# Table of Contents

<table>
<thead>
<tr>
<th>Section</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>Acknowledgments</td>
<td>i</td>
</tr>
<tr>
<td>Preface</td>
<td>iv</td>
</tr>
<tr>
<td><strong>Part one: Resources</strong></td>
<td></td>
</tr>
<tr>
<td>Introduction</td>
<td>1</td>
</tr>
<tr>
<td>1. Resources for Non-Humanist Studies of Technoscience</td>
<td>3</td>
</tr>
<tr>
<td>2. Qualitative Research as Partial Connection</td>
<td>28</td>
</tr>
<tr>
<td>3. Researching Partially Existing Objects</td>
<td>46</td>
</tr>
<tr>
<td><strong>Part two: Strategies</strong></td>
<td></td>
</tr>
<tr>
<td>Introduction</td>
<td>56</td>
</tr>
<tr>
<td>4. Political and Moralising Moments</td>
<td>58</td>
</tr>
<tr>
<td>5. Reading Digital Denmark</td>
<td>74</td>
</tr>
<tr>
<td><strong>Part three: Illustrations</strong></td>
<td></td>
</tr>
<tr>
<td>Introduction</td>
<td>94</td>
</tr>
<tr>
<td>6. The “Seamless” Web</td>
<td>97</td>
</tr>
<tr>
<td>7. An Experiment in Performative History</td>
<td>109</td>
</tr>
<tr>
<td>8. Standardisation and the Logic of the Differend</td>
<td>135</td>
</tr>
<tr>
<td>9. Citizen Projects and Consensus Building at the Danish Board of Technology</td>
<td>156</td>
</tr>
<tr>
<td>10. Infrastructural Fractals</td>
<td>187</td>
</tr>
<tr>
<td><strong>Part four: Implications</strong></td>
<td></td>
</tr>
<tr>
<td>Introduction</td>
<td>211</td>
</tr>
<tr>
<td>11. Technologic</td>
<td>212</td>
</tr>
<tr>
<td><strong>Bibliography</strong></td>
<td>251</td>
</tr>
<tr>
<td><strong>Dansk resume</strong></td>
<td>1</td>
</tr>
</tbody>
</table>
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From early university years, my interests in STS and constructivist thought was developed by Finn Olesen, and after he co-taught a course on STS and philosophy of science and technology with Andrew Pickering and others, it turned out to be a lasting impression. A similarly lasting influence was my one-year visit at Don Ihde’s *Technoscience Research Seminar* at State University of New York, Stony Brook (1998-9), spent heatedly discussing ANT and Deleuze with Evan Selinger and Jari Friis Jørgensen.

Upon returning to Denmark local interest in such issues had expanded and I have had the pleasure of never-ending discussions with Simon Kiilerich Madsen, Jane Bjørn Vedel and Christopher Gad and numerous other students from Information Studies, University of Aarhus, roughly since then. Several of these are now involved in the Center for STS-Studies, which started up in 2002. So is Peter Lauritsen who has co-written two of the chapters here presented, and provided commentary on many more.

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My analyses have been made possible by the friendly co-operation of multiple people, not least at the EPR development project in the Aarhus Region, the National Board of Health, the Danish Board of Technology, and the Danish Institute of Medical Informatics.

¹ Detailed acknowledgments for individual chapters are offered at the end of each piece.
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A number of chapters have been or are in the process of being published elsewhere.

Chapter 1, “Resources for Non-Humanist Studies in Technoscience: A Theoretical Assemblage” is submitted to Configurations.

Chapter 2, “Qualitative Research as Partial Connection: Bypassing the Power-Knowledge Nexus”, written with Peter Lauritsen, has been accepted for publication (2005) by Qualitative Research.

Chapter 4, “Political and Moralising Moments: Documents as Material Agents in Danish Visions of IT in Health Care”, written with Brit Ross Winthereik, was published in Information Technology and People (2002) 15: 3, p. 127-141. It is presented here in a mildly edited version.

Chapter 5, “Reading Digital Denmark: IT-Reports as Material-Semiotic Actors”, written with Peter Lauritsen, has been accepted for publication by Science, Technology, and Human Values.

Chapter 7, “An Experiment in Performative History: The EPR as Future-Generating Device” is submitted to *Social Studies of Science*.

Finally, Chapter 9, “Citizen Projects and Consensus-Building at the Danish Board of Technology: On Experiments in Democracy” has been submitted to *Acta Sociologica*.

Casper Bruun Jensen
Washington D.C., March 2004
Preface

1. Experimental Devices
I have named the present text *Experimental Devices: Studies in STS and Electronic Patient Records* to call attention to the relations between its diverse contents. It is unclear what the designation *experimental devices* refers to: one might naturally link the term with electronic patient records (EPRs) but this is not evident from the subtitle, which seems to mix up or conflate a set of theories or methods (Science and Technology Studies) with an object of inquiry (EPR). This is on purpose, for reasons I shall outline below and detail throughout the text: these are reasons relating to the project of formulating alternative understandings of the link between what we usually distinguish as theory and practice, description and normativity.

In the view here embraced these distinctions become porous. These are therefore not studies *of* EPRs; neither are they studies of both STS and EPRs: they are studies *in* them; their aim is experimental participation. Bits and pieces of method and theory, then, can be seen as small machines for intervening in a number of discussions relating to different themes and fields, as various experimental devices. Yet this is not the prerogative of STS. Electronic patient records also function as experimental devices although their milieu of experimentation is different: they have to do with reconfiguring health care practices.

2. Introductory Remarks
The thesis belongs to science and technology studies (STS), an inter- or transdisciplinary study-area with participants from fields such as anthropology (cultural and social), communication- and media studies, cultural studies, feminist theory, history, information studies, organizational theory, philosophy, social psychology and sociology. Each of these fields have developed important insights and this study draws liberally on many of them in its engagement with various issues in STS and social theory more broadly.

Throughout the study ”theoretical” discussions are carried out and ”empirically” illustrated. The empirical material has to do with a set of new technologies, referred to as electronic patient records (EPRs), which have been imagined as crucial future components of the Danish health care system for the last decade. EPRs currently hold the
attention of many different groups of people, such as nurses, medical doctors, engineers, medical informaticians, people in medical technology assessment, and politicians.

My work has been located at the department of information- and media studies at Aarhus University, a department, which boasts an eclectic core curriculum including courses in organizational studies, communication theory and semiotics, computer-supported co-operative work, history of technology, philosophy of science, and logic- and object-oriented programming, but remains located in the humanities.

Each of these contexts (theoretical, empirical, practical, disciplinary) offers diverse discourses for the present text to connect with and, thus, a series of partial, and only partially overlapping, determinations and expectations to be negotiated. What they must be negotiated with is the writer’s – my own – intellectual tastes and tendencies. As it has happened these have primarily resonated with what is often referred to, within cultural theory and philosophy, as post-structuralism, and which, in STS are known as post-humanism, cyborg theory, amodernism, or, as I call it in chapter one, simply non-humanism – all of which instantiate what Barbara Herrnstein Smith and Arkady Plotnitsky (1996) have called ”post-classical” thinking.

Given the diversity, institutional, no less than methodical and theoretical it is no surprise that I have regularly felt a complicated over-lay, both in the process of writing the following chapters, and in presenting them in different contexts. Leigh Star has suggested that such an effect is invariable when one moves in and between different social worlds (Star 1994). Invariably, the positions I have taken, and the analyses I have made, have seemed to make partial contact at best with its audiences. At times this has had me wondering whether my inspirations were too diverse; my analyses bound for incoherence. Other times I have felt encouraged; the juxtaposition and interlinking of often unrelated intellectual concepts have seemed refreshing, the results of doing so interesting or clarifying to myself and others.

In view of the lack of a ”natural” stability of either the present text or its (future, hoped-for) audience, an introductory characterisation becomes that much more important. Although we know, with Stanley Fish (1980) and Bruno Latour (1987), that a text is always in the hands of later users, this does not preclude an attempt at (gently) guiding
him or her\textsuperscript{1} towards certain understandings, found beneficial, and away from certain others found, perhaps, harmless but banal, slightly worrisome, or downright wrong.

STS-studies have learned from disciplines such as social and cultural anthropology, qualitative sociology, and history to pay serious attention to practical detail. In one way or another theoretical exposition has been seen as necessarily grounded, and it is hard not to concur with the naturalist and materialist bent of this approach. However, although this study is based, firmly, I hope, in empirical detail, it does not purport to be an STS-ethnography of electronic patient records and anyone looking for one will likely be disappointed.\textsuperscript{2} It is not that detail is lacking or irrelevant, but that the focus is somewhat different and may seem unnecessarily theoretical to some readers.

“Theoretical” as its complement “practical”, however, is a very loaded and highly equivocal term. With a non-humanist sensibility the idea that something “theoretical” exists, which is separate from something “in practice” is dubious. The idea of theory as unnecessary \textit{per se}, if anyone really holds it, is based in an understanding of empirical material explaining itself; that is, some version of positivism. No less problematic, however, is the superficially less naïve idea that the world shows (data) and the researcher \textit{then} interprets (theory).

One may repeat the point that it is by no means clear that the theoretical and the practical are so dichotomously related, and suggest (once more) that practice is, in some sense, always theoretical and that theory is, in some sense, also a practice. But clarification of what these relationships are is complicated and explorations into them will be of particular significance in this study. Rather than aiming to neutrally elucidate and explain the events of Danish EPR development, the approach is to use this material to effect an empirically based deconstruction of the theory-practice couplet. Such deconstruction is not in itself very interesting. Its supplement\textsuperscript{3} would be an exploration of what theoretically interesting and politically responsible alternatives become viable as a consequence. This would be of particular importance for considering what STS \textit{can do} and what it \textit{should do}. Such exploration, that is, would be of especial pertinence for thinking about STS and normativity.

It is noteworthy that although the practice-theory dichotomy and similar one’s -thinking and doing, the abstract and the practical etc. – are disavowed in current STS with almost ritual consistency, they still implicitly inform a great deal of STS analyses.
This constitutes something like a mainstreaming of the dissolution of the dualism: everyone claims to do it, yet everything seems like business as usual. The idea that theory and practice are inseparable is now used to suggest that STS-theory may offer (or should offer) particularly efficient means for guiding practice. While this may, in a sense, seem a logical trajectory it also effects a curious reversal to high modern conceptions of theory-practice relations - this time re-emerging in the guise of a performative argument. Yet the idea that theory should be applied to practice to beneficial effect, while old, is not necessarily so venerable.

Although there are manifest differences between my usage and that of Annemarie Mol (2002), I have intermittently adopted her characterisation of empirical philosophy for the work here presented. Empirical because the practices and people I have studied offer important lessons in how medical worlds, bureaucratic worlds, and technological worlds are becoming increasingly intertwined, and how different groups of people respond to the fact. But also because the concepts used to make sense of this situation are not given but can only be seriously articulated after engaging with these practices. But philosophy (or theory), because the aim of the study rather than aiming exclusively for enumeration of ethno-categories used by practitioners in the worlds of the EPR, as indicated above, is also attempting to do innovative work. Given that the EPR seems to be far from an immutable, static, modern thing, and rather more like a historically evolving composite of partially connecting entities, I ask and try to answer questions such as: What is an EPR? How can you study it? Where does it come from? Why now, and why in these particular Danish places? In a phrase of Bruno Latour’s, I study the EPR as a partially existing object, while simultaneously querying the manner in which one may do so.

There is a double sense in which I hope this may be innovative. First, through the obviously limited participation in reconfigurations of ethno-understandings in the worlds of the ECR. Second, in its, again partial, participation in reconfiguring the intellectual categories and understandings of STS and related fields. In all instances, I attempt to develop accounts of practices and knowledges as performative and dynamical all the way through, with all this entails.

The distinction between EPR worlds and STS worlds plays on the old separation between the internal and external, but does not strictly rely on it. Indeed, the
destabilisation of this distinction effectuated by non-humanist STS-studies enable me to claim simultaneous efficacy “inside” and “outside” STS, though the sustained focus on such destabilisation increases the likelihood that the study will be localised by readers as “internal”. This is OK: destabilisation does not mean that all boundaries disappear; rather they become more fluid. Esoteric content does not, in my analysis, equal broad irrelevance just like public relevance does not automatically signify theoretical insignificance or superficiality. Instead, the relevancies vary: and so must one’s experimental devices.

I do not, then, see intellectual analysis as distinct from practical work; rather it is a specific way of participating in it. In my view, case-studies in STS should have two not necessarily unrelated aims. They should work to develop the thought styles of their own and related disciplines. And they should work to make sense of their cases, in ways which are possibly of some use to the practitioners they talk about. The success of both goals hinges on learning to be responsive to the actualities of the field.

While the audience for the present text is imaginably going to be rather limited, given the situation outlined above, it nevertheless has the potential to be diverse, including scholars trained in classical social theory, critical and cultural studies in science and technology, researchers in management and policy-studies, and in medical informatics. Since, as I have described above, an important aim of the text is to make visible and problematise a series of assumptions, residual or core, rationalist, functionalist, positivist, or of classically critical bent, which some or all of these audiences hold to various extents, I imagine most readers to find the read frustrating and and provoking at times, but hopefully also challenging and generative at others.

3. Recursive Organisation

The thesis you are about to read will surely give the impression of an annoying proliferation. Perhaps it will be said that its author was unfortunately unable to define and master her subject. If mastery means the purified definition of an object that at the same time determines the tools that are adequate and the questions that are legitimate, then I recognize the truth of the criticism without the slightest scruple, but I would like, from this moment on, to explain why I was not unable but unwilling to purify my object (Isabelle Stengers 1982-3:1, as cited in Alliez 1996: 245-6n29)
I now turn to the question of how my intellectual agenda, as broadly outlined above, is played out in the organisation of the text. This is not an easily formal matter. My empirical material (as described below) has given rise to a very heterogeneous picture of the EPR. How to elegantly intertwine a range of theoretical considerations from social theory with adequate empirical description of Danish EPR projects? Perhaps citing Isabelle Stengers’ unwillingness to purify her object is pretentious given my results but at least her phrase invokes the aspiration to offer illustrations and examples, which elucidates the loose coherence, but also sometimes troubled contestations between issues observed in practice and (other issues) engaged with in theory. At least the contrivance in this organisation is explicit. It aims deliberately to maintain a certain discontinuity between themes and issues, while hoping that conceptual flexibility rather than undifferentiated confusion will result. All chapters navigate this risky territory, picking up bits and pieces of “theory”, discussing “method”, and “describing” empirical material. This is not an attempt at bricolage; a more properly recursive trope would be that of the fractal.

Lest it be thought that all is here chaos, I should ensure the reader that there is some order in what follows. With due qualifications part one “Resources” offers a theoretical and methodological tool-kit. Part two “Strategies” and part three “Illustrations” may be considered empirically oriented, while the emphasis in part four “Implications” is as indicated by the title.

Part one “Resources” has three chapters. The first, “Resources for Non-Humanist Studies of Technoscience: A Theoretical Assemblage”, is the most classically theoretical as it locates the present studies in an intellectual landscape. In it, I attempt to delineate a set of resources, which can function as experimental devices effecting conceptual or practical transformation in their encounter with the EPR (in later chapters). The topology is non-humanist STS, post-classical, constructivist thought, post-humanism and post-structuralism. Drawing on a broad range of theorists I develop an analysis surmising that non-humanist studies of science and technology aim rather differently than traditional critical scholarship because they understand these relationships as thoroughly performatve rather than representational, and ontological rather than epistemological.
These considerations are of methodical consequence. Chapter two “Qualitative Research as Partial Connection: Bypassing the Power-Knowledge Nexus”, co-written with Peter Lauritsen, aims to elucidate some important lessons for qualitative research as it has been imagined and discussed in recent years. Qualitative research, we argue, has leaned upon a dualist understanding of power and knowledge, the power-knowledge nexus, which has led to a predictive but rather facile discussion between modern and postmodern conceptions of the field. Identifying this nexus as one of Deleuze’s “bad problems”, we argue that the dichotomy can be suspended by paying attention to a number of non-humanist arguments.

Chapter three “Researching Partially Existing Objects: What is an Electronic Care Record? Where do you find it? How do you study it?” leads me towards the electronic patient record. Drawing on and extending insights developed in the two first chapters, I argue that this putative entity is far from unequivocal. In fact, it seems to have a paradoxical quality because, rather than being a singular, modern thing, it is a partially existing object.

The “Strategies” referred to in part two are reading strategies. The question raised is how one might engage or diagnose sociotechnical events as they are presented in multiple reports, mission statements and other political documents that proliferate wherever technical development is promoted. I view these writings as performative and multiple, rather than as simple communicative vehicles of intentions and ideas.

Chapter four is called “Political and Moralising Moments: Documents as Material Agents in Danish Visions of IT in Health Care” and is co-written with Brit Ross Winthereik. In it, we analyse documents from two IT-projects in Danish health care and propose that they can be seen as instantiating a certain information genre, ideal for flexible envisioning.

Chapter five “Reading Digital Denmark: IT-reports as Material-Semiotic Actors”, co-written with Peter Lauritsen, engages the theme of reading strategies head on. Through analysis of the important Danish IT-strategy Digital Denmark it exemplifies two such strategies, which we call reading “with” and “against” the text. The latter constitutes a classically critical and sceptical reading aimed at the uncovering of hidden or repressed meanings, which may be ascribed to the bad motives or dubious intentions of authors.
The former suggests instead that one follows the text as it travels in practices, and describes its concrete effects: where do they go? what do they do? This contrast is of normative significance and we discuss various political implications of reading “with” or “against”.

“Illustrations” follow the EPR into a number of more or less related settings. One could say that they follow EPRs in time and space, but it would be more precise to say that they follow how a time and a space is constructed, which the EPR could call its own.

Chapter six is called “The ‘Seamless Web’: Discourse Coalitions and Story-Lines in Developing the EPR”. The chapter is based on interview material from the EPR project in the Aarhus Region in Denmark. Although it makes theoretical points it functions primarily as background, setting the stage for later chapters. I discuss the modular organization of the Aarhus project, and emphasize that no element in this organization was given but had rather to be carefully constructed. The terms discourse coalition and story-lines are adopted to describe how this particular construction was imagined and what was done to make it hold.

Chapter seven “An Experiment in Performative History: On the Construction of a Future-Generating Device in Danish Medical Informatics” effects what may be perceived as a zooming out. Compared with the “locality” of the Aarhus project, this chapter engages the longer-term history of Danish medical informatics. It deals with the very heterogeneous interests of the members the Danish Society for Medical Informatics: from clinical societies and their databases, the red and green patient administrative systems, to the FynCom experiment with EDIFACT message exchanges and the set-up of a medical informatics department at Aalborg University. I argue that Rheinberger’s notion of a future-generating device can help capture important aspects of this technosocial genealogy.

Chapter eight “Standardisation and the Logic of the Different” is on controversy both in STS and in the Danish EPR networks. The issues are standardisation and standardisation research. The question in STS has to do with how to conceptualise the possible effects of one’s studies (in this case of standardisation): do STS studies generate normative, practically relevant knowledge? If not, what do they then do? This discussion is broached by surveying standardisation controversies in the Danish and European EPR
landscape, from the CEN TC-251 working group to the National Board of Health and regional hospitals. I invoke Lyotard’s logic of the differend to carry out this analysis in a manner, which pays due attention to the potential for incommensurability (both in the STS and the EPR fields).

In the following chapter, “Citizen Projects and Consensus Building at the Danish Board of Technology: On Experiments in Democracy”, the analytical move is in the other direction: how may one deal in a democratically responsible way with the problem of incommensurability. The chapter describes my participation as an expert in a project carried out by the Danish Board of Technology, to elicit citizen’s preferences for national Danish EPR development. Such consensus projects have been widely praised as counter-points to highly technocratic conceptions of the relationship between experts and citizens, and as I discuss, they may generate interesting and democratically relevant effects. However, I pose a number of challenges to such projects as they are currently conceived, by proposing to see them as inherently experimental situations.

In chapter ten I turn to “Infrastructural Fractals: Re-Visiting the Micro-Macro Distinction”. This chapter attempts to make sense of the transformations described in earlier chapters by analysing them as fractal. Such an understanding would be attentive to the multiple relationships encountered, and would suspend with the need for sorting such relationships into micro versus macro, small versus large, individual versus social, or important versus unimportant.

The final part is called “Implications” and has two chapters. Chapter eleven is called “Technologic: Conceptualising Organizational Transformation with the EPR”. I suggest that a technological pattern of sorts can be detected in the empirical material encountered so far. This ‘techno-logic’ resembles what Jacques Derrida has called a logic of supplementarity, and it poses particular problems and challenges for the conceptual and practical management of the relationship between EPRs and the organization of hospitals wanting to implement these technologies.

Chapter twelve “Established Sentiments, Alternative Agendas, and Politics of Concretisation” outlines a set of arguments by Barbara Herrnstein Smith and Isabelle Stengers, which have been especially formative for the present analyses. I suggest that behind their superficial opposition an intellectual topos can be opened up for exploration,
which would be highly relevant for normatively interested, non-humanist, post-classical studies of science and technology.

In spite of the lack of overt critical position-taking the following essays may be read as an attempt to reformulate dominant ways of thinking about various problems and themes in different research communities such as medical informatics, management and organizational studies, learning theory, policy studies, sociology, history, philosophy, and perhaps even literary theory.

I do not imagine that the present studies will change the practices or the theories of any of these disciplines very dramatically. But perhaps a less ambitious goal will do. If these essays generate in some readers some novel thoughts which they can bring to bear upon some of their problems elsewhere I think my effort will have been well spent.

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1 For reasons of textual economy I use the not-so-generic term “him” and “he” exclusively in what follows.
3 Pun intended. Derrida’s logic of supplementarity (Derrida 1976) is discussed as an empirically observed techno-logic in chapter eleven.
4 Admittedly I am not a schooled philosopher (as philosophers have sometimes been keen on letting me know) and the term may therefore be misleading. More precise would be empirical theory, which unfortunately sounds horrible. I have therefore stuck with Mol’s term.
5 In ”Netting Truth”, Barbara Herrnstein Smith glosses Ludwig Fleck’s denkstil, a notion defined in multiply overlapping ways throughout Genesis and Development of a Scientific Fact (Fleck 1979), in the following way: ”…the emergence and specific features of such events [in the history of thought] are made possible but also severely constrained by the operation of particular thought styles (Denkstilen): that is, systems of ideas and assumptions and related perceptual, classificatory, and behavioral dispositions that prevail among members of particular epistemic communities or, in his terms, “thought collectives” (Denkkollektiven)” (Smith 2000: 1091). Fleck notes that “All empirical discovery can…be construed as a supplement, development, or transformation of the thought style” (Fleck 1979: 92, emphasis in original).
6 Although one would expect that the understanding of what it could mean to be "of use" for practitioners would need to change from rationalist or instrumentalist interpretations. STS-studies would also be of help in specifying how (see chapter eleven and twelve).
Part one: Resources

And when it comes to neglecting fundamentals, I think I have nothing to learn, and indeed I confuse them with accidentals (Samuel Beckett as cited in Smyth 2002: 1)

“Resources” has three chapters. The first, “Resources for Non-Humanist Studies of Technoscience: A Theoretical Assemblage”, is the most classically theoretical as it locates the present studies in an intellectual landscape. In it, I attempt to delineate in a coherent manner a set of resources, which can function as experimental devices effecting conceptual or practical transformation in their encounter with the EPR (in later chapters). The topology is non-humanist STS, post-classical, constructivist thought, post-humanism and post-structuralism.

It should be noted that while I aim to be thorough I do not try to be comprehensive. All concepts in chapter one will be taken up in one or more later chapters, but not all concepts mentioned there will be found in chapter one. However, I imagine it to be possible to return to this “theoretical assemblage” in order to locate the sense and context of any term later encountered. I describe how Bloor’s symmetry doctrine and the more recent practice-turn in science and technology studies in different ways have challenged classical analytical epistemology and philosophy of science. Bruno Latour and Michel Callon’s generalisation of the principle of symmetry radicalised this challenge by advocating the inclusion of non-humanist actors in STS analyses.

The proliferation of relevant actors has led to an increasing complexity in such studies. This has been important for consideration of multiple issues, but discussions of the consequences of this shift for analysis of “power” and the possibilities for “critique” have been particularly vivid. Drawing on a broad range of theorists in STS and cultural theory, I develop an analysis of this issue by surmising that non-humanist studies of science and technology aim rather differently than traditional critical scholarship because it understands these relationships as thoroughly performative rather than representational, and ontological rather than epistemological.

Such arguments are of methodical consequence. Chapter two “Qualitative Research as Partial Connection: Bypassing the Power-Knowledge Nexus”, co-written with Peter Lauritsen, aims to elucidate some important lessons for qualitative research as it has been
imagined and discussed in recent years. Qualitative research, we argue, has leaned upon a dualist understanding of power and knowledge, the power-knowledge nexus, which has led to a predictive but rather facile discussion between modern and postmodern conceptions of the field.

Identifying this nexus as one of Deleuze’s “bad problems”, we argue that the dichotomy can be suspended by paying attention to a number of non-humanist arguments. We analyse the transformational capacities of terms such as Latour’s *chains of association*, Haraway’s *situated knowledges* and especially Marilyn Strathern’s *partial connections* and exemplify some of their effects by discussing Joan Fujimura and Danny Chou’s (1994) study of the controversy over AIDS-etiology.

Chapter three “Researching Partially Existing Objects: What is an Electronic Care Record? Where do you find it? How do you study it?” leads me towards the electronic patient record. Drawing on and extending insights developed in the two first chapters, I argue that this putative entity is far from unequivocal. In fact, it seems to have a paradoxical quality because, rather than being a singular, modern thing, it is a partially existing object.

The chapter argues that the problematic status of the EPR, as it is perceived by a non-humanist (and here I add *nominalist*) does not make it impossible to find or study, but engenders a different (and more fluid) field of problems and solutions. Rather than pre-defining topics, which must be taken into account, one tries to empirically track down variable definitions of the situation and the issues they entail. And rather than evaluating current practice as to its adequacy according to one’s externally defined standard, one participates with other actors in experiments to redefine the relevant contours of these issues. For such transformational purpose all actors draw on their own resources and experiences: all bring their own experimental devices to bear on the outcome.
Chapter one:

Resources for Non-Humanist Studies of Technoscience:
A Theoretical Assemblage

How to turn an opposition into a possible matter of contrast? Obviously, this is not only a question of goodwill. My guess is that we may do so through the experimental extension of the specific risks that singularize each position. Giving a chance for contrasts to be created where oppositions rule implies producing a middle ground but not a medium or average mitigating differences. It should be a middle ground for testing, in order that the contrasts evolve not from tamed differences but from creatively redefined ones (Stengers 2002: 236-7)

My work is situated within STS (science and technology studies). Locating it thus does not, however, determine its specific features very predictably because STS, like other fields of inquiry, is heterogeneous as regards the assumptions, theories, institutional affiliations, methods, approaches, goals, and interests of its practitioners. Even so, STS today can also be characterised as a relatively stable enterprise, with its own conferences, journals, professional organizations, and graduate programs. With Bruno Latour, one could talk about the black-boxing of STS, although Kathleen Jordan and Michael Lynch’s term “translucent box” might be more fitting (Latour 1987, Jordan and Lynch 1992).

In the following I want to make explicit the theoretical apparatus I bring with me in empirical studies. This, then, will be an exposition of STS-theory. But before starting I want to resist this simple identification. One reason for doing so is that the diversity and differentiation of the field makes any one description of it problematic.

As has been argued, disciplinary “looseness” is often an asset, rather than a weakness, for developing sciences and STS has certainly exemplified the point (e.g. Clarke and Fujimura 1992). Making a unified description of the themes, methods, and problems of the field would furthermore be false to some of the most important findings of STS; that scientific practices are always shaped in the historically contingent interactions of multiple kinds of actors with different aspirations, habits, and tendencies. The gesture of presenting one’s study as a ”strong case” by emphasising the coherent theoretical basis on which it is built thus reproduces an idea with which STS-practitioners have regularly taken issue; that the homogeneity of a scientific community is necessarily
a measure of the scientific merit of that community, and therefore conveys, or at least ought to convey, additional credibility to the statements of its members.¹

In my work I have drawn upon a range of ideas and concepts from scholars and researchers not all of who are from STS. Here, in particular, I refer to what are often called post-structuralists; Gilles Deleuze, Jacques Derrida, Michel Foucault, Jean-Francois Lyotard, Michel Serres, and some of their numerous interpreters and followers. All of these people are not talking about the same issues and they are not saying the same things. It is therefore not my interest to try to integrate all of them in a common eclectic framework.

Instead, I take my cue from philosopher of science Isabelle Stengers who, in the citation above, wonders about how to turn differences or, indeed, antagonistic oppositions into productive intellectual contrasts. And she suggests that a solution could be found in experimentally processing sets of positions through each other. Productivity would be created at the middle-ground, where no position would be able to silence any other, and where differences would therefore have to be respected, rather than made to disappear (either by force or by consensus; which is often a more invidious force since it is rarely recognised as such).

The latter qualification is important, because it emphasises that tolerance of alternative perspectives is not necessarily, or not always, the ideal (see chapter twelve). Specifically, it means that in the following I am not prevented from noting and, indeed, going some lengths to stress my disagreements with a number of positions within STS and more broadly. For I imagine that it is only by painstakingly working to clarify differences, rather than glossing them in the name of a pluralistic ”good-will” that a serious evaluation of possibilities, limits, and implications of various theories and their relationships can take place.

What follows is thus the presentation of some parts of STS-theory but also an experiment in the expansion of relevant resources for STS-theorising. Rather than presenting a history of the developing theoretical sophistication of students of science and technology, for example from Mannheim and Merton to Latour and Haraway, via Kuhn and Feyerabend, Bloor and Barnes and numerous other important figures, I try here to present a kind of theoretical assemblage, drawing selectively on multiple figures without, I hope, unnecessarily disfiguring them in the process.²
1. The "Practice-Turn" in Science Studies
As indicated by the name "Science and Technology Studies", researchers in this field have been interested in closely investigating differing aspects and relationships in the extremely broad sphere of contemporary life, which has to do with science and technology. They have done so from numerous approaches, as can be ascertained in helpful survey articles (e.g. Olesen 1996, Rouse 1993, Traweek 1993). A general change, which has occurred in these studies over the last decades, is from focusing exclusively on the content of science to focusing on the intertwining of content with multiple practical and material aspects of laboratory life.

Obviously, the practical and material effects of scientific knowledge; the technologies, which it has enabled the invention of, and the many applications it has made possible, has been regularly taken as an important indication of the superiority of Western science over all other systems of inquiry. However, the practice-turn in recent socio-cultural inquiries into science and technology refers to a quite different phenomenon, which is in an important sense corrective to the received view; that is, that if it works it must be true. For it focuses not only on material effects as they are enabled by scientific ideas, but also on the material, practical, and institutional aspects as they are participative in the construction of scientific content. This change of emphasis, which has been driven by empirical inquiries into the ways in which sciences are practically carried out, has been of consequence for discussions about epistemology and ontology.

2. Challenging Epistemology
Epistemology, in the tradition of analytic philosophy of science, concerns itself with investigating the foundations of certain knowledge. This inquiry has been almost universally premised on the idea of a split between the ideal and the concrete, and prioritised the abstract capabilities of the mind over the inadequacies of the body. Since scientific ideas are generated in the interaction with obdurate materials with unknown qualities, a prominent concern of epistemology has been with purifying science from the many biases, which could potentially invalidate its knowledge in this interaction. The point is thus not that epistemology refers to a realm of pure ideas, but that it tries to establish an ideal relationship between the level of scientific ideas and the level of their
practical validation and application. In contemporary epistemology this process of purification has been carried out on the level of method. Scientific method has been seen as the guarantee of the universality of knowledge if properly applied (in opposition, for example, to knowledge as contaminated by the partisanship or local provincialism of its producers), although the precise definition of what the scientific method would consist in and what it would mean for it to be properly applicable remains contested.\(^7\)

The challenge posed to classical epistemologists by STS-research, however, has been much more severe than these internal epistemological quarrels.\(^8\) For in insisting on the participation of practical and material effects in the production of knowledge, these studies have problematised some of the key-distinctions and relations in epistemology; for instance between knowledge and power, and between (scientific) ideas and their (technical) concretisations. By doing so they have ineluctably challenged the very goal of epistemology; of guaranteeing the possibility of formulating true, in the sense of decontextualised and universally reliable, statements about the world. This challenge of constructivism is of wide-ranging ramifications for the conceptualisation of science, technology, society and their interrelationships, as I will proceed to discuss in the rest of this chapter.

3. Principles of Symmetry

These ramifications are themselves variably reviewed depending upon the strand of STS-studies of one’s adherence (such as, for instance, standpoint feminism, sociology of scientific knowledge, social epistemology, symbolic interactionism, cultural anthropological studies of science or actor-network theory). Many of these studies would in principle agree with the famous symmetry doctrine, postulated by sociologist of science David Bloor, which proposes that statements that we take to be true and statements that we take to be false should be accounted for with the same set of explanatory devices (Bloor 1976).\(^9\) But what is viewed as following from this doctrine is highly variable.

In the ”chicken debate” between sociologists of science Harry Collins and Steven Yearley, reflexivist Steve Woolgar, and actor-network theorists Bruno Latour and Michel Callon, the former formulated one important version of the consequences of adopting a constructivist stance in the exploration of science.\(^10\) Collins and Yearley suggested a
dualistic model, referred to as *alternation*, in order to account for how one could simultaneously take seriously the realist findings of scientific research and the constructivist findings of science studies research. In matters scientific, Collins and Yearley suggested, we have no better bet than taking at face value the pronouncements of experts since they, not we, are the specialists in their respective areas. However, when it comes to explaining their means of achieving agreement on these matters, authority should be deferred to the sociologist of science, since this is his area of expertise. This model gives to the scientist with the one hand the epistemic authority, which it seeks to remove with the other. On the one hand the epistemological realist position of science is granted, but it is then doubled by the position of the sociologist, who is able to *really* point to how realism is the result of the open and negotiable work of scientists. In this gesture, common to Harry Collins and Pierre Bourdieu, the *final say* in epistemic matters is thus conferred back to the sociologist.\(^\text{11}\)

Similar double-movements of endorsing (anti-)epistemological principles but forgetting about their implications at the time when they would reflexively apply to one-self, a strategy which Barbara Herrnstein Smith has referred to as ”cutting-edge equivocation” (Smith 2002), are found in a number of STS-studies that claim to offer specific kinds of political leverage in their engagements with scientific practices, such as the ability to *criticise* or *resist* the status quo.\(^\text{12}\) So where to go from there?

### 4. From Epistemology to Ontology

In the view I present here it will be suggested that the challenge of constructivism is intimately bound up with a change from what could be called an epistemological to an ontological approach to the understanding of science.\(^\text{13}\) The gist of this change can be nicely summarised in a formulation of Bruno Latour’s: "*Essence is Existence and Existence is Action*" (Latour 1994), but its philosophical history can be traced, at least in some of their interpretations, to Greek philosophers such as Heraclitus and Lucretius, to Leibniz and Spinoza, to Friedrich Nietzsche and A. N. Whitehead and, unsurprisingly, to different radical thinkers in recent French philosophy who, in effect, have worked to re-interpret several of the above-mentioned; I think here in particular of Gilles Deleuze, Michel Foucault, and Michel Serres.\(^\text{14}\)
Essence is Existence. This is a claim which denies the purity of the ideal and refers all there is, in the first instance, to the material world. Existence is Action. But what exists? We do not know, at least not comprehensively, or not yet. But what this second formulations suggests is that we can try to find out; for, often enough, action and activity is empirically observable. Not, however, as something simply out there. For as scientists well know it is only through an organized and co-ordinated effort, using multiple machines and devices as mediators, that different entities become able to reliably ”express themselves”. The enabling by humans of such ”expressive displays” can be characterised as ”events” because of their unforeseeable character. The implication of this view is that novel aspects of the world (in the shape, for instance, of new effects, particles, or phenomena) may be articulated in the laboratory only because of the constellation of the particular forces that constitute the given experiment in which they so display themselves. As such articulation takes place at the intersection of (sets of) forces, which we regularly categorise as ”observers”, ”instruments”, and ”the natural world”, the distinctions among these forces and the properties that go with them themselves become problematic and are turned into topics for investigation, rather than taken for granted as resources in the investigation of science and technology (e.g. Latour 1987).

I have been talking here, in somewhat different words than usually used, about Michel Callon and Bruno Latour’s proposed generalisation of the doctrine of symmetry (Callon 1986, Callon and Latour 1992). This extension suggests that while it is true that all beliefs should be treated symmetrically, as Bloor suggested, STS should also develop models and concepts to account for the fact that there are large differences between the effects of beliefs. In their model, the stabilisation (legitimation, institutionalisation) of some set of beliefs and practices rather than others is crucially dependent on the successful delegation of actions and responsibilities to non-human actors (and vice versa), and their consequent practical re-definitions or translations. The inclusion of non-humans into the analytical picture is a move the meaning and consequences with which I will remain engaged in what follows.
4. Complexity

Following the doctrine of symmetry, I study techno-scientific collectives while taking
seriously, but not investing too much in, the self-descriptions of their practitioners.
Symbolic interactionists and cultural anthropologists have described how all human
beings are members of multiple social formations, each with its own requirements for
successful participation (e.g. Foster 1995, Franklin and Ragoné 1998, Star 1994). They
have told sophisticated stories about the artful work needed to successfully integrate the
many different and sometimes contradictory exigencies of stabilising social identities. In
recent years these stories have been extended to also cover the social formations involved
in techno-scientific production, and this has been one important strand in the
depurification of contemporary understandings of science. But what happens when non-
humans are added to the collective we want to describe?

Perhaps a first experience is one of increased complexity. Indeed, in some research
in cultural studies and STS the delight in making visible complexity seems to
overshadow the question of what productive differences such re-description could render
pertinent. Another perspective would view the notion of complexity as a “lure for
feeling”, with the capability of generating new, different and, perhaps, harder questions
for us to answer about sciences and society. This is the proposal of Isabelle Stengers:

As for the notion of complexity, it sets out problems – we don’t know a priori what “sum
of parts” means – and this problem implies that we cannot treat, under the pretext that they
have the same “parts”, all the “sums” according to the same general method (Stengers
1997: 12-13)

In this suggestion, noticing complexity is the mere beginning of the process of
understanding and transforming relations between the sciences and society. It is a
necessary beginning, however, because relevant questions regarding a given situation can
only be formulated if the situation has first been de-composed into enough divergent
elements to prevent its simple evaluation.

5. Power and Coalitions

…[T]oday, most technological-social innovations affect things in much more varied
modes than those anticipated by our questions, and thus create a gap between “things”, as
they are implicated in it, and their scientific representation (Stengers 2000:158)
That scientific innovations are effective in a multiplicity of ways, only a small number of which are anticipated is surely a lesson, which has been well learned in the past century (e.g. Mackenzie 1990, Perrow 1999, Fortun 2001). Stengers is not the first, or only, scholar to point to the fact. In the domain of the “human sciences”, for instance, Michel Foucault, has carefully analysed and described the multiple socio-political effects, for better and worse, of the invention of modern medicine, psychiatry, and criminology. In this work he has pre-figured, as well as functioned as a tremendous inspiration for, research in STS and, of course, numerous other disciplines. In order to take up the challenge of Isabelle Stengers; how to respond inventively to the fact that non-human actors are increasingly brought to bear on our lives in ways we not only do not, but probably cannot anticipate, some of the socio-political ideas of Foucault’s could prove useful.

Foucault’s political thinking was concerned, among other things, with how to “cut off the king’s head” (Foucault 2001: 122), by which he meant constructing a mode of political analysis, which would not primarily be organized about the classical themes of sovereignty and law, and which would consequently not have to imagine that power comes “from above”. Instead he would be interested in the ”play of power” as instantiated in myriad microprocesses throughout the social field. In different analyses Foucault showed how sets of practices were slowly and painstakingly composed, not least through the stabilisation of specific discourses, even though no common interests between their constituents existed prior to their engagements. Power (as efficacy of action) seemed thus not to be inherent in some actors (and not others), but rather to be always in the making. By considering socio-politics from this transformational viewpoint; as having to do with the composition of coalitions out of heterogeneous elements, rather than as having to do with stable formations with specific pre-defined interests and powers, Foucault could view the construction of disciplinary and institutional matrices as ”intentional but non-subjective”, rather than enforced by the powerful. This descriptive conclusion, based in historical analysis, also functioned as a practical heuristics in Foucault’s own political engagements. Since, in Foucault’s phrase ”power is everywhere”, but inherently unstable and transformational (because it is shaped in the ongoing interactions between practices, discourses, institutions etc.) to
resist specific functions of power, one must become *as flexible as it is*. As no overall political programme needs to be constructed for there to be resistance, no heavy apparatus *mimicking power* would be needed to *oppose it*.

The Foucaultian (and Nietzschean) conception of power and the conditions for political efficacy has been theoretically developed in recent political theory by scholars such as Ernesto Laclau and Chantal Mouffe in their influential *Hegemony and Socialist Strategy*, by Wendy Brown in *States of Injury: Power and Freedom in Late Modernity* and *Politics Out of History* and by Judith Butler in *Excitable Speech: A Politics of the Performative* (Brown 1995, 2001, Butler 1997, Laclau and Mouffe 1985). It has also been investigated in STS through case studies such as Steven Epstein’s *Impure Science* on gay and lesbian activism which shaped and redefined AIDS-research, Andrew Barry’s work on ecological activism, and Brian Wynne’s writing on sheep farmers’ controversies with scientists (Barry 2001, Epstein 1996, Wynne 1996, see also Dutton 1988, as cited in Stengers 2000: 128, 161).

6. Co-ordinating Science and Society

In the above examples I have focused on coalitions ”external” to normal science which formed in order to resist specific issues or kinds of development, but I do not want to view this simply as a matter of ”outside forces” trying to ”influence” what is (or ought to be) ”internal” to science. For, of course, one point in re-describing scientific controversies in the language of shaping and transforming coalitions is to point to the fact that society is *never* external to science. On the contrary the success of activists in challenging and redefining, for example, AIDS research can be seen as *one more bridge* being built between the “insides” and “outsides” of science. If this is a (partial) success story it is for the double reason that the articulation of parts of the AIDS research community with the agenda of activists *simultaneously* improved the situation of both communities: it enabled the scientists to do better science *and* improved on the lives and the chances of surviving for people with AIDS.

However, building successful coalitions between the outside and inside of science is a difficult task, because the sets of practices, relevancies, and interests of those who would need to co-operate are often vastly divergent. As the articulation of a novel entity in a laboratory can be described as an *event*, so can the (partial) success of the AIDS-
coalition in re-defining the relationship between science and the public, and for the same reason; that it required a unique constellation of forces which, far from being given had to be painstakingly constructed in order to effectively "express itself". Such technoscientific events can be characterised as experiments in democracy when they succeed in conferring on all interested parties the capacity of expressing a viewpoint, without trying to pre-determine what it is, or what the consequences of it should be.²⁴

STS-studies in various instantiations have been interested in analysing how the creative potential of scientific practices is opened up precisely to the extent that ideas, programmes, or techniques have had to be shared among what proto-STS scholar Ludwig Fleck referred to as different thought collectives with different thought styles (Fleck 1979). Scholars such as Leigh Star and James Griesemer has shown how the co-ordination of complicated scientific efforts is not only not dependent on the homogeneity of memberships of those trying to work together, but that disparity of goals of the cooperating members is productive for its realisation (Star and Griesemer 1989). In their work, which takes seriously the Latourian suggestion to incorporate non-human agents in our analyses, they show how material entities can function as boundary objects that give just enough structure to the interactions between members from heterogeneous social worlds (or thought collectives) to enable the successful co-ordination of their efforts, but does not try to impose on members the same definition of the work they are trying to accomplish. In their view, then, it is the mediation of non-human actors, which allow members of different social worlds to productively articulate their similarities and dissimilarities.²⁵

I started off this section by invoking Isabelle Stengers’ proposition that there is a gap between things as they become effective in the world and things as they are regularly scientifically represented. This led me into discussing coalitions as a means of engaging sciences, with the purpose of narrowing the gap between expectation and effect under specific circumstances; such as those of AIDS-research. Interestingly, this can be seen as a way of coming full circle back to the traditional philosophical questions of how to address questions of predictability and reliability in scientific research from a non-traditional angle; notably one which does not rely on a notion of universality to function successfully.
Finally, I discussed how the actual work of co-ordinating coalitions, of building bridges between different social worlds or thought collectives could itself be seen as a possibly inventive endeavour, and I pointed to non-human actors as important mediators and facilitators in the process. In the following I would like now to re-connect this discussion to my initial consideration of epistemology and ontology.

7. The Collapse of Epistemology into Ontology
Stressing the intertwinem ent of human and non-human actors in science offers an alternative to traditional epistemology because activities such as observing or representing are not seen as distinct from intervening or constructing; rather they are viewed as specific ways of intervening and constructing. In this view epistemology collapses into ontology, and the sciences are reformulated as activities aimed at (re)-building the world by adding new elements with new capabilities and new relationships to it. Knowing becomes a way of doing ontology, and ontologies are forged in practice.

Interestingly, this idea is almost as foreign to social constructivism as it is to epistemology because its focus is on the eventful reconfiguration of reality, taking place in laboratories and elsewhere, rather than on the replacement of naturalist explanations of science with social or cultural ones. The move, suggested in the Callon and Latour’s notion of generalised symmetry, of treating the sociality and the naturalness of the sciences as equally troublesome, thus opens up a space for viewing the sciences as vehicles for the construction of many different socio-natural entities; for a multi-naturalism replacing both (traditional, realist) mono-naturalism and multi-culturalism (see e.g. Latour 1999, Verran forthcoming). This position clearly leaves no room for the epistemological aspiration to define a method for the generation of objective knowledge within a discipline, much less in general, because knowledge is constructed precisely at the intersection of the many different agencies concretely interacting in the world. This condition of specificity, of course, does not prevent technoscientific constructions from becoming consequential on much vaster scales "in the hands of later users".

8. The ”North-West Passage”, Orders and their Others
Michel Serres has provided a nice analogy for knowledge-production by comparing it to the navigation of the ”north-west passage". Travelling the north-west passage has many
hazards such as wild tempests and drifting ice-floes but if these perilous conditions can be successfully navigated, novelty awaits at the end of the journey in the shape of a novel (and unexpected) environment.

In spite of the connotations of scientific heroism, which this image may evoke, I do not think glorification is the main point Serres is trying to make. It is possible instead to make some different observations. First, Serres, as Whitehead before him, presents a view of science as an adventurous journey in which curiosity and *jouissance* figure as important components. But it is a journey with specific risks and dangers. This is not a very esoteric observation, but one to which people as diverse as Cumbrian sheep-farmers and ACT-Up AIDS-Activists can testify. Since we really *do not know* how the landscape we encounter at the end of our journeys will look, this poses to scientists the important challenge of learning how to become responsible for all the entities of our making which, nevertheless, “we are not mastering” (Stengers 1995: 11). Likewise, it poses to our societies and institution the challenge of learning how to innovatively respond to the world-building activities of scientists and others.

Two additional points can be made. Throughout the Serresian sea journey disorder and uncertainty reigns, and multiple contingencies must be handled to prevent the ship from perishing. Only at *the end of the trip* does it make sense to credit the traveller with the courage and rationality necessary for its completion. But the ordering thus achieved also comes with a price, which is often forgotten; that now the traveller is *somewhere* and for that reason *not everywhere else*. Scientific orderings (practical, institutional, conceptual) thus always come packaged with their own disorders; their forgotten questions, irrelevancies, impracticalities and other invisibilities, which Marc Berg and Stefan Timmermans has called ”orders and their others”, and as Adrian Cussins has pointed to in his theory of cognitive trails (Berg and Timmermans 2000, Cussins 1992).

9. STS and Cultural Critique

The above-mentioned discussions may seem fairly abstract. Nevertheless scholars of widely diverging observations and from several disciplines have taken issue with a broad array of the claims made here, because of their presumed implications. Many of these focus on what they take to be the quietistic properties of positions such as the above-mentioned; that is, they see the unwillingness of such positions to engage normatively
with what they study (or, in other words, their insistence on rendering accounts symmetrical) as leading to complicity with the powers that be.\textsuperscript{33} Since this argument is recurrent, it is worth here to dwell on some of its sources and consequences, and I proceed to do so by engaging with a recent commentary by Sande Cohen, which has had wide circulation as it was printed in the \textit{Science Studies Reader}.

Cultural critic and self-proclaimed deconstructivist\textsuperscript{34} Sande Cohen attacks the ”language suppression” of Bruno Latour, by which she refers to his putative inattentiveness to the troping of his own discourse, and the imperialism of his rhetorics.\textsuperscript{35} This attack is fairly unsurprising since Latour has spent a good deal of writing energy demonising what he refers to as ”deconstruction” and ”postmodernism”.\textsuperscript{36} Since attentiveness to language is surely not a bad thing, whereas its obverse might well be, since deconstructive strategies has, indeed, already turned out to be beneficial to STS-studies\textsuperscript{37} and since, finally, Cohen’s interpretation of the consequences of Latour’s position is in line with a number of philosophical and cultural critiques in its suggestion that, ultimately, it is \textit{complicit} with existing power structures, some consideration of her commentary is warranted.

An obvious response to Cohen’s argument is that far from suppressing language Latour lets it overflow, I would say elegantly, in a stream of images, tropes, and word-plays, of which some, admittedly, are bluntly targeted at audiences they perhaps need not have aimed at. Exuberance might thus work better than suppression as a characterisation of the rhetoric of Latour.\textsuperscript{38} This already makes a difference because it does not posit Latour as a benighted writer of his own texts. So in effect, I would suggest that Cohen has identified a false problem. But her identification is instructive because it articulates an important difference between a \textit{critical} (deconstructive or otherwise) and a Latourian (and, I would say, Deleuzian) understanding of writing.\textsuperscript{39} For Latour, as discussed noted, essence is existence is action, and surely texts are effective in many ways. Thus texts, too, are delegates that can function in more or less effective and faithful ways for their writers. This, of course, is also a deconstructive insight, and probably one, which Latour has in part gleaned from Derrida, although he has never stopped to acknowledge it.\textsuperscript{40} But he draws from this insight a different lesson than the deconstructivist or the cultural critic. Whereas Sande Cohen becomes interested in taking apart discourses to show the many layers and internal destabilisations, which form the basis of their functioning, Latour
becomes interested in using exactly these capacities of texts to construct new ways of thinking about and acting upon the world.

Maybe this argument is hedged because Cohen can claim that these are also her goals. But the difference nevertheless remains because the Latourian assumption of the relationship between texts and readers is quite different from that of deconstruction. In deconstruction, Latour suggests, the problem the writer faces is that the reader is too naïve; too prone to be taken in by the subtleties of logocentric discourse (Latour 1988). Deconstructive reconstruction is therefore predicated on the need for first taking apart established logocentric binaries and then, slowly and with due qualifications, indicating the possibility for a future change. This analytic movement is slow and grinding so as to ensure that the reader cannot escape its consequences and naively fall back on his preconceptions. For Latour, on the other hand, the reader is presumed to be most of the time indifferent to the activities of the writer. For the text to function as a successful delegate, what is needed is therefore not (or not always) a rigorous slowness, but a lightness and vividness capable of seizing the reader and holding firm his interest. Deconstruction sometimes presents itself as a critical analysis (even as it is reformulating the notion of criticism) simultaneously and interminably pointing to the logocentric assumptions of both the text under investigation and those of its own making. Latourian texts, on the other hand, are constructive efforts using all available means to effectively make a case.

Robert Koch has, I think correctly, identified the position of Latour on this matter; that is, that criticism, for instance in the form of deconstruction, shares with science one of its most problematic gestures, that it ”is founded upon the need to establish the meta-language of Critique in the place that would otherwise be occupied by the object or work under examination” (Koch 1995: 339). Critique and epistemology thus have as a common feature the goal of substituting for the specificity of the world-building activities they investigate their own generalities. As Koch puts it: ”What becomes more pronounced in Latour’s later work is the idea that science and critique are in fact ”doubles”, locked together in a fierce struggle for supremacy…”(Koch 1995: 338).
10. Interventions and Incommensurabilities

Can one be unhappy with aspects of the world and seriously want to change it, while denying the classical grounds for critique? This, of course, has been strenuously denied by the vast majority of philosophers and other scholars over the last two thousand years, and is currently signalled by the impotence and vague (or blatant) heresy ascribed to researchers that can be termed ”relativist”. As Barbara Herrnstein Smith documents, even fairly radical such scholars regularly feel the need to defend themselves against these accusations, usually by claiming to ”steer between” the equally dangerous dangers of orthodoxy (of some kind) and ”extreme” relativism, often characterised as ”Scylla and Charybdis”. 43

Even sophisticated and friendly-minded scholars such as Malcolm Ashmore find the Latourian strategy difficult to understand:

For infrareflexive [Latour’s term for his non-critical style of conducting science studies] writing, on the other hand, you ”just offer the lived world and write” (1988:70). The necessary reflexivity is achieved by applying ”principles of analysis which are self-exemplifying” (1988:171), by multiplying genres, by getting on the side of the known (1988: 173), by gaining explanatory equality with those we study, and by refusing to build a metalanguage (1988:174). It would really be very simple if it wasn’t quite impossible. As ”a story, just another story” (1988:171) Latour’s tale of infrareflexivity is a romance; and I’ve seldom read a better one (Ashmore 1989: 60)

Of course, many other criticisms have been considerably less benign. Critically minded scholars, such as Sande Cohen, have repeatedly, and repetitiously, pointed to the presumed complicity of such non-critical thinking with the powers that be, with its Machiavellian bent, and with its seeming lack of concern for properly emancipative projects, such as feminism or Marxism. 44

The move is double. On the one hand Latour is criticised for being too critical of established categories, in his insistence on the necessity of using a ”flattened ontology”; that is, an ontology where things and people, social and natural entities, institutions and microbes are treated symmetrically. When Latour’s agnosticism infuriates critically minded scholars it is therefore in part because he works to deprive them of the conceptual tools with which they achieve their critical effects. But if Latour is viewed as too critical in his denunciation of modern categories, this is seen as making him too uncritical in not drawing the usual foregone consequences. For his focus on the specificity of situations (”throw-away explanations”) and on the transformational capacities of all involved actors
prevents him from pronouncing at the end of his story on who were really the bad guys. Why and how is this a problem?

I would suggest that the Latourian stance remains impossible to understand precisely to the extent that one situates oneself as a critical (anti-)epistemologist, trying to adequately represent the situation under investigation. Under a representational idiom (Pickering 1995) it seems obvious that representation is accomplished in a selective fashion, and to the extent that Latour’s writing does not match the preferences of the reader, criticism is easily enabled; as, indeed, criticism is always easily enabled.

In a performative, or ontological, understanding, however, the situation is quite different, as the writer is, first and foremost, a participant, with all the problems this involves. For the critical stance, the first of these is that it becomes impossible to claim for oneself a moral high ground. Or, rather, it is possible to exactly the same extent as it is to everyone else in the field. To the same extent, however, is not good enough if one wants one’s critical perspective to get an edge over other positions.45

It is at this point that the idea of strategic essentialism is regularly invoked;46 that is, the idea that at certain points one should cling to ideas otherwise acknowledged as theoretically crude, or even fictive, because it is imagined that doing so offers better opportunities for practically engaging in ”the real world”, than sticking to one’s more subtle actual beliefs.47

It can be argued, however, that it is unclear whether staunchly re-affirming a certain set of values validated by traditional critical categories is a very effective way of engaging with this putatively real world.48 Jean-Francois Lyotard, for example, has suggested in The Differend that ”a universal rule of judgment between heterogeneous genres is lacking in general” (Lyotard 1988: xi). Since discursive genres sometimes function in resolutely divergent ways, and there are no position from which to formulate a meta-rule that would decide on their relative merits, the possibility of incommensurability remains the background for intellectual and, indeed, all other work. As Barbara Herrnstein Smith phrases it:

The resounding reaffirmation of an absolute distinction between truth and rhetoric, fact and fiction, science and superstition, will not in itself do the crucial substantive, technical and often arduous work of effectively differentiating among specific competing, conflicting claims of truth or between mutual charges of falsehood. Nor will a general affirmation of the inestimable value, irrefutable possibility, and transcendent ideality of
genuine objectivity identify where, in any particular instance, objectivity lies – or (the pun is apt enough) "lies.” (Smith 1997: 29-30)

Interestingly, this suggestion resonates with a formulation from (non-critical) ethnomethodologist Michael Lynch, who relates the critical urge to "long-standing sociological pre-occupation with exposing and unmasking hidden agendas, back-stage conspiracies and sub rosa economies…” (Lynch 2000: 40). Why, asks Lynch, do we not ask the constructive question of what makes ”practices rational and effective”?

In a sense, of course, this is what Latour, and other STS-scholars ask. At the same time the innocuous ethnomethodological positing of this question also replicates the idea (troubling in its positivist inspiration) that a researcher can become able to ”just report what he sees” in those practices with a suitably developed method and vocabulary (e.g. Garfinkel 2001). While pertinent, this response does not quite address (nor does it, of course, try to) the Latourian question of how we can stop being (classically) critical while still taking seriously our engagement with, and participation in, the field.

11. Participations

In view of everything I have said above it should be clear that I do not believe any general method for solving this problem exists. Nor, I think, would my affirmation of such a method solve the dispute; since, indeed, many other people have already proposed many other rules and guidelines without much luck. But, of course, this does not prevent me from stating my position on the matter and offering what I think is adequate reasons for maintaining it. Contra Ashmore, I think this is one way of achieving what Latour referred to as ”getting explanatory equality”, both with other researchers and with those I study.

In fact, I think the dictum of explanatory equality is liberating. It liberates the researcher from the belief that there is a specific set of ways by which one can properly engage in research, and a determinable set of proper outcomes of such engagements. Both of these assumptions are, I think, continually falsified in practice. The suspension of explanatory categories along with the urge to critique at least in principle enables different research strategies, and modes of interaction to be tested with our ”research subjects”, those about whom we talk as scholars. In particular it forces us to learn to
respond more keenly to what is going on in the field, because we know that our explanations and interventions have to be renewed in each instance. Our research agendas are therefore forced to become increasingly shaped in ongoing engagements with the field. I think a similar idea is implied by Annemarie Mol’s phrase empirical philosophy, which she describes as follows:

It is possible to refrain from understanding objects as the central points of focus of different people’s perspectives. It is possible to understand them instead as things manipulated in practices. If we do this – if instead of bracketing the practices in which objects are handled we foreground them – this has far-reaching effects. Reality multiplies…Attending to the multiplicity of reality opens up the possibility of studying this remarkable achievement (Mol 2002: 4-5)

Being able to follow this “remarkable achievement” does not give the researcher any critical edge in the classical sense of being able to tell what is wrong, and what consequently ought to be done about it. But, arguably, this becomes less important, because as soon as a research site is re-described in some of the ways suggested here there is an automatic unsettling effect. Under an STS re-description, practices and social worlds, their inhabitants and their relationships start to look different. One good example is the work of both Marilyn Strathern (1991) and Michel Callon and Bruno Latour (1981) to re-figure the classical micro-macro distinction in social theory. The former accomplishes such a shift by understanding social relationships as fractal, the latter by viewing the presumed macro-actors as micro-actors situated “on top of many (leaky) black-boxes” (Callon and Latour 1981:286), containing more or less stabilised associations between human and non-human actors.

Providing such alternative descriptions to those who are not in STS, can be seen, in itself, as an intervention possibly enabling these people to respond differently to their environment. This, I think, would be already quite an achievement.

The effect of non-critical STS-studies, like everything else, would then be in the hands of later users. These might be scholars or researchers, or they might be “informants”, the old term for the co-producers of the research object. Both of these groups are audiences whose responses to our texts, as they were conceived, produced, and, finally, presented, will have also shaped the way these texts ended up looking. Assuredly, they are audiences whose engagements with our texts are highly variable.
The continual and dynamic relationship between our own interests and those we encounter, as exemplified by the above-mentioned readers/co-producers of our texts, is very nicely captured, I think, in a term of Gilles Deleuze, *the powers of the false*. Deleuze says:

A new status of narration follows from this: narration ceases to be truthful, that is, to claim to be true, and becomes fundamentally falsifying. This is not at all a case of "each has its own truth", a variability of content. It is a power of the false, which replaces and supersedes the form of the true, because it poses the problem of incompossible presents, or the co-existence of not-necessarily true pasts (Deleuze 1989:131)

The falsity here mentioned is not opposed to truth, as in the analytical opposition; for as in other theories discussed at length above, to Deleuze this notion of truth (as universal, de-contextual etc.) is exceedingly dubious. His playful invocation of falsety therefore at most suggests that *if* the standard of evaluation was classical truth, then we *would all be falsifiers*. However, since a standard theory of truth is, in fact, *not invoked*, the powers of the false signals rather an opening up towards *collective explorations* of situations, which are both important and complex, since no-one can claim immediate access to their solution (that is, to situations of "explanatory equality").

I think the image of our work and texts as participating in the *powers of the false* is a good one, but not a simple or gratuitous one. It is not simple and gratuitous because *no one has promised us anything*. First, no one has promised us that anyone will indeed become interested in our ideas, plans, or articulations; that we will be able to make them resonate with those with whom we would like to interact. Second, no one has promised us a successful outcome of our endeavours, even if we manage to interest others and collectively produce a new response to the problems with which we were dealing. But perhaps the biggest cognitive challenge relates to the fact that even when disappointments occur, as they inevitably do, due to the non-cooperativeness of those with whom we would like to cooperate, or to the non-effectiveness of the interventions we have struggled for, the grounds for making classical criticism are *still gone*; having no means of scape-goating all there is to do is to start a constructive effort *over again*.

Surely, this way of thinking about the concretisations, both as failures and successes, of one’s hopes, ideas, and aspirations, is a galaxy away from many
contemporary intellectual engagements. It is nevertheless one to which the present study aspires.

Acknowledgment: Thank you to Barbara Herrnstein Smith for many illuminating discussions and careful reading.

1 It can be argued, for instance, that a unified view shows merely that the discipline is in a stable state of "normal science" (Kuhn 1970) or, more critically, that conceptual unification brings its own invisibilities and kinds of blindness, as in the case of molecular biology (Lewontin 1993) and sociobiology (Oyama 2000). The work of Michel Foucault on the inevitable intertwining of power and knowledge is a crucial reference in this regard (e.g. Foucault 1980). Following Deleuze and Guattari's discussions of royal and minor science (Deleuze and Guattari 1987), Isabelle Stengers has worked to differentiate the work of power in science into a majoritarian and minoritarian mode (Stengers 1994). This distinction is discussed in more detail in note 23.

2 But, perhaps, some references are due to the canonical literature: see e.g. Mannheim 1952, Merton 1968 for classical expositions in the sociology of knowledge, Popper 1963, 1968 and Lakatos, 1976 for classical texts in philosophy of science, the "counter-tradition" in the history and philosophy of science is most famously exemplified by Kuhn 1970 and Feyerabend 1993, see also, for instance, Toulmin 1958. Fleck (1979 [1935]) was an important pre-curser of these developments. The strong programme in the sociology of scientific knowledge (SSK) radicalised many of the insights from these challengers, not least in David Bloor’s famous principle of symmetry (Bloor 1976, see also Barnes 1977). Later came laboratory studies (Latour and Woolgar 1979, Knorr-Cetina 1981, Pickering 1984), the empirical programme of relativism (EPOR) (Collins 1985), social construction of technology (SCOT) (Bijker, Hughes and Pinch 1989), reflexivism (Ashmore 1989, Woolgar 1988), ethnomethodological studies of science (Lynch 1985), actor-network theory (Callon 1986, Latour 1987, Law 1994, for a variant see Bowker 1994), symbolic interactionist studies in science (Clarke 1998, Fujimura 1996, Star 1989), cultural anthropological studies of science (Haraway 1976, 1989, Traweek 1988), newer philosophical approaches to science (Hacking 1983, Ihde 1979, 1990, Rouse 1987) and feminist analyses of science (Fausto-Sterling 1992, Fox Keller 1985, Harding 1986). For instance. Evidently this categorisation is to some extent random, as many of the categories overlap and resonate, and increasingly so, as these various scholars became aware of and influenced by the works of each other, and newer studies proliferated (for which see the following note).


4 The sociological focus on the content of science was initiated, not least, by the Edinburgh sociologists David Bloor and Barry Barnes, and had two targets. Against classical sociology of knowledge, as represented by, for instance, Mannheim or Merton, SSK aimed to show that the content of scientific knowledge was, indeed, amenable to sociological analysis, and that the classical distinction between external factors (which sociologists could study) and internal factors (which they could not) did not hold. Against classical epistemology, these studied aimed to show that what was regularly characterised as
internal to science was, indeed, influenced by the putatively external in many different ways, and the classical distinction between the *context of discovery* (which might be messy) and the *context of justification* (in which the mess was removed and the logical core of discovery was elucidated) could not be upheld.

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6 As case-studies has indicated, however, the situation is often in reverse; that is, technological possibility and change often drive scientific inquiry (e.g. Hughes 1983, Bijker, Hughes and Pinch (1989). For a view of science as an “indigenous knowledge system”, see Verran 1998, 2001, forthcoming.

7 This is premised on an idea of science as singular in its means and ends. For a different view see e.g. Galison and Stump 1996.

8 Although the radicality of this challenge is sometimes misunderstood, or underestimated, by commentators. In a recent example philosopher of science Helen Longino works strenuously to reconcile classical philosophy of science with STS-studies, claiming that both sides unwittingly believes in what she calls “the rational-social dichotomy” (Longino 2002, for a similar endeavour see Hess 1997). After ”disassembling” this dichotomy, Longino suggests that philosophers should indeed be able to appreciate the practice focus of STS-studies, while STS-scholars should acknowledge the existence of cognitive features of the kinds philosophers like to evaluate. Arguably, however, Longino’s disassembling and subsequent reconciliation of positions is premised on a number of ideas, which are themselves at stake in the debates she proposes to mediate. Her solution is thus not likely to convert many STS-scholars, since it can be viewed as re-asserting, rather than disassembling, the idea that it is relevant or, indeed, possible, to distinguish between social and cognitive “factors”. This, of course, is precisely the idea which STS-scholars have worked hard to ”disassemble” (and reassemble). In Andrew Pickering’s formulation: ”A more general formulation of this point is to note that studies of practice tend to cut across all traditional disciplinary reductions, not just sociological ones…This observation argues not only against accounts centered in a single discipline but also against disciplinary eclecticism that conjoins, say, a philosophical view of rationality with a sociological account of interests (though since the mid-1980s much scholarly work at the intersection of history, philosophy, and sociology of science has taken just this form)” (Pickering 1992 14-15 n.7). Evidently it still does. Longino’s text thus figures as a lucid example of what Barbara Herrnstein Smith has termed “equivocation”, that is, the move to reconcile what may be viewed as irreconcilable or incommensurable positions (Smith 2002). For a more general discussion of the dynamics of intellectual controversy (discussing philosophy of science and science studies in chapter 8), see Smith 1997.

9 Thus formulating a methodical response to the situation, characterised as lamentable by B. F. Skinner in 1953, that ”we have not gone all the way. We regard the common man as the product of his environment; yet we reserve the right to give personal credit to great men for their achievements…We want to believe that right-minded men are moved by valid principles even though we are willing to regard wrong-minded men as victims of erroneous propaganda. Backwards peoples may be the fault of a poor culture, but we want to regard the elite as something more than the product of a good culture…We dismiss those who disagree with us as victims of ignorance, but we regard the promotion of our own religious beliefs as something more than the arrangement of a particular environment” (Skinner 1953: 8-9). Skinner thought this symptomatic of a transitional cultural period, but if this is the case the transition is still underway.

10 The “chicken debate”, is named after the article ”Epistemological Chicken” by Harry Collins and Steven Yearley (Collins and Yearley 1992), in which they attack the positions of reflexivist Steve Woolgar and actor-network theorists Michel Callon and Bruno Latour. In the series of papers, both of these targets respond (Callon and Latour 1992, Woolgar 1992), and Collins and Yearley sum up the discussion in ”Journey Into Space” (1992a). The exchange is found in Pickering 1992.

11 See, for example Bourdieu 1990, Collins and Pinch 1998. On the former, see, for instance, commentary in Bowker and Latour 1987, also Pels 2002: 77-80.


13 This change is affiliated, if not identical with, what Pickering (1995) calls the move from a *representational idiom* to a *performative idiom*.
In a related move Isabelle Stengers has discussed issues of power in science. She contrasts minority and majority science: "I mean by minority not a part of the population which is not, but could and tries to become, the majority, but active minorities who do not dream of obtaining for themselves the power of a majority. Like Felix Guattari I dream about multiple connections among minorities, so that each of them would become able to work out its own singularity through the creation of alliances, not in isolation, and so that each individual would be simultaneously part of many minorities. Science, as I love it, is also a minority movement in a way" (Stengers 1994:41). Whereas minority science is seen by her as "in league with power: the invention of the power to confer on things the power of conferring on the experimenter the power of speaking in their name" (Stengers 1997:165, emphasis in original), majority science merely

For specific discussion of Lucretius and Heraclitus see e.g. Serres 1982, 2000; Leibniz see Deleuze 1993; Spinoza see Deleuze 1990, 1992, Montag and Stolze 1997, Nietzsche see Deleuze 1983, Foucault 1984, 1984a.

According to Deleuze (1983, 1990, 1992) this is one of the key insights of Spinoza (see also Spinoza 1959).

This "unforeseeableness" make events literally "just happen", when understood in a real-time, rather than retrospective, perspective (Pickering 1995). On experimental events see also Stengers 2000.

According to Alain Pottage this view can be contrasted with that of Foucault and Deleuze, a point which could be of consequence since I 'enrol' these scholars into the same theoretical narrative: "So, for example, in contrast with Bruno Latour’s model of networks of intéressement, in which each actor in a network seeks to 'perform' its representations...or, quite literally, to 'concretise' their interpretations...there are no 'quasi-material' facts which precipitate from the interaction of actors’ observations. Rather, events remain the attributions of each particular 'actor' (Pottage 1998: 24). Pottage reaches this conclusion, by reading Foucault through the system theory of Niklas Luhmann, and supports it by citing work of Habermas. The result of this eclecticism, which attempts to link together high modern theories of radically different observations (Habermas and Luhmann) with French post-structuralism, seems to me implausible and conceptually highly unstable. The projects of Latour and Foucault, on the other hand, are manifestly connected along a number of lines, as I will proceed to indicate in the present text.


A very similar move is made by Latour in "The Powers of Association", where he distinguishes between power understood as transmission of a substance (the diffusion model) and power understood as an effect of the successful work to stabilise associations among a set of actors (the translation model) (Latour 1986).

I take the useful term coalition from political theorist Maarten Hajer, who lays out his version of discourse analysis and discourse coalitions in chapter two of his The Politics of Environmental Discourse (Hajer 1995). At the same time I find Hajer’s appropriation and re-definition of Foucault rather problematic. In Hajer’s work Foucault is "corrected", first, for not sufficiently "observ[ing] how seemingly technical positions conceal normative commitments...[and] find[ing] out which categories exactly fulfilled this role, and which institutional arrangements allowed them to fulfill that role", and, secondly, for his perspective on social change and permanence. As regards the latter, Foucault should realise that "Discourse analysis, then, is not only essential for the analysis of subject positions but also for "structure positionings" (referring to which structural elements can be changed, and what institutions remain to be seen as fixed and permanent)" (Hajer 1995: 55-56). To fill this "gap" Hajer draws on social psychologists, Mick Billig and Rom Harré; in other instances Giddens dualistic structuration model, and the work of Bourdieu is used by Hajer as corrective to, or complementary of, Foucault (e.g. Hajer 1999). Both the identification of Foucaultian flaws and their remedies seem to me dubious. In the first instance it seems clear that most of Foucault’s work is obsessively interested precisely in the many intricate relationships between normativity and institutions. Second, just this focus on the institutionalisation of norms for conduct gives Foucault a fairly acute grasp of the relationship between permanence and change; arguably one which is more sophisticated than any of the theorists invoked by Hajer to remedy his putative deficiencies. For a relevant criticism of structuralism in general and structuration theory specifically see (Sewell 1992).

For example, Foucault temporarily participated in organizations such as Mouvement pour la Liberté de l’Avortement (Movement for Freedom of Abortion) (Foucault 2001: 423) and the Groupe d’Information sur les Prisons, which worked for reforms of the penal system (Foucault 2001:418-22).

In a related move Isabelle Stengers has discussed issues of power in science. She contrasts minority and majority science: "I mean by minority not a part of the population which is not, but could and tries to become, the majority, but active minorities who do not dream of obtaining for themselves the power of a majority. Like Felix Guattari I dream about multiple connections among minorities, so that each of them would become able to work out its own singularity through the creation of alliances, not in isolation, and so that each individual would be simultaneously part of many minorities. Science, as I love it, is also a minority movement in a way" (Stengers 1994: 41). Whereas minority science is seen by her as "in league with power: the invention of the power to confer on things the power of conferring on the experimenter the power of speaking in their name' (Stengers 1997: 165, emphasis in original), majority science merely
This point is made straightforwardly in Latour 1987, who likens the attribution of rationality and efficacy to successful scientists to "compliments and curses". See also Latour 1997. This argument, while it continues to have provocative value, is not new. In his historical study of what is now referred to as the "moratorium" on psychological explanations suggested in Latour (1987), Gooding formulates a similar idea as a criticism of Latour, stating that "The illusoriness of definite reconstruction (even by scientists) of their discoveries in new domains – particularly differences between scientists’ own accounts and routes based on laboratory notes – has led some to ‘black box’ the mental aspect of discovery. It is fashionable to deny a role for mental procedures in sociological accounts of experiment. Latour’s ‘black boxing’ is implausible given the interactive character of thought and action" (Gooding 1990: 132). Latour’s "moratorium" was not, I think, suggested with the purpose of rendering illegitimate conceptualisation of the mental aspects of science as such, but to pose a challenge to naïve psychologistic explanatory models. It also remains unclear what it might mean to term a methodical procedure (black boxing of mental aspects) "implausible". However, since the lesson has surely been learned that traditional intentionalist, or otherwise psychologistic, accounts are problematic, his point is well taken, and it may be time to reconsider how one can understand "amodern cognition". Steven Brown and Paul Stenner (Brown and Stenner forthcoming), David Gooding (1990), Ed Hutchins (Hutchins 1995), Barbara Herrnstein Smith (Smith 1997, Chapter 4, "The Language Loop"), and Helen Verran (Verran 2001) offer recent suggestions. Phenomenologically oriented accounts include Lave 1988, Lave and Wenger 1991.


The north-west passage refers to the much sought for polar passage, which would connect the Atlantic Ocean with the Pacific. Michel Serres uses the term as an analogy for the construction of connections between seemingly non-related cultural and scientific ideas and events, see Serres 1995: 104, 152.

This point is made straightforwardly in Latour 1987, who likens the attribution of rationality and efficacy to successful scientists to "compliments and curses". See also Latour 1997. This argument, while it continues to have provocative value, is not new. In his historical study of what is now referred to as the pragmatist philosophers, Louis Menand suggests the following as a key tenet of the group of thinkers: "In the end, you will do what you believe is "right", but "rightness" will be, in effect, the compliment you give to the outcome of your deliberations. Though it is always in view while you are
thinking, "what is right" is something that appears in its complete form at the end, not the beginning, of your deliberation" (Menand 2001: 352). See e.g. Dewey (1920, 1958), James (1996).


33 At this point it is worth remembering that the powers that be look rather different from the perspectives here discussed than from traditional perspectives in philosophy and social theory. Unsurprisingly, these differences become consequential for how one might want to engage such powers.

34 Cohen wants to defend deconstruction against Latour, particularly drawing on de Man. While the relationship between deconstruction and the work of Latour is interesting, I want to stress here primarily how she operates a set of critical gestures in her defence (and attack). First, because this aligns her with numerous critics of Latour (see e.g. note 32), and thus generalises the relevance of my invocation of her: it is not a matter of a local turf-war between deconstruction and Latour, but a much broader question of the relationship between critical and non-critical positions in philosophical, cultural, and social studies of science and technology. Second, because while deconstruction takes many shapes, and is often characterised by a taking serious of the text under study on its own premises, such serious interest seems to have invigorated Cohen’s analysis considerably less than the possibility of revealing the conservative consequences of We Have Never Been Modern.

35 In conclusion, for example, Cohen states that: "In Latour’s treatment of science studies, all the "theoretical enigmas" pertaining to language are directly suppressed; science studies is plunged into the discursivity of enthusiasm such that it aggressively precludes any "residue of indetermination" (de Man) which might intrinsically belong to knowledge production or cultural analysis…De Man’s notion that epistemic suppression requires the violence of language against epistemology, delivered by aesthetic formations of grammar and logic, seems confirmed” (Cohen 1999: 94).

36 It should be noted that these Latourian denunciations are, indeed, quite unsubtle.


38 As is, in a sense, acknowledged by Cohen’s use of the phrase “the discursivity of enthusiasm” as cited in note 35. Cohen, however, does not consider what might be the deliberate and constructive reasons for this “discursivity”, but imagines it to be primarily offensive, and possibly an unconscious reflection of the violence of Latourian “order-words”. Below, I discuss some of these reasons. Unsurprisingly, it turns out that one of the primary offences of the Latourian text has to do with its lack of critical potential: “…the language of enthusiasm turns study into a kind of revivalist salvation, a persistent downgrading of violence in the name of those "tiny networks" with their "miniscule" differences. This involves the very repression WHNB [We Have Never Been Modern] claims to reject. One wonders what the forced diaspora of dozens of millions round the world since WWII could mean to this grammar of a flattened world” (Cohen 1999: 92). As criticisms of Latour and/or actor-network theory go, this one is certainly well-worn (several of the studies cited in note 32 makes it), and it will be the focus of my attention below.

39 Some of these differences are discussed in Baugh 1997, see also discussions in Patton and Protevi 2003.

40 He could have referred, for example, to Derrida 1976.

41 This is well put by Barbara Herrnstein Smith, who first suggests that according to Latour’s own model he is presently unable to argue for its superiority (as this decision is in the hands of later users). As she then goes on to say, however: “On the other hand, the rhetorical energy and power that his account secures at the expense of historical modesty may turn out, in the long run, to be what makes it more effective (and credible) than the more scrupulously modest accounts of his rivals, sociological as well as philosophical” (Smith 1997: 137-8). That, of course, is exactly the point, and the self-exemplification, of the Latourian writing strategy.

42 But at this point the argument is generalisable to several other discourses interested in providing critical leverage through intellectual analysis.

43 For discussion of this point see Smith 2002.

44 I refer again to note 32.

45 This is precisely the situation and problem analysed in Latour 1988.
46 The term is from Gayatri Chakravorty Spivak, who defines it in the following way: ”A strategic use of positivist essentialism in a scrupulously visible political interest” (Landry and Maclean 1996: 214). An important recent example is found in Michael Hardt and Toni Negri’s Empire. Here, the authors withdraw from their otherwise defended post-structuralist inspiration and turn realists when normative pronouncements are called for. They suggest, for instance, that: ”In the context of state terror and mystification, clinging to the primacy of the concept of truth can be a powerful and necessary form of resistance. Establishing and making public the truth of the recent past – attributing responsibility to state officials for specific acts and in some cases exacting retributions – appears here as the ineluctable precondition for any democratic future. The master narratives of the enlightenment do not seem particularly repressive here, and the concept of truth is not fluid or unstable – on the contrary! The truth is that this general ordered the torture and assassination of that union leader and this colonel led the massacre of that village. Making public such truths is an exemplary Enlightenment project of modernist politics, and the critique of it in these contexts could serve only to aid the mystificatory and repressive powers of the regime under attack (Hardt and Negri 2000:155-6). Easy points are scored on this account, as it is impossible to argue with the idea that those responsible for torture and murder should be identified. The willful naivety of the statement becomes more visible, when is considered the complexities and uncertainties that pertain to evincing and displaying evidence, even for what would seem to Hardt and Negri quite obvious cases of wrong-doing (e.g. Lynch and Bogen 1996, Lyotard 1988). That is just the point, of course. For what seems to these scholars and critics an obvious wrong-doing on which a strategic essentialism or realism could be brought to bear, may be perceived quite differently by other agents. This is why the invocation of reality as a final arbiter of conflict does not work well. See also Smith 1997, Chapter two.

47 Although the term is never discussed by Stengers, it is easily seen as ”majority concept” in its attempt to close debate by reference to the essentials of a situation, strategic or not. As Stengers writes: ‘Furthermore, I do not know which kind of power would define what it is to be anti-racist, anti-classist, anti-sexist, and anti-culturally-coercive. All I know is that if it is in a position to select and direct it will be a majority power, and thus it will demand submission and refuse putting at risk the majority formulation of their values’ (Stengers 1994: 45)

48 This is argued very effectively in Smith 1997.

49 The term is from Barbara Herrnstein Smith, personal communication.

50 Isabelle Stengers suggests that: ”In most cases, scientists and epistemologists have been in a great hurry to explain this history, to show that the access was deserved and legitimate, the consequence of an ultimate rational method or interrogation. They have made the method, which ensued from the event, responsible for it, and have, as a result, obscured what is essential: no one has promised us anything, and in particular, no one has promised us that, in all the fields of knowledge, the same type of event will be reproduced (Stengers 1997: 88).
Chapter two:
Qualitative Research as Partial Connection:
Bypassing the Power-Knowledge Nexus

1. Introduction
Qualitative research and its various methods is often imagined as relating to a problematic of truth and power and variably evaluated according to this distinction. Thus, some advocates of ‘postmodern’ qualitative research view objectivity as a cover up for the exercise of power over weak practices rather than a desirable goal (Denzin 2001, Lincoln and Denzin 1998, Richardson 1991). In their view no discourse should be granted special privilege with regard to its truth, and the researcher, who is imagined as incomparably stronger than the practices he investigates, should be especially cautious not to enforce his perspective on the events he deals with. He should rather invest scholarly resources in helping silenced voices to be heard.

The dichotomy between power (as ability to repress) and knowledge, thus instantiated in many discussions about qualitative research seems theoretically and empirically dubious. With a term from Gilles Deleuze, I view these discussions, insofar as they are organized around a nexus that binds together truth and power in a pre-determined conceptual grid, as instances of a “badly posed problem” (Deleuze 1991). This characterisation points towards an exploration of some of the theoretical and practical consequences that might be entailed by the suspension of such understandings.

The theoretical problem lies in the limited variety of intellectual positions, which are rendered available by conceptualisations organized around the question of the relationship between power and knowledge. But this points towards a potential practical problem as well, since the imagination as to how one can productively engage with the field, inspired as it often is by the researcher’s understanding of his position in the power-knowledge nexus, may be unnecessarily delimited. In light of these problems I argue here that there is a need for a renewed consideration of the capacities of qualitative research and aim to initiate such discussion by drawing on insights from contemporary feminist theory and science and technology studies (STS).

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1 Co-written with Peter Lauritsen. I refer to ”I” throughout this edited version of the text to retain stylistic consistency.
In section two, I survey a number of arguments about power, truth, and reflexivity. As noted, I will argue that ‘postmodern’ discussions of qualitative research are organized symmetrically around a dualistic understanding of truth and power. Section three turns to the fields of feminist theory and STS for ideas of how to re-formulate this problem, and I discuss Donna Haraway’s notion of situated knowledges and some of its implications for analyses of science. In section four I move further into STS-territory. Here Bruno Latour’s understanding of both human and non-human actors as constituted in chains of association is presented and related to Haraway’s analysis of our contemporary cyborg condition. Both of these sets of thoughts suggest alternative perspectives on qualitative research.

In section five social anthropologist Marilyn Strathern’s notion of partial connection is adopted to characterise the position of the researcher in the world of technoscience. This can be seen as an effort to redefine qualitative research as it has been understood by traditionalists and postmodernists alike and to potentially re-link projects in qualitative research with broader social, political and institutional concerns. In section six I illustrate the strategies and strengths of such perspectives by drawing out several analytic points from Joan Fujimura and Danny Chou’s study of the controversy over AIDS-etiology (1994). Finally, in section seven, I discuss the implication of these considerations for the politics of qualitative research. Specifically I try to show how the analysis points to ways of imagining qualitative research, which are not shaped by dichotomous and restrictive understandings of truth and power.

2. Knowledge, Power and Reflexivity in Qualitative Research

‘Objectivity’ arouses the passions as few other words can. Its presence is evidently required for basic justice, honest government, and true knowledge. But an excess of it crushes individual subjects, demeans minority cultures, devalues artistic creativity, and discredits genuine democratic participation (Porter 1995: 3)

Qualitative research is often defined in contrast to ‘positivist’ or quantitative research (Creswell 1994). An example is the account given by Denzin and Lincoln (1998), who recognise that qualitative research took off in positivism at the beginning of the twentieth century. But in their writing of the history of the field, qualitative research has continuously distanced itself from its positivistic past through an accelerating
radicalisation. Thus, the “traditional” phase lasts from 1900-50, the “modern” one from 1950-70, while “blurred genres”, “the crisis of representation”, and “the postmodern challenge” become progressively shorter (Atkinson and Coffey 1999).

According to Denzin and Lincoln a “profound rupture occurred in the mid-1980s” (Denzin and Lincoln 1998:19) when qualitative research started to inquire reflexively and sceptically about its capacity to adequately represent itself and its objects. But one does not have to be a follower of Denzin and Lincoln to agree that the qualitative researcher, in recent decades, has increasingly been viewed as a non-neutral mediator between a practice and its description (e.g. Becker 1967). This has presented a problem for the researcher’s account, but has not necessarily been viewed as invalidating the goal of achieving objectivity (Gouldner 1968).

In Denzin and Lincoln’s reading of qualitative research knowledge claims are necessarily bound up with authority and power:

Epistemological validity can now be interpreted as a text’s desire to assert its own power over the reader ... A fertile obsession, validity is the researcher’s mask of authority … which allows a particular regime of truth within a particular text (and community of scholars) to work its way on the world and the reader (Lincoln and Denzin 1998: 415)

In this formulation it is proposed that qualitative research has lost the privilege of producing valid 'scientific' statements, for making a scientific claim is viewed as an exercise of 'power by other means'.

Separating the research activity from the issue of validity entails that the researcher cannot rely on pre-defined methods and procedures; that he can bring nothing with him that will ensure the scientific status of his work. Denzin and Lincoln capture this condition in their description of the researcher-as-bricoleur, who must deploy "whatever strategies, methods, or empirical materials are at hand" and "if new tools have to be invented, or pieced together, then the researcher will do this" (Denzin and Lincoln 1998: 3f.).

This situation, in which the researcher looks in vain for a general method to ensure the scientific status of his work, is not viewed by Denzin and Lincoln as regrettable. Rather it is a liberation of research. Without being confined by traditional standards of scientific activities the researcher may produce better, if not more objective, accounts of people's lives; for example through the use of poems and theater (Richardson 1994,
However, they see such alternative representational practices as more than mere aesthetic experiments. For with the freedom of the bricoleur follows a sensibility towards the power play in which researcher and informant are both caught. Thus, an important aim becomes the deconstruction of the ideologies of repressive powers, for instance corporate or scientific, and the empowering of underprivileged peoples.

Qualitative researchers such as Padgett (1998) and Silverman (2000) attempt to ensure validity through more or less traditional methodical guidelines. Others try to save it by means of reflexivity, for instance by telling confessional tales from the field (Van Maanen 1988). In these tales the ethnographer recognises that he is tied to the field under study and that his actions therefore have effects. On the face of it, this entanglement of the knower and the known is a threat to validity, but if the researcher acknowledges and confesses her actions and effects, objectivity can be saved.

In view of the above arguments it should come as no surprise that writers such as Denzin and Lincoln do not think reflexivity can or should save validity and objectivity. From their perspective, reflexivity is of broader implication than admitting the researcher’s effects on practice. Instead it should be acknowledged that: “the results of social research are constructed from … the broader sociohistorical context of researchers and the disciplinary culture to which they belong” (Davies 1999: 9).

As they acknowledge the inability of the qualitative researcher to speak from a privileged position, Denzin and Lincoln begin to imagine themselves as part of power struggles rather than as truth-witnesses. According to Denzin this is increasingly true in and after what he calls the ‘post-experimental period’ where "no discourse has a privileged place, no method or theory has a universal and general claim to authoritative knowledge" (Denzin 2001: 25, see also Richardson 1991). For this reason traditional views of objectivity and validity must be rejected and what is left is politics: “If validity is gone, values and politics, not objective epistemology, govern science” (Lincoln and Denzin 1998:415). Concurrently, anybody claiming to possess ‘scientific’ knowledge is suspected of using reference to truth to cover up the power of the research activity. That is why, it is argued, postmodernism is often resisted:

It is entirely obvious that persons whose power positions have been concealed behind notions of general, Archimedian truth would resist postmodernism. ... Postmodernism appeals to and benefits marginalized people who have been locally and historically denied access to power...(Richardson 1991:173f.)
This discussion has explicated how a ‘postmodern’ perspective on qualitative research is based on a dichotomous understanding of knowledge and power, which has roots at least decades back. At the knowledge-pole one finds ‘traditional’ qualitative research believing that neutral, objectivity is the goal which the researcher should aim for. Denzin and Lincoln see themselves as part of a movement which has turned this understanding upside down; where claims to objectivity merely signals ideological complicity with existing power structures. In similarity with the sociology of deviance, they imagine their studies (or even ‘postmodernism’ as such) as manifestly beneficial to the ‘victims’ they study. Thus, Denzin and Lincoln’s approach illustrates a “metaphysics of the underdog” (Gouldner 1968), with the complementary problem of underdoggedness, which Barbara Herrnstein Smith has identified as a tendency to “talk for the underdogs with a determination that is not matched by intellectual finesse”.

The structure of this debate points to it as a classical example of what, following Gilles Deleuze (1991), may be called a “bad problem”, which clearly calls for a reconsideration of its terms.5

3. Objectivism, Relativism and Relationism

Not content with the dichotomous framework in which discussions about truth and power were carried out, some scholars within feminist theory and STS-studies begun analysing objectivity as, in the first instance, a relational matter (Callon 1989, Haraway 1989, Latour 1988a, Law 1986, Strathern 1991).

In an especially well-known formulation, historian of science Donna Haraway’s equals good research with situated knowledges (Haraway 1991:183-203). Haraway points out that, in spite of the immense differences between humans, we are alike in all being technologically and metaphorically implicated in the world in multiple ways. Such inevitable, worldly immersion makes scientific aspirations to a neutral God’s eye view of things presumptuous. But it implies neither that all perspectives are equal nor that the strongest actors win per necessity. Rather, scientificity can be thought of in terms of partial connection:6 ”Here is the promise of objectivity: a scientific knower seeks the subject position not of identity but of objectivity; that is, partial connection” (Haraway 1991:193).
Haraway formulates an idea of how one should go about constructing understandings of the world as a shared and participatory project. One should realise that research is a “thing of this world” (Foucault 1980), and is instrumental in changing it or reifying it. This makes research a partial enterprise, for if knowledge is always situated in the relationship between knower and known, then science is about the hard work of creating sensible and sustainable relationships with different entities. And it would be in the encounter between a researcher with one set of ideas and interests, and subjects with different habits and practices, that surprising research questions could be posed and relevant answers formulated.

There is some resemblance between situated knowledges, Jean Lave’s communities of practice and Anselm Strauss’ social worlds perspective (Lave and Wenger 1991, Strauss 1978). But a difference emerges in Haraway’s insistence that situations are not solely, nor even primarily, defined by humans – but always by assemblages of entities. This insistence makes Haraway emphasise the cyborgian quality of contemporary actors. The idea of ‘non-human’ agency is likewise visible in the work of Leigh Star and James Griesemer (1989) who have developed the important concept of the boundary object to account for how successful co-operation may take place (their example is in a museum) even though people rarely have the same goals, hopes and understandings of what they are trying to achieve. In their word, boundary objects are objects, which “are both plastic enough to adopt to local needs and the constraints of the several parties employing them, yet robust to maintain a common identity across sites” (Star and Griesemer 1989: 393). Boundary objects may thus be seen as the glue, which holds together an inter-objective collective, for consisting only of human relations it would quickly dissolve (Latour 1996a).

Evidently, situated knowledges and boundary objects have little to do with positivist-inspired ideas of neutral observation and representation. Thinking of research as a process of elucidation through partial connection is rather a way of thinking about scientificity as a matter of ontological relationism.

Relationism, because connection, rather than separation and distance, is a necessity if anything is to be learned. Ontological because connections can allow for the articulation of new properties of a situation or new modes of action. Research, in this
sense, is therefore not about representing the given, so much as about participating in the construction of the new (Pickering 1995, 2003).

In fact this ontological relationism is the reason for Donna Haraway’s hostility towards a bland pluralism of the kind advocated by Denzin, for it is a view which makes one wary of claims to be able to do too many things or occupy too many positions at once. As Haraway suggests:

Such preferred positioning is as hostile to various forms of relativism as to the most explicitly totalizing versions of claims to scientific authority. But the alternative to relativism is not totalization and single vision, which is always finally the unmarked category whose power depends on systematic narrowing and obscuring. The alternative to relativism is partial, locatable, critical knowledges sustaining the possibility of webs of connection called solidarity in politics and shared conversation in epistemology. Relativism is a way of being nowhere while claiming to be everywhere equally. The ‘equality’ of positioning is a denial of responsibility and critical enquiry (Haraway 1991: 191).8

Evidently this does not mean that researchers should defend specific intolerant dogma, or develop frozen knowledge hierarchies, but that they should concentrate on learning the difficult task of articulating specific processes.9 It also means that criteria of propriety can be developed for scientific practices, including qualitative research (see e.g. Chertok and Stengers 1992, Stengers 1997, Verran 1998, 2002). But not in general and never from the outside. Rather the researcher’s work to become situated in relation to a given practice places him in the risky position of a partial insider – rather than the risk free one of external commentator.

Donna Haraway and Bruno Latour, among others, have developed sophisticated alternative accounts of how scientists and others engage with diverse practices, which do not build on such (overt or hidden) assumptions of epistemological self-privileging, but resolutely define their own endeavours as part of the field. In the next section I discuss Haraway’s development of the term cyborg and Latour’s work on as part human and non-human actors as related and defined in chains of associations.

4. Cyborgs and Chains of Association

Anthropologists and others who study material culture have long been aware that a good, although not comprehensive, way to characterise ‘man’ is as homo faber. Traditionally this has been demonstrated by showing how persons use tools, how they invest tools with
meaning, and how these therefore become increasingly important on practical as well as symbolic levels of human organization (e.g. Appadurai 1986). But STS (Science, Technology, and Society)-studies has worked to radically re-organize this understanding. Thus, Bruno Latour claims that most of the ongoing action between humans and non-humans is missed if they are viewed through a pre-determined grid, distributing what each can do (e.g. Latour 2000).

Instead of looking for humans using tools and ascribing meaning to their activities, Latour encourages us to follow ‘chains of associations’ of humans and non-humans. With the slogan ‘follow the actors’, Latour (1987) has suggested that we get a more sensible understanding of how effects of truth and power become established, by suspending traditional analytical categories and empirically investigating what humans and non-humans do to each other. This is a non-essentialising approach since one can only understand the competence of humans or non-humans in light of their positioning in an entire chain of relationships. Inspired by process philosophy, Latour makes the following suggestion:

Consider things, and you will have humans. Consider humans, and you are by that very act interested in things. Bring your attention to bear on hard things, and see them become gentle, soft, or human. Turn your attention to humans, and see them become electric circuits, automatic gears, or softwares. We cannot even define precisely what makes some human and others technical, whereas we are able to document precisely their modifications and replacements, their rearrangements and their alliances, their delegations and representations. Do technology, and you are now a sociologist. Do sociology, and you are now obliged to be a technologist (Latour 1991, available from <http://www.ensmp.fr/~latour/poparticles/1991.html>)

This pertinent description is closely related to science historian Donna Haraway’s notion of the contemporary ‘cyborg condition’ (Haraway 1991). In the famous ”A Cyborg Manifesto: Science, Technology, and Socialist-Feminism in the Late Twentieth Century” she describes the dissolution of three sets of limits that forces the critical researcher to rethink politics as well as science, and technology:

1. The limit between man and animal:
   language, tool use, social behaviour, mental events, nothing really convincingly settles the separation of human and animal. (Haraway 1991: 151-52)

2. The limit between natural and artificial:

"Qualitative Research as Partial Connection" 35
Late twentieth century machines have made thoroughly ambiguous the difference between natural and artificial, mind and body, self-developing and externally designed, and many other distinctions that used to apply to organisms and machines. Our machines are disturbingly lively, and we ourselves frighteningly inert (Haraway 1991:152)

3. And, finally, the limit between physical and non-physical, material and mental. This can be seen in the ways in which ideologies and metaphors are into technologies with increasing efficiency, becoming invisible and naturalised in the process:

   Writing, power, and technology are old partners in Western stories of the origin of civilization, but minituarization has changed our experience of mechanism. Minituriazation has turned out to be about power, small is not so much beautiful as pre-eminently dangerous, as in cruise missiles (Haraway 1991:153)

These interventions of both Haraway and Latour into humanistic frameworks of interpretation are matters not only of theory but also of practical reality. But, then again, for them this reality is never immediate or unmediated. Rather, as we have seen, it is the entanglement of technologies of representation, technologies of production and reproduction, and a never-ending array of other humans and non-humans. For Latour and Haraway we are cyborg, because we are always simultaneously part of chains of associations and have part in changing them (and thereby ourselves). As humans we are only ever a part of the world and therefore not necessarily the point around which every analysis must revolve. But this partiality can be viewed as liberating because it offers new possibilities for rethinking relations between humans, society, science, and technology, in ways which have until now been rendered invisible or impossible by the essentialising categories used to understand the interrelationships of these “entities”.

   As we are all cyborg, our access to the world is not only mediated by, but consisting of our many relations to many humans and non-humans in chains of associations. How then, could one imagine a scientific method neutrally rising to become capable of proclaiming the unitary truth about a phenomenon? Haraway characterises this idea as a ‘view from nowhere’. But the consequence of this position is not that all perspectives are even, or that science is purely a matter of power. In the following sections I discuss some of the consequences a ‘cyborg view’ might entail for qualitative research.
5. Re-Defining and Re-Linking: On Partial Connections

I have proposed to view the dualistic tension between truth and power in debates on qualitative methods, as an instance of a badly posed problem. Why badly posed? Because the ability to speak the truth or to act powerfully is always dependent on many linkages, far from all of which are human. The realisation that we are all multiply mediated in chains of association undermines the hope of attaining a ‘view from nowhere’, but at the same time it enables a re-consideration of what kind of position one may then aspire to as researcher. From this point of view questions concerning truth and power could be fruitfully re-viewed by focusing on the many institutional and disciplinary techno-mediations that partakes in the construction of powerful actors (e.g. Callon and Latour 1981, Shapin and Schaffer 1985).

Focusing on the chains of association in and through which the researcher conducts her investigation, inevitable makes visible the partiality, but not necessarily the subjectivity, or the repressive ideological capacities, of the research process. For whether research turns out to be about power or truth, subjective perspectives or shared conversations, depends on the ways in which researcher and researched are able to engage each other.

The notion of Partial Connections is taken from Marilyn Strathern’s book of the same name (Strathern 1991). She brings home the point that anthropologists relate to their fields and their investigations in multiple ways that are not solely dependent on the normative or scientific intentions of the researcher. For this reason a methodical stance built on reflexive considerations cannot sanction specific sorts of engagement with the field, anymore than one based on the hope of reaching validity and objectivity.

Undoubtedly postmodern qualitative research, as exemplified by Denzin, Lincoln and Richardson is dissonant with ‘traditional’ approaches on the level of discourse and rhetoric. But on the level of actual engagement with practice, these divergences do not necessarily imply a higher versus lower degree of reflexivity or ability to adequately represent events. Rather, the different methodical stances bring with them different politics for connecting or disconnecting with the field. In his attempt to save objectivity, for example, Alvin Gouldner’s (1968) argument (cited at length in note two) hinges on his own presumed access to the “larger picture” of social and institutional development in the U.S. Gouldner’s perception that a clear view of the situation was available to him...
gave him the moral right and obligation to denounce Becker’s (1967) studies as “ombudsman sociology”, complicit with the upper administrative levels in Washington.

On the other hand Denzin is nervous about big pictures and he increasingly associates his field-work with poetry and other activities imagined as transgressively benign, while denouncing those who have not reached this level of radical insight. What both do from the present perspective, however, is to partially construct relationships with what they study: no more and no less. This partiality is not in itself a problem; indeed, our argument is that it is a condition of research. More problematic is the temporary forgetfulness of this partiality, which in both cases here mentioned made researchers feel legitimated in adding to their interpretations a moralising overhead.

As we have seen, thinking in partial connections pre-empt the possibility of attaining a view from nowhere. Given the argument developed above it also makes one highly sceptical of arguments that aim to qualify specific approaches as inherently more suited to engage with practices in effective or benign ways.10

From the present point of view qualitative research should be precisely about experimenting in order learn how to articulate what takes place in practice. So, if, as I have argued, knowledge is generated through specific relations, then a method claiming to overcome the specific in the name of validity is misguided. However, as valuable knowledge clearly can be generated, something rather different from Denzin’s power plays must be taking place.

Knowledge is created to the extent that the powers and capacities of actors are made visible or tangible in new ways. This view does not aim at offering to the researcher a renewed source of power over subjects under study, nor over other researchers with different aims and opinions. Becoming aware that one is a partial participant of the field suggests rather that qualitative researchers should be suitably humble. But it is not a view, which argues only for humility. Indeed, it also offers flexibility, since research does not have to be forced into a pre-determined methodological frame for it to be presumed able to legitimately propose interesting questions and find interesting answers. So in contrast to Denzin and Lincoln, I do not believe that the position of the qualitative researcher calls for perpetual reflexive radicalisation. Likewise it becomes unnecessary to spend energy on constantly trying to
re-situate oneself at the peak of a historical moment, with the purpose of retaining belief in the legitimacy or importance of qualitative research.

The point, then, is not that in qualitative research, ‘anything goes’. Quite differently, the point is that almost nothing goes; and what actually does go can only be found out by continually taking into account the specificity of a situation. Good qualitative research must therefore by necessity pay attention to the partiality of the enterprise. Among other things, this means learning to take quite seriously that knowledge is always obtained concretely, and for that matter can never be ensured from the outside, but only through interested interaction. For that same reason no ‘guidelines’ will be found in the conclusion to this argument. Instead I offer an illustration of how the position can be worked with in practice.

6. An example: Situating the Aids Controversy

In their article “Dissent in Science: Styles of Scientific Practice and the Controversy over the Cause of AIDS”, Joan Fujimura and Danny Chou (1994) analyse “the co-production of facts and the rules for verifying facts over time,” as exemplified by the AIDS-controvery.

In 1984 the theory that HIV causes AIDS became widespread, but Peter Duesberg, a retrovirologist from Berkeley contested it vehemently. At first he was ignored by the specialist community, but as he received increasing recognition from Spin Magazine, talk shows, and a number of gay newspapers, a story eventually built up to the effect that the scientific community was covering over the fact that they did not really know what was going on. From then on Duesberg had to be taken seriously in the controversy, which is followed in painstaking technical and rhetorical detail in the paper. Here I briefly touch upon a few important points relating to Fujimura and Chou’s strategy for dealing with the case.

Fujimura and Chou note that much evidence for HIV as the cause of AIDS was based on epidemiological statistics. How do epidemiological studies gain stability and validity? Fujimura and Chou suggest that epidemiology follows “a particular style of practice that can be characterised as a mosaic constructing industry, where incongruities are common and do not stop the flow of action” (1024). How, for instance, does one
build up knowledge suggesting that HIV is a causative agent of AIDS? Here is a list of elements:

… quantitative population analyses associating HIV with AIDS at particular times, places, and within particular population groups; the continued association of these variables over long periods of time; the clinical production of information on individual symptoms, progression, and pathogenesis; clinical diagnoses to produce AIDS “cases”; laboratory technologies, especially molecular and biochemical assays for detecting the presence or absence of HIV in individuals; laboratory research on pathogenesis in vitro; and combinations of the above to study transmission patterns and rates of transmission of AIDS (1024)

And of course this list is far from complete, for all the elements traditionally surveyed by social researchers are also there, among these the initiatives relating to “political, medical and public health efforts to stop transmission and treat infected person” (1024).

While this “heterogeneous engineering” (Law 1986) is everyday life in epidemiological research, its divergence from the ideal methodical protocols of science also rendered it vulnerable to Duesberg’s attack. He pointed to numerous abuses, as he saw them, of “Koch’s postulates for relating causative agents to disease” (1023). Epidemiologists responded that Duesberg’s outsider status (being a retrovirologist) led him to misunderstand these crucial postulates; that he was…

…asking for an unrealistic application. [Epidemiologists] contend that histories of epidemiology and disease causation demonstrate that Koch’s postulate are historical objects that have been modified, interpreted, and used differently in different historical periods (1025)

Fujimura and Chou thus become able to point out that the activity of deconstruction is not a prerogative of postmodern cultural studies, but is easily found inside scientific controversies: “Duesberg has accumulated information to deconstruct their history of epidemiology and construct his own version” (1028).

In Fujimura and Chou’s analysis many different kinds of entities are constantly and completely entangled with the human scientists. Every part of the story, in other words, is:

…connected to … other[s] through processes of meshing together these mediating elements including ideas, (questions, background knowledge, systematic theory, topical hypotheses, modeling of the apparatus), things (target, source of modification, detectors, tools, data)...these elements and their ‘mesh’ with each other are constructed through time to ‘fit’ with each other. In other words, they are co-produced (1022)
This language has clear affinities with ‘chains of associations’ as well as with Haraway’s cyborgs and Star and Griesemer’s boundary objects. Notice here that the ‘enchainment’ of heterogeneous elements (human scientists, laboratory specimens, technical equipment) is exactly what enables the epidemiological mosaic of knowledge to function. It is thus through partial and particular processes of fitting heterogeneous elements to one another that this knowledge is constructed. However, when this process is considered from a point of view (Duesberg’s) that does not allow scientific practice to deviate from the epistemological ideals for science, the epidemiological claims can be easily challenged as being less than objective.

I can now briefly review the assumptions and strategies of this analysis, indicating first what the study is not. It can be seen immediately that this is not an underdog analysis, for an underdog perspective makes no sense while a controversy is in action. Who would one support? Is it Duesberg going up against the smug establishment of epidemiologists? Or, on the contrary, is it the hard-working and sober-minded epidemiologist challenged by an increasingly glamorous outsider, who can force them to take him seriously because of interventions by CNN and Spin? As the authors write: “We propose that all the debaters are real scientists…[who] use and invoke different styles for adjudicating truth” (1022). The analysis is therefore meticulously symmetrical and neutral. It does not imagine itself as having the capacity to elucidate and normatively objectify what is and ought to be the case (e.g. Gouldner 1968), since even insiders working with the problem are unable to agree. Neither does it aspire to unify or create consensus on the case, since micro-differentiations and specificities is what continually becomes empirically visible.

On the other hand Fujimura and Chou do not worry that their story might be too realistic and that they partake in a scientific play of power. They do not need to express themselves through poetry or theatre in order to efface power-effects, because their own study clearly demonstrates what Denzin and Lincoln do not realise: that scientists are far from being all powerful actors. As the case exemplifies, sciences are often rather localised and contingent affairs, whose effects can be neither derived from patriarchy or capitalism, but which can be described more or less interestingly and coherently, with
some set of points in mind. But the realisation that one brings to a case some set of points, need not send one into reflexive spirals, for it is entirely mundane:

Construing history is one means by which scientists reconstruct rules for verifying facts and findings; that is, construing history is part of the self-vindication process. (This act is not at all limited to the participants of this controversy. We all do this, as exemplified by this text and discussed by recent works in the writing of history (1028, see also Lynch 2000)

Symmetrical analysis, taking into account the multiplicity of actors relevant in a given situation, with reference to a specific problem, does not equal complicity or “quietism”.

In Fujimura and Chou’s article no one is accused and everyone soberly described. Duesberg claimed that then current health initiatives would be harmful rather than beneficial to AIDS-bearers, since they built on bad epidemiology. He also claimed that AIDS is not an infectious disease, with consequences sounding “dangerous to the ears of AIDS researchers”. “That is”, the authors explain, “that Duesberg is telling people that they will not contract AIDS from practicing unsafe sex and using dirty needles” (1032). This laconic description indicates that the authors share the worry of AIDS researchers. However, this is shown without adding to the worry (which has already been made available in the description) a layer of empirically unwarranted moralism.

Fujimura and Chou thus refuse to simplify their case. Indeed, the strength of the study is in the demonstration of the extreme level of practical complexity in contemporary technoscientific controversies, which also makes visible the uncertainty and fragility of many types of scientific practice, that are rather often viewed by qualitative researchers as dangerous and strong. Their study thereby exemplifies many of the virtues of qualitative research as partial connection.

7. Discussion: Politics of Qualitative Research

As I have discussed throughout this paper qualitative research can be easily imagined as relating to a problematic of truth and power. Truth: there is a question of the knowledge of the researcher. Power: there is a question of the relationship between the researcher and the practice.

While ‘traditionalist’ qualitative researchers (still) believe in validity and objectivity and aims at it, postmodernist qualitative researchers such as Denzin and
Lincoln worry about this ideal. They do not think that self-invisibility is a viable option, and knows about blind spots in putatively neutral methods. In spite of these shortcomings they realise the power science holds. They therefore become interested in using this power beneficially, to let the voices of the silenced be heard. But here they encounter the problem of how to do so while ascertaining that their own discourse does not drown out these voices. This quandary makes them wary of mimicking the brash scientific rhetoric of realist qualitative research. Instead they start to speak more and more softly in order not to impose on practice too much from the outside. But arguably this strategy merely makes their position weaker, without bringing about any particularly beneficial effects for those presumably suppressed.

From the vantage point here advocated research looks quite different. It is manifestly not a matter of looking at practice with a steady gaze to be able to represent it correctly. Believing in this epistemological chimera closes off many sorts of possible connections with the known, because it associates knowledge with distance. As Haraway and Strathern, among others teach us, knowledge-production happens in the relationship between knower and known. Having relationships does not make it possible for us to represent that with which we are related in a perfect way. But nevertheless relationships can be productive, and they need not all be about power in a repressive sense. They can enable the articulation of new properties of situations and new modes of action.

The traditional qualitative researcher thinks science has strength for a good reason: it speaks the truth (if, these days, approximate). The postmodernist feels the reason is bad, but that the strength is nevertheless real. Their responses to the truth/power locus are thus symmetrical. The traditional social scientist wants to keep his strength by staying distanced. The postmodernist wants to lose strength in order not to risk violating practice. If research is about partial connection these strategies are misplaced. Arguably, the problem of the social scientist is not that his connections are too many and too strong, but that they are too few and fragile (Latour 1988). If this is the case we need many more ways of linking with practices, and not methodical, theoretical or reflexive techniques for severing the existing ones. Hence, the main concern of the non-objectivist researcher should not be to prevent doing violence to practice, but to come up with ingenious solutions to the problem of how to become interesting enough for practices to care about. It is doubtful if either dry sociological monographs or elliptical reflexive writings are up
to this task. If research is not about representing practice but about exploring common futures with practices, then both methodical rigour and reflexive subtlety become less interesting.

To think of partial connection as research methodology is to think about ways of engagement that would simultaneously add more agency to researcher and to practices, since in this view there is no need to view science as an epistemological zero-sum game (Latour 1987a). In this light what is called for is an exploration of collaborative methods in various settings. If research is about exploring, not alone but with others, how diverse agencies could become more expressive in the invention of the future, then we need to explore more and better ways of connecting with the objects and subjects we are researching.

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1 See for instance (Clifford and Marcus 1984). For a number of rather different interpretations of the challenge posed by this work, see Fox (1991).

2 In “Whose Side are We On?” Becker (1967) made the argument that “we”, the social researchers, are on the side of the deviants, the underdogs. In a highly critical response to Becker, Alvin Gouldner (1968) notes his worry that “the myth of a value-free social science is about to be supplanted by still another myth … a metaphysics of the underdog and of the underworld.” For Gouldner the uninhibited sympathy with the underdog seems compromised first of all because the apparent empathic stance towards underdogs is reconfigured into “ombudsman sociology: critique of the middle man” (107). As an alternative Gouldner suggests that the sociologist is one who judges “in conformity with some stated normative standard” (113) as part of a moral struggle for objectivity: “Underlying the quest for objectivity, then, is the hope of dissolving the differences that divide and the distances that separate men by uniting them in a single, peace-bringing vision of the world” (116). This construction of unity is rather peculiar, because the normative standards of the sociologist need not be shared by those he studies, or even developed in concord with them, but only applied, and this regardless of the unpopularity such application may generate (114). As we shall see Gouldner’s solution is radically different from the position advocated in this paper. Nevertheless he touches on a number of issues of importance for those currently claiming to carry out postmodern missions for the underdogs.

3 In Denzin (2001) the "post-experimental phase" (1996-present), follows the "postmodern, or experimental phase" (1990-6). It is to be followed by the seventh moment, which presumably, will be characterised by even more fragmentation of the field in simultaneity with even higher degrees of reflexivity and increased social relevance. All of these claims are asserted but left more or less unsubstantiated. They are also highly dubious, not least in their naïve historiography.

4 Regarding this statement of Richardson, it could be observed that to a good many people it is manifestly not "entirely obvious" that postmodernism is of such beneficial effect. Among such sceptics, furthermore, many view themselves as interested in the same set of problems, of social inequality, as the ones that exercise Richardson. In a series of highly relevant analyses Barbara Herrnstein Smith (1997) has shown how the "cognitive conservatism" that informs the positing of one’s own one tendencies and
tastes as "entirely obvious" and those of others as manifestly dangerous, biased, or benighted, are highly important mechanisms in stabilising debates in ways that prevent their transformation.

5 “... it is the problem which orientates, conditions and engenders solutions, but these do not resemble the conditions of the problem” (Deleuze 1994: 212). A bad problem, consequently, engenders no solutions, because it is conditioned and orientated in a way so as to perpetuate itself.

6 I take up this notion (from Strathern 1991) in section five.

7 See (Latour 1988) and especially (Latour 1999, chapters 4 and 5). These ideas have clear affiliations with Spinozism, see e.g. (Spinoza 1959, Montag and Stolze 1997).

8 This view is related to that of Isabelle Stengers, when she suggests that "nothing is easier for a modern person than to be tolerant", and argues for the need to "have done with tolerance" (Stengers, n.d.). Perhaps, however, it is to move too quickly to accuse an abstract relativism for such faults. Barbara Herrnstein Smith (1997, 2002) has argued convincingly that it is not relativism per se but, more often, objectivist representations of it, which would claim that relativism entails a belief in the equality of positions. The scholarship of Denzin and Lincoln illustrate many of the problems espoused relativists may get into when they take on themselves objectivist-inspired ideas of what relativism involves.

9 See for instance (Strum 1987) on research in primatology, also (Rowell 2000) – on sheep. See also Latours elaborations of the importance of this research for questions in epistemology and ontology (Latour 2000).

10 The inevitable textual reconstruction of encounters with and in the field adds to the problem, for a traditionalist may in practice engage a field in a respectful and, at least from some perspectives, politically helpful manner, yet nevertheless write up an account of his research in a realist mode, which would seem outdated to politically interested postmodernists. On the other hand researchers of a theoretically postmodern bent are not by this fact immured from behaving in ways arrogant, self-centered, or otherwise damaging to the practices they engage with; for as is always the case, one’s espoused theoretical purity does not guarantee successful handling of the contingencies of practice. I restate this point because although one would imagine it to be lesson one in "postmodernism", it is one, which seems to be surprisingly often forgotten.

11 In a formulation of Isabelle Stengers: "The art of the experimenter is in league with power: the invention of the power to confer on things the power of conferring on the experimenter the power of speaking in their name" (Stengers 1997:165, emphasis in original).
Chapter three: 
**Researching Partially Existing Objects:**
What is an Electronic Patient Record? 
Where do you find it? How do you study it?

1. Introduction

The empirical focus of the present study is the networks surrounding and feeding into; indeed, constituting, the development of electronic patient records in Denmark. What then do I study, exactly?

Roundabout expositions and vague delineations like the one attempted in the introductory sentence above are surprisingly often interpreted, by academic interlocutors (peers as well as those higher in the hierarchies) and other conversation partners as suggesting that I am involved in the *practical development* of a specific technology. Alternatively I am understood as suggesting that I study *the political processes* relating to current events; consequently, I am offered evaluative comments about this situation, or asked to give my own judgment. As will become clear in what follows both understandings are rather far from the mark, however, offering a lucid exposition of just what and where that mark *is* has proven insistently elusive throughout my project.

In intellectual and especially academic terms there is something evidently troubling in being unable to straightforwardly state what one is analysing. Here I want to turn this annoying problem into a topic worth exploring in its own right, and consider what is happening when my explanatory attempts fail to convey to interested people what I spend my time studying.

I want to explore this issue not out of self-indulgence or because of a deep interest in reflexivity. Rather, I want to relate my experienced communicative dissonance to the problematic “being” of the electronic care record. I want to claim, in other words, that my inability to convey what it means to study the development of the electronic care record in Denmark is related to the seemingly paradoxical ontology of this entity as empirically encountered.

The paradoxical quality stems from the commonplace notion that *qua* technology an EPR must be *one homogeneous thing*. In practice, however, it seems to be rather more like a *multiplicity* of things, which forms a whole only sometimes, or for some purposes.
When I call the paradox *seeming* this is because the suspension of modern categories advocated in non-humanist STS dissolves the paradox: it is one only from within a modern ontology according to which the world *must* be filled with singular, well-defined, stable objects. Dissolution of the paradox, however, does not mean the disappearance of all conceptual or analytical problems: rather the field of problems and solutions change.

The problem will no longer be define proper issues, which must be taken into account, and use these to adequately represent the situation at hand. Rather the issue will be to empirically track down how situations *are* variously delimited and with which consequences. Since no adequate model is available, one has no access to an external standpoint from which to offer an evaluation: accordingly one cannot denounce actors for failing to see what one, as analyst, clearly sees. Rather, one participates with other actors in experiments to define and re-define what are the relevant contours of the problem and where its limits might be placed.

The first point of this paper will be to clarify *why* it is so hard to state succinctly what one is studying when one is studying developing technologies such as the EPR, and *what* one is, actually, studying. This will make available for exploration a number of possibilities, which come into view when one redefines the EPR, as I will continue to do, from what appears to be technological thing, to a set of more or less fluid practices.

### 2. What is an EPR? The Paradoxical Ontology of a Developing Artefact

One needs to be nominalistic, no doubt… (Michel Foucault 1990: 93)

The answer to the question what is X (e.g. what is *truth*?, what is *knowledge*?, what is *value*?) is, that it is, in the first instance, a *word*, with a *history* of variable and still changing usage (Barbara Herrnstein Smith)

Let me ask first in a mundane way what is an EPR? It is easy enough to find suggestions. For instance, the Danish National Board of Health, in their *National Strategy for IT in the Health Care Sector 2000-2*, offers the following:

An EPR is a clinical information system, which directly supports process-oriented examination, treatment, and care of the individual patient… ”Process-oriented” means a patient record, which directly supports coherence and quality in the clinical treatment

In this definition the EPR is centrally about supporting existing clinical practices, but enabling a more coherent technological framework for doing so. In other documents this
problem is specified as *conceptual*, and terminological clarification and standardisation is just what the National Board of Health aims to provide.

However, the ease of finding suggestions points also to difficulty of locating a clear answer to the question of what the EPR *is*, since they inevitably vary. The EPR development project in the Aarhus Region, for instance, adopts the definition from the National Board of Health, but stresses that several additional operational demands are crucial. For example, the EPR should be integrated with other hospital information systems and the record should be available as an efficient work tool for all kinds of health care workers. The Aarhus development group therefore stresses that: ”we are talking about long-term development projects, with an emphasis on organizational change and learning” (http://epj.aaa.dk).

Thus, even though their description starts with the citation from the National Board of Health, their end-point is quite different. What matters to the development group in Aarhus is not primarily conceptual standardisation but the organizational transformation and development said to be enabled by it. This definition is not stable or authoritative either; not even within the project in the Aarhus region. When one approaches nurses, secretaries or doctors, who will be using the technologies in practice, multiple other understandings, notions, ideas and hopes are retrievable (see discussion in chapter eleven).

Since understandings of the EPR are proliferating, resonating and overlapping, but rarely identical, one may feel forced to decide whose definition to believe in. One is *forced* to evaluate the trustworthiness of these various definitions, however, only to the extent that it is viewed as necessary to *start out* with a more or less singular and homogeneous definition of the subject matter. While this is a classical move in modern (social) science, there is another option: following Foucault, one can be nominalist.

In the phrase of Barbara Herrnstein Smith, this means insisting that the electronic care record (as any other thing) ”in the first instance” is a word. Insisting thus certainly adds interpretive flexibility to an investigation. It leads one to expect variable answers to any number of questions regarding the object: What is the EPR? Does it even exist? What does it do? Where is it found? What are the benefits of it, and for whom? What are its risks and costs, and for whom? According to nominalism all the answers thus retrieved can be taken as reasonable contextual responses to the given question. In contrast, if one
starts out assuming that the EPR is a specific *something*, then alternative suggestions encountered in practice would be viewed as deviant or benighted and, perhaps, to be corrected. The above formulations can be read as a re-statement of the anthropological dictum that a researcher of some set of social practices should take seriously the perspectives of *all* the groups of people he or she encounters, rather than merely the ”official” or institutionalised ones.

But enumeration of perspectives need not be the end-point of the nominalist investigation. STS-theorist and empirical philosopher Annemarie Mol advocates a shift “from understanding objects as the central points of focus of different people’s perspectives” (Mol 2002: 4). Instead, she proposes, we could look at the practices in which objects are manipulated:

If practices are foregrounded there is no longer a single passive object in the middle, waiting to be seen from the point of view of seemingly endless series of perspectives. Instead, objects come into being – and disappear – with the practices in which they are manipulated. And since the object of manipulation tends to differ from one practice to another, reality multiplies...Attending to the multiplicity of reality opens up the possibility of studying this remarkable achievement (Mol 2002:5)

Mol calls this research programme *empirical philosophy*. It is philosophical in its interest in understanding knowledge. But it insists that this can best be grasped through engagements with knowledge practices; by carrying out a *praxiography*. Which leads to a rather different set of concerns than traditional epistemology:

A new set of questions emerges. The objects handles in practice are not the same from one site to another: so how does the coordination between such objects proceed? And how do different objects that go under a single name avoid clashes and explosive confrontations? And might it be that even if there are tensions between them, various versions of an object sometimes depend on one another? (Mol 2002: 5-6)

In the nominalist formulation above I stated that the EPR had to be viewed, first of all, as a word. Now, however, following Annemarie Mol, I talk about materiality, practices and objects. This is not a ”performative contradiction”. Such shifting frames of reference are themselves necessitated by the variable ontologies of objects under study. The EPR certainly allows me to make the point.

Traced in practice, “it” traverses a number of modern taken-for-granted categories. When “it” is encountered in a given practice, for instance, there is no obvious answer as to whether the encountered referent is an ”empty signifier” or a ”real” technology,
something ”envisioned” or something ”concrete”. In some places ”it” is viewed as in existence, but others will argue that what one can encounter at hospitals currently using EPR technologies are not the real thing but, at most, vaguely related pre-cursors. In some places ”it” is being built, but as it exists only in beta-versions, and remains untested in practical situations pronouncements on its reality are marred by uncertainties.

Furthermore, the EPR is variably understood in local terms: developed as a solution to very specific medication procedures and problems, or in national terms: as an initiative carried out by the National Board of Health to rescue the Danish Health Sector from presumed deterioration and ruin. One really cannot know whether the EPR is discursive or material, local or national, beneficial or harmful, technical or political, or all of these to varying degrees and in various places. If it is presumed that entities have fixed properties this is a paradoxical claim, but the paradox diminishes if one begins to think in terms of variable ontologies; that is, if one starts imagining that the properties of entities are not essential, but are variably articulated (or constructed) in different practices.

In researching technological projects and developing technologies, STS-scholars ”need to be nonimalist, no doubt”. In all that follows I therefore stick with the assumption that any entity is a word ”in the first instance”. In the first instance but not necessarily in the last instance. Just as empirical philosophy has no investment in pre-determining what an entity must be at the beginning of an inquiry, it has no reason to claim that an entity will have to remain word-like. And, of course, words and visions sometimes do materialise as technological reality, although they tend to transform immensely in the process. Letting go of both a priories and finalities such inquiry facilitates a very flexible approach to the question of what actually happens in practice.

3. Where is the EPR? Locating Partially Existing Objects

I…study how different styles of research practices emerge and survive; I am interested in how their disputes are conducted and how factions are formed and maintained, how their community recognizes and limits variations in their practices. I am intrigued by how these practices differ along lines of class and gender, as well as lines of local, regional, national, and international political economy. I want to know how this powerful group creates and constantly recreates a discrete, identifiable community while operating all over the world in many different local, regional, and national cultures…(Traweek 1996a: 51)

Anthropologist Sharon Traweek describes her research interests in relation to the highly dispersed community of high-energy physicists. Her focus is on the relationship between
the homogeneity and heterogeneity of these people; that is, why everyone would agree that they constitute a community, even though everyone can also recognise that this group is differentiated in numerous important ways.

It is Traweek’s merit to show how coherence is practically maintained and stably reproduced as a "physicists’ community” without recourse to the powerful essentialising explanatory model, which imagines their similarity as consisting merely in them having shared ideas. Her questions and interests are clearly affiliated with those one could ask under a program of empirical philosophy. A contrast could be found in the willingness of the latter to use its insights to engage in somewhat more abstracted discussions of the results of such moves away from classical theorising.¹

Other recent articles have discussed the specific practical and methodological problems inhabiting attempts to make anthropological studies of information technologies (Henriksen 2002, Newman 1998, Star 1995). Anthropologist Dixi Louise Henriksen characterises the classical conceptualisation of the relationship between the researcher and the object of study in the following way:

…we tend to consider the site and object of study as preceding the empirical investigation. We think of a field study as a situation in which the researcher in person enters a bounded site, for example a particular organizational department or a single control room, to investigate the nature and characteristics of the setting for a specific period of time. Such notions of field study and fieldwork rely on the concept of a field site as an already delineated geographical location and on an object of study that pre-exists the study and lies out there just waiting to be discovered (Henriksen 2002:32)

This traditional approach, dubious in its naïve realist and positivist epistemology, encounters multiple practical problems in contemporary networked organizations, since the field site is often geographically dispersed and the objects of study, not least in the case of information technologies, are distributed. While this situation makes it impossible to survey the entire field (whatever that might mean), it also enables the emergence of a new set of opportunities and insights. Not least among these is the realisation that the perception of entities such as networked organizations and corporations as ”very large” or even ”global” is interrelated with the functioning of what we regularly understand as ”very small” localised activities. The effect of scaling up from “local” to “global”, that is, might be viewed as a complicated achievement of various sets of actors, constantly
testing which social and technical elements will be able to durably hold together if moved out of a specific setting:

Like tracking the assembly of actors and design issues, grasping the technical object involves frequent shifts and the judicious knitting together of disparate elements. As much as for the ethnographer, or more, it requires this virtuosity from the participants themselves...These activities are part of the practice of constructing a future of relations mediated by the technology-under-design. (Newman 1998: 258)

Such complexities return also to haunt the researcher trying to make sense of his data, as I will return to below.

Henriksen and Newman both indicate the difficulty of locating technologies as simple things, and point to their heterogeneity and distribution. The complexity of studying such artefacts is highlighted by the fact that both researchers encountered these practical and methodical difficulties in investigations carried out in what might superficially seem to be relatively circumscribed fields: Henriksen studied a Web-based information system in a pharmaceutical company, while Newman “organized...fieldwork primarily around the activities of a small, but changing, group of participants composed of system architects and developers and their managers” (Newman 1998: 237) in a product organization.

Problems of clearly delineating the field proliferate in the case of the EPR. Here, multiple actors from multiple sites, such as the Danish government, the counties, the National Board of Health, the health professional and patient organizations, individual hospitals, the medical informatics community, the Board of Technology, as well as software companies, standardisation organizations, and medical jurists try to define and influence the stakes of development.

Furthermore this debate is not only discursive but also exceedingly material, as indicated by very active attempts by different Danish regions to develop a well-functioning EPR model. To be able to demonstrate a successful system to politicians, other hospitals, and the public, would likely be a much stronger argument for the adoption of that particular system as the national standard than any amount of written statements.

Locating and investigating technologies in highly distributed and politicised environments is a complicated business. What one investigates in such instances seems
not to be a technology, which happens to be distributed but, vividly illustrating and
dramatizing the variability of practical ontologies, rather many different material and
discursive ways of ordering practice, which sometime go by the same name. This makes
explicit the possibility that the EPR might exist in a diversity of modes. Or should that be
– makes visible the fact that different varieties of EPRs exist? Again, this remains
undecidable because the ontological unity or multiplicity of the entity referred to as EPR
is at stake in these very practices. Thus, the EPR offers a concrete demonstration of what
Latour talks about as the partial existence of objects (Latour 1999); an object the
actualisation of which many different actors are invested in, in many different ways.

For these involved actors, as we shall see, the multiplicity of involved interests
have been both an asset – in that it has ensured a very broad support of the development
of a Danish EPR – and a challenge to the very existence of an EPR as a singular
recognisable object in the Danish context – because the multiple engagements in its
construction threaten to fragment development and turn it into many (Latour 1993, 1996,
1999). The complexity of this situation encourages, indeed demands, empirical studies of
the many practical and material events, which are variously taken to relate to the EPR.

My discussion so far has repeatedly stressed the productivity of adopting a
nominalist stance, and emphasised the multiplicity, heterogeneity, and variability of “the
EPR”. From this description it would seem that the EPR is very hard to pinpoint, as
indeed I think it is. But, conceding that, the point is not to show the complete randomness
or subjectivity of the phenomenon. Rather, the idea is that analyses, which are duly subtle
and responsive to the variability of actual occurrences in the EPR-landscape would be
facilitated if one became seriously de-familiarised with the traditional ontological
assumptions underlying most explanatory models of technological development.

Reaching that point it would become possible to closely follow the multiple
associations of objects, humans, and practices, which are working hard to produce
specific versions of the EPR. It would become possible to trace the ways in which some
varieties of the EPR are stabilising (gaining existence) and the counter-movements
through which other versions of it are disintegrating (losing existence) (Latour 1999,
chapter five). Finally, it would enable research into how an entity becomes an entity, with
an apparent unity, essence, and established definition, or how, contrarily, it does not
become an entity, but might become several different one’s. In proposing the study of the
ontological stabilisation and destabilisation of technological objects I am thus not saying that coherent, unified objects cannot exist. But coherence is an achievement: it requires lots of work to assemble complex technological objects and ensure their survival; it is never a matter of natural development or simple progress.

The nominalist stance also folds back on the writer who must be seen as himself implicated in the production, definition, and assembling of some versions of the EPR rather than others. As a social researcher one is not free to articulate versions of this entity: most often other actors would immediately jump on incredible claims and propositions as they encountered them. But the alternative to the feared scenario of unconstrained subjectivism and wild imagination, is not to view the researcher like the blank slate from the positivist ideal, on which information about the EPR could be neutrally imprinted. Rather, object and researcher may be seen as mutually articulating each other, like in Winthereik, de Bont, and Berg’s formulation:

the various EPRs encountered at different sites should be allowed to each “…yield new questions and guide [the researcher] into a different direction, whereby she ha[s] to redefine what would count as a research site (Winthereik, de Bont and Berg 2002: 51)

In this view, STS-studies should work to simultaneously elucidate the practices about which they talk, and add to the conceptual repertoires of the disciplines on which their investigations draw. This aim is what motivates my analyses in everything that follows.

Acknowledgment: Thanks to Barbara Herrnstein Smith for discussion and careful reading. Thanks also to Jeanette Pols and Brit Ross Winthereik for critical comments and suggestions.

1 Giving the topic this may seem surprising, but in recent years the development of electronic care records has figured as a controversial public issue in Danish media.
2 It is difficult to describe the EPR in ”regular” language given its equivocal status between thing and non-thing. In this chapter I use scare quotes liberally to point to the fact, but in the following chapters I take the point as established and refer simply to the EPR.
3 Nominalist investigation as here defined means, not least, adhering to the principle of symmetry as formulated by sociologist of science David Bloor. This principle, as discussed in chapter one, prevents the researcher from letting his analyses lean on the outcome of a given historical transformation (in terms of, for example, its truth or falsity, effectiveness or ineffectiveness, benefit or harm), as it is now perceived (Bloor 1976).
4 She discusses this unwillingness in Traweek 1992.
5 I do not think that these fields are circumscribed and, for reasons discussed especially in chapter ten, I find the idea of something being, as such, ”more limited” than something else, dubious. However, from the point of view of the classical micro-macro distinction, these field studies would look in this way; that is, as located firmly within the micro.
6 The first of these formulations suggests that the EPR is one technology, which happens to take different concrete forms in different practices. The second proposes that there is no such thing as the EPR but
many competing technologies exist in different places. The ontological unity or multiplicity of the EPR is not an esoteric question, but one, for example, which bears immediately on the relationship between the National Board of Health and the Danish regions, which are threatened with harsh measures if they do not adopt the model of the EPR developed by the former. The practical and ontological question arises from the fact that what it means to adopt the model is a negotiable matter.
Part two: Strategies

These words are not tools of communication they are shards of metal (Tim Parker “Release” on Blazing Arrow, Blackalicious, MCA Records: 2002)

The “Strategies” referred to in part two are reading strategies. The question raised is how one might engage or diagnose sociotechnical events as they are presented in multiple reports, mission statements and other political documents that proliferate wherever technical development is promoted. In my analysis reports are actors with important functions. Temporally: as they express technological possibilities in a flexible manner prior to their actualisation in more durable materials. And spatially: as these documents move around and engender effects in various practices. The question of where reports come from (certainly not from nowhere) will be broached in chapter seven, but here I discuss their efficacies: as the introductory citation intimates I view these as performative and multiple, rather than as simple communicative vehicles of intentions and ideas.

Chapter four is called “Political and Moralising Moments: Documents as Material Agents in Danish Visions of IT in Health Care” and is co-written with Brit Ross Winthereik. In it we analyse documents from two IT-projects in Danish health care and propose that they can be seen as instantiating a certain information genre, ideal for flexible envisioning. We analyse how argumentative agency is produced in an EPR pilot project at Hvidovre Hospital and in the Digital Doctor project carried out by the Danish Board of Technology, and we suggest that the reports written in both instances functioned to enrol and bind together people from multiple contexts. In the chapter we identify what we term political and moralising moments in these reports. Political moments are events through which the ends and means of practices are surreptitiously re-formulated. Moralising moments are exemplified by the grand pictures of technological progress put on written display in both projects, in spite of their limited practical success.

Chapter five “Reading Digital Denmark: IT-reports as Material-Semiotic Actors”, co-written with Peter Lauritsen, engages the theme of reading strategies head on. Through analysis of the important Danish IT-strategy Digital Denmark it exemplifies two such strategies, which we call read “with” and “against” the text. The latter constitutes a
classically critical and sceptical reading aimed at the uncovering of hidden or repressed meanings, which may be ascribed to the bad motives or dubious intentions of authors. The former suggests instead that one follows the text as it travels in practices, and describes its concrete effects: where do they go? what do they do? We exemplify the reading “against” by examining Digital Denmark’s conception of learning, which, unsurprisingly, is rather problematic. We then move with the text to see how it is translated in practice in the Digital North Denmark initiative.

Reading with texts is a matter of seeing them as properly material-semiotic actors. This contrast is also of normative significance and we discuss various political implications of adopting this strategy.
Chapter four:
Political and Moralising Moments:
Documents as Material Agents in Danish Visions of IT in Health Care

1. Introduction

Modern information technologies are penetrating all parts of societal life, changing organisations and types of work at an accelerating pace. Skilled use of the new possibilities is becoming more and more decisive for the competitiveness of companies, and for the ability of the public sector to provide good service of high quality for the citizens at manageable expenses. This development also affects the medical sector to a great extent. Data communication and electronic patient records are going to become increasingly important in daily work life (Sundhedsministeriet 1996: preface)

As I aim to show in various ways in this study, information technology (IT) coupled with ‘new’ administrative ideas have increasingly been presented as the cure for the 'sick' Danish health care sector in official reports and strategies, and in the media (Bentsen et al 1999). Among other benefits, IT has been imagined to enhance communication between the primary and secondary health sectors; between general practitioners and hospitals. Effective communicational integration is often based on a vision for the future in which IT facilitates “seamless” communication between sectors. Here IT is imagined as a neutral tool that can nevertheless work to immensely positive effect if properly applied in the setting of health care work and management.

However, problems and challenges related to the practical issues of integrating IT at local work places are seldom discussed at bureaucratic levels. The details of reaping the benefits outlined in such documents are instead left to health-care workers, administrators and IT departments who are locally responsible for implementing and learning to use the systems.

It is not my aim here to criticise this gap between policy statements and problems encountered in local work practices. Instead I am interested in the dynamics of the delegation of such responsibility. In fact I would prefer to abandon the distinction between the putative “micro-” and “macro-levels”, between “local practice” and “national vision”, and regard visions and the work carried out to produce them as

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1 Co-written with Brit Ross Winthereik. I refer to ”I” throughout this edited version of the text to retain stylistic consistency.
instances of practice to be scrutinised by analysis.\textsuperscript{1} This move is characteristic of actor-network theorists and social anthropologists, who prefer dealing with trajectories rather than pre-defined categories (Latour 1996a: 268, Strathern 1992, Wagner 1991).

In the following, I analyse ministry leaflets and policy recommendations as a kind of ‘information-genre’ with specific properties. I view this paper trail not as inert matter propagating visions from ‘above’, but as material agents, enrolled in the work of producing, negotiating and stabilising visions of health care.\textsuperscript{2} I suggest that ministerial reports and recommendations can be seen as agents whose capacities are consequential for the possibility of envisioning IT as a neutral tool with enormous potentials. Not simply representational devices, in this view reports are active participants in the transformation of health-care: they work by enrolling practitioners into networks of management and administration, where responsibility for “making the IT work” is delegated in new ways.

I am particularly interested in the moments when arguments seem to get ‘a life of their own’. But if arguments seem sometimes to thus ‘speak for themselves’ interest here will concentrate on the heterogeneous work enabling this to occur. Hence, I am interested in the production of argumentative agency, and in understanding how such agency may bind together various people in various contexts, and how it may enable interactions between various agents to become stabilised.

Following this agenda, an important question is how to conceptualise politics if political efficacy is not solely a matter of human activity. How can politics be understood, if what is traditionally viewed as the political proper, is only one part of an intertwining of humans, technologies and discourses? Below is developed the notions of political and moralising moments to answer these questions. They are not understood as abstract concepts but as ‘tools’, with which to capture several of the diverse events that took place in two experimental development projects in Danish health care.\textsuperscript{3}

The paper is organised as follows: First I introduce the notions of political and moralising moments. Then two cases are presented. The first deals with the HVEPS project (Hvidovre Hospital Elektronisk Patient Journal System), carried out from early 1994 to 1996, in order to expose “necessary user needs” for an integrated electronic patient record at a major Danish hospital. The second describes the Digital Doctor project

\textit{“Political and Moralising Moments”}
initiated by the Danish Board of Technology with the aim of imagining improvements of the future of Danish general practitioners’ practices by means of IT.

The two development projects relate to different sectors of the health care system but in both cases arguments and visions were stabilised by being reported. I want to show how, in both cases, this process led to the instantiation of a new distribution of agency and a new delegation of responsibility among the participants. In conclusion, I discuss the possible benefits of adopting a vocabulary of political and moralising moments in order to specify areas of invisible techno-politics (Star 1991a, Star and Strauss 1999, Wagner 1993) inscribed in official reports and recommendations.

2. Political and Moralising Moments

In this section the notions of political and moralising moments are deployed in an attempt to displace the divides between discourse and practice, the social and the material, or the technical and the symbolic. Following Bruno Latour, one can understand politics as having to do with the negotiation and translation of the goals and interests of networks (Latour 1994). Translation refers to ongoing changes in goals and strategies in an assembly of human and nonhuman actors, where

being an actant\textsuperscript{4} means mediating the activities of other actants – nothing more, and nothing less – in a never ending flow of incessant, but ultimately directionless motion (Berg 1996:254)

In this view the stabilisation of a network is a temporary effect, produced when a specific set of actants are able to align in a powerful way vis-à-vis other actants.

Negotiations do not take place between human actors only. Material agents are continually delegated roles in networks and granted the ability to carry out actions. But with delegation comes translation. Material agents rarely reproduce the intentions of human planning, and delegation processes often carries with it unexpected consequences (Latour 1994, 1999). Thus, doors jam, parachutes do not open, and computers crash, or seduce. Such unforeseeable consequences often remain un-thematised in social- and IS theory, with the result that the political work of negotiating with and through material actors remains invisible.

This work can be made more explicit with the notion of political moments.\textsuperscript{5} Political moments can be understood as events that reformulate practices. Often re-
definitions happen through the delegation of some task or other to a material agent. In this way politics takes place *through* materiality and *outside* what we usually think of as the political sphere - often at unexpected times and places. For this reason one may imagine politics not so much as planned and carried out, but rather as surreptitiously happening at specific unpredictable moments.

The situation as organisational analyst is attenuated by the fact of unpredictability: that political moments are indifferent to modern categories, and may cross technical, linguistic, scientific or political borders; thus being part of what Bruno Latour has called the work of hybridisation. Discussing political moments is an attempt to develop an analytical guideline of sorts that would allow one to grasp such changes as they happen in practice.

Contrary to the focus on political minutiae, grand pictures are often invoked to describe the goings-on in IT-development. I refer to such invocations as moralising moments. They are part of what Latour has called the work of purification (Latour 1993a) that cut the world (or the network) (Strathern 1995) up in specific pre-defined ways, and attribute efficacy or agency of particular types according to this distribution.

Moralising moments are often highly visible, or explicitly on display, but are of limited use in interpreting real time technologically mediated social activity. In a Nietzschean phrase they are the *reactive* counterparts to the proliferating, if invisible, *activity* of political moments. Wendy Brown explicated:

> moralistic discourse always harbors a certain anxiety about “practice,” it also operates as a strange substitute for action; it is what Nietzsche called “reaction” posing as action. Moralizing is aimed either at prohibiting certain things, words, or deeds or at compelling a very narrow set of words and deeds – and the latter, of course, is also a form of prohibition (Brown 2001: 40-1)

If reactive, moralising moments are nevertheless practically effective. As the pragmatists’ so-called Thomas theorem suggests: if something is believed in, then its consequences will be real as well; and the many varied effects of the imagined beneficial effects of IT surely makes the case. Political and moralising moments can be viewed as complementary analytical tools and methodical guidelines. Below, they are used to show how “the good future” is proclaimed in an unproblematic way within an area, IT implementation and use, which has a history of failures and problems (Edwards 1998).
3. Sites of Articulation

Here I examine two different interdisciplinary working groups within the Danish health care services. The purpose is to show how reports were written and disseminated, which subsequently became important agents in debates on the relationship between IT and health care. This happened in spite of the fact that both projects faced multiple practical problems, such as internal disagreements and trouble managing planned project trajectories. The focus of the first project, HVEPS, was on how to implement an information system at a major Danish hospital. To define the appropriate strategy a pilot project was carried out and a report produced. The second group worked on defining aims and goals for an intensification of IT-use in the primary sector.

The two project networks were engaged in different manners. In the first case the empirical backbone was publicly accessible articles and reports on the work, considerations and conclusions of the working group (principally DS Hæfte 4 1995, DSI Report 96.05 1996). These texts are approached by looking at their way of framing and distributing successes and failures to different sets of actors (human and non-human) at various times during the pilot project (as recommended in e.g. Latour 1986a, van der Ploeg 2002).

In the second case, Brit Ross Winthereik carried out observations at meetings in the Digital Doctor Project, and participated in a workshop and a conference arranged by the project group. Participation was granted by project managers from The Danish Board of Technology, notes were taken during meetings, documents and minutes collected, and individual project members approached for interviews.

4. Implementing IT: The HVEPS Project

Hvidovre Hospital, home of the HVEPS-project, is a large and modern hospital. Situated in the Copenhagen area, it has around 1000 beds, 4000 employees and an annual budget approximating 1.3 billion Danish Crowns. The hospital also prides itself on its advanced technology use in areas such as “diagnosis, patient care, registration, transport and communication” (DSI 96.05: 33).

The HVEPS project was carried out from early 1994 to early 1996 at Hvidovre Hospital. The purpose of the project, which resulted in a report (DSI 96.05), was to
“expose necessary user needs for the Electronic Patient Record, based on a practical pilot test” (DSI 96.05: 9).

The report is 150 pages long and organised in eight chapters with executive summaries in Danish and English. Individual chapters deal with topics such as aims, methods, relevant pre-conditions, pilot tests, results and recommendations. However, in spite of this straightforward layout some peculiar traits characterise the document. Foremost among these is the front page.

The headline is presented in yellow on a background of mauve. The cover shows a dustbin, crammed full of paper work and other accessories. More paper is stashed next to it. Behind it to the left, a computer monitor is shown. It is turned on and shows a copy of the dustbin in front of it. In a bizarre sense this Magritte effect seems to reproduce an experience often encountered in the IT-world: the use of a new system ought to facilitate new and handy routines, but organisational problems seem to re-surface in spite of all precautions. The recursive image on the front page hints at this feeling of organisational déjà-vu.

In the report, the process of uncovering necessary user needs is recalled in four steps: an analytic phase, an accumulation of experience of 'Electronic patient record (EPR)-aspects', experiences with the pilot test, and recommendations for further development of an electronic patient record.

It was not the first time practitioners at Hvidovre Hospital had experimented with an electronic patient record. As early as 1993 it was realised that a scattering of initiatives having to do with electronic records were taking place at various wards at Hvidovre. This realisation led to the appointment of an interdisciplinary working group with the purpose of uncovering advantages and disadvantages of introducing an overall electronic record system at the hospital. It consisted of five members, a doctor, nurse, technician, secretary and a representative of the administration (DSI 96.95: 114).

The group worked to develop an ‘in principle’ model of the electronic patient record. In the model, the record is to function as a 'gateway', which integrates various existing subsystems, including 'intensive monitoring data', 'lab data', 'x-ray images' and 'diagnostic data'. The development of this model marked the end of the preliminary work of the group; at the next step possibilities for its actualisation was considered.
On this background, a co-operative effort started in the spring of 1994, between Hvidovre Hospital and the Danish Institute for Health Services Research, an organization that is concerned with the development of health informatics in Denmark:

In many ways, informatics is the prerequisite and the tool for the current adaptation of the organisation and quality development of the health sector to environmental changes. Health informatics is therefore closely linked to DSI's activities within the development of databases for clinical quality (http://www.dsi.dk/frz_key.htm.)

The Ministry of Health supported the HVEPS project as a part of the national Action Plan for Electronic Patient Record (the HEP initiative). The purpose was to create a basis for the development of the electronic patient record in Denmark. In the early 1995, HVEPS was one of 14 such projects.

In the actual implementation process, the ‘in-principle’ model and its vision of a seamless work practice had to be compromised for at least two reasons. First economical compromise made it impossible to carry out the test at more than a single ward. Second, there was a technical compromise. It turned out that the structural model made 'in-house' by the working group was based on technological assumptions that could not be fulfilled by any existing technology at the time.

Instead, five criteria were formulated as a starting point for choosing a suitable record system. Multiple users had to be able to access the system simultaneously. The system had to be (more or less) ready for implementation. It had to be possible to code and classify the content of the record through the system. It also had to be possible to organize the record in a problem-oriented way. Finally, the system should fit one of the European pre-standards of record architecture. The Belgian system Health°one was chosen out of three potential record systems, primarily because it was standardised according to European conventions and was problem-oriented (DSI 96.05: 76).

Health°one was tested at the endocrinological ward. Here, technicians first had to build a local network, then install the system to make it run with existing one’s (such as those for patient administration). They also had to take care of maintenance and user support. Ensuring compatibility could only be accomplished in part. For instance, it proved impossible to integrate the system with existing patient information and lab systems (DSI 96.05: 75), although subsequent experiments made it possible to implement additional medication and diabetes modules. Technical implementation went along with
adaptations of organisational and co-operative patterns, and these turned out to be far from negligible:

Personnel had the option of choosing new ways of working. Doctors, for instance, preferred to continue using the dictaphone, whereas nurses preferred typing themselves...The largest operative problem was that data was entered into the record in wrong places because this was impossible to prevent, or that a pc ‘crashed’ with the result that the record was locked to other users. (DSI 96.05: 85)

In the HVEPS report the problems encountered are reflected upon and causes are assigned. First, the report suggests, users did not really want the EPR. Second, the record system was not tailor-made but a standard, which had to be adapted. Third, even though the needs of the users were analysed, the system could be adjusted only to a limited degree. Fourth, often the users had to adjust instead of making demands (DSI 96.05: 85).

In light of the diminished scope of HVEPS and its various shortcomings, it is noteworthy that the IT hopes, which invigorated the project in the first place, were not only virtually untainted but scaled up as a consequence of the pilot-test. The report concluded in the following way: “The HVEPS-project has fully shown that the challenges of introducing an electronic patient record can hardly be underestimated. The perspectives in this technology are no less than enormous” (DSI 96.05:7).

The description above shows a number of re-definitions or translations of the HVEPS-project. If the first move was to gather all the EPR-resources from many small groups to one centralised, then the second was to formulate a possible vision for the use of IT in the hospital at large. This problematisation made possible an alignment between the hospital administration, the National Board of Health, the Danish Institute for Health Services Research, and ‘moving spirits’ from different disciplinary backgrounds and wards at Hvidovre Hospital, interested in developing an IT-infrastructure.

But since the visions did not sustain themselves, and neither money nor technological solutions were as forthcoming as expected, a translation of the initial vision took place, as the project was located on a single ward. This could have led to the question of the extent to which it would then be possible to generalise from the pilot project to the overall hospital, or other specific wards, but this did not happen.

Considering the story of iterative trials and errors in the pilot project as it is narrated in the report and the small gains in functionality and instrumentality that this
work led to, the enthusiastic conclusion in the report seems remarkable. Indeed, it seems as if additional rhetorical work was needed to maintain the revolutionary potential of the electronic patient record in the face of continual difficulties.

Above political moments were characterised by their relation to the unexpected. I suggested that unpredictability were a consequence of the tendency of real life events to traverse modern categories, such as discourse and materiality. In the HVEPS-project a political moment can be located in the translation of the project from “large” and ‘in-principle’ to “actual” but “limited to a single ward”. Neither purely discursive nor material, this consequential change had to do simultaneously with information technologies, economy, and the organisation of work-practices at Hvidovre Hospital.

The intimate relationship between the political and the moralising becomes visible when is noted that this down-scaling of size of the experiment seemed to facilitate the up-scaling of expectation. Historian of science Geoffrey C. Bowker analyses a situation in the context of designing biodiversity systems and concludes that: “...integration cannot in principle be smoothly accomplished...without making politically charged decisions” (Bowker 2000). In the case of HVEPS integration was not smoothly accomplished, but the report allowed optimism to obtain in spite of the practical limitations and deficiencies encountered. The disappearing of such “politically charged” issues under a rhetoric of technological hyperbole constitutes the moralising moment in the report.

5. IT in the Primary Sector: The Digital Doctor Project

The second illustration of the production of visions relates to general practitioners and how the expectations of their role in the Danish health care system changed with increasing expectations of IT. The Digital Doctor Project was initiated by the Danish Board of Technology in mid-1998. The sector to be zoomed in on became primary health care which, according to the project managers of the board, was already “relatively IT-advanced”.

The project was defined as an exploration of solutions to the needs of general practitioners, which was meant to improve the quality and continuity of primary care given in Danish clinics. As in the HVEPS project user needs were seen as central ‘elements’ that had to be identified in advance. An exploration was conducted by an
interdisciplinary study group, which organised meetings, a workshop and a conference for central actors within primary care.

Representatives from eight different institutions were invited to participate in a number of meetings. Amongst these were representatives from Danish Institute for Hospital Research, The National Board of Health and the GPs’ association. During the first meeting it was decided that the main themes would be the use of diagnostic codes, communication between general practice and the secondary sector, the development of standards, and a future national health data net.

Before the second meeting, two general practitioners had been asked to make an account of the ways in which IT would be able to support daily work processes in their clinics. The general practitioners emphasised that communication between clinics and hospitals could indeed be standardised. According to the GPs, IT would also be of use for communication between their own practices. They had been invited to talk about their experiences and their overall message was that they did not need to change their present ways of working. Their presentations aimed at establishing and keeping stable their role as ‘gatekeepers’ to the health care services.

During the meetings two interrelated topics stood out. The first had to do with the relationship between the GPs and the National Health Insurance. The second had to do with the development of standards. The main argument from non-GPs was that it would become more and more important to measure the performance of general practices in order to ensure the quality of care. IT could be of central importance for such monitoring since coded patient data would form the backbone of clinical databases, on the basis of which comparison of the performance in primary care could be made possible. Moreover, it was argued that in order to ensure a more structured information exchange between hospitals and general practice, GPs would have to use diagnostic codes as a part of their clinical work. The representative from the National Health Insurance claimed that: “we have no way of measuring what you are actually doing out there in the clinics”, and the representative from the National Board of Health argued that it was very important that general practitioners themselves took part in the work of making specifications. Otherwise “the rules of the game would be made centrally”. But to the doctors, ‘making codes’ suggested working for the National Board of Health without reimbursement, and was consequently not seen as very attractive.
During the discussion of how GPs could be motivated to use IT more intensively, a number of factors “external” to the issue of IT came to the fore; such as the relationship between GPs, The National Health Insurance and the National Board of Health. In this way, historically ingrained organisational and political issues set restraints that were not easily compatible with visions of future intensification of IT-use. No unitary conceptualisation seemed to exist in the working group even though all participants were “for more IT”. And there was a serious divergence between GP’s view of their own work and its perception by other participants.

6. The Doctor Translated

At this point a number of GPs were invited to participate in a workshop to further the discussion on perceived “needs”. Approximately 30 GPs took part in a computer-mediated discussion session, which focused on a number of topics, one of which was codes. In the subsequent evaluation of the workshop by the working group, the primary focus was on the scepticism doctors showed toward the use of these. They had argued that the use of codes would lower the level of service, since the time spent on coding would be taken from the patients. Accordingly they would have little purpose other than enabling local authorities and the National Board of Health to survey GPs’ activities more thoroughly. Coding was not part of clinical work, which was understood by GPs as consisting in the personal interaction between the doctor and individual patients. However, the rest of the participants in the Digital Doctor project wanted the doctors to be interested in specifications. In their view standardisation of work routines and the use of codes could easily be seen as clinical work.

Apart from diverging conceptions of what constituted clinical work, at stake was also the image of the GP. To GPs it was crucial to keep the image of a doctor as primarily a referring actor; that is, as a generalist, who passes the patient on further into the health care system. According to the doctors the intensification of the use of IT should not imply a change of that role. To other participants precisely such change was crucial. However, to both doctors and “non-doctors” in the project group, an appealing image of the general practitioner was that of a ‘spider in a web’.

Behind this commonality was a difference in emphasis. Doctors thought of the spider as a more or less autonomous actor, while non-doctors focused on the web and on...
the spider’s ability to gather information. The latter saw the G.P. as an information manager rather than a gate-keeper; they envisioned a doctor who would collect the necessary information by means of his access to e.g. clinical data-bases. In this view diagnostic coding became crucial because the doctor could only take up the role of information manager if he was willing to provide those who maintained the databases with accurate and complete information of performances in the clinic.

At the end of the project, a report containing ‘visionary essays’ on the future use of IT in primary health care was written by the participants (Teknologirådet 2000). The recommendations were eventually presented to the government’s health committee and sent by mail to all members of the GPs’ association. In these recommendations, the present situation in general practice was presented as characterised by a lack of quality service and the best treatment possible, and it was argued that the right documents were often not available to involved health care providers at the right time. The solution to this predicament would be a coherent use of IT in the health care sector. Coherence and integration of various systems would ensure faster and more flexible access to the necessary information when it was needed. Furthermore it was argued that:

If the primary care sector does not succeed in seizing at these new opportunities, it will become increasingly difficult for general practice to keep its position as the supplier of advice and knowledge to the Danish population (ibid. pg. 4)

As in HVEPS a number of translations happened during the course of the Digital Doctor project. A political moment arose as questions of doctor identity, the value of the present organisation of doctors’ work, and autonomy and integration between sectors were brought up. From carrying the image of a gate-keeper, whose central tasks were face-to-face conversation with patients, and the referral of patients to proper treatment (and preclude some of them from specialist-shopping), the image of the general practitioner changed into that of an information manager. Within the horizon of this new image the doctor became somebody who would use IT to pick up specialist knowledge and treat more patients himself.

In contrast a moralising moment took place in a second translation, which changed the initial “needs” – those of the general practitioner – into the needs of patients. If doctors did not “seize” the new opportunities, it was argued, then patients would have to abandon them as they refused to live up to their duties as health suppliers. Thus, at the
moment when the gap between the participants in the working group became too wide, “needs” were translated from referring to doctors to referring to patients. A moralising moment arose when differences and conflicts were glossed over and the focus of the debate was implicitly shifted from GPs’ to patients’ needs. All that remained of this disagreement was a note at the beginning of the recommendations stating that “the members of the study group do not agree on all viewpoints presented in the document” (ibid. pg.3).

7. Reports and Recommendations as Material Agents

We know that these exchanges only take place by way of the language and the text, in the infrastructural sense that we now give to this word. And what we call production is necessarily a text, the system of writing and of a reading, which we know is ordered around its own blind spot. (Derrida 1976:164)

Both of the sites here visited constructed texts: evaluations, reports, recommendations, and information leaflets. In conclusion I want to discuss the particularity of these textual actors; how they can be seen as instrumental in framing the understanding of IT-use in the Danish health services. In both HVEPS and the Digital Doctor project, the “opportunities” of IT remained unexplained, as the mantra became “technological possibilities exist”. In the Digital Doctor project this meant that there was no need to examine specific intersections between IT and local work practices before visions were announced. In HVEPS, those intersections were examined during the pilot project but no one attempted to evaluate the problems encountered and its consequences for a larger scale development and implementation process. In the report, such issues are glossed over with the suggestion that, in spite of setbacks, “possibilities are enormous”.

In both cases, present practices were suddenly considered old-fashioned while responsibility for accomplishing change was delegated to local work practices. The outcry “technological possibilities exist!” placed the responsibility for attaining successful change at local wards and clinics, while technologies seemingly could not be faulted.

Viewing reports and recommendations as a specific ‘genre’ of material actors matters when trying to understand how they are granted agency in spite of the contradictions of many of their arguments. The HVEPS report and the Digital Doctor
recommendations were both characterised by tensions as they struggled to simultaneously contain heterogeneity and glossing it over. I do not think this is a matter of ‘performative contradiction’. Rather it points towards the report/recommendation as an actor whose aim is to bind together diverse practices and opinions through vague formulations and visionary signals. I suggest that the ability to carry contradictory meanings is an important feature of these types of reports, and argue that this sort of writing is interesting precisely because it attempts to negotiate the high expectations of information technologies with their often disappointing performances in practice.

For instance, the recommendations resulting from the Digital Doctor project allowed for some degree of alignment between the National Board of Health, the National Health Insurance, and the Danish general practitioner’s association through a visionary (if modest and compromised) account of the future of the work in general practice. HVEPS attempted to connect hospital administration with an array of IT interested health care professionals, the National Board of Health and the Danish Institute of Health Services Research, by focusing on necessary user needs. Discerning these necessary needs was conceptualised as crucial in order to get an EPR to work in the hospital locally, and in Danish health care more generally. These scenarios both indicate how reports can be fruitfully viewed as negotiation devices whose agency allows otherwise incompatible viewpoints of professional practices to co-exist, or even align.

If a primary function of reports and recommendations is to work as negotiation devices, then their textual flexibility should be interpreted as an asset rather than as a liability, at least to the extent that this is empirically is the case (I discuss these limits in chapter eleven). Indeed, the construction of such political inscriptions would be one important way in which the relationship between Danish primary health care and the use of information technologies were stabilised.21

In these discussions I have characterised specific events as political and moralising moments. The notion of political moments has been used to designate points and times at which important concepts or ideas moved into different domains, and was transformed in important ways. I have termed moralising moments those through which “official” discourse presents its approaches, hopes and visions in a seemingly neutral voice, which is linked only to the ‘enormous technological possibilities’ of new information technologies. Both cases have offered examples of both kinds of moments, and this
would be true in practically all cases. For this reason it would make no sense to oppose or criticise the work done by the groups examined for being misguided, or the reports they have prepared for being merely rhetorical. However, looking for political and moralising moments is a good guideline to go by, if one is trying to make sense of changing organisational practices. For they help to keep a focus on the minutiae of work processes, when it is all too easy to concentrate on grand mission and vision statements.

Concentrating on political moments means concentrating on practice, and the practical work of getting IT to work. A focus on political moments can help to see how accountability for making the IT-revolution happen is delegated. No longer just at work on official political levels, responsibility is being distributed into finer and finer capillaries of the health care system. And work is increasingly distributed to local health care workers without compensation. As mentioned, it seems unlikely that moralising moments and their rhetoric of progress and natural legitimacy will disappear. Developing an ability to find microscopic political re-definitions of responsibility and accountability in practice, however, may make more clearly visible the moralising aspects of tales of technology and progress, and incite to critical reflection on the possibilities, limits and invisibilities of emerging information infrastructures (Markussen and Olesen 2001).

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1 A theoretical discussion of the problems pertaining to micro-macro analyses is found in Chapter ten.


3 As Gilles Deleuze suggests in conversation with Michel Foucault: “…a theory is exactly like a tool box. It has nothing to do with the signifier. It has to be useful, it has to function, and not just for itself. If no one uses it, beginning with the theoretician him/herself (who then ceases to be a theoretician), then the theory is worthless and the moment inappropriate” (Foucault 1980a: 208)

4 Actant is a term introduced by ANT theorists to account for both human and nonhuman actors and thus produce symmetry in their studies. Actor and agent are used interchangeably to account for human as well as material actors, who have been granted the ability to act within a network.

5 In Political Machines, Andrew Barry invokes a similar distinction between what he terms politics and political action. For Barry, the term politics can be understood in its conventional form but it should be distinguished from the political: “This sense of politics can be contrasted with a notion of the political which I take here to be an index of the space of contestation. Thus an action is political in this latter sense to the degree that it opens up new sites and objects of contestation. And it is anti-political to the extent that it closes down the space of contestation. (Barry 2001: 194). Also in line with the argument here
developed, Barry suggests that: “what we take to be politics in the first sense is often anti-political in the second” (195).

As described in Latour 1993a.

See Deleuze (1983), see also Jensen and Selinger (2003) for an exposition.

The notion of sites of articulation follows Layne (1998). It highlights the working groups and the reports they produced as analytical contexts, where negotiation and translation take place and can be studied. This should be seen as opposed to the physically delimited site as study site, which separates material practice and discourse.

Surrealist painter Rene Magritte experimented with paintings in which the boundaries of different materialities were blurred and rendered uncertain. See Foucault 1998.

Later the group was expanded with a sociologist and a nurse from the Danish Institute for Health Services Research, see note 11 below.

The Danish Institute for Health Services Research is described as an independent non-profit research institute, although they are economically supported by the Ministry of Health.

At http://www.hep.dk.

The increasing interest in EPR systems is indicated by the fact that per September 2001, 52 EPR development projects were underway in Denmark (EPJ-Observatoriet 2001: 7). As discussed in later chapters many of these were later put on hold.

This resembles, almost literally, an interview statement made by the project manager of the EPR project in the Aarhus Region, according to which the ideas of this group in 1995 were ‘so visionary that the technology couldn’t follow us’ (see chapter six for specific discussion of the context).

In Geoffrey C. Bowker’s case politics was effaced by interpreting politically charged questions on how to standardise biodiversity data merely as a matter of improving meta-data. This is an equivalent to the Danish conceptualisation of semantic standardisation of the electronic health care record: the claim, in the face of politically charged decisions is that “no one will be missing language”.

Project aims and development can be found in English at http://www.tekno.dk/projekter/index.htm.

Translated from the Danish name PLO, Praktiserende Lægernes Organisation. PLO is an organisation under DADL (the common organisation of Danish doctors), which takes care of matters pertaining to the professional and economical interests of GPs.

We see here another surfacing of the recurring discussion of the merits of a centralised versus a decentralised health-care system. I discuss various aspects of this discussion in chapters eight and nine.

Translated from the Danish name Regeringens Sundhedsudvalg.


Through the intervention of such documents the ‘natural’ cause of action in health care is subtly shaped and re-defined. This does not imply that they are the sole agents, or that they are much more powerful than other actors. For there is, of course, no guarantee that a report or a set of recommendations will create good alliances, or even that they will be taken seriously. If the strength of reports is their ability to flexibly subsume many viewpoints under their headings, then their complementary weakness is that this makes them easy targets of abuse for actors who want to engage with strong stands. In this sense, reports are like all other actors; whether they will become effective or be abandoned cannot be determined in abstraction from the concrete alignments that they make or fail to make.

See Brown and Lightfoot (1999) for an analysis of the political capillaries of a different type of commercial setting.
Chapter five:
Reading Digital Denmark:
IT Reports as Material-Semiotic Actors

1. Introduction

During the last decades, numerous prophecies have been made about the changes that increased use of IT in most of the Western world might occasion. In Denmark this has happened in various arenas, but prominently through pronouncements in a number of public reports, which have aimed at uncovering the positive potential of IT, but have also warned about different potentially inexpedient implications of the increased use of such technologies. These reports have varied widely as regards focus and content; from investigations of specific practices, to very general discussions about the possibilities of developing the country as a highly networked IT-nation. The most well-known of these broadly focused reports is from 1999 and is called Digital Denmark – Conversion to the Network Society.

In this chapter, I discuss two ways of relating to, reading and understanding, reports such as Digital Denmark. To do so I make a detour to what can be thought of as a central and basic discussion of the relation between a text and its environment (Bloomfield and Vurdubakis 1994, de Laet 2002). The first way of relating to texts is critical and sceptical. It aims to uncover hidden, forgotten, or repressed meanings and make such underlying themes explicit. I call this broadly defined approach ‘reading against the text’. In contemporary cultural theory the notion of text has expanded so that (almost) everything can be read ‘as text’, thus indicating that strategies of reading against texts clearly open up a broad field of cultural investigation. I illustrate this way of reading with a brief discussion of the way education and learning is related to IT in Digital Denmark.

The second approach is inspired by STS-studies (Science, Technology and Society). From this perspective it becomes less interesting how to interpret and criticise a text (or any other entity) but much more interesting to see where it goes and what it does. I call this approach ‘reading with the text’. From this perspective, texts, if successful, are always on the move, and in that movement they overflow their textual functions. They may therefore advantageously be read as material-semiotic actors, having effects on their

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1 Co-written with Peter Lauritsen. I refer to "I" throughout this edited version of the text to retain stylistic consistency.
environment, which exceed or bypass discussions of content and intentionality. This line of thought is exemplified by a reading, which shows how Digital Denmark’s framing of education and learning in the network society is at most partially connected with specific activities. I illustrate such connection by relating the report to a number of local initiatives under the Digital North Denmark project.

The STS-reading is neither about criticising public IT-reports for their narrow definitions or simple-minded goals, although this is certainly possible, nor is it a matter of praising them for their visions. Contrarily, travelling with the Digital Denmark report makes visible some limitations of both critical and visionary approaches. An STS-inspired approach to reading reports does not render an understanding of content superfluous. But it does allow one to suspend narrow concerns with textuality and meaning, and add more agency to the text as it moves in practices.

I continue the argument in the following order. In section two, I discuss the relationality of texts and specifically relate this idea to Donna Haraway’s notion of material-semiotic actors. In section three I present the general argument of the Digital Denmark report, while section four briefly indicates the difference in interpretive effects of reading with or against the text. Section five schematically outlines the shape of a critical reading of the visions of learning presented in the report and section six delineates and illustrates a strategy for reading the text as a material-semiotic actor, rather than a vehicle for intentions and ideologies. In section seven I sum up the results of this alternative mode of reading. Finally, in section eight, I conclude by discussing the politics of this approach and considering how it may be understood as necessitated by the changing informational politics of the contemporary political landscape in Denmark.

2. The Relationality of Texts: On Material-Semiotic Actors

The idea of ‘reading’ is far from singular. For instance, classical hermeneutics occupied itself with the reading of biblical texts – exegesis – but later spread out to include readings of a far broader range of written expressions (Gadamer 1994, Palmer 1969). Contemporaneously, a displacement of the very idea of textuality has taken place, not least within the frame of cultural theory and deconstruction. It is now commonly acknowledged that one can talk about readings of not only written material, but also adverts, movies, the subconscious, sports activities or material artefacts (e.g. Balsamo
1996, Fiske 1987, Ortner 1991, Wolmark 1999). Here, a paraphrase of Derrida’s (1976) proposition that ‘everything is text’ is often invoked; it follows that everything can be ‘read’.4

This enormous conceptual extension can be viewed as simultaneously self-limiting and self-differentiating. Limiting, since a notion such as reading easily loses precision and consequently analytical merit, as it is used in more and more contexts. But for that reason also differentiating, since the notion of text must necessarily be worked over, re-specified and fit to each context (see Hayles 2000, also Chartier 1997, Chapters 6 and 7).

Insights from STS-studies are of value in such re-specifications. But translating the notion of reading into an STS context brings with it a displacement. As it is often used the idea of writing carries with it a connotation of an intentional subject putting down more or less transparent textual markers, which may nevertheless be deciphered by an acute reader.5 The point of analysing texts is then to break the code and find the meaning hidden beneath the surface of expressions.

From an STS point of view the assumptions concerning the relationship between texts, their readers, and their authors that informs this strategy of interpretation are problematic. Such a perspective would stress that human intentions are rarely able to imprint on, connect with, or otherwise affect the material world, while alone staying unaffected. Hence, it would suggest that the interaction between human (reader) and thing (to be read) cannot be understood on the basis of a distribution of agency, which imagines that active people decode passive things imbued with the intentions of other active people. Rather, one would become interested in elaborating how concepts such as historian of science Donna Haraway’s material-semiotic actors (Haraway 1997) can be used to characterise forms of interaction between humans and texts, in which both sets of entities are seen as active and transformational.

Below I evoke Donna Haraway’s understanding of material-semiotic actors in laying out an alternative to critical reading – reading with the text, I view it as simultaneously material and semiotic. But first I need to specify the term as well as the adoption of vocabulary from Donna Haraway, since I use it for what might seem a non-political reading of a political document, whereas Haraway often works in what seems to be) the opposite direction, unearthing the politicised dimensions of ‘non-political’ domains (In chapter twelve I will have occasion to question this easy opposition).
In the “Syntactics” section of *Modest_Witness@Second_Millennium*, Haraway tells us that: “I learned early that the imaginary and the real figure each other in concrete fact, and so I take the actual and the figural seriously as constitutive of lived material-semiotic worlds” (Haraway 1997:2). As this citation suggests, one of the most important aspects of the notion of the material-semiotic is its aim to construct a relation between the empirical and the figurative that is not simple, and non-naïve. In fact, these two “domains” are not separable at all, but always intertwined and co-constitutive, as Haraway shows in her analyses and works with in her re-figurations (such as the cyborg or the vampire).

Refiguring, among other things, is about learning to take responsibility for one’s productions, intellectual and otherwise, which Haraway elsewhere characterises as developing practices of situated knowledges (Haraway 1991: 183-203). Such practices of knowledge are alternative to those presenting themselves as neutral, a-temporal and objective, and represented by the *modest witnesses* of Haraway’s book-title.

Given that I present here an argument based on the limits of criticism, this raises the question of my own position. Do I attain to the position of modest witness in presenting a non-critical reading of what may easily be viewed as a superficial and problematic document? Evidently I do not think so. But the question relates to one of the analytical points emphasised in the text. I suggest that the capacity of contemporary theory for expanding semiotics to everything (everything is text) has had the effect of often rendering questions of materialisation seem superfluous in cultural theory. Haraway’s notion of material-semiotic activity balances this situation and, indeed, reverses it, when the material-semiotic actors one investigates actually are texts, classically conceived, because *even* a report, must be analysed also in its materiality.

When one does so, the ability to critically interpret its textual figurations becomes less central, or central only in connection with an understanding of its efficacy in different practices. This reformulates the critical potential of reading, since it now requires “an immersion in worldly material-semiotic practices, where the analysts, as well as the humans and nonhumans studied, are all at risk – morally, politically, technologically, and epistemologically” (Haraway 1997: 190).
3. Digital Denmark – Hopes of a Network Society

“I need someone to supervise me: where are we going?” With this motivation, the Danish Minister of Research appointed a two-person committee, consisting of a politician and a businessman, whose purpose was to formulate an IT vision for Denmark. At the introduction of the project, however, the commission of the group was not clear. A newspaper asked the Minister for a short explanation of the aspirations of the committee, to which he answered:

The committee means that we are getting an integrated IT-based educational network. A suggestion as to how the citizens can communicate directly with the authorities by means of IT – and follow their cases. Do you want more? This is all we have. So far.

In order to substantiate the vision of Denmark in the network society citizens and organisations were invited to contribute with their ideas and after a year of work the report *Digital Denmark – conversion to the network society* was published, in November 1999.

*Digital Denmark* is organized around a narrative with three parts. It makes a diagnosis of the current status IT in Danish society. The diagnosis makes visible a series of challenges, potentials, and problems. Coming up with solutions for how to effectively handle these problems and possibilities is the aim of the report. As part of the 'diagnosis' the report states:

Denmark is heading towards a network society in which incredibly rapid technological development makes it difficult for even the best technicians to see as little as three years ahead. A development in which globalisation is breaking down the boundaries standing in the way of inter-human communication, and in which the digital economy with e-trade and new electronic procedures is changing our views as to how money can be earned.

The challenge follows:

In short, Digital Denmark is about the ways in which Denmark can become an influential IT nation in the network society, while we still uphold the best values of our welfare society.

It took a year to write up the report but "one year is a very long respite in the age of the Internet" (6). Paradoxically, however, a year is also much too little time to be able to survey, much less think through, all the consequences this new age could bring with it. This is especially problematic insofar as IT is expected to intervene in every imaginable sort of practice, from nurseries, to administration of the environment, to surgery. Hence,
there is no naturally delimited focus for the report; the challenge is spread across the entire socius.

Consequently it was left to the choice of the authors to decide on five themes with matching challenges, which they found particularly pertinent. They proposed that the digital economy had a growth potential of up to 50% percent of the total BNP. Thus, the first challenge would be to get Danish businesses quickly established on the net. New technologies were supposed to make possible the tailoring of products to individual consumers, and offer cheaper goods to be purchased from the internet. The same technologies, however, could make possible unprecedented levels of surveillance. So the second challenge would be to establish a high level of consumer information and prevent technological encroachments on personal freedom. Citizens spend increasing amounts of time on the internet, on shopping, work and entertainment, and there would be a third challenge in shaping these technologies to enhance the quality of everyday life. This would also be dependent on the development of abilities such as creative thinking, personal engagement and becoming able to filter large amounts of information. A fourth challenge, then, would be to offer life-long learning to citizens.

Finally, the writers worry that the globalising tendencies of the internet may strengthen very close and very distant communities, at the peril of the national community. The fifth challenge will therefore be to improve the ability of educational institutions to convey common Danish values that ensure the solidarity of society. Thus, the report presents itself as simultaneously embracing the new technologies with all their possibilities, while also being the guardsman of traditional (social-democratic) values underpinning a crumbling Danish welfare society threatened by rampant neo-liberalism.8

4. Reading Digital Denmark

How does one read a 'boring’ text such as Digital Denmark? A first and obvious idea would be to read it critically. One could point out, for example, that the report builds on various assumptions about technologies, learning, or education, which becomes problematic to sustain if carefully reviewed from the point of view of a variety of perspectives in cultural and social theory. Roughly, its dispositions could be characterised as technocratic and deterministic. As already mentioned I term such readings, readings “against the text”.

"Reading Digital Denmark" 79
In doing so I argue specifically with recent developments in cultural theory, not least as inspired by deconstruction, which, surprisingly, given this inspiration, often retain a highly concretistic notion of what counts as a text to analyse or deconstruct. In spite of the deconstructive effort to expand the notion of text to everything, texts are still regularly read critically as stand-alone – texts. The same cannot be said of all critical social research. In relation to such endeavours the productive argument of the present text is that a strategy of reading with the text makes visible a number of otherwise obscured facets of the changing information politics of the contemporary political domain. Since these changes often do not take heed of, and sometimes distinctly undermine, classical categories of social theory, then following the text as a material-semiotic actor offers a flexible tool for investigating transformational sociotechnical processes.

From an STS-perspective it is possible to read 'with' the text, by regarding it as active. From this point of view the report is one 'material-semiotic' technology among others, which 'works' by associating with and affecting heterogeneous practices, and which is transformed in the process. If this is the case, it becomes less relevant to be able to judge whether the representations and characterisations in a report are true or false, but important to show how reports ‘travel’ to different practices and establish links. Read in this way Digital Denmark becomes a quite interesting actor of sorts, one of relevance to the media, the political system and to different business- and academic practices.

In the following I approach the Digital Denmark with this ‘double vision’, as I look at the report from both a ‘representational’ and a ‘performative’ point of view (Pickering 1995). The first will allow me to take apart and make explicit some of the various assumptions on which the report is built. Here I will be especially interested in the sections concerned with learning and education. The second approach allows me to travel with the report, to a few of the many places where it has settled and taken effect; to the Digital North Denmark Project. By looking at some ways in which the ideas of learning promoted in the Digital Denmark have been transformed as they became practically embedded in local developments in Northern Jutland, I hope to make visible the limits of an approach based on textual criticism.
5. Visions of Learning in Digital Denmark

In Digital Denmark, life-long learning for all citizens is emphasised as a necessary component in converting to the network society. But how to realise this goal? The report answers in a short version:

This objective is to be met through investments in, and initiatives aimed at an adjustment of, the educational system and by ensuring special IT competence (60).

This broad goal is specified in a section developing a national strategy for life-long learning. Here it is explained, for instance, that a “strategy should be drawn up for how all Danish citizens can be ensured access to life-long learning” (51). Among other things, this includes a “radical adjustment of the content of training and education, forms of learning and methods of evaluation” (52).

Such a formulation makes it relevant to consider how this radical conversion is conceptualised in Digital Denmark. Thus, I now want to put forth an, in no way exhaustive, critical reading of the sorts of explanations the report offers as to what ‘development of IT qualifications’ are, and how they are to be ensured.

According to Digital Denmark in 1999 conversion to the network society had already begun. The problem was that this process was driven largely by ‘moving spirits’. Moving spirits are particularly enthusiastic individuals, which start up local IT-initiatives, get them going, and keep them alive. Since local developments are almost exclusively driven by such initiatives, this becomes problematic, as it increases the variation in IT development between what are arguably otherwise similar practices. The first recommendation is therefore the construction of a common national strategy for life-long learning.

Digital Denmark distinguishes between two components of life-long learning and makes several sub-recommendations related to these. There are ‘basic IT qualifications’ that are about the “need to be able to handle the most common office applications and the Internet” (60). But there are also ‘derived IT qualifications’, which refer to workers’ capabilities to continually adapt to new IT-environments. For instance, it is recommended that teachers on the first and secondary school levels get better IT support; that they are offered a home-PC deal; that all courses become IT-integrated; that an internet connection per ten pupils is made available; and that English, as the language of...
the IT-revolution, is taught from nursery class. Concerning higher education it is recommended that virtual universities should be promoted, that there should be a larger intake of IT-related students at institutions of higher learning and that a larger amount of computer scientists should be produced. Finally, the unemployed should also be offered relevant IT-courses.

One can read some broader tendencies and assumptions from the ways in which these recommendations are formulated. Foremost among these tendencies is an assumption according to which the central focus of life-long learning is IT. Recommendations such as the home-PC deal and the necessary increase in the Internet connection to pupil ratio, intimates that if there is not enough computers, not enough can be learned. But what is learning then? As the locus of Digital Denmark is technology, learning is interpreted in a narrowly concrete and technology-driven sense related to the definition of basic IT qualifications. Roughly, it is viewed as a matter of becoming proficient in the use of Microsoft’s office pack, to be able to handle programs such as Word or Excel, rather than, for example, becoming able to work flexibly with many different problem areas by means of a broad array of (IT-based) tools. Within the framework of Digital Denmark such qualifications are viewed as ‘derived IT-qualifications’, which are considered irrelevant at first, since the explicit primary goal is to meet the assumed commercial demand that everyone become capable of handling a narrow specified set of programs. At this point it becomes easy to argue that the putative ‘radical learning’, which was advertised in the previous section of the report, has disappeared: nothing is particularly new, or radical, in insisting that workers need to learn to use simple and specific types of tools in order to accommodate the wishes of employers (see e.g. du Gay and Salaman 1992, Miller and O’Leary 1994).

Thus, read critically, the implied notion of learning is certainly not unproblematic. Neither is the implied concept of technology. In Digital Denmark technology is the (partly uncontrollable) motor, whose movements Danes need to adapt to. Anthropological studies have repeatedly shown how important it is to take into consideration ‘the human component’, in itself a short-hand and quite inadequate term, and that local practices are integral parts of successful technological developments. It has been shown that it makes no sense to talk about the IT-revolution independent of questions pertaining to what differences it makes to people situated in specific practices, with specific problems at
hand (see e.g. Edwards 1998, Lave and Wenger 1991, Star 1995, Suchman 1987). It seems evident that when the IT-revolution is concretised to a degree in which specific commercial computer programs are defined as facilitating it, then one is moving quickly towards a highly peculiar notion of revolution.

The point with these comments is not that it is wrong for the Digital Denmark to focus on IT, education and learning. But if we read the report critically its way of analysing these topics seem superficial. Ensuring access to a PC with a series of office tools, and adequate IT-support, cannot guarantee that life-long learning will obtain. Contrarily, one can argue that this way of conceptualising the conditions for learning are extremely narrowing because they signal a highly inflexible idea of the types of learning a network society, whatever this might mean, could need. It can be argued that Digital Denmark is also interested in a more flexible format – derived IT qualifications. But this interest remains implicit because it is claimed that the notion can only be given content “as the corporations re-structure organisations and working routines” (60-61). Hence, the ideas presented as derived IT-qualifications suffer from precisely the opposite malady of those of basic IT-qualifications: the formulations work on a level of abstraction that makes it exceedingly unclear what their relation to any real working practices would be. Thus, there are no suggestions of what could or should be learned or how IT would facilitate any such learning.

A further point here deserves notice, which do not relate to Digital Denmark’s treatment of learning as such, but bears on the organization of the document as a whole. I refer here to the blatant tendency of the report to present its arguments in an unspecified and undifferentiated language; that is, to talk to and about what Haraway has called ‘universal man’, in a manner, which assumes, or pretends to assume, that one size does, indeed, fit all.

6. Digital Denmark as Material-Semiotic Actor

... situated knowledge is ... not a knowledge that a given group simply ‘happened’ to develop. Rather, situated knowledge is something that is produced through being located in a certain position that allows for a specifically partial perspective (Biagioli 1996: 193)

Above I attempted to read parts of the Digital Denmark report critically. Although it is important to be able to recognise and analyse the assumptions implicit in the text, this is"
not the only thing that may be done with the report; for it is possible to read it in another register as well. The discussion above was about the representations of the report. However, one can also choose to ‘travel’ with the report, to see how it performs, and in what places it finds its effective domain, where its knowledge becomes situated in a partial way. This second reading, which I carry out below, is not so much focused on the textual content of the report. Rather it is concerned with the relations that the report becomes capable of forging with various practices. From an STS-perspective this approach to reading reports is more consequential than the representational perspective discussed above.13

After publication, Digital Denmark was commented upon and analysed in the press. Some commentators were critical. For instance, a number of businessmen and analysts took notice that the report was not sufficiently visionary. Others criticised the defective understanding of technology in the report. The authors in turn defended themselves by claiming that nothing would be easier than coming up with unrealistic visions.

Even though the public debate was partly focused on the report itself, many comments also related the report to the IT-political proposal that the Danish government introduced less than two weeks after the publication of the Digital Denmark. The suggestions in this proposal were collected in the Digital Denmark action plan, but selectively, and many had been led out. This led to criticism. Business and trading people accused the government of not putting serious effort into realising the visions. The government apologised by referring to lack of funds, and countered that some of the formulated proposals were not analysed well enough to be converted into specific initiatives.

In spite of the discussion and criticism, however, the report also became a starting point for a number of activities. Most conspicuous among these was the project Digital North Denmark, which was one of the two Danish IT ‘lighthouses’ to be established. In the Digital Denmark this idea was described in the following way:

…the IT lighthouses should also function as large scale IT-based experiments directed at citizens, the training and education sector, the public administration, trade and industry and the infrastructure (97)
The idea of making an IT-lighthouse in North Denmark was realised by dedicating money to the project in the action plan being proposed shortly after the publication of the *Digital Denmark*. *Digital North Denmark* took off early in 2000. At its homepage *Digital North Denmark* is presented in a way, which is recognisable from the formulations in *Digital Denmark*.

The general object of The Digital North Denmark is to create the future networked society and try out experiments aimed at tomorrow’s IT society.\(^{14}\)

As means to reach the goal of the project it focused on:

- expanding and enhancing the technological infrastructure
- enhancing the development of qualifications
- promoting employment and the competitive power of the industrial sector
- enhancing service and efficiency within the public sector.\(^{15}\)

As is indicated in these formulations a clear connection between the *Digital Denmark* and *Digital North Denmark* can be pointed at. But this is not to say that we are witnessing a ‘top-down’ implementation of a finished product:

It should be made clear that The Digital North Denmark should be considered in context with the development already evolving in North Denmark.\(^{16}\)

This remark emphasises an important aspect of the *Digital North Denmark*; that it connects the visions from the *Digital Denmark* with already existing regional practices. These practices can now be understood as embryonic expressions of the visions of a *Digital North Denmark*, which, again, come to stand metonymically for *Digital Denmark*.

Related to the above-mentioned points of focus, *Digital North Denmark* established experiments (or projects) in four areas: 1) IT infrastructure, 2) IT industrial development, E-business and IT framework conditions for the industrial sector 3) qualification and education and 4) digital administration. Applications were called for in each of these four areas, and economic resources were distributed. Measured by number of projects, *Qualification and Education* was the largest area, with thirty-two related projects. Below I take a closer look at this particular theme, with the aim of clarifying the way competence, learning and IT is articulated in North Denmark, and how these articulations...
were related to the notions of life-long learning as propounded in the Digital Denmark report.\textsuperscript{17}

At the homepage of Digital North Denmark the following description can be found. The main theme of qualification and education is intended to enhance the development of qualifications within the region of North Denmark on IT related fields or fields where IT can contribute to the development of qualifications in general. The object is to create a permanent boost in industrial development, in the services offered by the public sector, in exercising democracy and in the individual citizen’s participation in leisure activities, culture, consumption etc. A boost in qualifications is required, covering everyone in North Denmark, - from groups possessing none or few IT qualifications, to groups holding spearheading qualifications within one or several fields\textsuperscript{18}

These rather general expressions are concretised in relation to four areas where special effort should be put.

1) IT in education – which covers the idea that educational facilities should offer proper IT-education and broadly integrate IT in their courses.

2) IT in the labour market – which covers the idea that employees need to learn more about IT. “But the development of IT qualifications is not only a question of new capabilities. Private and public workplaces are challenged to use IT in more and more areas and organise work in new ways.”

3) IT in leisure and culture – which, among other things, covers “that virtual rooms are created, where citizens can meet, exchange ideas, and show each other what they have to offer”.

4) IT as a welfare booster – which prominently covers the notion “that exposed groups are integrated into the network of the welfare society by practically experiencing the possibilities of the new technology.

A further materialisation of how qualifications, education and learning can be handled in the information or network society takes place in the thirty-two projects. But this also brings about a displacement of original ideas and intentions. The first thing to note about these projects is their very heterogeneity, even under a single heading such as Qualifications and Education. Project titles are as diverse as ‘The IT-Village Package’, ‘ICT qualification and virtual learning under the SOSU-education’, ‘PC-Art in Primary School’, ‘Environmental Control for Handicapped by Means of IT’ and ‘GIS at School’.\textsuperscript{19}
What is the relation between these mixed initiatives and the *Digital Denmark* report? It seems clear that there are some relations. Obviously one can trace a path from the report, to the IT action plan, to the IT-lighthouse in North Denmark and to each specific project.

Furthermore, many of the above-mentioned initiatives refer explicitly to high-profile ideas from *Digital Denmark*. Take the ‘*ICT qualification and virtual learning under the SOSU-education*’ project,\(^{20}\) which aims at teaching social- and health care assistants how to use modern ICTs. Their “point of departure is an “internet classroom”, a virtual room of learning, where students can develop their abilities… in using the technology”.\(^{21}\) And it is explicit about the importance of learning:

> By using the technology each student will become more active in his own learning process. In this way the project that runs for a year, will be promoting the notions and ideas behind life-long learning, which, among other things, builds on an ability and motivation to adapt to the labour market\(^{22}\)

These goals and formulations are clearly inspired by the discourse on virtual learning promoted by the *Digital Denmark*.

But it seems equally clear that the relationship is complicated. The practical initiatives do not simply take over ideas or formulations wholesale. Rather they specify and transform them. This might even be seen as a consequence of materialising the visions: they can only become more material, concrete, local and real by becoming more mundane and compromised in comparison with ‘grand ideas’.

The *ICT qualification and virtual learning under the SOSU-education* project, for instance, was quite vocal about its own potential as creating an implementation of life-long learning. It offered portable PC’s to 50 new students at Hammer Bakker School, sponsored by local regions as well as private companies. These were to be used both for teaching at the school (theory) and student trainee periods (practice). But practical exigencies quickly altered the project format. For instance a week of workshops, which were to start the semester, had to be dropped because not enough PC’s had been made available. Likewise the project experienced continual unspecified problems with the education platform Quickplace:

> The problems has not delayed the project but made it twist a bit. Instead of working full-time on the Quickplace the students had to work in Word and use the facilities available there\(^{23}\)
Thus, the Quickplace environment, specifically chosen to facilitate an integrated learning process had to be (temporarily) scrapped and exchanged with the generic Word programme, which, presumably, offers no particular incentive for life-long learning.

I am interested in how to characterise the relationship between an authoritative text and its practical efficacy. The short discussion of the ICT qualification and virtual learning under the SOSU-education project has suggested to us that while the Digital Denmark report has certainly had some sort of influence on the concrete outcome at Hammer Bakke School, it is hard to see its formulations as more than loosely related to this practice. This looseness is limiting for approaches, which focus on and criticise content. A report such as Digital Denmark works, perhaps, rather like a relay between certain administrative and political practices and a diversity of local initiatives. Below this idea is discussed in more detail.

7. Reading ‘With’ or ‘Against’ the Text

What I would like to do, however, is to reveal a positive unconscious of knowledge: a level that eludes the consciousness of the scientist and yet is part of scientific discourse, instead of disputing its validity and seeking to diminish its scientific nature (Foucault 1994: xi)

Throughout this article I have broached the problem of reading reports from a double perspective. First, reading against the text, I have approached the report critically, with the purpose of evaluating its textual value. Secondly, reading with the text, I have looked at the partial connections the report may (or may not) facilitate in an array of practices. I want to argue that the first sort of reading of Digital Denmark as a (bad) representation of reality offers only a limited understanding of the report’s function. It is not that such approaches have no value in decoding cultural expressions. For instance, they enabled me to show how the Digital Denmark report builds on problematic assumptions about learning, technologies and their relations. Hence, the report presents a rather low degree of trustworthiness, if trust is established by a large amount of ‘neutral’ facts about the referent of the report; if, in other words, the criterion of trust is representational truth.

But viewed as one material-semiotic actor among many others, the report looks different: its aim is not primarily seen to be the statement of correct, factual representations or possibilities; nor does its representational faults diminish its ability to
become effective in diverse and heterogeneous locations. How might we go about reading reports if the text is not a passive representative but an active traveller?²⁴

Rather than concentrating on the representational façade of the report, this makes me interested in its productivity (de Laet 2002, Latour 1993a, Miller and Rose 1988, Mol and Berg 1994). If numerous business, educational, administrative or medical practices are to be associated with the visions presented in Digital Denmark, then the vague and contradictory formulations on the level of the written text becomes the very strength which makes the report flexible and robust as it becomes linked with other practices (as argued in chapter four). In an ANT phrase the report facilitates 'displacement without transformation' of the 'formal' content of the report, but in a paradoxical way, because the loose quality of the text renders ambiguous the determination of whether the text has been transformed or not in its displacement. In this way the effective domain of the report depends on textual flexibility as the property, which ensures that the report can partially connect with many heterogeneous practices (Granovetter 1973, Strathern 1995). If one considers Digital Denmark as a project aimed at engaging a very heterogeneous audience this 'impressionistic' information strategy becomes understandable, a point I return to in the final section.

Reports travel between practices and doing so they may link them, partially. They establish effective domains that are only indirectly related to their formal, representational content. This indirect efficacy can be characterised as the 'positive unconscious' of reports and it is this aspect of textual efficacy, which critiques of the representational (conscious, intentional) content of a text fails to grasp.

In this example the world constructed in Digital Denmark is at first unrelated not only to the 'real Denmark’ in the abstract, but also to specific local practices. However, it does not have to remain unrelated or incommensurable. As more and more work goes into constructing links between its vision and local practices, the Digital Denmark becomes connected with a diversity of localities of which I have briefly discussed only one.

Here we have seen how the visions from Digital Denmark were appropriated but at the same time modified. They were translated in the process of becoming a material part of the Danish infrastructure, and in this way the report has worked as a partial building block in the network society.²⁵
From this point of view the idea and function of texts and textuality is displaced compared with cultural theory. Not only because not everything is 'text' – but because even texts are not merely texts. More active and surprising than that, they are possible links between practices.

8. The Politics of Reading Strategies

In much cultural theory texts generally, and reports specifically, are understood in a manner, which reads such documents carefully but do not consider with equal attention the pragmatic moves facilitated by their equivocating formulations. In such readings the textual expression is taken seriously, but the written word is viewed sceptically, as implying a lack. This lack may then be filled by a critical humanistic or social scientific analysis, which adds depth to the surface of the text. It turns out, for example, that it is built on faulty assumptions or doubtful metaphysical or political commitments. The job of the analyst is to write an explication or commentary to the text, and “radicalness” can be measured by the depth of the critique offered.

As I have shown, an STS-perspective moves in a different direction. Here, the text is viewed as a vehicle for travelling in practices. There is no depth to ascertain behind this principle; it is purely 'superficial' or immanent. The text, as other actors, cannot get beyond the fact that it can survive only by multiplying links with others in material-semiotic networks.

From critical perspectives it is easy to point to the naiveté of this reading strategy because it fails to uncover any of the motives and ideologies expected and sought for in such theories. A defensive response to this line of attack would be to suggest that the argument here presented has been developed with the purpose of offering an alternative account of the efficacy of reports in the construction of political reality, not to make a complete theory of socio-political change.

But one could go further and emphasise with Bruno Latour that it is not due to ignorance that issues, such as class or gender, are not immediately raised in this reading:

But I’d rather anticipate the objection of my (semiotic) reader…’Where is capitalism, the proletarian classes, the battle of the sexes, the struggle for the emancipation of the races, Western culture, the strategies of wicked multinational corporations, the military establishment, the devious interests of professional lobbies, the race for prestige and rewards among scientists? All these elements are social and this is what you did not show with all your texts, rhetorical tricks and technicalities!’ I agree that we saw nothing of the
kind. What I showed, however, was something much more obvious, much less far-fetched, much more pervasive than any of these traditional social actors (Latour 1987:62)

In this approach it just might turn out to be the case that classically acknowledged actors (as listed by Latour) are responsible for the problems in the emerging network society.

But starting out with such a delimited set of relevant categories renders one’s inquiry particularly vulnerable to merely finding those same categories at play in practice. Thus, the analytical strategy of reading with, rather than against texts, and suspending classical categories, rather than emphasising them, is a way of enhancing the risk and possibility of becoming surprised by reality – as advocated in the earlier citation by Donna Haraway.

In this light I would like to venture another formulation as to the effect of reading with, rather than against, Digital Denmark. In this report, like in many other recent one’s, one is presented with a package, shiny in its vagueness, of ideas - balls thrown into the air, in the hope that someone will catch them - and formulations yet unfilled with any practical or even ideal content. Quite aside from any interest in esoteric debates on reading strategies in cultural and social theory, this way of presenting and organising political argumentation seems to indicate a situation of quite radical change in comparison with the information politics of recent decades, where reports had to presented as substantial, factual, neutral and properly backed by scientific indicators.

Furthermore, contemporary reports such as Digital Denmark do not respect older categories and divisions, but (shamelessly) combine and mix the human and the technical, as when the new information technologies are openly (albeit paradoxically) viewed as supporting and facilitating the free-standing individual – universal man – in a celebratory manner that would make many a cyborg theorist pale in comparison.

If these observations hold water and we are indeed witnessing a slippage in the style of policy documents, and a change in information politics, then perhaps developing new analytical tools, whose legacies are not bound up with understanding an earlier genre of political writing and manifestation, would seem a legitimate occupation. Indeed, one central aim of developing a strategy of reading with the text is to become able to follow such processes of change in the construction of the Danish political and practical reality. As we have seen, this understanding of the report takes away some of its strength, since it
cannot be read as manifesting the ideological structures of capitalism and sexism, or other such grand figures, but this does not render it powerless. This reading of Digital Denmark suggests that texts may add power to specific networks by allowing them to connect easily and durably. As politicians and their supervisors have carefully noticed, reports may paradoxically be good allies precisely because they are so flexible, that vastly different sets of actors and agendas may be connected through them.

When a goal is simultaneously as all-encompassing and as vague as moving towards the IT-society, reports are good allies, because they are so loose and abstract in content, but for that very reason recoverable in innumerable settings. For this reason it makes sense to suggest that the agency responsible for the (painstaking) Danish conversion to the network society, not wholly human, must be located at the interface between humans and nonhumans, practices and texts.26 It remains a large job to clarify such relationships and their stakes in a manner, which could enable their creative reformulation, rather than repeating a set of well-worn critical gestures. This would be a job for a score of Haraway’s mutated modest witnesses, among which I hope to be included.

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1 As can be ascertained at the homepage of the Ministry of Science, Technology and Development there is an enormous amount of material, http://www.fsk.dk/.
2 See for instance DSI Rapport 96.05 (1996) for a discussion of the implementation of an electronic patient record at Hvidovre Hospital.
3 Published 29/11, 2000. Available at http://www.fsk.dk/.
4 Aside from Derrida’s (in-)famous statement (see Derrida 1976, but also his elaboration in Derrida 1986), Lacan’s theories of the structuration of the unconscious as a language (Lacan 2001) and the analyses of Julia Kristeva of the relation between the symbolic and semiotic levels of language (Kristeva 1984), and Roland Barthes on writing (Barthes 1990) often serve as background for such approaches.
5 This is not so often the case in studies interested in taking deconstruction seriously as an analytical strategy, but this mode of reading is regularly encountered in the work of scholars who want to deconstruct with the ultimate purpose of getting a (classically) critical purchase on what they investigate.
6 From the newspaper Information December 1st, 1998.
7 Ibid.
8 That was in 1998, however, neo-liberalism has certainly been on the increase since then with a government shift to support it.
9 Both of these aspects are verifiable in relation to the development of electronic patient records in the health sector. Many of the Danish regions started up small development projects initiated by clinicians interested in computers in the early nineties. This entrepreneurship, which was encouraged and welcomed by the Danish authorities (Sundhedsministeriet 1996: 54) later became an issue of controversy, precisely because these different initiatives were local and not co-ordinated.
‘Magical properties’ are often ascribed to the information technologies; here it is assumed that more IT = more learning. This builds on a misunderstanding of how technologies spread, and how they work as they do so (Bowker 1995, Latour 1986, Markussen and Olesen 2001a).

In feminist sociologist Leigh Star’s phrase (adapted from A. N. Whitehead) we can talk of this conceptualisation as a matter of ‘misplaced concretism’ (Star 1994).

Given the Scandinavian environment, which historically has encouraged activism and criticism, not least in the shape of feminist interventions, it may seem surprising that such universalising gestures have been allowed to pass, without much more than a slight public buzz, but contextual Danish features clarifies the situation. It is especially noteworthy that while gender politics has been a legitimate item for deliberation in the public and political spaces of other Scandinavian countries, it has not been so in Denmark, where debates on gender have come to be structured almost exclusively around the question of equal rights and opportunities. Danish feminists currently struggle to change this unfortunate situation. For instance, the question of how to do so was high on the agenda at recent meetings such as Gender Makes a Difference – Gender Studies at the Cutting Edge of Europe, in November 2002 in Copenhagen, which featured both international and local feminists and Danish women politicians.

One important reason is that an analysis based on a critical reading of the representations of the report, implicitly places the cultural theorist above the practices about which he speaks. This implies a claim to know better than the local agents what is of concern and what is not, in a setting where, qua researcher, he is externally situated (Stengers 1999). It is to assume that the authors of Digital Denmark are ‘cultural dopes’ (Garfinkel 1967). An STS-reading works against this movement.

http://thedigitalnorthdenmark.com/index.php/m/312/.

Ibid.

Ibid.

In the analysis below I discuss a project, information about which is only available from the Danish part of Digital North Denmark’s homepage. Wherever possible I quote from the English version.


See http://detdigitalenordjylland.dk/index.php/m/69/.

SOSU is short for Social- and Healthcare Assistants (Social- og Sundhedshjælper).

See http://detdigitalenordjylland.dk/index.php/m/257/.

Ibid.

See http://detdigitalenordjylland.dk/index.php/m/203/Mode=Show&id=100

For a classical critique of the representational belief in the ‘glassy essence’ of subjects and objects see Rorty 1979. The view here presented is inspired by Deleuze 1994. For an STS-angle see e.g. Berg 1996a.

For a further discussion of the construction of complicated infrastructural ecologies see Star 1989a, Bowker and Star 1999.

The formulation is inspired by Andrew Pickering: ‘The historical actions in this story…is centred neither within nature itself, nor within society, but at the interface of the social and the nonsocial, the human and the nonhuman (Pickering forthcoming: 45). Of course I am not claiming that texts are the only nonhumans involved in these transformation processes. Rather, I propose more research into the many different ways in which humans and nonhumans share and distribute agency.
Part three: Illustrations

The name ‘cyborg’ sounds evil. When Clynes first mouthed it to Kline, Kline said, ‘It sounds like a town in Denmark’ (Hacking 1998: 211)

While no towns in Denmark bear the name (however, there is a Nyborg), cyborgs are easy to find: as I will discuss in the following “Illustrations” they appear plentiful at the intersection of IT and health care, not least under the name electronic patient records. Thus, Hacking’s humorous citation is not altogether irrelevant for what follows.

These chapters move with EPR into a number of settings, which are more or less related. One could say that they follow EPRs in time and space, but it would be more correct to say that they follow how a time and a space is constructed, which the EPR could call its own. All following chapter takes seriously the notion, discussed in chapter three, that one can currently watch EPR networks gain or lose existence. They also variously engage the question of what it means for the EPR to stabilise as an entity – which activities drive the EPR towards multiplicity and incommensurability and which activities attempt to make it cohere?

Chapter six is called “The ‘Seamless Web’: Discourse Coalitions and Story-Lines in Developing the EPR”. The chapter is based in material from the EPR project in the Aarhus Region. Although it makes certain theoretical points it functions primarily as background, setting the stage for later chapters. I discuss the modular organization of the Aarhus project and emphasise that no element of the organization was given but rather had to be constructed. The terms discourse coalition and story-lines offer tools for describing how this particular construction was imagined and what was done to make it hold.

Chapter seven “An Experiment in Performative History: On the Construction of a Future-Generating Device in Danish Medical Informatics” effects what may be perceived as a zooming out. Compared with the “locality” of the Aarhus project, this chapter engages the longer-term history of Danish medical informatics. It deals with the heterogeneous backgrounds and aspirations of the members the Danish Society for Medical Informatics: moving from clinical societies and their databases, Kommunedatas
Red and Green patient administrative systems, to the FynCom experiment with EDIFACT message exchanges and the set-up of a medical informatics department at Aalborg University. Why have such initiatives become centred on an interest in developing EPRs? While answering this question, the chapter attempts to describe historical events in terms that do not naturalise the focus on just these technologies. In accomplishing this task I rely on both non-humanist historiography and deconstruction, and I argue that Rheinberger’s notion of the future-generating device can help capture important aspects of this genealogy.

Chapter eight “Standardisation and the Logic of the Differend” is on controversy both in STS and in the Danish EPR networks. The issues are standardisation and standardisation research. The question in STS has to do with how to conceptualise the possible effects of one’s studies (in this case of standardisation): do STS studies generate normative, practically relevant knowledge? If not, what then do they do? This discussion is broached by surveying standardisation controversies in the Danish and European EPR landscape, from the CEN TC-251 working group to the National Board of Health and regional hospitals. I invoke Lyotard’s logic of the differend to carry out this analysis, in a manner which pays due attention to the question of incommensurability (both in STS and the EPR field).

In the following chapter, “Citizen Projects and Consensus Building at the Danish Board of Technology: On Experiments in Democracy”, the analytical move is in the other direction: how may one deal in democratically imaginative and responsible ways with cultural heterogeneity and potential incommensurability. The chapter describes my participation as an expert in a project carried out by the Danish Board of Technology, to elicit citizen’s preferences for EPR development. Such consensus projects have been widely praised as counter-points to highly technocratic conceptions of the relationship between experts and citizens, and as I discuss, they may generate interesting and democratically relevant effects. However, I pose a number of challenges to such projects as they are currently conceived, for in this understanding their democratic results are ensured by a method, which guarantees that a consensus will be constructed. In contrast with this view I develop the notion that citizen projects are valuable rather in their risky experimentation.
Finally, I turn to “Infrastructural Fractals: Re-Visiting the Micro-Macro Distinction” in chapter ten. This chapter attempts to make sense of the infrastructural transformations described in earlier chapters by analysing them as fractal. Such an understanding, much inspired by Marilyn Strathern, would be attentive to the multiple relationships encountered, and would suspend with the need for sorting such relationships into micro versus macro, small versus large, individual versus social, or important versus unimportant. Re-figuring these scales is of importance for questions of normativity and once more I return to this discussion; this time by analysing Vicky Kirby’s alleged deconstruction of a set of Strathern’s arguments.
Chapter six:  
The ‘Seamless Web’: Discourse-Coalitions and Story-Lines  
in the EPR Project of the Aarhus Region

1. Introduction
As discussed above visionary reports present information technology (IT) as the solution to perceived service problems in the health care sectors. The technological main actor of the imagined IT-mediated health care infrastructure is the integrated electronic patient record. While smaller EPR systems have been implemented in different countries, and larger systems are currently under development in Denmark, a thoroughly integrated, functioning system remains to be seen. Nevertheless, its arrival has been regularly remarked upon, and hoped for, by designers, health-care professionals and politicians alike. But while there has been widespread agreement on the advent of the EPR, there has been disagreement along virtually every crucial axis, on where, why, and when one was needed, not to mention how it would be possible to bring it into existence.

Since 1997, the Danish National Board of Health (which is mandated by the Ministry of Health) has worked hard at promoting nationally standardised models and guidelines, to which all Danish regions and hospitals must in principle submit in their work with developing ECRs (see chapter eight). But the mandate of the National Board of Health proved for a long time practically inefficient, and their recommendations have been continually challenged and debated.

The project group organizing the development of an EPR in the Aarhus Region has been involved in these debates. It has suggested that development projects can and should be developed in a decentralised mode. To the National Board of Health this seemed an intolerable proposal because it was seen as opening the possibility that the diverse ECRs so developed might turn out to be mutually incompatible. However, from the point of view of the project group in Aarhus this was not the implication of their suggestion. Instead they reversed the causal relationship: since the project in Aarhus was one of the largest in the country, and they also assumed it would become the most successful, then all other regions would be welcome to take over their model.

Taking this reversal seriously would have implied a serious redefinition of the work of the National Board of Health. For, in this case, their work with developing nationally
workable standards for the EPR would have already been solved in practice by the project-group in Aarhus. Left to the National Board of Health would be only to make smooth ‘migration-plans’, through which the Aarhus model would become able to travel easily to other parts of the country. Unsurprisingly they were not convinced by the logic.

There have been, then, diverse views as to how an EPR should fit into the landscape of Danish health care. The National Board of Health and other actors with a stake in central development processes have been unable to consistently convince regions to follow their ideas and, consequently, the primary objective in Aarhus (and other regions) has been to make EPRs’ functional within regional borders, rather than to work on a possible national integration.4

The above positions and strategies are not naturally given from criteria of, for example, administrative, technical or medical rationalities. Nor are they neutral. The varying stances on questions of standardisation, integration, and centralisation, imply specific versions of what kind of ‘seamless web’ (Hughes 1983) the Danish health care system is and should be, and how the EPR could facilitate the goal. Since various strategies manifestly exist it becomes interesting to understand why this is so and how they become efficient.

In order to do this, I survey the organization around the development of the electronic patient record in the Aarhus Region. This project is interesting for such reasons as its size and complexity, uncommon organizational format, and large budget. Furthermore this project has been gaining in confidence on the national scene, due to its perceived success so far.

Below I describe the modular organization of the EPR-project in the Aarhus Region, and I trace some of the rationales and consequences of this type of organization. Following STS-studies (e.g. Berg 1998, Cooper and Law 1995, Lee and Hassard 1999), I think that the confusion of technical, clinical and political issues, which this analysis reveals, should be made sense of as an interactive and interdependent whole, rather than analysed as if there were natural boundaries with matching intellectual compartments (medical, technical, administrative).

I examine the organization of the development project for the Electronic Patient Record in the Aarhus Region. The following discussion is based on various policy documents and reports,5 as well as a number of semi-structured interviews. Interviews
were carried out with project managers of the EPR organization in the Aarhus Region and with participants in the development process at local hospitals. Furthermore, I have interviewed members of the National Board of Health, the Danish Board of Technology, of the Virtual Center for Health Informatics and participants in the European standardisation work on health informatics carried out under the auspices of CEN (Comité Européen de Normalisation).

I use this material in order to make a presentation of the construction and framing of a specific project in light of various ‘discourse coalitions’ (Hajer 1995). In the next section I specify this concept before moving on to a description and discussion of the EPR-project.

2. Discourse Coalitions and Story-Lines
Political theorist Maarten A. Hajer discusses the notion of discourse coalitions in the context of the environmental controversy on acid-rain. He presents it in the following way:

The transformational model of social reality then maintains that society is reproduced in this process of interaction between agents and structures that constantly adjusts, transforms, resists, or reinvents social arrangements (Hajer 1995:58)

According to Hajer, the transformation of social reality is mediated by discourse. In this view language is given a prominent role in socio-technical processes of change, as it is not merely seen as a passive medium which, more or less correctly, is able to represent the order of things, but as an active participant in the construction of new ways of thinking and doing: “Hence discourse fulfils a key role in processes of political change” (59). A particularly important discursive mechanism is the story-line, the development of which is capable of temporarily reducing the complexity of reality by bringing several distinct elements (e.g. technical, administrative, clinical, political) in line in a coherent narrative (see also Haraway 1994, Jensen 2001). For instance Hajer identifies a conservative story-line in British policy concerning acid-rain:

..in Britain acid rain had to find its place in air-pollution discourse which was fully dominated by the concerns about urban air pollution and health effects. Britain had a proud record in air pollution; Britain had prevailing Western winds which made acid rain into a foreign problem; Britain had the best scientists working on the issue (Hajer 1995: 268)
Hence, the history of British pollution, urban health, wind conditions and scientific credentials are aligned in a political story suggesting that the British are in control of acid rain.

From the point of view of the analysis of the development, implementation and use of information technologies in a medical context, the notions of discourse coalitions stabilised by various story-lines open up interesting avenues of investigation. It suggests that familiar and recurring ideas (as we have already met them) such as the needs of the patient, the quality of treatment, or the progress of technology, are not given, but discursive constructs whose histories can be empirically investigated. It denaturalises taken-for-granted assumptions about medical reality by showing how they work within a “practice of classification that has a power of constraint” (Turner 1998: 114). Since such ideas are not given, they can be investigated as effects, capable of stabilising interactions between a diverse set of actors. It is a view, then, which suggests that any number of human and material agents can in principle be made to connect, sometimes in highly surprising ways, but that this happens in historically evolving processes, which can be therefore be empirically followed and uncovered.

In the following I use the notion of discourse coalitions to disentangle how agents hitherto unrelated or loosely associated became well connected in the large development project in Aarhus. They became so connected, I argue, through the construction of a story-line, which temporarily enabled the stabilisation of a number of relationships between a set of actors that were not naturally related. I aim to illustrate this point by discussing how the modular project organization of the EPR in the Aarhus Region was constructed through the gradual strengthening of a discourse coalition with specific elements.

3. Background
In 1994 the Aarhus Region, as other Danish political institutions, was writing up a general IT-strategy. From this point onward the contours of a discourse coalition can be seen as slowly emerging around a specific story about IT, health care, and cooperation.8

One of the chapters was to discuss the future of IT in health care, and the possibilities of developing an Electronic Patient Record (EPR) in particular. Hence ‘The Communication Group’ was set up by ‘The Managing Group of the Strategy of
Informatics in Health Care’. The Communication Group was led by the Chief Medical Officer of Skejby Sygehus, the largest and most prestigious hospital in the region. It consisted of twelve standing members from mixed backgrounds. These included a G.P, a hospital nurse, a head nurse, a chief physician, a medical secretary, an IT-coordinator, a lab technician, and a head clerk. Together they represented seven hospitals in the region, as well as the regional department of economics and planning.

The group was commissioned to pronounce on fourteen points at varying levels of abstraction. On the most specific level was mentioned ‘preparation of principles, demands, and plans for references, for discharge letters, for orders and entries and for the ‘evaluation of possibility for using portable technologies’. Broader questions involved ‘evaluation of ethical consequences of new systems’ and ‘elucidation of education/implementation’. To be discussed was also ‘identification of interested parties’ as well as ‘elucidation of economical demands to new systems’, of ‘the interaction between hospitals and the primary sector’, and of ‘administrative demands’. The resulting report came out in June 1995. The EPR was defined in the following way:

An Electronic Patient Record is one or more databases that, by means of flexible software, offers the possibility of storing, finding, and displaying patient data on an electronic media. In the long run EPR replaces current paper-based journals, notes, and the nurse’s record (Kommunikationsgruppens Rapport 1995)

To achieve this goal the EPR should be made able to access all patient data from more than one place at a time, to assist clinical decision-making, to support clinical and legislative documentation, to automatically write and sort discharge letters, and to ensure quality, better education, aid in literature search and to save physical space.

At this point, however, the story-line thus formulated by the background group did not fit well into the agendas of regional and national politics, and the publication of the report had little impact. According to the managing director ‘the report went into a desk drawer’ because ‘we were so visionary that the technology couldn’t follow us’. However, a few years later this view had changed as part of the more general trend of the mid-nineties, in which IT came increasingly to be seen as capable of beneficially transforming and enhancing institutions and organizations. Thus, the EPR came to be re-defined as a crucial actor in improving the quality and ensuring the efficiency of the health care sector (see chapter seven).
Consequently, the aforementioned IT-strategy was dug up and a proposal was made to the Hospital Management Council (consisting of members from hospital management at each of the regional hospitals), which resulted in a unanimous political decision to re-open the topic of the EPR, by setting up a managing group. The managing director of the communication group from 1995 were re-appointed and the fifteen people in the managing group set to work to find out “what an EPR should be able to do, but the visions [from the 1995 report] remained the same”.

On an abstract level such “sameness” is easy to suggest. From the point of view here adopted such sameness is not given, and this renders pertinent the question of how it was established. How did it become possible to transform the general idea of an EPR (from 1995) in such a way that it seemed realisable to the varied set of actors who would be needed to actually realise it?

This is an empirical question and in the rest of the paper I survey the initiatives taken to insure the strength and viability of the project, and discuss how they shaped the organization of the development project.

With Hajer’s discussion of discourse coalitions in mind I look for the emerging story-line of the project; one which would be flexible enough to keep a heterogeneous set of actors in place, and therefore enable the suggestion of the self-sameness of the project, as perceived by the managing director.

4. Organizing the EPR

4.1 A Modular Organization

How to organize the EPR from scratch? In principle this could be done in innumerable ways, but from an STS point of view, what is interesting is to follow the actual path of a system. In 1998 all that existed was an EPR managing group and a certain political goodwill. Less than a year later, in 1999, the group published a report called *The Electronic Patient Record in the Aarhus Region*, with detailed suggestions for a development project based on several modules. This report won general support on the regional political level, which consequently granted an initial 50M Danish Crowns for the project.

I now want to look at where the modules ‘came from’, and what consequences their arrival had. This is not clear from the report itself, since it merely states in the
introduction that “In connection with the description of the functional demands of the EPR seen from a clinical point of view, the project group has prepared suggestions for the following modules…” (EPJ-Projektgruppen 1999).

According to the managing director, the specific modules were decided upon at one of the group’s early meetings, and in the following I delineate the decision-rationales as he formulated them. In our interview was produced three selective principles for deciding on particular modules; two explicit and one more implicit. First and foremost, the managing director talked about clinical expertise:

PL: So it is your experience as a doctor.
BJ: As a clinician.
PL: As a clinician..that made the modules the way they are?
BJ: Along with the rest of the group…in discussion with the rest

But clinical rationality was not the sole criterion and a second reason was given by administrative concerns with efficiency: “It had to have several modules, to be built of modules. I do not want to be dependent on a single vendor, it is as simple as that, these are experiences you get when you are involved in these things”. Thus, it was seen as necessary to split the project into modules, each with its own software vendor, and this was furthermore seen as a method to ensure the efficiency of the project, rather than its complication. This way of conceptualising the project also made it possible to integrate the project in a story-line concerned with the development of a “tool for the clinician” rather than a “playground for software development”. Thus modules were chosen according to medical and organizational needs. But picking ‘the right modules’ was also viewed as a matter of direct political maneuvering:

Some of them are set. The Aarhus Region is so keen on booking that we can’t carry through without a booking module. Another thing is a medication module. That is what people can understand, it is where they get more security. Aside from these, what else do we need?

Thus, political, administrative, and medical concerns were all integrated in the modular organization of the developing project.

4.2 Auctioning Modules

Through this decision procedure eight modules were chosen. Notes, medication, order-entry, booking, and graphics were seen by the project group as clinically oriented. A data
module group was needed to program the ‘integration machinery’, which would allow the systems to communicate. Finally groups were needed to take care of legal and security issues, and to define the common user interface. How were these groups to be put together? The project-group made an eclectic decision to auction the modules. Auctioning was seen as a way to solve the problem of how to engage a large and distributed group of health care professionals in the project. It was considered a tool, which would localise people willing to put an extra effort into the project, identified by the project group as moving spirits. Such mobilisation was viewed as necessary if the project was to have a reasonable chance of surviving its transformation from the world of ideas to the world of reality. Hence, interested wards were encouraged to ‘bid’ to become the home of a module (aside from the integration and legal groups). This procedure was successful, and all but one module were ‘sold’ easily, some even in competition.

4.3 Project Infrastructure
Having geographically and institutionally placed the modules did not imply that a specific ward would now have a monopoly on the development of a given module. This was ensured by two sorts of organizational initiatives, which could be termed infra- and extra-modular.

The first was associated with the sense of the project group that it was necessary to bring more and diverse people in contact with the development of each module. They related this to the notorious difficulty of getting ‘foreign’ systems implemented on hospitals. In the words of the managing director

development is an important part but implementation is even harder. To have something implemented which you have not had any influence on can, at least in the hospital world, be very difficult

The organizational answer to this problem was complex. First of all, each module had to include a mixture of professional groups typically from three different wards and hospitals. For instance, in the case of the notes-module, gynecology-obstetrics at Skejby Hospital were made to co-operate with endocrinology at Aarhus Regional Hospital, as well as with a ward from the Psychiatric Hospital. Participants from these wards met in a primary working group, but work was also carried out in working groups locally at each ward. The primary working group referred to the module project management, which
included one member from the local hospital management, to ‘ensure management anchoring’. Wards whose employees participated in the working groups were compensated, so that active engagement in the development process would not be punished by extra work pressure.

Whereas these activities all had to do with the internal co-ordination of the modules, others could be called extra-modular. The first of these tasks was to find and hire software-vendors. As mentioned, this was done separately in each module, since having several suppliers was seen as strengthening the flexibility and autonomy of the project. These were explicitly hired “to sit on each other’s laps”; a co-operative obligation, which was written into the contracts. Nevertheless, from early on the project group described the work to make software companies co-operate to a satisfying extent as one of the toughest challenges of the project.9

There were also numerous extra-modular activities to ensure co-ordination, mutual communication, and preferably consensus, among the many modules and management. Local and primary working groups met regularly, while meetings between modular project management and software developers took place roughly every two weeks, as did meetings between all modular project management groups and the central managing groups. A number of POC (Proof of Concept) sessions have taken place at Skejby Hospital, where the module developers have presented their developments and discussed the prospects, possibilities and problems of the project in its current state with interested health care professionals. These have drawn audiences of several hundred people.

The development project was on schedule during its initial phases but unforeseeable delays appeared, as tends to happen in complicated organizational and technical systems. While the initial plan was to have the system ready for implementation in the beginning of 2003, this date has now been changed to 2004, and this is assuming that the system meets the technical requirements in a period of thorough testing. Initial such tests indicated serious performance problems as the modules were integrated, and everything now suggests that further delays will impede the implementation process.

However, regardless of whether these delays will turn out to have serious consequences for the long-term success of the project, they were viewed as constituting a key-phase from early on, and were consequently looked forward to with a degree of anxiety. In the words of the managing director: “you use a huge amount of money,
without getting anything in return for a long period… you really do not know what’s going to happen”.

My early interviewees expected that the implementation phase would be even more complicated than development, and would take much preparation and hard work. As we shall see this expectation has been fully borne out (chapter eleven). With these difficulties in mind, the implementation group prepared a number of initiatives, such as mapping the IT-competencies of all hospital workers by means of a questionnaire survey and preparing IT-educational plans for different levels of skill. While the project budget at the early stages of development was 100M Danish Crowns, this sum was expected to double through the implementation and education process.

5. Discussion
I have described several aspects of the development process of the in the Aarhus Region. While I have focused specifically on organizational issues, I have tried to clarify how these have not only to do with formal organization, but also with regional and national politics, ideas of the clinical expertise of various professional groups, as well as expectations of the capabilities of technologies at different moments in time. This exemplifies an STS-point: that projects are never pursued due to pure logics, such as administration or clinical rationality. Rather a project may be seen as well-functioning when it is able to align and stabilise many different interests in a coalition. In the Aarhus project such a coalition has formed around a story-line including at least the following elements:

1) Cooperation and co-ordination. It was seen as essential to bring in as many and diverse representatives of future users as possible, and draw on their ideas and expectations as much as possible. Hence the auction, and the complex formation of the project organization.

2) Management. Nevertheless, in this imagination it was viewed as especially important to have strong ‘management anchoring’, otherwise the project would run aground (as has so often been seen), due to too much local politics, and too little efficiency. This also had to do with the relation to IT-vendors: these should be kept weak (by using many), to prevent the project from becoming a playground for developers, rather than a tool for clinicians.
3) Finally, technology has been seen as transparent and unproblematic: although the project group was too visionary with respect to the technology of 1994 (and perhaps it still is), this is never perceived as a problem, since technological progress is expected to always surpass the expectations of the project group. Thus decisions as to which kinds of hardware and software should be used have been deferred as long as possible, since choosing early is seen as necessarily choosing prematurely.

This latter view co-exists uneasily with an organizational perspective, which would stress the complexity of successful integration into the everyday work practices of hospital workers, and consequently the necessity of sustained and thorough discussions of the organizational implications of different models of implementation (see chapter eleven).

The successful stitching together of these elements in a story-line may allow a project to appear as a ‘seamless web’ from the outside. But the continual work of connecting such elements in practice and thereby maintaining the interest of the many actors in a coalition is often overlooked in stories in medical informatics and management.

If the practical work of keeping a coalition in place remains invisible it becomes easy to imagine ideal ways of planning large socio-technical projects, like the development of an integrated ECR. If, on the other hand, one focuses on the arduous work to keep many kinds of actors in place in a project, it becomes easier to understand the problems that regularly haunt large technical projects. In the current situation it is plausible to foresee further complications, not merely in relation to the development and implementation of the Aarhus project but, especially, as regards the assumption that if the project in Aarhus would just work, it could be easily made to “migrate” to other regions.\(^1\)

Acknowledgment: An earlier version of the paper was presented at the “IT in Health Care – Sociotechnical Approaches Conference” at Erasmus University, Rotterdam, The Netherlands, September 2001. Thank you to anonymous reviewers from Methods of Information in Medicine for multiple helpful suggestions.

\(^1\) For instance, in a press release from the Ministry of Health on the April 19\(^{th}\) 2001, the Danish minister of health stated that: “I have said that my goal is that 40-50 percent of patient records are electronically stored before the end of 2003. It is an ambitious goal and it is still my goal…” (see http://www.e-tanken.dk/sundhedssektoren/sagt_on_patientjournaler.asp#.Modvilje%20mod%20amters%20IT-strategi).

\(^2\) These disagreements have been tempered between 2001, where controversy about which kind of development project was to take place in Denmark, and how it was to be organised, was full-fledged, and 2003, where the semantic model of the Danish Board of Health is increasingly acknowledged as a common standard.
The distinction between decentralised and centralised development has been important in the controversy. Centralised development has been promoted by the National Board of Health, which has worked to develop common standards that all regional projects must adhere to, to ensure inter-regional communication capabilities between hospitals. For various political and practical reasons this ideal has been criticised by some regions. They have argued that enforcing common standards is a return to the times of the “state-monopolies” in Denmark, and that the strategy is furthermore dangerous, since no alternatives to the systems would be available, if the standards proved useless or inefficient (see chapter seven and eight). From mid-2002 a sort of middle-road have been steered, in which smaller regions have put their projects on hold, while the Aarhus and Copenhagen projects function as two technically and organisationally different experiments in implementing systems that follow the semantic standards from the National Board of Health.

It has been suggested by the EPR-observatory that a detailed construction of “migration-plans” would be necessary to ensure the successful dissemination of development model, even if it comes to function properly within a given region. As yet the National Board of Health has not responded to this suggestion.

This type of study is facilitated by documentation on the development process found on the homepage of the Aarhus Region (see www.aaa.dk/epj).

Most of these interviews were conducted with Peter Lauritsen.

While I find the notion and idea of discourse coalitions crystallising around temporary story-lines fruitful as an analytical tool, there are problems; the most severe one has to do with the duality Hajer invokes between agent and structure. Duality talk is classical in sociological theory and Giddens, among others, have made it into key-elements of his theorising (e.g. Giddens 1981, Alexander et. al (eds.) 1987). In chapter ten I lay out an alternative fractal understanding of social theory. Here I would suggest that the importance of the notion of discourse coalitions is precisely that it allows one to bypass the question of the duality of structure and agency rather than “connect” the two. This, clearly, would have been the Foucaultian move, as would it be the move of STS-theorists such as Michel Callon, Bruno Latour, Andrew Pickering or Donna Haraway (Callon and Latour 1981, Pickering 1990, 1995a, Haraway 1997).

For historical detail see chapter seven.

In the most important instance of disagreement Ementor Danmark A/S brought the Aarhus Region to court for misconduct in its handling of the call for tenders of the modules to be developed (http://www.kflu.dk/index.htm, 2002 cases). The Aarhus Region was fined DK 150,000 (app. 21,000 Euro) and its contracts were declared invalid, but were nevertheless never re-negotiated.

This part of the financing, however, is to be taken from the budgets of individual hospitals rather than paid by the region. This issue is also discussed in chapter eleven.

The concluding formulation, written early in my project foresaw “further complications”. As we shall see in later chapters these have indeed arisen.
Chapter seven:
An Experiment in Performative History:
The EPR as Future-Generating Device

1. Introduction
For the way in which our historian considers history is conducive to the creation of something new, to the creation of connections which have not been taken seriously, viewpoints one would not have considered, problems one had not noticed. In short, a richer and even more complicated past. Perhaps not more ”objective” but certainly more interesting (Stengers 1999:17)

Philosopher of science Isabelle Stengers points to the possibility of developing new modes of historical writing. As in her other work she relates the quality of such writing to its capacity for arousing interest rather than for objectifying the practices under description. However, for Stengers interest is not a word that is used idly, and it certainly does not connote superficiality or subjective bias against methodical rigour. Stengers formulation thus poses a historiographical challenge, as it forces one to consider alternatives to the usual way histories have been written. Evidently this is not an issue, which calls for a simple solution but rather for an investigation of issues and possibilities. In the following I explore some of these by experimenting with two sets of theoretical positions, non-humanist STS-studies and deconstruction. In different ways both of these have posed radical challenges to classical historiography, but their insights have very rarely been tested with each other, as I aim to do in what follows.

The empirical focus of the paper is Danish medical informatics. A partial aim of the paper is to re-describe the development of this interdisciplinary field over the past several years. The field seems by now fairly well established, having entered the domain of public interest with the government publication, Info-Society 2000, in 1995.¹ This report signalled a heightened political engagement in the construction of an up-to-date and competitive information infrastructure. In the health sector this came to revolve around a commitment to develop a standardised electronic patient record.

Denmark is a small country and so, accordingly, is the medical informatics community. My topic is therefore relatively circumscribed, as the Danish Society of

“An Experiment in Performative History”
Medical Data Processing was formed in 1966 and the ’disciplinary population’ of my interest ranges from 200-400 members. It might seem that writing this history would be a relatively simple matter. This presupposition furthermore seems to have some empirical support, as Danish practitioners often claim that medical informatics has been strongly related to the development of electronic patient records. Indeed, sometimes they go so far as to equate the discipline as a whole with the current processes of EPR development:

The health care sector ought to use the continued development of the electronic media, to enable better service for the patient and a higher level of efficiency. The final vision is directed towards the electronic collection of all data, and their use as information sources in the electronic patient record (IT-Strategy from the Aarhus Region 1995: 49)

A high level of health care quality is crucial for the health care sector, and the quality of tests, treatment, and care must be optimal. A systematic development of the quality of health care activities can today hardly be managed without the use of electronic patient records (EPR) and clinical databases. The EPR will also ensure quality improvements, for instance, by lowering errors in medication. (National Strategy for IT in the Health Care Sector 2000-2002: 9)

While both of these statements identify EPR’s as crucial for improving several aspects of health care, a marked shift occurred between 1995 and late 1999. While the first stresses the EPR as an ultimate goal, the second emphasises the likely inability of the health care sector to improve at all if EPR’s are not central to its strategic considerations.

The history of this change is of some interest, but telling it would not qualify the present text as experimental. The experiment rather consists in processing this history through a combination of non-humanist historiography (notably ideas developed by Donna Haraway, Bruno Latour, and Andrew Pickering) and deconstruction. Experimental success can be said to obtain to the extent that the result enriches the understanding of empirical and historiographical questions, as they may be formulated from these positions.

Both non-humanist STS and deconstruction moves one away from the classical historiographic view of historical writing as aiming for correct representation of reality. Rather, both consider history as performative; but the way in which they do so is quite different. Below I adopt and transform Hans-Jörg Rheinberger’s notion of future-generating devices (Rheinberger 1994: 65) as a central concept which draws on the strengths of both sets of positions.
If language and things are all construed in their performativity, this includes the historian as part of the field. In a performative idiom, this realisation does not constitute a reflexive loop. Rather it suggests a re-thinking of what historians are doing when they write history. Quite varied suggestions for how to deal with this situation emerges from non-humanist STS and deconstruction, and a final consideration deals with how to tackle this problem.

There is a seeming gap between the provinciality of the case-study and the grandeur of its theoretical overlay, but rather than a deficiency this can be viewed as a strength. First, it allows me to show how deconstruction is not only a valuable analytical tool in highly esoteric contexts, but can also be used to elucidate quite mundane socio-technical occurrences of relevance for science studies. But hopefully this clarification also operates a “verfremdungs-effekt”, whereby the case is “de-banalised”, and offers insights that render the case of more general interest.

After a short introduction, I initiate a comparative discussion of the Latourian black-box and Gayatri Spivak’s notion of the “crypt”. This leads me to develop Rheinberger’s notion of future-generating device, as a concept capable of tapping into the strengths of both non-humanist STS and deconstruction.

2. Non-Humanist STS and Deconstruction
Using a deconstructive term, studies informed by different aspects of post-structuralism and STS-studies point to various degrees of *undecidability*[^2], which must be dealt with in interpreting and writing history. In some, perhaps more humanist-oriented formulations, the question that arises is one of perspective. In this case work is done to make explicit agencies that operate behind historical representation. One may work, for instance, to make visible the framing of a given historical narrative and show how it is complicit, rather than neutral, with respect to the problems it thematises (e.g. Harding 1986, Merchant 1983). Showing how putatively neutral historical narratives are perspectival, can offer a powerful means for telling histories differently, as feminist studies have repeatedly demonstrated (e.g. Alpers 1982, Hubbard 1990, Keller 1984).

Non-humanist STS-studies have similar aspirations, but try to reconstruct history by focusing on the pragmatic, ongoing action which makes history. By closely following

[^2]: For a discussion of this concept, see Derrida’s “An Experiment in Performativity”.^1^
empirical occurrences, STS-scholars have realised that historical action is often situated not in the ideality of discourse as much as in the engagement of humans with a multiplicity of machines, apparatuses, techniques and disciplines (see e.g. Bowker 1994, Hutchins 1995, Latour 1995, Lynch 1995), in what can be called fields of cyborgian action (following e.g. Haraway 1997, Pickering 1995).

Deconstruction as a historical method takes a somewhat different route towards the destabilisation of historical grand narratives, which has had little influence in STS, probably influenced by the disciplinary fact that it is often taught in literary studies, and is superficially known for the much maligned suggestion that “everything is text”. With some justification STS-ethnographers and sociologists have worried about the unduly representational focus of literary and cultural studies of science and technology (Pickering 1995a). But the argument is hardly that everything should be viewed as if it was a written expression. Rather, Derrida suggested that everything can be viewed as textual “in the infrastructural sense that we now give to this word” (Derrida 1976: 154). Materially interested STS-researchers need therefore not imagine the position as stating merely that the entire world is made of paper.

More elegantly, Donna Haraway has suggested that technoscience instances “a gesture of materialised deconstructions that literary Derrideans might envy” (Haraway 1997: 102). In this formulation Haraway proposes that deconstruction can learn from non-humanist STS about the performativity of things. On the other hand deconstruction can surely teach science studies lessons in how language facilitates material transformation. This is not a “dual structure” solution, but it does suggest that non-humanist STS and deconstruction have different emphases, which I now continue to survey.

3. Black Boxes and Crypts
I have briefly indicated a few important ideas from non-humanist STS-history and deconstruction. The first concentrated on the interplay between multiple material and social forces in history, the latter on the undecidability of historical action; which, among other things, points to the constitutive capacity of history for defying the intentions and rationales of its actors.
To clarify these differences I now discuss two concepts in more detail: The first is the Latourian black-box, the second Gayatri Spivak’s consideration of the “crypt”. The notion of the black-box is well-known in STS at least since *Science in Action* (Latour 1987). It referred originally to engineering processes whose internal workings could be forgotten as long as a stable output was ensured, and it therefore functioned as an analytical device for simplification. Latour’s suggestion, then, was that science and technologies often functioned as black-boxes for both the public and for social scientists, and that the latter should open these up and show their internal workings. This has been a strong research guideline in trying to understand, for example, military projects (Mackenzie 1990) or particle physics (Pickering 1995a). But in relation to the present case, the strategy of opening black-boxes seems a mixed blessing. The problem is that if one needs a black-box as the starting point for analysis, this may make the STS-student reconstruct encountered events as more closed and structured than warranted, so that categories can be duly destabilised and boxes opened up through his or her intervention. This issue is especially pressing in cases such as Danish medical informatics, a discipline, which is still fairly ‘young’, ‘weak’ and unstable; yet which has an interesting history to engage with.4

Here the deconstructive term *cryptonomy* (Spivak 1977: 23–4) could be of use. In Spivak’s interpretation, a “crypt” refers to a name or designation, which more and more other terms are taken as necessarily referring to and which, therefore, increasingly can be said to incorporate or freeze its scope of reference. A crypt thus marks the false consciousness of mistaking a name for a thing, because focusing on the frozen referents of the term necessarily directs attention away from the processes through which these referents were incorporated in the term in the first place.5 Similar to Latour’s analyses, re-description takes the shape of opening up the “crypt” and making visible the elements underneath the encrypted designation.6 However, cryptonomies are always carried out with the purpose of unpacking any term regardless of its putative stability, and consequently a truly black box is not required to kick-start the operation. Thus, I suggest that whereas the notion of the black-box bring can bring with it a tendency to view cases as unduly stable (otherwise there would be no need for one’s study to open them up), cryptonomical thinking may remedy this emphasis through its relentless insistence on conceptual instability.
Non-humanist STS-studies have been regularly accused of naivety in claiming to merely describe what they observe, while deconstruction has been criticised for its interminable reflexive character. In both instances the lack of normative foundation offered by such studies have been condemned. This understanding of both description and reflexivity, however, makes sense only within a representational horizon, which Richard Rorty succinctly summarises:

The question of ‘accurate representation’ will arise only if one takes seriously claims to the effect that a given text is really about e.g., incest rather than class struggle, or really about the Incarnation rather than the impossibility of reading. For the use of ‘really’ suggests that one reader has been successful in a task at which other readers have failed – the task of representing the text more accurately than other readers have represented it, according to some agreed-upon scheme of projection. In particular it suggests that the reader has succeeded in representing what the text itself really represents (Rorty 1993: 128).

As Rorty says, such critiques always come out of a representational view, since the idea of more or less correct representation is what guarantees that a critical meta-perspective can be obtained, from which normative consequences can be seen as flowing. In contrast with representationalism the positions here discussed both claim to be performative, and this aspiration defines the two central questions explored in the present text: How does one analyse historical events in a “performative idiom”? And what difference does it make that the writer partakes in the same historical field that he or she writes about?

As noted the performance thus referred to varies; deconstruction privileges the agency of a duly expanded textual field, whereas non-humanist STS tends to focus on material agency. Unsurprisingly both of these ‘dimensions’ are present in the history of medical informatics and electronic patient records in Denmark, but their relationships are highly variable depending on time and place, and I venture that this is often the case if one examines highly unstable technical projects – what Latour tellingly has called “partially existing objects” (Latour 1999). To capture important aspects of this history I will adopt and work over Hans-Jörg Rheinberger’s term the future-generating device (Rheinberger 1994), and show how it can flexibly incorporate crucial strengths from both black-boxes and crypts.
4. Future-Generating Devices

STS-historian Hans-Jörg Rheinberger, has been inspired by deconstruction in his attempt to "think history without "origins” and without "grounds" (Rheinberger 1994: 65, 1997). As Rheinberger notes "the present as the future of the past is not a "result” – whatever that means – of the past; the past is the result of a future – its presence as a surrogate” (Rheinberger 1994: 70). Dismissing talk of ‘results’ is Rheinberger’s way of getting around the problem of writing teleological history. In his phrase, certain scientific and technological objects may be viewed as a ‘future-generating devices’; whose futures are structured by différance.8

Différance is characterised by the two linked aspects of differing and deferring, which can be glossed in the following way. Whatever one studies it will be found that it is used and understood with dramatic difference, depending on where and when this happened, in other words, depending on how this concept was deferred from one practice and time to another. In a deconstructive view différance is not just a way of talking about changing perspectives, for perspectivism presumes that there are different perspectives on an object, which is somehow obstinately there prior to the application of perspectives to it, whereas this is precisely what deconstruction denies.9

Future-generating devices bear some resemblance to Star and Griesemer’s well-known boundary objects (1989) and I can further specify my use of the term through a comparison. Boundary objects are similar to future-generating devices in that they are objects, which are capable of flexibly gathering people and work practices around them, in such a way that their ‘users’ need not be in agreement about the tasks they are carrying out.

In Star and Griesemer’s analysis, however, boundary objects are in themselves fairly stable, coherent and, not least, materialised. They can be transportable between certain sites, but there is a loose structure to how this happens, which is precisely what enables them to function as boundary objects. Future-generating devices, in contrast, have very little structure, and few material and discursive limits are (yet) inscribed in them. To the extent that they do generate more lasting futures such structure will, of course, have to evolve. They are thus good examples of Latour’s partially existing objects; black-boxes with a highly diffuse and ephemeral content. This property of
vagueness enables their flexible distribution and adoption in widely variable material and discursive circumstances.

For these reasons future-generating devices are excellent for thinking about technical development projects. In their capacity for dealing with the paradox of envisioned objects: that they live in-between material and discursive realities, future-generating devices draw on the strengths of both the ‘cryptonomic” attention to the performance of language, and the non-humanist STS attention to the performance of things. In later sections I show how this is relevant for analysing Danish medical informatics.

5. Cyborg History and Medical Informatics

The material and social spaces of production have been reconfigured in accommodation to a set of computer-based techniques of surveillance, command, and control, themselves evolving in a process that serves to determine at once the properties of humans and nonhumans (Pickering 1995: 41)

Sociologist of science Andrew Pickering has proposed the term cyborg-history to account for post-WW II developments at the intersections between science, technology and society. By the term he wishes to emphasise performative aspects of history; how historical developments in the sciences, or in organizational or commercial contexts, are not determinable through analysis solely in terms of the progress of technological and scientific knowledge, but must be reviewed in their ”social, material and conceptual heterogeneity” (Pickering 1995: 1).

One of Pickering’s main ambitions is to lift from historians and sociologists the burden of writing history as the history of representations, with all its attendant reflexive problems. His idea, therefore “…is to explore how knowledge and systems of representation fit into the performative field rather than study them as autonomous objects” (Pickering 1995a: 4).

Medical history, particularly after WW II, would surely be a gold mine for cyborg historians. Its relevance also springs to mind for students of medical informatics, as computers have certainly been related to humans in multiple transformative ways, both real and imagined (e.g. Plant 1997, Turkle 1984).
However, telling the history of medical informatics as a cyborg history seems only partly satisfactory in the case of medical informatics and the development of electronic patient records. While the Danish Society for Data Processing has been in existence since 1966, for a long time it led an unnoticed existence, and only small projects with clinical systems were carried out at various hospitals. It would certainly be possible to deploy a cyborg vocabulary to describe these but it would not fully enable a characterisation of the long-term formation of medical informatics as a discipline because all of these projects remained fairly insignificant.

The increased interest in medical informatics and the EPR since the mid-nineties, thus, has not only to do with the actual reconfigurations of humans and machines in hospitals, but even more with the envisioned possibility of achieving such change. This peculiar development points to the existence of genealogies of technical and cultural reconfigurations that are somewhat different from those described in Andrew Pickering’s The Mangle of Practice. It points to the relevance of the notion of future-generating devices as a term, which can be used to characterise the ability of the electronic patient record, even in a distributed, semi-materialised or imaginary state, to gather up a diverse collective of interests.

6. The “Open Black Box” of Medical Informatics
Before 1960 computers had little interest as medical research instruments. Computer use in medicine, at this point, was primarily used in relation to administrative systems and practices, followed by medical education, and medical literature retrieval (Collen 1995: 49). But from the late 1960s large medical organizations and clinical societies showed an increasing interest in using computers as research aids, exemplified, for instance, by automated clinical laboratory systems and multiphasic health testing systems. Only many years later would anyone contemplate full-fledged attempts to integrate systems of interest for clinical and administrative practices in a “seamless web” (see chapter six).

At the time research activities were described with the term biomedical computing, which covered all relationships between the engineering disciplines, biology, and medicine. The word informatics was of Russian origin, and appeared first in a book title from 1968; the term medical informatics was not used in English until the mid-1970s. Until then, various terms had been proposed, such as medical computing, computer
medicine, medical electronic data processing and medical software engineering (Collen 1995: 38). Approximately at the same time a number of university departments were set up in France, Holland, and Belgium entitled *Informatique de Medecine* and *Informatique Medicale* (Collen 1995: 39). This phrase seemed contagious and was adopted into English (and numerous other languages) during the 1970s.

In 1960 the International Federation for Information Processing (IFIP) was formed. This organization had numerous technical committees, some of which worked within medical computing. In 1974 IFIP’s committee TC 4 arranged a very successful conference on such topics, called MEDINFO. This conference was so popular that TC 4 changed its name to IMIA (International Congress of Medical Informatics Association) as a result.14

Nevertheless the term initially seemed rather curious as it was used only once in the 194 papers of the first IMIA conference. Even after *medical informatics* had been generally accepted as a term, the question of what it meant remained diffuse and this situation would continue. As Collen writes:

In the discussions of what this subject was and how it should be taught, however, it was apparent that medical informatics was meant to embrace all the following: medical computing, medical data processing, medical information processing, medical computer science, medical information science, health care information systems, computer hardware and software, computer and information technology, and applications of computers and data processing to the health services and to basic concepts of computer science fundamental to medicine (Collen 1995: 40-1)

It is easy to see in this formulation how medical informatics was a quite “open black box” with a very fluid scope of reference.

### 7. Danish Beginnings: Kommunedata and Patient Administrative Systems

In Denmark *Regnecentralen* (Center for Scientific Calculations) was the only Danish producer of hardware until 1970 (Heide 1996: 321), a centre built on ingenious craftsmanship and inventive curiosity, rather than formal engineering knowledge. After a controversy between governmental and municipal authorities concerning the shape of the developing Danish computerised infrastructure “the base was created for a more permanent arrangement for the municipal centers [for calculation]. The result was the establishment of Kommunedata [Municipal Data] in 1972” (Heide 1996: 249).
Kommunedata grew very strong on the Danish market for public data systems and has been accused of gaining a near monopoly for several years, as municipalities and government consistently chose to purchase its products. In time, Kommunedata shifted the focus of its product line from hardware to software development, and became central in the development of modern patient administrative systems from the early 70’es and on. In these years, the so-called red system was developed, with the purpose of enabling registration of patient data through the unique Danish citizen identification system (CPR), which had been introduced in connection with the implementation of a new computerised tax system. This considerable innovation in bureaucracy administered a unique identifier to each Danish citizen at birth; which follows Danes throughout their lives.

The red system was widely adopted in the Danish health care sector and covered 75% of Danish hospitals as late as 1987 (Müller, Kjær-Rasmussen and Nøhr 1988:53). However, it focused exclusively on the registration of codes and was therefore “only for secretaries”. In the seventies Hvidovre Hospital was also built outside of Copenhagen (home of the HVEPS-project discussed in chapter four), and a new system, Elkom, was specifically designed for it. Each patient had one punch-card, which could be used to order X-ray and other laboratory tests and services. The system made quite an impact in Denmark, but when the Copenhagen municipality had to renew its system some years later it was pressed by Registertilsynet (The Danish Data Protection Agency) to forego the inventive Hvidovre system for security reasons.

Kommunedata had to develop the new system for the Copenhagen Municipality and learned a number of things from the innovative Hvidovre system. Aside from improving security, their new “green system” added a basic booking function, in the shape of a computerised calendar improving administrative capacities at the hospital. The National Board of Health had developed a National Register of Patients to which all hospitals had to report information on diagnoses, surgery and out-patient treatments, and the green system therefore remained directed towards reporting functions. But the system could also be used for a limited amount of clinical purposes such as ordering tests and receiving answers, and helped to blur the distinction between administrative and clinical use. This created a fertile basis for imagining increasingly integrated systems and when the green system started up in 1987-88, Kommunedata estimated that its adoption would...
increase the amount of computer users at hospitals with a factor twenty (Müller, Kjær-Rasmussen and Nøhr 1988: 57).

8. The Danish Society for Medical Informatics

In April 1986 the Danish Society of Medical Data Processing (DSMD) decided to start the publication of its membership journals. Increasing computer use for both clinical and administrative purposes made the mid-eighties an important time in the history of medical informatics, a time in which the problems it defined for itself became more visible and began to seem more relevant to a broader community of people. This is made explicit in the “Words from the director”, which is on the cover of the first issue of the membership journal.

These years the area of interest to the society – information technologies etc. - attract growing attention, also within the health sector. At the general meeting on March 20th of this year a board was chosen, which considers important the task of informing as well as possible the members of the society about meetings and other activities within this rapidly expanding field (DSMD: April 1986)

In 1987 the name of the society was changed to the Danish Society for Medical Informatics (DSMI). At this point the organization had less than two hundred paying members. Many of these were biostatisticians working for the clinical societies, but engineers and programmers soon became involved in projects to develop technologies such as the national cancer and cardio-vascular databases.

In spite of the limited population of the society, the perception that a radical expansion of the field was on its way was a recurring theme in several early membership journals. These illustrated well the rather low-key information politics of the society, with a length of eight to twelve small pages and a primitive black and white lay-out. They offered various content. First they gave information on monthly membership meetings and printed abstracts and synopses. Second, they notified members of national and international congresses as well as local work-shops and other activities. Finally, they reported from international meetings and congresses, and encouraged members of the society to participate in these events (DSMD April 1986). What did work in medical data processing consist of? In a 1986 journal was presented a list of meetings, which had taken place in the society during the ’last few years’:
Medical data processing – status and perspectives, computer-supported diagnostics and decision-making, artificial intelligence, the possible uses of computers by the visually handicapped, the law of registration in theory and practice, computers in general practices, diagnostic medical imaging, injury information systems, registration of diagnoses, information search in patient-registration systems, computer record, nursing and computers and the green system (DSMD: May 1986)

The diversity of these initiatives is striking, and remained so in the following years, as medical informatics distributed and reproduced itself in diverse contexts, with a characteristically low degree of “encryption”.

Undoubtedly, however, Danish medical informatics was under transformation in the early 90es and the annual 1990 report from the society stressed the importance of paying close attention to a number of national and international initiatives. On the national level, it was pointed out that few Danish institutions recognised medical informatics as a discipline ”with independent departments and personnel fully employed within the field” (DSMI: February 1990) and predicted that this situation would have to change within the next decade. In fact such a department was set up at Aalborg University in 1994 with important implications.

Internationally, two activities were seen as crucial for the future of Danish medical informatics. First the EFMI had offered provisional commitment to place the MIE’ 1996 conference in Copenhagen. Second the report remarked on the substantial amount of money (20 million ECU) made available by the European community for research in medical informatics through the AIM (Advanced Informatics in Medicine) program in the late eighties. At the end of this AIM period a EUROFORUM was held in Seville, and it was ”as good as certain that 97 million ECU will be accorded over the next four years for ‘AIM 91-94’” (DSMI 1991, February). As a Danish AIM expert wrote:

Naturally the content [of the AIM project] will be decided on the basis of the proposals that are accepted. But areas such as medical imaging, communication, knowledge-based systems, working stations, resource management, data-protection and standardization seems to have a high priority among the twelve member countries (DSMI: November 1990)  

Here is yet again a list, which manifests the heterogeneity of medical informatics, not least in its juxtaposition of themes of clinical origin, such as medical imaging and knowledge-based systems with those of administrative concern, such as data-protection and standardisation. The structure of différance argued by deconstruction is recognised in the capacity of this discipline to function as a loose assemblage of activities distributed in
time and space but with little similarity in anything but name. From the point of view of the present it is also remarkable to note the absence of electronic patient records as a relevant category in applying for EF-funds. Only a few years later these records would be omnipresent in discussions about Danish medical informatics. As we shall presently see, this happened as certain practical initiatives created enough attention to interest politicians, and were coupled with their visionary discourse. This association generated an increasing amount of money, and the capacity to imagine a completely integrated Danish health care sector has been perpetuated and up-graded from the mid-nineties and on, in spite of the inability of any system to live up to the visionary expectations.

The early 1990s saw a burst of interest in the society. Thus the March 1992 issue registers 39 new members; an increase over a one month period of almost 20% (from 207 to 246 members including company representatives) and membership, particularly among nurses, continued rising in the following years.

During these years a number of initiatives affiliated with medical informatics were on the way. Important among these were the development of the Danish SKS-classification, which was to be used for standardised documentation of the treatment of patients in the National Register of Patients. Building on the ICD (International Classification of Diseases) the SKS-classification has partaken in the move to turn registration into a tool for clinicians and researchers. In these years, decision-makers increasingly came to interpret free flow of information in the health sector as a prerequisite for effective treatment practices. In this regard the FynCom project figured centrally. FynCom (the health data network of the Funen region) started up in 1993. Its purpose was to develop EDIFACT standards specifying the format for exchanges of “pieces” of information, between hospitals, laboratories, pharmacies and G.P’s offices. Initially FynCom made it possible to send electronic discharge letters and for hospitals to receive information about tests carried out at clinical microbiology and pathology at the university hospital in Odense and certain other labs on Funen. The structured format of information exchange enabled hospitals to send relevant information directly to the “electronic mailboxes” in G.P’s offices, from
where they could be automatically transferred into a medical record. Quickly thereafter was added a feature enabling G.P.’s to electronically order medicine at the pharmacies, making it available as patients arrived. In 1993 the system exchanged around 4000 messages per day, and a re-configuration of hospital practices were underway on Funen.

The success of project was noticed by the National Board of Health and the Ministry of Health, and in May 1994 the project was made national under the name MedCom 95’. FynCom and MedCom were both organized around the potentials of a heightened structuring of clinical information; in the bureaucratic imaginary of the National Board of Health and among some members of DSMI, standardisation thus became more and more important, and the truly cyborgian vision of integrating the entire Danish hospital sector in a ‘seamless’ web of information developed. With this idea in mind, the work in the CEN (Comité Européen de Normalisation) working group TC-251, to develop common European standards in the field of medical informatics have been attentively followed by DSMI representatives and other officials since the early 1990’s (as discussed in chapter eight).

In January 1994, the DSMI held a constitutive meeting for a contact group for electronic patient records defining their tasks as ”arranging 2-3 thematic meetings: with presentations about current projects, from industry and government, concerning standardisation and other international work etc. To start collecting material for a Danish status of the EPR” (DSMI: September 1994). This meeting dove-tailed with the publication of the Info-Society 2000 report, which emphasised the necessity of developing electronic patient records, and in combination these events can be said to constitute the birth of the electronic patient record as a future-generating device.

Chapter five of the Info Society, “A Better Health Care Sector with Quicker Treatment”, which takes up five pages, spends one of these showing figures demonstrating “improvements in efficiency by use of EDI [Electronic Data Interchange]” in the Funen Region, a possibility which lead the authors towards considering even more integration of information in an electronic patient record: “Further pronounced gains in service and efficiency could be achieved by use of electronic patient records, which can gather information across the wards of individual hospitals, and whereby it is possible to exchange information between hospitals and with G.P’s” (Info-society 1995: 48). These ideas are repeated in the slightly more substantial supplement to the report (Supplement
193-205), which specifies concrete initiatives circling around FynCom, MedCom, and stresses the need for more standardisation, by deploying the classical figure technocratic rhetoric in which future efficiency will be ensured by technological progress.

10. The Differential Reproduction of Medical Informatics and the EPR
The publication of the *Info Society* report also had immediate consequences for DSMI, which saw the quickest and most drastic increase in members in the history of the society. The changed situation did not escape the board of the society who wrote:

> 1994 has been an unusually good year…Of course, this is highly related to the general societal development, which in 1994, not least from political quarters, was forcefully steered towards the information society. From the point of view of the society this is very gratifying but also binding” (DSMI: April 1995)

And from 1995 and onwards an increasing number of projects with the development of EPRs have taken place in Danish hospitals and regions, encouraged by formulations found in this report and later governmental publications (e.g. DSI Rapport 96.05, Sundhedsministeriet 1996).

Other initiatives of continuing importance likewise began. An education for medical informatics was set up at Aalborg University, a comparatively new and experimental university, with an already active “department for societal development and planning”, which had done research projects on “Informatics in Hospitals: Technology Assessment and Planning”. Aalborg was an ideal location because, in contrast with Copenhagen, Aarhus and Odense, the other central Danish university cities, it had no medical school, and negotiations with “conservative” physicians could therefore be avoided.

The medical informatics education would take two years part-time study and aimed at an audience with a background in health care. In combination with its status as a ”problem-oriented project-organised study” (DSMI April 1994, also Nøhr and Andreassen 1995) this format has proved particularly attractive to nurses. Typical project studies have had to do with analysing organizational change processes as IT was implemented at accelerated pace at hospitals throughout the country.

The favorite method (or metaphor) developed to grasp organizational complexities within Danish medical informatics, is “Müller’s Square”, developed by graduate engineer
Jens Müller from Aalborg University. Müller’s square contains a grid of four fields: information, technology, education, and organization, which should always be considered in their interrelations. This is an approach, which, on the one hand moves the understanding of technology away from narrowly deterministic conceptions, but on the other hand maintains the rationalised idea that technological development can be controlled if an increasing amount of variables are taken into account.

The department in Aalborg became a central actor in Danish medical informatics and it has continued to diversify its interests. For instance, a co-operative effort between Aalborg University, the health sector of the Region of Northern Jutland, the medical faculty of the University of Aarhus, the Aarhus Region, Danish Institute for Health Services Research, and Center for Health Informatics in Odense, Funen, was soon started up under the name Center for Health Informatics (CHI). The center was situated in Aalborg as a consequence of the fact that the education of medical informatics was located at this institution.

The centre interpreted their field of work in the following way: "In a simplified way we define health informatics as the discipline that, by technological and organizational initiatives ensure that: “the right information is present in the right form at the right time and at the right place” (DSMI: June 1995). CHI was to work as an umbrella organization for projects at the intersection of health care and information technologies. These would be related to themes such as

- health technology assessment,
- implementation of technology,
- quality assurance,
- decision-support,
- clinical epidemiology,
- interfaces/multimedia,
- networks and protocols,
- communication and competence

and the electronic patient record (DSMI: June 1995)

Shortly after, the centre was re-named the Virtual Center for Health Informatics, to emphasise its status as a "joint venture" between the above-mentioned organizations and not in principle bound to Aalborg. At this point the series of themes listed above had crystallised into five main areas of engagement for the centre: health technology assessment, decision-support and databases, organization, education and distribution of information, and finally communication and standardisation, where V-CHI became active in the development of standards at CEN TC 251. In relation to the EPR, V-CHI came to house the so-called EPR-observatory that had the purpose of surveying and promoting Danish EPR initiatives. The observatory is partly funded by the 6th office of the National
Board of Health, which opened in 1997 to work specifically with medical informatics issues. As a consequence of these various initiatives, in 1996 the first graduates from Aalborg were able to step directly into managerial roles in the projects then starting up.

In the preceding sections I have suggested that the association of the moderately successful FynCom project, European standardisation initiatives, and the envisioning capacities of certain politicians and clinicians enabled the generation of the EPR as a future-generating device in the Danish context. The EPR would be the final step in the direction of a health sector with all activities fully integrated and a free flow of information.

A future-generating device is not self-reproducing. As I have tried to show it has rather had to be differentially reproduced over various contexts involving many actors. In a phrase the electronic patient record, up to this point, has been interactively invented. This situation sends me back to the historiographic consideration with which the paper started, for it raises the question of what part there is for the historian in such invention. To open a space for this consideration I re-visit the classical historiographical idea of the reality rule and consider a number of ways in which deconstruction and non-humanist STS challenges it.

11. Challenging the Reality Rule

The stage [of history] is theological for as long as its structure, following the entirety of tradition, comports the following elements: an author-creator who, absent and from afar, is armed with a text and keeps watch over, assembles, regulates the time or meaning of representation, letting this latter represent him as concerns what is called the content of his thought, his intentions, his ideas. He lets representation represent him through representatives, directors or actors…Interpretive slaves who faithfully execute the providential designs of the "master" (Derrida 1978: 235)

Evaluating the work of historians has traditionally been seen as a matter of verifying whether their narrative sequences were in factual order and their subsequent causal explanations valid, but this scheme has been criticised from many positions in recent decades. One controversial attempt to grapple with it is found in Hayden White. In the essay "The Question of Narrative in Contemporary Historical Theory", White suggests that "…Barthes challenged the distinction, basic to historicism in all its forms, between "historical” and "fictional” discourse.” (White 1987: 35). Many historians have
appreciated the value, or at least tolerated the necessity of narrative in history, but only to a limited extent. Historical narrative has been acknowledged as necessary but strictly separable from historical explanation. So, after the historian had discovered the true story of "what happened" and accurately represented it in a narrative, he might abandon the narrational manner of speaking and, addressing the reader directly, speaking in his own voice, and representing his considered opinion as a student of human affairs, dilate on what the story he had told indicated about the nature of the period, place, agents, agencies, and processes (social, political, cultural, and so forth) he had studied (White 1987: 28-9).

Richard T. Vann in his contribution to *A New Philosophy of History*, formulates the problem with the stance thus described in the following way:

This posture, as Hayden White was later to point out, easily allowed for a sort of bad faith, in which attacks on history for its lack of rigour could be deflected by pointing to its artistic character, while critics who pointed to the poverty of imagination and reliance on outmoded literary forms displayed by historians could be fobbed off by the claim that these arose from the constraints imposed by fidelity to fact (Vann 1995: 42).

And the challenge posed by Barthes, in fact, was to claim that historical and fictional discourse was equally ideological. For this reason his interest was not so much to oppose science to non-science as to "distinguish between progressive and reactionary, liberating and oppressive, ideologies" (White 1987: 35). Barthes’ (and White’s) attack on scientistic history was established by denying the validity of what has been called “The Reality Rule”, which insists on Ranke’s dictum that the historian must write about history “wie es eigentlich gewesen” (Vann 1995: 53).

It is easy to see how cryptonomy moves one away from “reality”, with its proposal that fundamentally unstable amalgamations will be found inside every “crypt”. Deconstruction points to the structural condition of *différence*; this inevitable sliding of meaning as concepts are reproduced in different times and spaces, has rendered problematic the distinction of fact from fiction and made ambiguous the specification of any concept. The reality rule does not apply from a deconstructive viewpoint, then, because reality can never be captured in the historical play of differences, which always would enable more layers to be uncovered, but never a final one.

However, in spite of the broad expansion of the notion of text, classical textual concerns often remain at the centre of deconstructive attention (witness the types of ‘actors’ engaged in Derridean interpretation), and an outside seems rarely to impinge on
such analyses. Deconstruction never manages to quite let go of representation: paradoxically its performativity can be viewed as still functioning within a representational horizon. As has been noted, deconstruction therefore tends to view a kind of reflexive sophistication as the best cure against realism. It is this problematic recursive situation, which has led Derrida to talk of deconstruction as *hauntology* (Derrida 1997), for while the “real” can be forever traced or chased, one is bound to always find only its ghostly shadow.\(^{23}\)

Be that as it may, the “reality rule” has a long history and strong background in historiography and is difficult fully to let go of. Even as sophisticated practitioners as cultural historian Roger Chartier, is unwilling to leave it behind, and I invoke him here as an instance of a scholar of radical epistemological bent, who nevertheless at crucial moments withdraws from its implications, thus instantiating what Barbara Herrnstein Smith has called “cutting-edge equivocation” (Smith 2002).

For example, because Chartier binds himself to a distinction between the objective and the subjective, he cannot but evaluate Hayden White as subscribing to the rather silly notion that history and its writing is *solely* a matter of subjective preference. Inveighing against this view Chartier suggests that

To define the “scientific” nature of history in this manner – as the best possible, most nearly adequate rendering of the referential reality it aims at – is neither to deny its fundamentally narrative nature nor to think of historical thought within the “Galilean paradigm” of the mathematical sciences (Chartier 1997: 35)\(^{24}\)

Roger Chartier makes these criticisms because he worries that the concern with problems of representation has turned historical inquiry topsy-turvy; that interminable conceptual loops remove the focus from the history to be described. To stop this downwards spiral he feels forced to refer to an extra-discursive referential reality as the stop-gap of representation.

Criticising Hayden White or Jacques Derrida for their somewhat limited interest in the mundane, pragmatic aspects of history and, indeed, advocating a focus on the materiality of institutions is clearly laudable from the point of view of a non-humanist STS historiography. However, as should by now be clear, I think that the ground on which Chartier situates himself when making these criticisms is rather unstable. For this ground is none other than a version of 'referential reality' as we are able to know it
through our most sober historical methods; in other words it is a version of the “reality rule”.

There is one aspect of cryptonomy, which I have not yet remarked upon. In its psychoanalytic inspiration\(^{25}\), the crypt, as a site of burial, also marks cryptonomy as a work of mourning. While it is a deconstructive tenet that concepts are open to interminable analytical destabilisation, it is concurrently conceded that this situation will necessarily be experienced as (more or less) lamentable. Here, deconstruction defines itself in direct relation to the logocentric tradition it questions, by proposing that even while undermining the quest for conceptual unity (e.g. “historical reality”), any endeavour of thought must nonetheless be tinged with such an urge. Thus, in deconstruction one always, and necessarily, remains in the metaphysical grip of logocentrism no matter how much one struggles.

It is in this presupposition that deconstruction diverges most from non-humanist STS-studies. Andrew Pickering’s cyborg history, with its critique of the urge to ”explain collective social phenomena [by] populating the domain of the invisible with hidden principles and axioms” (Pickering 1997a: 326) hardly takes one away from reality, although it does move one away from the reality rule. Not by re-working the logocentric tradition from within and moving towards more and more fine-grained conceptual distinctions, but by increasing analytical openness to the multiplicity of empirical occurrences. Rather than struggling with reflexive problems such work attempts to engage practices at the level of material transformation, and show how historical novelty is exhibited in the only semi-controllable interactions between human and non-human agencies.\(^{26}\)

If everything is part of the performative field, this includes historical representation and representations of history. A Derridean strategy points us in the direction stylistic and reflexive experimentation, but these are experiments on the level of the text as classically defined. If, however, texts perform in a socio-material field, the performative historian can legitimately view his work as partaking and intervening in ongoing sociotechnical transformation by engendering new ideas, formulating solutions to known problems, and raising new questions. As non-humanist STS-historiography focuses on the many surprising effects, which arises out of the interactions between people, technologies, institutions, so it would be concerned with its own capacity for...
effecting existing practices. In conclusion I briefly want to characterise some of the re-
definitions suggested and some of the questions raised by analysing the history of Danish
medical informatics in terms of the construction of the EPR as a future-generating device.

12. Performative History, Performative Historians

I suggested that processing the history of Danish medical informatics through a number
of concepts from non-humanist STS and deconstruction could be viewed as an
experiment, which would be successful to the extent that it was interesting; that is, to the
extent that it would enrich historical and historiographical sensitivity.

Rather than correctly trying to represent historical reality, the ideal of such histories
would be to open up new frames of understanding and expand analytical imaginaries. In
the case of technological development projects, these are often limited to narrow,
determinist conceptions, which are manifestly unable to handle the complexity of reality.
This situation is only made more interesting by the fact that problems with technological
projects always seem equally surprising to both spectators and participants and can
therefore only be interpreted in terms of notions such as “failure” or “scandal” (see
chapter eleven).

I have analysed the development of Danish medical informatics by adapting the
notion of the future-generating device to capture important strengths from both non-
humanist STS and deconstruction, and I have used it to analyse the genealogy of Danish
medical informatics and electronic patient records. This history has not been
characterised by a high degree of black-boxing. Rather, it has displayed how disciplinary
and practical transformation was enabled for a long time by a material and discursive
looseness, into which political visions could easily tap. The association of political
visions with the limited practical success of FynCom eventually enabled the generation of
the electronic patient record as a future-generating device.

Evidently this sort of history emphasises the contingency rather than inevitability of
events, and the heterogeneity rather than homogeneity of interactions. This specific
history also demonstrates how socio-technical development cannot be strictly dependent
on the principle of black boxing, for medical informatics started as an “open black box”
and remains open to a high degree, even as multiple transformations has occurred in the
field over the last decades. Given this situation the opening of black boxes cannot be a
methodical *sine qua non* for researchers of technoscience, for there are many interesting things to describe, but few of them are highly stable. Due to lack of material stability visions came to play a very prominent a role in the generation of the EPR as a future-generating device, and the importance of rhetorical construction points to deconstruction as a valuable contributor to studies in technoscience, although as argued above, it cannot be adopted wholesale.

The smallness and openness of the networks of Danish medical informatics and developers of electronic patient records may indicate the lack of interest of the current case. I have wanted to turn this argument upside down in order to suggest that this case offers particular insights into the performative capacities of the historian. It is because the material networks here described are so comparatively unstable that visions have come to function as stabilisers of medical informatics, and I have argued that this is the reason why discursive effects have had such a central function in the construction of the EPR as future-generating device.

But discursive effects are also the order of the day for the historian. Their importance points to his potential relevance as analyst of naturally occurring events in technoscience; for in the half-life of technological development projects, between words and things, futures are still very much up for grabs. As the technological future is not yet boxed in, posing relevant questions and suggesting alternative answers offers opportunities for participating in changing its course.

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1 The document *From Vision to Action – Info-Society 2000: Statement to Parliament on "Info-Society 2000" and IT Political Action Plan 1995* is available from the homepage of the current ministry of research and information technology at http://www.fsk.dk/fsk/publ/it95-uk/. The ministry summarises the content in the following way: "In October 1994 the Government Committee on the Information Society by the Year 2000 submitted a report, "Info-Society 2000". Shortly before, in connection with a reshuffling of the Government, the jurisdiction of the Ministry of Research and Information Technology had been extended to comprise information technology and telecommunications. The decision to do so was made to radically upgrade the political priority given to these areas, thereby strengthening Denmark's realisation of the information society."

2 The idea of undecidability is discussed in *Plato’s Pharmacy*. In the introduction to this essay in *A Derrida Reader*, Peggy Kamuf describes the term in the following way: "The principal guide Derrida chooses to follow within the intricacies of this play [the *Phaedrus*] is the family of pharmaceutical terms that, more or less explicitly, are associated by Plato with writing, but particularly the term *pharmakon*.

"An Experiment in Performative History“ 131
classical Greek, a *pharmakon* is a drug, and as such it may be taken to mean either a remedy or a poison, either the cure of illness or its cause. It is this essential undecidability of the *pharmakon* that poses the problem of translation which, as Derrida points out, is not simply the problem of translating Plato’s Greek into another language, but already introduces within that single language (which happens to be the inaugural language of philosophy) the necessity of translating Greek to itself” (Kamuf 1991:112). To anticipate a discussion of some of the differences between deconstruction and non-humanist STS-studies, to which I will return in the main text, I juxtapose Kamuf’s elucidation of undecidability with a recent commentary on this Derridean reading made by Michel Serres in *The Birth of Physics*: “The children’s top, *strobilos*, *rhombos* or bull-roarer, games and magic rituals frozen in the diamond-form of Euclidean space, here reveal a solution, easily formed, to all the difficulties of the self-same operator functioning, almost at will, towards contradictory results. Is it stable? Yes. Is it unstable? Yes, again. Is it rotating, does it follow a circumference? Yes, ever again. The top is a *circum-stance*…So ask some questions constructed around yes and no; the possibility of finding, building, and observing an object that will not destroyed by this difference is hereby established…What is more, a single plant, for us too, may kill or cure us. This pharmacology is under the sign of the top. Not only is the thing decidable, it is constructible, look at the top. That is how it is, in the thing itself, and no discourse can change it. As if the contradictions separated themselves, as if they repelled each other, in the battle of reason and language, while the contraries cohabited in the black box of things. If, one day, some subtle and playful dialectician disconcerts you, be quiet, don’t answer, join the children, play at tops” (Serres 2000: 29-30).

Faye Ginsburg and Rayna Rapp, for example, distances the ethnographic approach from critical cultural studies in the following way: “*Time* magazine… regularly publishes cover stories on everything from new infertility treatments to the search for *in utero* methods of screening for genetic diseases…[T]hose using [cultural studies] methods might view *Time* magazine as an unproblematic stage for the display of scientific hegemony. From an anthropological perspective, this kind of analysis relies on outdated Durkheimian models in which the image and its interpretation are isomorphic “(Quoted in Franklin and Ragoné 1998: 6).

Medical informatics may be more reasonably characterised as a “translucent box” (Jordan and Lynch 1992).

As regards re-descriptive capabilities, Bruno Latour has in particular praised the work of art historian Svetlana Alpers (see Latour 1990, also Alpers 1982, 1983).

Arkady Plotnitsky argues that “The process he [Derrida] describes is much closer to his own practice of interminable analysis – “incessant deciphering” – of often, perhaps always, undecidable relations than to Nietzsche’s practice. Nietzsche’s economy of active interpretation operates more by fictively positing and, as it were, experimentally testing "truths" and "fictions", or complementarily both, rather than, and often *against*, engaging incessant deciphering or unveiling. Derrida’s psychoanalytic metaphor suggests that the latter is modeled on Freud’s concept of interminable analysis…suggesting a continual return of the patient of the analysis” (Plotnitsky 1994: 194)

This term is thoroughly discussed in ”Differéance” in *Margins of Philosophy* (Derrida, 1982). Peggy Kamuf offers an introduction in (Kamuf, 1991: 59-60).

This theme is laid out in chapter three. Coming from a completely different angle Annemarie Mol argue in a similar manner against the notion of "perspectives". Her solution to the problem – empirical philosophy – is far from deconstruction, and much closer related to what I here characterise as non-humanist STS-studies (Mol 2002: 7-13).

As I discuss later, a performative perspective does not delete the idea of representation, but rather views it as a specific aspect of performativity. Andrew Pickering phrases the point in the following way: “One can think of factual and theoretical knowledge in terms of representational chains passing through various levels of abstraction and conceptual multiplicity and terminating, in the world, on captures of and framings of material agency. And…conceptual structures (scientific theories and models, mathematical formalisms) can themselves be understood as positioned in the field of disciplinary agency much as machines are positioned in fields of material agency. Conceptual structures are like precisely engineered valves, too, domesticating disciplinary agency. Again, though, conceptual engineering should not be thought of as independent of the engineering of human and material agency…” (Pickering 1997: 65).
Illustrations: Chapter seven

Experimental Devices

One can consult the material presented for example in Casper 1998, Kevles 1997, Lenoir 1998, to get an idea of the complex intertwining of humans and machines in recent medical practice.

Even less complicated management information systems used for administrative and reimbursement purposes in the hospital world, face staggering problems in practice. As Marc Berg notes: “Although precise data are lacking, a general handbook on management information systems estimated that from the large systems that end up being used as much as 75% should be considered to be operating failures. They might be in operation, but they are too cumbersome or too functionally deficient to even remotely be called a ‘success’” (Berg 1999: 88)

In a phrase with similar evocations, Judith Gregory talks about the development of electronic patient records as an “incomplete utopian project”: ”The incomplete utopian project of electronic health record invention draws from several utopias with long historical roots: the search for the perfect language, the desire to eradicate mistakes, managerial desires for intricate and far-reaching control over decision-making and standardization of practices, the quest to rationalize and scientize medicine, and, more recently, the idea that everything can be electronically connected and traceable through software whose automata can intelligently ‘read between the lines’.” (Gregory 2000). This term shares with that of the future-generating device both the invocation of multiple hopes and desires, and the simultaneous possibility of their (potentially endless?) deferral.

Finally, in 1988, IMIA was completely separated from the IFIP organization.

In his discussion of The System of Professions, sociologist Andrew Abbott characterises naming as of performative importance for evolving professions: “It is by their claims that groups identify themselves; to claim a jurisdiction is to claim it for someone…By the change of name the groups identities itself as “the profession of x”. To say a profession exists is to make it one.” (Abbott 1991: 81).

In fact, this second round of applications was delayed. This is remarked upon in (DSMI: January 1992).

In 2002 the “national register of patients contains approximately 40 million pieces of information about patients. It is the duty of all public and private hospitals in the country to make monthly reports of all hospitalizations, ambulatory treatments and visits to the casualty wards to LEC [the software company running the register].” (http://www.lec.dk/indhold/lec9072.htm)

During 1994 private memberships increased from 193 to 342.

There is an interesting change in terms since the 1986 volume Samfundets Teknologi – Teknologiens Samfund (The Technology of Society – The Society of Technology), in which the fields are called “knowledge, organization, technics and product” (Müller, Remmen and Christensen 1986: 15-29).

The context of this quotation is a discussion of Antonin Artaud’s challenge to the classical idea viewing theatre as the representation of the author’s intentions. The comment can, however, be taken as indicative of Derrida’s general stance towards logocentric thinking, as is manifested in both non-historians self-descriptions and in classical historiography. Indeed, in another phrase: “If the word history did not in and of itself convey the motif of a final repression of difference, one could say that only differences can be “historical” from the outset and in each of their aspects” (Kamuf 1991: 64).

Sandoval (2000) interestingly discusses a number of concepts and ideas from Barthes and White as well as Derrida, although I am not convinced by her synthesis of these ideas into a general methodology of the oppressed.

See also Smith 1980.

This has led Derrida into increasingly elliptic stylistic experiments in a number of recent books, such as Glas or The Postcard (Derrida 1986a, 1987).

Even though he spends precious little time discussing it, Chartier also views deconstruction as idealist, and proposes that it cannot take into account the materiality of the text and the corporeality of the reader as is needed in order to “reconstruct the "play of differences" of which Derrida spoke” (Chartier 1998: 117).

See note 7.

Cyborg history as well as actor-network theory has given rise to a substantial amount of debate, which merits an extended note (see, for instance, Galison 1995, 1997, Gingras 1995, 1997, 1999, Pickering 1995b, 1998, 1999). The focus of these theories on change and transformation has been read as suggesting that everything is actually continually changing. I think that what they have emphasised is rather that everything is in principle subject to change, and so in unforeseeable ways. They change furthermore, in ways our regular conceptual repertoires fail to grasp, because, as the "incantation" goes"the technical and the social cannot be distinguished" (Gingras 1995: 124), and because these

“An Experiment in Performative History” 133
categories are themselves shaped by such changes. Yves Gingras, in particular, has denounced Andrew Pickering’s criticism of the notion of constraint in his historical and social analyses as "always lead[ing] to the conclusion that these things 'just happen'" (Gingras 1997: 331). I imagine the point rather to be that the notion of constraints is empty because it can always only be applied retrospectively. Cyborg history is particularly (not absolutely) interested in analysing transformational processes. One model for thinking about the relationship between stability and change, which has been adopted by various scholars, such as Peter Galison, Manuel de Landa, and Geoffrey C. Bowker, is the tripartite temporal model of Annales historian Ferdinand Braudel (e.g. Braudel 1993, Galison 1997, de Landa 1998). This model is put to interesting, and interestingly different, uses by the above-mentioned scholars, but its application, in any case, opens the question of which sorts of events would enable one level of the model to influence the others. Cyborg history and ANT would question the ability of any historian to define a typology suitable for answering that question. In any case, the relevant question is not whether a “pure” cyborg history is possible or what it would look like, but what interesting questions non-humanist historiography could produce.
Chapter eight:
Standardisation and the Logic of the Differend

1. A Tension in Standardisation Studies
I want to start this chapter by indicating what can be seen as a tension in present studies of standardisation. It relates to the perception that while standards and the process of standardisation, in a number of respects, may be viewed as highly important, both product and process are often taken, colloquially as well as professionally as a trivial and dull matter for techno- and bureaucrats. This reflects on research in standardisation research as well, as an endeavour which risks being seen as carrying the same traits.

Both of these aspects (importance and boredom) are touched upon in introductions to such studies. Georges J. E. De Moor, for example, in his abstract for an article relating the activities of CEN TC 251, a technical committee, whose work will be discussed further below, stresses “the importance of standardization in the domain of healthcare informatics” (De Moor 1994: 1). In the special issue of the Journal of the American Society for Information Science on “Information Technology Standards”, the guest editors begin their introduction by suggesting that “While few people spend their days worrying about standards, no one exists in our complex social system for more than a few minutes without being affected directly or indirectly by standards” (Spring and Lunin 1992: 522). Kai Jacobs echoes this sentiment in the preface to the first issue of the recently established International Journal of IT Standards and Standardization Research, in a paragraph, which starts out noting that “I frequently get the impression that IT standards research is one of the most underestimated research domains” (Jacobs 2003: i), goes on to list some of the innumerable standards we are all surrounded by, and ends up concluding that “I would therefore argue that it should be in virtually everyone’s interest to learn as much as possible about how standards emerge, what exactly shapes them, which impact they have, and other questions that surround IT standards and standardization” (ibid.).

While it is probably hyperbolic to imagine catching the attention of everyone it seems clear that such studies make important points, which it could be worthwhile taking seriously. Undoubtedly, they are right in emphasising how standards are intertwined in the social fabric in many important ways. The question that remains is how one moves
from that insight towards an understanding of standards and standardisation, which would not seem too technical, boring, or otherwise irrelevant to its potential broad audience.¹

I think it is safe to say that the participation and contribution of social theorists in standardisation research has been important and will remain so. For if researchers in IS and management studies have been quick to point to the overall social importance of standardisation and, not least, standardisation choices, then they have often lacked the analytical methods of social researchers in making sense of that importance in broader societal terms (see e.g. Agre 1990, Forsythe 2001, Suchman 1987, 1988, Star 1995, 1995a). Rob Kling has characterised the general problem thus: “a remarkable fraction of [computer and information scientists’] accounts are infused with a hyper-rational and under-socialized view of people, computer systems, organizations, and social life in general…Further computer systems are portrayed as powerful, and often central, agents of organizational change” (Kling 1998: 50).

Of course, the complementary problem has also obtained; that is, that the social theorists analysing IT in general and standards specifically, have not had the necessary technical understanding to go much beyond broad characterisations of the impact of technology on society (see e.g. Monteiro and Hanseth 1995).

In this view it is not surprising that some have found a set of ideas, tools, and methods from the field of science and technology studies (STS) and in particularly actor-network theory (ANT) congenial to analysing standardisation processes, for these are theories that view the social and the technical not as distinct spheres, but as mutually constitutive in socio-technical networks (e.g. Berg 1995, 1999, Timmermans and Berg 2004). Ideally, such approaches would, then, offer the “best of both worlds” (technical and social), although an STS approach would view such a “two cultures” understanding as misguided, since these studies show how there is only one world, albeit shaped by heterogeneous elements.

With this development all would thus seem to be well. However, an additional complication of standardisation research exists, which follows from the question “what is the purpose of these studies?” This question will hold my attention here. For some, this question might be answered by pointing to “knowledge for its own sake”, “enhancing formal standardisation procedures”, “gaining competitive advantage”, or a number of other possibilities. But within the purview of social theory this is not often the road taken.
As mentioned, comparatively little attention has been paid to standardisation from this area of academia. However, at specific points it has been able to exercise the imagination of social researchers. Most often these have been times when standards are seen as encroaching upon practice.

The arguments brought forward in such cases often oppose the procedural efficiency, which standards are presumed to bring about, with the living, embodied knowledge of the skilled worker, which can never be fully captured by standardised procedures (e.g. Heath and Luff 1996, Wilkinson 1983). In the health care sectors such debates have been recurring, for instance as relates to the question of the use of clinical expert systems for diagnosis and treatment (Berg 1995). But from the point of view of STS-studies, the typologies adopted in such analyses have tended to reify both standards and practices. Standards have been viewed by social critics as inherently static and reductive, whereas work-practices have been seen as unpredictable, creative, and supported by tacit professional expertise (Berg 1998).

In contrast to this view STS-scholars have looked carefully not only at standards at work, but also at the construction of standards. Where do they come from? Who makes them? And how do they travel? In practice standards come in many forms and shapes. They come from different places and relate to many different problems. Some relate to the same problem but propose incongruent solutions. This is not surprising since the successful implementation of a standard can be an effective way of reconfiguring health care ontologies, by changing and re-defining what competences and tasks are relevant from within the work-practices. This has made it important to closely analyse the genealogies and implications of different standards (Timmermans and Berg 1997, 2004).

What then is the complication referred to above?

In spite of their many and often radical differences IS research, management studies and qualitative sociology often share a normative inclination. Thus some researchers want to find how out how to build better systems, some want to ensure economic efficiency, and some want to support emancipative projects. STS-studies, on the other hand, at least in its actor-network format, has often been cast as descriptive, for its methodical symmetry (which enables its taking into account simultaneously the technical and the social, rather than prioritising one over the other), is based on not evaluating actors’ accounts. Such “even-handed intolerance” (Smith 2002: 202) has
seemed to compromise the practical use of the STS re-description, and a number of attempts have therefore been made to go “beyond” this methodological premise (see Berg 1996 for a theoretical argument, Hanseth and Monteiro 1995 for a claim to empirically do so). While this may be a potentially valuable path for socio-technical standardisation studies to embark upon, it is also one, which emphasises a sub-set of STS ideas in order to procure its effects. Specifically, it strategically downplays the otherwise central concepts of heterogeneity and incommensurability that tend to always immensely complicate the question of “what is to be done”; a focus which would characteristically compromise the certainty of the normative benefits, which some researchers argue flows from such analyses.

While I generally sympathise with current STS-inspired approaches to analysing standardisation processes, I therefore choose in the following to explore a somewhat different set of questions, focusing on the variety and multiplicity of actors, both in terms of interests, strength, and technical, institutional, and political capacities, that partake in the networks working to standardise the electronic patient record in Denmark.

In this exploration I rely on Jean-Francois Lyotard’s concept of the differend. I choose this as a guiding concept for two reasons. First because, in accordance with the agenda outlined above, the differend is a concept, which consistently pays attention to the propensity of socio-technical interactions for taking place among incongruous practices with antagonistic aspirations. The differend is well suited to highlight this dimension of incommensurability, which, while it is also regularly acknowledged within STS-studies, is often subsequently repressed when the time comes to formulate normative benefits. I turn therefore to the differend as a concept, which is not already burdened with an extensive history of uses and abuses, as are many otherwise related ideas from actor-network theory and STS more broadly. By translating this philosophical concept into the area of IS and standardisation research I hope thus to be able to contribute to the increasingly sophisticated understanding of standardisation currently evolving by questioning the easy trajectory towards normativity and practical relevance.

2. Differend

As distinguished from a litigation, a differend [differénd] would be a case of conflict, between (at least) two parties, that cannot be equitably resolved for lack of a rule of judgment applicable to both arguments. One side’s legitimacy does not imply the other’s
lack of legitimacy. However, applying a single rule of judgment to both in order to settle their differend as though it were merely a litigation would wrong (at least) one of them (and both of them if neither side admits this rule)...[T]his...suggests...that a universal rule of judgment between heterogeneous genres is lacking in general” (Lyotard 1988: xi)

The differend, as defined above, is an instance of conflict, which cannot be easily resolved, because there is no common agreement about a rule that could be applied to decide on the contested issue. The differend therefore refers to situations of radical incommensurability between actors’ perspectives. Fittingly, Lyotard’s main example of a differend is the controversy about revisionist historians’ denials of the holocaust. But he does not propose this concept to differentiate between a static truth of the reality of the holocaust against a static falseness of the denial of it. Exactly the opposite is the case. He uses the differend as a concept to symmetrically destabilise all narratives that take for granted that such clear-cut distinctions exist naturally to be found and applied. Against this hope, Lyotard suggests that: “a universal rule of judgment between heterogeneous genres is lacking in general”. But if no meta-rule of conduct is given in a particular instance it can nevertheless be made. Barbara Herrnstein Smith formulates the point in the following way:

The resounding reaffirmation of an absolute distinction between truth and rhetoric, fact and fiction, science and superstition, will not in itself do the crucial substantive, technical and often arduous work of effectively differentiating among specific competing, conflicting claims of truth or between mutual charges of falsehood. Nor will a general affirmation of the inestimable value, irrefutable possibility, and transcendent ideality of genuine objectivity identify where, in any particular instance, objectivity lies – or (the pun is apt enough) "lies.” (Smith 1997: 29-30)

The relevance of this for standardisation research is not immediately obvious. I suggest, however, that a perspective on health standardisation processes informed by the logic of the differend, rather than other imaginable logics, such as, for instance, economic, rational, or clinical, offer a distinct vantage point from which to re-conceptualise many of the vexing questions such processes raise. These would include questions of how to understand the development of standards, as well as their migration and adoption. They would also be important in estimating which of these problems STS standardisation research can solve, and to which level of resolution.

Here I want to emphasise two points. First, the differend “inverts the idea of reality we spontaneously have: we think something is real when it exists, even if there is no one
to verify that it exists; for example, we say that the table is real if it is always there, even if there are no witnesses to the place it occupies” (Lyotard 1988: 32). While I have no interest in participating in philosophical considerations as to the existence of certain tables, the relevance of the inversion of realism advocated in Lyotard’s formulation is pertinent, too, when considering standardisation processes in which most participants would agree that nothing is as yet real, in the sense of stabilised and made to practically function.

What Lyotard’s clause adds to such a “common-sense” agreement is that it will also be impossible to fully steer or plan the development of standardisation by realist and/or rationalist assumptions, because the reality of a world in which standards have become effective, will be the outcome of the successful standardisation process (see also Latour 1987). Such inversion changes the question of how to plan and manage effectively into a consideration of “how the reality of the referent [can] be subordinated to the effectuation of verification procedures, or even to the instructions that allow anyone who so wishes to effectuate those procedures” (Lyotard 1988: 32). As a corollary to this point can be added a second one, which is that the often invoked needs of institutions, organisations, sectors, or individuals (such as patients), are not, in this view, given or found, but rather constructed in large measure by those involved in standardisation processes and the discourse that surrounds it (see also Cooper and Bowers 1995, Woolgar 1991). As Lyotard phrases it “and just as the flow of uses can be controlled, so can the flow of information…As a need is diverted and a motivation created, an addressee is led to say something other than what he or she was going to say.” (Lyotard 1988: 12).

Such emphasis on the constructive and formative aspects of standardisation processes shaped in interactions between sets of actors with potentially very different hopes and aspirations inevitably points to the broadly political dimension of standardisation. In fact, it is to foreground this dimension that the constant possibility of incommensurability is highlighted in the logic of the differend. Possibility does not imply that all processes studied empirically will be thus adversarial; neither does it imply that the logic of the differend is only relevant if they so are. Rather it can be viewed as a continual reminder that since difference rather than similarity tends to structure any set of interactions, then the success of such interactions must be viewed as collectively achieved results rather than as following from any sort of activities that could be
generalised or methodically specified. But reflexively this obtains even if one attempt to use STS analyses to generate change, for these analyses just add to the already contentious field, and there is no reason to view them as obviously more capable of planning beneficial outcomes (or even determining what these would consist of) than any other set of ideas. This makes the endeavour of normative STS studies rather more problematic than if they could function simply, like in a modern imaginary, as theoretically informed interventions. As I will exemplify, this problem accounts for the vagueness of a number of normative STS recommendations in studies of information systems and standardisation.

With these considerations in mind I proceed to develop the argument in the following stages. In section three I define standards, and discuss some of the levels of standardisation in Danish health care. In section four I introduce the formal European work of standardisation and in section five, likewise, I briefly present the Danish National Board of Health. In section six I indicate some of the quite variable activities of the standardisation and health care landscape, and their, sometimes contradictory, aims and measures. Following this, in section seven I discuss initiatives surrounding the development of electronic patient records more specifically. In section eight I indicate that while this development can be seen as illustrating a movement from incommensurability towards commensurability, it does not signal a stable resolution of the differentials of the Danish standardisation landscape. In section nine I return to the intellectual domain of standardisation research, in specific in its normative STS variant, and I review a number of ideas as they connect with points made in the present study. I briefly sum up in section ten.

3. Levels of Standardisation in Danish Health Care

In Denmark, the development of electronic patient records is currently one of the hottest topics within the health services. Debates on how these records are, and ought to be, developed often revolve around questions of standardisation. I discuss some of these debates, and their relation to the formal European standardisation procedures in more detail, but first offer I an intentionally broad characterisation of what a standard is, developed by Geoffrey C. Bowker and Leigh Star:

1) A "standard" is any set of agreed-upon rules for the production of objects.
2) A standard spans more than one community of practice. It has temporal reach as well in that it persists over time.
3) Standards are deployed in making things work together over distance and heterogeneous metrics.
4) Legal bodies often enforce standards, be these mandated by professional organisations, manufacturer’s organisation or the state.
5) There is no natural law that the best standard shall win. (Bowker and Star 1999: 13-4)

Although I do not discuss each of these points in detail, the following will demonstrate the relevance of these definitions. In particular I will qualify the fifth suggestion, which inverts a pseudo-Darwinist conception by pointing to the constructedness of standards. The logic of the differend can be used to argue that even though heterogeneous constructive processes are ubiquitous, no particular normative path follows from this insight towards better practices for standardisation.

To get a first grip on the standards at play in the development of the electronic patient record I initially present a reference-model constructed with that purpose in mind by the Danish EPR-Observatory. This model has three (and a half) levels of standardisation.

The user level relates to hospital organisation, work situations and information access. The intermediary level between users and software refers to interface-presentation and functionality. The logical and conceptual level concerns data-content and structure. Finally, the technical level has to do with system architecture, data formatting and transmission. All of these levels can be standardised in many different ways, but not all are equally contentious at present. Contentiousness can, of course, be defined in many ways, but here it may be related to the distinction between de facto and de jure, or formal, standardisation. De facto standards are often developed by market consortia whereas formal standardisation is what takes place under the quasi-democratic auspices of standardisation organisations such as ISO, CEN, ANSI and similar initiatives (see e.g. Lehr 1992).

If one connects this distinction to the model developed by the ECR-Observatory, the most unproblematic level of standardisation, from the point of view of many developers of electronic patient records, is the technical level. The reason for this is that standards for transmission are not domain specific; that is, they do not have to be
developed specifically with the health care sector in mind. This level is dominated by de facto standards developed by private corporations.

The situation is quite different, however, at all other levels of the model. The user level is neither formally nor de facto standardised, and although the Danish National Board of Health and hospitals are struggling with the question of how to ensure that correct information would reach only the correct persons a solution has not been forthcoming. Likewise neither de facto, nor satisfying formal standards are available at the level of interfaces. Finally, none exist at the logical and conceptual level. This latter level, which relates to issues having to do with data structure and semantics, have been taking up as of particular concern by formal standardisation bodies as it has been understood that “especially in Europe, where the information crosses management boundaries and, in many cases, regional and national boundaries, agreement on information content and message structures is necessary (de Moor 1994: 3). Clearly “harmonisation” of health care standards in Europe is a complicated business. How is it carried out?

4. The Formation and Work of CEN TC-251

In 1990, CEN (Comité Européen de Normalisation) approved the establishment of a technical committee on healthcare informatics (TC 251), which was subsequently split up in seven working groups (de Moor 1994). The first of these concentrated on specifying a logical information structure for medical records. This was by no means a simple task - and not just for conceptual reasons. When a CEN-standard is ratified, any task for which companies are invited to submit tenders has to be in compliance with the standard. Consequently, the standardisation work is of interest to a large and diverse group of people.

The TC 251 consists of members from the different national standardisation organisations. In Denmark, for example, Danish Standards set up a so-called ‘mirror group’, DS/S-273, to follow and participate in the work done at CEN. Members of DS/S-273 are from public and private institutions and organisations, primarily software companies and health care organisations, with an interest in being actively engaged in the standardisation work (DS Hæfte 4 1995). But there are other actors to take into account. CEN TC-251 has worked closely with the medical expert group of the European
Workshop for Open Systems (EWOS/EG-MED), the Western European EDIFACT Board’s Message Development Group for Healthcare (WEEB MD9) and many others. Internationally, the committee is in contact with ISO (International Organisation for Standardisation), ANSI/HISB (American National Standards Institute/Healthcare Informatics Standards Board), which is the American equivalent of CEN/TC 251, and has developed the well-known HL 7 communication protocol. Likewise there are contacts to Japanese and Australian standardisation initiatives. These contacts are rarely full-fledged instances of co-operation, but “exchanges of experience” (DS Hæfte 4 1995:27), which work to ensure that individual standardisation groups are not “re-inventing the wheel” (DS Hæfte 4 1995:54). Due to the extraordinary amount of actors and interests it is not surprising that formal standardisation bodies move slowly, as has, in fact, often been criticised by proponents of free market decision procedures (see discussions by e.g. Sherif 2003, Warner 2003).

In 1996, the first standard for electronic patient records, EHCRA (Electronic Healthcare Record Architecture) was ratified by the national standardisation organisations. This, however, was not a final standard, but an ENV; an Europäischer Norm-Vorausgabe, a pre-standard. A pre-standard, in contrast to a final standard is characterised by not carrying any legislative force (e.g. for the submission of tenders). It is, instead, delivered to national organisations, software companies and other interested parties for testing in practice over a two-year period, after which it is re-evaluated.

5. The National Board of Health

On a local level the National Board of Health in Denmark works to develop and implement health political initiatives coming from the government. In 1997, the board set up an office for medical informatics and became actively involved in the debates on electronic patient records. From the point of view of the National Board of Health the main issue was to ensure homogeneity of the standards adopted in Danish regions, which represent the political level locally responsible for the health care systems. Carrying out this task was made complicated by the fact that a number of concrete development initiatives were already under way in different regions. In the fall of 2000 the National Board of Health published a report on the proposed basic semantic structure of the Danish EPR. This structure has since been tested at two wards at Copenhagen Regional
Hospital\(^5\), which has resulted in the publication of a new version called G-EPJ 1.0, from December 2001. Meanwhile an improved pre-standard was also published from TC-251 in 1999.

6. Heterogeneous Aims and Activities in the Standardisation Landscape

The rapid juxtaposition of the TC-251 initiatives and some of those from the Danish National Board of Health makes visible the heterogeneity of the formal standardisation landscape. The CEN committee is putting a tremendous effort into standardisation yet this does not prevent the National Board of Health from re-formulating their results in the Danish context. Simultaneously, the Danish regions work on local projects, some of which started years before the office for medical informatics was set up at the National Board of Health. Thus the standardisation landscape turns out to be highly heterogeneous, as is well documented in other instances (e.g. Berg 2001, Graham, Spinardi, Williams and Webster 1995). Furthermore, this is an irreducible feature of the situation, which cannot be standardised or centralised away. For, indeed, the proliferation of groups, committees, initiatives, projects and organisations which span countries, continents and regions has partly been constructed as an attempt to reduce the acknowledged divergences and ensure broader co-operation between standardisation efforts.

But if this development was, at least in a sense, planned, why do we encounter the seeming incongruence between the concrete activities taking place in TC 251, the National Board of Health and the EPR initiatives at local hospitals? Moving closer to the local rationales, what seems like non-compliance from the distance, becomes less clear-cut. The National Board of Health is clearly interested in the work from TC 251, and has recently translated an abridged version of the latest pre-standard ENV 13606 in cooperation with Danish Standard. They are not trying to promote their basic structure of the EPR as an initiative opposed to the European standardisation work, which they are actively trying to ‘raise consciousness’ about in the Danish health services. Rather, they view their effort as a matter of re-specification. Precisely because of the internationally co-operative approach used in CEN, the standards produced there become abstract. It defines the terminology to be adopted, and specifies the general data structure through the use of UML (Unified Modeling Language). And this model could in principle be used directly in each of the Danish regions. However, the problem with this approach is that
the level of abstraction of the CEN-standard is so high that the probable outcome would be the construction of numerous local translations of it, which still might be unable to communicate with each other. The CEN-standard, then, ensures technical and logical compatibility, without for that matter guaranteeing compatibility in practice (see e.g. Bearman 1992). Ensuring practical compatibility, according to the National Board of Health, requires the specification of national semantic standards. This task cannot in principle be accomplished by international committees, because semantic standardisation has to reflect the actual organisation of the national health care sectors, which are exceedingly diverse.

For instance Denmark, as many other countries, relies on the latest international classification of diseases, ICD 10. The codes given in this classification, however, are translated into the Danish SKS-classification, which is used for standardised documentation of the treatment of patients in the National Register of Patients. Other procedures, for instance for surgery, are developed in co-operation with the Nordic countries, whereas x-ray and clinical protocols are made in Denmark. The National Board of Health defines itself as a crucial mediator between international standardisation efforts and the Danish health care sector, precisely because of the extremely complicated information infrastructure into which the EPR has to fit. Furthermore, the necessity of making local specifications is also acknowledged by TC 251. They explicitly limit their competence to develop standards, which are able to maintain and communicate medical knowledge without trying to delimit what a health care professional can or should do in context. As they express it: "TC 251 does not work with the content of a classification – classification is a job to be carried out by experts from the medical specialty" (DS Hæfte 4 1995:34).

7. The Development of Electronic Patient Records in Denmark
The 2001-report from the EPR-Observatory identified 52 Danish EPR-projects. Most of these were small, based in a single ward, some involved two or more wards, within a single hospital, a dozen were coordinated between several hospitals, and two involved all hospitals in their respective regions (EPJ-Observatoriet 2001: 7). Thus, local Danish hospitals were in full swing developing systems that were widely diverging – among themselves, and in relation to the European standardisation work (see e.g. Markussen and
Olesen 2001, Svenningsen 2002). In public debates in Denmark such initiatives were regularly referred to as irrational, selfish or political. They were seen as promoting the autonomy of individual regions at the peril of patient’s free choice of hospital, of improved inter-regional communication and presumed qualitative and economical benefits, which would follow from having a single national system. But the regions used different lines of argument in defence of their dispositions.

As we have seen, EPRs seriously entered the political imaginary only in the mid- to late-nineties, while a number of clinicians and informaticians had been spokespersons for their relevance years earlier. The sudden high profile of these systems made money available for development projects, which it would have been pointless to let slip away. Furthermore, official state-funded projects like HEP (Action-plan for the Electronic Patient Record), actively encouraged local initiatives and experimentation with the construction of such systems even though they emphasised that standardisation would have to follow. Late in 1995 they wrote:

> Although initiatives have now been taken towards national standards for electronic patient records, there is no reason to stop local or regional initiatives. The IT-development moves very quickly, and slowing down initiatives already effectuated could cause the loss of accumulated knowledge along with the commitment of the people working on the cause (Sundhedsministeriet 1996: 54)

But furthermore local developers point at the question of speed, or lack of it, of formal standardisation. In this they replicate many other proponents of market driven generation of standards, but the point is more specific than that. As mentioned, some have been working with smaller development and implementation projects for years. For them the National Board of Health has moved exceedingly slowly, not even setting up an office related to the problem of electronic patient records until 1997. And it remains the case that although TC 251 has been in existence since 1990, they have yet to ratify a final standard for the document architecture of the EPR.

For these reasons, local developers aimed at making their own systems work, even though on a less grand scale than sometimes imagined by politicians or in standardisation committees. They wondered, for instance, whether it was strictly necessary to create inter-regional compatibility between systems in Denmark, when the number of patients that ever cross the border of a region is comparably diminutive. One interviewee
suggested that “making an EPR does not un-invent the telephone or fax machine”, thus making explicit a peculiar assumption behind many IT-visions; that IT not merely adds to or transforms the existing information infrastructure but, as business process re-engineering would have it, effectively obliterates it (Hammer 1990).

In 2001, then it seemed manifestly unclear whether money spent on formal and national standardisation were well spent. In principle this could have opened a political debate regarding the expenditure of money and effort in a health care sector already hard pressed but this never happened.

In the EPR-report from 2001 the above-mentioned incongruities led the authors to suggest that detailed migration-plans and validation-plans, which would describe how the proposed national standard from the National Board of Health could be validated against the heterogeneous systems then being developed, and how it would become capable of travelling and entering into these emerging information infrastructures. However, no initiatives were taken in this direction, while political and economical pressure was applied to ensure compliance from “rogue regions”. The result, at present, is that several regions have put their projects on hold, while a few high profiled projects, notably in the Aarhus and Copenhagen Regions, continue the development of sophisticated “2nd generation” EPRs. Depending on the perceived success of these projects other regions are expected to either adopt the models there developed, or at least ensure compatibility with them.

The above discussion points at some of the many layers and complications of standardisation, which is shown to be far from a monolithic ‘thing’ that can be imposed on a practice. Instead, in historian Simon Schaffer’s, felicitous phrase: ”Standardisation [is] at once the technical obsession and the political and moral problem” (Schaffer 2000: 78). Where does this lead STS studies of standardisation?

8. From Incommensurability to Commensurability: A Solution to the Differend?

One of the central tensions in creating and achieving universalization is the relationship with past infrastructures, procedures, and practices. Standards will attempt to change and replace those practices but...the same standards need to a certain degree incorporate and extend those routines. To understand the ‘universalization’ of standards, it is crucial to look at these processes of incorporation and transformation. (Timmermans and Berg 1997: 2-3)
Above, I have described a number of such attempts at incorporating standards and transforming work-practices, undertaken in the complicated networks of Danish and European health care standardisation. These attempts take place on different levels not all of which fit together nicely. In “Irreductions” Bruno Latour suggested that “Everything may be made the measure of everything else” (Latour 1988b: 158) for nothing is, in itself, either compatible or incompatible, with anything else. If this is the case, what matters is the concrete and practical work carried out to create an agreement or coordinate an effort.

The work of negotiation enables the temporal emergence of associations and alignments between actors’ that were hitherto unrelated. Although hard to accomplish this work is, in principle, capable of constructing commensurability out of incommensurability. This can be seen, for instance, in Timmermans and Berg’s work on standardisation, which focuses on what happened when two new standardised medical protocols were put to use. They found, that such protocols did not impose on work routines a completely new order. Rather, they were implanted into an assemblage of human and non-human actors, the orderliness of which was reconstructed by their entrance.

Such slow stabilisation around formal standards is recognisable in the Danish health care landscape, where initial “chaos” has been replaced with an order of sorts, as a number of political interventions have taken place to make sure that regions follow the guidelines of the National Board of Health. As I have discussed, one consequence of this imposed orderliness, is that a number of regions have put their projects in a waiting mode, to see if the large Aarhus or Copenhagen projects materialise satisfactorily, which presently seems uncertain.

The current situation has thus enabled the emergence of a certain commonality in choice of standards, although some find that the price of this order is excruciating slowness and others doubt whether the intended efficacy will materialise as a result. Undeniably, one result is an enormous complication on the level of organisation, where CEN, national “mirror-groups”, the National Board of Health, but especially local regions and hospitals each have to transform their practices to accommodate their interactions, the latter often having comparatively few resources to effectively do so, and regularly also lacking the incentives. Thus, the fact that a measure of stabilisation and network
convergence can be found around Danish standardisation initiatives does not imply that
differential views on why and how standards are needed have been worked out, but rather
that a differential case can be temporarily “shut down” by political fiat.8 In contrast a
resolution of a differential situation would have to be constructed in response to a
recognised as involving multiple, sometimes incommensurable agendas. Needless to say
this is very hard to accomplish, and if it happened one could properly characterise it as an
“event” in the Danish health care information infrastructure.

The question remains, however, where one may go with an insight into the
complexity of the work of standardisation? There are various answers to this both in- and
outside STS, and I now point to a number of suggestions, primarily within recent
normative STS research.

9. Normativity and the Logic of the Differend

It seems that many different people can agree that standardisation is an important issue,
however their evaluation as to its kind of importance vary. For some recent discussants
the benefits of standardised IT systems are obvious but it remains up to the users to reap
them (Chauvel 2003). Others, realising perhaps that standardisation is in the least a
complicated and variable affair, offer typologies and distinctions concerning the
possibilities and difficulties of different standards or ways of standardising. Under the
headline “Surrounded by Standards, There is a Simpler View”, for example, information
scientist Judith A. Molka, developed a classification based on four functional areas of
interoperability; this is, putatively, the simpler view.

But as Molka is driven to acknowledge this view is not that much simpler, even
though the admission is hedged by claiming that this will only remain the case “in the
near future” – “While much is being done to simplify the information technology
standards environment, in the near future it is likely to remain complex” (Molka 1992:
530). However, nothing that has happened since the early nineties would lead one to
believe that we are approaching this simpler future.

In this light a number of recent studies by researchers who are well versed in the
technicalities of several different aspects of IT and standardisation development and
educated in social theory is a welcome development. In an ongoing dialogue at the
boundary of IS research, medical informatics and science and technology studies, such
scholars as Marc Berg, Geoffrey Bowker, Randi Markussen, Leigh Star, Lucy Suchman, and others have done much to make researchers appreciate the strengths of inter- and cross disciplinary learning (see e.g. Berg 2001, Bowker 2003, Markussen 1996, Star 1992, Suchman 1987).

What has come out of these efforts? In his recent “Implementing Information Systems in Health Care Organisations: Myths and Challenges”, Marc Berg, for instance, delineates a set of problematic assumptions guiding many practical development and implementation efforts, and outlines an agenda of alternative questions, challenges and possibilities based on a socio-technical approach. He highlights the embarrassing fact that “there are many more failure stories to tell than there are success stories” (Berg 2001: 143), but emphasises also that the question is not so much to evaluate failure and success as such, as to become aware that those very criteria are necessarily variable and multidimensional. He therefore calls for an appreciation of the fact that success can never be measured statically, but only as a consequence of the mutual transformation and alignment between the new technologies and the organisations and work practices they are meant to improve (I discuss this analysis in more detail in chapter twelve).

If this is the case then how can standardisation research in STS be used to guide practice? This is a difficult question, which moves into a normative terrain from which STS-studies often stay aloof. Socio-technical approaches clearly cannot view properly standardised EPRs as automatically beneficial change agents. But in the attempt to move away from non-critical analysis and description and towards critical and practical intervention, such research seems to sometimes posit its authors as replacing the technologies as enabling the accomplishment of productive change.

This could seem a rather backwards move and normative pronouncements, in the form of predictions, visions and suggestions also tend to be uncharacteristically vague and woolly compared to the detailed investigations leading up to them. For instance, under the headline “Beyond Actor-Networks”, Monteiro and Hanseth (1995) conclude merely (and, I think, clearly fallaciously) that symmetrical ANT studies must be superseded because they cannot “properly deal with institutions, i.e. how they shape actions as they very same actions shape the institutions”. In a curious analysis they suggest that: “another important aspect of INI [Information Infrastructures] which ANT neither can take into account (sic) is their openness”. Hanseth, Monteiro and Haitling
(1996) develop this conclusion further with the surprising suggestion that symmetrical analysis should be suspended and replaced with a focus on “types of technologies”, because symmetry encourages the study of “bicycles, hamburgers, work practices, professional concepts and hotel keys with basically the same tool-kit”, thus re-stating the argument, familiar from various criticism of ANT, that such studies tend to erase all difference. Finally, drawing on structuration theory as a corrective to this perceived problem, they advocate “an intermediate position which comes close to “soft” versions of technological determinism”.

The obvious problem with this analysis is that the authors understand the ANT principle of symmetry as an analytical end-point rather than as a methodical starting point. The symmetry doctrine as methodical starting point disables researchers from adopting simple technological typologies, precisely because a typological endeavour allow practically encountered differences to be submerged because a set of entities have been defined as of the “same type”. In contrast, one starts with principled symmetrical equivalence because one wants to find differences, as all those studies of bicycles and hamburgers have in the main done. Of course, developing a capacity for typological generalisation is just what is needed if one wants to facilitate normative pronouncement. But in just this move the variability of practices is lost, and with it, the empirical grounds for adopting a specific normative stance.

Rob Kling has proposed that: “assessing people’s preferences for systems’ designs is an exercise in social inquiry” (Kling 1998: 55). Marc Berg (2001) suggests that: “the most “successful” implementation processes appear to be those in which an obsession for control and planning is replaced by an obsession for experimentation and mutual learning” (Berg 2001: 154). I agree with both of these formulations, but in my view it can lead only towards a consistent insistence on the differential sites and practices of ICT development and implementation.

However, for Berg and his collaborators, they ought to lead to “empirical normative analysis” (Berg, Ter Meulen and Van Den Burg 2001). What does this mean? The article carefully discusses different kinds of empirically observable normativities in Dutch health care and notes that there is a tension between formal and practical rationalities, which: “will only be fruitful …when the ‘messiness’ of practical rationality is not condemned from the clear-cut order of formal rationality” (95). But arguing for
recognition of the messiness of practice is hardly the prerogative of an especially normative strand of STS; it could be said to characterise almost all studies cited in this chapter. Why then claim a specific normativity?

The solution of a differend; that is, the temporary stabilisation of a concept, a practice, or a network consisting of many not naturally aligned actors, with vastly different hopes and aspirations, can only work by a creative (“eventful”) collective re-definition of the stakes of all involved parties, it neither can nor should be the prerogative of the particularly insightful STS-student to formulate how this is to happen. If it is unfair to claim that Berg and his colleagues’ insistence on the productive messiness of health care practices signals such grand ambitions, as I think it is, this raises again the question of why it is important for them to figure their work as more normative than all those other STS-studies making visible practical complexity.

10. Danish Health Standardisation Revisited

The logic of the differend is the starting point for an analysis, which cannot assume a common logic between actor’s perspectives and does not hope for one. It acknowledges that actors bring to a situation radically different hopes, experiences and expectations, and it is they who account for the diverse understandings of a given project, rather than some actors’ failure to grasp the potential of, for instance, European standardisation initiatives or their failure to grasp the implications of, for instance, a complicated set of moves in social theory.

The standardisation efforts I have discussed are consequential for the Danish health care services, and will probably become more so. As solutions to technical, social and organisational problems are progressively folded into standards, it is likely that their adoption will entail change on a number of levels of the Danish health services. Different solutions to the problem of standardisation implies different visions and understandings of what the health services are and should be. They involve different distributions of competences and tasks, and different professional relationships. In short, different standardisation efforts entail different ontologies for the health services of the future.

As I have emphasised above, the current convergence of Danish EPR development glosses rather than engages this fact, and the consequence as it can be evaluated so far is that smaller Danish regions, hospitals and wards, bears the burden of standardised IT-
visions on their shoulders, without being in any particular way re-compensated for their economical and organisational efforts. In spite of the legislated process of increasing homogenisation of Danish EPRs, through adoption of the standards developed in CEN and transformed by the National Board of Health, it is therefore by no means clear that “success” will obtain or, in other words, that the differend here in play will be solved.

In their definition of standards Bowker and Star emphasised that “There is no natural law that the best standard shall win”. The structure of the differend as I have presented it, indicates that there is not even a natural law, which enables one to evaluate whether one standard is better, or more relevant, than another. Since some standards and some systems nevertheless come to look manifestly superior or, at least, without alternative, I would suggest that the continual highlighting of the differential processes of construction by which this happens is one of the most productive ways for STS and social theorists to participate in the current standardisation debates and efforts. For while no law of successful standardisation is given, many possible ones can certainly be imagined and, given a sufficiently creative and flexible process, perhaps even made.

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1 This statement has been perceived as highly offensive by some readers; as arguing that all things technical were boring. Rather, I was sharing the concern of Jacobs and Spring and Lunin, cited above, that in spite of the interest of standardisation this research is in fact often perceived as too technical and difficult, and therefore too boring by an audience, which it might, and perhaps ought to interest. The question, then, is how to change this situation.


3 The EPR-Observatory is a Danish project under the auspices of V-Chi (Virtual Center for Health Informatics) funded by the Ministry of Health, with the purpose of surveying the developments initiated in Denmark with the purpose of making an EPR.

4 An obvious analytical move from the point of view of STS would be to deconstruct precisely such models in order to point to the technicalities of what is taken for granted as the social level of the model and show the many layers of social embedding, which enters into the putatively technical level. In this paper, however, I take seriously the discourse of medical informatics, which generates these distinctions, and follow the threads of one of the levels of the model. This approach, too, quickly points to the co-construction of the technical and social, albeit from a somewhat different angle.

5 The pilot-wards were Thorax surgery R and Cardiology P.

6 The national register of patients contains approximately 40 million pieces of information about patients. It is the duty of all public and private hospitals in the country to make monthly reports of all hospitalisations, ambulatory treatments and visits to the casualty wards to LEC [the software company
running the register]. The national register of patients is a unique register, in Europe and world-wide, because it contains patient data (diagnoses, surgery, treatments etc.) of high quality, covering a time-span of more than 25 years (http://www.lec.dk/indhold/lec9072.htm).

7 For instance by the National Board of Health, or by the Danish Board of Technology, whose former president Erik Bonnerup has declared: "Then each region has to invent the wheel for themselves, ten times. A central system is also more flexible as regards staff. With a centralised system, the nurse from Hjørring [a provincial town in Northern Jutland] can go to work at the National Hospital [in Copenhagen] without having to take a course in its patient system. (http://www.prosa.dk/Nyheder/sundhedsvaesnenet270401.shtml)

8 The term moralising moment was introduced in chapter four to identify such instances.

9 A similar point is made by Gerson and Star who emphasise the importance of articulation work in order to obtain what they term due process in organizational work. In an expression closely related to the points I make in the current paper, they write: "However, in complex systems, the procedures by which one solves problems themselves become objects of adjudication. Because there are independent rationales for answers and because there is no central control mechanism, there is no way of determining a globally correct answer. Rather, there are multiple, possibly inconsistent, competing answers, none of which has a unique claim to validity" (Gerson and Star 1986: 263).

10 This point is made by Geoffrey Bowker in the broader context of a survey of "The New Knowledge Economy and Science and Technology Policy". He writes: "So standards are necessary – from social protocols down to wiring size. Indeed, when you foreground the infrastructure of your lives, you will see that you encounter thousands of standards in a single day. However, the development and maintenance of standards is a complex political and philosophical problem; and their implementation requires a vast amount of resources. Standards undergird our potential for action in the world, both political and scientific; they make the infrastructure possible” (Bowker 2003).
Chapter nine:
Citizen Projects and Consensus Building at the Danish Board of Technology: On Experiments in Democracy

1. Introduction

[T]he Danes have pioneered a practice of establishing panels of ordinary citizens, selected from a pool of people who indicate an interest, but not professional expertise or a commercial or other organized stake, in an area of technology…The particular conclusions would not please everyone, and the process is not perfect. [But]…the process embodied in the consensus conference is part of what I mean by fostering situated knowledge. (Haraway 1997: 95-6)

The Danish (and Scandinavian) tradition of encouraging citizen deliberation on large technoscientific projects is regularly remarked upon positively by commentators, as a counter-point to the highly techno- and bureaucratic procedures used in a number of other countries (Bijker n.d., Einsiedel, Jelsøe and Breck 2001, Haraway 1997, Rip, Misa and Schott 1996, Sclove 1995). In the introductory citation, for instance, Haraway uses consensus conferences as an example of how to construct situated knowledge about technoscience. Among other things, situated knowledge is characterised by not disowning the circumstances of its production, but viewing them as generative of its strengths. Evaluating the extent to which actual citizen projects succeed in thus situating their own knowledge production is hard, however, since there have been no sustained empirical investigations of the concrete workings of such participatory projects, at least as carried out by the Danish Board of Technology.¹

In the fall of 2002 the board carried out a project on the development of electronic patient records (EPRs). As is customary this resulted in the writing of what is called the consensus document, which was made public at a press conference on October 7th, 2002, and subsequently handed over to the Danish Parliament as a policy recommendation. This paper is the result of my involvement with the EPR-project from its inception in the beginning of 2002 and until the final report was produced and presented to the public in October that year.

I view the project as a democratic experiment with a potential capability to generate relevant, situated knowledge through the interactions of citizens and experts.² Characterising it thus does not render its actual development unproblematic. Specifically
it does not prevent the formulation of a number of challenges to the practical organization of such projects in their current guises.

The Board of Technology views democracy as exercised by citizens as they get together and make an amount of esoteric knowledge their own; this is presumed to enable the construction of a specific citizen’s viewpoint. Thus, the many different kinds of interactions that took place during the EPR-project were framed with a specific conception of democracy (e.g. Grundahl 1995, Klüwer 1995). The board’s choice of how to engage citizens also involves a specific conception of what it means to learn. For it is the citizens’ interiorisation of expert knowledge that is viewed as enabling the formulation of their singular perspective on the case. Concepts of democracy, learning, and expertise are thus put in play to practically determine the sorts of engagements that are proper between experts and laypeople when one’s goal is the production of a consensus.

In a phrase of Annemarie Mol, the uncovering of the relationship between abstract ideas and empirical modes of execution can be termed empirical philosophy,. This term points to the constant practical entanglement of seemingly abstract concepts, and it works to show how such entanglement produces specific effects that may be quite different from what would be expected from merely conceptual analysis:

Philosophy has a rich tradition of painstakingly seeking to establish standards for ‘the good’: good technology; good knowledge; good management; good policy; good action. Here, we work differently. Instead of seeking to frame ‘the good’ ourselves, we explore how others go about this task. For this is an everyday activity. Attempts to differentiate between errors and achievements, failures and successes, falsehoods and truths, problems and solutions, or catastrophes and triumphs (the terms vary), are not the prerogative of a specialist academic discipline. Most everyday practices make use of, or try to create, scales to measure or contrast ‘goods’ and ‘bads’. This opens a space for an empirical philosophy. An ethnographic interest in practice can be combined with a philosophical concern with ‘the good’ to explore which ‘good/bad’ scale is being enacted, and how this is being done (Law and Mol: http://www.comp.lancs.ac.uk/sociology/soc090j1.html)

I give a general introduction to the work of the Danish Board of Technology. I then describe how my contact with the board was established and how I became actively involved with the EPR-project. In the following sections I discuss the background of this particular project, how it was carried out, and which conclusions were reached. These discussions are framed in the context of the above-mentioned questions of democracy and learning.
2. Citizen Projects as Democratic Experiments

The following text is intended not only as a quasi-ethnography of a specific project carried out by the Danish Board of Technology, but also as a discussion of the possibilities and limits of such projects as instances of *democratic experiments*. What does this term imply?

A minimal definition of democratic situations, according to Isabelle Stengers, is that they are situations in which "confer[red] on individuals the capacity to take a stand".\(^3\) As is well-documented and criticised this criterion is rarely met in technoscientific practices (e.g. Wynne 1992, 1996) but the projects of the board of technology can be viewed exactly as experiments in how to give voice to citizens in such situations. Viewing these projects as experiments, however, is in opposition with the self-understanding of the board, which considers projects as validated by their consensus-creating method. In contrast, the notion of experimentation implies an exploration of the unknown, which cannot be methodically guaranteed.

This view of experimental democracy does not enable the researcher to eventually evaluate the suitability or success of the concrete suggestions made by the participants in the project, because what counts as a suitable result is defined just by the experiment. But it does allow one to look closely at the interactions and events of the project and analyse which factors worked to increase the open-endedness of the interactions between members of the board, citizens, and experts, and which factors on the contrary worked to delimit the flexibility, and thereby the capacity to respond to the situations at hand.

3. The Danish Board of Technology

The board of technology was established as an independent institution in 1986. In 1995 the board was organizationally re-arranged.\(^4\) The original board of 15 persons was abolished and a structure with a governing body of 11 people and a council of 50 was put in its place. Daily operations are run from an office in central Copenhagen, where 22 employees are situated, among them ten project managers, a number of secretaries and several student helpers. The board has a number of functions and methods, which I will touch upon below, but in general the aim is to:
disseminate knowledge of technology, its possibilities and consequences, for humans, society, and environment. The Board of Technology therefore promotes debate about technology, assesses technology, and advises the parliament and the government in technological matters. (http://www.tekno.dk/subpage.php3?page=statisk/dk_om_os.htm&toppic=om_os, my translation)

The Ministry of Research supervises the board, and contact to Parliament is established through their research commission.

Citizen involvement and the promotion of democratic procedures in policy deliberations having to do with technological issues, broadly defined, are key concerns of the board. A range of methods, some developed by the board and others adapted from other countries, are used to draw on and articulate citizens’ perspectives. These include “future panels”, ”hearings of citizen’s groups”, ”perspective workshops”, ”future search conferences”, ”questions and answers”, ”policy exercise – role play”, ”interdisciplinary work groups”, and ”consensus conferences”.

Every year the board makes a call for project-proposals. Proposals may be submitted by MPs, authorities, organizations, corporations or citizens. The complete list of proposals is discussed and sorted by the directorate. A few of these are selected as ”full-scale” citizen projects. Other projects are chosen for thematisation in the publication ”Technology and Debate” or in a newsletter for the parliament, called ”From Board to Parliament”.

In 2002, six projects were chosen for citizen involvement. These were: ”GMO’s and the third world”, ”How are we going to assign value to the environment?”, ”Hydrogen in an unremitting energy system”, ”IPR/Copyright”, ”The Cities – a fine place to live”, and ”The Electronic Patient Record”.

The institutional foundation of the board was destabilised in the spring of 2002, after a Danish right-wing coalition took over government, and decided to cut funding for 99 boards and committees, many of these having to do with progressive social issues. Rumour has it that the Board of Technology, which was on the so-called ”death-list”, was saved in the last second, when a prominent and technology-friendly M.P. from the governing party Venstre threatened to resign if the board was abolished. In any case, the board was saved, although its funding was cut with almost 33%, from app. 15M Danish Crowns annually to app. 10M.

"Citizen Projects and Consensus Building at the Danish Board of Technology” 159
4. The Electronic Patient Record

How has the development of electronic patient records come to be defined as broadly relevant to the Danish public? As discussed in chapter seven one can point to a range of political, technical and administrative issues, which in combination have helped to make probable such a definition. In 1995, the then Social Democratic Danish government published the first national IT-strategy called *Info-Society 2000*. This report emphasised the necessity of radically upgrading research and investment in the development of a Danish information infrastructure in many different sectors, health care included. At this point, the medical informatics community was working on a number of smaller projects on using computers to enhance clinical and administrative processes and products. With the publication of the *Info-Society* report, interest in such work greatly increased, as did funding possibilities, not least from the regions. From this time on, a number of pilot-projects on developing small EPR-systems have been going on in Denmark. And, increasingly, hopes have been placed in such systems to improve on the health sector, which is seen as economically ailing, and putatively in poor health as regards administrative efficiency and clinical quality.

At the same time a number of state financed IT-projects have been presented as scandals in Danish media. Most famous among these is the *AMANDA*-case, a system developed for the Labour Market Administration, which was delayed for more than two years, and whose price eventually went from the estimated 268M Danish Crowns to app. 500M. Present discussions about the development of EPRs in Denmark often revolve around the question of how to ”prevent another AMANDA”. But suggestions on how to achieve this goal vary. Thus, EPRs are presently a hotly debated political issue. The media regularly quote gloomy predictions of technicians and clinicians regarding the consequences of the present development. Contrarily, the developers of these systems speak for their excellence.

Many observers claim that, at best, an EPR would become a resource for the health sector on several levels (e.g. Dick and Steen 1991). It could prevent double-entry of data in the medical record, KARDEX, and other records. It could offer decision-support to the clinician, and minimise the possibilities of medication errors. It could support cooperation across professions, because a single electronic record would replace the many
paper-based ones. It could also be connected with existing administrative systems and supply structured data for research databases. Finally, the EPR could ensure a smooth flow of data between the primary and secondary health sectors, and across regional borders.

State agencies, prominently the National Board of Health, have criticised current projects for their lack of integrative ambition. In their view, the biggest advantage of the EPR lays in the demand it places on clinicians to standardise and structure data. So if the EPR is to make administration more effective as well as improve research possibilities, a conceptual standardization of clinical communication is necessary. Conceptual clarification is done by the National Board of Health and they view the implementation of its model as a *sine qua non* for the success of the EPR in Denmark.

However, this vision is answered with critical questions from the regions in charge of current development projects. They object that the development of integrated systems, which would be able to deliver the above-mentioned benefits, would demand an injection of time and money which is not available, and which furthermore is exorbitant in comparison with the many important hospital tasks that are already lacking in funding. This type of argumentation is relevant when considering the regularity with which large IT-projects are too late and too expensive yet nevertheless yield too little, when compared with the visions underlying their development (Berg 1999).

As can be ascertained from the long list of problems to be solved by the EPR, high hopes are placed in these technologies. It is clear, then, that the EPR is often viewed as a technical solution to a number of complicated, and interwoven, political and organizational problems. In this sense, visions of EPRs often frame such technologies, in the words of Sheila Jasanoff, as *technologies of hubris*, which show "a kind of peripheral blindness toward uncertainty and ambiguity" (Jasanoff 2003: 238).

A number of important questions regarding the kinds of health services "we" want in Denmark are raised in these public and political debates. But it is worth noticing that the "we" invoked, is a specific (and peculiar) one. For, to a very large extent, the horizon of the discussion was defined by a distinct set of actors: clinicians, technicians, and administrators. Evidently all of these have indispensable functions in making the health care sector function in a satisfying manner. However, it could be argued that there is another group to whom the question of how to make health care work is exceedingly
relevant: the citizens. These are, of course, the actual or virtual clients of the health sector. And this was in fact the argument brought forth by the Danish Board of Technology, who remarked upon lack of citizen involvement in these debates and consequently reviewed a proposed project about the EPR positively.

The EPR qualified as a citizen-project by meeting a number of the criteria of relevance for such activities in an exemplary manner. These criteria included: that the theme was of current relevance from a social point of view; that the theme was technical, controversial, and complex; and that there was a need for defining attitudes and goals in relation to the topic.

5. Consensus-Conferences and Development Spaces

The method to be used for the EPR-project, called development spaces, was determined by the project manager, prior to inviting experts to join the background group, which help with the detailed planning. Development spaces are based on the so-called consensus-conference model, which has been used at the board of technology since 1987, and has been exported to a number of countries. In a sketch, consensus-conferences work in the following way. A number of experts in various aspects of the given problem area write up introductory material, which is then sent out to a citizen panel. The panel meets and discusses this material carefully during two weekends under the supervision of a project leader and a process-consultant, who helps facilitate productive interactions between the members of the citizen group and the invited experts. The result of the two weekends is a document in which citizens formulate a number of important questions they need to have answered in order to reach consensus and make recommendations. New experts, with different perspectives on the topic, are called upon to answer citizens’ queries at a consensus-conference held during the third and final weekend of the process. After the conference, the citizens’ panel write up a final document with their findings. This consensus-document is subsequently presented to the parliament as a policy recommendation.

Development spaces are rather closely modelled after consensus-conferences. Here, a background group is also formed by a set of experts, with the purpose of writing the introductory material for the panel. Likewise, the final goal of the panel is the construction of a consensus-document. Finally, citizens also meet for two weekends,
before the final consensus-seminar. The difference lies in the organization of the interactions between citizens and experts. In contrast with consensus-conferences, citizens’ deliberations during their weekends are not based primarily on the introductory material initially produced by the background group. Instead, citizens’ are confronted with a set of live presentations from relevant experts during each weekend. As a consequence of these interactions, the panel is encouraged to re-construct the preliminary set of relevant questions proposed by the background group and return it to this group. The job of the background group is then to interpret the re-articulated statement, and respond by choosing experts able to relevantly answer the questions posed by the panel, during the coming weekend. The difference between consensus conferences and development spaces therefore lies in the temporal distribution of the engagement between experts and lay people. Whereas consensus conferences maximise engagement at the end of the process, development spaces spread out the interactions throughout the process. This method is used in projects that are supposed to be technically rather than ethically complex.\(^{14}\) In such situations regular expert participation is viewed as necessary because their absence might hinder the ability of citizens to understand important aspects of the topic.

A more mundane consideration to take into account when deciding on method is cost. Consensus-conferences, which take place during the final weekend of such projects, have a large contingency of media representatives, politicians, and experts to cater for, and cost app. 100.000 Danish Crowns, out of a project budget of roughly 600.000 Danish Crowns. In contrast, Development Spaces are cheaper, budgeted around 400.000 Danish Crowns. With the recent drastic budget cut such economical considerations, presumably, became more likely to impinge on the methodical decisions.

6. Selecting the Experts, Planning the Project

I visited the Board of Technology in late February 2002 to interview the project manager about the upcoming EPR-project. Shortly after I received an e-mail asking whether I would be interested in actively participating in carrying it out. I thus, rather accidentally, came to figure as an expert in EPR-matters. How was this possible?

For methodical reasons, to be discussed below, the board is committed to putting together background groups with participants that are as diverse as possible. This
selection poses a problem since the project manager cannot by definition be a specialist in the area of investigation; for this would be to bias the project from the beginning. Experts are instead chosen after the project manager, along with assistants, carry out an informal search to uncover which networks are involved in the discussions relating to the given area of problems.

In the original proposal, the background group for the EPR project would consist of the following members: a patient representative, a member of the Danish Council of Ethics, an IT-representative from the EPR-Observatory and V-CHI\textsuperscript{15}, and an IT-humanist and specialist in knowledge management. This composition was changed for a number of reasons. First, the idea of having a patient representative participate was abandoned because citizens in themselves are potential patients and, accordingly, should not be represented in the group of experts. Second, the Council of Ethics did not want to participate, as they viewed the content of the project as primarily technical.\textsuperscript{16} Finally, the so-called IT-humanist eventually participated in only one meeting. As replacements were chosen a surgeon turned IT-Consultant, a representative for the IT-political committee of the Danish Medical Association and myself, who, then, would be the token representative of the humanities and social sciences. My role was rather special in other ways. First, because I identified myself at the introductory meeting as knowledgeable about certain aspects ("social \”, "humanistic" rather than "clinical", "technical") of relevance for a broad thematisation of the EPR, but simultaneously as carrying out participant observation. Second, because I was a Ph.D. student among a group of professionals and tenured professors. These ”overheads” interacted in the micro-processes of the meetings and certainly influenced my felt ability to engage equally with other members in this setting.

The project was introduced by the project manager and the director of the board. They stressed the role of the board as a transparent mediator between citizens and experts, as this position enabled the board to maintain public credibility. According to the board, credibility could be maintained precisely to the extent that the experts in the background group verifiably and visibly represented heterogeneous perspectives on the technical, social and ethical implications on the EPR. The Board of Technology thus presented itself as the democratic anchor, enabling unprejudiced communication and transmission of knowledge between experts and lay people. In this aspect as in the goal to achieve
consensus, the methods of the board is influenced by a problematic Habermasian conception of democracy and the public sphere.\textsuperscript{17} However, as I will discuss at length below, the actual, rather than abstract, happenings of the EPR project opens a space for a reconstruction of the democratic potentials of such projects.

The purpose of the background group was to discuss how to pertinently present the complexities, problems, and possibilities of EPRs to a citizen panel. Our initial job was to construct a problem catalogue, with a number of loosely defined themes. Relevant themes were those about which substantial disagreement or ambiguity existed, either in the background group or more broadly. We also had to write up a question catalogue, posing questions, which were relevant to the formulated problems. This catalogue would function as the dynamical mediator between the background group and the citizen panel, since the citizens would have to re-formulate its content in response to the expert input they received during their first weekend meeting. The catalogue would thus be the main material facilitator of the iterative process that experts and citizens were to engage in.

Finally, we were told about the selection criteria for the citizen panel, as the background group would have to approve of its composition. This description pointed out that while it is obviously impossible to involve all citizens in a project, it is statistically almost as difficult to make a strictly representative selection of citizens.\textsuperscript{18} The board therefore aimed to create a many-sided forum for debate, exchange of opinions, and consensus, rather than to claim "scientific" representativity. One-thousand citizens, randomly chosen from the national register of citizens, received an invitation to participate in the project. To be legible they had to write an application explaining why they would be suitable citizen representatives. Applications would then be filtered according to a number of criteria. Obviously interested parties, such as software developers from companies involved in the development of EPR-equipment, would be removed. So too, would so-called "hidden experts", such as GPs, who could not avoid knowing about some of the relevant problem areas, even though they were not strictly involved in them. Finally, clearly mad people, a number of which are said to always apply, would be removed. The remaining contenders would be filtered to ensure a broad composition of the panel, as regards gender, age, education and geographical representation.
7. Introductory Material

We met again early in June. In the meantime, the background group had been further expanded with a nurse from Svendborg Hospital. In between meetings, a reporter had used the input from our first discussions and subsequent e-mail exchanges to write down a draft of the introductory material for the citizens. The purpose of the meeting was to clarify and qualify this material, and to come up with suggestions for other themes and questions. In their entirety, these exchanges can be viewed as the response of the background group to the question of how expert knowledge about the complexities of the EPR could be represented adequately and succinctly to the public. The response of the background group had its own dynamics, which related to the composition of the group and the interests of its members.

During the first meeting it had been quickly agreed upon that the question of whether an EPR was needed at all was futile. It was hard not to reach this conclusion with four out of the five participants actively engaged in its development or promotion. Thus, questions came to revolve around what sorts of benefits an EPR might be able to provide, and how this might happen, rather than around the arguably equally important discussion of why EPRs are viewed as evidently necessary. Experts offered different opinions on these questions. What could be called an IT/clinical-perspective was fore-grounded on the second meeting. This perspective stressed the prominent concern of questions of standardisation, although discussion quickly revealed that standardisation was a heterogeneous specimen, which could refer, for instance, to hardware, software, logic, interfaces, or clinical processes. The point of view expressed by members of the group with a clinical background generally agreed that standardisation was an important topic. Their main concern, however, was to ensure that the EPR should function primarily as a tool for the clinician, which would enable a better flow of information between various departments, wards, and sectors. According to the representative of the medical association this flow would mainly be inhibited by the unstructured data from Kardex, and a main standardisation task would therefore fall on nurses.

As our discussion had to do with the formulation of themes for a question catalogue, it was suggested by the representative of V-Chi that the EPR might be presented to citizens as a vehicle for empowerment, as patients’ access to and control over their own records could be improved by use of the new technologies. The clinical
representative countered that accessing data would be of interest mainly to healthy people. In contrast, he claimed, sick people primarily wish to become healthy, and would therefore support the organization of the EPR according to the needs of clinical workflow as decided by medical practitioners, rather than organized around the question of patients’ access of their own information. It seemed a highly curious coincidence that these objectives were viewed as incompatible just at the point where IT, otherwise viewed as a supremely flexible tool, might be used to displace current doctor-patient relationships.

Along with the discussion of the content of the problem catalogue, we also talked over the program for the first weekend of the citizen panel. The representative of the EPR-Observatory and V-Chi had proposed that a 1992 advertisement for Hewlett-Packard, called Imagine, was shown as the citizens’ introduction to the EPR-theme. This movie showed a futuristic series of mini-narratives, in which potentially lethal injuries and diseases were prevented by the use of an integrated EPR, which gave simultaneous access to medication modules, booking, laboratory modules, tele-medicine, and administrative units. The representative stressed the importance of enabling the formulation of citizens’ visions, and proposed the video as something of a “visionary kick-start”.

The problem catalogue ended up presenting a number of themes: background and visions for the EPR, patient advantages, diffusion and concrete initiatives. It also called attention to problems such as standardisation, security and patient information, and future visions as possible pivots for the discussions in the citizen panel. Reading through the catalogue a few months later I was struck by the monotony of the catalogue, which, rather than presenting the EPR in its heterogeneity, presented some rather clearly defined problems and proposed paths of solutions to them. It stated, for example that “With the EPR, the paper-based record is a relic of the past. But the introduction of the EPR offers more possibilities than merely storing patient data electronically. EPR offers a series of advantages compared with the current records” (Intro-material: 1). This mode of writing presented the EPR as a logical improvement of the paper-based record. The EPR was described as a homogeneous phenomenon, with the consequence that the problems one could pose merely related to the question of ”how” to reap its benefits. Complementarily,
failure to do so could be interpreted in terms of a lack: administrative, monetary, or visionary.

Clinical and IT-perspectives were regularly framed in such an idiom but coming with a background in STS-studies, this tendency to naturalise the trajectory of the technology seemed to me blatant. It also gave rise to controversy regarding who might count as relevant experts during the weekends. There could be no doubt that EPR developers and involved clinical personnel were to present their views. Legal aspects were also high on the agenda. But critical socio-technical perspectives, whether from sociologists, anthropologists, historians or philosophers, were viewed by other members of the background group as rather more peripheral to the enterprise. When discussing the relevance of involving the perspectives of people doing micro-studies of actually occurring events, I was often faced with the tendency of other participants to render such studies anecdotal. Paradoxically, the fact that these studies were current and actual, made them less rather than more relevant, since the citizen project was viewed as a matter of constructing visions for the future. But with the intervention of the project leader, who was committed by the working methods and guidelines of the board to ensuring as broad an array of perspectives as possible, I eventually managed to secure two spots during the weekends, for STS-friendly presentations.19

8. Citizen Panel and Development Spaces
In the meantime, a proposal for the citizen panel had been put together. The panel was slightly changed after I pointed out that three engineers out of eighteen initially chosen participants was hardly representative of the Danish population. But eventually the panel seemed fairly heterogeneous; a mixture of men and women of ages 25 to 60, covering much of Denmark geographically, and with job titles such as: undertaker, traffic inspector, student, librarian, associate professor, telephone operator, and plumber. This group of people received the introductory package and a schedule for the weekend-seminars. They met for the first time on the 16-18 August in Silkeborg, Jutland and on the 14-15 September in Middelfart on Funen. The final seminar was on the 6-7 October in Copenhagen, at the end of which the consensus-document was produced.

The citizens met for an introduction to the EPR project on Friday evening in August on Funen. The project was introduced and citizens gave short presentations of
themselves and their reasons for participating. These ranked from highly positive expectations to the EPR-mediated future to markedly negative ones. Positive images revolved around economical efficiency and the quality of care, whereas negative ones were ethically motivated, and related, for example, to questions of information access and data security. The specifics of these evaluations varied, depending on citizens’ experiences as, for example, patient, ex-nurse, programmer or parent.

On Saturday morning the citizens received a more thorough introduction to the idea of reaching consensus. The panel was placed around tables in groups of four, and had to discuss the pros and cons of the EPR. Discussions were documented by participants’ writing on the paper-cloth of the tables. Examples from one table include:
- Vision: portables on the ward rounds – patient participation.
- Risk: focus removed from the patient to the technology. Like when people talk on their mobile phones instead of talking to those who are present.
- Who owns the record/data?
- Risk: blind faith in the technology (virus) -> similar risk with paper record. High level of security must be prioritised.

Afterwards, the panel gathered to listen to the first expert presentation, whose headline was ”What is the EPR?” The presentation stressed both the importance of developing a well-functioning EPR and its socio-technical complexity, a focus which was not appreciated by all participants, but interpreted rather as an attempt to evade public accountability, as indicated by the following comments:

Citizen1: What IS IT that cannot be categorised?
Citizen2: This isn’t exactly rocket science. Have you met people with an interest in complicating it?
Citizen3: Use pictograms; it is a practical problem, which can be easily solved

Following the presentation, citizens were re-shuffled into new four person groups to discuss what they had learned about the EPR. This discussion was followed by one hour of ”Open Space” in which citizens could assemble to debate a specific point of interest. One more expert presentation by STS-philosopher Finn Olesen, on ”Patient Records for the Patients”, was followed by another re-grouping and discussion.

At this point, citizens were asked to write down all points of agreement, on small green stickers, and all points of disagreement or doubt on yellow ones. A representative was chosen from each of the tables by the process consultant. This representative had to
remain seated, while the other group members visited the tables of other groups, and had their conclusions presented by the representatives.

Eventually, all green and yellow stickers were put up on a central plate and sorted by prioritised themes chosen by the citizens. Fourteen questions were agreed upon as thematically central in this exercise, and these were written into a questionnaire, which each citizen then had to fill out with four possible answers: agreement, partial agreement, partial disagreement, or disagreement. After filling out this form, the answers were debated in two large groups, which had to come as close to an agreement as possible. Finally, the entire panel repeated this endeavour, which was called by the process consultant, ”a consensus exercise”.

On Sunday of the first weekend two more experts presented on ”GPs and Electronic Patient Records” and ”Records and Privacy”, and the iterative attempts to define questions and reach agreement continued. At the end of the weekend the panel prioritised the themes and questions relating to these were sorted according to what the panel agreed on, did not agree on, and did not yet know. This document was returned to the background group to enable our selection of relevant experts for the next weekend. While the background group worked to choose experts, the citizen panel continued their discussions by e-mailing to a closed discussion list made accessible by the Board of Technology.

During the next weekend a similar procedure took place, focusing on the slow composition of a common point of view, from the multiplicity of expert and citizen perspectives. Presentations focused on ”Quality-improvements with the EPR”, ”EPR Implemented”, ”The Relationship between Doctor and Patient with the EPR”, ”Decentralised Solutions: Why?”, ”Centralised Solutions: Why?”, and ”The EPR and consequences for health care practice”. ”EPR Implemented” was presented by nurse who was the latest member of the background group, and accompanied by a visit to Svendborg Hospital, where an interdisciplinary EPR had been used at the medical wards for a few years. At the end of the second weekend a final set of themes, each with a set of questions attached was returned to the background group.
9. Final Weekend

For the final weekend, seven themes were selected: "Decentralised vs. Centralised Systems", "Patient Information and Privacy", "Visions for the EPR", "How do we involve the staff?, "Surveying One’s Own Course of Health "From Cradle to Grave”, "Research and Quality Assurance", and "Legislation". Each of these aspects were shortly discussed by an expert. During lunch break, the citizen panel formulated a final series of questions, relating to these short presentations. These were directed at specific experts, but contrary to earlier weekends, all experts were now gathered together with the explicit aim of generating discussion among them. This was not hard to do as advocates of both centralised and decentralised models of development sat next to each other, as did members of patient organizations and representatives of medical research in Denmark. However, few of these opposing views were manifested in open conflict. Thus, the citizen mediation of expert discussion, in this instance, clearly seemed to have a consensual effect. The question remained, of course, whether this consensus was substantial, or merely an expression of ”good sense”, a cover over differences of opinion in the name of an abstract generality.21

Citizens expressed various kinds of criticisms of expert perspectives. First, they wondered how the expert visions related to the practical realities of the health sector. As an exasperated citizen said after repeated failures to get an expert to make an estimation of the costs of various development models of the EPR: "But you cannot answer the question of costs with a pure guess". While it is indeed very hard to make good estimates of the cost of large socio-technical projects, this situation also opens an opportunity for experts to choose to forget costs, and concentrate instead on visions, which are not so materially bogged down. And, as we recall, visions were indeed what the citizen panel was asked by experts to consider in the problem catalogue. But at the end of the project an important divergence had occurred. For it turned out that citizens and experts were not in agreement on the visionary aspects of the EPR.22

IT-visions pointed to the potential advantages of a nationally integrated EPR. Among other benefits this would improve the success rate of the NPM (New Public Management) inspired political initiative called “Free Choice of Hospital”, according to which citizens can access information about waiting lists and use this knowledge to choose where they want treatment. Citizens, however, formulated their doubts regarding
this initiative. As one participant put it: "Free choice of hospitals is very technocratic. No one wants to move – you get no visits!".

But in particular medical visions stressed the necessity of ensuring the "flow" of information among sectors and that data was sufficiently standardised for research purposes. In contrast the citizen panel persistently focused on the capability of the EPR to protect patient data and increase patient rights.

At the end of the discussions, citizens got together for the last time in order to produce the consensus document, which would be presented at a press conference the following day. Discussion carried on until 4 AM, and the panel was ready for the press conference at 10 the next morning.

In the following section I discuss the content of the consensus document, which was presented in its headlines by a citizen representative, and made available in print at the press conference. But before discussing the results in more detail, I point to some of the reactions, which the presentation of this document occasioned among experts and others present at the press conference.

For journalists, and others, who had not been involved in the project, the process of reaching consensus seemed almost as interesting as the results. The panel was asked, for instance, about the quality of the answers, which experts had provided, and which of these answers had been most surprising. These were reflexive questions with consequences for the evaluation of the results of the project. The panel expressed general satisfaction with the response from experts, but they mentioned their surprise at some of the answers they received. These had to do in particular with the questions of data security:

Citizen1: We heard several times that the law was not observed because it is important for research, for humanity, and what have you.

Citizen2: [I was surprised] when a doctor told us how he worked in practice and the lawyer said that he was not allowed to do that.

Citizen3: I was surprised to find out who owns data. One doctor thought he did, but a lawyer said plainly that the patient did.

The panel was also asked how they felt about their role as generalised citizens. Since the development spaces method was presented as new, and the panel had continually pointed to time as a factor which limited in-depth probing of some of the complicated issues, a journalist queried "how confident you feel with your own work?". This question clearly
touched upon the legitimacy of citizen projects in general and the project manager responded promptly by pointing out that while the specific method was new, laymen’s evaluation was practiced in many countries, “with remarkably similar results”. She also suggested that: “Consensus reflects something generally human” and pointed to the practical limitations of carrying out such projects: "Of course, with endless amounts of time and money, every weekend for a year…This is people using their spare time”.

The citizen representative also responded. In her response, she noted how the panel had worked closely together with the project manager and the process consultant, to ensure the sobriety of the result. Along with other members of the panel, she also made clear their keen awareness of their own limitations as representatives:

C1: We represent all of Denmark, but there are groups of people without the energy to participate in such things.

C2: We are never prepared well enough, even the experts aren’t prepared well enough, we have to wait and see.

A third citizen pointed out that this awareness had made some members of the panel actively take the point of view of those groups who were not represented.

In contrast to these reflexive questions, many expert queries related prominently to the practical issues of their own concern. In this regard, one of the most illuminating questions came from a representative from the EPR-Observatory, who asked the panel to clarify: "Why do we need an EPR? How do we get the money?" This question, in my view, clearly illustrates some of the most problematic aspects of the project when conceived in terms of knowledge transmission from experts to laypeople. As I have shown the EPR-project was defined from the beginning as a way of thinking about the kinds of benefits the EPR might offer and how to reach them, rather than of critically reviewing the notion of it. Throughout this process citizens were encouraged to think in visions (recall the introductory Imagine video) without taking into account pragmatic issues such as costs about which, furthermore, no good estimates were made available. It is clear that the visionary frame was set up by the background group and not seriously re-framed by the panel. Consequently, it is obvious that this process cannot be used by experts as a visionary feedback mechanism generating political support for their own agendas. But while the democratic potential of citizen projects do not lie in this
direction, zooming in on the details of the consensus document will allow me to mobilise a different conception of the project as a democratic experiment.

10. Consensus Results
As noted the final document from the citizen panel was made public at a press conference on October 7, 2002. What conclusions were reached in it? The final document describes the attitude of the citizen panel to six problem areas, which are summarised in an introductory vision. Consensus was reached, "that the primary purpose of the EPR – from the point of view of the patient – is an improved and efficient treatment. The EPR should also be a valuable tool for the health care sector.” This is a broad formulation, with which many would agree. But the abilities of the panel to formulate visions that diverge from those of clinicians, bureaucrats, and technicians is made visible through the argument supporting the vision, and the recommendations. The argumentation indicates that improved treatment is intimately connected with the delegation of influence and responsibility to the patient. And the visionary recommendations distinctly circle about the implementation of an EPR, which can support a higher degree of influence on the patient’s own course of illness. This plea has two components. The first relates to the possibility of citizens’ becoming actively engaged in the construction of their own records. It suggests that patients get the opportunity of having free access to their own record, and that they are allowed to add data to the record in a specific field of entry.26

The second recommendation had to do with the right of patients to delimit access to their own data. Here, among other things, it was proposed that: ” Patients’ permissions are needed in all but acute instances to access data from the record”. This is suggested, for example, to ensure that citizens’ health data is solely used for research, which they are willing to approve of. Likewise ”it is only personnel directly involved in the treatment of patients, who has access to the record”.27 Optionally, it is suggested that such security could be achieved by ”use of a citizen/health card, where information can only be passed on with patient consent.” The citizen panel has reached consensus on a number of other topics. For instance, they point to the importance of ensuring a high degree of clinical involvement during the introduction of EPRs, and to the need for improving medical quality assurance by use of decision-support systems. But the democratic relevance of the EPR-project is made particularly visible in the panel’s recommendations in relation to
their status as potential patients. For in these recommendations the distance between the input from the invited experts and the consensus of the panel is largest. I find it promising that such singularity in perspective could be achieved in spite of the sometimes homogenising dynamic of the expert-citizen interaction.

11. Learning in the Process

For scientists, of course, are not the only legitimate representatives of things. They represent things only to the degree that we have succeeded in inventing questions for their subject, which permit them to put to the test the fictions that concern them. But today, most technological-social innovations affect things in much more varied modes than those anticipated by our questions, and thus create a gap between “things”, as they are implicated in it, and their scientific representation (Stengers 2000:158)

As I have touched upon earlier, learning is an important notion for the board of technology, because the democratic success of citizen projects is dependent on an adequate learning process resulting from interactions between experts and laypeople.

In this section I argue that learning is, indeed, a crucial notion, if one’s aim is to democratise technoscience, but a notion with some rather different implications from those suggested by the board. According to Isabelle Stengers, learning is an important part of the ”adventure of reason”, because societies can respond accountably and imaginatively to new situations only by learning to collectively formulate new questions adequate to them. This is a task that requires the mutual engagement of experts and laypeople.

From the point of view of empirical philosophy, the abstract theory of learning embraced by the board of technology tells only a partial story, a story of ”good sense”. To complete this story one must look at learning-in-action as it took place during the citizen project. What, then, was learned in the process of the EPR citizen project? How was learning concretised in practice?

Three points are worth emphasising. First, the learning process was iterative or recursive. Experts formulated a catalogue of themes for citizens to consider, which was transformed as a consequence of other expert presentations, and citizen deliberations. The background group then had to learn from this document which perspectives the citizens found missing or under-developed in its first version, and respond by attempting to find relevant experts for the next meeting. Thus, the sort of learning embodied in the project
was in principle bilateral, but clearly asymmetrically so. The process could work to the extent that both experts and citizens were willing to learn. Citizens needed to learn about the multiple technical and social aspects intertwining in a large socio-technical complex such as the EPR, and how to collectively sort relevant from irrelevant information. Experts, however, needed to learn, and constantly recall, the partiality of their own perspectives. They needed to do so to be able to suitably respond to citizen queries, which may have seemed to them strange, irrelevant, or plain stupid. Nevertheless, citizens were obviously more dependent on experts than vice versa in that experts unable or unwilling to represent the diversity of the phenomena at hand, could more or less covertly frame the debate and its conclusions in specific ways.

The sort of learning facilitated by the project was uneasily situated between a cognitive and a bodily understanding of the learning process. The board distinguished between education as enlightening and as training, and was clearly interested in placing their projects within the latter idiom, with its connotations of practice, and its emphasis on the lived experience of the participants. And, indeed, as Isabelle Stengers describes it, other citizen interventions have shown how:

Noncompetent citizens, when they do not have to "learn" science "as at school", but are put in a situation where they can demand that scientists respond to their questions, make the effort to render the "information" they possess pertinent and usable – in short, to address themselves to them as if to interlocutors on whom their work depends – have…been capable of taking a position on a very difficult technical problem…” (Stengers 2000:161)

The distinction between "learning in school" and "learning for real", or "enlightening" and "training" seems at first to match studies, such as those of Jean Lave, which points to a need for relating learning to experienced real-life situations, rather than cognitive puzzle-solving (Lave 1988, Lave and Wenger 1991). But in spite of citizens’ drawing on their own experiences, it is hard to claim for the project a phenomenological embodied status. As noted, the panel in fact visited a ward with an implemented EPR-module at Svendborg Hospital. But even this visit can only in part be characterised as ‘hands-on’ relevant to the understanding of the EPR about which citizens were supposed to learn. For to many experts, the EPR of the future is imagined as radically different from anything in existence. In effect, this makes questionable if the EPR can be characterised as being presently phenomenologically available anywhere. The system at Svendborg, for instance, was viewed as old-fashioned and severely limited, as indicated in the comment
from a clinician that: "Notwithstanding the excellence of Svendborg, we are now several generations further ahead." This comment responded to the suggestion that the problems encountered at this hospital could be more broadly symptomatic of the possibilities and limits of using EPRs.

To take this pervasive and sceptical perspective seriously, is to conclude that, at present, there are no experiences to be had, and no models ready for imitation in relation to the EPR. The fact that learning arguably took place, this paradoxical situation notwithstanding, poses a problem for representation- and imitation-based models of learning (Schön1987), but also for those based on a prioritised notion of lived experience.

I would argue that it is precisely because no models are available for imitation that visions came to play so prominent a role in the EPR-project, from the presentation of the Imagine commercial, to the final consensus document. But the world of visions is dangerous because it has the capability to completely separate itself from the world of experience and accountability, and render the actual anecdotal.

In contrast to visionary approaches, Gilles Deleuze presents the problem of learning in the following way:

The movement of the swimmer does not resemble that of the wave, in particular, the movements of the swimming instructor which we reproduce on the sand bear no relation to the movements of the wave, which we learn to deal with only by grasping the former in practice as signs. That is why it is so difficult to say how someone learns: there is an innate or acquired practical familiarity with signs, which means that there is something amorous – but also something fatal – about all education. We learn nothing from those who say "Do as I do". Our only teachers are those who tell us "do with me", and are able to emit signs to be developed in heterogeneity rather than propose gestures for us to reproduce” (Deleuze 1994:23)

The excerpt indicates an understanding of learning as an ongoing and dynamical process, through which participants together become able to produce, not a consensus (imitating others or finding a common denominator) but a movement towards a state, which is not given, and which necessarily remains unstable. And to the extent that the EPR-project was a learning experience, the interaction between citizens and experts can, indeed, be described in terms similar to those of Deleuze. In certain instances, incongruent perspectives on the EPR made visible its heterogeneity and complexity rather than visionary gestures for repetition. As we have seen this happened primarily in relation to the question of the protection of the privacy of patient data. In the citizen panels’
discussion of this set of signs "emitted by experts", they re-formulated its relevance in their own multiple idioms, shaped by their "practical familiarities". And in this situation the movement between these different points of view enabled the expression of an aspect of the EPR in a novel and relevant mode.\textsuperscript{30} This outcome, however, was not, and could not have been guaranteed with reference to the method of development spaces, which offers merely an abstract form for the concrete interactions of the project. That success is thus uncertain but can nevertheless be achieved makes viable a characterisation of citizen projects as experiments in democracy. And experiments, as we know, may be partially successful.

12. Democratic Experiments and the Powers of the False

A new status of narration follows from this: narration ceases to be truthful, that is, to claim to be true, and becomes fundamentally falsifying. This is not at all a case of "each has its own truth", a variability of content. It is a power of the false, which replaces and supersedes the form of the true, because it poses the problem of incompossible presents, or the co-existence of not-necessarily true pasts (Deleuze 1989:131)

As noted above, citizen projects can be described as experiments because no method can guarantee their successful, democratic, outcome. The democratic invention, which ideally takes place in such encounters, can be called, with a phrase from Gilles Deleuze, the power of the false. The falsity here mentioned is not opposed to truth, as in the classical opposition; for to Deleuze the notion of truth (as universal, de-contextual etc.) is exceedingly dubious. His playful invocation of falsety therefore first of all suggests that if the standard of evaluation is classical truth, then we are all falsifiers.

Narrative renderings are therefore not about objective description of a state of affairs, but only about expressing points of view, which are per definition partial. In Deleuze’s formulation this necessary partiality takes nothing anything away from a perspective, it does not signal a subjectivistic lack to be remedied by a more objective account. Rather, the partiality of a viewpoint necessitates its complementation by different one’s, in relationships that are nevertheless not consensual, because the partial insights do not necessarily "agree” in any simple sense, and possibly cannot "live together”. This potential incommensurability of viewpoints is implied in the Leibnizian
notion "incompossible presents" and points in a direction almost opposite that of the ideal of consensus.\textsuperscript{31}

But in situations where no one can claim immediate access to a true or rational solution to a problem of what to do, what is demanded is precisely \textit{a collective exploration of possibilities}, and where differences rather than easy agreements should consequently be highlighted. Thus, \textit{the powers of the false} designate the inventive possibilities of situations, in which it is unknown, unclear, or contested what \textit{would} be the true, real, or rational course of action to embark upon, and it points at the \textit{generative capabilities} of such complicated situations.

Such powers are therefore quite different from \textit{the powers of envisioning} as instantiated at various points in the project, insofar as these worked by effacing the heterogeneous possibilities of different situations and often masked an attempt to impose a specific direction on the future in the name of a generality; ”for the better for everyone”.

In my view, citizen projects as democratic experiments can become vectors of change precisely to the extent to which they tap into the imaginative, rather than visionary, powers of the false.

In the EPR project clinical perspectives consistently focused on the necessity of viewing the EPR as a tool, which predominantly should be used to support clinical practice. Clinical perspectives therefore found problematic the idea of having to ensure consent to the use of individual pieces of patient information. In contrast, the citizen recommendations demonstrate that the \textit{privacy of the patient} is a notion, which the health care sector must become better at taking into consideration. As such, the citizen recommendations formulate no \textit{solution} to this problem, and a solution must under all circumstances be collectively constructed by the involved parties. But we can talk of an \textit{articulation} of an important problem relating to the EPR, which hitherto has not been made clear. And as such we can talk of a successful experiment in democracy.

The limits of the conceptual frame, which regularly separates \textit{experts} from \textit{laypeople}, become manifest in the surprise often generated by the fact that the active involvement of citizens in important public debates, can contribute with new and relevant insights (see e.g. Wynne 1992). But it is precisely the realisation that such activation carries possibilities, which is the basis for what can be termed the \textit{democratic experiments} of the board of technology.
To make such experiments, and try to ensure a degree of democratic validity is not a simple task. First of all it places demands on the ability of the board of technology to put together background groups, whose members have perspectives that are hard to join. It is, so to speak, the basic dissensus among the experts, which forms the background of the possible consensus of the citizens. One can go so far as to affirm that if the process, which leads to relative consensus, has not been sufficiently heterogeneous in its modes of presentation then the problem has not been formulated in a sufficiently relevant way. Accordingly, the process demands of the board of technology, that it is careful to ensure that broadest possible array of articulations on the problem. But the process equally places demands on citizens and experts. A demand on citizens’ vigilance in understanding the complicated problem, they are asked to come to a decision about. But also a demand to identify groups of experts who seem too much in agreement. Inversely, a demand on the experts, to always identify the singular aspect, which they represent in their presentations; that is, not to be carried away by the opportunity to use their specific mandate to promote a general perspective, whose premises remain implicit. Democratic experiments, therefore, are not simple. But neither are they unreachable ideals. As I have tried to indicate, the EPR project exemplifies some of the possibilities, which the formulation of alternative visions for the health care sector, by active citizen participation, offers.

13. Challenges of Framing and Execution
Nevertheless the project was far from faultless and I have pointed to a number of its peculiarities throughout this text. In ”Technologies of Humility: Citizen Participation in Governing Science”, Sheila Jasanoff points to a number of ”focal points around which to develop the new technologies of humility” (Jasanoff 2003: 240). Two of these seem particularly pertinent for the evaluation of the EPR-project. First, there is the question of framing. As Jasanoff states:
It has become an article of faith in the policy literature that the quality of solutions to perceived social problems depends on the adequacy of their original framing. If a problem is framed too narrowly, too broadly, or simply wrongly, then the solution will suffer from the same defects (Jasanoff 2003: 240)
Development spaces in principle responds well to this problem, because the in-built iterative learning process, involves the circulation of a processual document through which citizens can re-shape the original framing. Indeed, as we have seen, they did so, with their insistence on patient empowerment. But aside from this specific point the themes and problems defined in the problem catalogue remained remarkably stable throughout the process. It could be argued that this exhibits the thoroughness of the background group in writing up the catalogue. However, even as I had been an active participant in its formulation, I keenly experienced the insufficient representation of the broadly social, organizational, and pragmatic dimensions of the catalogue. In particular I noticed an excessive focus on EPR as a technical solution to a multitude of complex problems, which would consequently not have to be taken seriously into account. Thus, the invocation of complexity became a convenient way for some experts to evade questions of practical accountability, while visions were simultaneously used to generate support for the cause of developing EPR’s. Likewise, I noted a significant emphasis on clinical perspectives rather than patients’, and doctors’ rather than nurses’.

Since these themes were in fact never seriously considered, it seems clear that the shared and implicit assumptions of a large group of the invited experts effectively diminished citizens’ possibilities of radically redefining the original framing. This case allows me to define an important first challenge for the citizen projects of the future; a challenge of framing. The democratic potential of citizen projects is crucially bound up with preventing the homogenisation of expert panels by members with superficial disagreements based on deeper underlying agreements.

The citizen mediation of expert views during the final weekend was seen by the project manager as generating a consensual effect. In contrast with this view I suggest that this consensus merely consisted in the coming to light of the common assumptions the experts had brought to bear on the project all along. This, of course, does not indicate a success but a problem: it shows that the divergences out of which consensus seemed to emerge were merely cosmetic. The challenge of framing consists in finding ways to prevent invited experts, particularly in the background groups, from regarding only specific aspects of a case, while agreeing on the general assumptions of the discussion.

Jasanoff calls the second issue vulnerability. This relates to the consequences of the differential positions from which citizens encounter and experience technological
systems. Consensus-conferences and development spaces, again, are *in principle* representative, according to the in-house theory of democracy. In practice, however, there are clear limits to representation. As mentioned, this question of representation was posed at the press conference at the end of the project. And the citizens, indeed, answered in a way that showed their awareness of their peculiar position. They knew well that they were "strong" citizens, and had to take careful account of those, not there in person, who were "weak".

In spite of the reflexive sophistication of the citizen panel, it is worth recalling that no sick people, no people over sixty years of age, and but a single individual of a different ethnic background (e.g. non-white) participated in the panel. While patients and pensioners were deliberately excluded from the panel other exclusions were wholly implicit. It seems obvious that the challenge of executing democratic experiments involves a careful consideration of which citizen categories are taken into account, and of their criteria for inclusion and exclusion.

I have pointed to a number of instances, where the ideal situation of experts training citizens in a space transparently mediated by the board of technology, is problematic. For instance, it seems clear that great care must be taken to prevent situations in which specific experts can use the panel as a more or less covert mechanism for self-legitimisation. Obviously, unbiased, unprejudiced communication did not take place in the project, and accordingly the ideal of consensus was compromised. Citizen projects must thus come to terms with how to ensure the accountability of their procedures if heterogeneity and incompatibility rather than common sense and good sense is the relevant background for their executions.

These comments are critical but this is not my main point. I definitely agree with the Board of Technology that citizen projects can achieve beneficial democratic effects. But the success of this aim has to do with supporting the exploration of how to construct a singular viewpoint out of a multiplicity of "incompossible presents", rather than with achieving unprejudiced communication and consensus.

14. Humility and Insistence: The Challenge of Consequences

Jasanoff suggests the designation *technologies of humility*, for technologies that "make apparent the possibility of unforeseen consequences; make explicit the normative that
lurks within the technical; and acknowledge[s] from the start the need for plural viewpoints and collective learning” (Jasanoff 2003:240). In such formulations, she is not distant from the view espoused by the recent Latour and by Michel Serres. In The Natural Contract the latter writes:

If man holds back. We arrange the world for ourselves alone, now exclusively political animals, inexorable winners of the war of survival, enclosed forever in the city built without limits, coextensive with the planet: already, who can leave the city called Japan or the greenhouse called Holland? When greenhouses cover the earth – disaster. In the midst of stone and glass, men will have nothing but glass and stones beneath them, for building, and, in front of them, for living, in a world finally vitrified, subjected to their law alone…Doubtless humanity begins with holding back. (Serres 1995: 116-7)

Perhaps citizen projects are experiments in learning to hold back, or inventing technologies of humility. In that case, I think that humility must refer to the process rather than the real outcome or imagined effect of such projects. It is necessary to be humble with respect to the fragility of democratic projects. They are fragile because their democratic potential consist in the composition of a singular perspective out of a multiplicity of viewpoints, Deleuze’s incompossible presents. Accordingly, they can easily break down if the framing is off-balance and experts come to homogenise discussion rather than continually opening it. Above, I have therefore related the theme of humility to two challenges, which I think citizen projects need to confront - a challenge of framing and a challenge of execution.

But precisely because the democratic success of such projects is so challenging, one can speak of a democratic event when a singular point of view emerges from citizens’ collaborations. In this situation, indeed, it would become possible to talk about visions in a novel sense; visions as an art of consequences. Such visions would capture the relationships between the multiply diverging positions of and between citizens and experts, while understanding that these positions cannot easily be brought into co-existence, and that their incompatibilities and contradictions must therefore continually be taken into account.

Were this to happen, humility, momentarily, could be exchanged with an insistence: an insistence on the relevance of the powers of the false, as generative of viewpoints that ought to be taken seriously into account in political decision-making. This challenge of consequences would be out of the hands of the Danish Board of
Technology. But one could wager that *insofar* as the board successfully met the democratic challenges posed above, their recommendations would, indeed, be harder to ignore and would seem more urgent to react to. It would, nevertheless, remain a challenge of the political institutions to learn from such projects important and relevant lessons about citizens, and their wishes and rights. Were they able to do so, citizen projects, I think, would really instance situated knowledge.

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1 Haraway’s reference to Danish consensus conferences is Richard E. Sclove’s work on *Democracy and Technology*. Sclove’s analysis of the Danish initiatives is built on articles discussing various methods of technology assessment rather than investigations into how these are practically accomplished. Likewise is *Public Participation in Science: The Role of Consensus Conferences in Europe* (Joss and Durant 1995).

2 In the present paper I carry out such a quasi-ethnographic investigation.

3 A number of articles in Rip, Misa, and Schott (eds.) 1995, notably by Herbold and by Remmen discuss technological development and implementation in terms of *social experimentation*. Herbold’s use of this term remains fairly scientific and technocratic in its exemplifying how “technological inventions are increasingly tested via experimentation in a social context” (186). Remmen’s discussion is more multifaceted and his emphasis that “to democratize technological change, research methods must be generated that are capable of establishing a basis for exchange of knowledge between technicians, users, researchers and other relevant groups” (202) is clearly related to the ambition of this paper. Remmen recommends methods such as dialogue and future work-shops as ways of ensuring increased democratization of technological decision processes. My contribution in this paper is to discuss in detail a number of the limits, problems, and potentials with the related methods of consensus conferences and development spaces. John Durant’s (1995) discussion of an English consensus conference as “An Experiment in Democracy” shares little with the ideas here presented.

4 From “What knowledge should be taught in schools”, presented at the Responsibilities: From Principles to Practice seminar organised jointly by the Council of Europe and the European Cultural Centre at Delphi, Delphi (Greece), 15-17 October 1999. Available at http://www.coe.int/T/e/Cultural_Cooperation/Education/E.D.C/Documents_and_publications/By_Subject/Responsibilities/responsibility.asp.

5 In Danish this was also signalled by a change of name from Teknologinævnet to Teknologirådet. Both of these names are translated into the Danish Board of Technology.

6 Discussion of these different methods can be found at http://www.tekno.dk/subpage.php3?survey=16&language=uk

7 The complete list of 84 proposals for 2003 is currently available from http://www.tekno.dk/subpage.php3?article=543&language=dk&category=7&toppic=kategori7

8 I have no information on the actual events that led to the board being placed on the list but subsequently removed. One interviewee has suggested that it was a simple mistake, which in the current political climate became “very hard to admit”.

9 This publication was part of the intense European and American focus on the possibilities of IT throughout the nineties. It is available from Available from http://www.fsk.dk/fsk/publ/it95-uk/
"Good sense" is among the four primary philosophical vices attacked by Gilles Deleuze in Difference and Repetition, and refers to the assumption that differences can be submerged by common judgments as to the proper way of hierarchising values. As Isabelle Stengers glosses it: "Gilles Deleuze reminds us, in Difference and Repetition that, according to Hegel, good sense is a partial truth associated with the
feeling of the absolute” (Stengers 1997: 70). Below I return to the problematic affiliation between consensus formation and "good sense".

22 Signe Svenningsen has pointed out that there is a sliding in the treatment of experts, citizens, and their relationships; whereas each group is initially identified as heterogeneous, this is underplayed in statements such as the current, where experts and citizens are presented as uniformly opposed. Experts remained in important disagreements among each other (in particular Signe Svenningsen and I were in general disagreement with the visions constructed by other experts) as did citizens. These internal differences, in my opinion, are not only irreducible, they are also generative (that is, positive rather than problematic) of the productive democratic outcomes of such projects. Indeed, a main point of the present paper is that differences should never be toned down in the name of consensus or "good sense". Differences can also be categorised otherwise, and here I tried to point to the heterogeneity exposed between the IT/clinical perspective espoused by most experts and the interest in other issues demonstrated by a good deal of the citizen panel.

23 Which, of course, is but a single example of the problems of transforming concrete situations into "ideal speech situations".

24 A similar qualification to that found in note 22 is due. Of course, some experts attempted, with some regularity, in meetings and in presentations, to critically review IT/clinical views of the benefits of the EPR. Again, however, a prominent group of experts agreed that the EPR would be beneficial in the many above-mentioned ways, and disagreed only on how to reach those goals. As I go on to discuss below, this is not to suggest that these disagreements were not practically consequential in important ways, but to signal that problematisation, in the main, related not to the question of "what we want to achieve" but to "how we are going to achieve it".

25 Markussen (1996) offers a detailed discussion of the use of IT-designers for the category “user”, and the way designer’s own interests and preferences are made invisible or legitimised by reference to the more or less idealised "wishes" of these. See also Cooper and Bowers (1995).

26 For instance on their use of alternative treatments.

27 These visions are actually implemented in current legislation, although they are regularly bypassed for practical reasons as mentioned above. Currently, an organization paradoxically called Danish Society for Patient Protection work to change legislation so active dissent rather than consent would be needed to prevent non-primary care-givers and researchers to access one’s own data.

28 I view the project as democratically relevant to the extent that its interactions conferred on participants the ability to take a novel stand as regards the problematic issue. Democratic experiments are thus experiments in learning how a collective of heterogeneous people can make democratically relevant decisions or get democratically relevant ideas. This points to a redundancy in the term; for in order to be relevant, I imply that decisions must be democratic in the ways I specify. However, I retain the connection because the terms are rarely so co-implied.

29 The qualifications from notes 22 and 24 obtain here as well. Pointing to the large difference between experts and citizens is not an attempt to homogenise either group, but to point to some of the important differences (rather than oppositions) that were produced in their interactions. Highlighting these, rather than toning them down, as I argue, is an important democratizing function of citizen projects.

30 That this is the case can be seen plainly from the fact that many other themes were not re-framed in the least in the citizen’s consensus document.

31 It is impossible here to properly elaborate the Deleuzian understanding and transformation of the Leibnizian concept of incompossibility, not to mention of Leibnizian philosophy in general. For an English translation of one of Deleuze’s lectures on the topic see http://www.webdeleuze.com/html/TXT/ENG/150480.html. For a detailed exposition see Deleuze 1993.


33 As, indeed, I think should be the case with the citizen panel’s plea for using EPRs to increase rather than decrease the protection of patient data.
Chapter ten: 
Infrastructural Fractals: 
Re-Visiting the Micro-Macro Distinction

1. Three ways of relating to the Electronic Patient Record

I am interviewing the head of department of the sixth office of the Danish National Board of Health, the office for health informatics. The scene is a crowded, open office-scape with a view over Holmen in Copenhagen. We discuss the development of electronic patient records in Denmark and a broad political field is laid out before me. My interviewee talks about EU initiatives concerning conceptual standardisation of the development and he talks about government regulations. He also mentions non-compliant regions and potential sanctions. Meetings and people from all over Denmark, and even Europe, are referred to.

Contrast this meeting with another of my interviews, taking place at Randers Central Hospital in Eastern Jutland. I am talking to an informatician about her part in the development project of electronic patient records in the Aarhus Region. She is (among other things) the project manager of the group developing the order-entry module, one of several components to be integrated in the final system. The scene is a small office in a quiet corner of the hospital. She mentions the challenge of composing work groups enabling relevant information to reach all interested parties. There are software vendors, hospital management and non-participating clinicians to inform, and other development groups with which co-ordination must be ensured. “Meetings, never-ending meetings”, she says.

Another interview is taking place in the same office in Randers, this time with a nurse, a “lay-participant” in the development of the order-entry module. She has had neither formal education in information technology nor much experience with it. But she became curious and took some courses. Along with a number of other clinicians of different specialties and from different wards, she is painstakingly decomposing her everyday work practice in workflow analyses. These are handed over to software developers, who use them to model relationships in an object-oriented programming language and to prepare cardboard mock-ups suggesting how these tasks are to be accomplished at the interface. These proposals are carefully tested, evaluated, and
transformed in discussions with the nurse and the rest of the group.

2. From the Micro-Macro Link to Infrastructural Fractals
I have briefly exemplified three relationships between different persons and the Danish electronic patient record; in preceding chapters we have met many more. All are culturally “validated” in the sense that they are encouraged, appreciated, and paid for, by relevant institutions and organizations. Yet they do not seem equal. One actor seems large, connected to things powerful and important, having to do with the “macro” development of the national health infrastructure, related as his work is to questions of policy and bureaucracy, whereas another actor seems small, her work comparatively mundane and trivial in its “micro” concern with drawings on cardboard.

The perceived discrepancy in the “size” of contexts, situations, and practices has been a matter of sustained discussion in the sociological and anthropological literature, but these debates have often been framed within the dualistic idiom of micro or macro. The relevant question, then, has seemed to be “how to connect” the two separated poles. This is clearly suggested in book titles such as Karin Knorr-Cetina and Aaron Cicourel’s edition Advances in Social Theory and Methodology: Toward an Integration of Micro- and Macro-Sociologies from 1981 and Alexander, Giesen, Münch, and Smelser’s The Micro-Macro Link from 1987, and it is manifested in their contributions.¹ Rather than re-framing the debate in terms that could possibly allow for different ideas to emerge, these are attempts to bridge a dualism whose terms are left intact. This is not the place for detailed exegesis of the point (I urge readers to consult the volumes) but an exemplification may be in place, and I take it from the introduction to the Micro-Macro Link.

In their introduction “From Reduction to Linkage: The Long View of the Micro-Macro Link” Jeffrey C. Alexander and Bernhard Giesen detail how sociological theory is premised on a separation between epistemology and ontology. In sociology, they claim, epistemology is turned into the “problem of action”; that is, the determination of whether the activities of “knowing” actors are “rational” or “interpretive”. An additional question relates to the ultimate source of the knowledge based on which actors act. This is the question of ontology whose sociological variant is termed the “problem of order” (Giesen and Alexander 1987: 13). These two sets of proposed relationships are viewed
through a combinatorial grid of sorts, and the authors suggest that at least five resulting views of the relationship between the micro and the macro are represented in major sociological works.

Sociological theory has maintained that (1) rational, purposeful individuals create society through contingent acts of freedom; (2) interpretive individuals create society through contingent acts of freedom; (3) socialized individuals re-create society as a collective force through contingent acts of freedom; (4) socialized individuals reproduce society by translating existing social environment into the microrealm; and (5) rational, purposeful individual acquiesce to society because they are forced to by external, social control (Giesen and Alexander 1987: 14)

With this conceptual “locking mechanism” in place classification becomes a natural next step, although its details in some cases remain complicated. Max Weber, for example, is seen as waveri...
contributors along multiple substantive, theoretical, and methodological lines, all “argue that the link between micro and macro must be made” (Alexander and Giesen 1987:31).

The advertised newness of this common argument remains rather unconvincing, not least in view of preceding elaborations of how Max Weber and Talcott Parsons, for instance, tried to make just such connections. But in spite of such problems the ambition of *Micro-Macro Link* of “open[ing] new windows onto the sociological imagination” (Alexander and Giesen 1987: 37) seems to me a good one. It is just that the strenuous maintenance of a dualistic framework narrows the imaginative exercise unduly.

Some scholars, however, notably social anthropologist Marilyn Strathern and actor-network theorists Michel Callon and Bruno Latour, have posed the question of how to understand action and order, epistemology and ontology, the individual and society and, consequently, the micro and the macro in non-dualistic terms. For the former, an analogy, which has been central in the effort to dispense with the dualistic framework, has been that of the *fractal*. The move to understand infrastructural transformation in a fractal rather than dualistic mode is what exercises my imagination in the remainder of the text.

In the rest of this chapter I explore how one can understand and link the events constituting the EPR landscape in a fractal rather than dual manner, and I elaborate on some of the consequences, theoretical and normative that such a move can be seen as entailing.

In the next section I invoke a distinction of Marilyn Strathern’s between two Western “orders of perception”; juxtaposition and magnification, and two orders of scale. Following Strathern I suggest that the realisation of their intertwinement could enable an imaginative re-figuration of scales as isomorphic rather than organized hierarchically into the small and the large, the important and the unimportant, and so on. This would entail viewing relationships not as given but as enacted, a proposal I detail in section four. This elaboration aims to elucidate differences not only between a fractal approach and a dualist one, but also between a fractal understanding and that of a cultural critic. I indicate a number of such differences in section five, which considers a discussion between Marilyn Strathern and cultural critic and avowed deconstructivist feminist Vicki Kirby. One purpose of this exegesis is to clarify how fractal approaches to social theory aim to construct new modes of analysis rather than deliver tools for (classical) criticism.

Perhaps such new modes of understanding would be of help in responding to the
real enough challenge of the unpredictability of events. I discuss two kinds of unpredictability and some of their consequences in section six. In section seven I move on to discuss the phenomenon of changing scale, that is, the ability of actors to grow; this is viewed as a feature of their fractal constitutions, their enchainment and co-implication with other actors. In section eight I elaborate on the consequences of this view for the sociologist, anthropologist, or social epistemologist. Finally, in section nine, I return to my starting point and consider some of the ways in which scale both matters and does not.

3. Perspectives and Scales: A Fractal Geometry

At least two orders of perspectives can be readily identified in the way Westerners take up positions on things. One is the observer’s facility to move between discrete and/or overlapping domains or systems…The other is the facility to alter the magnitude of phenomena…These orders share an obvious dimension themselves. The relativizing effect of knowing other perspectives exist gives the observer a constant sense that any one approach is only ever partial, that phenomena could be infinitely multiplied (Strathern 1991: xiv)

As indicated in the introductory interview excerpts and throughout earlier chapters, one is quickly struck by proliferation and multiplicity when investigating the electronic patient record. By proliferation I refer to the feeling that since no common idea as to what the phenomenon consists of is available, many different places could be visited, many different people could be talked to in order to try to catch it. By multiplicity I point to an experience, following from this procedure, which suggests that electronic patient records are a heterogeneous lot according to the actors who work with them, talk about them, and care for them. It becomes uncertain whether one is studying one phenomenon, a group of interrelated ones or, by misclassification, has grouped together for studying several different phenomena (as discussed in chapter three).

These complications are all captured in the first order of perspectives, identified by social anthropologist Marilyn Strathern above: “the observer’s facility to move between discrete and/or overlapping domains or systems”. More complications are added by paying attention to the second one, “the facility to alter the magnitude of phenomena”. The difference between the two orders is between playing with the notion of juxtaposition and paying attention to the achievement of scale as size.8 This is of
particular interest because another sort of argument, which would see size as a consequence of the *importance* of actors, is regularly taken as a solution to the problem of juxtaposed contexts. The idea is that if relevant scales were observed, the many different places, opinions, and contexts visited, could be properly sorted and evaluated. But then the two kinds of scales are interrelated, for an increase in relevance is supposed to be what enables increase in size.

This understanding is certainly not hard to come by in the policy-oriented vicinity of attempts to develop electronic patient records. It seems *evident* that one needs to go to certain people who can sort relevant from irrelevant, for advice on how to act in relation to various problems, such as which standards to adopt, how to ensure nation-wide co-operation, which software to use, and which hardware, and how to implement the technologies. The people with such specialised sense of proportion are called experts, and the reliance on them instantiates a distribution of expertise as a substance of which some people have more than others. This is an everyday occurrence. But it is one, which carries a hierarchising effect. For it naturalises the idea that some persons and the places they inhabit are larger, more connected, more important, or more influential than others, since they are the ones with important knowledge. But what would happen if belief in the naturalness of this state of affairs is suspended and importance and relevance are viewed as constructed categories?

Marilyn Strathern, points to a different possibility for thinking about (sorts of) scales, taking off in an analogy with fractal geometry. Connections between people may take many forms and may be studied on what is perceived as many levels, she suggests, but complexity is replicated in each instance:

> To suggest that the ‘quantity of information’ thereby remains constant is to suggest that the intensity of perception is a constant. The single person is as complex to analyze as a corporation composed of many. But what does not appear disproportionate as a symbolic device (e.g. metonymic), becomes disproportionate when the replication confounds what are maintained as different levels of information. If the corporation is defined as complex by contrast with the single person, the two cannot be analyzed isomorphically (Strathern 1991: xix)

Fractal geometry thus offers a view in which all social events are on the same level, in the sense that each set of events can be described as equally complex regardless of their putative fit into a micro- or macro-picture, as understood by classical social science. This
raises the question of what happens if this seeming disproportion between levels and sizes is disregarded, scales of observation and relevance are rendered isomorphic and one cannot therefore be imagined as the basis for another. The move dispenses simultaneously with the “dream of presence and authenticity” promoted by some scholars of “micro-social events” and its pendant, the “dream of overview” harboured by some researchers into “macro-sociology”. Rather than offering such purchases, changing scales or events merely enable one to make explicit various sorts of differences.\(^\text{10}\)

5. Enacting Relationships

It is hardly surprising, then, that we should encounter not just individuals enhancing and enlarging their own spheres of influence under favorable demographic and economic circumstances, but widespread interest in the elaboration of relationships as such. The enactment or realization is an elaboration on its existence. In making connections visible, people assert their ever-present capacity to act upon them (Strathern 1991: 102)

When scales of observation are rendered isomorphic a first experience, for many people, is one of disproportion and cognitive dissonance.\(^\text{11}\) Sometimes the mere entertaining of the idea is viewed as morally offensive. How, it is asked, can we ensure a proper evaluation of true and false, or better and worse, or good and evil, if all scales are rendered equivalent.\(^\text{12}\)

In the following section I discuss this question in some detail, in order to differentiate between the fractal understanding here described and that of cultural criticism. But it is worthwhile to presently offer a preliminary to the answer. It has two parts. The first is merely to note that isomorphism is not the same as equivalence.\(^\text{13}\) The second is to emphasise that analytical isomorphism is the beginning of the inquiry rather than its end-point. Differences, then, are to be captured in the enactment of relationships rather than in given substances.\(^\text{14}\)

I return to the two offices housing the three interviews evoked in the beginning of the paper. There is the grand office of the National Board of Health and the smaller one at Randers Central Hospital; there is the head of department of the 6\(^\text{th}\) office of the National Board of Health, the project manager of the order-entry module, and the “lay” nurse in Randers. All of these actors can be viewed as isomorphic in their forming networks of relationships. For example, in my interview with the head of department he continually
discusses his associations with other actors and he worries about their strengths and weaknesses. But on this level of characterisation, his activities seem no different from those of the nurse in Randers. It is just that whereas the head of department invokes standards, agencies, hospitals, governments, and regions as his relevant associates, the nurse invokes software developers, clinicians, patients, and cardboard mock-ups. Regardless of the presumed scale of these interactions each seem obsessed with the relationships, which make their current interest and activities coherent and meaningful for themselves and others.

A second point follows. That is, that in the making explicit of relationships (which I am trying to facilitate in my interviews) quantity of information seems indeed to remain constant in each instance. While from a distance it may seem obvious that the National Board of Health is large and well connected, whereas a development group at Randers Central Hospital is rather more peripheral to the “grand picture”, this perception changes drastically upon immersion in practice. In all interviews dozens if not hundreds of people, places, groups, meetings, organizations, institutions, and artefacts are brought to bear on the questions at hand. As in fractal pictures, such as Mandelbrot’s, complexity seems reproduced regardless of the detail on which one zooms in.

Under this description it comes as no great surprise, therefore, that actors’ display a “widespread interest in the elaboration of relationship as such”. For relationships are what these actors seem made of; consequently reproducing relationships is an act of self-reproduction. For Strathern this seems increasingly the case in contemporary Western society, where

nothing is in fact ever simply part of a whole because another view, another perspective or domain, may redescribe it as ‘part of something else’. When that something else is perceived as a context or underlying assumption, the very grounds on which things appear become another perspective upon them…Perspectives themselves are created in the redescriptions (Strathern 1992: 73)

In their vivid descriptions of strong or faltering associations between, for instance, the National Board of Health and Danish Regions or the order-entry module and the integration machinery, my interviewees can thus be seen as not so much representing an external situation as enacting their important relationships into continued existence.

But while the actors and their many activities can be viewed as analytically
isomorphic in their interest in and capacity for forming relationships, these relationships are themselves different; for they are composed of different elements and to different effects. And these differences offer themselves easily to cultural critics as needing evaluation.

6. Fractal Relationships and Cultural Critique

Literal minded Westerners surprise themselves by finding metaphors. The discovery creates the possibility of a critique of society through language, treating texts as simultaneously literal (constructing social reality) and metaphoric (realising a social construct). Preoccupation with words themselves is part of the wider phenomenon of literalness that feminists recognise, one that prompts ‘science’ to know better and deeper, to see inside the bodies of things. The problem is that we do not know how to conceal what has been revealed, reassemble what has been taken apart, restore surface meanings. There ‘is no whole picture that can be “filled in”, since the perception and filling of a gap lead to awareness of the gaps’ (Strathern 1989: 63)

Along with other scholars in STS the description of the formation and enactment of relationships and the consequences of these are what exercises my imagination. From the point of view of a number of critics, however, stopping at such description is stopping short. For, to them, the necessary end-point of investigation is assumed to be evaluation of the activities and practices under consideration. Unwillingness to engage in such overt evaluation is then perceived as an inability or lack.

In this section I follow in some detail one particularly relevant debate about such issues, by commenting on the exchange between feminist and cultural critic Vicky Kirby and feminist and anthropologist Marilyn Strathern, which followed the publication of an article by the latter on the relationship between these two endeavors (anthropology and feminism).

In the article “Dislodging a World View: Challenge and Counter Challenge in the Relationship Between Feminism and Anthropology”, Marilyn Strathern points to some of the tensions between the (putatively) descriptive practices of anthropology and the (putatively) political and interventionist practices of feminism. She argued that although feminism has been increasingly taken seriously within anthropological practice, the term has primarily been adopted as a modifier, whereby “‘Feminist anthropology’ is …tolerated as a specialism, a ‘part’ which can be absorbed without challenge to the ‘whole’” (Strathern 1985: 5). But Strathern is equally worried about another ‘radical’
response, according to which the classical anthropological paradigm is *overthrown* by new feminist insights, with the consequence, beneficial to the radicals, that this gesture “constantly reconstructs feminism as offering challenge” (9). Strathern indicates a number of problems with the very idea of conceptualising anthropology after a paradigm model, which Kuhn originally used to describe scientific revolutions in the natural sciences (Kuhn 1970). But most of all she worries about the easy assimilation of theoretical academic practice to more straightforward political ends:

As a political device, feminism may have to use the rhetoric of ‘paradigm change’ in the sense of overturning existing premises based on male privilege. Yet in another sense the last thing we want is a world view. We would lose the flexibility of seeing the social world as necessarily containing many views. Who for instance, would wish for a ‘normative sociology’ or a ‘normative anthropology’? (20)

As we have already seen there are indeed several proponents of just such an endeavour; likewise as yet unmentioned research, such as Steve Fuller’s program in social epistemology (Fuller 1988) immediately springs to mind. In the foreword to the recent second edition of this book, for example, Fuller cites from the definition of social epistemology in the *Norton Dictionary of Modern Thought*:

The question for social epistemologists, then, is whether science’s actual conduct is worthy of its exalted social status and what political implications follows from one’s answer…those who say “no” [it is not so worthy] address the more fundamental issue of determining the sort of knowledge that people need and the conditions under which it ought to be produced and distributed (Fuller 2002: ix)

Commenting on these possibilities Fuller immerses himself fully in a rhetoric of radicalism by emphatically stating that: “I count myself among those social epistemologists who continue to say “no” (ix). In fact, the idea of normative sociology and anthropology links scholars of otherwise quite dissimilar inclinations. For example, feminist and cultural critic Vicki Kirby share the normative impulse, too, as she made clear in direct response to Strathern’s article in her “Capitalizing Difference: Feminism and Anthropology”.19

Kirby discusses Strathern’s article from an avowed postmodern and deconstructive perspective. Commenting on Strathern’s scepticism towards a reconciliation of feminism and anthropology Kirby responds: “In other words, Strathern wants to discourage both a rigorous feminist practice within anthropology as well as its inverse, a critical, anthropological approach within feminism (Kirby 1989: 2). She reads Strathern as

“*Infrastructural Fractals*”
encouraging separation and “binarism” between the two modes of inquiry, and suggests that this is a way of “centr[ing] the subject in a secure relation to the familiar topography of Truth, Progress and Reality” (Kirby 1989: 3). Kirby’s interest is to unsettle this familiarity by deconstructive tools as found in Derrida and writing in, and inspired by, recent French feminism in order to re-capture “difference differently” (Kirby 1989: 3). Only by doing so, she suggests, can anthropology reflexively engage with the unequal power relationship its enterprise inevitably instantiates by representing the other. Which is of political consequence:

A type of textual ‘terrorism’ can re-organise readings/meanings by laying claim to colonised ground and making visible the political economy which has worked to silence this occupation (Kirby 1989: 21)

In counter-response, Strathern evokes cross-cultural comparisons drawing on Melanesian material.

The exhilaration of discovery for Westerners is that the further one probes, one will bring to light new phenomena that will affect how one views the covering layers. Or mimics the constraints of language by pushing it to excess and thus bursting its capacity for limitation. If society distorts women’s experiences, making those experiences explicit could yield a basis for a different kind of society (Strathern 1989: 62)

This is in contrast to a Melanesian understanding: “Since Melanesians compose and recompose their bodies, language simply works alongside that process. With its own outsides and insides, they use language to draw attention to the analogical facility itself” (Strathern 1989: 62). Elaborating on such ideas, Strathern suggests that cultural criticism can be accomplished through deconstruction only in a society that imagines society as layered, so that secrets can be uncovered. However, if relationships are understood as constantly created and re-created on the surface of things, a different imaginary is called for. In this imaginary is what I am interested in the present text; I think it is one which severely compromises hopes for normative gains of the sort advocated by Fuller in his suggestion that sociology is the “ultimate metascience” (xiii) because all other sciences are simpler not only in their topical areas, but also in their self-understandings (xiii-xiv). Before considering it in more detail I want to make a further digression into the debate between Kirby and Strathern. For the relationship between anthropology, ethnography, and their potential political leverage and authority is at stake in the discussion.
Kirby accuses Strathern of ignorance, or at least naivety, in her refusal to interject a critical feminist and interventionist agenda into her anthropological one. Figuring “difference differently” would enable her to do so. But strangely the called for deconstructive attention to the subtleties of language does not extend to Kirby’s reading of Strathern’s own text. Rather than ambiguously fluctuating, her sentences are read in a realist vein. Strathern comments on the fact herself: “Kirby’s literal, face-value reading of what is taken as my principal ‘prescription’ (‘we have good reason to actually keep away from dialogue of this kind’) only works because my words are not credited with the slippage, lability, irony, and all the virtues of trespass that Kirby’s own words advertise.” (Strathern 1989: 26-7). There is a reason for this, and it is located in the uneasy relationship between deconstruction as a method of analysis that would destabilise all signification and an agenda that can only tolerate the destabilisation of select ideas. The reification of Strathern’s text is thus a necessary component in Kirby’s argument. For if she did not so reify it no critical purchase would be gained from deconstructive analysis. Thus Strathern is made a victim of strategic essentialism.

It is strategically possible to take up a universal truth (a Western or masculine perspective) in order to undermine it with another (Kirby 1989: 21)22

As her text exemplifies this is, indeed, possible, but at the cost of no longer reading carefully the work under examination.23 My characterisation of Strathern as a victim is therefore deliberate, for as Kirby herself emphasises her text can surely be seen as having an effect of terrorism.24 As I have indicated, however, what is lost in the terrorist act is the relevance of the deconstructive apparatus. For it seems to be precisely when deconstruction is wielded as a critical weapon, with the purpose of undermining or, indeed, destroying a point of view, rather than making explicit its internal mechanism, as exemplified in much of Derrida’s work, that it becomes unusable as an analytical tool. Strathern points to the problem in her response:

The problem is that we do not know how to conceal what has been revealed, reassemble what has been taken apart, restore surface meanings (Strathern 1989: 63)

Strategic essentialism can then be seen as the attempt of the cultural critic to restore essential differences after deconstruction has revealed their contrivance (and thus non-essentiality). But for just that reason the device does not work.
Deconstruction remains an interesting and viable method of analysis precisely insofar as one does not try to attach it to a specific normative agenda, and undoubtedly the same goes for post-human (or fractal) studies in STS. Whereas Steve Fuller would like to replace anti-social epistemology with social epistemology, Strathern would prefer to develop a fractal anti-epistemology: the “anti” signalling that the meta-level has disappeared. Thus, rather than criticising, an important point would be to learn how to come to terms with the unpredictability of events.

7. Unpredictability

I prefer complex trajectories to blurred genres. They give us marginally more purchase for dealing with the unpredictable (Strathern 1999: 25)

After this detour, it is perhaps worth restating the trajectory of the text. Empirical complexities in investigating the electronic patient record, has suggested to me the fruitfulness of viewing the current transformation of infrastructure as fractal, rather than based in a micro-macro distinction. The fractal view exempts the scholar from scaling contexts, people, and places as to their size and importance, but enables the observation of how people are themselves constantly instantiating scales and hierarchies through building and maintaining relationships with other people, with institutions, and with objects. While this allows me to point to the hierarchising effects of these activities it does not enable easy evaluation or overt criticism of the practices observed for, as discussed at length above, this would entail adopting a version of the highly troublesome idea of strategic essentialism. But if complex trajectories are what we have how does that allow us to deal with the unpredictable?

I began my research on the development of electronic patient records by reading governmental reports and searching Danish newspaper databases for information, and I quickly realised that an important project took place in the region where I lived. This seemed obvious to investigate so I sent a letter to the project manager and asked for an interview. The interview was only partially successful, since I knew little about the project at the time and consequently had difficulties formulating pertinent questions. It was nevertheless good enough to generate a number of new ideas for topics and get
suggestions for new contact persons. I moved on to talk to a number of these people about a number of these topics. In this process there is an “illimitability of a certain kind” (Strathern 1999:240), which is also well known to followers of the Latourian dictum to “follow the actors” in order to uncover an actor-network (Latour 1987); there are always more people to talk to, more situations to participate in – and how do you choose the right one’s? Inevitably, it seems, engagements with the field are partial.

A first kind of unpredictability in ethnographic STS-studies would then relate to the researcher’s initial lack of knowledge of the field and the impossibility of covering everything. Rather than neutrally observing, the ethnographer is forced to make “critical decisions” (Strathern 1999: 240) about where to go and what to do. This is not the lone predicament of the ethnographer but one, which is shared with other actors trying to accomplish tasks in environments they are not fully controlling; that is, everyone (Latour 1999).

So there is an initial unpredictability as to what is in the field; what connects to what. But there is also another unpredictability, which is shared between the ethnographer and other actors. It is the unpredictability as to what will be the effect of the relationships that are found in place. I encountered, for instance, a (seemingly unstable) relationship between the National Board of Health and Danish Regions. I found (seemingly well-functioning) relationships among software companies working together on the Aarhus project and between these companies and the project organization. I also found widely varying relationships between programmers, their individual coding, and the common product they were trying to construct. But the consequences resulting from each of these relations remained uncertain.

Would the National Board of Health, for example, be able to enforce their semantically standardised model of EPR’s on the regions, possibly with the help of punitive measures, or would the regions be able to defend their agenda of individualised development? Would software companies and the project groups in Aarhus maintain friendly working relations as deadlines near? Would the intricacies of programming turn into severe economical obstacles, or would they be smoothly integrated? The consequences, in each case remained, indeed, uncertain, but all the involved actors work zealously to test these relationships, and thereby trying to achieve certainty. And the STS-ethnographer watched them as they did so.
Achieving certainty and stability in its relationships is what makes a practice (or person) strong; having relationships fall apart is what makes it weak.\textsuperscript{32} However, the constant work to do so remains invisible if one comes to the field with the belief that size and power is an already settled matter. This is one “marginal purchase” of following the “complex trajectories” of all of these actors.

Whereas I have refrained from using a specific scale as a resource in understanding the activities of actors, these, of course, are avid users of multiple scales. Indeed, it is to better be able to focus on the scales enacted by the actors themselves, that fractal studies suspend their use.

8. Changing Size: Fractal Actors and the Enchainment of People

Elaborating (while also functioning as an inspiration for) Strathern’s work on the fractal in social anthropology, Roy Wagner discusses “The Fractal Person”: “A fractal person is never a unit standing in relation to an aggregate, or an aggregate standing in relation to a unit, but always an entity with relationship integrally implied” (Wagner 1991: 163).\textsuperscript{33} But if a nurse is not a unit part of a larger aggregate called the health care system, what does it mean to claim that her relationships are “integrally implied”, that she can be viewed as a “conglomerate” (Strathern 1992: 83)?

I would venture that an important difference between this view and the received dualist one has to do with focusing on the interdependence between people, situations, and practices (regardless of their presumed scales) rather than on some set of factors becoming capable of determining others. The reciprocity of effects thus becomes an issue. I would imagine, then, that there is reciprocity, if indirect, between the situations of the three actors I have invoked in this text, the administrator in Copenhagen, and the informatician and nurse in Randers. The uncertainty of effects, as described above, pre-empts the possibility of claiming that the actions of one unilaterally impinges on those of the other, for it might turn out that the opposite is also the case.

In fact, it is possible to point to reciprocal interchanges between ”local” projects and ”non-local” Danish and EU initiatives. To return to an earlier example, standards for EPR development are constructed under the auspices of CEN (Comité Européen de Normalisation), technical committee TC-251. The members of this group are from various national standardisation organizations; in Denmark from Danish Standards.
Danish Standards have made a “mirror group”, which closely follows the EU work and discusses its possibilities and problems, and which sends representatives to meetings in the committee to influence the course of development. Who are the participants in such a group? Not only are there representatives from the National Board of Health and Danish Regions, participants also came from TietoEnator HealthCare A/S and Systematic A/S, from Aalborg University and from the Business School in Copenhagen. At this point we begin to see the contours of reciprocity, for Systematic is developing the order-entry module in Randers, and constantly updated on the needs of clinical personnel by the project group there, as the nurse from my interview made clear. And the project manager of this module received her Master’s diploma from the medical informatics department at Aalborg University.

Thus, while it might initially look like EU bureaucracy is dictating requirements from nowhere (or from somewhere very distant and powerful) for the National Board of Health in Denmark to adopt and impose on Danish Regions who, again, enforce it on local projects, actual dynamics are much more interrelated. Nothing seeps from the ”large” to the ”small”, but many things are transported from one practice to another, bypassing distinctions between the “individual” and the “societal” or the “small” and the “large” in the process.

In a dualist framework such movement could be considered a version of the butterfly effect but this is not a precise description; for while the butterfly effect is imagined as a small encounter leading to a large impact elsewhere, the view here entailed would just suggest an encounter somewhere (albeit a somewhere often considered local) leading to an encounter elsewhere (although an elsewhere often considered non-local).

But if encounters are all there is, and the results of them are not pre-given, then research into the genealogies of seemingly stable relationships seem called for. For it would be in the “enchainment of people” (Wagner 1991: 163, STS-researchers Latour and Callon 1981 would add, and objects), that the micro and the macro were found as effects. In Callon and Latour’s narrative:

At other times actors who had always defined themselves and had always been defined as micro-actors ally themselves together around a threatened district, march to the town hall and enroll dissident architects. By their action they manage to have a radial road diverted or a tower that a macro-actor had built pulled down…A tiny actor becomes a macro-actor, just like in the French nursery rhyme: ‘The cats knocks over the pot, the pot knocks over the table, the table knocks over the room, the room knocks over the house, the house
knocks over the street, the street knocks over Paris: Paris, Paris has fallen!’ (Callon and Latour 1981: 295-6)

The tale of Callon and Latour is both similar and different (that is, also, fractal) from Strathern and Wagner’s. For while the latter stresses the many ways in which relationships are enfolded in persons whose movements cross what is regularly analysed in the dualist framework of the micro or macro, individual or societal, the former, with the children’s rhyme just cited, stress how actors are continually changing size; an insight, which is also enabled by analytical isomorphism. Wagner and Strathern points out that a multiplicity of scales are always in play in social interactions but Callon and Latour emphasise that the effect of these interactions may well be to re-figure the balance of power and size of actors.  

In both cases, however, it is because initial belief in pre-determined categorisations of micro and macro, important and unimportant are suspended that it becomes possible to see how fractal actors are able in practice to change sizes – from small, to large and, possibly, back to small again.

9. Fractal Sociologists and an Alternative Leviathan

Roy Wagner’ article has a nice reflexive twist, for he points not only to the scales used and invoked by other actors but also those adopted by anthropologist and sociologists in coming to terms with social activity:

Special terminologies are pressed into service to focus attention on the form of reduction or scale-change intended – behavioral, psychological, symbolic, economic, or ecological. The result is that many forms of heuristic ‘order’ are attached to the subject as scale-changing heuristics can be imagined: that once system and order are assumed to be what society is doing, the anthropologist is given carte blanche to propose alternative heuristics (Wagner 1991: 172)

This point is also by Callon and Latour but with a different emphasis. The citation is a reminder of the reflexive implications of analytical isomorphism, for the researcher is no better located to observe the proper scales than any other actor is. It can be suggested that this situation facilitates the perpetual proliferation of methods, issues, perspectives and theories inhabiting the fields of social science. But as Callon and Latour points out, such calm observation is also no longer the issue. For the sociologists as all other actors, are participants in the construction of scales, not neutral observers of them:
So what do sociologists do? Some say there is a social system...What else does the sociologist do? He or she interprets the Leviathan, saying for example that it is a cybernetic machine. Here again the Leviathan is built up by this type of description: it is proud to be a machine and immediately, like any machine, starts to transmit forces and motions in a mechanical way. Of course this interpretation is added to all the others and struggles against them. For the Leviathan is – sometimes and in some places – a traditional and not a cybernetic machine, likewise a body, a market, a text, a game, etc.. (Callon and Latour 1981: 297)

Isomorphism: like all other actors the sociologist and anthropologist try to make durable associations; often, however, without realising that this is what he does, for, as Wagner suggests, the assumptions he brings to bear on his activities seem to offer a carte blanche for making new proposals. One such “alternative heuristic” has been that of the fractal approach: the heuristic of the partiality of any heuristic.

This is not a view that endorses the equality of all viewpoints, nor does it entail the foreclosure of all debate. Rather it opens up for continued debate and investigation. For as partiality seems a condition of social participation, rather than something one can or should avoid, what most arouses suspicion would be self-totalising gestures as encountered in theory and elsewhere in practice.

10. Scale Matters

Much of the above text has been inspired by Marilyn Stratherns’ “Environments Within: An Ethnographic Commentary on Scale”, where she discusses, as I have, scaling as a double process. But her article has an extra turn, to which I will now pay attention. Strathern’s argument relates, as it often does, to Melanesia, but translates interestingly into Danish health care. Scaling is a double process, Strathern suggests, because its effects are dependent on keeping some things constant, while varying others. In the article the example is inflation. In the nineteen fifties and sixties pearl shells became available on an unheard of scale on Papua New Guinea. But this scaling up did not diminish interest in shells; rather it re-focused islanders’ attention to their quality: “their critical judgment keeping pace with the new opportunities” (Strathern 2000a: 47). Thus, “shells retained their signification of wealth and strength”. The playing out of the relationship between the constant and the variable made for unpredictable effects in the Melanesian context.

In Denmark, and the Western world more generally, information technologies have
been increasingly thought about, developed, used, and valuated over the last few decades, with a multiplicity of effects. As discussed in earlier chapters these technologies are imagined as facilitating communication, rational decision-making, quality assurance, effective planning, and research. For all these reasons electronic patient records are seen as inexorably leading to improved patient care, if they could only be properly developed and implemented.

Some things are kept constant, some varied. Increased technological possibilities have surely facilitated the contemporary imaginary. At the same time, this imaginary in itself has remained fairly unchanged. For when, indeed, did hospitals not aim for good communication, effective planning, rational decision-making, and quality of patient care? However, as in the Melanesian case, the relationship between medical relevancies and medical means in the form of new technologies, could bring with it unanticipated effects. I think here, for example, of the computational “lure for feeling”, which encourages that quantification and accounting procedures be adopted into “the most variegated aspects of social life” (Brown, Ashmore and MacMillan 2003: 17).

This propensity of the technological imagination renders pertinent Strathern’s conclusion: that scale both matters and does not. Scale clearly matters, since an increasing amount of money is poured into development projects with increasing hopes attached to them (and increasing disappointments when they fail). Scale matters, too, since one derivative of the IT-explosion is a general awareness that these technologies enable quantification of ever more aspects of life and with that, it seems, an inexorable feeling that this capability carries its own telos; that since measurements can be made they must.

But scale also does not matter. For imagining our responsibilities as they pertain to this new situation “draws, as it were, the environment within ourselves” (Strathern 2000a: 65). It will then be up to our imaginary to learn to cope with and, perhaps, re-direct, the scales of relevance and importance that are currently being enacted in the developing infrastructure.
Acknowledgment: I would like to thank Marilyn Strathern for her comments. I am grateful to Christopher Gad for his careful reading and suggestions.

1 Thanks go to Manuel de Landa for directing my attention to the volume.
2 For example: “Giddens and [Randall] Collins have tended to bring theoretical options 1 and 2 (instrumental and interpretive individualism) into contact with option 5 (objective structuralism). By contrast, Habermas’s sensitivity to cultural gestalts has led him to connect option 5 with linkage arguments that stress homology and socialization (option 4)” (Giesen and Alexander 1987: 33).
3 In the quite different fields of biology and psychology the terms Nature and Nurture function in a remarkably similar manner as the glue that binds together an otherwise heterogeneous set of positions and in doing so prevents the development of other modes of understanding (Oyama 2000).
4 Acknowledging this, the authors refer instead to the intensity of the engagement with the issue rather than the novelty of the idea as constituting the new: “We suggest that in the present decade a quite different phase of theoretical debate has emerged, one marked by the serious ongoing effort within every theoretical tradition and from both sides of the great divide to link micro- and macro- perspectives (Alexander and Giesen 1987:31). In spite of this increase in intensity the authors nevertheless imagine that readers might not have noticed it, and paradoxically the endeavour of the edition is therefore to bring it to attention: “Our purpose in this introduction, however, has not been to argue for or against any one of these proposals for linkage. Our purpose has been to draw a circle around all of them, to demarcate them as a new phenomenon in sociological discourse, and to commend this new discourse to the community in large” (Alexander and Giesen 1987: 37).
5 Contributors to the volume narrow their imaginative possibilities further, rather surprisingly, by refraining from consideration of the work of relevant recent French theorists such as Pierre Bourdieu and, especially, Michel Foucault (e.g. Bourdieu 1987, 1990, Foucault 1980a, 1988, 1991, 2001). One contribution briefly notes and adopts an article of Michel Callon and Bruno Latour from Knorr-Cetina and Cicourel (ed.) 1981. I refer to this specific piece later in the argument.
6 Latour has commented sceptically on the increasing use of fractal imagery in STS. In his contribution to Actor Network Theory and After he suggests that “The topology of the social…is rather bizarre, but I don’t think it is fractal. Each locus can be seen as framing and summing up.” (Latour 1999a: 18). The problem he perceives with fractals has to do with its suggested analogy between the “large” and the “small”; differences seem effaced in the process. On the one hand this points to an important suggestion made by Callon and Latour: that the putatively large is of the same kind as the small, but amplified to generate a different order of effects, a point Latour finds made earlier by Gabriel Tarde (Latour, pers.comm and Latour 2001). On the other hand the criticism of isomorphism as leading to an inability to perceive differences seems to be a fairly exact replication of what humanist critics regularly say about Latour’s work – that clearly actors have different capacities – so why choose not to recognise them? Given the history of these accusations towards both earlier and current versions of (post-)ANT it would probably be worthwhile for Latour not to reiterate this gesture too quickly. In any case I refrain from doing so in the following.
7 In a recent piece Steve Woolgar comments on “some dynamics of duality interrogation” (Woolgar 2002). His paper points to the fact that dualisms seem to become entrenched and consequently exceedingly hard to get rid of – as he emphasises, work tends to be directed to “connecting” the sides of dichotomies rather than dissolving them. So in spite of his earlier call for a “glorious bonfire of the dualities” (262) he is now rather circumspect: “Yet in our explication of this duality we remain prisoners of the language conventions that both support and derive from just this duality…Bonfires clearly aren’t enough.” (269). If by this he means that additional work to construct alternatives is needed after having rejoiced in the “bonfire of dualities”, surely he is right. If on the other hand he suggests that since dualism seems so hard to get rid of we might as well retain its poles and try to re-work it from within, then this is a move distant from the one made in the present text. It is also one, I think, which runs counter to the scholarship of Strathern and her colleagues in STS, not least Michel Callon and Bruno Latour (see also note 6).
8 The following discussions of scales and scalings are heavily indebted to an article of Marilyn Strathern’s “Environments Within: An Ethnographic Commentary on Scale” (Strathern 2000a). I discuss this article explicitly in the concluding section of the paper “Scale Matters”.

“Infrastructural Fractals”
For details on fractals as a mathematical idea, see Gleick 1988.

This paragraph is indebted to Christopher Gad’s exploration of Strathern’s work in his Master’s Thesis *A Post-plural Attitude: Reflections on Ontology and Subjectivity with Post-Actor Network Theory and Artificial Insemination* (Gad 2004 unpubl.)

In the preface to *Belief and Resistance: Dynamics of Contemporary Intellectual Controversies*, Barbara Herrnstein Smith describes the phenomenon in the following way: “The experience itself is common: an impression of inescapable noise or acute disorder, a rush of adrenalin, sensations of alarm, a sense of unbalance or chaos, residual feelings of nausea and anxiety…As well as sensory or aesthetic, the percepts that elicit cognitive dissonance can be more or less intellectual and, in fact, textual. Thus a sense of intolerable wrongness in some journalist’s description or fellow academic’s analysis can set the mind’s teeth on edge and produce a frenzy of corrective intellectual and textual activity…In all these cases, the tendency, understandably, is to end the pain, to get things to be, feel, or look right (or “normal”) again.” (Smith 1997: xiv-xv).

This has been the inevitable chorus accompanying criticisms of actor-network theory and related post-human perspectives in science and technology studies (e.g. Feenberg 1999, Pels 1996, Radder 1992, Vandenberghe 2002, Winner 1993).

This is a variant of what Barbara Herrnstein Smith terms the “egalitarian fallacy”, and describes in the following way: “I call this general supposition and argument the Egalitarian Fallacy. It is a fallacy because if someone rejects the notion of validity in the classic (objectivist) sense, what follows is not that she thinks all theories (and so on) are equally valid but that she thinks no theory (and so on) is valid in the classic sense. The non sequitur here is the product of the common and commonly unshakably conviction that differences of “better” and “worse” must be objective or could not otherwise be measured” (Smith 1997: 78-79). Proponents of ANT has struggled with this non sequitur for years, but without much luck in making themselves understood, as seen from the recurrence of just this criticism. In the present text we will see how otherwise avowedly radical deconstructivist Vicki Kirby repeats the gesture in her criticism of Marilyn Strathern and in her espousal of essentialism as the necessary ground of political efficacy. See note 22.


The organization Danish Regions was founded in 1913 to represent the interests of all 14 Danish regions and provide them with services and information. Danish Regions promotes and supports the principles of regional autonomy and acts as a spokesperson for the regional councils in all matters related to the central government and other national associations. It also serves as the central collective wage bargaining organization of the regions (http://arf.dk/English/DanishRegions.htm). The 14 regions are responsible for all public hospital services in Denmark and they also administer the National Health insurance service. Under this service all persons with permanent residence in Denmark have a right to receive help from doctors and specialists free of charge. (http://arf.dk/English/RegionalTasks.htm).

Here it is to be noted that one can only know what the elements of the relationship are, as a consequence, of their successful participation in this relationship – this enable their definition to be established. This is a Spinozist point: We do not yet know what a body (an actor) can do, but now we know that it can do this. See Deleuze 1988, 1990, Spinoza 1959.

I say “putative” to stress that these designations are from the point of view of numerous contributors to the respective “fields”. This is of consequence since in her reading Kirby takes this description of received views as Strathern’s held normative position.

In her article “Writing Against Culture”, Lila Abu-Lughod likewise comments on the piece (Abu-Lughod 1991: 138-9, 141), suggesting that “In highlighting the self/other problem, Strathern takes us to the heart of the problem”, but arguing that, having identified this central problem Strathern nevertheless shuns “the problematic of power…in her strangely uncritical depiction of anthropology” (138). Abu-Lughod’s proposal is therefore that “the awkwardness Strathern senses in the relationship between feminism and anthropology might better be understood as the result of diametrically opposed processes of self-construction through opposition to others – processes that begin from different sides of a power divide” (139). Arguably, however, it is Strathern’s keen attention to problematics of power and empirical

“Infrastructural Fractals”
detail that makes her unwilling to thus characterise the relationship between anthropology and feminism. For close attention to the heterogeneities of both of these practices and to those they observe and talk about makes viewing them as simply opposed, looking from different sides of a power divide, rather implausible. An alternative model is that of a fractal relationship as discussed in this text and in Strathern 1991.

Kirby cites Simone de Beauvoir, Rosi Braidotti, Luce Irigaray, Alice Jardine Michele Montrelay and Gayatri C. Spivak, along with Pierre Macherey and Edward Said.

In later work Strathern makes explicit this idea: “At certain junctions…I suggest that ways in which ‘Melanesians’ objectify social relations could enrich the impoverished conceptual repertoire with which ‘Euro-Americans’ seem lumbered; however, there are warnings as well as delights here” (Strathern 1999: 24).

The position is restated by Kirby in her recent book *Telling Flesh: The Substance of the Corporeal*, here in a formulation attacking Toril Moi: “In her fervid desire to remain unsullied by essentialism, Moi forgets that essentialism is the condition of possibility for any political axiology: the minimal consensual stuff through which political action is engendered is already essentialism’s effect” (Kirby 1997: 71–2, see also 97–99 and 149–163). Kirby registers her “irritation over knee-jerk repudiation of essentialism” (155), her “increasing impatience with the morphology of such arguments” as states that “universal knowledges stand condemned by situated, local understandings; anti-essentialism provides a remedial against essentialist reductionism” (150–151). She also remarks on her own prior endeavors to show how “certain attempts at reflexivity within the discipline [of anthropology] are sometimes cloyingly self-congratulatory in ways that actually avoid the most troubling implications of translation” (180 n.3), and here she cites “Capitalizing Difference”, without however, referencing the responses this text has given rise to. As these instances vividly articulate Kirby remains under the assumption that one must always return to essentialism, always remain in a relationship with the “metaphysics of presence”. Another option, however, would suggest that for researchers such as Strathern (along with Donna Haraway and Zoe Sofia who receive critical treatments in Kirby 1997: 129–49) it is less a matter of forgetting this putatively inevitable truth than of attempting to actively re-figure the notion and implications of political axiology. Kirby is, of course, welcome to believe that this is impossible for theoretical reasons found in Derrida. But even in that case her moralising discourse is surely less than likely to convince the (benighted) “victims” she analyses and corrects. Strathern, for instance, is not struck by the happiness of Kirby’s tone and comments: “But whereas men remain sexed, women also appear as ‘feminists’, charting an intellectual geography and doing good things like appreciating epistemological breaks. A relation between the encompassing and the contrary: more than binarism, this is hierarchy” (Strathern 1989a: 29). As it happens a number of other scholars also do not share Kirby’s assumptions about the conditions of possibility for political efficacy. For discussions of non-critical axiologies (political and otherwise) see in particular Smith 1988, 1997, also Deleuze 1994, Stengers 2002.

Arguably the insistence on careful reading is otherwise one of the main strengths of deconstruction.

In *The Parasite*, Michel Serres discusses how cultural and scientific advancements seem predicated on relentless denunciation of that which came before. Inspired by Rene Girard (e.g. Girard 1978) he characterises such critical achievements as following a sacrificial logic (Serres 1982a, see also Brown 2002, Smyth 2002), and Kirby’s text exemplifies it well. Serres aims at developing different ways of making knowledge and has been a great source of inspiration for Bruno Latour (e.g. Latour 1987a, 1990a, Serres 1995) and others in STS, although Latour’s denunciations of other positions sometimes seem to mimic parasite logic rather more closely than they might have.

This point, too, is remarked upon by Strathern: “Yet I think I know what my real solecism is: if I slip anywhere it is between political positions. And that is an inadmissible form of betrayal – whereas the prescriptions laid out in CD [‘Capitalising Difference’] all take the perspective of a single political position, which allows one to appear to be slipping between (‘betraying’) everything else. But I am not patriot enough to be a terrorist; dutiful sister is not it seems to me much of an exchange for dutiful daughter” (Strathern 1989a: 27).

Discussed, for instance, in Miller 1997.

To cite, yet again: “Social anthropology bases its practice on what we might call the unpredictability of initial conditions, unpredictable, that is, from the viewpoint of the observer” (Strathern 2000a: 44).
This can be ascertained, for example, in the supremely non-reflexive Arguably, Bruno Latour has changed position (although I would claim it has been more often a matter of continued delays and frustrations. Regions have been threatened with economic measures if they fail to comply with the guidelines for development provided by the National Board of Health but at present it seems as if the negotiating of the future dissemination of EPRs has calmed down. Possibly this relates to the frustrated position of the Danish Regions, which are increasingly regarded as an outmoded political unit to be dissolved. Unfortunately, as it (often) happens, the former seemed to be the case. The first tests of the Aarhus system displayed severe performance problems, which have continued to haunt the project to this day. The implementation phase has therefore been slowed down significantly, and includes in the winter 2004 only small parts of what is imagined to be the integrated system in its entirety. Whether this will ever be realised remains questionable.

As an addition to references mentioned in note 15 can be added Strathern and Godelier Big Men and Great Men, in which is detailed the heterogeneous means and measures used to establish oneself as an important actor in Papua New Guinea.

In his discussion of contemporary (postmodern) organizational theory, Rolland Munro dismisses the metaphor of the fractal as elaborated by Wagner as merely fashionable, and claims more substantiality and sophistication for a Derridean account. His two sentence disqualification does not seem to me to demonstrate the irrelevance of Wagner’s analysis but surely the potential for deconstruction for STS-studies remain largely unexplored (I discuss some issues in chapter seven). This more or less recapitulates the difference between the accounts made under the symmetry doctrine defined by sociologist of scientific knowledge David Bloor (Bloor 1976) and those made under Callon and Latour’s program of generalised symmetry (Callon 1986, Callon and Latour 1992) as discussed in chapter one. Arguably, however Strathern and Roy Wagner are considerably more sophisticated in their ascription of “interests” to the actors they describe than most practitioners of SSK.

Arguably, Bruno Latour has changed position (although I would claim it has been more often a matter of elaborating his position) on a number of issues since the publication of this text in the early eighties, but this is not one of them. Thus, in 2003, we can read: “Instead of the surfaces so typical of the first modernities – the ‘domains’ of science, or economics, of society, the ‘spheres’ of politics, values, norms, the ‘fields’ of symbolic capital, the separate and interconnected ‘systems’ so familiar to Luhmann, where homogeneity and control can be calmly considered – we are now faced with the rather horrible melting pot so vividly described by historians and sociologists of science. But contrary to what social scientists believe, those melting pots are neither beyond description nor beyond political action: they just require other definitions of what empirical enquiries and representations mean” (Latour 2003: 38).

This can be ascertained, for example, in the supremely non-reflexive The Micro-Macro Link. I mention this book not to particularly vilify it, since in this regard it seems no better or worse than many other books in social theory, but merely because it has already been the focus of attention in the present text. In Hans Haferkamp’s article in that volume, the Callon and Latour article from which I have just cited is positively invoked (Haferkamp 1987: 179-80), but it is, to say the least, unclear how he reads the text, for in the same article he suggests that “The micro level involves a small number of actors who are able to observe one another. The maximum number of actors is that of a small group, comprising about thirty persons who are in the same place at the same time” (177). “In contrast, the macro level has many actors who are not in direct interaction with one another” (178). This distinction, predictably, gives rise to a grid of range and meaning, where intentionality in the microarea equals complex meaningful action, non-intentionality in the microarea equals behavior structures, intentionality in the macroarea equals planned associations of actions and non-intentionality in the macroarea equals creation of social structure (179). That Callon and Latour can be used as support for such an argument seems to me absurd in a
somewhat hilarious way. It certainly also offers its own comment on the sociological politics of reading and adopting other texts.


39 To try to absolve myself from being accused of “binarism”, I refer again to Strathern, who qualifies in the following way: “All that remains to be added is that if its double senses suggest a dualism, let me repeat that this pair (the two senses of the concept of culture) is not binary, dichotomous, or dialectical. Rather, each element has its own complex trajectory. For the sake of the present topic, I have characterized the two trajectories as sensitivity and insensitivity to scale change” (Strathern 2000a: 65).

40 For various suggestions to the consequences of this tendency I refer to the references cited in note 37.

41 Strathern suggests that this feeling relates to the idea that technologies have the ability to enhance people or practices: “The problem of excess comes when technology is regarded as enabling, as a prosthesis that enhances personal performance, and when people become obliged to demonstrate they have been enhanced” (Strathern 1995a: 26), however, characteristically, this process can also go in reverse: “the person ‘uses’ or ‘exploits’ the technology, as the technology ‘determines’ or ‘allows’ the person to do this or that” (Strathern 1992: 83).
Part four: Implications

At this point the analysis more closely resembles vivisection than critique. For the question is not “how closely do these ideas approximate the truth,” but “what effects do these ideas (which may or may not happen to be true) bring about?”…To say what such an apparatus does is not a critique, still less a refutation. Would we say that the vivisection of a frog constitutes a critique? Or that it aims to “refute” the frogs’ organs? When performing such cold-blooded operations, neither correction nor judgment is called for. One can only aim to be, in Nietzsche’s terms, a good physiologist (Ferguson 1994: xv-xvi)

This final part is called “Implications” and has two chapters. Chapter eleven is called “Technologic: Conceptualising Organizational Transformation with the EPR”. I suggest that a technological grammar or pattern of sorts can be detected in the empirical material encountered so far. This ‘techno-logic’ resembles what Jacques Derrida has called a logic of supplementarity. I discuss how “organization” and “practice” function as supplements to “technology” in the imaginaries of Danish EPR projects. By analysing some of the conceptual and practical problems this technologic poses for the EPR project in Aarhus, I want to open up for a consideration of the benefits and capacities of non-modernist STS-studies for participating or “intervening” in such projects.

Chapter twelve “Established Sentiments, Alternative Agendas, and Politics of Concretisation” moves this discussion back to the interior of STS. I outline a set of arguments by Barbara Herrnstein Smith and Isabelle Stengers, which have been especially formative for the present analyses. These arguments seem initially highly diverging, since Smith encourages “extremity”, while Stengers warns us to not “overturn established sentiments”, but behind this superficial opposition a common topos emerges, exciting for non-humanist, post-classical students of science and technology interested in theory and intervention. In the course of developing this argument I carry out a critical conversation with Marc Berg’s recent work, which urges contemporary STS to follow a normative and practice-guiding trajectory.
Chapter eleven:
Technologic: Conceptualising Organizational Transformation With the EPR

1. Introduction
So far I have discussed a number of issues in STS and social theory more broadly by drawing on empirical material from various sites involved in the Danish networks of EPR construction. While partially overlapping both in terms of “theoretical content” and “empirical material”, each argument has been developed in response to a specific set of issues. In this chapter I want to characterise the empirical material in slightly more general terms, in order to detect the emergence of what I suggest can be seen as a certain conceptual grammar of these technical projects and imaginaries, a “techno-logic”. This grammar resembles what Jacques Derrida has called a logic of supplementarity.

In the following I illustrate how “organization” and “practice” function as supplements to “technology” in the imaginaries of Danish EPR projects. This understanding is in several ways limiting for an understanding of the relations between technical development and organizational transformation, because it constructs a persistent enigma of organization. By analysing some of the conceptual and practical problems the logic of supplementarity entails for the EPR project in Aarhus, I want to open up for a consideration of the benefits and capacities of non-modernist STS-studies for participating or “intervening” in such sociotechnical projects. Their specific capacities for doing so will be discussed in the concluding chapter.

2. An Imaginary of Technological Projects
It does not, I think, require an eye made especially sober by the light of too many setting suns to find in the word new a certain pathos (Smith 1978: 157)

In contemporary Denmark technologies are proclaimed as harbingers of better times to come with an obviousness that merits consideration. As regards the empirical focus of the present work, the electronic patient record, the lines of argument are by now familiar; in a future, soon to arrive, they are imagined as simultaneously facilitating more efficient administrative practices, more productive health practitioners, vastly improved research
implications, co-ordinated interdisciplinary work-flows, rationalised medical decision-making and, increasingly, lowered costs.

As STS-studies have shown, technological successes are regularly circuitously seen as generated by the beneficial effects of the technologies. On the other hand when technological projects are viewed as failures, then the “technology” as such is persistently perceived as neutral and an extra component needs to be invoked in order to explicate what went wrong. Most often this component is perceived as having to do with something intangibly “social” or “cultural” or “organizational”, which promotes “resistances” of certain sets of actors towards the new technologies or prevent their efficient adoption (Latour 1986). Thus, an effect of the pathos of the new is brought into play, for those who promote new technologies can accordingly be viewed as struggling against social stasis in the name of a brighter future.

There is a paradoxical quality to this situation. On the one hand technological development is imagined as following a development that cannot be avoided, as when, in a recent IT-strategy for health care, it is suggested that:

A systematic development of the quality of health care activities can today hardly be managed without the use of electronic patient records (EPR) and clinical databases. The EPR will also ensure quality improvements, for instance, by lowering errors in medication (National Strategy for IT in the Health Care Sector 2000-2002: 9)

This line of thinking informs views of medical informatics and its practitioners as facilitators of technological progress. But they are also regarded as something like the “technological entrepreneurs” of health care practices; experimental pioneers, who must always move uphill against the conservatism of established norms and habits.

How to analyse this “double vision”, which enables problems of technological development to be interpreted in terms of something “inevitable” being “resisted”? A non-modern position, as here advocated, works to re-formulate the double asymmetry that turns technologies simultaneously into inevitable progression and positive achievement, while constructing social and organizational practices as blank surfaces for technological innovation and opaque domains resistant to change. It is concerned with what happens to the interpretive topology if one suspends assumptions according to which EPRs can be imagined as free-standing future-generating devices (see chapter seven), and surveys their socio-technical formation more broadly.
In order to clarify the entailments of this characterisation I want to here diagnose a “techno-logic”, which has become discernible through my investigation of the EPR-project in the Aarhus region. The logic as here presented is specifically tied to the Danish EPR landscape, but I am going to venture that several of its assumptions have wider currency, and that a number of its ramifications would be encountered in technological projects more broadly.

In general one can suggest that conceptions of the relation between technology and society (or organization or practice) operates in spaces, through which are promoted certain understandings of technological development projects, their possibilities and effects, and that practices of technological development are organized around these understandings. While such spaces are certainly productive in that they enable some experimental devices to brought into play, in this process of specification other possibilities are lost, forgotten, or never even considered. Each imagined set of efficacies, then, comes with its own blindness, or as Marc Berg and Stefan Timmermans write, elaborating on the work of Michel Serres, each order comes packaged with its own disorder (Berg and Timmermans 2000).

Given this understanding, the present text has pointed to some of the many ways in which the technological and the social and organizational, rather than being clearly distinct domains, can be understood symmetrically, as co-extensive, co-produced, fractal, and hybrid; this I have tried to illustrate by examples drawn from empirical studies of the EPR development initiatives in Denmark. I now re-visit several events in the Aarhus project in order discuss a specific “techno-logic” there displayed. I construct this grammar after Jacques Derrida’s logic of supplementarity, and diagnose some of its practical consequences.

3. The 80-20 Model and the Logic of Supplementarity

This play of the supplement, the always open possibility of a catastrophic regression and the annulment of progress… (Derrida 1976: 298)

As noted above the position of the medical informatician, as I have observed it, is interestingly dual in character. An argument presented to me early in the early stages of the present inquiry makes the point. During an interview I asked why the development of
Electronic patient records was presently so important. The rather deterministic response was that since an informatting of the health care system would occur anyway, it would be by far preferable to guide the process. The formulation emphasised that while technological development cannot be prevented it can be sensibly steered. As we shall see, the rationalist idiom of guidance systematically underestimates the capacity of the organizational practice for undermining this goal.

Throughout this analysis has been stressed how work, such as the work of co-ordinating technical, disciplinary, and institutional initiatives and making them proliferate, make the electronic patient record seem as a simple part of technological progress; a self-standing future-generating device. My interviewee, who have been working in medical informatics for years, and have participated in constructing it as a field capable of effectively perceiving and responding to specific sets of problems, analyses this work as passive; a matter of facilitating technological development. The constructive and creative but also political effects of this work disappears behind such formulations that claim to merely guide a self-motored process.

Duality is suggested by the fact that this interpretation can be turned upside down at will, without any perceived paradox. Thus, under the banner of the EPR-observatory it is argued that analysis and intervention is crucial for a successful implementation of these technologies. A dual register between passive facilitation and active construction obtains, then, in which the person sometimes “uses” or “exploits” the technology” while at other times the technology “determines” or “allows” the person to do this or that” (Strathern 1992a: 83). As we shall see, this mutual determination does not occur in practice in a symmetrical manner.

Here I want to argue that there is a logic to the uneasily reciprocal relationship between the technical and the social, organizational, or practical. Some of its important aspects can be captured through the notion of a logic of supplementarity as discussed by Jacques Derrida. This grammar enables me to make sense of a number of practical effects, which arise as a result of the dual register used to evaluate technology, organization and their relation. In particular it will enable me to characterise what could be called the persistent enigma of organization within the discourses and practices of Danish EPR projects. With inevitable simplification, I illustrate four steps of the dynamics of a logic of supplementarity as they link with these.
In the logic of supplem entarity, an issue, a concept, or an idea, emerges, which initially seems self-evident and self-explanatory. The health care sector, for instance, may be improved by a “technological fix”; we have seen how the list of benefits, which the EPR will bring to practice are presented as expansive and unproblematic. In the citation above, the technological fix is even considered necessary to maintain the health care in a “manageable” state. Improvements of the health care sector are thus imagined as following from a technological progression into which one merely needs to tap.

In quick succession it is realised that the prior term (technology or EPR) is connected to a supplement; there is *something* to manage, and this is health care practice, conceptualised as unruly and without sufficient quality and efficiency. The new technologies are introduced precisely to enhance on this organizational “something” in various ways.

Third, this recognition “lays bare an additive substitution” (Derrida 1976: 270): it tends to make “the something”, organization, stand out as a topic in its own right. Once the organizational supplement has been “added”, and brought into the open it gains in visibility and comes to seem increasingly important. Eventually it begins to unsettle the original priority between terms, as technologies are re-interpreted as mere tools to be used in living practice. Organization as “addition” threatens to substitute and replace the focus on “technology”.

But while organization is turned into a central concern it is still perceived as unruly. This raises the problem of how one might ensure the beneficial organizational use of technologies, and this question has no simple or general answers. At this point organizational practice becomes a “dangerous supplement” (308) threatening “catastrophic regression and the annulment of progress” (298), which was originally guaranteed through technological intervention.

Current EPR projects constantly emphasise the importance of organization and its transformation. From *within* the discourse of project managers and health informaticians involved in the Danish projects, it has by now become a mantra that “organization is 80% and technology is 20%”. It is clear that the literalness of this proposition is deceptive, and the idea of measuring the technological against the organizational is nonsensical. Nevertheless it has important practical and rhetorical functions, as it makes it seem increasingly important to intervene in organizational processes, in order to control, guide,
or otherwise “repress” the “dangerous supplement”. But while many models are available for doing so, almost all of them are imagined on the basis of the original dualism between technology and organization. If this model has some plausibility it is thus not surprising that the organizational “parts” of EPR projects remain in many cases oblique and resistant to intervention and “improvement”. In the language of a project manager working with implementation: “We knew the dogma, which says that technology is only 20% of a development project, while organization is 80%, but we did not know it would be this hard”. Organization as a supplement, which it is imagined that one can sensibly steer, which must indeed be sensibly steered, seems to be: “Less than nothing and yet, to judge by its effects, much more than nothing” (314).

In Derrida’s characterisation the supplement “is neither an absence nor a presence” (314). In EPR projects organization is certainly not an absence: it is constantly talked about, worried over, and intervened in. However, at the same time organization, as understood from within STS, seems strangely non-present, and the putative referent remains in several instances spectral. Some examples will clarify the point and I touch upon a number in chronological order.

4. Imagining Changed Work Practices with the EPR

To be sure, reliable information on risks and benefits was limited and incomplete in all cases. Studies of particular clinical innovations have shown that the poorer the data and the less rigorous the evaluation, the more exaggerated the claimed benefits tend to be. Such uncertainties, commonplace in the early stages of medical innovation, provide a built-in bias toward further development (Dutton 1988: 231)

In the early summer of 2001, three working groups, consisting of a number of doctors, nurses, and secretaries at Aarhus University Hospital, were commissioned to develop perspectives on ordinary work-life after the EPR implementation at the hospital. Specifically, the groups were asked to respond to “how they would work on 01/04-04?\(^1\), how it would be possible to plan a good implementation process to make all groups of personnel feel safe about the new system?, how to de-mystify the EPR?, and how to prepare the personnel in IT-terms?” (Secretaries’ report: 3-4, Nurses’ report: 4).

After a number of meetings each of these groups produced a report (each 20-30 pages long), which summed up their considerations and was placed on the EPR-
homepage of the Aarhus Region. What immediately struck all groups as peculiar was their organization into separate groups. Doctors, for instance, stated that “We found that co-operation with other groups on the issue (EPR) was impeded for the obvious reason that we were not in group with them, and had no opportunity for discussing points of contact with the EPR” (3).

Secretaries wrote that they had worked “in a mono-disciplinary group with patient administrative tasks…early in the work processes [could be] concluded that the analysis of work-flow could not be conducted in mono-disciplinary groups” and that “It has been an exciting work process, although we have missed interdisciplinary dialogues and discussions” (1).

The groups thus had a keen sense of one important consequence of the framing of their commissioned task: that its main point, to get an initial grasp on some of the multiple organizational “aspects” of implementing EPRs was already slipping between the fingers of the groups, because those issues had to do quite precisely with the redistributions of tasks and responsibilities that would happen in the interactions between members of those groups with the advent of the EPR.

This had an additional consequence, the negative implications of which the group of secretaries had a particularly firm idea; that is, that in keeping the groups apart, the tendency of each group to textually re-create old patterns of professional relationships would be considerably increased, just because ideas of what the relevant issues were from the alternative points of view of other groups did not need to surface. The secretary group pointed to several areas, such as organizational format, co-operative interfaces, task distribution, conception of other professional groups, culture, norms and attitudes that all would have to be carefully worked through on each hospital ward, otherwise “there will be a risk that the EPR is not used optimally, because problems will remain [in an idiomatic Danish expression “the pixie will move along”] and bad old habits will stay” (22).

In the nurses’ report a similar problem is noted; that “the lack of contact between the groups has necessitated an array of assumptions concerning the routines and work flows of other professional groups” (5). But the separation has already had a narrowing consequence in this report, for in their consideration of the entailment of interdisciplinary work, this work is conceived only as relating to doctors, and secretaries are nowhere
Implications: Chapter eleven

Experimental Devices

mentioned: “The different professional groups will get easier access to the work tools of one another. Doctors will work become more oriented towards [patients’] needs, and nurses will become better at documentation” (19).

Finally, as already mentioned, the lack of interaction between groups was also noticed in the doctors’ report, but here the quite different problem raised focuses on doctors’ need to protect themselves against unwanted new tasks: “Will the implementation of the EPR entail task slippage, so that the group of doctors will be expected to take care of more routine tasks, such as writing in the record, booking of examinations etc ‘since it is so easy’?” (Doctors’ report: 4, original emphasis). With this worry in mind, they therefore propose that: “it is important that specific groups retain the possibility of emphasising/justifying the specific interests and problems in relation to its own tasks” (5).

The division into mono-disciplinary groups had another revealing effect, which can be appreciated by approaching their arguments and suggestions. Given the lack of precise information, the nurses’ group decided to discuss a number of different models that could be used in relation with the implementation. They evaluated advantages and disadvantages relating to the possibility that “EPR work spaces are located in an office adjacent to the ward”, “a portable computer is placed on a moving table, transported to the patient”, “doctors and nurses each have a pocket PC” (7) and so forth. They emphasised the need to solve practical and economical problems such as “too few PCs, PCs that are too old and unreliable, and complicated and slow log-on and off functions” (17). This led them to conclude that: “work is needed with the attitude of the personnel. Frequent problems with and crashes of the current IT-systems effect user attitudes negatively” and “implementing the EPR will not enable economical savings in the short run”. They also suggested that educating users would be very costly but highly necessary. And, undoubtedly inspired leader of the group, who had a background in health informatics, they advocated the necessity of conducting work-flow analyses on several types of wards.

As noted, secretaries were highly sceptical as to the benefits of mono-disciplinary groups. In their recommendations they focused on “implementation – the good process” (24), and proposed the appointment of an interdisciplinary EPR implementation group on each ward. They also suggested that an EPR user organisation based around specifically
educated super-users should be developed. Currently, a version of both of these ideas is, in fact, being put in place.

The doctors’ group, however, took a quite different direction in their report. Initially they indicated “frustration” regarding the lack of exact information about the EPR-project in terms of “budget and schedule” as well as the separation from other groups (3). But this situation was also productive for a certain discourse in that it enabled the group to concentrate fully on the “EPR viewed with visionary doctors’ eyes” (3). Although a number of relevant economical, administrative, co-operative and technical issues were briefly noted, the group agreed that: “these questions should not have the effect of inhibiting enthusiasm” (5). This view had distinctive consequences for the writing style of the report. For example, the appendix called “an anaesthesiologists’ expectations of the EPR” is littered with sentences like “everything concerning security and backup is, of course, solved, such that the system is up 99,9% of the time, and the remaining 0,1% is taken care of”, “the table top of the moving table for the ward round, by the way, is a computer with finger touch screen”, capable of showing x-rays and “when you have dictated a note to the EPR (secretary or ‘voice recognition’) it immediately appears as a draft on the screen” (9), and so on.

The visionary attitude gives rise to a number of issues, of which the easy conflation of the EPR and a secretary in the preceding sentence is only one instance. In general the move is such that this mode of writing renders irrelevant the consideration of an array of topics relating, for instance, to the interests of other disciplinary groups, to the everyday work-practices of doctors and others groups of personnel, and to the relation between technological visions and economical reality.

The visionary modus thus turns the report decisively away from any realistic estimation of the implications of the EPR as a work tool in an organizational practice, and rather than “demystifying” it renders its potentials and problems more opaque. Some of these issues will re-appear in a highly visible shape, as I turn to a description of the first pilot-test of the medicine module a little over a year later.

5. Pilot Testing the Medicine Module, Fall 2002
It is interesting to note how the common observation of the many difficulties of other innovative projects continue to easily co-exist with the feeling, or at least the steadfast
argument, that precisely this project, here, will be different and deliver the envisioned goods. This understanding was literally brought to the test as the Aarhus system began to materialise and could be finally used experimentally on pilot wards.

Here I relate the experiences from the test of the medicine module (as discussed in chapter six, this was one of several independently developed modules comprising the Aarhus EPR system) in the autumn of 2002. I did not participate myself in these events, but rely on a detailed report generated by anthropologist Claus Bossen and a participant from the medicine module.

In chapter four a similar “collection of experience”, the HVEPS-report, was discussed. I focused on its highly equivocal information strategy, which on the hand enabled it to document many of the practical problems encountered and the severe limitations of the project as compared to its initial envisioning, but which on the other hand was capable of rendering these experiences anecdotal in comparison with the potential futuristic success that it foresaw.

Brit Ross Winthereik and I characterised this aspect of its information strategy as “moralising” and juxtaposed it with another strategy, more attentive to the micropolitics of practice. We suggested that such attentiveness could enable a more sophisticated analysis of the processes of organizational transformation, which go along with technical projects, because it would work against textual or practical “disappearing” of contentious issues and for the “appearing” of issues, which tend toward invisibility.

The logic of supplementarity here identified can be seen as an effective system for not confronting political moments in technological projects. I should probably repeat that such a “logic” exists not because of the bad intentions or ignorance of any specific actor, but can be seen as a part of larger sets of interconnected disciplinary, institutional, and organizational assumptions, which are constructed and re-constructed as they travel through various practices relating to technical projects.

The Report Concerning the Pilot Test of the Medicine Module in the Electronic Patient Record of the Aarhus Region, Autumn 2002 (MEM) offers a detailed qualitative investigation of this phase of testing. However, this adds to the paradoxical quality of the report, because what it documents so thoroughly is the many problems the pilot tests ran into. Let me briefly bring up some examples from the text, which will allow me to make
a number of points concerning organizational supplem entarity and the enigma of organization.

In the autumn of 2002 the medicine module was tested at four wards in order to review its usefulness for clinical work and gain knowledge about such aspects as its stability, functionality and co-ordination with work flow in the hospital (2). Pilot wards would be encouraged to report errors, suggest improvements, and come up with ideas for how the system could be “rolled out” at other wards. In the curious “neutrally positive” language of the introduction, the “pilot test offered insights into the functionality and structure of the medicine module, which was, generally speaking, satisfying, and it led to wishes for new functionalities and performance-improvements, discovery of some errors, and considerations concerning the change of work flow in hospitals” (2).

The main text considerably qualifies this evaluation. It consists of twenty pages packed with problems concerning organization, technology, education, practicalities, errors and wishes for improvements, and work-flow. I remark on only a few, selected because their potentials had already been predicted in the reports discussed above. The first, although, grouped as “technical”, in fact immediately points to the impossibility of maintaining this category as distinct from social and organizational issues:

On November 4th, attempts are made to begin tests again. A check on the set-up of users shows that while relevant nurses and doctors have been created [in the system], this does not hold for chief physicians and secretaries. The IT-department had not understood the request to have all physicians on haematology created as users, as including chief physicians. In relation to secretaries is made visible the problem that the “roles” (delimiting rights in MEM and EPR) are only set-up for “doctors”, “nurses”, “administrators”, and “porters” (!), but not secretaries. The IT-department suggested the registration of these as “nurses”, which would entail their access to medicine administration in MEM. Secretaries end up without access to MEM (14)

One can sense why a high emphasis on interdisciplinarity, “cultural diversity”, and cooperation was found in the secretaries’ report. It is indeed both sad and funny how secretaries, are forgotten as users of this new information system. It is noteworthy that the invisibility of their work is clearly related to its delegation as “practical” and “organizational” rather than “technical”, as in medicine prescription. In any case their disappearance in the work-flow of prescribing medicine completely erases the everydayness of the test, and thus exemplifies how secretaries embody organizational supplementarity; they are vital but unseen.
My second example starts out with a problem with log-on time, which, incidentally, the nurse’s report foresaw as a potential source of frustration. This fairly narrow issue unfolds, however, into a series of connected problems in the following excerpt from the diary from the haematological ward, November 8, four days into the test phase:

MEM/EPR runs slower the more patients are entered: creation of one patient took, in one instance 2 minutes and 40 seconds on a PC with 260 Mb Ram, after log-in…Prescription of 5 sorts of medicine took 4 1/2 minute…Medicine administration for one patient on a portable computer with 256 Mb Ram took 8 1/2 minute altogether …There is only one common password for all users, and it cannot be changed. Pilot tests are run only in daytime, when the test-responsible or super-user is present, i.e. MEM is not a part of clinical everyday practice. Education cannot practically be carried out, since MEM on the DVH-Installation is unstable and is an older version (35)

These and many other examples aroused understandable indignation. A doctor expressed the following sentiment:

We clinicians do not usually use anything until we are certain it works. This is necessary because of the patients. We usually do not start the airplane before everything is ready, tested, and running. We had been told that it would be somewhat different in connection with IT and the EPR. We had been warned that we would probably have to put together the airplane while in the air. But during this pilot test we haven’t even received the parts (6)

Given the criticisms offered in the report, it is not surprising that a number of problems are summed up in conclusion. It refers to a “lack of a clear distribution of responsibilities and tasks”, a “lack of procedures for carrying out the task”, a “recognition of unforeseen problems or that it takes longer than expected”, and “trouble involved in interdisciplinary communication between groups with different specialised languages and norms” (21-2). At the same time the conclusion operates a rather more equivocal distinction, according to which “from the clinicians’ point of view the pilot tests of the autumn 2002, offers good insights into parts of the functionality of the MEM, with which there is general satisfaction” (21). In spite of everything else, then, the technological component seems capable of an evaluation, separated from and positive in comparison with organizational aspects, in a gesture thus proposing that “everything else” indeed impeded proper success, but also implying that in a future to come this may change.

The report finally recommends that “experiences are transmitted”, for instance by letting the “participants tell the story, through reports, by creating forums and networks,
whereby these experiences can be told, and by ensuring continued clinical participation in the planning of pilot tests and/or implementation of MEM…this could be decisively strengthened by giving these forums and networks official status in the EPR organization and earmark resources” (22).

The context of this latter recommendation merits a comment. On January 1st, 2002 the original modular organization of the project was abandoned, and responsibility for the project was officially moved to the IT-departments of each hospital. They would participate in a much more exclusive “common group” along with regional and hospital administration, and a few software developers and clinical representatives. The result of the re-organization was that clinical “moving spirits” were no longer compensated for their work, which nevertheless remained indispensable for the continued development of the project. The MEM-report points out that one key to a good implementation process might lie in the developing a capacity for learning about effective configurations of “system” and “organization”; an endeavour that would need the involvement of those who have working knowledge concerning both.

Although the MEM-report displays a certain “glossing” tendency in its rather friendly evaluation of efficacy of the tested system as abstracted from its socio-technical environment (where it failed utterly to deliver), it mainly shows a keen awareness of the importance of socio-technical relations. Such understanding, however, did not reach far into the development organization. After publication the MEM-report was taken up at a meeting of the managing project committee, where it was “made note of and filed”. I do not think this indicates a mere lack of willingness to take seriously the analysis made in the MEM-report on behalf of this committee, although it also signals this. The report may have been so quickly disposed of also because few conceptual or material means were available within the project structure for improving on the problems thus made visible.

I want to suggest that this lack of certain kinds of resources is related to the way in which “organization” functioned from the beginning as a supplement to the “technical” part of the project; organization was imagined as the neutral “medium” into which the new system would be inserted, or on top of which it would be “rolled out”. The realisation that the “medium” was not, in fact, neutral, made it seem highly complex, but offered no new resources for how to deal with its opacity.
Making explicit the assumptions of the “techno-logic” of supplementarity, which guided the project, pointing to some of its consequences and indicating how it could be reconfigured might therefore be a valuable contribution to the socio-technical considerations of this or other projects. An example discussing conceptualisations and initiatives taking place in connection with the second round of pilot-testing develops this point in more detail.


For nothing is ever repeated exactly, and the most analogous lives, which, thanks to the kinship of persons and similarity of circumstances, we may select in order to represent them as symmetrical, remain in many respects opposite (Proust 1934: Vol. 2, 733)

In the citation Marcel Proust is talking about interpersonal human affairs, but in the non-humanist idiom of the present text, I am tempted to talk of development projects, too, as having lives, which can be imagined as closely analogous to each other, yet always surprise by their differences.

The following section on the implementation strategy for the second round of pilot-projects, will exemplify two ways in which this is so: nothing is ever repeated exactly as planned, and nothing is ever repeated from one setting to another. And it will suggest that this evident and everyday fact remains very hard to see because of the supplementary logic that guides project understandings of the relationship between “technology” and “organization”. Specifying the implications of these Proustian differentiations is a first move towards a reconfiguration of the terms in the logic of supplementarity.

From the beginning of the project, technological aspects had been economically separated from organizational one’s, such that the former would be paid for by the Aarhus region, whereas the latter would be an issue for the individual hospital. This is not an unusual model, for Danish hospitals are independent institutions, whose organizational activities cannot easily be dictated by the layers of political organization. The situation moulded the structure and emphasis of the project, as it was easy for hospitals to participate in technological development at regional expense. Imagining, implementing and paying for the organizational transformation necessitated by the
adoption of these new technologies, however, would be the problem of individual hospitals.

The regional office for informatics funded what was called the “IT-villa” or “the virtual hospital” (DVH), in an old house by Aarhus Regional Hospital. This building had several offices, which housed a number of regional employees. These functioned as secretaries for the various modules and committees and had a central co-ordinating function in planning and keeping track of the proliferating project development. The virtual hospital also housed a basement of computer servers, where modules and components are tested before being approved for pilot-testing at the wards.

The regional EPR implementation group was based in this environment. This group was in a curious position, for as it explains:

Hospitals are responsible for the implementation of the EPR, concerning economy as well as planning and practical accomplishment. The task of the regional implementation group is to develop “tools”, which are made available for all hospitals during implementation (http://www.aaa.dk/aaa/index/om- amtet/it_i_amtet/epj/epj-implementering.htm)

These tools, as available per January 2004, included a questionnaire to uncover IT-competencies of personnel, a manual for the booking module, an education plan for super-users, a “guiding norm for minimal demands for a PC, in order to run the EPR”, and a number of outlines, forms and suggestions for carrying out work-flow analyses. It was up to individual hospitals to decide what, if anything, they want to do with this toolbox. As it happened, a number of hospitals have made implementation plans, which were also available through the homepage. Moving closer to the implementation processes as it takes place at Skejby Hospital, I specify both of the above-mentioned Proustian differentiations: from planning to reality and from site to site.

The implementation strategy of Skejby has four ‘legs’: information, education, organizational change, and technology. The conceptualisation is recognisable: it is an updated version of “Müller’s square”, as discussed in chapter seven. This is not a coincidence; the implementation manager at Skejby has a degree from the health informatics education at Aalborg University. The idea behind this square, as may be recalled, is that technological development and implementation is complicated but can be guided by simultaneously taking into account each of its four dimensions.
A temporal chart presents how this is to happen up to and after implementation (“M-day”) at Skejby. On the information axis are “newsletters”, followed by a “general orientation”, yet later a “ward orientation” and a few weeks after the intro an “evaluation”. Education includes “planning courses” (e.g. EPR-Organisation: length 1/2 day, work-flow analysis and organizational change: one day, IT-technology, problem identification and problem solving: one day), “basic education in a play-environment”, then a “course for management”, “reminders for all users”, “24-hour hotline” on M-day and subsequent “evaluation”. Organizational change revolves around “work-flow analyses”, “discussion” of these, followed by “prediction of changes” that will be induced or facilitated by the EPR, such that “improved work flows” can be worked into the ward, preferably prior to implementation, and an “evaluation” fourteen days later. Technology must be “bought”, “prepared” (e.g. the play-environment) and “tested”. On M-day there must be IT-assistance all day; here, too, an evaluation follows.

Read “against the text” there is an ethereal quality to this description; everything seems to go quickly and smoothly: from the one-day course in technical problem solving (even though the project has taken years and everyone knows it is immensely complex) to the half-day course in organizational change. In the chart, organizational transformation takes little time and work; the course in work flow analysis and the actual work flow analysis are both placed three months prior to M-day, one month later this analysis must be discussed among super-users and management, then two weeks later it must be clarified how this relates to the EPR, and in the last two weeks before implementation, according to the outline, work flows are simply changed at the wards.

Technicalities are described in the same planning idiom of effortlessness; thus buying technologies, testing them and preparing them, as well as creating users can all be done within a few months. The implementation report from the psychiatric hospital frankly acknowledges that: “the estimated hours of use of resources are based on non-empirical data” (Psychiatric Implementation Report: 10) and one comes away with the distinct impression that this is also the case here.

Reading “with the text” one may note that in practice things are and will be quite different, as the implementation group at Skejby is also well aware. Delays in relation to the finalisation of certain modules, as well as the less than perfect first pilot test described above have changed plans, so that a secondary pilot phase was to be run prior to the
general implementation process. At Skejby this pilot experiment revolved again around the medicine module. Additional components would be added only slowly as the grand common implementation envisioned in the implementation plan will be replaced with a gradual and incremental upgrading of pilot tests to more full-scale uses.³

This is not in itself too shocking, as pointed out in the truism that “it seems almost automatic that software is never produced on time, never meets specification, and always exceeds its estimated cost.” (cited in MacKenzie 2000: 166). But zooming in on the enigma of organization and its supplementary structure, allows one to grasp a seemingly paradoxical complement to this insight; that no matter how many times this “automatism” is encountered, subsequent technological development practices seem not to learn to make more realistic estimates of the requirements of money, time, energy or of expected gains.

Given the above-mentioned situation it has been necessary as one project worker said: “to redefine implementation a lot”. On the one hand it was necessary to begin the process, because of the high level of political pressure, and because the EPR has been held in prospect as nearly ready to hospital employees for so long that a number have started to express doubts if it will ever arrive. The official start of the implementation aims to quell such worries through “control of expectation”. In fact, however, the actual testing of the model would not take place immediately; for the module was not yet ready when implementation was to start on October 6th, 2003. Implementation was therefore translated to mean “work-flow analysis” for the foreseeable future. Before I return to work flow analyses, which now figure as a crucial component in pilot-tests and implementation, I briefly note one more re-definition.

According to the guideline from the regional implementation group, hospitals ought to offer seven hours of teaching per employee; this was estimated as enabling proper use of the medicine module (more hours would be added for each other module). In Skejby three hours per person is viewed as necessary and this amount is made “flexible depending on the target group”. Doctors, for example, will not be forced to participate in training sections, because these do not match their individualist “culture of learning”. To facilitate this culture a portfolio with educational material would be made available by means of which they can practice individually. But a rationale for cutting training hours, which is perhaps at least as pressing as doctors’ individualism is funding; it would be
“mega-expensive” and extremely complicated to meet the regionally developed recommendations. An analysis made by project workers estimated that with the current educational capacity it would take more than three years to train all hospital personnel in the region. Even this task would constitute only half of the problem; the other half consisting in far too few health practitioners being available to replace those being trained.

This is but one instance in which complicated but important issues having to do with organizational transformation is quickly bypassed, because material and conceptual resources for engaging these issues are scarce commodities under a techno-logic of organizational supplementarity. This scarcity in combination with the emphasis on guiding work practices has led to a high focus among implementers on work-flow analyses. These are imagined as having the double advantage of being both cheap means of grasping central aspects of hospital work, and effective means of changing them.

The idea is simple. Each work-flow is described on a separate one page sheet, first in its “present” form, then as “envisioned”. Each “flow” is given a name and a person responsible for it; it has a “beginning event”, a set of “actors”, “tools”, “interested parties”, and “pre-requisites”, all of which have space for a few lines of description, along with the central “actions (who, what, where)” that takes up roughly half a page. Finally the “result” is summed up at the bottom of the page. While the “present” page offers a description of current activities, the “envisioned” page imagines how nicely things could look in the future: as always “it is necessary to separate analysis of present and envisioned work flow, to not let present relations inhibit the possibilities of future work flows”.

There are well-known anthropological arguments pointing both to the gap between clinical self-description and reality (e.g. Forsythe 2001) and to the gap between knowing how one would like something to be, and actually making it so. As for this second gap its many manifestations in relation to technological projects has taken up a substantial amount of space in the present text. But in any case, as already mentioned, the promoters of this thin model of work practice and organizational change are also well acquainted with thick practice.

For example, I have learned from these people how implementation must proceed differently not just from hospital to hospital, but also between wards, since “one does not
give medicine in the same way at surgery”, and since wards are recognised as highly variable work spaces. Thus “X is a giant ward, the size of a small regional hospital”, which slavishly follows the method of work-flow analysis advocated by the implementation group. On the other hand the Y ward is much smaller and has personnel that has been involved with implementing IT-systems at earlier times; its tendency is rather to “make decisions over lunch”.

According to one interviewee, successful implementation therefore involves the capacity to interact productively with different cultural and disciplinary ways of handling the task at hand. For the implementation group at Skejby this is a part of the “huge experiment” of implementing an EPR, with its requirements of organizational transformation, of adding “a layer of loosely coupled systems on top of a traditional hierarchical organizational structure”. Yet paradoxically this experiment is imagined in such a way that transformation can be understood as flowing smoothly from clinical self-description.

7. From a Logic of Supplementarity Towards Proustian Differentiation
In the juxtaposition between local analyses of the practical realities of pilot projects and the general implementation schedule, we can ascertain a replication of a pattern earlier encountered. On the one hand the schedule presents EPR implementation as a perhaps complicated, but certainly controlled procedure, which can be planned in the abstract and merely needs to be followed. On the other hand we see a “huge experiment”, where one “lives in chaos”, and where “loosely coupled systems” meet “traditional hierarchies”.

This is the gap in which the supplementary structure for thinking about the relationship between technical development and organizational work practice becomes visible. With a focus on this gap it becomes striking how an organization can be viewed as a neutral medium in the abstract, but in practice seems a highly opaque and somewhat “dangerous” thicket, which it is nevertheless necessary to try to work one’s way through.

As already said it makes no sense to blame either project management or implementation group for this situation, for as we know the image of accomplishing a swift organizational re-engineering, which would smoothly alleviate an array of problems is widespread and engrained in a modern technological imaginary (e.g. Hammer 1990). But while no accusations are warranted, commentary is.
The image of quick and efficient change is one which always survives better on the level of political visions and strategies, where the enigma of organization does not have to be scrutinised too closely, than on the level of local practice where it must be. This gives the current chapter its critical edge even as it claims to read “with” the text. For moving with these texts into the practices that they are supposed to connect with, one sees how the flexibility and vagueness that, during envisioning processes are so productive turn from assets into liabilities. It is the tension encountered as political visions hit the shop-floor.

The distribution, which ensures that (moralising) visions are formulated among politicians, whereas their materialisation take place in rather different locations, ensure that very few resources are ever allocated to those issues not immediately captured on the level of visions. Therefore very little intellectual effort goes into analysing “political moments”, in comparison, for example, with the countless hours used by programmers on technical detail. Nevertheless the gap between political vision and concrete action is sooner or later encountered and, from then on, the costs are predictable: an increase, in comparison with promises and expectations, in terms of time, money, and organizational and professional energy from both project workers and from health personnel that have to suffer frustrations and disappointments. Talk of another “IT-scandal” may then start to surface.

But if such situations sometimes tend to take on a scandalous character, it is rarely because people have been doing bad jobs. It is rather because an enigma of organization is created, which renders invisible a great deal of what it actually takes to make anything work in practice, and offers simplistic conceptual tools for dealing with those aspects that come into view. The problem, in short, is that the horizon of available expectations is rendered completely unrealistic by its reliance on the supplementary logic.

This logic is so prevalent, that even as “organization” surfaces and starts to interfere with technical matters the alleviation of this complication is constructed within an idiom of sensible control and execution, such as the 80-20 model and the solution of organizational transformation in terms of work-flow analyses. According to the analysis here carried out this logic increasingly brings “organization” to light and attempts to intervene in “it” through managerial initiatives ala Foucault, but the organization it looks
for is spectral, and the one it finds is therefore rendered only obliquely visible; consequently it remains obstinately “resistant” to the “new”.

For this reason, the present view remains distinctly unimpressed with the pathos surrounding this newness. Scepticism towards the new does not automatically equal a conservative preference for the old. As Foucault phrased it, the “point is not that everything is bad, but that everything is dangerous” (Rabinow 1984:343). For Foucault this position leads to neither conservatism nor apathy but to a “hyper and pessimistic activism”. Turning the negative formulation of Foucault upside down, Barbara Herrnstein Smith suggests that analyses should lead to “the continuous development and refinement of more richly articulated, broadly responsive, and subtly differentiated...accounts” (Smith 1988: 172).

By pointing in various instances to the “dangers” of the present, Foucault offered no easy solutions, nor did his analyses make him capable of doing so. Rather he viewed the headaches he gave those interested in transformation as part of that process itself. In his spirit I offer the following headaches.

One must recognise that in thinking about technological projects and technical implementation, organization and work practices are important phenomena in their own right. Organization is therefore not easy to steer, control, or manipulate, but neither is it a dangerous supplement. It is, however, what must, first of all, be made to “function” in a manner flexible enough to meet the requirements of numerous groups of people in health care, regardless of the complexity of this task. When this is taken into account one may become less shy about the fact that organizational transformation is inevitably a difficult and resource-heavy affair, which rarely allows for elegant and quick solutions. To the extent that this could be realised, perhaps some of the problems experienced in projects such as the one currently being described, could be remedied (one imagines, in the least, alleviation of such problems as forgetting whole groups of personnel).

But it would have also be realised that this improvement can only ever be partial, for since conceptualising “organizational issues” is also a way of intervening in the organization under discussion, such conceptualisations are as likely as other kinds of initiatives to be “resisted” and transformed as they encounter practice. This is a predicament, which makes it chimerical to imagine any given alternative as the proper solution to perceived “scandalous” problems.
Because of this predicament, it seems to me that accepting the challenge of working out Smith’s list of refinement, articulation, responsibility and subtle differentiation is the best one can do in empirical non-modernist STS-studies. Ideally, it enables sophisticated analyses of the multiple Proustian temporal and spatial differentiations between vision and reality and from site to site, which one continually encounters in practice. In the final chapter I elaborate in more detail some of the entailments of this challenge for conceiving of relationships between the empirical, theoretical and normative.

Acknowledgment: Thanks to Claus Bossen for helpful conversation.

1 As far as can be foretold, doctors, nurses, and secretaries will work on 01/04-04 exactly as they have always done. Exaggerated projections for change is part of the logic of supplementarity, whose premises I attempt to make explicit in the following.

2 Compare with the formulations in chapter nine.

3 I have encountered less positive scenarios estimating that several modules will never be integrated into the solution, and that the final product may end up having only a very vague resemblance with the seamless information tool envisioned in reports from 1995 and until now.

4 There are many variations over the theme here described. The present is from the software developer Systematic. It is available through the homepage of the EPR project in the Aarhus Region at http://www.aaa.dk/aaa/index/om-amtet/administration/sundhedsstaben/sus-soep_sygehuse/epj/epj-implementering/epj-impl-vaerktoejer.htm.
Chapter twelve:
Established Sentiments, Alternative Agendas
and Politics of Concretisation

I want to locate this concluding essay on the efficacies, political or practical or, again, intended, imagined, or real, of scholarship in science and technology studies, which I shall call their politics of concretisation, in the space between two arguments by the extraordinary thinkers Barbara Herrnstein Smith and Isabelle Stengers.

The first is from Smith’s recent article “Cutting-Edge Equivocation: Conceptual Moves and Rhetorical Strategies in Contemporary Anti-Epistemology” (Smith 2002). In this piece Smith survey a number of putatively radical contemporary arguments on the current academic scene, and suggests that

We can derive some sense of the way intellectual life is experienced in an era from the recurrence of certain metaphors to describe its conduct – for example, the frequency with which, in our own time, intellectual projects and achievements are described in terms of navigational finesse: the charting of passages between extremes, the steering of middle courses, the avoidance of the twin perils of Scylla and Charybdis (Smith 2002: 187)

As Smith shows the capacity for moderation is often praised by commentators precisely because of its “extremity avoidance”. But she is far from convinced by the strategy. For as she argues, such “navigational feats risk becoming not so much a steering-between as a steering-in-two-directions-at-the-same-time” (187); it is such self-induced ambiguity with regards to where one is steering that she terms “cutting-edge equivocation”, and she defines her stance as a defence of extremity:

Conversely, what gives many of the “extreme” proposals their conceptual power is, among other things, precisely their extremity – that is, the unhedged explicitness of their questioning or rejection of various traditional ideas, and the consistency of the alternative ideas they develop. Contrary to what the term extreme may suggest, these intellectual virtues are the product not of uncontrolled excess or exhibitionist derring-do but, rather, of an effort at clear and precise formulation and a rigorous working-through of theoretical and practical implications – at least where such characteristics are in fact displayed. The intellectual virtues of some challenges to orthodoxy, “extreme” or otherwise, may, of course, be quite meager (Smith 2002: 191-2)

The problem Isabelle Stengers defines for herself in The Invention of Modern Science is quite different. Here she retrieves what she calls the “Leibnizian constraint”, “according to which philosophy should not have as its ideal the ‘reversal of established sentiments’”.
Implications: Chapter twelve

Experimental Devices

(Stengers 2000: 15). As Stengers remarks “few philosophical statements have been as badly viewed as this one” (15); Gilles Deleuze, for instance characterised it as Leibniz’ “shameful declaration”. But yet, she continues, “it is easy to ‘speak the truth’ against established sentiments, and then to be proud of the effects of hatred, ressentiment, and panicked rigidity one has aroused as so many proofs that one has ‘reached the beast’ – even at the price of persecution, since the martyr and the truth are good bedfellows” (15).

How to read these two formulations together? Perhaps, they could be easily taken to suggest opposite strategies for dealing with issues in science and technology; where Smith would represent the raving iconoclast, Stengers would be the figure of moderation.1 Yet this interpretation would be incorrect, for no one is less (classically) critical than Barbara Herrnstein Smith, and few contemporary intellectuals are, in certain respects, quite as radical as Isabelle Stengers. It is therefore a worthwhile task to explore the topos opened up by their diverging arguments, with respect to the theoretical, practical, and therefore political and normative capacities it may engender.

Before I return to this task, however, I want to take a different path, in order to discuss certain moves and gestures of currency in recent STS studies of normative inclination. I broach this topic by analysing some of Marc Berg’s forays into that area. Berg is my exemplar, first because he has been a pioneer in making sophisticated STS-accounts of IT in health care, and second because he has increasingly urged that these studies are or should be of special normative merit.

To analyse these interventions I invoke the term politics of concretisation. I use it to characterise the relations a text (or other pronouncement) tries to establish between itself, its intellectual environment, and its (hoped-for) readers. Politics of concretisation are thus manifested in any text, this one included, and are often particularly visible in abstracts and conclusions. In particular I intend the term to capture the rhetorical tendencies exhibited as one pronounces on the strengths of one’s research, and emphasises the goods that it provides. Often this is done in explicit or implicit juxtaposition with other studies, which are presented as failing to meet a set of specified values and which one, concurrently, can claim to supersede. Politics of concretisation thus involves at least two aspects. First, they illuminate how an author works to control the relation between his text and its reader by providing guidelines for how it should be
read. And second they show how an author attempts to demonstrate some form of superiority or other through his capacity to deliver according to these guidelines in a better manner than comparable texts.

Politics of concretisation are manifested in very variable ways, but in my estimation they take two main forms in connection with present STS-studies, both of which lean on a sense of the merit of being “normative”. The first of these forms is most readily available from scholars of overt political orientation in STS and related fields, and it has been used to argue that while non-humanist STS-studies have perhaps provided some valuable insights, they are nevertheless limited because they fail to engage critically with the powers that be. This is the charge of “complicity”. The second orientation is often found in the work from researchers located in practical, or “applied” environs, such as policy institutions or management departments. Here, too, it is repeatedly argued that non-humanist STS fails to properly cash out its value; but in this version of normative argument this is because such studies are viewed as insufficiently capable of providing practical guidelines for how to improve some set of affairs, such as, in Marc Berg’s case, the use of IT in health care. To remedy these variably conceived deficiencies such arguments regularly claim to go “beyond” Latour, ANT, or similar designations (e.g. Berg 1996, Monteiro and Hanseth 1995).

The remainder of the text has the following steps. First, I identify two sorts of STS normativity: “political” and “practical”. I exemplify a version of the former in the work of Donna Haraway, and discuss its principled refusal to be too quickly or literally practical. Marc Berg wants to use STS with this aim and in section five I analyse some of the complications this leads into in an argument attempting to convey the benefits of sociotechnical studies to an audience of medical informaticians.

There is increasing agreement that STS should not be perceived as representing other fields from above, but should be seen rather as located “next to” them. In section six I argue that this does not entail that STS have a specific obligation to become prescriptive. On the contrary, the emerging agreement that intellectual labour is not radically detached from other sorts of work makes it increasingly important for such endeavours to maintain a non-prescriptive breathing space. In this argument, the value of non-humanist STS-studies can be located, to a large measure, in its singular stance of symmetry. Far from sidestepping or evading normative issues, this allows non-humanist
STS studies to offer effective suggestions for how to redefine what counts as political, practical, and relevant. In section seven, I attempt to specify some of these issues by entering the topos opened up by the arguments by Smith and Stengers. In particular, I try to convey an idea of the sorts of politics of STS that are enabled if one foregoes classical criticism and stays within the parameters of a non-humanist intellectual landscape.

2. Normative Questions in STS

One can observe a heightening interest, in STS and related fields, as to the question of how to be *practical* in doing *intellectual* work. It is itself remarkable and worth inquiry that this question is increasingly raised in such terms, since they rely on a number of outdated distinctions between, for example, mind and body, the ideal and the material, and so forth, which have been extensively problematised from within these fields. Various explanations for the (recovered) felt need to connect what is viewed as the abstract labour of thought with the concrete work of some real life practice or other; that is, to “produce” something practical, could undoubtedly be constructed, and would certainly be relevant. But rather than looking for causes, I want emphasise a symptom, which can be described by inverting one of Isabelle Stengers formulations.

Stengers has suggested that “nothing is easier for a modern person than to be tolerant”, a formulation I discuss in detail in section seven. First, however, I will entertain the idea that a corollary to this suggestion is that few things seems more important to a number of politically motivated intellectuals, than to remedy this perceived deficiency by becoming (again, perhaps, or for the first time) duly normative.

Normativity, unsurprisingly, takes multiple shapes, but I want to here mention two broad kinds. The first can be referred to as *political* or *critical*, as it is regularly inspired by left-wing thought in general, and Marxism in particular, and is embraced by self-described radical scholars. The second could be called *practical* or *instrumental*, and is associated with the question of how to accomplish some tasks or other better, or more efficiently, according to some agenda; be it bureaucratic, corporate, or emancipating.

In a discussion piece considering the work of Bruno Latour, Marc Berg has posed a number of questions, which can be taken as introductory for a discussion of normativity in and of STS. Berg frames Latour as a residual modernist, because “although Latour would be the last to deny that every representation is also an intervention, his work does
breathe the ghost of that denial” (Berg 1996: 256). He suggests, referencing Donna Haraway, that “the inseparable connection between depiction and intervention should be embraced rather than fled from” (256) and urges Latour “to spell out the political implications of his own creations” (256). How, wonders Berg, “can theories like these be put to work? And anticipating a topic that will be taken up later, he asks, “In the case of (information) technologies for work practices, for example, is there any way in which such insights can be drawn upon to design better technologies, for example, or to implement them more successfully?” (256).

In the argument Berg refers to a version of normativity formulated, as he sees it, in Donna Haraway’s work on cyborg feminism, and he wants to bring her political sensitivity to bear on themes relating to technology use in health care. Specifically, he wants to associate her question of the normative with his interest in making better IT systems and ensuring better implementation procedures. Thus Haraway’s political ideas are transported and translated into a realm of practical application, which, as I will indicate in the next section, is somewhat foreign to her own project. This is not problematic as such – people borrow concepts and ideas from each other all the time, and bring them to bear on new settings – but it opens the question of whether Berg can sensibly use Haraway in his argument against Latour’s putative residual modernism. This use is troublesome because Haraway’s own formulations far from offering practical suggestions for their application explicitly argue against such use.

3. Normativity as Determined Ambivalence

Genealogy aims to unfix the terms of the contemporary political situation, and it does so from a particular normative set of investments; but it doesn’t tell us what is to be done, or even what is to be valued. It does not replace the truths and convictions it renders historically contingent and discursively containing; rather, it questions whether truths and convictions make up the right ethos for critical political consciousness. Should these challenges constitute a source of political anxiety for left intellectuals? I think otherwise (Brown 2001: 120)

To illustrate some of these differences, I start by interpreting a few aspects of Haraway’s “Mice into Wormholes: A Comment on the Nature of No Nature”, a well-known article which introduces the OncoMouse™, a rodent that has by now become famous in cultural studies and STS-circles (Haraway 1998: 209-45).
While this text points continually to political issues it does so in a determinedly ambivalent manner, which alternates between rather simple principled stances towards certain issues and the subsequent complication of those same issues. This can be exemplified in connection with the discussion of the famous OncoMouse™, which qua patented animal Haraway in stated principle opposes, but in textual actuality only sort of opposes.

Thus, on the one hand the ethical stance is clear as daylight: "I oppose patenting of animals, human genes, and much genetic material" (218). Yet this message co-exists with a pragmatic sensibility, as when is pointed out that: “Whether or not I agree to her existence and use, s/he suffers, physically, repeatedly, and profoundly, that I and my sisters may live.” (224). Clear-cut stances are out of the question, then, for both mice and women suffer, the former sacrificed to save the latter. In evaluating the tension between these ethical and pragmatic issues, Haraway considers the fact that mice have “rodent feelings” and “mousy cognition”:

I do not think that fact makes using the mice as research organisms morally impossible, but I believe we must take responsibility for using living being in these ways and not talk, write, and act as if OncoMouse™, or other kinds of laboratory animals were simply test systems, tools, means to brainier mammals’ ends, and commodities (225-6)

The array of statements of increasing complexity here delineated certainly elicits the political interests and investments of the author. Haraway is decidedly unworried about offering “normative” statements regarding the state of affairs she discusses. However, her clear or simple political propositions never stand alone; either they run in parallel with a set of qualifications, or they remain in an oblique relationship with other textual elements. The result is that her stance towards practical, pragmatic, or, indeed, practice-guiding issues remain thoroughly obscure. Read in the hope of finding such ammunition, one comes away disappointed: Haraway seems to both oppose the OncoMouse™ and not necessarily oppose it; to agree that it is morally reprehensibly to experiment with these mice, while simultaneously recognising that since it is done anyway, with (some) benefits for (some) women, then we should take responsibility for them. How might we take such responsibility? At the end of the essay, we do not know, certainly not in anything resembling a “practical” sense.
Lest it be misunderstood, the purpose of drawing attention to this tension is not to criticise Haraway for not being normative enough after all. It is rather to point to her quite different conception of what normativity entails: the definition of new and relevant political issues that readers will be “forced, kicking and screaming to notice” (Haraway 1990: 199). In her work the facilitation (or enforcement) of such noticing is the normative point because: “The power to define what is technical and what is political is at the heart of technoscience” (1997: 231).

Haraway wants at all cost to avoid the fallacy of misplaced concretism, as can be witnessed not least in her highly metaphorical and convoluted writing style. But the point is also made explicit, when she describes *Modest Witness* as: “In its most basic sense, … my exercise regime and self-help manual for how not to be literal-minded” (Haraway 1997: 15). This characterisation poses a challenge to those who would like to adopt it as a model for practical normativity, because it emphasises that intellectual work, no matter how substantive and elegant does not, and cannot, translate directly or easily into this kind of efficacy.

4. Concretising the Normative: From “Political” to “Practical”

In his critique of Latour’s “lingering modernism”, Marc Berg has taken on himself the task of rendering STS-insights relevant in the setting of Dutch health care. Specifically he wants to participate in improving its information infrastructure, while also theorising the possibility of this efficacy.

As will be recalled he argued that the “inseparable connection between depiction and intervention should be embraced rather than fled from”. But what he has committed to doing is, in some sense, considerably more difficult to accomplish than Haraway’s task. As we saw she viewed her job as one of opening up avenues of investigation, while also indicating the political issues she believes these would need to take into account. Berg’s task, however, is to use STS theory to make better IT systems and ensure better implementation processes.

An objection to this idea could come from the ANT-vocabulary itself, in its suggestion that the attempt to *apply* anything to anything else, will always result in an unforeseen outcome. In other words, even if ANT is applied with the purpose of securing a specific organizational effect, the outcome is nevertheless bound to offer surprises since
any intervention transforms the playing field (e.g. Berg 1996). While these surprises may be pleasant or unpleasant they are in any case viewed from within ANT as inevitable: Latour has recently called their effect the “slight surprise of action” (Latour 1999). However, knowing that one is not fully in control need not, of course, detain one from setting to work. In the following I survey some of the interventions recently described by Marc Berg.

5. Practical Heterogeneity and Abstract Practicality

I take my examples from the article “Implementing Information Systems in Health Care Organizations: Myths and Challenges”, directed at the medical informatics community through its publication in the *International Journal of Medical Informatics*. The paper has two aims. Given that the implementation of patient care information systems seems to be always a daunting task, it wants to first discuss three myths, which regularly continue to guide and “hamper implementation processes” (abstract) and, second, to explore the alternative guidelines for such processes, which could come out of a “sociotechnical approach” (144).

Before the explosion of myths and subsequent re-building of the landscape of medical informatics can take place, Berg formulates two caveats, both concerning the measure of “success” in relation to implementation. This is important because, in a nominalist gesture, “[t]he final decision is about the attachment of the label “success” or “failure” (or anything in-between) to a particular situation” (144). So “success” is not a simple thing, but is rather multi-dimensional: it can have to do with effectiveness, efficiency, organizational attitudes and commitment, worker satisfaction, patient satisfaction – and not all parties in and outside of the implementing organization may agree about which dimension should be the most relevant (145)

This point is typical STS-fare in its stressing of the multiplicity and heterogeneity of actors, aims and measures, but it is mixed up with the admission that

It is of course also possible to be less relativistic, and to set a success measure outside of an organization’s own deliberations…Only in this way, after all, can one compare different implementation processes (144)

The claim exemplifies a willingness to let go of the STS insight in order to secure a soothing rhetorical effect on behalf of the more rationalist inclined practitioners of
medical informatics. It enables readers to re-assure themselves that *that*, after all, is what they are interested in accomplishing, and that their particular endeavours are therefore *not* included in the otherwise argued for relativistic field.

Berg is put in a difficult situation by this assurance, for in his second caveat he asks, “how successful are success factors?” (146). While on the one hand he wants to argue “not so important after all”, he has already opened the possibility that *this* answer can co-exist with “quite important” or, indeed, “crucial for some purposes”. Berg becomes progressively entangled in the ramifications of this initial admission, as he introduces his main argument against the three prevalent myths, identified in relation with implementation and evaluation processes. For what he has termed myths are precisely the tacit assumptions that practitioners of medical informatics rely on in their work to develop, implement and assess new health care technologies. Thus he suggests that:

This is not to say that we cannot outline certain insights that seem to be a sine qua non to the realization of successful systems, however, defined. Indeed, in the following paragraph some of these insights will be discussed in the form of prevalent ‘myths’ that stand in the way of fruitful implementation (146)

Berg’s myths are in fact the everyday assumptions of his present readers and the elucidation of this point is the main contribution of the article: it is what opens up for an alternative formulation of the challenges of medical informatics. In the sentence just cited, which immediately precedes the statement of this demythologising aim, however, these myths are rendered equal to “certain insights that seem to be a sine qua non to the realisation of successful systems”.

Are they then valuable *insights* or *myths* or, indeed, ‘*myths*’ – where the inverted commas work to guard against the dangerous idea that he *really* think the mythical assumptions of medical informaticians are, well, mythical? Again, are the insights really discussed *in the form of myths*, or are they clarified in *contradistinction* to prevalent myths? In other words, are the myths really insights, or is the insight, rather, that the ‘myths’ are really myths?

From an STS point of view, there is of course no doubt that the latter is the case. The equivocation arises because the argument has been arranged in such a way that the stated goal is to replace one set of assumptions with an alternative set whose advantages
can be argued from an STS point of view, while the implications of these assumptions are elided in order to not offend the sensitivities of one important set of readers. The article nevertheless continues to elaborate the myths; that “PCIS implementation is the technical realisation of a planned system in an organization”, that “you can leave IS implementation to the IT department” and that “IS implementation can be planned including the required organizational redesign”, in a manner, which leaves no doubt as to Berg’s convictions concerning the myth-ness.

I turn now to the “Conclusion: The Challenge”. It can be first noted that the normative implications are spelled out here not as a set of practical recommendations, but rather in terms of a future common challenge. The challenge is to recognise implementation and development projects as mutually transforming all participants in the process, and to see them as a “balancing act between setting goals and targets for the implementations”, while “stimulating the mutual learning processes that will inevitably transform these goals and targets” (154).

Berg’s conclusions as regards these challenges are in significant accord with arguments I shall develop in the remainder of the text, notably in the suggestion that “accepting, and even drawing upon, th[e] inevitable uncertainty might be the hardest lesson to learn” (154), and he goes so far as to suggest that “searching for critical success or failure factors reinstalls exactly the urge for control that we should abandon, or at least mitigate, in order for the full potential of IS synergy to emerge” (154-55). While these are clearly excellent STS-points I wonder if they are considerably more normative than those made in many other such studies. If they are not clearly so, I wonder about the politics of concretisation that goes into naming them thus.

6. Prescriptive and Participatory Problems

While statement limits meaning and makes it sterile, allusiveness keeps the word open as to its deployment and makes it poignant with meaning (Jullien 2000: 354)

It is worth noting that the idea of better theory enabling better practice is not a particularly new one, nor, necessarily a particularly liberating one, nor necessarily, for that matter, a particularly sophisticated one. Indeed history can provide many examples of benevolent scholars aiming to improve society in multiple ways and with quite
variable results (e.g. Gould 1996). I propose that while STS arguments and articulations can and should take many different forms, they should not rely on a claim to *especial* prescriptive value.

In the conclusion to their interesting new book *The Gold Standard*, Stefan Timmermans and Marc Berg argue strongly for a positive engagement with what they call the Quality Improvement movement in health care. They suggest that: “entering into debate with the subjects of this study, so to speak, is what is now most relevant” (215). Sociology is rendered helpless and “deadly stale” by its repetitive critiques of rationalisation, McDonaldization, and so forth: “Failing to redraw our own politics of standardization would not only render us blind to all the transformations that are occurring in front of us. It would also render us powerless in its further development” (216).

It is definitely true that one cannot work, in non-humanist STS, with an old-fashioned conceptual separation between theory and practice. Timmermans and Berg interpret this predicament as implying that a theory would in some sense be legitimated by its practical efficiency. However, both engagement and practicality can mean many things. Wendy Brown, following Foucault, suggests distinguishing between studying *for* or *in terms of* a contemporary situation (Brown 2001: 42-3). The difference indicates whether

intellectual life will be submitted to existing political discourses and the formulation of immediate political needs those discourses articulate, or will be allowed the air of independence that it must have in order to be of value as intellectual work for political life” (43)

Her formulation connects with the view here embraced. This view emphasises that theory is *always a form of practice, always a form of intervention* and, therefore, *always political*. Since this is always the case one needs to do *nothing special* to accomplish the politicisation of research. Of course, this does not specify how practical, political, or any other consequences of one’s research will play out, and this can surely happen in multiple ways, in relation to which one is not totally helpless but certainly also not in full control of. It suggests, however, that an issue, which is perhaps as pressing as learning how to engage with practices on their own terms, is to retain breathing space for “non-practical” inquiry, by “sever[ing] critique from prescription” (Brown 2001: 118).¹ This is not an
argument for a return to a dark and solitary ivory tower; neither does it signal a withdrawal from participation in a vibrant field. As Brown points out to argue for a separation between intellectual and political life is not to detach the two. The point instead is to cultivate among political intellectuals an appreciation of the productive, even agonistic, interlocution made possible between intellectual life and political life when they maintain a dynamic distance and tension. By itself a political act at a time when universities are increasingly underwritten by “interested” corporate, private, and state funds, such cultivation is also quite possibly a route to freeing political life from its current moralizing despair and intellectual life from its grip of bad conscience (43-4).

One can, of course, attempt to spell out the specific political implications of one’s intellectual work as interpreted by oneself, but this is always a volatile gesture, as one’s (intellectual) interpretation is necessarily put into the hands of (practical) later users. As we know, these users have a tendency to confound expectations, and turn putatively radical ideas to conservative ends, or making creative use of a dangerous legacy (as in Haraway’s cyborg figure).

I would suggest, in addition, that the claim to be able to deliver especially useful guidelines, while it can be used to significant rhetorical effect, carries a high risk of back-firing in the long run. For insofar as the promised beneficial effects fail to materialise as promised, as in all likelihood they will, this may well be seen by those who have believed in these effects as demonstrating that STS is not in a position to deliver the practical goods of improving health care in much the same way as other management methods (business process re-engineering, new public management) have not been so able to deliver. This would furthermore be true, according to this analysis, but that is because STS-studies have a different task than delivering those goods.

If one takes seriously these propositions it becomes uninteresting for STS to claim access to a special capacity for guiding other practices. As argued in chapter eleven, they should concentrate on “the continuous development and refinement of more richly articulated, broadly responsive, and subtly differentiated…accounts” (Smith 1988: 172).

In my estimation such withholding of implications would furthermore be more conducive to the experimental processes of mutual learning and transformation that Berg recommends than offering “position taking, policy formulation, or blueprints for action” (Brown 2001: 43). As in Jullien’s description at the beginning of this section, the ambivalence and non-explicitness regarding consequences may “keep the word open as
to its deployment and make it poignant with meaning” and might thereby better facilitate a creative process of transformation.

7. Thinking In Front Of…and Learning to Laugh

As Deleuze said, to think (or create) is to think “in front of” or “for” analphabets or dying rats or alcoholics. This does not mean addressing them, or helping them, or sharing hope or faith with them, but, rather, not insulting them with our power to justify everything. Thinking with them “in front of” us means thinking with the feeling and constraint that we are not free to speak in their name or even side with them (Stengers 2002: 238)

In the sections above I have referred to a theoretical preference for “extremity” and against “equivocation” advocated by Barbara Herrnstein Smith. How does this relate to Isabelle Stengers rather differently sounding Leibnizian recommendation that one should not overturn established sentiments? It will be necessary to first of all prevent “the alternative” and “the established” from being turned into steel-cased oppositions and rather use the formulations to explore possible openings and productive contrasts.

Some of these become more visible if we follow the “intellectual vectors” of these scholars. Barbara Herrnstein Smith comes with a special interest in the potentialities, in many different fields, of constructivist thought. Her worry, as displayed in “Cutting-Edge Equivocation” and elsewhere is that the important differences and most crucial consequences established by these new styles of thought, are flattened in the name of old conceptual or political categories.

Stengers, on the other hand, comes with an interest in thinking about the possible development of an “ecology of practices”, which would not be homogenised by power, which she identifies with the figures of the Politician, the Policeman, the Scientist, and the Critic. This is the connection in which she argues that: “nothing is easier for a modern person than to be tolerant”. She identifies the ease of tolerance with freedom of a particular kind, as instantiated in the figures listed above; it is the freedom brought about by a power by means of position, which ensures that one does not have to engage with other practices on their premises. In this way a generalised state of easy tolerance articulates a negative phenomenon. It indicates that too many practices manage too well to ignore and stay indifferent to each other, each filling out its own limited niche in a fragmented ecology.
Tolerance quickly turns antagonistic when differences become serious enough to threaten key understandings of the involved parties. As an example consider how arguments relying on notions of “genetic determinism” would be evaluated in a scientific ecology where molecular biology was not virtually guaranteed to be taken more seriously than arguments from the social sciences, or how grand visions of technological improvement might be interpreted if STS-studies had a status similar to engineering.

Encounters whose results are not pre-determined by position may force practices and actors to re-consider who they are, what they are doing, and what their relationships are to other parts of the ecology. Easy tolerance is a symptom of stasis and fragmentation, whereas encounters entailing genuine risk, while “int tolerable” may therefore enable the construction of creative links between practices.

In Stengers’ ideal, practices would not exist in separated alcoves, but would be in significant exchange with each other. The shared trait of the types identified above is their inability or unwillingness to participate in such exchange. Because these figures are capable only of tolerating different practices, they cannot respect them as partners or foes in engagements involving transformation. All this suggests that tolerance partakes in a logic of power:

power, when it grows a capital letter, transforms the rhizome into a tree: each branch is “explained” by its relation to another branch, one closer to the trunk, and indeed to the roots, that is, to the site – occupied by a “logic” if not by actors – from which all the rest can be denounced as puppets, acted on beyond their intentions and plans (Stengers 2000: 124)

The organization of the sciences into a hierarchy according to the depth of their explanatory power (typically with physics on the summit and the humanities in the basement) has been under heavy fire, not least from Bruno Latour, who has likened epistemology to a “professional hazard…much like a bad back” (Latour 1996). Stengers points out, however, that “it is difficult to put…the ‘error of the epistemologists’, rather than power, in the role of the thing responsible for everything that does not go well” (124). “Error”, she continues, “does not have to be denounced any more than power. It explains nothing, except insofar as it is a product of the network, characteristic of the style of the network that belongs to our epoch, and of the political problem it poses” (124).
This formulation indicates a different point of entry to an analysis of the politics of concretisation and it points to the fruitfulness of locating an investigation into the recurrent felt need to be political or practical on the level of causes – asking “what kind of networks make these interests and strategies understandable?” – rather than symptoms – asking, as I have done, “how does this manifest itself textually and argumentatively, and with what consequences?”

One might, then, agree with Stengers, that denouncing “epistemological errors”, or conceptual problems more broadly, explains nothing, and also that such explanation would have to take into account the broader intellectual, disciplinary, and institutional dynamics, which have STS-scholars located in widely diverging intellectual milieus with equally varying requirements and exigencies. Undeniably one’s various audiences have a more or less direct influence on the way one’s own discourse is shaped, since it is shaped in the attempt to be understood, responded to, and acted upon by those audiences.

However, the fact that a symptomatic reading of politics of concretisations explains nothing, does not indicate that it is without function. When Barbara Herrnstein Smith spends her energy pinpointing what Stengers might view as “errors of epistemologists”, it is just because these positions are “intolerable” to her as a participant intellectual who is in various ways affected by such claims. Neither are her alternative descriptions of multiple phenomena separated from an understanding of the structures which compulsively re-generate “epistemologists’ errors”, for they are also attempts at making those networks and their activities intelligible in new terms (see Smith 1988, 1997). Nevertheless the tension emphasised by Stengers raises the interesting question of how one can remain a radical and unrepentant constructivist without merely alienating “established sentiments”.

The topos defined by these agendas can be opened up by Stengers’ specification that

the problem designated by the Leibnizian constraint ties together truth and becoming, and assigns to the statement of what one believes to be true the responsibility not to hinder becoming: not to collide with established sentiments, so as to try to open them to what their established identity led them to refuse, combat, misunderstand (Stengers 2000: 15)

The “responsibility not to hinder becoming” has a high degree of openness and intellectual versatility as a pre-requisite, because nurturing such transformational
processes require a capacity for engaging divergent discourses and practices, with respect but therefore neither with tolerance nor with submissiveness. In specific the responsibility involves letting go of what Deleuze called “the indignity of speaking for others” (Deleuze 2004: 208) with its attendant modern tendency to “justify everything”. This, I think, is what it means to think “in front of” other actors, situations, and practices. Stengers explicates one of its entailments:

We must be clear that this does not mean we will reach a world where everyone will be beautiful and kind. I hope to make myself hated, but I would like to try to not be hated those whom I have no desire to offend – that is, all those who submit to the mobilizing power of words that recruit them into antagonistic camps, without for all that having an active stake in the maintenance of this antagonism (Stengers 2000: 17)

Hatred, of course, is an unusual word to evaluate positively in a philosophical discourse. Consider, in contrast Donna Haraway’s methodical dictum to “critically analyze, or “deconstruct” only that which I love and only that in which I am deeply implicated (Haraway 1997: 151). Who then is the critical scholar? Is it Stengers, who urges us to stop trying to overturn established sentiments, while condemning tolerance and encouraging a form of hatred, or is it Haraway who approvingly cites Walter Benjamin’s *Theses on the Philosophy of History*, to the extent that “we shall clearly realize that it is our task to bring about a real state of emergency, and this will improve our position in the struggle against fascism”, while vowing to deconstruct only that which she loves? As is the case with many other terms, neither hatred nor love goes through the intellectual machinery of these scholars, without emerging in new constellations.

The “rhizome” surreptitiously connecting the superficial opposition is humour. Stengers notes that to “relearn to laugh is never insignificant” (2000: 17), for “the laughter of someone who has to be impressed always complicates the life of power” (17-18). Her laughter is neither critical nor ironical, but humorous in its aim to “comprehend and appreciate without waiting for salvation”, and its capacity to “refuse without letting itself terrorize” (18). In a similar gesture Haraway writes: “I laugh, therefore I am...implicated. I laugh, therefore I am...responsible and accountable” (Haraway 1997: 182).

In this vein the present text, rather than attempting to unveil or denounce contemporary Danish EPR visions and projects, has had a rather different aim. Its
direction has been one of empirically based conceptual experimentation. Through a number of specific thematic discussions, drawing on a range of conceptual tools in non-humanist STS studies and its disciplinary neighbourhoods, I have tried to pose a number of intellectual and, why not, practical and political, challenges to some of the organizations, institutions and disciplines that tend to “suffer from an advanced case of hardening of the categories” when they envision technical solutions to a multitude of perceived problems. I hope to have also conveyed some resources for retaining if not a laugh then a smile when confronted with the many impressive instances of the pathos of the new, which hovers around the notion of IT development and electronic patient records.

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1 For some her figure seems distinctly too moderate, not to say conservative. Braidotti refers to Stengers regressive “post-poststructuralism” (1994: 23), while Nina Lykke (1996: 21-22) likens her position to that of a modern Auguste Comte!
2 As I note in part six, Isabelle Stengers in fact encourages such an analysis.
3 An expanded version was printed in Modest Witness… (Haraway 1997: 49-119).
4 In Brown’s formulation: “We do no favor, I think, to politics or intellectual life by eliminating a productive tension – the way in which politics and theory effectively interrupt each other – in order to consolidate certain political claims as the premise of a program of intellectual inquiry. Indeed, we usurp the increasingly scarce space allocated today to thinking” (Brown 2001: 41).
5 The analysis of strategic essentialism in Smith (1997) has inspired these considerations.
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Resume af Experimental Devices:
Studies in STS and Electronic Patient Records
(Dansk Titel: Eksperimentelle Anordninger:
Studier i STS og Elektroniske Patientjournaler)


Afhandlingen er en del af science and technology studies (STS), et inter- eller transdisciplinært studieområde med deltagere fra feminist teori, filosofi, historie, informations-, kommunikations- og medievidskab, kulturstuder, organisationsteori, socialantropologi, socialpsykologi og sociologi. Alle disse felter har udviklet vigtige koncepter og modeller, og i dette studie trækker jeg frit på mange af disse, mens jeg udlægger og omfortolker en række problemstillinger indenfor STS og en bredere samfundsvidskabelig horisont.

I løbet af afhandlingen udfoldes ”teoretiske” diskussioner som illustreres ”empirisk”. Det empiriske materiale har at gøre med ovennævnte sæt af nye teknologier,
elektroniske patientjournaler, som igennem de sidste ti år i stigende grad er blevet set som centrale komponenter i fremtidens danske sundhedssektor. Elektroniske patientjournaler synes i dag interessante for mange sundhedsfaglige og relaterede grupperinger, som sygeplejersker, læger, ingeniører, medicinske informatikere og politikere.

STS-studier har lært fra discipliner såsom social- og kulturantropologi, kvalitativ sociologi og historie at være særlig opmærksom på empirisk detalje. På forskellig vis er teoretisk udlægning blevet set som nødvendigvis empirisk baseret, og det er svært at være uenig med en sådan naturalistisk og materialistisk indstilling. Men selvom de følgende kapitler er baseret på grundige empiriske studier kan de ikke siges at udgøre en STS-etnografi over udviklingen af elektroniske patientjournaler, og søger man efter en sådan vil man sandsynligvis skuffes. Problemet ligger ikke i en mangel på detaljeret observation, ellers dennes irrelevans for senere analyser, men i at afhandlingens fokus ligger andetsteds, og kan synes unødigt teoretisk for visse læsere.

"Teoretisk" såvel som den komplementære term ”praktisk” er imidlertid yderst ladede og meget tvetydige kategorier. Med en STS sensibilitet bliver det højst tvivlsomt at der eksisterer noget der er ”teoretisk”, som på simpel vis kan adskilles fra noget ”i praksis”. Forestillingen om at teori per se er unødvendig, hvis nogen virkelig holder den, er baseret på en ide om at empirisk materiale forklarer sig selv; det vil sige en version af positivisme. Men lige så problematisk er den tilsyneladende mindre naive ide at verden viser (data) og forskeren derefter fortolker (teori).

Det er på ingen måde klart at det teoretiske og det praktiske er således dikotomisk relæteret. På den anden side er det en kompliceret affære at udforske og klargøre hvordan disse termer faktisk er relateret, og en sådan undersøgelse er af central betydning for afhandlingen. I stedet for at forsøge mig med en neutralt beskrivende udlægning af hændelserne indenfor den danske udvikling af EPJ, er tilgangen således at bruge mit materiale til at udføre en empirisk baseret dekonstruktion af teori-praksis koblingen. Dette er særligt relevant for en overvejelse af hvad man kan med STS-studier og hvad disse studier bør bruges til. En sådan udforskning er med andre ord særlig vigtig for en gentænkning af relationerne mellem STS og normativitet.

Det er bemærkelsesværdigt at studier indenfor STS og megen anden samfundsvidenskabelig forskning implicit bygger på teori-praksis dikotomien og


Projektet forsøger at være nyskabende på to fronter. For det første igennem dets, selvsagt begrænsede, indvirkning på etno-forståelser i de forskellige verdener der beskæftiger sig med EPJ. For det andet igennem dens, igen delvise, deltagelse i en rekonfigurering af intellektuelle kategorier og forståelsesrammer i STS og relaterede forskningsområder. Uanset indsatsområdet er min teoriudviklende ambition at udvikle
alternative konceptualiseringer, der er i stand til at se forskellige praksisser og typer af viden som helt igennem *performative* og *dynamiske*, med alt hvad dette indebærer.

Ovenstående distinktion mellem EPJ verdener og STS verdener leger med den gamle dualisme mellem det interne og det eksterne, men beror ikke ganske på den. Tværtimod er det diverse STS-studiers destabilisering af just denne distinktion, der tillader mig at have en forhåbning om at disse studier kan have samtidige effekter ”indenfor” og ”udenfor” STS, omend afhandlingens vedholdende fokus på sådanne destabiliseringer forøger chancerne for at den vil blive lokaliseret som ”intern” af mange læsere. Det er i orden: destabilisering betyder ikke at alle grænser nedbrydes eller forsvinder; istedet for bliver de mere flydende. I denne analyse signalerer esoterisk indhold derfor ikke bred irrelevans, ligesom offentlig relevans ikke nødvendigvis indikerer at et tema eller en indsigt er teoretisk ubetydelig og overfladisk. Men relevanskriterierne er variable, hvilket fordrer samme kvalitet af ens eksperimentelle anordninger.

Med denne indstilling er intellektuel analyse ikke adskilt fra praktisk arbejde; det er blot en særlig måde at udføre det på. STS-studier har derfor to formål, der ikke nødvendigvis er urelaterede. De foretager konceptuet arbejde indenfor deres egne og relaterede discipliner. Og de forsøger at skabe meningsfulde sammenhænge ud af deres studier på måder der i bedste fald har relevans for de praktikere hvis arbejdsområder de beskæftiger sig med.

I resten af dette resume følger en beskrivelse af hvordan min intellektuelle agenda, som kort udlagt ovenfor, foldes ud i teksten. Dette har været et knudret problem, fordi det empiriske materiale har åbnet op for et yderst heterogent billede af dansk EPJ udvikling. Hvordan sammenvæver man elegant en serie af samfundsteoretiske overvejelser med en adækvat beskrivelse af empiriske processer?

I afhandlingens organisering har jeg forsøgt at bibeholde en vis diskontinuitet imellem temaer og emner, i håbet om at resultatet vil være konceptuel fleksibilitet snarere end udifferentieret forvirring. Samtlige kapitler navigatorer dette risikable farvand, idet de på forskellig vis benytter stumper af ”teori”, diskuterer ”metode” og ”beskriver” empirisk materiale. Dette er ikke et forsøg på *bricolage*: en mere passende rekursiv trope er fraktal.
Organiseringen er som følger. Med alle ovenstående kvalificeringer *in mente* tilbyder del 1 "Resources" en teoretisk og metodologisk værktojskasse. Del to "Strategies" og del tre "Illustrations" er empirisk orienterede, mens fokus i del fire "Implications" indikeres af titlen.

Del 1 ”Resources” har tre kapitler. Det første “Resources for Non-Humanist Studies of Technoscience: A Theoretical Assemblage”, er det mest klassisk teoretiske, idet det placerer afhandlingen i et intellektuelt landskab. Kapitlet angiver omridset af et sæt af ressourcer, som kan fungere som eksperimentelle anordninger med henblik på at effektuere konceptuel eller praktisk transformation i deres møde med EPJ (i senere kapitler). Topologien er non-humanist STS, post-klassisk teori, konstruktivism, post-humanisme og post-strukturalisme. Kapitlet trækker på en bred vifte af teoretikere under udvikling af en analyse der hævder at non-humanistiske studier af teknologi og videnskab har et temmelig anderledes sigte end kritiske kulturteorier, fordi de analyserer disse forhold som gennemført performative snarere end repræsentationelle, og ontologiske snarere end epistemologiske.


Kapitel tre “Researching Partially Existing Objects: What is an Electronic Care Record? Where do you find it? How do you study it?” peger i retning af den elektroniske patientjournal. Idet jeg bygger videre på pointer fra de første to kapitler, argumenter kapitlet at EPJ er en yderst tvetydig entitet. Faktisk synes den at have en paradoksral kvalitet, for snarere end en singulær moderne ting, kan den ses som et delvist eksisterende objekt.
Strategierne i anden del af afhandlingen er læsestrategier. Spørgsmålet er hvordan man kan diagnosticere og engagere sig i sociotekniske begivenheder som de præsenteres i mangfoldige rapporter og andre politiske dokumenter, som hurtigt spreder sig hvorend teknologisk udvikling promoveres. Jeg analyserer disse tekster som mangesidige og performative snarere end som simple kommunikative enheder, der spreder intentioner og ideer.

Kapitel fire hedder “Political and Moralising Moments: Documents as Material Agents in Danish Visions of IT in Health Care” og er skrevet med Brit Ross Winthereik. Kapitlet analyserer dokumenter fra to IT-projekter i det danske sundhedsvesen og foreslår at disse kan ses som eksempler på en specifik informations genre, der er særlig velegnet for udvikling og vedligeholdelse af visioner.

Kapitel fem “Reading Digital Denmark: IT-reports as Material-Semiotic Actors”, er skrevet med Peter Lauritsen, og beskæftiger sig direkte med spørgsmålet om læsestrategier. Vi analyser den væsentlige danske IT-strategi Det Digitale Danmark igennem to sådanne strategier, som vi kalder læsning ”med” og ”mod” teksten. Læsning mod teksten kendetegnes ved at være kritisk og skeptisk i dens søgen efter gentem eller undertrykte meninger, som kan tilskrives aktørers dårlige motiver eller tvivlsomme intentioner. Læsning med teksten indebærer istedet at forskeren følger denne, som den rejser fra praksis til praksis og beskriver dens konkrete effekter: Hvor bevæger rapporter sig og hvad gør de når de ankommer? Denne kontrast har normativ betydning og vi diskuterer de politiske implikationer af læsninger ”med” og ”mod”.

”Illustrationes” følger EPJ i en række varierende kontekster. Man kunne hævde at disse kapitler følger EPJ i tid og rum, men det ville være mere præcist at sige at de følger hvordan EPJ skaber en tid og et rum for sig selv.

at tydeliggøre hvordan denne eklektiske konstruktion blev udviklet og hvad man gjorde for at stabilisere den.


I det følgende kapitel “Citizen Projects and Consensus Building at the Danish Board of Technology: On Experiments in Democracy”, bevæger analysen sig i den modsatte retning og spørger hvordan det er muligt at håndtere potentielt inkommensurabilitet på demokratisk forsvarlige måder. Kapitlet beskriver min deltagelse som ekspert i Teknologirådets konsensus-projekt om elektroniske patientjournaler. Disse projekter er internationalt velkendte og meget roste for deres borgerinddragelse, og som min analyse viser er de i stand til at skabe interessante og demokratisk relevante effekter. Men jeg formulerer også en række udfordringer til projekterne som de fortolkes af Teknologirådet, og foreslår at se dem som uafvendeligt eksperimentelle situationer.

I kapitel ti vender jeg mig mod “Infrastructural Fractals: Re-Visiting the Micro-Macro Distinction”. Dette kapitel analyser transformationerne påvist tidligere i
afhandlingens sidste del hedder ”Implications” og har to kapitler. I kapitel elleve “Technologic: Conceptualising Organizational Transformation with the EPR” foreslår jeg at en form for teknologisk mønster kan opdages i det empiriske materiale beskrevet i afhandlingen. Denne ’tekno-logik’ minder om hvad Jacques Derrida har kaldt en supplementaritets-logik, og den konstruerer specifikke problemer og udfordringer for praktisk og konceptuel styring og planlægning af forhold omkring EPJ udvikling og organisationel hospitalsudvikling.


Selvom afhandlingen ikke er åbenlyst (eller klassisk) kritisk, kan de følgende essays læses som et forsøg på at omformulere traditionelle og dominerende forståelsesrammer indenfor diverse områder, såsom medicinsk informatik, management og organisationsstudier, læringsteori, sociologi, histori, filosofi og måske endog litteraturteori.

Jeg forestiller mig ikke at disse studier kan eller skal forandre disse praksiser og teorier på særlig dramatisk vis. Men måske er et mindre ambitiøst mål tilfredsstillende. Hvis læsning af de følgende kapitler generer visse nye tanker hos nogle læsere – til transformation og brug i andre projekter med andre problemstillinger – vil det være hele arbejdet værd.