normal. Thus, I rather thought: Well, why do you actually put up a fuss about the animal stables?" (L4/eP/183).[lxxxii] Other lay participants shared this response: "I do not believe that they desperately have something to hide, but she [one researcher] maybe also communicated this somehow with her behavior" (L5/eP/175).[lxxxiii] In an ironic turn, some lay participants were rather disappointed that they had not seen things that shocked them, describing the mice stables as "nothing special" or stating "I have seen more awful things, I have to admit" (L5/eP/55).[lxxxiv] This argument was supported by the plain and down-to-earth description one lay provided of the mice stables: "I mean, yes, there are many mice caged and everything is in artificial light and through the experiments of course some mice will die, and in these mice there will be certain genes … deactivated. That then was not so tragic for me" (L2/eP/7).[lxxxv]

The crucial point in this debate was not the mice stable as such, but the lay participants' expectation of the gesture of being invited into the stable as an expression of the researchers' trust that the lay people could cope with it. The very act of inviting the lay participants into the stable enacted a *mutual* trust relationship. For those among the lay people whose trust in science was already established, seeing the mice stables became an interesting event only. However, for the majority of the lay people the negotiations over whether and why (not) they should be allowed to see the mice stables were much more important than the stables themselves. Thus, once they had seen them, they were not interested anymore, as the researchers had "proven"—in this context and for a limited time—to be trustworthy, and that they also trusted the lay people. The "institutional body language" (Wynne 1991, 1992) of the involved researchers, rather than the facts and information provided, was crucial for the possibility to create a trust relationship.

While the episode of getting access to the mice stables shaped the trust relation between researchers and lay participants to a great degree, it also had important implications for the lay participants, because it triggered a process of regarding themselves as a more coherent group sharing particular interests. This was clearly expressed by one of the lay participants after the Round Table, who said that "I rather liked the group dynamics that developed when the researchers absolutely did not want to show us the mice stable. That was somehow funny. There we [the group of all lay people] suddenly were very much as one" (L10/eP/295).[lxxxvi]

The researchers also interpreted the mice stable episode in a positive light after the Round Tables, as they also wanted the lay participants to have good insights into the GOLD project:

> I was astonished that the members of the public evaluated the visit to the mice stable positively. I think the animal stable is not particularly beautiful, and there are animal stables which are more convenient for mice. … Whereas I had the feeling at the end of the discussion [that] most of them actually were convinced that it was necessary, and that we really try to use other methods than animal experiments. (S6/eP/165)[lxxxvii]

The researcher assumes that providing deeper insights into their research convinced the lay participants of the need for animal experimentation. For the lay participants, the question of seeing the mice stables was not associated with the fundamental question of the necessity of animal experimentation, but rather with the question of how far the researchers were able to trust the lay participants. To borrow the concept of "see-through science" from Wilsdon and Willis (2004), the lay participants wanted to see a more comprehensive scope of scientific practice in order to be able to make sense out of it with regard to its social and ethical aspects. Looking behind closed doors—in our case those of the mice stable—was a crucial factor for establishing trust relationships between science and society. Of course, genomics as such remained an opaque network for the lay participants, impossible to see through. More important was to be invited in and to see particular places where the lay participants were able to see research in action as a possible test field for trust.

"Authenticity" as a touchstone for the public's trust

Apart from the question of a rather institutional trustworthiness of science enacted through the question of getting access to the mice stables, the researchers themselves were crucial factor for the possibility of a trust relationship between science and society. For the lay participants, the way the researchers dealt with animals was not only a question of ethics, that is, do the researchers treat animals according to ethical norms? While this was an important issue (see previous chapter), the debate also focused on another aspect. Based on the shared assumption that "authenticity" was a value that might help to develop a more stable relationship between science and society, the possibility of animal experimentation as an ethical question was translated into a question of mutual trust. Brown and Michael (2002) have observed that "authenticity" in scientists' engagement with society has gained some importance in recent years, accompanied by a loss of relevance of enactments of expert authority. The importance of authenticity is, too, accompanied by calls for "transparency" in science policy, which emphasize "democratization" and "transparency" as new strategies for regaining the public's trust. Transparency today is a nearly unquestioned democratic value; however, transparency alone is not a convincing vehicle to overcome the crisis of public trust (Brown and Michael 2002). While "transparency" addresses a rather anonymous institutional dimension of science, "authenticity" steps in at the personal level as a powerful rhetorical device to persuade others from their own goals and interests. It is a rhetorical means to convince others that attitudes made explicit in words are consistent with the "real" attitudes and emotions of the person expressing them. The perceived coherence of these implicit and explicit attitudes can serve as a measure for the trustworthiness of the person.

The researchers' disclosure of their authentic emotions when killing mice for research purposes was pivotal for the lay participants in assessing the trustworthiness of the researchers. The particular episode I want to describe here to exemplify my argument took place during the second Round Table discussion, when a researcher concerned with the making of knockout mice[36] was invited to the discussions. He described his relation to the mice as a rather emotional one, as he regarded them as "my family". He emphasized his care for the animals and their housing. However, he also explained that he had to eliminate them if they did not carry the right color of fur, which was an indicator for carrying a specific gene defect he wished to generate. A lay participant then asked the researcher, "How do you feel when you have to simply eliminate these black mice? (L6/RT2/2/316).[lxxxviii] The lay participant was not interested in the general question of the rights and wrongs of animal experimentation, but was interested in the very personal feeling of the researcher. The researcher's response provides a personal account of the suffering he underwent when he started to kill mice during his PhD work. He said that, when killing the first mice, he had "sleepless nights" and "there the pulse is at 180".[lxxxix] He then described the killing as a habituation process, because "it belongs to my work, I decided in favor of it".[xc] He emphasized that he tried to kill the animals as quickly as possible, but the killing of mice remained a balancing between different rationalities for him:

---

[36] Knockout mice are genetically engineered mice for studying functions of genes. The role of certain genes are studied by „knocking out", that is, turning off these genes. The inactive genes cause differences in the physiology of the mice. The first knockout mouse was created in 1987-1989 (see Nature 2002).

> Indeed, one can say that by and by one becomes dulled, but I believe one can only really do this if one identifies a purpose in it. If I were doing work where I was not convinced that it is important, then it would not be thinkable to kill the animals. (S9/RT2/2/330)[xci]

What is particularly of interest here is how the lay participants responded to such enactments of "authenticity"—openings of the "conscience" of the researcher in form of a "confession". Many lay participants valued the researcher's account because he was ready "to provide a very personal commentary on my question, because I have had the experience that scientists never speak truly about personal things" (L3/RT2/3/277).[xcii] This researcher's way of talking was compared to images of how scientists are, namely to images of emotionally detached thinkers, which are particularly present in public images and stereotypes of scientists (LaFollette 1990). He was seen as an exception to an assumed rule of killing mice without emotions following only the rationale of generating new knowledge. Another lay participant also expressed her sympathy for this researcher, as he "was attracted to his mice" (L7/eP/51).[xciii] Yet another lay regarded the statements of the researcher as reflecting wider cultural dealings with animals and thus holding a mirror up to the lay participants' own everyday practices that involve the killing of animals:

> that he is reproached of course always with killing the mice. If you look at it from the flip side, one has to say that we all have to be vegetarians if we really refuse [to kill mice]. I only put this up for discussion [because] in principle no one is reflecting on that when he enjoys dinner. (L5/RT2/3/193)[xciv]

The disclosure of the researcher's struggle when balancing the needs of research with his own emotions was perceived as an "authentic" account by the lay people. Consequently, he initiated reflections about wider cultural unease regarding the killing of animals for human purposes. The researcher was perceived positively because he did not provide a ready-made and clean "solution" for the problem of animal experimentation in research. What at least a good proportion of the lay people at the Round Table seemed to demand was that scientists should not perform the "image of difference" (LaFollette 1990), but act and think like "normal" citizens.

While this particular researcher and his accounts of animal experimentation were perceived in a positive light, this does not hold for other researchers at the Round Table. Other researchers concerned with management tasks, who do not kill animals themselves anymore, who are concerned with managing the public communication of the consortium and thus who have more advanced rhetorical abilities were perceived rather ambivalently by the lay people. The ability to "sell" science in a rather smooth way to the public was not seen as a property an "authentic" scientist should have. This way of communicating was seen as "more smoothed", including the tendency to leave out aspects that do not fit into a clean message.

While rhetorical competence and a smooth way of talking were evaluated in a negative light with regard to trustworthiness, the same holds for silence. This point of critique by the lay participants particularly concerns the young researchers involved in the Round Table discussion who—for a different set of reasons—did not contribute as much to the discussions as expected by the lay participants. The lay people interpreted the young researchers' silence with regard to the animal killing issue as indifference towards the suffering of the animals. While both the researcher creating the knockout mice and the young researchers have to kill animals as a practical aspect of their work, only the latter were criticized for it:

> Either they did not care, or they maybe did not dare to say that they really have crises. But I do not believe that, because by and by all becomes routine. And they know in advance what they have to expect if they are working there, probably. They did not have to do it. Because I do not believe that one goes in there and one does not know what one has to do there. These people do this voluntarily. (L2/eP/27)[xcv]

While perceiving the young researchers in their way of dealing with the animal experimentation issue rather negatively, this lay also found the project heads "quite nice". Other lay people came to the opposite conclusion. They saw the young researchers as dependent on the benevolence of their supervisors, and therefore as having no choice whether or not to conduct animal experimentation. The young researchers were also seen as discussing animal ethics issues more than senior researchers did. However, those researchers who appeared the most "authentic" in the eyes of the lay participants were perceived in the most positive light. The question was why the young researchers who in part very openly discussed their emotional struggles and suffering with animal experimentation were not considered as trustworthy as the researcher who created the knockout mice. One explanation is that the long-lasting silence of the young researchers with regard to the animal issue was perceived either as indifference towards the suffering of the animals or as inability to publicly confess their unease while discussing this issue among themselves. On the other hand, when they finally revealed their struggles regarding animal experimentation, their unease with animal experimentation was perceived as so tremendous that the lay people asked themselves why they went into research in the first place, in the fashion of "they should have known when they got involved". Also, smooth ways of communication perceived as a façade hiding the speaker's "true" interests were negatively evaluated by the lay people, and led to personal mistrust towards these persons. The main objective for the lay participants when engaging with this issue was to find out "what is the motivation behind this", and they did not expect an answer: "If you are addressing the people personally, then they often have no answer, and probably there is not really an answer, is there" (L1/eP/43).[xcvi]

The way ethical questions about animal experimentation were addressed by both parties at the Round Table could be indicators of a shift in science-society relations. In the context of animal experimentation—and this was different in other contexts at the Round Table—the researchers emphasized that scientific work is not different from other societal domains, and that scientists are "normal" people. They "disenchanted" their own practice in order to depict it as a cultural endeavor having the same legitimacy as, for instance, the killing of animals for food. The lay participants, on the other side, evaluated research by asking for inner motivations and emotions with regard to animal experimentation. They were less interested in rational explanations, and more interested in personal accounts.

Discussion: Beyond facts—From the authority of knowledge to the authenticity of knowledge-producers

For the lay participants, the case of animal experimentation was a means to make sense of the work of the participating researchers in particular, but also of genomics and science in general. The mice were concrete objects for the lay participants in the rather complex field of genomics, which was regarded as opaque and missing many material references present in other technologies. As one of the researchers expressed this after the Round Tables: "DNA is too far away, isn't it? It is too abstract too. It is not really graspable. But the animal, you can see it, the mouse that squeaks there" (S4/eP/21).[xcvii] Therefore, the mice allowed for a down-to-earth discussion of ethics in genomics that was elsewhere regarded as too complex both in its epistemic content and in its entanglements with society. For the lay participants, too many players were involved in the game, and they were unable to identify a central actor "steering" the whole development—a wish sometimes expressed by them, although they believed that there could be no such central authority able to govern genomics. Their implicit assumption was that: if the researchers were able to conduct animal experimentation in accordance with common societal moral standards, then this might save scientific development from being corrupted and taking morally questionable trajectories.

The lay participants' problematization of the use and status of mice in genomics can be interpreted as an articulation of more general reservations about genome sciences and the way technosciences are governed in our society (Michael 2001). While particular concerns of the lay people regarding the researchers' treatment of mice quickly disappeared after the visit to the mice stables, the general reservations about genomics actually increased throughout the discussions at the Round Table. This raises some questions regarding stereotypical science policy expectations for techniques of public engagement, which often assume a linear development from less trust to more trust over the course of the engagement event. However, as the discussions at the Round Table show, public trust is not simply a question of more or less, but rather one of developing more complex and even ambivalent opinions. While science policy often demands univocal public acceptance of "science", the setting of the Round Table allowed for a more fine-grained understanding of "research" in its multiple facets (Latour 1998). Rather than re-enacting the myths of science, the Round Table contributed to the understanding of research as a deeply social enterprise.

An interesting observation about the debate on animal experimentation at the Round Table is that what could be seen as a relatively straightforward ethical question—the way animals are treated in the laboratory—was relatively soon redefined as a question of mutual trust. The question "Is animal experimentation right or wrong?" while still present to a certain degree, was also transformed to the question "On what basis can we trust researchers to treat animals appropriately?" posed by the lay participants, and "How can we as researchers demonstrate that we are trustworthy?" posed by the researchers.

This points to a transformation process of science and society relations observed by Nik Brown and Mike Michael (2002), who assume a shift from science enacting authority to a perceived need for researchers to perform "authenticity". The crisis of scientific authority has provoked a series of responses, such as democratization, public participation, science communication, transparency, the inclusion of ethics and the performance of authenticity. While the classic image of scientists stressed their emotional detachment (LaFollette 1990), the changed conditions of science in society seem to demand a more emotional repertoire from scientists. "Authenticity" of scientists includes the requirement that scientists should feel and act like normal persons to be considered as trustworthy. The rationale behind this is that if they are like "normal citizens" they will not act contrary to the social and moral norms shared by "normal citizens". This societal discourse was also reflected by the researchers at the Round Table, as they in many contexts wanted to prove "that we are totally normal humans" (S6/eA/694).[xcviii] A positive perception of particular researchers was closely linked to the willingness to reveal very personal emotions with regard to animal experimentation. The ability to communicate "authenticity" was a token of the researchers' trustworthiness.

The question is on what basis science is evaluated in the public sphere, where neither the social and epistemic authority of experts nor the authenticity of researchers when communicating with the public is the ultimate answer. At the Round Table, what was important were "gestures of transparency" that gave the lay participants the impression that they were able—in principle and at least on a small scale—to see through science and thus get some hint where the whole bandwagon might go. As such, the discussion of animal experimentation was crucial for both researchers and lay people. However, the transition from authority to authenticity remains a double-edged sword, as authenticity will become "staged" as rather empty gestures and a mere means to persuade the public.

9. The challenge of discussing "ethics": Discursive micro-politics at the Round Table[37]

In the prior empirical chapters, I analyzed the main ethical issues that emerged at the Round Table, focusing predominantly on their ethical content. This chapter focuses on the discursive processes and the "micro-politics" of the discussions—that is, *how* did the participants discuss ethics? To focus on this aspect of public engagement is relevant for a couple of reasons.

First, dominant evaluation exercises of public engagement and participatory settings (Rowe and Frewer 2000, 2004; Rowe, Marsh, and Frewer 2004) widely neglect the procedural and performative character of public engagement, and are not very sensitive to what happens *in* such settings between the different participants (Felt et al. 2009; Harvey 2008). They concentrate predominantly on the outcomes of public engagement as one measure of its effectiveness. However, the "outcome", in terms of what people think about the setting itself and its contextualization within a wider moral, social, political, cultural, economic and technoscientific environment, is strongly influenced by the particular discursive actions in the public engagement setting and the way they change over time. Therefore, to understand what public engagement does and to contextualize its "outcomes", a more detailed view of the actual processes within is needed.

Second, participation and engagement settings are not "white rooms" in which different pre-existing positions, values and arguments are reported and mutually acknowledged, but actively framed fora in which positions are developed and negotiated actively between the participants. The positions that emerge in such settings are much more than the sum of the different values each participant holds for her- or himself. Hence, the "nature" of a public engagement setting is to a great extent defined by the actual micro-politics of the event.

Third, the larger context of public ethical debates and the presumptions of professional ethics often suggest idealized visions of how ethical debates should be led. In particular, among the discursive norms of professional ethics is the idea that the best argument should prevail in the end and result in a consensual decision about how to deal with morally controversial issues. This idea is accompanied by the assumption that experts lead a rational discourse while the wider public engages with moral issues in a way that is too emotional. As Bogner and Menz (2005) have shown for the negotiations in the *Bioethikkommission beim Bundeskanzleramt*, the normative idea of a rational discourse is not even realized in an expert committee,

---

[37] The main argument of this chapter has been made earlier by my colleagues Ulrike Felt, Maximilian Fochler, Annina Müller and me in the article *Unruly ethics: On the difficulties of a bottom-up approach to ethics in the field of genomics*,written in 2006, and now forthcoming in *Public Understanding of Science* (Felt et al. 2009). See also footnote 1.

while the members of this committee simultaneously continue to demand a more “rational” and “informed” discourse in the public. However, “politics” cannot be bypassed by demanding a more “rational” discourse. Thus, it is important to shed some light on discursive games and micro-political processes, as they are an integral part of an ethical debate—and not its enemies.

In the following chapter, I will describe two forms of micro-politics that framed the discourses on ethical issues at the Round Table. I will start with what I call the *politics of labeling*, that is, how certain arguments are signified and thus placed into a certain context, which defines their relevance and the validity with regard to ethical questions and thus can serve to disqualify arguments as well as to sanitize certain statements. The second form of politics I will analyze here is the *politics of delegation,* by elaborating on the question of who is and who should be concerned with ethical questions. Before going into the details of the two forms of micro-politics, I will elaborate on the question of what requirements the participants regarded as necessary to participate in an ethical discussion in the first place and be taken seriously: Here, “expertise” was a central dimension.

9.1. How to be taken seriously in an ethical discussion? Knowledge, ignorance and the legitimacy of "public" participation

As we decided in our research project to accompany a real scientific research project within a dedicated genomics program, the composition of the researchers' group was already given. Thus, we had researchers who to some degree knew each other. On the other hand, the lay participants did not know each other in the beginning. This fact provided the researchers a head start in terms of the social structure of the setting, as they could start as a given "imaged community" (Anderson 1983). The lay participants, on the other hand, first had to establish themselves as a more coherent group through the assessment of their common interests. The experience of sharing common interests was a crucial resource for them to see themselves as a more coherent group. In this regard, being allowed to enter the mice stables was pivotal (see chapter 8.4). The initial reluctance of the researchers to grant access to the animal housings constituted them as a collective with a common agenda, and also created more explicit differences between lay participants and researchers. The lay's self-perception as a group of increased the longer the discussion process lasted, and in the end many of the lay participants referred to the group as "we".

The researchers perceived themselves as part of an epistemic community (Haas 1992) already before the Round Tables started. Thus, they shared rather stable common assumptions of what science in general is, how research practices work and what relations to society are like. The enculturation process of science education provided them a series of narratives as resources for the discussions with the lay participants. Thus, they could more easily employ *ready-made narratives* in the engagement process. They could easily access these narratives in the discussions and provide responses to all kind of questions of the lay participants. The lay participants, however, did not have such common narratives that allowed them to instantly respond to or even challenge the researchers' assumptions. They first had to work them out in a lengthy process, so that they were only gradually able to form a more coherent collective with a common agenda. The lay participants' dominant vision of responsibility is a good example of this learning process (chapter 8.1).

Finally, the lay participants not only had to accommodate each other and develop shared narrative resources, they also had to get used to the public engagement setting itself. Many of the lay participants were not trained to argue in a larger group. The researchers seemed, compared to the lay people, better equipped to engage in a discussion; however, even among them there were huge differences between the younger ones and the senior researchers.

Given these differences, the question was *how could the participants be taken seriously in the discussion?* This question came up especially for the lay participants, both implicitly and explicitly. The researchers assumed that they would be taken seriously in the discussion, as it was their research, their practice and their profession that was at stake. For the lay participants, the question of being taken seriously revolved around the question of expertise and the balancing of knowledge and ignorance. As such, the *performance of expertise* was often seen as a requisite for qualified participation in the discussion. Thus, the first two to three Round Table sessions were characterized by the lay people's frequent requests to clarify some factual information. This could be interpreted to mean that the lay participants did not see themselves as prepared to be qualified participants in a discussion. At this stage of the discussion, the lay participants also did not feel well acquainted with arguments on the basis of values. The lay participants' factual queries illustrated their wish to demonstrate expertise, as they seemingly saw this as the expected and proper feedback to the researchers' accounts. Demonstrating expertise took also place by *not* asking questions, as a silent "proof" that the statements of the researchers had been understood.

Thus, demonstrating technical expertise in the domain of the researchers was one discursive strategy employed by the lay people in order to be taken seriously and to be acknowledged as equal discussion partners. Moreover, expertise as such was regarded as an important legitimizing reason to participate; thus, the lay participants often performed expertise they held from their professional backgrounds. It was not so much relevant to have particular expertise in science, but rather to perform any kind of expertise.

The display of expertise was crucial especially in the beginning of the Round Table discussions. Gradually, there was a shift from performing technical expertise by requesting additional information to the promotion by the lay participants of their own values, and thus a shift to social and ethical expertise. Increasingly, and especially after they had been empowered by the participation of the ethicist as a guest at the fifth Round Table, the lay participants were more confident to argue on a moral basis. However, expertise remained a crucial factor for the lay participants to be able to contribute substantially to a debate, as they, for example, used the ethicist as a proxy for expressing their concerns, because he was seen as more able to challenge the researchers owing to his epistemic authority as an ethicist. While the appreciation of expertise was high during the initial phases of the Round Table and even later, in the Round Table devoted to the lay participants' reflection on the whole process, it was the lay's uptake of expert knowledge that was problematized by one of the participants:

> The danger—and what is interesting for me, or what one has to ask—if I look at it, is: Are we still lay people? Are we now spent to possibly take part in such a talk again? Are we still unbiased enough to take on such a task if there is a next question, a next project? … if we are more formed in our response behavior than a totally unselected, so to speak, real lay group? And that

> would also now be, so to speak, the question if I say, okay, with regard to many questions lay are supposed to participate, then I ask myself have we consumed the voluntary lay somewhere along the road and got professional lay? (L6/RT7/2/148)[xcix]

What is interesting in this quote is that this lay differentiates between “unbiased” and “consumed” lay. He sees only fresh lay as legitimate participants of public participation, while he and the other lay participants can already be regarded as “experts” disqualified from participation, and indicates a transformation process from facts to unbiased values as resources for legitimate public participation over the course of the Round Tables. This ambivalence regarding the characteristics of lay is also present in normative imaginations of lay publics in science policy. On one hand, the call for citizens to be educated and informed before they can legitimately take part in decision-making processes in science and technology has been widespread within the framework of PUS since the 1980s. However, there is a certain tension with perceptions of citizens, as Alan Irwin (2006) has noted for public engagement exercises in Britain (e.g., *GM Nation?).* Here, science policy is concerned with the “representativeness” of the citizens taking part in public engagement, and thus often prioritizes the “open-minded” and “innocent”, which means that the citizens should not have too much expertise in the issues discussed. In particular, certain forms of expertise are sought to be excluded—for example, representatives of self-help groups. In the lay people’s negotiations of expertise, two different rationales met: With regard to the micro-politics of engagement settings, expertise was regarded as a pivotal resource to be able to challenge the assumptions of researchers. However, with regard to the societal embedding of public engagement, lay people holding a great deal of expertise may disqualify themselves for participation, as they might already be biased in a certain direction.

9.2. The politics of labeling

Many features of the discussions of ethical issues were characterized by a *politics of labeling*. This term describes a discursive politics that aims to govern the discussion process by attributing the issue at stake to a certain societal domain or framework. This framework then defines the way the issue can be discussed, that is, who is concerned with the answering of the question, what elements are relevant to a “rational” discussion in this domain, what kind of “solutions” are regarded as reasoned, and so forth. Such politics were enacted at the Round Table by explicitly and implicitly invoking attributions and categorizations such as “ethical”, “scientific”, “economic” or “social”, and hence introducing a certain set of potential values and a notion of who could hold the expertise to be able to participate in the discussion. Thus, labeling both opened up and limited possible negotiations of what was at stake. On one hand, the politics of labeling made certain issues negotiable at the Round Table; on the other hand, it often led to a closure of ethical debates, as it shifted the responsibility for discussion elsewhere.

Ethics and morality

One of the main characteristics of the "ethical" discussion at the Round Table was the uncertainty of all involved participants if a certain discussion could be defined explicitly as an ethical one. This was because the participants went into the discussion without a well-elaborated idea of what "ethics" was exactly, as well as because "ethics" was mostly assigned to the domain of professional experts in that field. This has to do with the dominant way ethics is performed in society, which promotes the notion that ethics demands sophisticated expert knowledge as well as certain institutional conditions in order to successfully participate in a societal debate. Thus, the participants at the Round Table often performed a self-exclusion from ethics by not regarding themselves as able to contribute to this debate in a "legitimate" way.

At the Round Table, the participants often avoided labeling their arguments explicitly as ethical ones, but felt more comfortable when discussing values. The difficulties the participants had in terming something ethics resonate with my own difficulties in analyzing the ethical discussions at the Round Table (see chapter 1.3). Ethics is an "empty signifier" (Laclau 1996) that provides a basis for a global project that is held together by the signifier itself. Ethics as an empty signifier is a hybrid of a particularity (professional ethics has rather clear definitions of what ethics is and what it is not) and a universality (the societal proliferation of ethics, which multiplies the meanings of ethics).[38] Thus, the uncertainties regarding ethics cannot be attributed to the participants' ignorance or to a methodological weakness of this dissertation. Rather, it is a societal phenomenon that needs to be explained.

At the Round Table, a discussion of the "values" promoted by science in contrast to values that (should) prevail in society set in very early in the discussion process. The participants hesitated to label their own way of discussing morality and values "ethics". This changed, however, during the fifth Round Table, where the ethicist took part as a guest. In his introductory statement, he explicitly referred to the prior discussions of the participants: "I also already discovered the first traces of an ethical problem awareness in the protocols. That means one seems to have the vague feeling that there might be something that could be ethically worthy of discussion" (E/RT5/1/37).[c] In the following, he redefined the participants' discussions as ethical ones. Thus, the participants were now able regard their way of arguing as "ethical". This was expressed also by the sheer number of times "ethics" was said by the participants at this Round Table. Thus, empowered by the presence of the ethicist, all participants felt more confident in explicitly labeling their talk ethical. However, this also demonstrated that ethics was still seen as an expert domain, as the participants needed an expert whose "authority" could transfer the legitimacy to speak ethically to the participants.

---

[38] Wullweber (2008) uses the concept of the empty signifier for nanotechnology.

What “ethics” was mostly remained undefined throughout the discussions. The participants saw ethics as something that was handled by specific experts in society, and thus distanced from their own values. The difficulties concerned the content of the discussions, where most of the participants were unable to elaborate more specifically what counts as an ethical issue. Ethics was thus rather understood as a particular form of discourse. Many of the lay participants claimed at the introductory round at the first Round Table that they participated in order to discuss ethics. This claim can be read as a political statement to the researchers, aiming at a proclamation that “other” aspects than scientific ones should play the lead part in the discussions. Hence, ethics worked as the “empty signifier” around which all discussions revolved. No one dared to fill the empty space with more concrete content, as there was too much unease about the exact nature of ethics and its institutional language as an expert domain. Thus, the participants instead relied on a discussion of a diverse set of “values”, with which they felt more comfortable.

Basic and applied research

Another important facet of the politics of labeling was much more explicit: the distinction between basic research and applied research or application, which was evident in particular in the narratives of the researchers.[39] The main feature of the researchers’ definition of basic research was its distance from application. “Basic research” meant the production of scientific knowledge for the purpose of the advancement of knowledge itself. Often its unpredictability was emphasized (epistemological definition). Regarding the intentional dimension, the researchers cited curiosity as the main driver for going into research. Regarding the institutional definition of basic research, the researchers emphasized the ideal of academic research carried out at universities; for example, one researcher stated, “Universities of applied sciences do not conduct [basic] research in my eyes” (S8/RT3/2/146).[ci] Disclosure norms were also of high relevance, for the researchers expressed a duty to disclose all outcomes to the community and the public. Further, “basic research” was assigned to a specific place in knowledge production, that is, the laboratory. While the researchers did not refer explicitly to particular disciplines that counted as “basic”, the presence of a laboratory as well as the method of experiments was a crucial criterion for being “basic research”. On one hand, this was seen as rooted in the democratic principle of the “freedom of research”. On the other hand, researching without an application in mind also provided a pool of ideas that were seen as preconditions for “surprising” new technologies. The researchers often denied participation in applied forms of technologies, because such a way of knowledge production was seen as irreconcilable with their understanding of research that was clearly based in scientific norms (Merton 1973).

---

[39] For the use of this distinction in science policy see Calvert (2004), who identified six different ways to define basic research: epistemological definitions, intentional definitions, distance from applications, institutional definitions, disclosure norms, and substantial definitions (scientific fields).

As a consequence, and by introducing the labels “basic” and “applied”, the researchers introduced two different forms of ethics. Basic research was mostly characterized by an absence of ethics that addresses and includes relations between science and society. The only viable form of ethical thinking in basic research was a scientific ethos concerning the means of knowledge production and the internal organization of science. For the “applied” side, a different version of ethics was applied. This time, society alone was concerned with ethics. Symptomatic of the politics of labeling research as “basic” or “applied” was the reaction of a researcher to a statement of the invited ethicist, where he posed a series of fundamental questions showing that any science contains value assumptions and thus is ethically relevant:

> I do not understand that at all … that research practically entails moral arguments to receive money. It does not do that. We are writing purely scientific proposals to receive money, and there is not a single ethical argument, there is not a single moral argument … I believe this entanglement between research … [and] application: Our research is absolutely not application-oriented, and possibly it annoyed me a bit that the entanglement is rather present … but that is not our research. (S7/RT5/1/61)[cii]

What this quote demonstrates is how the assignment of different labels—basic and applied—worked as a delimitation of an ethical discussion “in here” and “now” because the researchers did not consider themselves concerned with ethics. The ethical relevance was ascribed only to a different kind of research (which I will address below under the heading of the politics of delegation, chapter 9.3). While other elements of the politics of labeling supported the idea that ethics could be discussed at the Round Table, this particular one mostly served to avoid ethical debates, as it displaced ethical questions elsewhere. However, this strict separation of “basic’” and “applied” also prompted lay participants’ reactions that hinted at the ethically problematic nature of such a distinction:

> I find it rather difficult just … to justify basic research with the pure purpose of satisfying the human thirst for knowledge. So, if I say “We are doing it, we want more knowledge, that is a kind of basic need”, then I find it personally rather difficult to justify killing animals for this purpose. (L13/RT5/4/313)[ciii]

The lay participant notes that, if “basic” research does not consider itself to share societal moral standards and thus ethical arguments, it also must not violate societal moral ideas through its production of knowledge. As soon as it does, different goods (new knowledge and the killing and suffering of animals) have to be balanced.

Hence, the introduction of the distinction between basic and applied research predominantly inhibits the process of opening up the issue of research as a question of ethics. By referring to this distinction, the researchers could too easily bypass critical lay questions, in particular with regard to the future consequences their research might have in society.

Hierarchies between "facts" and "values"

Another game of labeling concerned the attribution of the terms "fact" and "value" to certain strands of the discussion. Referring to "facts"—be they scientific or societal facts—was one of the central resources for framing the discussions. The Round Table therefore became a space where "values" met "facts" in many different constellations. To possess the "right" facts about a specific topic allowed the discussion to be guided in a specific direction, or often even to be closed. An ethical problem put on the table was quite frequently "solved" by introducing the "right" information. By mobilizing facts, a problem could be reframed in such a way that ethically motivated doubts had no place anymore. Thus, labeling something a "fact" was a flexible resource for the researchers in order to pursue their interests.

Much more important than the lay-expert hierarchy that was performed through this politics of facts, another hierarchy established itself: the hierarchy between values and facts. That arguing on the basis of facts was ranked higher than arguing on the basis of values was a common assumption that both lay participants and researchers shared. At the Round Table, the fact-value hierarchy was to a certain degree "naturalized" and remained unquestioned. There was implicit agreement that facts speak for themselves and that the right kind of knowledge overrules value-based ethical objections.

This evidently also had an important impact on identifying what an ethical issue was and on whether it could be discussed. Here's an example from the Round Table: The lay participants wondered what makes research worth funding and what criteria are drawn upon in making such decisions: "Does one rather consider insights and knowledge, or is it economic interests that are in the foreground? Does the end justify the means?" (L2/RT5/3/131).[civ] Comparing research on malaria and lipid disorders was thought to demonstrate that economic interests played a crucial role in defining public research agendas. Thus, malaria research was assumed to be neglected because "there is not so much potential and not so many sick people, of course" (ibid). In the eyes of the public, this was clearly an ethical question, because they asked for the researchers how they would balance economic interests and social justice. One researcher responded:

> Unfortunately, that is not true. There is a huge EU project at the Technical University. They are working only on malaria. This [project] has, I think, 8 million Euros. But … it is not done for the people who are there [in nations affected with malaria] but of course for the tourists. Because their numbers are high enough, so it pays off for the EU to fund this. [They] receive twice as much money for malaria as we do for lipid research. (S6/RT5/3/133)[cv]

Stressing that the lay participants built their argument on the wrong facts, while at the same time possessing the right ones, makes it possible to close the ethical argument. Interestingly, the researcher was confirming the moral argument the layperson made—that research money is apparently granted according to economic relevance—but the lay participant felt that she was discredited by the "fact" the researcher provided. This simultaneously devalued her moral argument, so that in the following discussion she was unable to provide other evidence, and thus the debate on this issue was closed rather quickly. By mobilizing their professional knowledge, the researchers upheld hierarchies and promoted a model in which, if people have the "right" knowledge, certain ethical questions are not at stake anymore. As this example shows, there was a general implicit assumption even by the lay participants of the necessity of using "the right facts", even when building on abstract value-based arguments.

The next example demonstrates much more explicitly the deployment of "facts" as a way to close a debate on moral positions. I exemplify this with a discussion of whether obesity was to be regarded as a medical condition or if—as the laypeople often argued—it could also be addressed in a broader, psychosocial way or by lifestyle changes. The researchers conceptualized obesity as a purely medical problem linked with an assumed societal desire for being slim as well as with the "fact" that diets and changes in lifestyle rarely lead to the desired effects. Thus, "It does not work!" was a recurrent claim made by different researchers. This line of argument rendered alternatives to genome research hardly credible. One layperson questioned this understanding of obesity as a societal problem: "and there is the saying 'The fat are jovial.' … They do not want to lose weight at all" (L4/RT5/2/379).[cvi] Here, the researchers were able to provide the right "facts" in order to translate a value-based problem into a scientific one by introducing two sets of knowledge. On one hand, the researcher acted as a "lay sociologist" by claiming social "facts", for example, the apparent need of people to become slim. On the other hand, the researcher referred to scientific "facts" in order to transfer the lay participant's argument of obesity as a matter of personal values into the domain of "nature" and thus of "non-choice": "Excuse me, but obesity is not, is not only a psychological problem. It is not about whether someone is happy and fat. It is unhealthy even if one is happy with it" (S6/RT5/2/416).[cvii] As this explanation still turned out to be too vague and too unconvincing, she introduced medical "facts" to the discussion:

> Fact is that 50% of people have too many kilos—that is fact. Whether a particular person thinks she may be too fat or if she really is, that is an individual problem. Fact is, half of people would live healthier if they would reduce their weight. And 20% have to reduce massively. That is fact, and whether they are psychologically happy with it or not, that makes no difference in obesity being unhealthy. (S6/RT5/2/449)[cviii]

After this statement, the discussion on this ethical issue stopped instantly, and the debate shifted to another topic. In this debate, the researchers successfully managed to rule out value-oriented dimensions of obesity provided by the laypeople by redefining the framework in which obesity was seen to be a problem. The researchers prioritized a biomedical notion of "health" over "happiness" as a common value for what it means to lead a good life. By employing fact talk, the researchers managed to exclude the value talk of the lay participants; however, the value assumptions of the researchers—health as an uncontested societal goal—

were carried and masked by the enrolment of "facts". On the other hand, as the example of malaria research showed, the lay people also connected moral claims with what counted as "facts" in their eyes. This demonstrates the entanglement of facts and values in arguments, in which they must be carefully balanced and managed. The researchers seemed more successful at playing this game.

Private and public ethics

Another micropolitical mechanism that shaped the debate on ethical issues at the Round Table was the distinction between moral positions that were regarded as "private" and those that were made "public". This was mainly the case with regard to issues that were regarded as extraordinarily sensitive, such as the case of animal experimentation. Here, two different ways of dealing with private and public ethics came to the fore: first, the implicit absence of certain ethical positions in the more "public" plenary discussions coupled with the articulation of the ethical matter in other spaces that were regarded as more "private"; and second, the explicit refusal to discuss certain issues in "public" as they were considered personal matters.

The first set of micro-political practices focuses on the non-said or on "absent presences" (Law 2004). The latter term stresses that frequently ethical issues seemed to be on the participants' minds when discussing at the Round Table, but, for a complex set of reasons, they did not explicitly address them. Absent presence was made evident by the way discussions in the peer group meetings (laypeople or researchers)—where potentially controversial ethical issues were discussed—did or did not make it into the plenary. The peer groups were perceived as sufficiently socially robust to allow rather delicate issues to be openly addressed. Being among a collective that seemingly shared the same values allowed participants to consider the space more "private", and thus to address ethically sensitive matters. The lay participants and researchers developed an implicit understanding of what issues from the peer group should go into the plenary. While it is not astonishing that the researchers developed this capacity, as they were used to perceiving themselves as a group that shared the same values, interests and epistemology, it is remarkable how this took place for the lay participants as well. They seemed to constitute an "imagined" (Anderson 1983) and "epistemic community" (Haas 1992) of non-scientists, a perception strong enough to allow for quite strong internal openness. The plenary, on the contrary, was seen by both groups as a much more socially fragile setting in which people preferred not to address certain ethical questions in order not to endanger the mutual relationship.

To give an example, in the peer group discussion of ethics, the laypeople identified animal experimentation as a crucial ethical topic. In this setting, some of them had rather distinct positions; for example, one layperson stated that he was against animal experimentation in general. When presenting the outcomes of this discussion in the plenary, the issue of animal experimentation remained unarticulated. While the lay participants never explicitly discussed and agreed to self-silence on this issue, they had implicitly decided not to share their position as they expressed it in the peer group. The interviews with the participants after the Round Table meetings supported this, as one layperson argued, "[in the plenary] there were however two fronts so to speak, I would say, two opposing parties I call it for now. And this inhibition was overcome when they [the researchers] were not here anymore. There was a more casual and more direct talking to each other" (L4/eP/67).[cix]

Not only the laypeople but also the researchers addressed issues differently in the peer group. In the researchers' peer group there was a debate on the status of animals in society. As mentioned in an earlier chapter (8.3), they identified a paradox in the treatment of animals: Society does not see the need for ethical discussions when killing animals for food, but does see the need for ethical discussions about animal experiments in research. As one researcher put it: "There were two among [the lay] who say, yes, they don't eat meat … because they have pity on animals. But they are consistent at least. I think someone who eats meat is not allowed to be upset with animal experiments that are done according to the highest standards. And this I think is an important argument concerning animal experiments, not only if but also how" (S6/RT5/sgS/427).[cx] However, this argument was not made explicit in that way by the researchers in the plenary discussions, but much more cautiously, in order not to endanger the setting perceived as rather fragile.

Besides the absent presences of certain ethical issues as a mechanism that shaped the way ethics was discussed in our engagement setting, a second, more explicit differentiation between public and private ethics was at work. In the discussions of animal experimentation, some researchers aimed to explicitly displace ethical questions from the public into the private. This move was accompanied by a shifting of the questions from a more collective level to an individual one. As described in chapter 8.3, on the construction of individual and collective identities, the project management aimed to set up a collective norm for the treatment of mice in the laboratory that said that, in principle, every member of the group must be ready to kill animals for experiments, in order not to create morally good and bad individuals. A young researcher responded to this account by re-labeling the issue from a collective norm to a matter of private attitude: "So, I do research with them … and I vaccinate them too, but I certainly kill no mice. So, that [is] actually *a personal attitude* of mine" (S5/RT5/4/88; emphasis added).[cxi] Thus, the issue of animal ethics was regarded as a very private matter that could not be discussed on the level of collective norms. The perception of (animal) ethics as an extremely private matter was further emphasized by the fact that it took until the fifth Round Table before the young researchers were able to articulate their concerns. The personalization of ethics was not only present in relation to the lay participants. Even with other researchers such a discussion was difficult, as the following quote exemplifies:

> S6: Would you like to ask for a discussion [about animal experimentation] with the project coordinators? So strange this now [sounds] …
>
> …

> S5: I believe it is always easier if you talk directly with someone who is in the same context, for example, if someone is at the same department and doing the same thing, so that you discuss directly on site. (RT5/4/359-371)[cxii]

The lay participants and the researchers shared the assumption that there is a space of ethics that is reserved for a more "private" dealing with ethical questions. It is important, for public engagement settings, to reflect about how different spaces are perceived and whether they are understood as "private" or "public," and to find ways to organize a translation between these spaces. This micro-political mechanism, too, is an ambivalent one. On one hand, it often impedes a more open discussion, especially on matters that are regarded as emotionally sensitive. On the other hand, "private" spaces for ethics allow for a discussion that otherwise would remain unarticulated. Such a difference of ethics in practice is thus relevant for the way ethics is discussed in the public sphere. There, virtually every ethical issue is situated on the same, mostly rationalistic, level, and thus the existence of "private" spaces, whose functioning is highly dependent on their distance from unemotional ways of debating ethics, is often neglected.

Inflation and deflation of ethical arguments

The last micro-political mechanism shaping the ethical debate at the Round Table that I want to discuss here is somewhat different from the politics of labeling analyzed above. Here, no different labels were attached to discussion strands, but rather the argument at hand was either inflated—that is, extremely generalized—or deflated—that is, broken down to anecdotal evidence. Thus, this mechanism consisted of switching between levels of abstractness and concreteness with the aim of either supporting the assumptions of the speaker or devaluating those of other participants.

The first example concerns the inflation of an ethical argument. In a discussion with the invited ethicist, a post-doc repeatedly insisted on the difference between basic research and applied forms of knowledge production, with the former free of ethical concerns and motives. The ethicist responded:

> E: That [the distinction between basic research and application] is only acceptable for society … that it is financed if society has the opportunity to decide how to deal with research.
>
> S7: No, to deal with the application, not to deal with research.
>
> …
>
> E: And there society must have the ability to decide … otherwise we dismiss any idea of democracy.
>
> S7: No, no, then we dismiss the idea of the freedom of research. And then I need no, and then I could close down the university in a moment. (RT5/2/140-150)[cxiii]

Here, the ethicist tries to challenge the assumption of the researcher by arguing that society should participate in making decisions about how to deal with basic research. Then he inflated the argument, saying that if this not the case we will abandon democracy. The researcher, however, countered with the same strategy by saying that if society decided on basic research we would abandon "freedom of research". After this short dispute, the discussion did not continue on the issue of whether and how society can and should participate in basic research, but switched to another topic. Both ethicist and researcher used an inflation strategy in order to prevail in the discussion. But this discursive strategy led to "impossible choices" (either basic research or democracy; either societal participation in basic research or freedom of research) that only work as rhetorical devices but do not pose "real" choices.

Second, arguments made on a more general level were often "deflated" by providing anecdotal evidence in order to "prove" that the more general argument was wrong. The example comes again from the fifth Round Table on ethical issues of genomics. After peer group discussions, the lay participants summed up their deliberations on the issue of the responsibility of science and with regard to the consequences of scientific knowledge. Their argument was that in principle everyone, including the researchers, should take on responsibility (see also chapter 8.2). The lay participants suggested thinking about scientific responsibility in a reflexive way to handle the moral uncertainties of innovation processes. In her response, a researcher framed the question quite differently. Her argument was that an assessment of future technologies could not work in general, but could only apply to a few exceptions: "That only will work out if I invent the slingshot. Then it is foreseeable that somebody's head will be hit by a stone and he will then die" (S6/RT5/2/25).[cxiv] Her argument was that technology assessment was only possible in cases of applied research and technologies. For basic research, it is different:

> Mr. Pasteur, when he was discovering penicillin—discovering by chance—did not know that multi-resistances would emerge … That means the assessment is difficult, in the same way as assessing positive applications … the laser was not invented so that I could have a laser beamer … and scan my eyes or whatever. … These are applications that were totally unforeseeable. That was not invented because of an application; that was actually a totally different product. … Per se, I can assess then what all I could have done with it: That, I believe, is asking too much. (S6/RT5/2/25)[cxv]

This researcher breaks down the lay's argument for wider societal reflection to anecdotal evidence, in order to "prove" that the argument does not apply to the kind of research the participating researchers conduct. This was typically accompanied by an individualization of the assessment process. It was impossible for Pasteur himself to assess the entire nature of his discoveries. She then inflated the anecdotal "proof" again to a rather general level of argumentation:

> if I want to be on the safe side that nobody is misusing it—my technology—then I have to say we should stop at the current state of technology … and we

> prohibit research and development in general. Then I can be sure that there will be no negative effect emerging from a research outcome. Otherwise I cannot prevent it. That is the only possibility to make sure that nothing bad emerges. … nothing beneficial emerges either, but nothing new and bad will emerge. (S6/RT5/2/29)[cxvi]

The evidence drawn from the stories of Pasteur and the laser, and the re-introduction of the reframed argument on a general level, made it impossible for the lay participants to uphold their argument, which was made on a more general, this is, institutional level. Thus, switching between different levels of argumentation turned out to be a prominent rhetorical feature of the discussions at the Round Table. This micro-political mechanism worked to impede a continued ethical debate, as it often led to the closure of ethical questions.

9.3. The politics of delegation

The second set of micro-political strategies I discuss in this chapter is the politics of delegation. This refers to the attempts by participants to shift expertise about what was to be regarded as an ethical issue, as well as responsibility, to somebody else. Thus, ethics was delegated elsewhere, with the effect that ethical issues were rendered non-negotiable in the setting of the Round Table itself. The politics of delegation was generally characterized by a model of the division of labor between science and society, wherein science takes on specific tasks, especially the production of new knowledge, and society is concerned with handling the ethical repercussions of scientific knowledge. The ethical work was partly assigned to experts within science; however, this task was attributed to "soft" sciences, while the task of the natural sciences was seen solely in the production of facts.

This politics of delegation is rooted in rather classical notions of science and its norms (Merton 1973), and is in contrast to newer understandings of science-society relations that abstain from a strict separation of societal functions but rather emphasize science's entanglement with society (Gibbons 1994; Gibbons et al. 1994; Nowotny, Scott, and Gibbons 2001). The lay participants clearly preferred this later idea of a shared responsibility between science and society. However, they too subscribed to the division of labor model with regard to ethics. Their rationale was not a Mertonian notion of science, but rather a certain degree of distrust of the self-regulation abilities of science and their assumed failure to constructively contribute to a reflexive dealing with ethical questions.

In the following I will discuss the mechanisms of *social delegation of ethics* (to ethical experts, to law and regulation, and to "negative others") and of *temporal delegation of ethics* (shifting ethical questions "downstream" or "upstream"). These two dimensions of the micro-politics of delegation are not mutually exclusive, but often appear closely intertwined.

Social delegations of ethics: Experts

According to the division of labor model to which many of the participants subscribed, the task of discussing and deciding on ethical questions was often delegated to professionals ethicists. Throughout the discussions, all participants argued that experts—that is, both people and institutions seen as holding epistemic authority over ethics—were better equipped to deal with ethics. While researchers and lay participants often agreed on this, their reasons were quite different. The main rationale of the researchers was that the division of labor model supported their distinction between basic research and applications, which protected “basic research” as an autonomous space of action. Attributing ethics to specialists—and not embedding it in the daily practice of basic researchers—reinforced the boundary between basic and applied research. This was explicitly expressed in a small group discussion among the researchers:

> [The ethicist] should do that, he actually is an ethicist … because we are scientists in the field of molecular biology, and that’s why we do that. We are not expecting ethicists to do our work, and that’s why the ethicists should not expect us to do theirs. (S6/RT5/sgS/1)

The laypeople implicitly subscribed to this expert model too. While they called for a certain self-responsibility of each individual researcher, they also did not rely on the researchers to critically question and reflect upon their own practices. Thus, they stressed the necessity for more institutionalized forms of ethical reflection and responsibility. The reasons for establishing special institutions concerned with ethical questions, however, were not rooted in maintaining the boundary between basic and applied research, but rather in its critique. The researchers were regarded as unable to reflect “objectively”, as the lay people did not buy into the idea of basic research, but saw the work of the participating researchers as already too strongly entangled with economic and political interests. Thus, more independent institutions were seen as necessary to complement regulatory institutions: “I think that, foremost, artists and philosophers and ethicists are our control organs if the medical professionals and technicians do not reflect on themselves” (L2/RT7/1/556).[cxvii] However, this did not necessarily mean that the lay participants unconditionally trusted currently existing ethics bodies, as they were not regarded as “representative” enough to reflect common societal values. The reasons why the lay people would rather have delegated ethics to experts also lie in the fact that they were regarded as having the same authority as other scientists, and thus able to challenge the assumptions of the researchers.

Social delegations of ethics: Regulation and law

This micro-political discursive mechanism describes the translation of ethical question into legal questions as a way to displace ethics elsewhere, as the question is transformed from one to which everyone can respond to one that only legal experts and political decision-makers can address. It also shifts the responsibility for engagement from “in here” to society, as law is considered a domain that builds on a societal consensus. This mechanism was employed in various ways at the Round Table. The mechanism shifts the ability to act and to define norms from oneself to others who cannot be controlled, and thus responsibility for norms cannot be assumed.

One way of delegation to law was to refer to the “laws of science”. This frequently took place when ethical arguments from the lay participants questioned the research practices of the participating researchers. In what followed, they often referred to the norms and laws of science as an excuse for behaving the way they did. For example, it was argued that “what we research is worth being researched, researchers in the US have decided” (S6/RT5/2/306).[cxviii] Another frequent example of the unwritten laws of science was the peer-review and publication system, where only that research attracts sufficient attention that is published in “top journals”. These define the relevancies in a specific field. It was argued that science’s economy of attention defines norms of success and failure, and these norms are not open for interpretation by individual researchers if they aim to prevail in the field. The message of this was that the lay people were addressing the wrong actors with their concerns.

The second example of the delegation to law concerns the shifting of an ethical question to regulation in a classical sense. To highlight this I will use a discussion on animal experimentation. A layperson raised the question whether it is morally acceptable to kill animals in basic research where there is no explicit benefit but only the “pure purpose of satisfying the human thirst for knowledge” (L13). The reaction from the researchers was:

> S6: But you are not allowed to do that! There are regulations. … You don’t get a free ticket to do all animal experiments. Every time we, for example, want to make a certain knockout-mouse with a certain gene, we have to apply for, explain … what kind of function that gene has, and what our presumption is, and why we need that.
>
> L13: And according to which criteria is that decided then, whether that is ok or not?
>
> S6: That’s a good question, I have no idea. You have to ask the person from the ministry, he knows that. I don’t know, do you know? (RT5/4/315-319)[cxix]

In her answer the researcher argues that she does not need to deal with this ethical question, as legal permission is always required for mice experiments. It is regulated and thus regarded as unproblematic. What is legally permissible is equated with the assumption that there is no ethical problem and therefore no need for an extended ethical discussion—or at least that the lay people were addressing the wrong actors. This argument managed to end the ethical debate on the decision criteria for animal experiments almost instantaneously. It also exemplifies the readiness to reduce complex and multilayered ethical issues to regulatory problems. For the researchers, this reduction meant a simplification of their practice, which was also reflected in the researchers' demand to ethics to deliver clear-cut guidelines rather than opening up questions.

There is also a temporal aspect in this: The delegation of ethics to law suggests that society has already agreed upon certain rules and norms independently from the researchers. Once regulations are in place and research practices comply with them, the reopening of this "black-box of ethics" seems neither necessary nor useful.

Social delegations of ethics: "Negative others"

The creation of "negative others" (Birke, Arluke, and Michael 2007) was a common feature of the ethical discussion at the Round Table. "Negative others" are those actors and practices that do not comply with the moral standards articulated by the participants at the Round Table. As elaborated in a previous chapter (8.3), these "negative others" of the researchers may be located either outside, mostly in foreign countries, or even inside the research project in the form of those colleagues who conduct animal research. In creating "negative others", the ethical question is delegated to these actors, because the ethical critique does not apply to oneself but must be considered by those who do not meet ethical standards. The creation of "negative others" implies a rather flexible method of boundary work and identity construction, because who counts as "other" is dependent on the criteria that characterize the "otherness". For example, if one addresses science in general with a ethical concern, the "negative other" disappears within science and may be located in industry, for instance in the pharmaceutical industry. The "negative other" may be also part of the scientific enterprise, but situated in a foreign country that is regarded as less ethically advanced than Western science. The flexibility of the "otherness" thus provides insights in the contextualized constructions of "science", "good science" and "bad science", as well as more individual scientific identities.

At the Round Table, "negative others" often served to delegate ethical concerns elsewhere. As this discursive strategy was used quite often on a diverse set of issues, I will select some prominent examples from the Round Table discussions. A particular manifest "otherness" that served to deflect ethical concerns was the researchers' distinction between "basic research" and "applied research". This distinction was the main feature of the researchers' epistemic cosmology, and thus served to explain a wide range of matters. The researchers situated themselves on the "basic" side, which they regarded as the ethically safe side. Ethical consideration were assigned to those who are concerned with "applications". The basic-applied model acted in such a strong way that all application-oriented features—patenting, public justifications employing usefulness arguments—of the researchers' project could be ignored.

More concretely, the researchers depicted "foreign" research (in China or South America), other branches of science (human cloning, green biotechnology) and domains outside of basic science (industry research on pharmaceuticals) as "negative others", which allowed them to display their own practices in a rather positive light. In order to draw ethical boundaries (Wainwright et al. 2006) they provided examples of what they were not prepared to do, that is, conducting animal experiments for cosmetics and cloning humans. This qualified their personal practice with animal research, and thus delegated ethical concerns and the reflection on them to those who were involved in these amoral practices.

Temporal delegations: Shifting ethics downstream and upstream

Based on the assumption that there is a "right" moment for ethics, while in other moments ethical reflection does not apply, the researchers developed a particular model of when ethics should be discussed. Against the backdrop of the distinction between basic and applied science, the main argument developed in a number of statements was that ethical consequences of research would have to be treated either before or after the actual research process—understood as "basic". In other words, ethical questions were displaced or delegated "downstream" or "upstream" with respect to an innovation process imagined as linear (Godin 2006). Thus, the linearity of the innovation process provided an underlying structure allowing them to temporally shift ethics elsewhere. Temporal delegation also allows for affirming the importance of ethics while arguing that the right moment is not now: "Ethical considerations, yes, but not now!"

When explaining the aims of their (basic) research, the researchers employed a narrative of ordering research with the effect that ethical considerations were displaced "downstream". Following the statement of the laypeople that only the extensive promises of future applications would secure the generous funding of the field, one researcher argued:

> within our research aims we have classified it like this: we have direct aims, which are ours in the laboratory. Our direct aim is to identify genes and to clarify metabolic pathways ... Then there are indirect and long-term aims ... And the long-term aims would be to reduce obesity, to reduce arteriosclerosis, heart attacks, cancers and so forth. But these indirect and long-term aims are not our aims. These are only societal aims, which are realized by others. We do not do them ourselves. We cannot even do it. (S6/RT5/2/314)[cxx]

By introducing a distinction between "direct" and "indirect" or "long-term" aims, the researchers tried to establish a boundary between a space where ethics is relevant and a space where it is not. Following the logic of this distinction, ethical considerations were shifted downstream to those whose intention was to produce concrete applications that were related to societal health problems. The researchers argued that, even if their research was somehow driven by societal aims, these were not immediately linked to their present work. Somebody else would take these steps. Here, the particular model of the innovation process makes it possible for them to reach multiple aims: On one hand, basic research and applications are linked enough to allow for promising future benefits to legitimize present research. On the other hand, basic research and applications are distinct enough to displace reflection about the societal consequences to the application end of the innovation process. Thus, future and present are tied together in particular ways in order to end up on the ethically "safe side".

While a strict separation between basic research and social accountability would de-legitimate their work in terms of the public funding it receives, introducing two different sets of aims allows them to justify the funding of their research. Public funding in research is seen as an investment in the future by policy-makers. The model of direct and indirect aims makes it possible to simultaneously justify the spending of public money (indirect aims) and to preserve the autonomy of science in the form of basic research (direct aims). As the present knowledge is seen to enter society only in the future through the work of others, no need is perceived to discuss ethical concerns at present. Furthermore, as future applications remain unclear, facts necessary to make an ethical evaluation are seen as missing. Ethics is pushed downstream, to a moment when concrete applications can be discussed, and society is then responsible for considering the ethical trade-offs of the applications.

The temporal delegation was further underpinned by the individual researcher's inability to assess the full range of ethical problems his or her research might cause. The researchers often switched the level of argumentation; that is, transforming more general arguments into micro-narratives that mainly referred to small episodes, individual researchers, elements of history and anecdotal cases served to uphold the border between basic and applied research, as well as to demonstrate that upstream discussions of ethical problems were hardly possible. For example, during a discussion of the ethical aspects of the production of the fission bomb, where it was argued that a debate was necessary already during basic research, a researcher replied: "When is in time? … before Niels Bohr learned about the structure of atoms, is that in time? Or before the [Manhattan] Project started?" (S6/RT5/2/188).[cxxi] As the lay people demanded to paddle upstream, the researcher asked "Where do we have to stop? … Niels Bohr, the atom model … because we would not have been allowed to develop this? Or does it start with the Manhattan Project itself?" (ibid, 192).[cxxii] Finally, she rhetorically asked "Is Darwin responsible for euthanasia?" (ibid, 200).[cxxiii] Starting from the example of the figure of the individual researcher being unable to assess the full consequences of his or her research, this argument was extended to science and society as a whole. As neither the individual researcher nor society can have sufficient knowledge at any point in time, nobody can prevent unintended negative consequences of research. Consequently, the ethical debate could be closed by arguing that dealing with ethical consequences of research should happen further downstream—when concrete applications are foreseeable.

While ethical reasoning was often delegated to the future, the shifting of ethics to past decision-making also occurred at the Round Table. Downstream ethics closed ethical debates "now" because there would be plenty of time (and sufficient facts) available in the future. Shifting ethics "upstream" also led to a closing of the debate, because it was assumed that societal value questions had already been clarified in the past. The example from the Round Table takes up a discussion where the researchers legitimized their work by constructing research as a rather passive endeavor that only "responds" to societal demands. In that light, dedicated research programs were perceived as an "offer" where science only "jumps in" to "help out".

> We are not those who initiate these projects, but we are those who accept the offer, and, in order to somehow send this responsibility a bit away from us again … the society has to reflect then what we do with it. Society has, I believe, beforehand—under whatever circumstances—made up its mind and developed these projects. (S7/RT5/2/490)[cxxiv]

In the researchers' narrative, the responsibility for the social consequences is shifted away from science. It is society that has to reflect on ethical issues. The existence of dedicated research programs demonstrates for the researchers that society has already done so. Under what conditions this has been done, who participated in the decision making process, is not regarded as their concern.

This micro-political mechanism also reflects and comments on the way ethics is dealt with in society. In this wider context, the question of when we should discuss ethical issues is highly relevant and contested. A common feature of political ethics in the form of ethics committees is that they often react to facts released by science. In doing so, ethics aims to compensate for past value decisions (Levidow and Carr 1997). Ethical reasoning is conducted on the basis of those facts, while ethicists often refuse to act on "speculations" about the future because facts are currently not available. On the other hand, the implicit values and assumptions embedded in existing innovation regimes are barely opened up by institutionalized ethics, for example, posing the question whether we need this technology at all (Wilsdon and Willis 2004).

Both the politics of labeling and the politics of delegation reflected wider societal presumptions about the relations between science and society, as well as the role of ethics in society. This particularly became evident in the way the participants avoided the term "ethics", mirroring societal practices in which ethics is dominantly seen as an expert domain. Thus, at the Round Table, lay-expert hierarchies between the participants and the invited ethicist were re-performed. They were also performed through the division of labor model between science, society and ethics. Here, the re-activation of more traditional understandings of the role of science (and society)—the researchers' understanding of "basic research"—aimed to absorb the impositions and uncertainties of new modes of producing scientific knowledge in which science and society are much more intertwined. Thus, the researchers' aim was to preserve a safe space to guarantee the "autonomy" of research, which was seen as endangered. The most relevant issue concerns the temporal politics of ethics, that is, the question of when ethical issues of science and technologies should be discussed. Here, current innovation regimes that have already been “naturalized” in science policy place ethics nearly exclusively on the back-end side of innovation. In that sense, these discursive strategies and micro-political rhetorical means of labeling and delegation reflect society in a nutshell. Thus, the processes in public engagement settings, and the way positions are articulated and enforced, seem a rewarding field to observe science-society relations and the way they change over time as well as from one topic to the next.

10. The Round Table as a "learning process" on ethics

In this chapter, I will analyze the participants' *ex post* evaluations of the ethical discussions at the Round Table and investigate what the participants "learned" from this experience and to what extent it made a difference for them. Against the backdrop of the dominant evaluation schemes of public participation, where relatively little voice is given to the participants' own criteria of assessment, this is an important question. Furthermore, the effectiveness of public participation exercises is often assessed within the framework of a deficit model of public understanding of science—that is, did the laypersons receive the message as intended by the organizers? In these contexts, "learning" often assumes two particular notions: Is the public now "better informed" on technoscientific issues? And did the experts, scientists and science policy-makers learn to communicate their complex ideas effectively to the public? That is, public participation predominantly follows an educational paradigm that strongly discriminates between those who know and those who do not know. Many aspects of the evaluation of public participation can be questioned, however.

The notion of "learning" used in this dissertation seeks to go in a different direction. "Learning" is understood as a mutual process of interaction between all participants that may trigger changes in the participants' attitudes. Consequently, "learning" is not defined as an uptake of factual information, but rather as social learning of what it means to directly engage with researchers, and vice-versa with lay people. In such a process, different and situated value systems are negotiated that may lead to a transformed understanding of "science" as well as the "public". Here, the respective "body language" of the participants is considered together with the exchanged knowledge itself. Thus, by "learning" is meant the exchange of knowledge and experiences in their social dimensions that may result in changed attitudes towards the respective "other" and also towards the individual and collective self. Furthermore, the knowledge and experiences gained in the course of the engagement are not necessarily connected with a higher degree of certainty. While this may be the case with respect to certain aspects, uncertainties may also increase. Another important aspect of the learning process is that the setting itself must be included, as it is not a neutral means to communicate something else; the setting also communicates itself. In the sense of Marshall McLuhan's (1994) famous one-liner, one could say that the medium is the message too. That is, people are not only making sense of the content of the engagement setting—in our case genomics and science in general—but are also making sense of participation and engagement in relation to wider political contexts and their experiences with these contexts.

The main assumption that guides the analysis in this chapter is that what changed over the process of the Round Table was not so much the image of ethics itself, but rather the mutual perceptions of researchers / research and lay participants / "the public", as well as the societal and political context in which ethics was situated. These changes in mutual perceptions resulted in a learning process with regard to the image of ethics, as it was strongly connected to both the role of science and the role of the public. The importance of the mutual perceptions of lay participants and researchers as a basis for their image of ethics was highlighted by the participants themselves, for example when a lay noted, "I strongly aimed to understand what the individual motivation of the involved [researchers] is … who is actually involved in what happens there based on what the scientists do" (L13/eP/175).[cxxv] And for the researchers, "possibly the appraisal of the public has changed rather than the [appraisal of the] issue" (S6/eP/19).[cxxvi] Thus, ethics is not as an entity that can be discussed separately from other societal concerns, but one deeply entangled with visions of science and society.

In the following, I will discuss the main dimensions that describe the learning process at the Round Table with regard to ethics: changes in the mutual perceptions of lay people and researchers (10.1), the changing perceptions of ethics itself (10.2), the relations between ethics and expertise (10.3) and the question of who—after the experiences at the Round Table—should be concerned with ethics in society (10.4).

10.1. Mutual perceptions of researchers and citizens: Ethically disengaged researchers and critical publics to be convinced

How did the lay participants perceive the way the researchers addressed ethical issues during the Round Table? The dominant bottom line was that most laypeople noted that the researchers did not engage much with ethical questions in and of their work. They identified a set of different reasons for the researchers' non-engagement with ethical questions. Some lay argued that ethics was regarded as "other" to science: Ethics is not part of the core activity of science, and thus it requires additional efforts to pose ethical questions to oneself: "My impression was that it is however something rather rare, that is, that it is not an essential part of the scientific practice that one is occupied with these things. This impression came up in simply in talking with the scientists" (L13/eP/203).[cxxvii] Thus, the researchers developed strategies to avoid being confronted with ethical issues: "on ethical matters they have been a bit evasive, haven't they" (L2/eP/159).[cxxviii] The lay participants noted that the researchers perceived ethics as a kind of disturbance that could interfere with their work:

> What I took away for myself [from the discussions]—and this corresponds with other experiences [with] devoted scientists—is that … this idea of asking oneself this [ethical question] is felt as an interference, felt as time-consuming, power-consuming, and as counterproductive, because if I pose too many questions I no longer do things so easily. It is, I believe, a really essential point in ambitious research, that I do many things in the first place and then reflect afterwards. (L6/eP/152)[cxxix]

Ethics was seen by this and other lay as a critical reflection that—if consequently carried out—impedes scientific progress and collides with the requirements and constraints of the scientific system that allows the researchers to progress in their careers. Thus, while the lay participants' arguments expressed a great deal of criticism of the researchers, with the demand that they should personally take on more responsibility, they also expressed empathy and understanding for the researchers' positions:

> You have already heard it: they conduct research, I do not know for how long, and then eventually something comes out. This [means] of course many frustrations. … They go through many things. Patience, and then it goes up and down, and if someone then comes and says: "Well, morally please, and what are you doing there, think about it eventually what you are doing there, in what direction it might go, this could get out of hand and so and so". And then they have to start discussing … that is indeed prohibitive for the work. (L2/eP/183)[cxxx]

Drawing these statements together, the lay people's more general assumption was that to behave ethically and to behave according to the science system's rules for success and failure are mutually exclusive. A researcher can only decide for one side—to be ethical or to be successful in science. The lay people's allegation of the researchers' disengagement with ethics was supported by some statements of the researchers themselves, in which they, too, regarded ethics and science as mutually exclusive: "that is indeed the dilemma of science, isn't it, that we actually have not so much time to wait on ethical discussions with our work" (S4/eP/168).[cxxxi] This quote accounts for the multiple requirements researchers are confronted today. On many instances, the researchers emphasized that ethical reflection should be more central, however, ethics often conflicts with other requirements of science as a system.

However, there were huge differences in the way the ability for ethical reflection was ascribed to specific researchers in the accounts of the lay participants. Some of them saw engagement with ethics only by the project leaders, while the other researchers were "only doing their job". The assumption was that the "professors" had the time for ethical reflection, while the younger researchers were "reckless", as they only sought to pursue their career. This assumption was seen as enforced by hierarchical structures wherein younger researchers have fewer choices but need to do what's necessary. Only a minority of the lay participants saw a deficit of ethical reflection on the side of the "professors". The lay participants in the way ethics was discussed also felt the hierarchical structure of the group of researchers. One lay participant particularly observed an interesting difference in the way different researchers engaged with ethical questions:

> regarding the researchers, I noticed that there were hierarchical basic structures. ... if something went beyond their own field, the call for the ultimate boss was there, and he already anticipated this, and he talked in principle for all others. And actually concrete questions were only answered freely or directly by single low-ranked people if they were about concrete experiences. (L6/eP/128)[cxxxii]

The lay argues that the more established researchers were entitled to talk about "ethics in general", while the younger researchers could only provide statements on "ethics in particular".

While the lay participants criticized the researchers' disengagement with ethics during the Round Table, they also perceived that this way of dealing with ethical question did not change during the Round Table:

> I actually felt this always, that the opinions of the scientists from the different groups would not change. They expressed their opinions, exchanged [them] and that was it ... what opinion the public actually has, what I actually do, that, I believe that is secondary [for the researchers]. (L14/eP/187)[cxxxiii]

The lay argues that what the lay people argued in the discussions made no difference to the researchers. As the lay people perceived the Round Table as a setting for mutual engagement, they expected a certain readiness on the side of the researchers to take up the lay people's arguments. This evaluation of the lay people can be underpinned by the fact that in the *ex post* interviews the researchers barely mentioned the lay people's ethical arguments, but were much more interested in the discussion with the "real" ethicist, as well as in hearing the ethical viewpoints of their colleagues, especially related to the question of animal experimentation. This may mirror a learned asymmetry in science-society interactions rooted in traditional models of science communication, in which the lay are thought to be the listeners while the researchers talk. It shows that a unidirectional understanding of science communication is still present in the minds of the researchers.

While both researchers and laypeople pointed to a lack of ethical engagement on the side of the researchers, there was disagreement on the reasons and the implications. The lay participants surely expressed some understanding of the researchers' position, but also saw their statements as excuses in order to be able to continue work untroubled. The lay clearly called for more ethical reflection and individual responsibility by scientists. The researchers, too, in part identified the lack of engagement with ethics as a deficit; however, it was displaced to a question of education in the early career or ascribed to the constraints of the scientific systems. The ethical disengagement could also be explained by the difficulties of the researchers in relating wider ethical questions to their own practice, especially if they regarded their research as producing "basic knowledge" that is *per se* not able to do harm. Here, the difference between "ethics in general" and "ethics in particular" was opened up. "Ethics in general" included questions that fundamentally challenged the work of the researchers with regard to their aims and the kind of responsibility they might have for society. The discussion of "ethics in general" was left to the more experienced senior researchers, who were seen as better able to represent the ethics of science. The younger researchers, on the other hand, were seen as entitled to provide more personal accounts of "ethics in particular", that is, moral questions that occur in scientific practice.

Turning to the researchers' ideas on the public and ethics, the image of a potentially adversarial public to be encountered at the Round Table was constantly present in the statements of the researchers in the *ex ante* interviews. This was especially true for the researchers' expectations with regard to an anticipated debate on animal experimentation: "there you are partly strongly offended. Because that is indeed an issue that is not very appreciated in the population" (S1/eA/79).[cxxxiv] In particular, those who were directly involved in animal experimentation—the younger researchers—identified ethical discussions on animal experimentation as a controversial issue for the upcoming discussions. In their *ex post* accounts of the debates on animal ethics at the Round Table, they came to an ambivalent conclusion. While they thought that they had managed to persuade the public in the particular ethical question of animal experimentation, they saw themselves as having failed to establish the lay participants' wide-ranging trust of them as researchers.

In retrospect, the researchers claimed that they had managed to change the participants' ethical attitudes towards animal experimentation. They assumed that all participants were against animal experimentation before the discussion started, and that they were gradually convinced about the necessity of animal experimentation.

> And what was surprising to me in a certain way was that the approach changed before the people were in the animal housings and in the laboratories and afterwards. That is, there were still people then who said that they do not totally agree with this. But it was not like that anymore, I would say now by and large, it was not so definite anymore. (S7/eP/203)[cxxxv]

The change in the lay people's attitudes was reached by opening the doors of the laboratory and the mice stables, and thus, for the researchers, "transparency" and a rational way of argumentation were the main cornerstones of convincing the public of their aims and basic values. The *ex post* reflections, however, showed a rather asymmetrical engagement with ethical questions. In general, the researchers expressed less interest in the ethical positions of the laypeople than the lay participants did with regard to the researchers. Thus, the ethical views of the public on animal experimentation were one of the view exceptions. The researchers' aim was to persuade the public of their own values rather than to engage with the lay participants' ethical arguments. Consequently, they did not reflect in the *ex post* interviews on the ethical positions of the lay participants that had been extensively discussed at the Round Table, but evaluated what had happened against worst-case expectations that are much more present in wider societal and media debates. For example, a sub-project leader said that he was glad that no "Frankenstein" scenario was brought up at the Round Table.

This reflects the presence of public science stereotypes in the researchers' minds and the anticipation that these public images would be part of the Round Table debates. Thus, the researchers engaged with an imaged public and their assumed hypercritical attitudes rather than engaging with the arguments that were in fact brought up during the Round Table by the lay participants. The researchers evaluated the ethical arguments of the lay participants against ideal-typical images of scientific values they held, rather than openly engage with the ethical reasoning that really took place at the Round Table.

However, when looking at the lay participants' statements and reflections, a contrasting image emerges. At least in the *ex ante* interviews, there were no lay who argued fundamentally against animal experimentation. Most of them did not even mention animal experimentation as an issue. But the lay participants' attitudes towards animal experimentation changed as much as the researchers claimed. For the lay participants, the issue of animal experimentation was an issue of "transparency", "authenticity" and "trust" rather than a purely ethical one (see chapter 8.4).

Apart from the issue of animal experimentation, there was another area in which the researchers articulated interest in the ethical positions of the lay people. For one researcher who was more concerned with the managerial tasks of the research consortium, the interaction with the lay participants was of great relevance:

> what we often believe is that we communicate this very well, or that we can communicate this very well, that this is the right way .... And there is surely much potential for learning, that I have to admit. And second concerns the interaction: we often have this belief that if the arguments are good enough then everyone will share the opinion. That this opinion-making, however, runs so emotionally, that the arguments maybe come in the tenth place and that we cover this emotional aspect poorly, that we approach the whole thing very rationally because we have overall a rational approach and [we] do not pose this emotional question at all ... And there we have to—I am not yet sure how one can implement this and how one can learn this—but we should broaden our knowledge by all means. (S6/eP/27)[cxxxvi]

Based on her role in the research group, she reads the discussions with the lay people in a particular way; that is, she asks what can be learned from the interactions in order to communicate more effectively so that the public can be addressed in their own terms. Of course, this way of learning has a strong instrumental dimension, aiming to develop better ways to convince the public of the goals and benefits of research. This was closely related to the researchers' wish for the lay people's unconditional trust of science in general as well as of the researchers participating in the Round Table in particular, which was not seen as realized through the engagement:

> although we spent much time together, I did not have the feeling that they [the lay participants] felt that we were people who could be trusted in the way [we] work. I did not have the feeling. But until the very end I actually had the feeling that they found that we were indeed quite nice but that there must absolutely be someone looking over our shoulders. (S6/eP/65)[cxxxvii]

10.2. Reappraising the role of ethics: Meta-language or idle talk?

The participants experienced and performed ethics in different ways at the Round Table. They had to engage with the issues themselves outside of expert-led discourses, but also could discuss issues with the invited ethicist. Hence, the question is what role they now—after the Round Tables—ascribe to ethics. Interestingly, most participants claimed that their image of ethics had not been influenced by the discussion at the Round Table. This is because "image of ethics"—the way this question was phrased in our *ex post* interviews in order to allow for a rather open response—meant different things to them. What the participants often meant was that they had not changed their basic values, for example, their skepticism towards animal experimentation. However, what changed to a great extent during the Round Table was the lay people's appraisal of the way science in general and in particular works, and in what ways it is related to other societal contexts such as politics and the economy. This led to re-defined evaluations of the role ethics, as a way to deal with technoscientific questions, might play in society. In this regard, some relevant changes—or learning processes—can be observed. The same overall observation holds for the participating researchers. For example, both participating post-docs claimed that their image of ethics had been widened through the discussions at the Round Table, but they simultaneously said that it nevertheless had not changed their actual research practice, because "we do not make animal experiments directly" (S2/eP/231).[cxxxviii] Thus, apart from the question of animal ethics, it was extremely difficult for the researchers to connect the ethical discussion with concrete laboratory practice. While they had gained a widened perspective on ethics during the Round Table, ethics was still located at the margins of what was thought of as the core work of research.

Many of the lay participants, especially, described ethics as a complex, wide and integrative field after the Round Table discussions. This indicates a change over the course of the Round Tables, as they were not able to express their thoughts on ethics in that way beforehand. Based on his experiences with the discussion on responsibility (see chapter 8.2), a lay argued in the *ex post* interview:

> There I became aware of the scope of this moral dimension for the first time, and I think the others felt the same way. And therefore [ethics] was so extremely interesting for me, because the knot was cut. I have always tried to somehow find the rub in this process [who is concerned with social responsibility], but I think it is not there but one has to reflect upon the matter on this meta-level and then the knot is perhaps cut somehow. … So I hit it only then, that this circle like I have sketched it out is there. But I believe that the circle as such is not able to dramatically change without the meta-level changing, the moral preconditions, the ethical preconditions changing. (L13/eP/161-179)[cxxxix]

This lay puts great hope in the ability of ethics to "cut the knot", as he expresses it; that is, ethics as an overarching language and technique could eventually resolve dilemmas of conflicting interests, in his eyes. Hence, this positively evaluated version of ethics has an *integrative function* for society. Ethics was seen as a "meta-language", that is, a common tongue able to re-unite the diverse sub-parts of society that have fallen apart. These accounts were sometimes accompanied by romanticizing narratives of a better past, where for example science was much more integrative, not yet split into disciplines, and where ethics was an integral part of every science. Ethics, here, was supposed to provide a kind of language that allowed for communication between different actors, allowing them to negotiate common societal values and goals. In this view, ethics serves as a societal governance mechanism that steps in as other domains fail to address moral questions. Thus, ethics was regarded as a kind of "soft" law-making that parallels traditional political law-making but compensates for its shortcomings, as state law-making is bound to partisan politics and the representation of particularized interests.

However, the narrative of ethics as a wide and complex matter that addressed values on a "meta-level" also had a flip side, described by both lay participants and researchers to the same extent. This narrative describes ethics as *idle talk*. Ethics, so the argument was, was only talk, not leading to concrete outcomes and thus providing no answers to pressing questions and the need for guided action. The meta-linguistic character of ethics was this time interpreted as a weakness that abstracts too much from concrete practical problems and also does not produce outcomes able to sanction moral misbehavior.

> … because they all became blinded by routine, and they all have their economic interests, their positions to hold, and their assignments, or further research in the area that was assigned to them. And thus they will not say … here we meet the limit, here I simply cannot go further, will they? (L11/eP/237)[cxl]

This and other lay participants stressed the inability of ethics to effectively control what researchers do. Ethics did not allow for setting boundaries that restrict researchers from transgressing moral thresholds. The participants also criticized that ethics in general, as well as ethics as discussed at the Round Table, did not lead "to a real outcome such as in mathematics" (L10/eP/31),[cxli] thus comparing ethics to an imagined precision and ideal vision of the natural sciences. They also described ethics as a "popular sport"[cxlii] and said it had the "character of a mind game."[cxliii] For another lay participant, ethics was partly "just splitting hairs" (L8/eP/119).[cxliv] In sum, many lay participants employed metaphors of ethics that questioned its character as a real engine for societal change and instead pointed to ethics as a virtual game that does not lead to real changes in the practice of research and science.

Many of the researchers came to the same conclusions when reflecting on the way ethics was discussed at the Round Tables. Often, their point was "that one is discussing entirely in circles and actually reaches no end" (S5/eP/157).[cxlv] Ethics was regarded "as a book that one reads when one has time" (S6/eP/19).[cxlvi] Rather than endlessly maintaining a discussion, the researchers expect from ethicists (and ethics)

> more of a guideline and more concreteness … I would like that, if experts are concerned with something, for example, assessing what is a good procedure, that finally a concrete recommendation comes out. Only to raise questions is—for me viewing it from a natural scientific perspective—not what I would define as a goal. (S6/eP/141)[cxlvii]

Explicitly referring to her background as a "natural scientist", this researcher expects substantial outcomes from ethics in the form of guidelines that deliver instructions for practical actions at the bench. This points to the arguments of the lay people that the researchers largely avoided critically reflecting on their own practices. For the lay people, following "ethical recipes" in order to be on the ethical safe side does not suffice, as this impedes a more open and wider reflection on science as a social enterprise.

After participating at the Round Table as a setting where ethical issues of genomics were discussed, the participants had different expectations of ethics. On one hand, especially the way the ethicist approached it, ethics was seen as having the potential to ask fundamental value questions underlying current science and science policy, and thus make visible the questions of what the common grounds of a more integrative societal and technological development would be. On the other hand, all participants learned that, even in such a long-term setting as the Round Table was, a consensus on value questions may be nowhere in sight, and discussing ethical questions may lead to mere idle talk that results in no personal or institutional commitments regarding the conduct of research. Hence, ethics turned out to be quite ambivalent for the participants.

10.3. Assessing the role of ethical experts: Knowledge hierarchies, "proxy ethics" and public participation in ethics

Despite the overall positive evaluation of the ethics debate by the lay participants, concerns were raised about their ability to take part equally in such a discourse. On one hand, they were able to challenge the researchers' fact-like value assumptions many times during the discussion and hint at the incoherencies of their arguments. On the other hand, many lay participants questioned their own ability to take part in an ethics debate in the *ex post* interviews:

> I come more or less as a lay and ask a knowledgeable person, and we will never escape from this basic relation: He is the researcher who investigates lipases and I am the one from the public. If somebody whose profession is another one in which he is also good is juxtaposed on an equal footing, and he says: "I'm the ethicist and now we are talking", then there is not the aspect of "I now explain to you what genome research is", but then there is a different level … That is, I believe, what is simply more difficult to be spontaneously able to do, what is easier if the other has, so to speak, this authority in the first place. (L6/eP/160)[cxlviii]

This lay participant—who was a medical doctor with some research experience, and thus not completely alien to the world of science—identifies knowledge deficits on the side of the lay people and their missing "authority" in terms of expertise as important obstacles that impeded an equal discourse on ethical questions. For him, it is not so decisive to have special knowledge in the field of the researchers, but rather to have any expertise on any scientific field that qualified for participation in the debate. Expert authority is seen as a requirement of talking to researchers "at eye-level". Some other lay participants also expressed discomfort when asked to provide an opinion about the ethical debate at the Round Table: "Now I do not know if I can express my opinion on ... how I evaluate qualitatively these discussions or confrontations between scientists from different areas of science [between genome researchers and the invited ethicist]" (L14/eP/187).[cxlix] They connected the ability to participate in such a discussion process to the question of how far scientific knowledge was understood. The lay participating at the Round Table did not regard themselves as having that knowledge, and thus the participation of ordinary people in institutional bodies on ethical questions was regarded as "futile". Based on a deficit model of public understanding of science and the assumption that "authority" is a decisive factor to be listened to in a debate, the lay participants self-marginalized public contributions to ethical debates in society. Many lay participants did not trust that the public was able to lead a discourse on ethical issues. The role of the public was seen—based on their experiences at the Round Table—as rather limited. Their claim often was that the public should be better informed, or that those who deliberate on ethical issues should listen to the public, as "the public cannot take on the responsibility" because the "public is no expert" (L14/eP/339).[cl]

Classical hierarchies between experts and lay people, as well as assumptions about science-society relations, were still present and framing the ethical discussions. To have expert knowledge—some kind of expertise—was seen as a prerequisite for participating in a debate, as the possession of knowledge was closely related to the social authority to speak up in public discourses. As a consequence, some saw the invited ethicist as a proxy promoting and defending the lay participants' ethical concerns against the researchers. This was connected to concrete experiences at the Round Table: For example, at the fifth Round Table, the ethicist took up the arguments the participants had made in prior Round Tables and explicitly reframed them in terms of ethical arguments. As a professional, he was more able to oppose the researchers' counter-arguments. Also, all senior researchers missed this Round Table for different reasons. For the lay people, the senior researchers were the main advocates of science as a purely epistemic endeavor rejecting a moral dimension of research. Many lay participants expressed disappointment that the senior researchers did not participate at this particular Round Table, as the ethicist was regarded as being able to resist their rhetoric. At the next Round Table, with the senior researchers present again but without the ethicist, some lay participants tried to reiterate the ethicist's ethical arguments; however, the debates could be deflected and closed rather quickly by the senior researchers. This experience led to the conclusion that the help of a professional ethicist is needed to be able to stand one's ground in an ethical debate with researchers. The accounts of the lay participants hint at the ambivalent role of the invited ethicist as an expert at the Round Table. On one hand, he provided discursive resources for the lay people and was able to speak in the name of the lay participants as a proxy for their concerns. However, as Callon (1986, 216) notes, "To speak for others is first to silence those in whose name we speak". Thus, based on the experience they had, while often seeing themselves as much more capable to personally reflect on ethical issues of technoscience, the lay participants saw participating in a wider public ethical debate as less feasible.

The researchers, too, attributed authority to ethical experts, but for a different set of reasons. They stated that ethicists should be concerned with ethics in terms of a division of labor between natural sciences and ethics. Experts should provide expertise on their respective domain only. As a consequence, they displaced the task of ethical reflection to specific experts. The researchers aimed to set up boundaries between ethics and science in order to protect (basic) science from being infused by societal values. As such, the researchers' way of dealing with ethics mirrors the wider societal dealing with ethics, where, too, the production of facts and the reflection on values take place in two domains whose boundaries are carefully patrolled (Wynne 2001; Levidow and Carr 1997).

10.4. Who is responsible for ethics? Central ethical agency, social distribution of ethics and the ambivalence of politics

Many participants highlighted that ethics should ideally have an integrative function in society in order to identify common values that allow for robust socio-technological development and prevent particularistic interests from prevailing in the governance of science. However, this desire left open who actually should and can decide what technoscientific trajectories society should take. For the researchers, relying on their basic narrative of the separation of basic research and applied research, it was clear that only science should govern itself. Only once applications are "ready" is it up to society to decide upon their use. Who exactly should do this, and on what terms, was not further explored by the researchers, as it was not regarded as their concern anymore. Sometimes, they referred to individual preferences for markets as a way of deciding what applications should be approved for societal use. For the lay participants, on the other hand, it became increasingly unclear during the discussions at the Round Table who actually should govern technoscientific trajectories in society (Fochler 2007). For them, the question was, if science proclaims its unconditional freedom of research, who, then, can set ethical boundaries if society considers certain technological developments unwanted or even dangerous? Their view of the network nature of technoscience and its entanglements with other societal domains, such as the economy, gave rise to the idea among the lay participants that there was real demand for an identifiable actor who would control these developments. However, there was no consensus among them about who this might be:

> So, I experienced from the beginning that there was again the question of regulation, of rules: Who defines boundaries? Who sticks to limitations? Who is in control of compliance with limitations? That I experienced again and again as an important point that came not only from me but also from many

> other sides ... And the answer is this, which remained for me so to speak: We do not know who does it. There is actually no one who does it. Somehow it is a self-organizing process. (L6/eP/156)[cli]

This statement strongly emphasizes the desire for a centralized actor powerful enough to set up and control commonly binding rules. Ethics is not seen as being able to provide the definite answer to this question; in fact, for this lay participant it remains unanswered. The lay participants' statements, then, describe the difficulties that emerging technologies pose to the public understanding of politics, and thus challenge current political procedures, but without having an alternative answer. The necessity of a central actor was also challenged by one of the participants:

> everyone should actually have to take on responsibility. ... for sure, some now say that we would need an institution somewhere above that is responsible, but everyone has to do it himself because this institution simply does not exist and never will. Therefore, everyone who does this [conducting research] should ask himself to what extent this is still okay. (L12/eP/333)[clii]

The assumed impossibility of a central actor suggests the importance of individual responsibility. Most of the other lay, however, believed in the necessity of a centralized actor governing science and society. One, for example, suggested "religion", but most of the others preferred more secular solutions, such as expert round tables where professionals from different domains would meet and work out rules. Another lay participant was in favor of politics, understood in the narrow sense of state-oriented politics: "Of course, in many cases today a good politics cannot bypass contacts with science, it would have to include this, but the final responsibility can only lie there [in politics]" (L11/eP/334).[cliii] Others explicitly denied the ability of politics and politicians to perform a leading role: "And it is my feeling that there is not an ethical dimension that politicians have in their hands and say, 'Yes, I will take care that', but that it somehow—be it for financial reasons—is so that they [the scientists] do not go [abroad]" (L6/eP/84).[cliv] Here, politics as a central actor is described as malleable by the interests of industry. The lay participants predominantly longed for a central actor who would take the lead in governing science according to common societal ethical standards. However, they were quite unsure who this actor might be and if there could be such a central actor in principle.

Ethics in its multiple meanings at the Round Table was not seen as able to take care of a socially and ethically robust knowledge politics, as it was seen rather as a deliberative tool and not as a means for binding decision-making. Ethics as a means to govern science only works in an ideal version of society where everyone is ready to take on responsibility and to reflect on possible consequences. This ideal was not seen as having any chance to be realized, and thus remained a normative demand only. Hence, the lay participants expressed the need for a central actor that would take care of ethical socio-technical development. This central actor was too hard to identify for the lay people. With a few exceptions, politics in a narrow sense as a traditional institution for societal decision-making was not considered trustworthy enough to take care of it. On the other hand, the participants did not want to relieve politics from its obligation to govern society in a comprehensive sense (morally, technologically, economically, etc.). Thus, the uncertainties that were related to genomics and the ethical questions they generate resulted in a "wandering about" between the emphasis on deliberative ethics as a means to trigger a more integrative development and the longing for a strong and centralized actor that would set up binding rules. This ambivalence reflects the uncertainties triggered by emerging technologies, which puzzle society with regard to moral questions and increasingly with regard to politics as such.

11. *Conclusions:* Moving beyond good and evil: Towards a new understanding of ethics in the governance of techno-science and society

The previous chapters aimed to provide a comprehensive view of the multiple facets of a bottom-up ethical discussion that went beyond expert-led discourses by letting “ethical lay” develop their own ideas and views of some ethical issues of science and technology. I considered the range of ethical issues discussed, the particular discursive micro-politics that shaped the ethical discussion and what sense the participants made of the experience for themselves after such a long time debating. Given the richness of the material of the empirical setting of the Round Table discussions, as well as the complex societal entanglements between ethics, politics and society, I will focus my conclusions on three main aspects that seem worthwhile to take into account in further reflections.

The first implication concerns the way ethical lay people—in our case both lay participants and researchers—handle ethical issues. Here, I suggest putting greater emphasis on *lay ‘ethical’ knowledge,* as lay provide a crucial resource for critique of existing regimes of innovation that ethics is a part of. Taking this into account, I will then argue for an *ethics of innovation,* which could provide an alternative to the currently dominant “ethics of compensation” by opening up questions of science and technology governance in a much broader way, and thus eventually contribute to a more robust decision-making process. My last point concerns the relation between academic ethics and social sciences, which have largely been framed by the concept of *“empirical ethics”.* While this debate is framed as one of “interdisciplinary methods”, I suggest that this falls short of appropriately addressing the concerns that I outline in the first and second points of the conclusion. Thus, I suggest considering the relation between ethics and social science a political one, that is, as a struggle for who is entitled to represent “public” values.

11.1. Lay ‘ethical’ knowledge as a resource for challenging institutionalized ethics

> The analysis of the ethical debate at the Round Table does not confirm the assumption that ethical lay are not able to engage with ethical issues in a reasoned way, going beyond mere moral opinions. Instead, the participants showed that they were able to engage with ethics in a differentiated and reasoned way, although also sometimes struggling with it. The engagement with ethics at the Round Table produced a range of irresolvable ambivalences and questions, which, overall, drew an incoherent picture of what ethics was at the Round Table. While the discussion could be read in terms of a deficit model of public understanding of ethics, wherein the participants failed to use well-established frameworks of professional ethics due to a lack of appropriate knowledge, such a conclusion would fail to address arguments from the Round Table discussions that are ordinarily omitted in societal ethical discourses. Thus, my argument and hypothesis is that lay ‘ethical’ knowledge could become a crucial resource for reflecting on the dominant way society deals with ethics, and could point to unacknowledged problems of institutionalized ethics. As ethics is often “in service” of administrations with rather hegemonic visions of how innovation process should run, and thus of what kind of ethical issues are admissible, the consideration of lay ‘ethical’ knowledge could be a valuable resource—even for ethicists—to question taken-for-granted assumptions about ethically relevant issues.

The project started with the assumption that a different kind of public engagement setting would trigger alternative ways of discussing ethical and social aspects of genomics. Our question was what kind of “ethics” could emerge if ethical issues were discussed in a more bottom-up way, outside of expert framings. Our “experiment” resulted in a quite complex picture of lay ‘ethical’ knowledge, which on one hand took up many elements of the wider societal discussion on ethics, but which on the other hand was quite subversive to dominant framings of ethics. My argument here is to take lay ‘ethical’ knowledge much more seriously in public ethical policies. Because lay ethical epistemologies are largely incompatible with existing ethics regimes, institutionalized ethics has to initiate change in its procedures and behavior. So far, it has been able to deflect public concerns by referring to its authority, which is based in an epistemological hierarchy of lay and expert treatments of ethics: It has been the public that has to change, that is, that has to incorporate the experts’ framing before legitimately being able to participate in “ethics”. I argue that it is rather expert ethics that has to change its framing, by developing more advanced means to give voice to lay epistemologies on ethical issues. Such changes would trigger a loss of authority by virtue of the “expert” role, but lead to a gain of authority by virtue of a broader involvement of public concerns. I will discuss three hypotheses that can be drawn from the analysis of lay 'ethical' knowledge.

My first hypothesis is linked to the uncertain nature of what ethics is. At the Round Table, different, often conflicting, visions of ethics were brought to the table by the participants, which were highly dependent on the context of the discussion as well as the experiences and backgrounds of the participants. These ethical concepts defined a framework in which ethical arguments were made sense of, but also were themselves subject to ethical questioning. Ethical arguments were embedded in theories of the social and the political as reference points lending sense to ethics. The participants' dealing with the label "ethics" also reflects the uncertain nature of ethics. In general, they felt more confident talking about "values". Explicit "ethics" was rather assigned to other domains outside the setting of the Round Table, in particular to expert-led discourses. Thus, the participants performed "ethics" implicitly, and self-distanced themselves from explicit ethical discourses. In a manner similar to that described by Michael (1992) on "science", the participants on one hand regarded ethics as an abstract entity distant from themselves, to which they positioned themselves partly as "ignorant" ("ethics-in-general"). On the other hand, they connected "ethics" to phenomena in which they were involved, and to which they were able to contribute substantially ("ethics-in-particular"), but only in form of a "value" discussion. Thus, there was a tension around ethics, between ethics as "other" and ethics as part of their own experiences. This reflects ethical expert notions on the difference between "ethics" and "morality". From the experiences at the Round Table, this distinction is not only an epistemic one, but, when enacted in societal practice involving non-experts, it works as a way to exclude certain positions from the ethical debate.

The tension between the practices of dominant institutionalized ethics and the way most lay participants at the Round Table imagined ethics in society can be described in the way Jasanoff (2003c) has described current political regimes of innovation: Currently, policy-makers perform "technologies of hubris", which are characterized by a blindness towards ambiguity and uncertainty. They pre-empt political decisions by creating high entry barriers for legitimate positions that do not fit into dominant discourses, and they are limited in their capacity to internalize challenges that arise outside their framing assumptions. This description of "technologies of hubris" nicely fits the way dominant institutionalized ethics is performed. As an alternative to these "technologies of hubris", Jasanoff suggests so-called "technologies of humility", which are characterized by (1) methods for revising the initial framing of issues, (2) citizen participation in the analysis of their vulnerability to regain their active status, (3) distribution, and (4) the design of avenues through which society can collectively reflect on the ambiguity of their experiences. Ethics as a "technology of humility" is akin to the concepts of ethics many participants at the Round Table advocated. Lay 'ethical' knowledge makes a strong case for reflecting in a more detailed way on the multiple roles and practices of ethics and carefully balancing between different visions of the role of ethics in society. Lay ethical epistemologies are characterized by inherent ambiguities about the nature and role of ethics. These ambiguities, however, are not only local properties of the discussions at the Round Table, but reflect wider societal inconsistencies of ethics. Thus, the participants' accounts of these ambiguities provide fruitful resources for rethinking dominant practices in ethics.

My second hypothesis regards the role of expertise in ethical reasoning and its relations to lay 'ethical' knowledge. So far, the relation between lay and expert ethics has been characterized by clear boundaries with the result that the public has not been regarded as having a particular voice in ethical decision-making on science and technology, which has instead been handled by experts. At the Round Table, the role of ethics experts was perceived as ambivalent. The researchers attributed authority to ethicists by a division of labor model; however, in this model ethics was not meant to interfere with basic research beyond existing legal frameworks that govern knowledge production. The researchers' deflection of ethics and the lay people's experience that they often failed to challenge the researchers' moral presumptions, as well as a lack of trust in the researchers' abilities of critical ethical reflection, resulted in a call for professional ethics to undertake this task. The lay people regarded ethicists as proxies for their concerns. However, the authority that the lay participants ascribed to ethicists does not necessarily correspond to the practices of institutional ethics, which is often regarded as the handmaiden of R&D interests. For the lay participants, rather than denoting the "real" practices of institutionalized ethics, "ethics" served as a placeholder for a different engagement with science and technologies that involve questions of societal values to a higher degree. In particular, expert ethics was seen as a place that assembles a wide range of potential moral issues of science and technology that are then opened up for a wider societal discussion. Thus, the lay participants' expectations imply a notion of ethical expertise contrary to many social science accounts of this issue, that is, ethics as a rather "thin" discourse (Evans 2002) that imposes its narrow framing on public meanings (Wynne 2001), for example a single vision of the role of citizens, as well as a way to bypass democratic legislation processes (Tallacchini 2006). In short, the lay participants' expectations of institutionalized expert ethics were of a different nature than its actual practice; therefore, the confidence of the lay participants in expert ethics was based not on how it "is" but rather how it "ought" to be.

Ascribing huge authority to ethical experts resulted in a devaluation of the lay participants' own arguments, which they often considered not "ethical" but only "moral", and thus which in their own eyes did not enjoy sufficient authority to counter the researchers' arguments. Hence, my hypothesis is that the lay participants' participation in the discussion suffered not a lack of arguments, but rather a self-ascribed lack of authority. This also had an impact on the evaluation of lay participation in ethics on a more general level beyond the Round Table discussions. While the lay participants did regard themselves as able to challenge the researchers on the grounds of implicit and explicit values embedded in the researchers' accounts, they expressed some unease about the contributions of the public in a societal debate on ethical issues. Mirroring the distinction made by professional ethicists between "ethics" and "morality", the lay participants did not regarded normal citizens as able to overcome their individual experiences on value questions and thus to provide "ethical" arguments that could be authorative to researchers. Hence, the lay participants simultaneously expressed the wish for participation and called for experts. The question is, what does this mean for institutional ethics, the practices of which often stand in sharp contrast to the expectations and hopes of the lay participants at the Round Table? The outcomes of my analysis indicate the necessity of a changed role of ethical expertise in society; that is, ethics should not so much enact the role of framing ethical issues in terms of academic priorities, but should help to enable a wider discourse on ethical issues that seeks the inclusion of voices that have not been heard so far. This does not mean that institutionalized ethics would become subject to "moral populism", but it would necessitate an advanced reflection on how public moral concerns can be translated into political decision-making processes.

The third hypothesis concerns the question of whether "ethics" is opening up or closing down ethical questions. The question of "opening up" or "closing down" has been quite prominent in literature on public participation (Wilsdon and Willis 2004; Stirling 2008). At the Round Table, different visions of ethics conflicted according to whether the role and aim of ethics was thought to be to install a fixed set of norms for individual and institutional behavior—and thus to end deliberations—or to act as a means to make problems subject to deliberation—and thus to open them up. For many of the lay participants, ethics was seen as a place of continued deliberation that accompanies the whole innovation process. Public ethical deliberation should step in early in the innovation process, and thus re-open questions that are already seen as closed and taken for granted. While the deliberative role of ethics was clear to the lay participants, it was rather obscure to them in what ways this would influence and govern innovation processes. For the researchers, on the contrary, ethics had mainly the function of closing down questions for good. On several occasions they expressed the wish that ethicists provide them a fixed set of principles in the form of guidelines that they would be able to practically consider in their research, thus suspending the need for continued reflection on the possible consequences of their work. This also explains their move to set ethics and law equal, as the latter also presents a normative fact where further reflection seems pointless. On the other hand, after the Round Table all researchers mentioned the importance of a continued ethical discussion in science as well as with the public. Here, the question is how these two visions of ethics relate to each other. However, the Round Table was a place where these conflicting notions of ethics as opening up and closing down were brought into a debate. While the Round Table could not provide a definite consensus or solution on this issue, it provided a set of arguments that should be considered in the societal debate on ethics, where this question of the role of ethics is not often addressed. Drawing from the discussions at the Round Table, the question of opening up and closing down is a useful tool to reflect upon wider societal dealing with ethical questions, where the dominant practice of institutionalized ethics tends to close down potential questions, rather than open them up, by applying a fixed set of available criteria or principles to issues. This is in contrast to the defined role of ethics committees to facilitate a public debate. As such, institutionalized ethics is caught between political constraints that require formal procedures and solutions, which tend to close down ethical discussions, and the necessity to debate on value questions in a more open way in order to absorb future conflicts that may emerge by prematurely fixing values in regulations. The discussion of different visions of ethics at the Round Table nicely reflected this ambiguity.

11.2. From the “ethics of compensation” towards an “ethics of innovation”

My main hypothesis in this section is that the discussions at the Round Table suggest a need to move beyond a narrowly defined ethics that compensates for past decisions and towards an ethics of innovation in a much broader sense. While institutionalized ethics understands itself as a necessary complement to scientific risk assessment, the division of labor between risk and ethics reinforces the distinction between “facts” and “values”. Science policy often finds comfort in this distinction. At the same time, it is a source for large public controversies over science and technologies, as shown by the example of GMOs, where politics largely focuses on technical risk aspects but fails to address concerns about underlying cultural values. Likewise, institutionalized ethics largely fails to address broader public unease that is rooted in concerns about innovation regimes and the relations between science and society as such. The public rejection of concrete technological applications can be seen as an expression of a wider unease about current innovation regimes. Thus, in order to overcome the problems resulting from the distinction between risk and ethics, a more open “ethics of innovation” seems necessary. This ethics, however, is not one that provides ready-made answers, but is more modest in defining “problems” and in providing frames to be considered as common, and asks rather what the question at stake might be in the broader context of a debate over how society should address technoscientific futures.

Recent studies of the way innovation is governed suggest that the current dominant regime of risk governance fails to address broader questions of public concerns and culture, and thus suffers serious deficits in contributing to a more robust innovation policy. The main argument of the critics is that public involvement comes into play at a rather late stage, leaving space only for questions of technical risk (Felt and Wynne 2007; Wilsdon and Willis 2004; Wilsdon, Wynne, and Stilgoe 2005). Institutionalized forms of ethics are integral parts of the risk governance approach, as a kind of “moral risk assessment”. The division of labor between risk assessment and ethical evaluation fosters the idea that technical features of emerging technologies can be treated separately and independently from questions of values, and thus reinforces the division between “facts” and “values” as one of the main sources of public conflicts on science and technology.

This distinction was also a key feature of the ethics discussions at the Round Table, and often turned out to be an “argument” capable of closing down deliberation on ethical issues (see chapter 9). On the other hand, the researchers’ insistence on the “factish” nature of their knowledge production triggered lay participants’ objections, and thus widened ethical issues not only with regard to future impacts of possible technologies on social values systems but also with regard to innovation regimes already in place, and therefore with regard to past value decisions embedded in institutional practices. The participants' treatment of this issue is a strong argument for re-opening already settled—and thus black-boxed—decisions as *explicit ethical questions in which a wider range of people should participate*. In short, the argument is that the new relations between science and society (as well as changes in "science" and "society") need to be addressed by a different type of "ethics" that transgresses and redefines its present competences. I call this new type of engagement with ethical questions an *ethics of innovation,* which is in line with the call for a "governance of innovation" in a recent report of the European Commission entitled *Taking European Knowledge Society Seriously* (Felt and Wynne 2007). The expert group argues that regimes of risk governance fail to appropriately respond to the public's unease about science. The roots of public mistrust are systematically misinterpreted using a deficit model; that is, the public is thought to be in need of education in a "proper" understanding of science (Wynne 2007; Felt and Wynne 2007). "Risk" and "science" are often taken as unambiguous concepts by policy-makers who do not take into account that these things have different meanings for different people.

The main features of this suggested ethics of innovation is that it should include a wider range of actors in the debate and simultaneously broaden its scope. The ethics of innovation marks a clear departure from the current dominant practice of the "ethics of compensation" (Levidow and Carr 1997), the aim of which is to compensate for past value decisions embedded in current innovation regimes. Institutionalized ethics usually draws the lines and defines limitations for given technologies, but rarely considers and evaluates the innovation regimes that underlie these technologies, or wider cultural understandings of innovation as a complex process. Furthermore, such a scheme fosters the idea that past value decisions are reversible by means of institutionalized ethics.

The shift towards an ethics of innovation was suggested by the way a wide range of ethical issues were discussed at the Round Table: First, the distribution of social responsibility for research outcomes was a major issue for the participants. While the regulation of research provides a legal answer to this question in concrete rules and norms, the moral dimension of this question was still unanswered for the participants, especially for the lay people. While such a discussion is seldom the subject of opinions of institutionalized ethics on specific technologies, the discussion at the Round Table showed that this question needs to be posed continually with every new technology, particularly if research is said to be increasingly carried out in the context of application (Nowotny, Scott, and Gibbons 2001).

Second, concrete applications of the researchers' work were subject to discussion at the Round Table, in the form of the "fat pill". However, the focus was not so much on the specific ethical aspects of the fat pill once introduced in society, but on the more basic ethical question of who defines what counts as a "problem".

Standard ethical assessment takes “problems” mostly as given. The lay people at the Round Table suggested that particular “solutions” also define the “problem” to be addressed. Thus, what is needed is a wider discussion of what kind of problems should be addressed with what means in the first place.

Third, animal experimentation was a huge topic at the Round Table. While this may look like a straightforward problem of animal ethics, the participants related this questions to wider ideas of innovation processes. In particular, responding to the researchers’ distinction between (“socially useless”) basic research and applied research, the lay people asked how one can justify the killing of animals for the production of merely “culturally relevant” knowledge. Thus, animal experimentation was contextualized in the wider context of innovation regimes.

These discourses suggest that institutionalized ethics, as a kind of “blinker ethics” that does not consider the wider cultural and societal context of knowledge production, is poorly equipped to engage with the realities of contemporary research and the complexity of innovation processes. A different form of ethics needs to address innovation in a much broader sense than that in which it is currently addressed, and must go beyond the current institutional settings that define more or less exclusively what is an “ethical issue” and what is not. Furthermore, in a suggested ethics of innovation, ethics and participation should not be treated as two entirely different strands in the governance of science, but public involvement in ethics should be institutionally appreciated. Lay ‘ethical’ knowledge should no longer be considered insufficient accounts of ethical issues, but rather relevant resources for deliberations on how future technoscientific trajectories should be designed. As a consequence, a broader range of actors could be included in the process of ethical deliberations. In the ethics of innovation, experts would have a different role than in current institutionalized settings, where they work as gatekeepers of what is an ethical problem and what is not. The new expertise of ethics would entail opening up questions rather than closing them down. This new kind of expertise would move beyond the current “expertise of reasoned argumentation” in ethics and towards a new expertise of community (Fochler 2007; Felt and Fochler 2008) that regards ethics as a collective social practice.

In order to reach an inclusion of lay ‘ethical’ knowledge, more sensitive and differentiated “mechanisms of voicing” of lay ethics are needed. The Round Table was an attempt to provide such a mechanism of voicing. The inclusion of lay ethical knowledge does not mean that lay voices *directly* influence political decision-making on ethical issues—something that is often feared by ethicists and rejected as a populist version of ethics. Even the participants at the Round Table themselves were very skeptical about such a “mechanical representation”. A different set of mechanisms of voicing requires also a different set of “translation” technologies. Taking into account lay ‘ethical’ knowledge does not mean that the “is” now informs the “ought”. Rather the “is” could inform the way and the context of how the “ought” is produced.

11.3. Two cultures? Tensions between social science and ethics

Contrary to most of the literature on “empirical ethics” and the collaboration between the social sciences and ethics, I call for caution regarding a more intense and premature co-operation of social science and ethics. While I welcome dialogue and critical discussion between the disciplines, real interdisciplinary collaboration based on shared methods could turn out to be problematic to a certain degree, in particular because basic tenets and premises of both fields have not yet been clarified. My hypothesis is that social sciences and ethics often do not share the same understanding of being “empirical”. Interdisciplinary misunderstanding is based on the different epistemic culture of each discipline, that is, how practices of proof and objectivity as well as styles of public knowledge-making are constructed (Jasanoff 2005). I will argue that there is an important political dimension behind the debates on interdisciplinary method, namely a struggle for public authority. My argument is that if the social sciences are willing to collaborate with ethics in the form of empirical ethics, they must keep well in mind the epistemic differences as well as the discrepancies in the politics of each.

The issues addressed in this thesis are usually seen as the domain of ethics. For quite some time, ethics has had a quasi-monopoly on the evaluation and assessment of ethical dimensions of science and technology. While the lament of the disengagement of social sciences from ethical questions is quite old (Fox 1976), it has not been until recently that an increased mutual interest between social sciences and ethics has emerged. This happened in two ways: First, ethical and moral practices were discovered as a relevant field of study for the social sciences, especially anthropology and ethnography that engaged with ethical practices at the bedside to explore the concrete meaning of esoteric concepts like dignity (Marshall 1992; Parker 2007; Hoeyer 2004). However, this focus is also accompanied by an ignorance of the larger-scale institutional and policy contexts in which ethics is practiced, which have been addressed only in a few studies. More recently, other fields of social science—for example, sociology and STS—have jumped on the bandwagon and engaged with the institutional dimensions of ethics, resulting in an increasing body of studies that take serious ethics not only as a way of reasoning but also as an often authorative discourse shaping social relations (Evans 2002; Tallacchini 2006; Jasanoff 2005; Kelly 2003; Salter and Jones 2005; Salter and Salter 2007).

Second, ethics itself expressed a need to open itself towards social science, although in a rather particular way. Thus, in this last section I will assess the mutual interest of social science and ethics, and argue that the current discussion between ethics and social science suffers substantial deficits. The relations between ethics and the social sciences as academic disciplines are mostly addressed under the paradigm of "empirical ethics", a new field that suggests that social science and ethics can be reconciled by investing in the development of an *interdisciplinary methodology* that allows both disciplines to contribute to knowledge production on normative issues. I will argue that the mutual engagement of social science and ethics on methodological issues—while surely fruitful in particular contexts—can take on a problematic aspect when considering the wider contexts of the role of ethics in society, because such a frame tends to obscure the political dimensions inherent in ethics. I make two arguments: The discussion of methodology can be seen as a *struggle for public authority,* in which the access to people's hearts and minds is a crucial political resource to legitimate particular normative decisions. Second, there are different visions of what it means to be "empirical" in the social sciences and ethics. In ethics, the empirical is predominantly understood in the sense of proved facts having an alibi function for legitimizing normative rule-making.

I start with the argument that, in empirical ethics, empiricism is supposed to lend authority to ethical expertise in a public struggle for who is authorized to speak for common values. This can be related to an assumed "crisis of expertise" that has accompanied debates over technological controversies over the last year, for example the BSE crisis, as well as the debates over GMOs in many European countries. As a response, there have been calls for a democratization of expertise (Gerold and Liberatore 2001), which suggests that a broader inclusion of public views would be able to re-establish trust in expert-led decision-making. It's not my concern here whether this crisis is real or only a matter of the self-perception of experts, based on a few incidents while expertise predominantly enjoys good health. My assumption is that the call for a democratization of expertise finds its particular articulation in ethics in the form of "empirical ethics", which can be seen as an attempt to gain societal legitimacy while maintaining expert authority over ethical issues. From the point of view of ethics, the social sciences hold a certain public authority by being "empirical", that is, being able to represent society and public values with its methods.

In the social sciences, public values are held by smaller and larger proportions of the population—to be carefully distinguished from the "published opinions" put forward in media discourses, which do not necessarily represent public opinion. Public opinion is regarded as conferring legitimacy to political decision-making (Noelle-Neumann 1979). In ethics, however, what counts as commonly shared morality is not necessarily related to the values held by members of the public. The body of norms shared in society is derived from a set of principles on which it is *assumed* that everyone *could agree*. Thus, for ethics, public opinion on moral issues as such does *not* confer legitimacy in normative rule-making. What "ought" to be must be rooted in well-reasoned arguments, and not in asking people what they find right or wrong (Crosthwaite 1995). However to go "empirical" is seen as a crucial dimension for the advancement of ethics. Borry et al. (2005) argue that ethics is now more prone to empirical work because of dissatisfaction with a foundationalist interpretation of applied ethics, strong integration of clinical ethicists in the medical setting and the rise of the evidence-based paradigm. Ives (2008, 5) argues that "in having 'encounters with experience', we can make our ethical and philosophical deliberations more applicable, more useful, and more real."

Thus, many advocates of empirical ethics express dissatisfaction with standard ethical methods, which fail to make ethical analysis more "real". However, in all the writings on empirical ethics—be it from the social sciences or ethics—one point seems to be missing. That is, being more "real" is not only a matter of appropriately representing "facts". Being more "real" also means that the suggestions provided by ethics experts are *realized*, that is, that (political) influence is deployed in concrete social environments. Thus, in that sense, the debates around empirical ethics can be read as a struggle for public authority over societal norms and values. Institutionalized ethics bodies in particular have gained great power in defining what an ethical issue is and what kind of solutions should be employed to compensate for the impacts on the rights and dignity of the individual. However, in the course of the shift from classical state government to more inclusive forms of governance, such as public engagement, the question of the political legitimacy of public decision-making is posed again. Legitimacy today rests on an appropriate representation of society—not as an impersonal structure or system, but rather as a body of citizens who have rights and duties in the political process. It is rather difficult for theory- and principle-based ethics to provide such a scheme; thus, it seeks to include empirical findings in order to uphold the authority of ethics in defining what counts as common morality. If so, my argument is that "empirical ethics" is not merely a methodological and interdisciplinary struggle between social science and ethics, but has a clear political dimension. Thus, "public authority" has two different meanings: on one hand, authority *over* public meaning, and, on the other, authority that is gained *from* the public. These aspects, too, have not been carefully separated in the debates. The ignorance of ethics of these aspects of "empirical ethics" also has to do with its self-perception as a critical voice in biomedical developments, where "modern bioethics has adopted the role of a discipline that critically observes all kind of developments in modern society. It seems, for example, that modern bioethics now fulfils the critical role toward medicine and health care that medical sociology fulfilled in the seventies of the 20th century" (Dekkers and Gordijn 2005, 271). This argument is contrasted by critiques that regard ethics as a handmaiden of economic and research interests.

The second argument I want to make is that empirical ethics seeks to enroll social sciences in ethical reasoning. However, the question is how precisely ethics is imagining such a collaboration. The basic assumption is that normative ethics is often missing facts crucial to ground normative rule-making. Thus, the assistance of social science is sought to support ethics in assembling the necessary empirical data. In seeking that assistance, ethics defines a particular vision of what it means to do empirical work: The empirical is defined as the counterpart to normative reasoning, based on the meta-ethical distinction between “facts” and “values”, or “is” and “ought” (Borry, Schotsmans, and Dierickx 2005). Thus, “empirical” is set equal with “facts” and the “is”. Particularly revealing is one of the latest fashions, named “evidence-based ethics” (Goldenberg 2005), suggesting that empirical data of social relations enjoy the same status as the facts provided in “evidence-based medicine”.

One of the key features of the empirical social science—in particular the qualitative social sciences, which are the reference point for my argument—is that the rather hermetic distinction between “facts” and “values”, or empirical and normative, does not exist. In putting the social sciences in the empirical corner, the ability to take a legitimate normative approach is denied to the social sciences, because ethics is only importing the descriptive content from the social sciences and not its normative arguments. However, the assigned role of the social sciences as a supplier of “facts” is contrary to the attempts of social science to position itself as a normative enterprise. Recent discussions in sociology and STS have aimed to position these disciplines as normative institutions, that is, institutions that advocate for particular norms that are regarded as crucial. For example, Burawoy (2005), President of the American Sociological Association, called for a “public sociology”, to work “in the defense of civil society”. A particular normative task that STS scholars should take on is to contribute to a democratization of technological culture, as Bijker (2003) argues. The task of public STS intellectuals is to show that all science and technology are value-laden, and thus to make all science and technology subject to political debate (Bijker 2003; see also McKenzie Stevens 2007). Hence, STS claims that all scientific knowledge carries implicit and explicit norms, and that one cannot get rid of them by sorting out what is “fact” and what is “value”—something that empirical ethics claims to be able to accomplish. Some currents in STS argue for greater democratization of technoscientific cultures. Other STS scholars have directly engaged in policy processes and have actively argued for a greater political engagement of STS (Webster 2007). Thus, while empirical ethics assumes that social science is and should be only “descriptive”, it is already a rather normative enterprise.

The experiences at the Round Table suggest the necessity to take seriously the ethical practices and discourses that take place in everyday contexts. The “empirical” should not be considered another source of more or less objective and value-free data or “facts” in the course of normative rule-making. Rather than opting for increased collaboration, I suggest that the social sciences and in particular ethics must develop a more reflective understanding of their entanglements with the political world. Thus, I suggest taking the term “empirical ethics” seriously and asking what ethics does in societal practices. Both social sciences and ethics have to be aware of their political roles when discussing the empirical. The empirical is never simply given, nor can it be considered a fact. It is made, and thus a more reflective engagement with the performative dimension of method seems required. While there is a certain truth in the ethicists’ claim that the social sciences are often unaware of their normative assumptions in their analyses, ethics also has to consider that the world cannot so easily be separated into “is” and “ought”, and that this distinction has a strong influence on the way science is governed in current innovation regimes. However, the discussion of “empirical ethics”, as one of interdisciplinary methods, will not go far unless its political dimension is addressed in a more open way.

12. References

Aho, E. 2006. Creating an innovative Europe. Report of the Independent Expert Group on R&D and Innovation appointed following the Hampton Court Summit and chaired by Mr. Esko Aho. Brussels: European Commission.

Anderson, B. 1983. *Imagined communities: Reflections on the origin and spread of nationalism*. New York: Verso.

Banks, S., J. Leach Scully, and T. Shakespeare. 2006. Ordinary ethics: Lay people's deliberations on social sex selection *New Genetics and Society* 25 (3):289-303.

Beauchamp, T. L., and J. F. Childress. 1994. *Principles of biomedical ethics*. Fourth ed. New York: Oxford University Press.

Beck, U. 1992. *Risk society: Towards a new modernity*. London: Sage.

Beck, U., A. Giddens, and S. Lash. 1994. *Reflexive modernization: Politics, tradition and aesthetics in the modern social order*. Princeton: Stanford University Press.

Bensaude-Vincent, B. 2001. A genealogy of the increasing gap between science and the public. *Public Understanding of Science* 10 (1):99–113.

Bijker, W. E. 2003. The need for public intellectuals: A space for STS. Pre-presidential address, Annual Meeting 2001, Cambridge, MA. *Science, Technology, & Human Values* 28 (4):443-450.

Bioethikkommission beim Bundeskanlzeramt. 2007. Beschluss der Bioethikkommission beim Bundeskanzleramt vom 13. Juni 2007. Nanotechnologie, Katalog ethischer Probleme und Empfehlungen. Vienna: Bundeskanzleramt der Republik Österreich. Available at http://www.bundeskanzleramt.at/DocView.axd?CobId=23987 (accessed August 12, 2008).

Birke, L., A. Arluke, and M. Michael. 2007. *The sacrifice. How scientific experiments transform animals and people*. West Lafayette, IN: Purdue University Press.

Bister, M. D., U. Felt, M. Strassnig, and U. Wagner. 2009. Refusing the information paradigm. Informed consent, medical research, and patient participation. *health: An Interdisciplinary Journal for the Social Study of Health, Illness and Medicine* 13 (1):87-106.

Bogner, A., and W. Menz. 2005. Bioethical controversies and policy advice: The production of ethical expertise and its role in the substantiation of political decision-making. In *Democratization of expertise. Exploring novel forms of scientific advice in political decision-making*, edited by S. Maasen and P. Weingart, 21-40. Dordrecht: Springer.

Bogner, A., H. Puchrucker, and R. Zimmer. 2004. Partizipative Politikberatung am Beispiel der BürgerInnenkonferenz 2003 (Analyse). Endbericht. Vienna: Institut für Technikfolgenabschätzung der Österreichischen Akademie der Wissenschaften.

Borry, P., P. Schotsmans, and K. Dierickx. 2004. Empirical ethics: A challenge to bioethics. *Medicine, Health Care and Philosophy* 7 (1):1-3.

———. 2005. The birth of the empirical turn in bioethics. *Bioethics* 19 (1):49-71.

———. 2006. Empirical research in bioethical journals. A quantitative analysis. *Journal of Medical Ethics* 32 (4):240-245.

Brown, N., and M. Michael. 2002. From authority to authenticity: The changing governance of biotechnology. *Health, Risk & Society* 4 (3):259-272.

Burawoy, M. 2005. 2004 American Sociological Association Presidential address: For public sociology. *The British Journal of Sociology* 56 (2):259-294.

Burgess, M. M. 2004. Public consultation in ethics: An experiment in representative ethics. *Journal of Bioethical Inquiry* 1 (1):4-13.

Burgess, Michael M. 2003. What difference does public consultation make to ethics? Electronic Working Paper Series. W. Maurice Young Centre for Applied Ethics, University of British Columbia. Vancouver: University of British Columbia. Available at http://www.ethics.ubc.ca/workingpapers/deg/deg003.pdf (accessed July 15, 2008).

Bush, V. 1945. Science - The endless frontier. A report to the President by Vannevar Bush, Director of the Office of Scientific Research and Development. Washington: United States Government Printing Office. Available at http://www.nsf.gov/od/lpa/nsf50/vbush1945.htm (accessed March 29, 2007).

Callon, M. 1986. Some elements of a sociology of translation: Domestication of the scallops and the fishermen of St Brieuc Bay. In *Power, action and Bbelief. A new sociology of knowledge?*, edited by J. Law, 196-233. London, Boston and Henley: Routledge & Kegan Paul.

Callon, M., and V. Rabeharisoa. 2008. The growing engagement of emergent concerned groups in political and economic life: Lessons from the French association of neuromuscular disease patients. *Science, Technology, & Human Values* 33 (2):230-261.

Calvert, J. 2004. The idea of 'basic research' in language and practice. *Minerva* 42 (3):251-68.

Chilvers, J. 2008. Deliberating competence. Theoretical and practitioner perspectives on effective participatory appraisal practice. *Science, Technology, & Human Values* 33 (3):421-451.

Collins, H. M., and R. Evans. 2002. The third wave of science studies: Studies of expertise and experience. *Social Studies of Science* 32 (2):235-296.

Cook, M. 2002. Designer babies? Don't leave it to bureaucrats to decide. *The Age*, April 23.

Cooke, B. , and U. Kothari, eds. 2001. *Participation: The new tyranny?* London: Zed Books.

Corrigan, O. 2003. Empty ethics. The problem with informed consent. *Sociology of Health and Illness* 25 (7):768-792.

Crosthwaite, J. 1995. Moral expertise: A problem in the professional ethics of professional ethicists. *Bioethics* 9 (5):361-379.

De Vries, R. D., and S. Y. H. Kim. 2008. Bioethics and the sociology of trust: Introduction to the theme. *Medicine, Health Care and Philosophy*. doi:10.1007/s11019-008-9158-7.

Dekkers, W., and B. Gordijn. 2005. The proper role of bioethics. *Medicine, Health Care and Philosophy* 8 (3):271-2.

Deutsche Forschungsgemeinschaft (DFG). 1998. Empfehlungen der Kommission 'Selbstkontrolle in der Wissenschaft'. Vorschläge zur Sicherung guter wissenschaftlicher Praxis. Bonn: Deutsche Forschungsgemeinschaft. Available at http://www.dfg.de/aktuelles_presse/reden_stellungnahmen/download/empfehlung_wiss_praxis_0198.pdf (accessed accessed May 11, 2007).

Dosi, G., P. Llerena, and M. S. Labini. 2006. The relationships between science, technologies and their industrial exploitation: An illustration through the myths and realities of the so-called 'European Paradox'. *Research Policy* 35 (10):1450-1464.

Durant, J. R., G. A. Evans, and G. P. Thomas. 1989. The public understanding of science. *Nature* 340:11-14.

Dzur, A. W. 2002. Democratizing the hospital: Deliberative-democratic bioethics. *Journal of Health Politics, Policy and Law* 27 (2):177-211.

Economic and Social Research Council (ESRC). 2005. Research ethics framework. Swindon: Economic and Social Research Council. Available at http://www.esrcsocietytoday.ac.uk/ESRCInfoCentre/Images/ESRC_Re_Ethics_Frame_tcm6-11291.pdf (accessed March 28, 2008).

Elam, M., and M. Bertilsson. 2003. Consuming, engaging and confronting science. The emerging dimensions of scientific citizenship. *European Journal of Social Theory* 6 (2):233-251.

Epstein, S. 1995. The construction of lay expertise: AIDS activism and the forging of credibility in the reform of clinical trials. *Science, Technology, & Human Values* 20 (4):408-37.

Etzkowitz, H. 1998. The norms of entrepreneurial science: Cognitive effects of the new university-industry linkages. *Research policy* 27 (8):823-833.

European Commission. 2001a. Eurobarometer 55.2. Europeans, science, and technology. Brussels: European Commission.

———. 2001b. European governance. A white paper. Brussels: European Commission.

———. 2002. Science and Society Action Plan. Brussels: European Commission.

———. 2004a. Europe and basic research. Communication from the Commission. Brussels: European Commission.

———. 2004b. Innovation management and the knowledge-driven economy. Brussels: European Commission.

———. 2005a. Frontier research: The European challenge. High-level expert group report. Brussels: European Commission.

———. 2005b. The European Charter for researchers. The code of conduct for the recruitment of researchers. Brussels: European Commission.

———. 2007a. Ethics for researchers. Facilitating research excellence in FP7. Brussels: European Commission. Available at ftp://ftp.cordis.europa.eu/pub/fp7/docs/ethics-for-researchers.pdf (accessed Noevmber 16, 2007).

———. 2007b. Ethics in EU research projects. Brussels: European Commission. Available at http://ec.europa.eu/research/science-society/index.cfm?fuseaction=public.topic&id=36 (accessed February 12, 2007).

———. 2007c. Inventing our future together. The European Research Area: New perspectives. Green Paper 04.04.2007. Brussels: European Commission.

———. 2008. Ethical issues in EU research proposals - checklist. Brussels: European Commission. Available at http://ec.europa.eu/research/science-society/index.cfm?fuseaction=public.topic&id=370 (accessed March 27, 2008).

European Group on Ethics in Science and New Technologies to the European Commission (EGE). 2007. Opinion on the ethical aspects of nanotechnology. Brussels: European Commission.

Evans, J. H. 2002. *Playing god? Human genetic engineering and the rationalization of public bioethical debate*. Chicago & London: University of Chicago Press.

Evans, R., and A. Plows. 2007. Listening without prejudice? Re-discovering the value of the disinterested citizen. *Social Studies of Science* 37 (6):827-853.

Evers, K. 2003. Codes of conduct. Standards for ethics in research. Brussels: European Commission.

Felt, U. 2003. Sciences, science studies and their publics: Speculating on future relations. In *Social studies of science and technology: Looking back ahead*, edited by B. Joerges and H. Nowotny, 11-31. Dordrecht: Kluwer Academic Publishers.

———. 2005. Nichts als die Wahrheit...?! Betrug und Fälschungen in der Wissenschaft. In *Der Wille zum Schein. Über Wahrheit oder Lüge*, edited by K. P. Liessmann, 173-197. Wien: Zsolnay.

Felt, U., and M. Fochler. 2007. Citizens' epistemologies at work. Comparing knowledge narratives related to biomedicine. Unpublished manuscript. Vienna: Department of Social Studies of Science, University of Vienna.

———. 2008. The bottom-up meanings of the concept of public participation in science and technology. *Science and Public Policy* 35 (7):489-499.

Felt, U., M. Fochler, A. Mager, and P. Winkler. 2008. Visions and versions of governing biomedicine: Narratives on power structures, decisionmaking and public participation in the field of biomedical technology in the Austrian context. *Social Studies of Science* 38 (2):233-257.

Felt, U., M. Fochler, and A. Müller. 2003a. Public consultation and foresight initiatives in Austria: Late start and hesitant implementation. In *Optimising public understanding of science and technology. Final report*, edited by U. Felt, 362-366. Vienna: Department of Social Studies of Science, University of Vienna.

———. 2003b. Sozial robuste Wissenspolitik. Analyse des Wandels von dialogisch orientierten Interaktionen zwischen Wissenschaft, Politik und Öffentlichkeit. Gutachten für das Büro für Technikfolgenabschätzung (TAB) beim Deutschen Bundestag. Wien: Institut für Wissenschaftstheorie und Wissenschaftsforschung, Universität Wien. Available at http://www.univie.ac.at/virusss/documents/339044135.pdf (accessed July 15, 2007).

Felt, U., M. Fochler, A. Müller, and M. Strassnig. 2009. Unruly ethics: On the difficulties of a bottom-up approach to ethics in the field of genomics. *Public Understanding of Science*. Published on November 11, 2008 as doi:10.1177/0963662507079902.

Felt, U., M. Fochler, A. Müller, M. Strassnig, and P. Winkler. 2006. Transferable standardized packages or travel-weary local configurations? On the situatedness of public engagement. *ERA SAGE Conference 'Comparing ELSA'*, November 17-18: Basel, CH.

Felt, U., M. Fochler, and M. Strassnig. 2003. Evaluierung des 'Diskurstag Gendiagnostik'. Analyse der dialogischen Wechselwirkung zwischen Wissenschaft und verschiedenen Öffentlichkeiten. Evaluierungsbericht. Vienna: Department of Social Studies of Science, University of Vienna. Available at http://www.univie.ac.at/virusss/documents/227195477.pdf (accessed August 12, 2008).

Felt, U., and B. Wynne 2007. Taking European knowledge society seriously. Report to the Expert Group on Science and Governance to the Science, Economy and Society Directorate, Directorate-General for Research. Brussels: European Commission.

Festing, S. 2005. The animal research debate. *The Political Quarterly* 76 (4):568-572.

Fleetwood, J., and S. S. Unger. 1994. Institutional ethics committees and the shield of immunity. *Annals of Internal Medicine* 120 (4):320-325.

Fochler, M. 2007. Participating in which kind of governance? Tracing bottom-up perspectives on the governance of science and the possibilities and limits of public engagement. Dissertation, Department of Social Studies of Science, University of Vienna, Vienna.

Fochler, M., and A. Müller. 2006. Vom Defizit zum Dialog? Zum Verhältnis von Wissenschaft und Öffentlichkeit in der europäischen und österreichischen Forschungspolitik. Wien: Institut für Technikfolgenabschätzung, Österreichische Akademie der Wissenschaften. Available at http://epub.oeaw.ac.at/0xc1aa500d_0x001372da (accessed August 12, 2008).

Fox, R. C. 1976. Advanced medical technology. Social and ethical implications. *Annual Review of Sociology* 2:231-268.

Fox, R. C., and R. DeVries. 1998. Afterword: The sociology of bioethics. In *Bioethics and society. Constructing the ethical enterprise*, edited by R. DeVries and J. Subedi, 270-276. Upper Saddle River, NJ: Prentice Hall.

Fox, R. C., and J. P. Swazey. 1984. Medical morality is not bioethics – Medical ethics in China and the United States. *Perspectives in Biology and Medicine* 35:336-360.

Funtowicz, S., and J. Ravetz. 1993. Science for the post-normal age. *Futures* 25 (7):735-755.

Galison, P. 1999. Trading zone. Coordinating action and belief. In *The science studies reader*, edited by M. Biagioli, 137-160. London and New York: Routledge.

Gerold, R., and A. Liberatore. 2001. Report of the Working Group 'Democratising Expertise and Establishing Scientific Reference Systems'. Brussels: European Commission. Available at http://europa.eu.int/comm/governance/areas/group2/report_en.pdf (accessed June 20, 2007).

Gibbons, M. 1994. The emergence of a new mode of knowledge production. In *Social studies of science in an international perspective. Proceedings of a workshop, University of Vienna, 13-14 January 1994*, edited by U. Felt and H. Nowotny, Vienna.

———. 2000. Mode 2 society and the emergence of context-sensitive science. *Science and Public Policy* 27 (3):159-163.

Gibbons, M., C. Limoges, H. Nowotny, S. Schwartzman, P. Scott, and M. Trow. 1994. *The new production of knowledge: The dynamics of science and research in contemporary societies*. London: Sage.

Gieryn, T. 1999. *Cultural boundaries of science: Credibility on the line*. Chicago: University of Chicago Press.

Gieryn, T. F. 1995. Boundaries of science. In *Handbook of science and technology studies*, edited by S. Jasanoff, G. E. Markle, J. C. Petersen and T. Pinch, 393-433. Thousand Oaks, London and New Delhi: Sage.

Gmeiner, R., and U. H. J. Körtner. 2002. Die Bioethikkommission beim Bundeskanzleramt - Aufgaben, Arbeitsweise, Bedeutung. *Recht der Medizin* 6 (November):164-173.

Godin, B. 1998. Writing performative history: The new *New Atlantis*? *Social Studies of Science* 28 (3):465-483

———. 2003. The knowledge-based economy. Conceptual framework or buzzword? Project on the history and sociology of S&T statistics. Working paper No. 24. Montreal: Canadian Science and Innovation Indicators Consortium (CSIIC). Available at http://www.csiic.ca/PDF/Godin_24.pdf (accessed March 19, 2008).

———. 2006. The linear model of innovation. The historical construction of an analytical framework. *Science, Technology, & Human Values* 31 (6):639-667.

Goldenberg, M. J. 2005. Evidence-based ethics? On evidence-based practice and the "empirical turn" from normative bioethics. *BMC Medical Ethics* 6 (11):1-9.

Gottweis, H. 2001. Die unsichtbare Kommission. *Der Standard*, December 13.

Gottweis, H., and R. Triendl. 2006. South Korean policy failure and the Hwang debacle. *Nature Biotechnology* 24 (2):141-143.

Groß, M., H. Hoffmann-Riem, and W. Krohn. 2005. *Realexperimente. Ökologische Gestaltungsprozesse in der Wissensgesellschaft*. Bielefeld: transcript.

Haas, P. M. 1992. Introduction: Epistemic communities and international policy coordination. *International Organization* 46 (1):1-35.

Hagendijk, R., and A. Irwin. 2006. Public deliberation and governance. Engaging with science and technology in contemporary Europe. *Minerva* 44 (2):167-84.

Hagendijk, R. P. 2004. The public understanding of science and public participation in regulated worlds. *Minerva* 42 (1):41-59.

Haimes, E. 2002. What can the social sciences contribute to the study of ethics? Theoretical, empirical and substantive considerations. *Bioethics* 16 (2):89-113.

Haimes, E., and R. Williams. 2007. Sociology, ethics, and the priority of the particular: Learning from a case study of genetic deliberations. *The British Journal of Sociology* 58 (3):457-476.

Haraway, D. 2008. *When species meet*. Minneapolis and London: University of Minnesota Press.

Harvey, M. 2008. Drama, talk, and emotion. Omitted aspects of public participation. *Science, Technology, & Human Values*. Published on February 1, 2008 as doi:10.1177/0162243907309632.

Hausman, D. M. 2004. Polling and public policy. *Kennedy Institute of Ethics Journal* 14 (3):241-247.

Hedgecoe, A. 2004. Critical bioethics: Beyond the social science critique of applied ethics. *Bioethics* 18 (2):120-43.

Hedgecoe, A. M. 2006. Context, ethics and pharmacogenetics. *Studies in History and Philosophy of Biological and Biomedical Sciences* 37 (3):566-582.

Henwood, F., S. Wyatt, A. Hart, and J. Smith. 2003. 'Ignorance is bliss sometimes': Constraints on the emergence of the 'informed patient' in the changing landscapes of health information. *Sociology of Health & Illness* 25 (6):589-607.

Herrera, C. 2008. Is it time for bioethics to go empirical? *Bioethics* 22 (3):137-146.

Hirsch, H., and H. Nowotny. 1977. Europe's nuclear debate (1): Austria: a case study. *Nature* 266 (5598):107-108.

Hoeyer, K. 2006a. 'Ethics wars': Reflections on the antagonism between bioethicists and social science observers of biomedicine. *Human Studies* 29 (2):203-227.

———. 2006b. The power of ethics: A case study from Sweden on the social life of moral concerns in policy processes. *Sociology of Health & Illness* 28 (6):785-801.

Hoeyer, K. L. 2004. Biobanks and informed consent. An anthropological contribution to medical ethics. Dissertation. Umeå: Medical Ethics, Department of Public Health and Clinical Medicine, Umeå University.

Hoeyer, K. L., and R. Tutton. 2005. "Ethics was here": Studying the language-games of ethics in the case of UK Biobank. *Critical Public Health* 15 (4):385-397.

Hoffmaster, B., ed. 2001. *Bioethics in social context*. Philadelphia: Temple University Press.

Hope, T. 1999. Empirical medical ethics. *Journal of Medical Ethics* 25 (3):219–220.

Horlick-Jones, T., J. Walls, G. Rowe, N. Pidgeon, W. Poortinga, and T. O'Riordan. 2004. A deliberative future? An independent evaluation of the *GM Nation?* public debate about the possible commercialization of transgenic crops. Understanding Risk Working Paper 04-02. Norwich: University of East Anglia. Available at http://www.pgeconomics.co.uk/pdf/gm_future_top_copy_12_feb_04.pdf (accessed April 12, 2007).

Horst, M. 2007. Public expectations and their performative effects on scientific citizenship. *Science, Technology, & Human Values* 32 (2):150-171.

International Council for Science (ICSU). 2005. Science and society: Rights and responsibilities. Paris: International Council for Science. Available at http://www.icsu.org/Gestion/img/ICSU_DOC_DOWNLOAD/825_DD_FILE_Science-and-Society.pdf (accessed December 14, 2007).

Irwin, A. 1995. *Citizen science. A study of people, expertise and sustainable developement*. London: Routledge.

———. 2001. Constructing the scientific citizen: Science and democracy in the biosciences. *Public Understanding of Science* 10 (1):1-18.

———. 2006. The politics of talk: Coming to terms with the 'new' scientific governance. *Social Studies of Science* 36 (2):299-320.

Ives, J. 2008. 'Encounters with experience': Empirical bioethics and the future. *Health Care Analysis* 16:1-6.

Jasanoff, S. 1999. The Songlines of Risk. *Environmental Values* 8:135-152.

———. 2003a. Breaking the waves in science studies: Comment on H.M. Collins and Robert Evans, 'The third wave of science studies'. *Social Studies of Science* 33 (3):389-400.

———. 2003b. (No?) accounting for expertise. *Science and Public Policy* 30 (3):157-162.

———. 2003c. Technologies of humility: Citizen participation in governing science. *Minerva* 41 (3):223-244.

———. 2005. *Designs on nature. Science and democracy in Europe and the United States*. Princeton & Oxford: Princeton University Press.

Joss, S., and S. Bellucci, eds. 2002. *Participatory technology assessment: European perspectives*. Gateshead: Athenaeum Press.

Kafatos, F. C. 2007. Keynote speech. Session 1: The ERC: A new impulse for top-level research in Europe. *Excellence in Research through Competition. ERC Launch Conference*, February 27/28, 2007: Berlin.

Kelly, S. E. 2003. Public bioethics and publics: Consensus, boundaries, and participation in biomedical science policy. *Science, Technology, & Human Values* 28 (3):339-364.

Knorr Cetina, K. 1999. *Epistemic cultures. How the sciences make knowledge* Cambridge, MA: Harvard University Press.

Kohler, R. 1994. *Lord of the fly. Drosophila genetics and the experimental life*. Chicago: Chicago University Press.

Körtner, U. 2008a. Stammzellenforschung: Diskussionstand in Österreich. Available at Science.ORF.at, http://science.orf.at/science/koertner/150490 (accessed January 16, 2008).

———. 2008b. Wissenschaft und Öffentlichkeit. Welche gesellschaftliche Rolle spielt die Wissenschaftsethik? Available at Science.ORF.at, http://science.orf.at/science/koertner/53171 (accessed July 9, 2008).

Laclau, E. 1996. Why do empty signifiers matter to politics? In *Emancipation(s)*, edited by E. Laclau, 34-46. London: Verso.

LaFollette, M. C. 1990. *Making science our own. Public images of science 1910-1955*. Chicago and London: University of Chicago Press.

Lambert, H., and H. Rose. 1996. Disembodied knowledge? Making sense of medical science. In *Misunderstanding science? The public reconstruction of science and technology*, edited by A. Irwin and B. Wynne, 65-83. Cambridge: Cambridge University Press.

Lanzerath, D. 2006. Research ethics and the problem how to teach it. *Acta Medica Lituanica* 13 (1):12-20.

Latour, B. 1987. *Science in action. How to follow scientists and engineers through society*. Cambridge, MA: Harvard University Press.

———. 1993. *We have never been modern*. Cambridge, MA: Harvard University Press.

———. 1996. Porträt eines Biologen als wilder Kapitalist. In *Der Berliner Schlüssel. Erkundungen eines Liebhabers der Wissenschaften*, 113-144. Berlin: Akademie Verlag.

———. 1998. From the world of science to the world of research? *Science* 280 (5361):208-209.

———. 2004. *Politics of nature: How to bring the sciences into democracy*. Cambridge, MA: Harvard University Press.

Law, J. 2004. *After method. Mess in social science research*. New York and London: Routledge.

Lenhard, J., H. Lücking, and H. Schwechheimer. 2006. Expert knowledge, Mode-2 and scientific disciplines: Two contrasting views. *Science and Public Policy* 33 (5):341-350.

Levidow, L. 2001. Utilitarian bioethics? Market fetishism in the GM crops debate. *New Genetics and Society* 20 (1):75-84.

Levidow, L., and S. Carr. 1997. How biotechnology regulation sets a risk/ethics boundary. *Agriculture and Human Values* 14 (1):29-43.

Levitt, M. 2003. Public consultation in bioethics. What's the point of asking the public when they have neither scientific nor ethical expertise? *Health Care Analysis* 11 (1):15-25.

Lévy-Leblond, J.-M. 1992. About misunderstandings about misunderstandings. *Public Understanding of Science* 1 (1):17-21.

Loewy, E. H. 2003. Education, practice and bioethics: Growing barriers to ethical practice. *Health Care Analysis* 11 (2):171-179.

López, J. 2004. How sociology can save bioethics…maybe. *Sociology of Health and Illness* 25 (7):875-96.

Luhmann, N. 1992. *Die Wissenschaft der Gesellschaft*. Frankfurt am Main: Suhrkamp.

Lynch, M. 1988. Sacrifice and the transformation of the animal body into a scientific object. *Social Studies of Science* 18 (2):265-289.

Marshall, P. A. 1992. Anthropology and bioethics. *Medical Anthropology Quarterly* 6 (1):49-73.

McKenzie Stevens, S. 2007. Speaking out: Toward an institutional agenda for refashioning STS scholars as public intellectuals. *Science, Technology, & Human Values*. Published on December 19, 2007 as doi:10.1177/0162243907310162.

McLuhan, M. 1994. *Understanding media. The extensions of man*. Cambridge and London: The MIT Press.

Merton, R. K. 1938. Science and the social order. *Philosophy of Science* 5 (3):321-337.

———. 1973. *The sociology of science: Theoretical and empirical investigations*. Chicago: University of Chicago Press.

Merton, R. K., and E. Barber. 2004. *The travels and adventures of serendipity. A study in sociological semantics and the sociology of science*. Princeton: Princeton University Press.

Michael, M. 1992. Lay discourses of science: Science-in-general, science-in-particular, and self. *Science, Technology, & Human Values* 17 (3):313-333.

———. 1996. Ignoring science: Discourses of ignorance in the public understanding of science. In *Misunderstanding Science? The public reconstruction of science and technology*, edited by A. Irwin and B. Wynne, 107-125. Cambridge: Cambridge University Press.

———. 1998. Between citizen and consumer: Multiplying the meanings of the 'public understanding of science'. *Public Understanding of Science* 7:313-327.

———. 2001. Technoscientific bespoking: Animals, publics and the new genetics. *New Genetics and Society* 20 (3):205-224.

———. 2006. *Technoscience and everyday life*. Maidenhead and New York: Open University Press.

Michael, M., and L. Birke. 1994. Accounting for animal experiments: Identity and disreputable 'others'. *Science, Technology, & Human Values* 19 (2):189-204.

Michael, M., and N. Brown. 2005. Scientific citizenships: Self-representations of xenotransplantation's publics. *Science as Culture* 14 (1):39-57.

Mol, A. 2002. *The body multiple. Ontology in medical practice*. Durham: Duke University Press.

Nature. 2002. Timeline: The mouse genome. *Nature* 420 (6915):510.

Nelson, J. L. 2000. Moral teachings from unexpected quarters. Lessons for bioethics from the social sciences and managed care. *Hastings Center Report* 30 (1):12-17.

Noelle-Neumann, E. 1979. Public opinion and the classical tradition. A re-evaluation. *The Public Opinion Quarterly* 43 (2):143-156.

Nowotny, H. 1979. *Kernenergie - Gefahr oder Notwendigkeit, Anatomie eines Konflikts*. Frankfurt/Main: Suhrkamp.

———. 2000. Transgressive competence: The narrative of expertise. *European Journal of Social Theory* 3 (1):5-21.

———. 2006. Real science is excellent science. How to interpret post-academic science, Mode 2 and the ERC. *Journal of Science Communication* 5 (4):1-3.

Nowotny, H., P. Scott, and M. Gibbons. 2001. *Re-thinking science. Knowledge and the public in an age of uncertainty*. Cambridge: Polity Press.

ORF ON Science. 2003. Forderungen der BürgerInnenkonferenz liegen vor. Available at http://science.orf.at/science/news/79025 (accessed November 11, 2008).

Österreichischer Forschungsdialog. 2008. Joint Venture Veranstaltung des Österreichischen Forschungsdialoges: Ethik in der Forschung, 26. Mai, Wien. Bericht des Bundeskanzleramtes. Vienna: bmwf.

Österreichisches Lebensministerium, and Bundeskanzleramt der Republik Österreich. 2008. Standards der Öffentlichkeitsbeteiligung. Empfehlungen für die gute Praxis. Wien: Österreichisches Lebensministerium and Bundeskanzleramt der Republik Österreich.

Parker, M. 2007. Ethnography/ethics. *Social Science & Medicine* 65:2248-2259.

Power, M. 1994. *The audit explosion*. London: Demos.

Powers, M. 2005. Bioethics as politics: The limits of moral expertise. *Kennedy Institute of Ethics Journal* 15 (3):305-322.

Rader, K. A. 2004. *Making mice. Standardizing animals for American biomedical research*. Princeton: Princeton University Press.

Rat für Forschung und Technologieentwicklung. 2001. Vision 2005. Durch Innovation zu den Besten. Vienna: Rat für Forschung und Technologieentwicklung.

———. 2003. BürgerInnenkonferenz "Genetische Daten: woher, wohin, wozu?" Dokumentation und Stellungnahme des BürgerInnenpanels. Vienna: Rat für Forschung und Technologieentwicklung.

Rauprich, O. 2008. Common morality: Comment on Beauchamp and Childress. *Theoretical Medicine and Bioethics* 29 (1):43-71.

Ravetz, J. R. 1999. What is post-normal science. *Futures* 31:647-653.

Rose, N. 1999a. Inventiveness in politics. *Economy and Society* 28 (3):467-493.

———. 1999b. *Powers of freedom: Reframing political thought*. Cambridge: Cambridge University Press.

———. 2000. Community, citizenship, and the Third Way. *American Behavioral Scientist* 43 (9):1395-1411.

Rowe, G., and L. J. Frewer. 2000. Public participation methods: A framework for evaluation. *Science, Technology, & Human Values* 25 (1):3-29.

———. 2004. Evaluating public-participation exercises: A research agenda. *Science, Technology, & Human Values* 29 (4):512-556.

———. 2005. A typology of public engagement mechanisms. *Science, Technology, & Human Values* 30 (2):251-290.

Rowe, G., R. Marsh, and L. J. Frewer. 2004. Evaluation of a deliberative conference. *Science, Technology, & Human Values* 29 (1):88-121.

Salter, B., and M. Jones. 2005. Biobanks and bioethics: The politics of legitimation. *Journal of European Public Policy* 12 (4):710-732.

Salter, B., and C. Salter. 2007. Bioethics and the global moral economy. The cultural politics of human embryonic stem cell science. *Science, Technology, & Human Values* 32 (5):554-581.

Shamir, R. 2008. The age of responsibilization: On market-embedded morality. *Economy and Society* 37 (1):1-19.

Shapin, S. 1995. Trust, honesty and the authority of science. In *Society's choices: Social and ethical decision making in bioethics*, edited by R. E. Bulger, E. M. Bobby and H. V. Fineberg, 388-408. Washington, DC: National Academy Press.

Shinn, T. 2002. The triple helix and new production of knowledge: Prepackaged thinking on science and technology. *Social Studies of Science* 32 (4):599-614.

Smith-Doerr, L. 2004. Learning to reflect or deflect? U.S. policies and graduate programs' ethics training for life scientists. Boston, MA: Boston University. Available at http://people.bu.edu/ldoerr/reflectordeflect.pdf (accessed November 11, 2008).

———. 2008. Decoupling policy and practice: How life scientists respond to ethics education. *Minerva* 46 (1):1-16.

Steyaert, S., and H. Lisoir, eds. 2005. *Participatory methods toolkit. A practitioner's manual*. Brussels and Vienna: King Baudouin Foundation and the Flemish Institute for Science and Technology Assessment (viWTA).

Stirling, A. 2008. 'Opening up' and 'closing down'. Power, participation, and pluralism in the social appraisal of technology. *Science, Technology, & Human Values* 33 (2):262-294.

Strauss, A., and J. Corbin. 2000. *Basics of qualitative research: Techniques and procedures for developing Grounded Theory*. Thousand Oaks: Sage.

Sugarman, J., R. Faden, and J. Weinstein. 2001. A decade of empirical research in bioethics. In *Methods in medical ethics*, edited by J. Sugarman and D. R. Sulmasy, 19-33. Washington: Georgetown University Press.

Tallacchini, M. 2006. Politics of ethics and EU citizenship. *Politeia* XXII (83):101-13.

The Royal Society. 1985. The public understanding of science. Report of a Royal Society ad hoc Group endorsed by the Council of the Royal Society. London: The Royal Society.

Torgersen, H., and F. Seifert. 1997. Aversion preceding rejection: Results of the Eurobarometer Survey 39.1 on biotechnology and genetic engineering in Austria. *Public Understanding of Science* 6 (2):131-142.

Turner, J. Z. 1998. I don't want to see the pictures: Science writing and the visibility of animal experiments. *Public Understanding of Science* 7 (1):27-40.

Turner, L. 2003. Zones of consensus and zones of conflict: Questioning the 'common morality' presumption in bioethics. *Kennedy Institute of Ethics Journal* 13 (3):193-218.

UK House of Lords Select Committee on Science and Technology. 2000. Science and technology - Third report. London: House of Lords. Available at http://www.publications.parliament.uk/pa/ld199900/ldselect/ldsctech/38/3801.htm (accessed April 16, 2008).

van der Scheer, L., and G. Widdershoven. 2004. Integrated empirical ethics: Loss of normativity? *Medicine, Health Care and Philosophy* 7 (1):71-79.

von Schomberg, R. 2007. From the ethics of technology towards an ethics of knowledge policy & knowledge assessment. A working document from the European Commission Services. Brussels: European Commission. Available at http://ec.europa.eu/research/science-society/pdf/ethicsofknowledgepolicy_en.pdf (accessed February 12, 2007).

Wainwright, S. P., C. Williams, M. Michael, B. Farsides, and A. Cribb. 2006. Ethical boundary-work in the embryonic stem cell laboratory. *Sociology of Health & Illness* 28 (6):732-748.

Waterton, C. 2005. Scientists' conceptions of the boundaries between their own research and policy. *Science and Public Policy* 32 (6):435-444.

Weber, M. 1995. *Wissenschaft als Beruf*. Ditzingen: Reclam.

Webster, A. 2007. Crossing boundaries: Social science in the policy room. *Science, Technology, & Human Values* 32 (4):458-478.

Weingart, P. 1997. From 'finalization' to 'mode 2': Old wine in new bottles? *Social Science Information* 36 (4):591-613.

———. 1999. Neue Formen der Wissensproduktion: Fakt, Fiktion und Mode. *TA-Datenbank-Nachrichten* 8 (3/4):48-57.

———. 2001. *Die Stunde der Wahrheit? Zum Verhältnis der Wissenschaft zu Politik, Wirtschaft und Medien in der Wissensgesellschaft*. Weilerswist: Velbrück Wissenschaft.

Weingart, P., and P. Pansegrau. 1999. Reputation in science and prominence in the media: The Goldhagen debate. *Public Understanding of Science* 8 (1):1-16.

White, P. S. 2005. The experimental animal in Victorian Britain. In *Thinking with animals. New perspectives on anthropomorphism*, edited by L. Daston and G. Mitman, 59-81. New York: Columbia University Press.

Wilsdon, J., and R. Willis. 2004. *See-through science: Why public engagement needs to move upstream*. London: Demos.

Wilsdon, J., B. Wynne, and J. Stilgoe. 2005. *The Public value of science. Or how to ensure that science really matters*. London: Demos.

Wullweber, J. 2008. Nanotechnology – An empty signifier *à venir*? A delineation of a techno-socio-economical innovation strategy. *Science, Technology & Innovation Studies* 4 (1):27-45.

Wynne, B. 1991. Knowledges in context. *Science, Technology, & Human Values* 16 (1):111-121.

———. 1992. Misunderstood misunderstanding: Social identities and public uptake of science. *Public Understanding of Science* 1 (3):281-304

———. 1993. Public uptake of science: A case for institutional reflexivity. *Public Understanding of Science* 2 (4):321-337.

———. 1996a. May the sheep safely graze? A reflexive view of the expert-lay knowledge divide. In *Risk, environment and modernity: Towards a new ecology*, edited by B. Szerszynski, S. Lash and B. E. Wynne, 44-83. London: Sage.

———. 1996b. Misunderstood misunderstandings: Social identities and public uptake of science. In *Misunderstanding science? The public reconstruction of science and technology*, edited by A. Irwin and B. Wynne, 19-46. Cambridge: Cambridge University Press.

———. 2001. Creating public alienation: Expert cultures of risk and ethics on GMOs. *Science as Culture* 10 (4):445-481.

———. 2003. Seasick on the third wave? Subverting the hegemony of propositionalism: Response to Collins & Evans (2002). *Social Studies of Science* 33 (3):401-417.

———. 2007. Public participation in science and technology: Performing and obscuring a political-conceptual category mistake. *East Asian Science, Technology and Society: an International STS Journal* 1 (1):99-110.

Yesley, M. 2005. What's in a name? Bioethics—and human rights—at UNESCO. *Hastings Center Report* 35 (2):8.

Ziman, J. 1998. Why must scientists become more ethically sensitive than they used to be? *Science* 282 (5395):1813-4.

———. 2003. Non-instrumental roles of science. *Science and Engineering Ethics* 9 (1):17-27.

Zussman, R. 2000. The contributions of sociology to medical ethics. *Hastings Center Report* 30 (1):7-11.

13. Annex

13.1. List of abbreviations

AFM	Association Française contre les Myopathies
BSE	Bovine Spongiform Sncephalopathy
COMEST	World Commission on the Ethics of Scientific Knowledge and Technology
COMETH	The European Conference of National Ethics Committees
EGE	European Group on Ethics in Science and New Technologies
ELSA	Ethical, Legal, and Social Aspects
ELSI	Ethical, Legal, and Social Issues
ERC	European Research Council
ESRC	Economic and Social Research Council (UK)
EU	European Union
GEN-AU	GENome Research in AUstria
GM	Genetically Modified
GMO	Genetically Modified Organism
GOLD	Genomics of Lipid-associated Disorders
IBC	International Bioethics Committee
IRB	Institutional Review Board
IGBC	Intergovernmental Bioethics Committee
IT	Information Technologies
NEC Forum	Forum of National Ethics Councils
PR	Public Relations
PUS	Public Understanding of Science
R&D	Research and Development
RTF	Rat für Forschung und Technologieentwicklung (*Austrian Council for Research and Technology Development*)
STS	Science and Technology Studies
UK	United Kindom
UNESCO	United Nations Educational, Scientific and Cultural Organization
US	United States of America

13.2. Explanation of shortcuts in the quotations from the Round Table

L1-14: lay participants
S1-8: researchers regularly attending the Round Tables
S9-13: researchers invited for special presentations
E: invited ethicist

eA: ex ante: Interviews conducted before the Round Table started
eP: ex post: Interviews made after the Round Table ended

RT1-7: Round Table number 1 to 7
sgL1-2: small group discussions with lay participants only
sgS: small group discussion with researchers only

Example:
L11/RT1/2/23-27: Lay participant #11 at Round Table 1, discussion block 2, paragraphs 23-27
S3/eA/145: Researcher 3 from ex ante interview, paragraph 145

[ ]	additions by the author
…	ellipsis

13.3. Original quotations

---

[i] … die ‚BürgerInnenkonferenz' zu einem fixen Instrument bei der Behandlung brisanter Themen zu machen.

[ii] Nanotechnologie als neue Technik wirft keine grundsätzlich neuartigen ethischen Fragen auf.

[iii] Innovation ist dabei Motor der wirtschaftlichen Prosperität und bildet eine solide Basis für die soziale und kulturelle Weiterentwicklung dieser Gesellschaft. Diese Tatsache ist der Öffentlichkeit zu wenig bewußt und sollte der Bevölkerung durch ein Bündel entsprechender Maßnahmen verdeutlicht werden.

[iv] Instrumente partizipativer Demokratie sind freilich keine überzeugende Alternative. Sie berufen sich auf ‚das Volk', das in plebiszitären Kampagnen immer neu erfunden wird, um Politik als mediale und emotionalisierte Veranstaltung zu inszenieren. … Die Forderung nach stärkerer Einbindung der Öffentlichkeit in die bioethische und biopolitische Debatte ist demokratiepolitisch nachdrücklich zu unterstützen, stößt aber auf erhebliche Schwierigkeiten bei ihrer Einlösung. In der pluralistischen Gesellschaft gibt es nicht ‚die' Öffentlichkeit, sondern unterschiedliche Öffentlichkeiten, die einander nur zum Teil wahrnehmen und sich nur selten zu der einen großen Öffentlichkeit zusammenführen lassen.

[v] Was ‚die' Öffentlichkeit denkt, ist schwer zu erfassen. Der Versuch, in diese ‚Black Box' einzudringen, ist das Arbeitsfeld der Meinungsforschung, die freilich auch nur Konstrukte von Öffentlichkeit liefern kann.

[vi] Allerdings sei dabei wichtig, dass die systematische Herangehensweise in der Reflexion nicht verloren gehe. Ethik-Diskussionen sollten nicht auf ‚Stammtisch-Niveau' ausgetragen werden.

[vii] Das Problem, an dem wir arbeiten, ist die neue Seuche, und zwar das ist die Fettleibigkeit und der Grund, warum wir so wenig davon wissen, ist, dass es schwer zu fassen ist: es ist eine sehr komplexe Erkrankung.

[viii] Als Grundlagenforscher sind wir immer noch – interessiert mich zumindest – einmal primär schon der Erkenntnisgewinn – ich will wissen, wie Fettstoffwechsel funktioniert ...

[ix] … wir haben sozusagen über diese Programme von der Öffentlichkeit das Angebot gekriegt, wir als Öffentlichkeit bieten euch viel Geld für etwas, was uns interessiert. Und wir haben uns eingeklinkt.

[x] Also, wenn man, wenn Sie schauen, wie viele Leute die Brigitte-Diät kaufen oder solche Dinge oder bei Slim Fast mehr oder weniger. manchen ist es anscheinend ein Bedürfnis. Wenn Sie, also wenn Sie am Regal vorbeigehen mit Frauenzeitschriften, steht überall am Titelblatt irgendeine neue Diät. Auf den Bestsellerlisten der Sachbücher ist die South-Beach Diät an erster Stelle und die Atkins-Diät an zweiter Stelle und zwar Monate lang.

[xi] ... die Definition von Krankheit [ist] für mich auch fragwürdig, und dann ist als nächstes dann gekommen, na ja, wir wissen, dass ein Großteil von denen, die, der

---

Adipositas eine genetische Erkrankung ist. Wenn ich jetzt sage, also, ein Großteil der Bevölkerung ist adipös, das Ganze ist eine genetische Erkrankung, dann spreche ich den Großteil der Bevölkerung per Definition auf einmal krank.

[xii] Eigentlich in einer Wohlstandsgesellschaft, wo ein riesiges Angebot an vielem gibt, ich, als Übergewichtige, auf etwas verzichten muss, was mir schmeckt. Ich glaube, dass dieser psychische Bereich … auch eine Rolle spielt. Was mir schmeckt, muss ich verzichten, um mein Gewicht zu reduzieren … Also, einerseits … ich habe das Dilemma, auf etwas zu verzichten, und andererseits haben wir das riesige Angebot, diese verführerische Kraft des Angebotes. Und wo finde ich jetzt die Mitte? Und das hat schon mit der Psyche etwas zu tun.

[xiii] Und zwar glaube ich, dass, nicht alle, keine Frage, aber ein sehr großer Teil der Bevölkerung eine sehr ähnliche, nicht die gleiche, aber eine sehr ähnliche Auffassung von gutem Leben hat.

[xiv] Andererseits gibt's sozusagen [das] Argument, Leute und so, die sagen, es gibt also nichts Schlimmeres als diesen Jugend-, Schlankheits-, Abnehmwahn … Und es gibt ja das Sprichwort, … die Dicken sind gemütlich. Also sind das sozusagen keine Argumente, die man gegen das hernehmen kann, dass ich sage, ich will gar nicht dünner oder ich will gar nicht schlank werden. Die wollen gar nicht abnehmen.

[xv] Jetzt gibt es verschiedene Möglichkeiten, wie ich das lösen kann. Der Einfachste ist, … ja, geh ins Studio, mach ein bisschen einen Sport und iss ein bisschen weniger, ein Apferl am Tag, wird dir ja wohl reichen, ned! … das funktioniert anscheinend nicht, sonst hätten wir das Problem nicht, wenn das so einfach auch wäre. Eine kurzfristige Änderung ist in den meisten Fällen in irgendeiner Form zu erreichen, nur langfristig funktioniert es einfach nicht. Das heißt, es wäre das Gesündeste, es ist das Beste, es ist vor allem das Billigste, und trotzdem funktioniert es nicht. Das heißt, es muss andere Möglichkeiten geben.

[xvi] … wir möchten einmal diese Fett spaltenden Gene finden, die wir nicht kennen.

[xvii] Und dann kann man nicht mehr so schnell damit aufhören. Und ich denke, mit der Fettpille wäre es dasselbe, weil natürlich nicht alle schlank sein wollen, ja. Ich meine, [in] manchen Kulturen sollen die Frauen gar nicht schlank sein. Und wenn jetzt die Fettpille dort auf den Markt eben geworfen werden würde gemeinsam mit westlichen oder europäischen Schönheitsidealen, würde das wahrscheinlich fatale Konsequenzen haben.

[xviii] Jetzt macht man die Genforschung in Fettstoffwechselstörung, man findet ein Medikament, das behebt oder irgendein Verfahren, es muss ja nicht unbedingt ein Medikament sein, sondern Verfahren, wo man das beheben und beherrschen kann, und die Leute werden schlank, das hat den Erfolg, dass die Krankenkassa sich viel Geld erspart. Und dann könnten's ja - und jetzt kommen wir zu Ethik – dann könnten's auf die Idee kommen, dass man das verwenden muss.

[xix] Es wird aller Voraussicht nach nie so sein, … dass jeder in der Früh seine Fettpille einwirft und alle rennen herum mit Idealmaßen. DAS wird aller Voraussicht nach, dass es ganz nebenwirkungsfrei geht …, und ich nehme das vorbeugend ein Leben lang - solche Medikamente gibt's quasi nicht. … wenn ich eingreife in einen Stoffwechselweg, dann bringe ich viel durcheinander, immer. Ich kann versuchen, das zu minimieren und meine Nebenwirkungen herabzusetzen, aber dass ich ein

---

vorbeugendes Mittel habe, das wirklich so effizient ist, das ist mehr als unwahrscheinlich.

[xx] Schaut man da jetzt mehr auf Erkenntnis und Wissen oder sind wirtschaftliche Interessen im Vordergrund?

[xxi] … die Gesellschaft, die das Geld gibt, die hat ja nicht die Malaria.

[xxii] … wo’s in ganz Europa nur, ich weiß nicht, 500 Erkrankte gibt. Da gibt die Pharmaindustrie für die Forschung überhaupt nichts aus, weil da ist kein Geschäft zu machen.

[xxiii] … hier Unterscheidungen [zu] treffen ... in der Grundlagenforschung ist das nicht der Fall und soll auch nicht der Fall sein, um Gottes Willen.

[xxiv] Wo ich als Steuerzahler irgendwie das Empfinden habe, ich finanziere einen Riesen-Apparat und ich erwarte mir etwas davon - da muss etwas rauskommen, was mein Leben besser macht, weil es ist ja mein Geld dann, ja.

[xxv] Das Gesundheitswesen will eine Tablette haben, dass die Stoffwechselstörung runtergeht, dass die Kosten runtergehen. Das ist ja noch immer gut. Dann gehen’s runter, nur … dann würden wir verpflichtet diese Pille alle Tage zu nehmen. Da ist Zwangsverpflichtung, … du musst alle Tage zwei Sortis schlucken, weil usw. … Und das ist meiner Meinung nach ein Ethikproblem. Muss ich den zwangsverpflichten, dass er nicht dick wird, oder ist es seine freie Entscheidung, dick zu sein? Ob er vielleicht dafür die gewisse Selbstbehalte bei der Krankenversorgung oder Sozialversorgung dafür selber zahlen muss, ist vielleicht diskussionswert, aber wo bleibt die Freiheit?

[xxvi] … wenn er viel isst, zahlt er ja auch viel Mehrwertsteuer.

[xxvii] Ja, du kannst ja im Endeffekt genauso entscheiden, dass du jetzt kein Fast Food isst oder kein fettes Essen, wie du dich entscheiden kannst später, was weiß ich, nicht die Fettpille zu essen.

[xxviii] Das heißt, die Verhaltensänderungen sind eine sehr problematische Angelegenheit und damit wird’s absolut notwendig sein … hier in anderer Weise auch einzugreifen.

[xxix] Ja, das ist der Anfang eigentlich, nicht bei dem Arzt oder nicht bei dem Forscher die Verantwortung suchen, sondern für sich selbst die Verantwortung. Warum bin ich dorthin gelangt, einen Arzt aufzusuchen? Das ist das Problem. Warum habe ich zugenommen? Weil es so viel, riesiges Angebot an diesem und jenem gibt? Nein, weil vielleicht etwas mit meiner Disziplin nicht funktioniert. Also, nicht der andere ist schuld.

[xxx] Die Eigenverantwortung für sich selber … das zu lernen, Eigenverantwortung zu tragen, einmal in kleinster Form für mich selber, dann für die Familie, und die Gesellschaft kommt erst, ja, wesentlich weiter, nicht. Also, für mich selber, für Geschwister, Familie, Freunde dann, und dann kommt irgendwann die Gesellschaft. Aber das Ganze, die Keimzelle ist die Eigenverantwortung.

[xxxi] … wir haben uns das so gedacht. Zum Beispiel McDonalds auf jeden Burger … oder irgendwelche Firmen, die halt … ungesunde Produkte herstellen … also der Kunde zahlt halt mehr, ist egal, 50 Cents mehr für den Burger, so und so. Und diese 50 Cents dieser Fettsteuer gehen natürlich an sinnvolle Dinge, wie zum Beispiel

---

Vorsorgeförderung der Gesellschaft, Forschungsförderung und Verhaltensänderung.

[xxxii] … weil ich halt einfach als Forscher ganz stark trenne zwischen dem, was ich im Labor mache, wo ich vom Gesetz her gezwungen bin, das abzuschotten, nichts rauszulassen, dafür zu sorgen, dass meine Mutanten, dass alles, was damit ist, wirklich im Labor ist, und erst dann, wenn's zerstört ist und mir kein Unheil anrichten kann, nach außen kommt, ich in einem ganz einem anderem, in einer ganz einer anderen Position bin, als ein Pflanzenphysiologe, der seinen Mais auspflanzt, als ein, irgendeine Firma, was auch immer, die gentechnisch manipulierten Kartoffeln, Tomaten oder was auch immer rausbringt, aufs freie Feld, wo ich unmittelbar, nicht nur als Konsument, sondern auch in meiner Umwelt damit konfrontiert bin. Aber ich als Natur-, als Laborwissenschaftler, wenn ich mich so bezeichnen darf, bin in einer ganz einer anderen Position.

[xxxiii] … ich will wissen, wie Fettstoffwechsel funktioniert und mich interessiert es wahnsinnig, wie dieser Fettstoffwechsel mit Diabetes zusammenhängt.

[xxxiv] Ich habe da, bei unseren Forschungszielen haben wir das so unterteilt, und zwar wir haben die unmittelbaren Ziele, die unsere sind im Labor. Unsere unmittelbaren Ziele sind Gene finden und Stoffwechselwege aufklären. Dann gibt's die mittelbaren Ziele und die langfristigen Ziele. Mittelbares Ziel könnte man sagen, jemand greift das auf, um zu forschen, ob man bestimmte Substanzen entwickeln kann, mit denen dieses Gen oder die Produkte daraus so manipulieren kann, dass jemand dünn oder dick wird oder auch um eine ganz eine Krankheit heilen. ... Und die langfristigen Ziele wären dann eben die Reduzierung der Fettleibigkeit, Reduzierung von Arteriosklerose, Herzinfarkt, Krebserkrankungen, etc. Nur, diese mittel- und langfristigen Ziele sind nicht die Ziele von GOLD. Die sind die gesellschaftlichen Ziele, die andere dann auch machen, die machen wir nicht selber. Wir könnten es nicht einmal machen.

[xxxv] … der Laser ist nicht erfunden worden, damit ich einen Laserbeamer habe und … meine Augen abscannen lassen kann. ... Das sind Anwendungen, die total unabsehbar waren, das ist nicht wegen einer Anwendung entwickelt worden … per se, man kann dann sagen, ich kann dann abschätzen, was ich alles damit machen werde können. Das ist glaube ich, zu viel verlangt.

[xxxvi] … es wird immer die Möglichkeit geben, die Dinge negativ einzusetzen. Die Frage ist dann, muss es die Forschung sein, die ich unterbinde oder sollen's mögliche negative Anwendungen sein, die ich versuche zu unterbinden?

[xxxvii] Ist Darin schuld an Euthanasie?

[xxxviii] Jemand, der Kampfgase haben möchte, wird eh nicht drauf warten, dass wer anders das entwickelt und der das Beiprodukt nehmen kann. Er wird's im schlimmsten Fall selber machen. Also, ich kann, jemand, der negative Motive voraussetzt, den werde ich dadurch, dass es verboten ist, ja sowieso nicht abhalten davon.

[xxxix] Es wird eigentlich überall verlangt, wenn man um ein Projekt ansucht, dass man auch ein Formular ausfüllt, in dem genau steht, was man an ethisch relevanten Sachen vorhat, und warum man genau das machen will usw. Also, es wird eigentlich sehr streng überwacht.

[xl] … quasi moralisch geboten … Fettstoffwechsel zu beforschen. Es ist einfach ein Problem, es sterben die meisten Menschen, die häufigste Todesursache auf der ganzen Welt, also die unterernährten Länder da mit eingerechnet, ist einfach Auswirkungen von Fettleibigkeit.

[xli] Und jetzt ist für mich irgendwo ein bisschen die Dialektik ein bisschen schwierig – einerseits Wissensgewinn im Sinne von Grundlagenforschung zu wollen und andererseits einen gewissen spürbaren Begeisterung irgendwo auch, da die Anwendung in Form von Medikamenten, … von diversen Firmen zu sprechen, die Vision zu haben, es gibt da eine Pille. Also, … irgendwo habe ich Schwierigkeiten abzunehmen, dass es wirklich nur um das Wissen geht, nur um die akademische Forschung geht.

[xlii] … wenn ich halt an der Universität womöglich lehre oder so, dann bin ich doch irgendwie in hervorragender Stellung, und dann muss ich schon irgendwo auch, eben Verantwortung übernehmen oder ein Bekenntnis abliefern oder, ja, einfach über den Dingen stehen. Da kann ich nicht irgendwo vielleicht daheim einmal im stillen Kämmerlein mir Gedanken machen sondern da muss ich schon Position beziehen auch zu grundlegenden Fragen.

[xliii] Was macht die Politik, sie setzt eine Kommission zusammen und lässt sich beraten, oder?

[xliv] Die Politik ist letztlich ein Instrument auch oder wird auch von der Wirtschaft, glaube ich, vor sich hergeschoben.

[xlv] … wir nähern uns wieder diesem Kreislauf, der aus Politik, Medien, der Gesellschaft an sich, den Forschern, der Wirtschaft und den Ethikern besteht, die, ja, die immer mehr die Verantwortung aufeinander schieben.

[xlvi] ... wo jeder im Prinzip seine eigenen Parameter optimiert ...

[xlvii] Weil der Politiker, … weil was macht der? Der ist genauso jemand wie wir, der ist ja auch kein Fachmann und damit er eine Entscheidung treffen kann, lädt er Wissenschaftler ein, die ihn beraten. Und da sind wir dann schon wieder im Kreis, nicht? Der Politiker schafft die rechtlichen Rahmenbedingungen für den Wissenschaftler, der seinerseits wieder den Politiker beratet, damit er diese Rahmenbedingungen schaffen kann. Und so geht das im Kreis herum und letztendlich ist dann niemand mehr verantwortlich.

[xlviii] … die Frage der Verantwortung wird ja immer erst im Moment der Katastrophe gestellt.

[xlix] … wir haben diese Frage der Verantwortung diskutiert, und auch wir sind zu keinem eindeutigen Ergebnis gekommen.

[l] … Verantwortung tragen wir im Prinzip alle.

[li] … diese Frage nach der Verantwortung, das heißt, die Frage zu verhindern, dass irgendwas besonders Schlimmes passiert, vielleicht am besten dadurch zu verhindern wäre, dass man am Weg, den man geht, konsequent fragt, ob das, was man macht richtig oder falsch ist. Das heißt, dass die Gesellschaft in gewisser Hinsicht dafür sorgt, dass der Weg, den die Gesellschaft geht, permanent kritisch hinterfragt wird.

[lii] … regelmäßigen Denkanstöße von außen, das heißt, dass es gewisse Personen oder Organisationen gibt, die einfach kritisch hinterfragen, ob das, was passiert, das Richtige wäre.

[liii] Wir haben Mäuse als Modelltiere, die sehr gut zu verwenden sind, weil sie leicht, relativ leicht zu halten sind, und weil sie dem Menschen schon sehr nahe kommen, und der Hefe. Das Problem, das ich immer habe, ist: Je einfacher mein System ist, desto leichter kann ich damit arbeiten, aber desto weiter ist es natürlich vom Menschen entfernt.

[liv] … die Gene, die für diese Eiweißstoffe codieren, kann ein Tier ausknocken. Was auch geht, er kann den, die Zellen im Tier veranlassen, das Protein über zu exprimieren, das heißt, jetzt macht die Zelle viel mehr Eiweiß als sie sonst machen würde. Also man kriegt grundsätzliche biologische Informationen und auf der anderen Seite kriegt man eine Information darüber, welche Rolle spielen die Enzyme, wenn der ganze Fettstoffwechsel nicht richtig läuft.

[lv] Ja natürlich schon einerseits aufzeigen, was machen wir eigentlich, warum machen wir es, welche Ziele. Also dass wir es nicht einfach machen, weil wir Spaß daran haben irgendwelche Tiere abzutöten, also was durchaus nicht gegeben ist, sondern dass wir halt wirklich ein Ziel verfolgen, was wir natürlich für sinnvoll sehen und was wir natürlich auch … rübergeben will an andere, … Medikament gegen Arteriosklerose eventuell in Zukunft mal zu finden, finde ich, ist ein Ziel, das wahrscheinlich alle sehr interessant finden oder so geht es mir ja auch, dass einem dann ja die Leute sagen: das ist interessant und sie möchten es haben und wir sollen uns beeilen. Was dann halt letztendlich kommt. Und dann einfach aufzuzeigen, dass auch Genomforschung in diese Richtung gehen kann, also auch positive Sachen für den Menschen bringt.

[lvi] … die Mäuse, die sind unser Primärkapital, nicht. Sie können sich vorstellen, wenn ein Postdoc drei Jahre lang arbeiten muss, damit es zu so einer Maus kommt, die hat einen enormen Wert im Labor. Das ist also nichts, mit dem wir leichtsinnig … Es kostet eine gigantische Menge Geld. Die [Mäuse] sind, also, das wahre Gold, das wir dann im Endeffekt besitzen. Das heißt, die sind normalerweise besser versorgt als die Dissertanten oder … andere Mitglieder des Labors.

[lvii] Im Prinzip ist es ja ein bisschen meine Familie. Ja, die Mäuse bekommen dann so zwischen fünf und zehn Junge auf die Welt. Und, ich bin jetzt auch natürlich sehr neugierig, und ich habe schon geschaut im Tierstall und fast täglich gehe ich hinein weil am Anfang sind die Mäuse ohne Haare ganz fleischige kleine blinde Knöllchen. Und, sobald sie dann die Haare haben wird es für mich spannend weil dann ist es nämlich so wenn die Mäuse schwarz sind, ist es für mich schlecht und für die schwarze Maus schlecht.

[lviii] … weil beim Menschen gehe ich ja auch so vor.

[lix] Es arbeitet dann ja im Prinzip auch nicht wirklich jeder mit den Mäusen. Aber man sollte zumindest prinzipiell dazu bereit sein. … Na ja, es wird dann immer dazu führen, dass es die Gutmenschen gibt im Labor und die Mäusemörder. Und das ist ... gruppendynamisch sicher sehr ungünstig. … prinzipiell soll’s nicht so sein, dass es dann welche gibt, die sich als moralisch reiner betrachten als andere. Also, dafür, für solche Spannungen ist in einem Labor sicher kein Platz.

[lx] … ich sage, wenn jemand so reagiert „wann kann ich endlich meine erste Maus da... [umbringen]", der braucht einen Arzt. Der hat in einem Labor nichts verloren. Ich kenne keine Person, die mit großer Lust und Liebe sich freut, wenn sie das erste Mal in den Tierstall geht um eine Maus zu töten. Das ist in jedem Fall ein gewöhnungsbedürftiger, wenn auch notwendiger … Schritt. Ähnlich wie, ich das sage immer auch bei jenen Personen [die] viele Hühner im Laufe ihres Lebens essen, aber sich schwer tun, ein Hendl zu töten: Theoretisch, konsequenterweise, müsste man sagen wenn ich es esse, dann müsste ich auch fähig sein, es zu töten.

[lxi] … ich kann nur sagen aus eigener Erfahrung: … ich habe immer mit Tieren gearbeitet und meine Problematik war eine Zeitlang transgene Tiere. … Und habe am Anfang also diese Hürde auch nicht übersprungen. [Ich] habe also selbst für mich gesagt: „… das kannst du nicht tun. Muss man wirklich Mäuse genetisch modifizieren?" Und habe dann erst also im 91er Jahr oder 92er Jahr begonnen damit. Also da sieht man schon, man macht sich natürlich auch selbst Gedanken und nicht alles, das dann später fast zur Selbstverständlichkeit wird, ist am ersten Tag eine Selbstverständlichkeit.

[lxii] … weil man's einfach muss, weil das zur Arbeit dazu gehört, was man dann wieder verarbeiten muss.

[lxiii] Und das ist halt einfach das, was … vom Anfang meines Studiums bis heute immer noch und wahrscheinlich auch in Zukunft wird's immer wieder was geben, denke ich mir.

[lxiv] Ich habe zum Beispiel sehr viele Bekannte, die im Gefängnis arbeiten. Da herrscht auch eine andere Sprache. … Das ist, meiner Meinung nach, normal, also, das zieht sich durch alle Bereiche - man stumpft ab gegen das, mit dem man arbeitet.

[lxv] In China gibt's diese Regelung nicht. Und in China wurde dieses Experiment dann auch durchgeführt.

[lxvi] … dem chinesischen Forscher, der nach unseren Standards sein unerlaubtes Tierexperiment macht, bleibt theoretisch wirklich nur ein Schlupfloch - er publiziert's in einem chinesischen Journal. … Nur in der Praxis hat's heutzutage keine Bedeutung, denn ... diese Journale liest niemand.

[lxvii] Also, ich finde, jemand, der Fleisch isst, darf sich nicht über Tierversuche aufregen, die am bestmöglichen Niveau sind.

[lxviii] … dass sie eben, die immer narkotisiert sind, und dass die halt immer gleich umgebracht werden, dass die nicht einmal quieken können, so schnell geht das, dass die das eh nicht mitkriegen.

[lxix] … so angenehm wie möglich für die Tiere zu machen.

[lxx] … man kann einen bestimmten Leidensdruck, der kurzzeitig ist, sehr wohl vielleicht vor sich selbst rechtfertigen, dann wenn das Tier sofort stirbt. Bei diesem Versuch allerdings hat sich's um einen Zeitversuch gehandelt, der über 24 Stunden gegangen ist. … Die Versuche sind absolut international üblich. Ist nicht was, was verboten wäre, aber wir haben uns halt intern drauf geeinigt, … dass wir die nicht machen.

[lxxi] [Es wird] immer mehr gängig, dass die Publisher verlangen, die Genehmigungsnummer des Tierversuchs. … dass diese Versuche auch durchgeführt werde durf-

---

ten. Also, es ist auch bei den Publikationsorganen inzwischen üblich, ähnlich wie das bei Humanversuchen ja schon längere Zeit der Fall ist, dass die Ethikkommission nachweislich damit beschäftigt war.

[lxxii] Das waren eigentlich eher so dann die Gespräche in den Pausen beim Kaffee. Wo's dann so gefragt haben, nicht nur, ob man jetzt mit Mäusen arbeitet? … Gott sei Dank, habe ich immer gesagt, nein, ich bekomme sie fertig faschiert. [lacht]

[lxxiii] … ich habe eigentlich einmal in einem Tierphysiologiepraktikum einen Frosch umgebracht und habe dann gesagt, ich mache das nie mehr wieder. Das reicht mir. Ich habe nur gedacht, ich muss das einmal ausprobieren, wie's mir dabei geht und habe dann für mich einfach festgestellt, okay, ich möchte also nicht mir höheren Tieren.

[lxxiv] … nachdem ich erfahren habe, dass eben dort auf dem Institut Mausforschung, Tierversuche betrieben werden, war für mich klar, ich bringe sicher keine Maus um. Also, ich forsche damit … ich impfe sie, aber ich bringe sicher keine Maus um. Also, das [ist] eigentlich eine persönliche Einstellung von mir … es gibt einen eigenen Tierstall. Sie haben eh gesehen, wie das funktioniert. Es gibt dort Leute, die das machen, und wenn ich's nicht machen muss, ich will es nicht machen. Also, das ist meine persönlich Einstellung.

[lxxv] … eigentlich der Henker, der's verantwortet trotzdem.

[lxxvi] Überrascht hat mich dann eher auch die Reaktionen einer gewissen Kollegin, die dann überrascht war, als ich sagte, dass wir als Jungwissenschaftler uns schon ethische Gedanken darüber machen. … Weil es ist ungelogen, dass wir's verarbeiten müssen. Und je nachdem, was es ist, ist es auch ziemlich heftig für uns, das zu verarbeiten. … vor einem Monat oder so hatten wir einen Versuchsreihe, die auch für mich relativ schwierig war mit Tieren, wo wir zu dritt zusammen gearbeitet haben, wo wir wirklich einige Tage einfach nur zusammen gesessen haben, immer wieder drüber geredet haben. Andere konnten sich schon gar nicht mehr zu uns setzen. Einfach, weil wir's selber für uns verarbeiten mussten. Und ich war dann eher überrascht, dass die Kollegin da so sehr erstaunt war. Weil, ich denke mal, also für mich ist es halt auch wichtig, … dass wir uns da, sowohl ethischer, als auch … vom Handling her, Gedanken drüber machen, was wir da tun, und da nicht einfach nur gedankenlos an die Arbeit gehen.

[lxxvii] … Was mich jetzt eben beunruhigen würde, wie die große Laserkanone, was ich nicht gesehen habe. Die haben's echt nicht gehabt.

[lxxviii] … wo Regale drinnen stehen und Käfige.

[lxxix] … dass man will etwas verbergen.

[lxxx] Es wird ja immer interessanter, weil man es nicht darf.

[lxxxi] Wir haben sicher, also, die ganz braven tausend Mäuse gesehen. … [diese] furchtbar anzuschauenden Mäuse haben wir natürlich eh nicht gesehen. Ja, gut, das passt da für mich sozusagen auch in das Bild, dass halt das sehr, sehr positiv dargestellt worden ist. Aber wie gesagt, ich verstehe das natürlich. Also, ich würd's wahrscheinlich genauso machen, wenn jemand zu mir kommt, und will sich das alles anschauen. Dann werde ich ihm auch nur die schönen Sachen zeigen oder erzählen.

[lxxxii] … dass halt Tierversuche auch dazu gehören. … es war ganz normal. Eben, ich habe mir dann eher gedacht: Ja, was macht ihr eigentlich so ein Theater um den Tierstall?

[lxxxiii] … ich glaube eh nicht, dass sie unbedingt was zu Verbergen haben, aber sie hat vielleicht immer das irgendwie vermittelt mit ihrem Gehabe.

[lxxxiv] Da habe ich schon Schlimmeres gesehen, muss sich ganz ehrlich sagen.

[lxxxv] Ich meine, ja, sind halt viele Mäuse eingesperrt und das ist eben alles im künstlichen Licht und durch die Versuche natürlich werden einige Mäuse sterben müssen, und diese Mäuse werden da die Gene … werden ausgeschaltet. … Und das war dann für mich nicht so tragisch.

[lxxxvi] Sehr gefallen hat mir auch die Gruppendynamik, die sich entwickelt hat, wie die Wissenschaftler uns absolut nicht den Mäusestall zeigen wollten. Also, das war irgendwie witzig. Da waren wir uns auf einmal alle sehr, sehr einig.

[lxxxvii] Ich war erstaunt, dass die Mitglieder der Öffentlichkeit den Besuch im Tierstall eher als positiv bewertet haben. Ich finde, der Tierstall ist nicht besonders schön, und es gibt Tierställe, die für die Mäuse sicher angenehmer sind. … Wobei ich das Gefühl hatte am Ende der Diskussion waren die meisten eigentlich davon überzeugt, dass es notwendig ist, und dass wir wirklich versuchen andere Methoden zu verwenden außer Tierversuchen.

[lxxxviii] … wie geht's einem, dass man diese schwarzen Mäuse … eliminieren muss ganz einfach?

[lxxxix] … da ist der Puls auf 180.

[xc] … es gehört zu meiner Arbeit, ich habe mich dafür entschieden.

[xci] Man kann zwar sagen, dass man mit der Zeit vielleicht ein bisschen abgestumpft wird, aber ich glaube man kann das wirklich nur selber machen, wenn man … für sich selber, die Sinnhaftigkeit sieht. Wenn ich jetzt etwas machen würde, von der Arbeit, wo ich nicht überzeugt wäre, dass das wichtig ist, dann wäre das nicht denkbar dann die Tiere zu töten.

[xcii] … einen sehr persönlichen Kommentar heute abzugeben auf meine Frage, weil ich die Erfahrung gemacht habe, die Wissenschaftler sprechen eigentlich nie über Persönliches.

[xciii] … zu seinen Mäusen hingezogen gefühlt hat.

[xciv] … dass man ihm natürlich jetzt immer vorhaltet, er bringt die Mäuse um. Wenn man das von der anderen Seite betrachtet, muss man sagen, müssten wir alle Vegetarier sein, wenn wir uns wirklich dagegen verwehren - ich stelle es aber nur in den Raum, denkt im Prinzip wahrscheinlich auch keiner drüber nach, wenn er das Abendessen genießt.

[xcv] … entweder ist es ihnen wirklich egal, oder sie trauen sich vielleicht nicht sagen, dass sie wirklich Krisen haben. Wobei ich das nicht glaube, weil irgendwann mit der Zeit wird alles Routine. Sonst, sie wissen ja vorher, was sie erwartet, wenn sie dort arbeiten wahrscheinlich. Sie müssen es ja nicht machen. Weil ich glaube nicht, dass man dorthin geht und dann weiß man gar nicht, was man dort tun muss. … diese Leute freiwillig gemacht, na?

[xcvi] … wenn man die Leute persönlich darauf anspricht, dann haben sie oft selber auch keine Antwort und wahrscheinlich gibt es auch nicht wirklich eine Antwort, na?

[xcvii] DNA ist zu weit, ned. Das ist also zu abstrakt auch. Das ist nicht wirklich fassbar wirklich, ne. Aber das Tier, das sieht man halt, ned, die Maus, die da fiept.

[xcviii] … dass wir ganz normale Menschen sind.

[xcix] Die Gefahr, und das, was für mich interessant ist oder was man fragen müssten wenn ich mir jetzt mal anschaue, sind wir jetzt noch Laien? Sind wir jetzt verbraucht vielleicht noch mal an einem solchen Gespräch teilzunehmen? Sind wir jetzt überhaupt noch unbefangen genug, solch eine Aufgabe wahrzunehmen, wenn sich eine nächste Frage, ein nächstes Projekt stellt? … ob wir sozusagen jetzt schon geprägter sind in unserem Antwortverhalten, als eine völlig unselektionierte sozusagen echte Laiengruppe. Und das wäre für mich jetzt auch sozusagen die Frage, wenn … ich sage, okay, bei vielen Fragen sollen Laien mittun, dann frage ich mich, ob wir bereitwillige Laien irgendwann einfach aufgebraucht haben und Berufslaien kriegen?

[c] … ich habe in den Protokollen auch schon die ersten Spuren von ethischem Problembewusstsein entdeckt. Das heißt, man scheint das vage Gefühl zu haben, da gäbe es etwas, was ethisch diskussionsbedürftig wäre.

[ci] Fachhochschulen forschen in meiner Wahrnehmung nicht.

[cii] … das ich überhaupt nicht verstehe, … dass die Forschung praktisch moralische Argumente bringt, um Geld zu erhalten. Und das tut sie nicht. Wir machen reine wissenschaftliche Anträge, um Geld zu erhalten, und da ist kein einziges ethisches Argument, da ist kein einziges moralisches Argument …. ich glaube, … diese Verquickung zwischen Forschung … [und] Anwendung. Unsere Forschung ist absolut nicht anwendungsorientiert, und vielleicht hat mich das einfach ein bisschen gestört, dass diese Verquickung sehr stark da ist … aber das ist nicht unsere Forschung.

[ciii] … ich tu mir dann genau an dem Punkt besonders schwer die Grundlagenforschung zu rechtfertigen als reinen Zweck der Befriedigung des Wissensdurstes des Menschen. Also, wenn ich sage, wir machen das, wir wollen mehr Wissen, das ist so ein Grundbedürfnis, dann täte ich persönlich mich besonders schwer, an diesen Zweck zu rechtfertigen, dafür bringen wir Tiere um.

[civ] Schaut man da jetzt mehr auf Erkenntnis und Wissen oder sind wirtschaftliche Interessen im Vordergrund? Heiligt der Zweck die Mittel?

[cv] Das stimmt jetzt leider nicht, es gibt auf der TU jetzt ein Riesen EU-Projekt, die arbeiten jetzt nur an Malaria. Das hat glaube ich 8 Millionen Euro. … aber der Grund ist, es wird nicht für die Leute, die dort sind … gemacht, sondern natürlich für die Touristen, ja. Also, weil die jetzt genug hohe Zahl haben, damit sich das auszahlt für die EU das zu fördern. Kriegen doppelt so viel Geld, wie wir für die Fettforschung für Malaria.

[cvi] Und es gibt ja das Sprichwort … die Dicken sind gemütlich. … Die wollen gar nicht abnehmen.

[cvii] Entschuldigung, also Fettleibigkeit ist kein, ist nicht nur ein psychologisches Problem. Es geht nicht damit da, ob glücklich und dick zu sein. Es ist trotzdem ungesund, auch wenn man glücklich ist damit.

[cviii] Fakt ist, dass 50% der Leute zu viel Kilos haben, das ist Fakt. Ob sie sich das ein, ob das sich jetzt eine bestimmte Person einbildet, sie sei so dick oder wirklich zu dick ist, das ist ein individuelles Problem. Fakt ist, die Hälfte der Leute würde gesünder leben, würden sie etwas abnehmen. Und 20% müssten massiv abnehmen. Das ist Fakt, und das kann ich nicht durch, das kann ich, ob die jetzt, ob's denen jetzt psychologisch gut geht oder nicht gut geht, ändert nichts daran, dass Fettleibigkeit ungesund ist.

[cix] … es waren ja doch sozusagen zwei Fronten, sage ich jetzt einmal, zwei gegnerische Parteien, nenne ich's jetzt einmal. Und da ist dann sozusagen die Hemmschwelle weggefallen, wenn die nimmer mehr da waren. Ist sozusagen viel lockerer und viel direkter miteinander geredet worden.

[cx] … [da] waren dann zwei dabei, die sagen, ja, sie essen kein Fleisch … weil ihnen tun die Tiere leid. Aber die sind wenigstens konsequent, ja. Also, ich finde, jemand, der Fleisch isst, darf sich nicht über Tierversuche aufregen, die am bestmöglichen Niveau sind. Und das, finde ich, ist ein wichtiges Argument bei den Tierversuchen, ist nicht nur ob, sondern auch wie.

[cxi] Also, ich forsche damit … ich impfe sie, aber ich bringe sicher keine Maus um. Also, das [ist] eigentlich eine persönliche Einstellung von mir.

[cxii] S6: Würdet's ihr euch von uns als Projektkoordinatoren … eine Aufforderung zur Diskussion wünschen? So abwegig das jetzt [klingt] …

S5: Ich glaube, es ist auch immer leichter, wenn du direkt mit jemanden redest, der im gleichen Umfeld ist und zum Beispiel wenn man am gleichen Institut ist und das gleiche macht, dass du gleich direkt vor Ort redest.

[cxiii] E: Das [die Unterscheidung zwischen Grundlagenforschung und Anwendung] ist doch für die Gesellschaft nur dann akzeptabel …. dass man's finanziert, wenn die Gesellschaft die Möglichkeit hat, über den Umgang mit dieser Forschung zu entscheiden.

S7: Nein, über den Umgang der Anwendung, nicht über den Umgang der Forschung.

…

E: Und da muss die Gesellschaft entscheiden können … anders verabschieden wir uns von jedem Gedanken von Demokratie." S7: „Nein, nein, dann verabschieden wir uns von der Freiheit der Forschung. Und dann brauche ich keine, und dann kann ich die Universitäten gleich zusperren.

[cxiv] Das wird dann funktionieren, wenn ich eine Steinschleuder erfinde, dann ist es absehbar, dass jemand dann einen Stein gegen den Kopf kriegt, und der wird dann sterben.

[cxv] Der Herr Pasteur hat, wo er das Penicillin entdeckt hat, durch Zufall entdeckt hat, nicht gewusst, dass dadurch Multiresistenzen entstehen. … Das heißt, diese Abschätzung ist eine Schwierigkeit, genau so die positiven Anwendungen abzuschätzen. … der Laser ist nicht erfunden worden, damit ich einen Laserbeamer ha-

---

be und meine Augen abscannen lassen kann oder was auch immer. Das sind Anwendungen, die total unabsehbar waren. Das ist nicht wegen einer Anwendung entwickelt worden, das war eigentlich ein ganz anderes Produkt. … ich kann dann abschätzen, was ich alles damit machen werde können: Das ist glaube ich, zu viel verlangt.

[cxvi] … wenn ich auf Nummer sicher gehen will, dass niemand das missbraucht, meine Technik, dann muss ich sagen, wir behalten den Stand der Technik … und wir verbieten Forschung und Weiterentwicklung generell. Dann kann ich sicher gehen, es wird nirgends aus irgendeinem Forschungsergebnis ein negativer Effekt rauskommen. Ansonsten kann ich das nicht unterbinden. Das ist die einzige Möglichkeit, wie ich sicher gehen kann, dass nichts Schlechtes entsteht. … es kommt auch nichts Gutes raus, aber es wird nichts neues Schlechtes entstehen.

[cxvii] … ich finde, dass da vor allem Künstler und Philosophen und Ethiker unsere Kontrollorgane sind, wenn schon die Mediziner nicht selber und die Techniker nicht nachdenken.

[cxviii] … das, was wir forschen, forschungswürdig ist, entscheiden ForscherInnen in den USA.

[cxix] S6: Das dürfen Sie nicht, also, es gibt ja Vorschriften … Sie kriegen nicht den Persilschein, dass Sie alle Tierversuche machen können. Jedes Mal, wenn wir z.B. eine bestimmte Knockout-Maus machen wollen, mit einem bestimmten Gen, müssen wir einen Antrag stellen, darlegen, was wir, was dieses Gen für eine Funktion hat, und was wir vermuten, und wieso wir das brauchen.

L13: Und an welchen Kriterien wird das dann gemessen, ob das jetzt okay ist oder nicht?

S6: Ist eine gute Frage, ich habe keine Ahnung. Müssen Sie den Herrn vom Ministerium fragen, der weiß das. Ich weiß nicht, wissen Sie das?

[cxx] … bei unseren Forschungszielen haben wir das so unterteilt, und zwar wir haben die unmittelbaren Ziele, die unsere sind im Labor. Unsere unmittelbaren Ziele sind Gene finden und Stoffwechselwege aufklären. … Dann gibt's die mittelbaren Ziele und die langfristigen Ziele. … Und die langfristigen Ziele wären dann eben die Reduzierung der Fettleibigkeit, Reduzierung von Arteriosklerose, Herzinfarkt, Krebserkrankungen, etc. Nur, diese mittel- und langfristigen Ziele sind nicht die Ziele von GOLD. Die sind die gesellschaftlichen Ziele, die andere dann auch machen, die machen wir nicht selber. Wir könnten es nicht einmal machen.

[cxxi] Wann ist rechtzeitig? … bevor Niels Bohr den Atomaufbau geklärt hat, ist das rechtzeitig oder bevor das [Manhattan-]Projekt gestartet ist?

[cxxii] Wo muss man aufhören? … Niels Bohr, das Atommodell … weil er hätte ja das nicht entwickeln dürfen?

[cxxiii] Ist Darwin schuld an Euthanasie?

[cxxiv] Wir sind also nicht die, die diese Projekte initiieren … sondern wir sind diejenigen, die das Angebot annehmen, und, um diese Verantwortung wieder irgendwo von uns ein bisschen wegzubringen … die Gesellschaft muss sich dann Gedanken machen, was wir damit machen. Die Gesellschaft hat sich, glaube ich, vorher schon, unter welchen Voraussetzungen auch immer, jetzt schon drüber Gedanken gemacht und diese Projekte entwickelt.

[cxxv] Ich habe sehr stark versucht, zu verstehen, was ist die individuelle Motivation der Beteiligten, … wer ist überhaupt beteiligt an dem, was dort passiert ausgehend von dem, was die Wissenschaftler machen.

[cxxvi] … hat sich eher vielleicht die Einschätzung der Öffentlichkeit verändert, als zum Thema.

[cxxvii] Mein Eindruck war der, dass das doch etwas eher etwas seltenes ist, also, dass das nicht ein fester Bestandteil der wissenschaftlichen Tätigkeit darstellt, dass man sich mit diesen Dingen beschäftigt. Der Eindruck ist für mich entstanden einfach im Gespräch mit den Wissenschaftlern.

[cxxviii] … bei den ethischen Dingen sind sie uns dann ein bisschen so ausgewichen, ned.

[cxxix] Was ich für mich [von den Diskussionen] mitgenommen habe, das ist - und das deckt sich mit anderen Erfahrungen [mit] Vollblutwissenschaftlern - dass … diese Dimension, sich dies [ethische Fragen] zu fragen, als Störung empfunden wird, als Zeit raubend, Kraft raubend empfunden wird oft und als kontraproduktiv weil wenn ich zu viele Fragen stelle, mache ich viele Sachen nicht mehr so einfach. Das ist, glaube ich, ein ganz wesentlicher Punkt in einer ambitionierten Forschung, dass ich viele Dinge erst einmal mache und dann hinterher hinterfrage.

[cxxx] Sie haben eh gehört, die forschen, ich weiß nicht wie lange. Und dann kommt einmal was heraus. [Das bedeutet] ja wahnsinnig viele Frusterlebnisse. … Die haben einiges mitzumachen, ned. Geduld und dann geht es immer so auf und ab und wenn dann wer kommt und dann sagt: ‚Na, bitte moralisch und was Sie da machen. Jetzt denken sie einmal nach … was sie da arbeiten, in welche Richtung das gehen könnte, das könnte so und so entarten.' Und dann müssen sie zum Diskutieren anfangen … das ist ja hemmend für die Arbeit.

[cxxxi] … das ist ja das Dilemma der Wissenschaft, ned, dass wir eigentlich nicht so viel Zeit haben auf die ethische Diskussion warten zu können, ned, auch mit unseren Arbeiten.

[cxxxii] … bei den Forschern habe ich schon gemerkt, dass es hierarchische Grundstrukturen gibt. … wenn es über das eigene Feld hinaus … der Ruf nach dem letztendlichen Chef da war, beziehungsweise der dem oft schon vorgekommen ist und im Prinzip primär für auch andere gesprochen hat. Und eigentlich nur sehr konkrete Fragen frei oder direkt von Einzelnen niederchargierten Leuten beantwortet wurden, wenn es um konkrete Erfahrungen geht.

[cxxxiii] … ich habe das eigentlich das immer gespürt, dass die Meinungen werden die Wissenschaftler aus verschiedenen Gruppen also nicht ändern. Sie haben ihre Meinungen gesagt, [sie] ausgetauscht, und das war es. … was [für] eine Meinung die Öffentlichkeit überhaupt hat, was ich eigentlich mache, das, ich glaube, dass das ist im Hintergrund [für die Wissenschaftler].

[cxxxiv] … da wird man zum Teil auch sehr stark angegriffen. Also das ist ja ein Thema, was gerade in der Bevölkerung negativ aufstößt.

[cxxxv] Und was für mich doch in gewisser Weise überraschend war, dass sich das geändert hat am Zugang, bevor die Leute wirklich im Tierstall waren und in den Labors waren und nachher. Das war, also es hat dann immer noch Leute gegeben, die gesagt haben … sie sind damit überhaupt nicht einverstanden. Aber es war al-

---

les nimmer so, würde ich jetzt im Großen und Ganzen sagen, es war nicht mehr so eindeutig.

[cxxxvi] … was wir oft glauben, dass wir das sehr gut vermitteln oder dass wir das gut vermitteln können, dass das der richtige Weg ist, dass das ganz anders ankommt. Und da ist sicher noch viel Lernpotential, das muss ich schon sagen. Und das zweite, was diesen Umgang angeht: wir haben oft diesen Glauben, dass wenn die Argumente gut genug sind, dann ist jeder dieser Meinung. Dass das aber so emotional eigentlich abläuft diese Meinungsbildung, dass die Argumente an zehnter Stelle vielleicht kommen und dass wir diesen emotionalen Aspekt viel zu wenig abdecken, dass wir sehr rational an das Ganze herangehen, weil wir einfach einen, insgesamt einen rationalen Zugang haben, und [wir] diese emotionale Frage gar nicht stellen … Und da müssen wir - ich bin mir noch nicht sicher, wie man das umsetzen kann, und wie man das lernen kann - aber da sollten wir auf jeden Fall noch was dazu lernen.

[cxxxvii] … obwohl wir soviel Zeit miteinander verbracht haben, ich nicht das Gefühl gehabt habe, dass sie [die Laien] das Gefühl haben, wir sind Menschen, denen man vertrauen kann, in dem was sie arbeiten. Das Gefühl hatte ich nicht. Sondern bis zum Schluss hatte ich eigentlich das Gefühl, dass sie zwar finden, dass wir ganz nett sind, aber es muss unbedingt jemanden geben, der uns auf die Finger schaut.

[cxxxviii] … wir machen keine Tierversuche, direkt.

[cxxxix] Und da ist mir erstmal die Tragweite dieser moralischen Ebene bewusst geworden und ich denke, das ist anderen auch so ergangen. Und deswegen war für mich [Ethik] so wahnsinnig interessant, weil er diesen Knoten gelöst hat. Ich habe immer versucht, irgendwie den Haken in diesem Prozess [welcher Bereich ist zuständig für gesellschaftliche Verantwortung] zu finden, aber ich denke, dort ist er nicht zu finden, sondern man muss auf dieser Meta-Ebene über die Sache nachdenken und dann löst sich vielleicht irgendwo der Knoten. … Also ich bin ja dann erst darauf gekommen, dass dieser Kreis, wie ich ihn skizziert habe, da ist. Aber ich glaube, dass der Kreis an sich nicht in der Lage ist, sich dramatisch zu verändern, ohne dass sich die Meta-Ebene verändert, dass sich eben die moralischen Voraussetzungen, die ethischen Voraussetzungen verändern.

[cxl] … weil die werden alle betriebsblind und die haben alle ihre wirtschaftliche Interessen, ihren Posten zu halten und die Aufträge oder in dem Bereich weiterzuforschen, der ihnen vermittelt wurde. Und daher werden sie nicht sagen … hier sind wir an solche Grenzen gestoßen, hier kann ich einfach nicht mehr weitermachen, ned.

[cxli] … zu einem wirklichen Endergebnis so wie in der Mathematik.

[cxlii] Volkssport.

[cxliii] Gedankenspielcharakter.

[cxliv] Diskussion um Kaisers Bart.

[cxlv] … dass man voll im Kreis diskutiert und eigentlich zu keinem Schluss kommt.

[cxlvi] … wie ein Buch, das man liest, wenn man dann Zeit hat.

[cxlvii] … mehr eine Anleitung und mehr Konkretes … ich hätte schon ganz gern, dass wenn sich Experten mit was beschäftigen, z.B. um festzustellen, was eine gute Vor-

---

gangsweise wäre, dass am Schluss eine konkrete Empfehlung hinauskommt. Nur Fragen aufzuwerfen ist - von mir natürlich aus naturwissenschaftlicher Sicht gesehen - nicht das, was ich als Ziel definieren würde.

[cxlviii] Ich komme als mehr oder minder Laie und frage einen Wissenden und wir werden aus dieser Grundbeziehung - er ist der Forscher, der an Lipasen forscht und ich bin der aus der Öffentlichkeit - nicht herauskommen. Wenn jetzt jemand, dessen Profession eine andere ist, in der er auch gut ist, gleichberechtigt daneben gestellt wird und sagt, „Ich bin ein Ethiker und wir reden jetzt mal“, dann ist nicht die Dimension da „Ich erkläre dir jetzt, was Genomforschung ist“, sondern dann ist eine andere Ebene da. … Das ist glaube ich was, was einfach schwieriger ist, spontan zu können. Was leichter ist, wenn der Andere sozusagen von Vorneherein diese Autorität mit hat.

[cxlix] Jetzt weiß ich nicht, ob ich meine Meinung darüber äußern kann … wie ich qualitativ diese Gespräche oder Konfrontationen zwischen Wissenschaftler aus verschiedenen Wissenschaftsbereichen [GenomforscherInnen und eingeladener Ethiker] beurteilen kann.

[cl] … die Öffentlichkeit kann nicht die Verantwortung übernehmen. … Die … Öffentlichkeit ist kein Experte.

[cli] Also, ich habe es erlebt eigentlich von Anfang an waren immer wieder Fragen der Regulierung, der Regularien: Wer setzt Grenzen? Wer hält Grenzen ein? Wer beachtet die Einhaltung von Grenzen? Das habe ich immer wieder als einen wichtigen Punkt erlebt, der nicht nur von mir … aber von vielen anderen Stellen auch gekommen ist ... Und die Antwort ist sozusagen die, die mir auch sozusagen übrig geblieben ist: Wir wissen nicht, wer es macht. Es gibt eigentlich niemanden, der es macht. Irgendwie ist das ein sich selbst organisierender Prozess.

[clii] … es müsste eigentlich jeder einzelne Verantwortung übernehmen. … sicher, dass paar jetzt sagen, wir bräuchten also irgendwo eine Institution oben drüber, die das verantwortet, sondern das muss eigentlich jeder selber machen, weil’s diese Institution einfach nicht gibt und nicht geben wird. Drum sollte sich auch ein jeder fragen, der das macht [forschen], wie weit das dann noch okay ist.

[cliii] Natürlich eine gute Politik wird heute … am Kontakt mit der Wissenschaft in vielen Fällen nicht vorbei können, wird das einbinden müssen, aber die letztendliche Verantwortung kann nur dort [bei der Politik] liegen.

[cliv] Und es ist mein Gefühl, dass es keine … ethische Dimension gibt, dass Politiker das in der Händen haben und sagen: ‚Jawohl, ich werde dafür sorgen, dass’ sondern dass irgendwo, sei es aus finanziellen Gründen, damit mir die nicht abwandern.

Kurzzusammenfassung /Abstract

Hintergrund:
In der politischen Steuerung von Wissenschaft nehmen "Ethik" und "Partizipation" einen zentralen Platz ein. In der politischen Praxis hingegen ist das Verhältnis dieser beiden Ansätze zueinander weitgehend ungeklärt. Während partizipative Ansätze eine weiter reichende Einbeziehung von Akteuren und Interessen versuchen (insbesondere von "Laien"), beruht "Ethik" weitgehend auf Expertenwissen. Die epistemische und politische Autorität von Ethik wird mit dem Unterschied zwischen Ethik, als vernünftige und systematische Reflexion von moralischen Annahmen, und "Moral" als unhinterfragte Zuordnung von "gut" und "schlecht", gerechtfertigt.

Ziele:
Das Ziel dieser Dissertation ist zu analysieren wie "ethische Laien", d.h. Mitglieder einer weiteren Öffentlichkeit und GenomforscherInnen mit ethischen Themen und Fragen in einen interaktiven Setting umgehen—d.h. ohne die Involvierung von EthikexpertInnen. Dabei sind drei Dimensionen zentral: Welchen ethischen Themen und Fragestellungen haben die ethischen Laien als besonders relevant erachtet? Was sind die diskursiven und mikropolitischen Muster in einer solchen Ethikdebatte? Welche Erfahrungen wurden in den Verhandlungen um ethische Themen gemacht und welchen Sinn generieren die DiskussionsteilnehmerInnen für sich daraus?

Theoretische Herangehensweise:
Die theoretische Perspektive ist im Feld der soziologischen Wissenschaftsforschung angesiedelt, insbesondere in der Theoretisierung einer veränderten gesellschaftlichen Wissensproduktion und eines sich veränderten Verhältnisses von Wissenschaft und Gesellschaft ("mode 2 science"), des Weiteren kritische Public Understanding of Science- und Partizipations-Ansätze sowie sozialwissenschaftlicher Forschung, die sich kritisch mit der institutionellen Dimension von Ethik auseinandergesetzt hat. Auf dieser Grundlage führe ich das Konzept von "lay ethical knowledge" ein, welches von einer hierarchischen Unterscheidung von "Ethik" und "Moral" Abstand nimmt und den Kontext, in welchem Ethik verhandelt und somit Bedeutung erzeugt wird, betont.

Methoden:
Die Dissertation beruht auf einem Forschungsprojekt—"Reden wir über GOLD!—in welchem ein Partizipationssetting geschaffen wurde, wo sich Laien und ForscherInnen regelmäßig über einen längeren Zeitraum hinweg trafen – den sog. "Runden Tischen". Das Projekt versucht Partizipationsansätze mit der Diskussion von ethischen Fragestellungen zu verbinden. Die Diskussion am Runden Tisch wurde mit qualitativen sozialwissenschaftlichen Methoden (Grounded Theory) analysiert.

Resultate:
Die qualitative Untersuchung hat gezeigt, dass sowohl die teilnehmenden BürgerInnen als auch die ForscherInnen einen reflektierten und kontextbewussten Umgang mit ethischen Themen und Fragestellungen gezeigt haben. Diese sind dergestalt, dass sie die authorative Art der Expertenethik durchaus in Frage stellen. Die TeilnehmerInnen des Runden Tisch thematisierten andere Themenfelder als institutionalisierte Standardethik. Trotzdem, ihre Einschätzungen von Ethik sind relativ ambivalent und spiegeln somit den weiteren gesellschaftlichen Umgang mit Ethik. Des weiteren weisen die Ergebnisse darauf hin, dass gängige gesellschaftliche Ethikpraktiken, welche versuchen vergangene Wertentscheidung zu

kompensieren und eine Teilung zwischen Risiko- und Moralaspekten einführen zu kurz kommen, wenn es darum geht, auf weiter reichende öffentliche Bedenken hinsichtlich Innovationsprozessen zu reagieren.

Schlussfolgerungen:

Das ethische Wissen von Laien eignet sich dazu, den Umgang von ExpertInnen mit Ethik in Frage zu stellen. Laienethik ist eine kritische Ressource um als selbstverständlich angenommene Rahmungen in der moralischen Steuerung von Forschung zu hinterfragen. Die TeilnehmerInnen gingen auf reflektierte Weise mit ethischen Fragestellungen um, was die Hierarchie zwischen "Ethik" und "Moral" in Frage stellt, um als Kriterium für die Einbeziehung in eine ethische Debatte zu dienen.

Der Umgang der TeilnehmerInnen mit ethischen Fragen legt nahe, dass eine anderer Umgang mit Ethik von Nöten wäre um den veränderten Bedingungen von Wissenschaft und Gesellschaft gerecht zu werden. Während Standardethik immer versucht, für vergangene Wertentscheidungen zu kompensieren, schlage ich eine "Ethik der Innovation" vor, welche im Innovationsprozess viel früher ansetzt und selbstverständliche Annahmen zur Disposition stellt.

Curriculum Vitae *Michael Strassnig*

1992	A-Levels („Matura“)
1992-1995	College for graphic-design at the HTL Linz
1996	Military Service
1996-2004	Studies of communication science, history, history of arts and sociology at the University of Vienna
1999-2000	Erasmus term at the University of Bielefeld, Department of Sociology
Since 2002	Contract researcher at the Department of Social Studies of Science, University of Vienna
Since 2004	Doctoral student for Dr.phil. (Sociology) at the University of Vienna

Collaborations in Research Projects (all at the Dept. of Soc. Stud. of Science)

Evaluation of the »Diskurstag Gendiagnostik« of the »Austrian Genome Research Programme GEN-AU«. Funded by the Federal Austrian Ministry for Education, Science and Culture, GEN-AU

Challenges of Biomedicine: Socio-cultural contexts, European Governance and Bioethics. Funded by FP6 of the European Commission

“Let’s talk about GOLD!”: Analysing the interactions between genome-research(ers) and the public as a learning process. Funded by the Federal Austrian Ministry for Science and Research, GEN-AU

Informed Consent: Space of Negotiation between Biomedicine and Society. Funded by the Federal Austrian Ministry for Science and Research, TRAFO

Making Futures Present. On the co-production of Nano and Society in the Austrian Context. Funded by the Austrian Science Fund (FWF)

Managing editor of the international peer-reviewed journal *Science, Technology, & Human Values*

Selected Publications

Felt, U., M. Fochler, A. Müller, and M. Strassnig. 2009. Unruly ethics: On the difficulties of a bottom-up approach to ethics in the field of genomics. *Public Understanding of Science.*

Bister, M. D., U. Felt, M. Strassnig, and U. Wagner. 2009. Refusing the information paradigm. Informed consent, medical research, and patient participation. *health: An Interdisciplinary Journal for the Social Study of Health, Illness and Medicine* 13 (1):87-106.

Bister, M. D., U. Felt, M. Strassnig, and U. Wagner. 2008. Zeit und Partizipation im transdisziplinären Forschungsprozess. In: Elisabeth Reitinger (ed.): *Transdisziplinäre Praxis. Forschen im Sozial- und Gesundheitswesen*, 35-46. Heidelberg: Verlag für Systemische Forschung im Carl-Auer Verlag.

Teaching

Since 2005	Seminars at the University of Vienna, Dept. of Soc. Stud. of Science