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ORGANIZING SCIENCE IN SOCIETY – THE CONDUCT AND JUSTIFICATION OF RESPOSIBLE RESEARCH

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– THE CONDUCT AND JUSTIFICATION OF RESPOSIBLE RESEARCH

PhD School in Organisation and Management Studies

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CBS COPENHAGEN BUSINESS SCHOOL
HANDELSHØJSKOLEN

ORGANISING SCIENCE IN SOCIETY

THE CONDUCT AND JUSTIFICATIONS OF RESPONSIBLE RESEARCH

Cecilie Glerup

Copenhagen Business School
Doctoral School of Organization and Management

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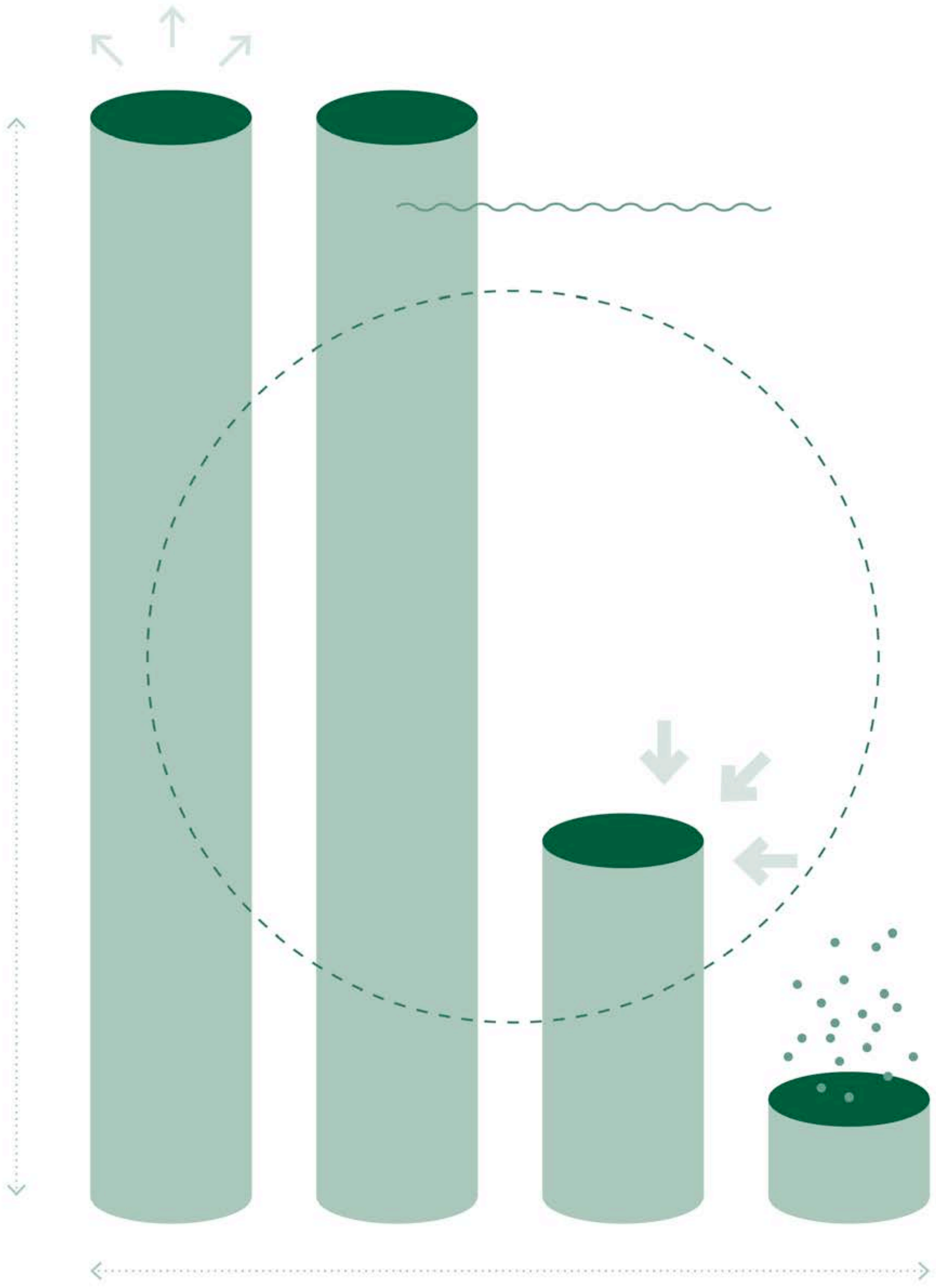
The Doctoral School of Organisation and Management Studies (OMS) is an interdisciplinary research environment at Copenhagen Business School for PhD students working on theoretical and empirical themes related to the organisation and management of private, public and voluntary organizations.



'In an orgy of virtue, we seem to lose our grip on decency'

Bayliss Manning, quoted by John Rohr (1998) in:

Public Service, Ethics, and Constitutional Practice

I ACKNOWLEDGEMENTS

They say it takes a village to raise a child. The same turns out to be true about completing a PhD project. These first two pages constitute a formulation of my deepest gratitude to those members of the village who have played a great part in helping me realise this dissertation.

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individual who was able to convince me that the best dissertation is the finished dissertation. Your mere existence is the most powerful lesson in what is important and dear in life. Just for that, I am forever in your debt. I wonder what other things you are going to teach me as your presence makes itself increasingly known.

Copenhagen, August 2014,
Cecilie

ABSTRACT

Within the last couple of decades, a range of new concepts that all propose that science should be done 'more responsibly' has emerged within science governance literature as well as in science government in both the USA and across Europe. Terms such as '*Responsible Innovation*' (Owen et al. 2013) and '*socially robust science'* (Nowotny, Scott, and Gibbons 2001) have gained momentum within science governance. Generally speaking, the calls share the view that there is a need for more external governing of science as a vital supplement to the internal professional ethics that also guide scientific conduct (Braun et al. 2010; Jasanoff 2011). Moreover, they agree that there is a need to enhance scientists' abilities to reflect upon the 'outcomes' of their inventions – that is, the social, environmental and ethical consequences of introducing new scientific knowledge and technologies into society. Though the calls for '*Responsible Science*' are plentiful, few have actually studied how 'Responsible Science' is done in practice and how the demands affect the scientific work, i.e. the organisation of science, the scientists' professional identities and their well-being at work. This dissertation examines how public scientists relate to current demands for 'Responsible Science'. Based on a Foucauldian-inspired document study of scientific journal papers as well as an STS-inspired ethnographic study of two laboratories, it answers the research questions:

How is 'Responsible Science' conducted and justified by public scientists – and what are the consequences of these responsibilities in their daily work?

Based on the document study, a typology of 'political rationalities' of 'Responsible Science' is constructed. Four political rationalities are identified: The Demarcation Rationality, which aims to exclude the social from the scientific production in order to make it objective and thereby responsible; the

Reflexivity Rationality, which considers it science's responsibility to choose research areas based on problems in society; the Contribution Rationality, which insists that responsible science should live up to public demands for innovation and democracy; and the Integration Rationality, which advocates that science should be co-constructed with societal actors. The four rationalities describe idealised versions of what being a 'responsible scientist' entails. Each rationality is distinct, but it is argued that all of them address the issue of a boundary (or integration) between science and society. The political question is how this relationship is to be defined and regulated.

Based on the ethnographic laboratory studies, three '*modes of responsibility*' (Law 1994) are identified: Vocation, Oikos and Citizenship. The three modes of responsibility illustrate the responsibilities that the scientists have to live up to in their daily work and it is described how they attempt to do so. It is also shown that each mode entails a specific idea about what the scientists have a responsibility *for*: namely, the 'truth', the 'business' and the 'public'. Each mode also entails specific ideas about science's role in society and the scientists' professional roles. While the scientists do find each responsibility more or less reasonable, the study also shows how they find it frustrating and confusing to navigate between all of them in their daily work. Not only are they at times in doubt about the evaluation criteria associated with what they are doing, but they also have to work hard to live up to all three responsibilities. At the same time, they feel that their professional autonomy is threatened. Based on these findings, this study concludes by asking how 'Responsible Science' can be developed in ways that make it more meaningful and less frustrating for the scientists to relate to in their daily work.

DANISH ABSTRACT

I løbet af de sidste årtier er der opstået en række krav om, at offentlig forskning skal være 'social ansvarlig'. Disse krav ses både i science governance-litteraturen og er også reflekteret i forskningspolitik både I USA og Europa. Termer som '*Responsible Innovation*' (Owen et. al. 2013) og '*socially robust science*' (Nowotny, Scott & Gibbons 2001) har vundet indpas i forskningspolitikken. Overordnet set deler de forskellige krav det syn, at der er brug for mere ekstern styring af forskningen som et vitalt supplement til de interne etikker der allerede guider det videnskabelige arbejde (Braun et al 2010; Jasanoff 2011). Der ud over ser der ud til at være enighed om, at der er brug for at hæve forskeres generelle færdigheder udi at reflektere over de bredere konsekvenser af deres forskning – det vil sige de miljømæssige, sociale og etiske implikationer af at introducere ny videnskabelig viden og nye teknologier i samfundet. Selvom disse krav vinder indpas i forskningspolitikken internationalt set, er der indtil nu kun meget få studier af, hvordan 'ansvarlig forskning' bliver gjort i praksis og hvordan det påvirker det videnskabelige arbejde, dvs. organiseringen af det videnskabelige arbejde, den videnskabelige profession og forskernes generelle tilfredshed med deres arbejde. Dette forskningsprojekt undersøger, hvordan offentlige forskere forholder sig til disse krav om 'ansvarlighed'. Baseret op Foucault-inspireret dokumentstudie af videnskabelige artikler og et STS-inspireret etnografisk studie af to laboratorier, svarer det på forskningsspørgsmålene:

Hvordan bliver 'ansvarlig forskning' foretaget og retfærdiggjort af offentlige forskere og hvad er konsekvenserne af at skulle leve op til bestemte former for ansvar i forskernes daglige arbejde?

Projektet udvikler, baseret på en dokumentstudie, en typologi over ‘politiske rationaliteter’ om ansvarlig forskning. Fire politiske rationaliteter identificeres. ‘Afgrænsningsrationaliteten’, som søger at ekskludere det ’sociale’ fra den ‘videnskabelige’ produktion for at gøre forskningen objektiv og dermed ansvarlig. ‘Refleksivitetsrationaliteten’, som ser det som videnskabens ansvar at lade sig guide af problemer i samfundet i sit valg af forskningsområde. ‘Bidragsrationaliteten’, som anser det for vitalt, at forskningen lever op til samfundets krav om demokrati og innovation og til sidst ‘Integrationsrationaliteten’, som advokerer for, at forskningen skal skabes i tæt samarbejde med samfundets andre aktører. De fire rationaliteter beskriver hver især idealer for, hvad det vil sige at være en ‘ansvarlig forsker’. Hver rationalitet er distinkt, men der argumenteres for, at de alle adresserer en problematik om en ‘grænse’ mellem forskning og samfund. Det politiske spørgsmål bliver i forlængelse heraf, hvordan denne grænse skal defineres og reguleres.

Baseret på et etnografisk laboratoriestudie bliver tre ‘modes of responsibility’ (Law 1994) identificeret. De får navnene ‘Kald’, ‘Oikos’ og ‘Medborgerskab’. De tre ‘modes’ illustrerer de former for ansvar, som forskerne har i deres daglige arbejde og det er beskrevet, hvordan de forsøger at leve op til dem. Det bliver også vist, at hvert ‘mode’ indeholder en distinkt, abstrakt ide om, hvad man har ansvar *for*, nemlig ‘sandheden’, ‘organisationen’ og ‘offentligheden’. Hvert ‘mode’ indeholder også en forestilling om forskningens rolle i samfundet og forskerens professionelle rolle. Selvom forskerne finder hver form for ansvar nogenlunde fornuftigt, viser studiet også, at de finder det frustrerende og forvirrende at navigere imellem dem alle tre i deres daglige arbejde. Samtidig føler de, at deres professionelle autonomi er truet. Baseret på disse resultater bliver afhandlingen konkluderet med at spørge, hvordan der kan arbejdes med ‘ansvarlig forskning’ på en måde, som forskerne finder mere meningsfuld og mindre frustrerende i deres daglige arbejde.

1. INTRODUCTION

It is early summer and, fortunately, also the final months of this research project. In addition, it is the week before the biannual, European science fair, 'Euroscience Open Forum'[1] (ESOF), occupies Copenhagen. As I cycle home from work, posters along the road, at bus stops and surrounding the continuing roadwork try to entice me to go to the fair, pointing both to the wonders of science and to science's immediate relevance to me by stating: '*Science is a power-nap*', '*Science is out of space*', '*Science is Art*' and '*Science is YOU*'.

ESOF is arranged with the threefold goal to: *'showcase the latest advances in science and technology; promote a dialogue on the role of science and technology in society and public policy; stimulate and provoke public interest, excitement and debate about science and technology.'* And it is supposed to attract the '*top researchers from all sciences; business leaders; senior EU and government officials and international scientific media. They come to discuss the best of European science and to address all of the current major global scientific challenges, including energy, climate, food and health.'*[2]

The public part of the event, where citizens and participants are invited to explore the intriguing world of science, is grander than it has ever been before. While the fair itself is not accessible to the public, the "Science in the City" Festival invites the public in. It takes place at the same venue as the rest of the fair, the former factory grounds of the international brewery Carlsberg, which is near the city centre of Copenhagen. Carlsberg has now moved most of its production away from the grounds, which are now going to be turned into a brand-new residential part of the city. The placement of ESOF and "Science in

[1] ESOF is organised by 'Euroscience', a European non-profit grassroots organisation, who represents European public and corporate scientists as well as public universities and research institutes. From www.euroscience.org/welcome-to-euroscience 25.08.2014
[2] From www.esof2014.org/info/about 25.08.2014

the City” in itself feels symbolic: Carlsberg is one of Denmark’s most successful international companies. Moreover, Carlsberg’s brewing methods have since its founding been based on, at the time, revolutionary scientific knowledge about fermentation and yeast; knowledge that the founder shared with other brewers across Europe at no cost. The Carlsberg foundation has been a substantial provider of funding for Danish research and art for more than a hundred years. Carlsberg itself still has big laboratories and conducts science, and it therefore only seems fitting that an event that is supposed to bring together the public, industry, policy-makers and science in order to produce ‘*dialogue on the role of science and technology in society’* takes place on these historical grounds.

Representatives from most of the Danish universities are in attendance at the “Science in the City” festival, with each university giving presentations of their cutting-edge research, mixed with exercises that are meant to activate the visitors in various ways, so they can understand, discuss, respond, marvel, and *engage*. The festival is huge. Among hundreds of activities, there are, for instance, small talks (five minutes maximum) given by scientists about their own research; there is a large sculpture of the Nordic ice cap, which children can climb; the Department of French from Copenhagen University serves French crêpes; and Copenhagen Business School presents interpretations of the recent financial crisis. The tent of the Danish Technical University seems to be the most popular one. Here, you can, for instance, try to tattoo your name on an apple with the newest laser-technology, experiment with a 3D printer and look under the bonnet of an electric car. But what are the fair and the public festival all about? Why are so many resources and so much energy being put into the organisation of this festival?

The European Commission regards European science as a lever for the European economy and general welfare after the financial crisis. Europe must produce science that benefits its citizens and its markets. As the Commission said about their next funding programme for research and innovation, Horizon 2020:

'Seen as a means to drive economic growth and create jobs, Horizon 2020 has the political backing of Europe's leaders and the Members of the European Parliament. They agreed that research is an investment in our future and so put it at the heart of the EU's blueprint for smart, sustainable and inclusive growth and jobs.'[3]

Science is supposed to be sustainable, relevant to the common European future and create inclusive growth. This presumably requires a lot of effort for those who are supposed to conduct the science. The inclusion of people, networks between scientists, industry, policy and the public is needed to make these visions a reality – as the Horizon 2020 webpage also states. ESOF and the "Science in the City" festival can be interpreted as an effort to do so – initiated by Euroscience – a grassroots organisation for European scientists.

However, it is not only in the EU that this seems to be the goal. As Strathern (2000) argues: '*the twinned precepts of economic efficiency and good practice*' are pervasive on a global scale as a specific form of government culture that is visible in strategies such as '*Corporate Social Responsibility*', '*Good Governance programs*' and the '*Rio declaration*' (Strathern 2000: 2). This dissertation is occupied with this kind of government in the domain of science, in which it takes particular forms. 'Social responsibility' has emerged as a

[3] From www.ec.europa.eu/programmes/horizon2020/en/what-horizon-2020 20.08.14

particular concern within science over the last three decades. It can perhaps best be described as a particular discourse or rationality that is both apparent in academic literature and political practice in the Western world. At the international policy level, the rationality is visible in soft law initiatives by the EU and UN, but it has also spread to national policies in, for instance, the UK, the USA and Denmark. Yet there is no clear, coherent definition of what this 'responsibility' is, even though it is underlined as being important (Davies and Horst 2012). Despite the lack of a coherent definition or precise protocols of practice, a certain agreement seems to manifest itself around the two themes of economic growth and democratisation. In order to be socially responsible, science should preferably produce innovation, while democratic participation and public debate are supposed to secure the balancing of the outcomes, so that the knowledge and technologies are considered legitimate

While this dual focus may be a recent phenomenon, the idea that science has responsibilities toward society is not. Historically, there have been several notable discussions of science's moral responsibility in relation to the development of new and potentially very dangerous technologies. The Manhattan Project – the development of the atom bomb during WW2 – is one noteworthy example. In the seventies, the discovery of recombinant DNA and its far-reaching potentials for biological design spurred the Asilomar conferences, where the scientists' ability to govern themselves responsibly was on the agenda (Braun et al. 2010). These discussions inspired new governmental organisations such as ethics committees in a number of countries and instigated more general discussions of the most optimal way to govern science. Many technological developments between these two incidents kept the debates alive because they were seen as controversial, specifically, the use of nuclear power as an energy source, the development of the hydrogen bomb and the first IVF technologies.

In the eighties, the gradual discovery of far-reaching environmental disasters due to human inventions led sociologist Ulrich Beck (1986/1992) to suggest that we now lived in a '*Risk Society*'. This notion implies a society where both social structures and material objects are (perhaps) pervaded by the negative, unintended side effects of technologies. The other side of the coin of the 'Risk Society' is that we always imagine the world as uncertain and risky – even when it is perhaps not. Around the same time, the philosopher Hans Jonas suggested that it is *'not a sin to delay a benefit'* of a technology and advocated for a new 'responsibility' paradigm (1981). These works were followed by requests to enhance the '*reflexivity*' of science and integrate social values and democratic decision-making into the techno-scientific developments (Wynne 1994; Irwin and Wynne 1996; Irwin 1995)

These debates about science's moral obligations and controversial technologies have been running parallel to debates about public science's role in modern society. Since the end of WW2, science has become deeply integrated within the government's structure. The US government, for instance, has historically been the frontrunner in funding both basic and applied sciences and demanding groundbreaking technologies in return – not least in the area of national defence (Smith 1990). Later, notions of 'globalization' and 'the knowledge economy' gained momentum in policy discourse at the same time as the demands for scientists to become 'innovators' and 'drivers of the knowledge economy' became prominent. With these developments, the role assigned to public scientists has also changed. Several university reforms have been initiated since the eighties. The biggest changes meant that scientists were now expected to compete for funding and establish close collaborations with industry. All in all, they were designated a role as part of a public-private network, as opposed to being firmly rooted and paid by the state. The notion of the '*entrepreneurial scientist*' was developed to describe this new role (Etzkowitz 1989). Meanwhile, older professional norms on how to conduct oneself and one's work have also survived the eras of fluctuating science-society relationships. Weber describes 'The Scientific Vocation' as a disciplined pursuit of knowledge in 1917(Weber, Owen, and Strong 2004), and Merton (1973) emphasises that strong norms within the professional community of scientists such as objectivity and scepticism guide their daily work. These ideas about the scientific job are still pervasive within scientific communities, despite the changing role of science in society.

While the 'entrepreneurial scientist' and the consequences of the '*marketization*' of the universities have been objects of study for some time (Etzkowitz 2003; Mirowski 2011; Hackett 2014) few have made studies of how the research organisations and the practising scientists are affected by demands to become more 'responsible'. One of the few examples is a recent study by McCarthy and Kelty (McCarthy and Kelty 2010), which shows that scientists translate the rather diffuse demands for 'responsibility' into specific, meaningful tasks so as to make them '*do-able*' in their daily work. In the case study by McCarthy and Kelty (2010), '*do-able responsibility*' becomes two practical tasks: The first task is to work to protect the discipline (nanotechnology) and the scientific community against critique, and the second task is then to protect society from the eventual negative implications of nanotechnology by doing more research.

This study takes its point of departure in the lack of focus on the practical aspects of doing responsibility. Similar to Mccarthy and Kelty (2010), it focuses on the specific practical work connected to being a 'responsible' scientist. This study will focus on the questions of what a scientist needs to do in order to be 'responsible' and how that can be related to the emergent government rationality that I described above – or other rationalities. Of particular interest is how the notion of responsibility affects broader concerns in the research organisation, such as the idea of the boundaries between the organisation and its environment, the relationship between the daily work and the profession, the scientists' ideas about science's role in society and, not least, the emotional well-being of those working in science.

By stating that I look at the connections between rationalities and 'daily work', I also endeavour to emphasise that the angle of this study is on science as a type of *work*, that is, a job. This is in contrast to many recent studies of science, which have focused on the *practice* of fact-making (e.g. Latour 1987; Cetina 2009; Pickering 1992) With the term 'work', I indicate that there are certain expectations of science that extend beyond the scientists' abilities to produce facts and technologies that come from it being a job. There are expectations of the pace and quality of the scientists' work and of the ways by which they get rewarded for their endeavours. There are expectations of science's obligations and of the scientists' ideas about careers and advancements. On the other hand, I also emphasise that the assemblage of practices is not that special. While the expectations may look different, depending on the profession, we all have expectations of how jobs such as nursing, school teaching or accounting should ideally be conducted. In this dissertation, not only do I look at the scientific endeavours as a type of work, but I also look at a special part of that work; namely, 'Public Science'. This is because I believe that there are some special expectations of public science that are not present in corporate science, even though many argue that these terms do not necessarily make sense anymore as the two domains have become increasingly entangled. These preliminary excursions lead me to the following research question:

1.2 RESEARCH QUESTION:

How is responsible science conducted and justified by public scientists – and what are the consequences of these responsibilities in their daily work?

1.2.1 CLARIFICATIONS

The term '*responsible science'*, refers to the scientists' own ideas about what it means to be responsible at work[4]. The terms '*conducted'* and '*justified*' indicate that I look at the connections between the scientists' daily work practices and their general ideas of science's role in society. The term '*consequences*' indicates that I interested in the effects of such responsibilities on the organisation of research and the scientists' wellbeing. The study is based on a document analysis and an ethnographic study of two laboratories working in the field of Synthetic Biology. See the chapters Methods and Methodological Considerations for further details.

[4] I use this term in two ways: To describe the scientists' own ideas about what is 'responsible' (as in the research question) and as an umbrella term for new science governance initiatives such as 'Responsible Innovation' and 'Socially robust science' (see Theory Section).

2. THEORY

In this chapter, I will describe the theoretical framework for this dissertation and, in doing so, describe how I situate the theme of 'Responsible Science' within a government theory framework. The reason for choosing such a framework is that the theme inherently addresses the question of how science should be conducted in the most optimal way and how it should be governed in order to do so. Therefore, I draw on Foucauldian studies of power and politics in order to understand the phenomena (e.g. Foucault 1993a/b) as well as science governance studies from the field of STS. Given that the main task of this study is to show how scientists actually conduct and justify 'Responsible Science' in their everyday work life, I will also include perspectives from the sociology of science that focus on how different institutional conditions affect everyday science (Lynch 1997).

In this chapter, I will first address the current calls for 'Responsible Science'.[5] I argue that they share a concern for science's lack of democratic accountability and that they propose ways to introduce such accountability through dialogue and participation. I argue that the recent calls can be seen as a shift in the hitherto ways of governing science, an argument that I support with examples of recent developments in the government of science in the USA, the UK and Denmark. Following that, I examine the demands for responsibility in a historical context, demonstrating that the theme of 'responsibility' as it is articulated now relies on a long history of debate regarding the role of science in society. Finally, I consider the calls in relation to broader developments in how the public sector should be governed and show the similarities between the calls for 'Responsible Science' and a range of other demands for accountability in public service in general. In doing so, I also underline the perspective that the

[5] As I show on this page, these demands have many different names. Since I consider them as representing roughly the same normative stance on how science should be conducted because, the conduct they advocate and the justifications for it are similar, I have not chosen to address one of them specifically throughout this study. Instead, I refer to these new demands for more responsibility in science as 'Responsible Science'. I also use this term, when I refer to ideas about responsibility among the scientists in the ethnographic field work.

development of science's role in society is intimately interlinked with changes in ideas about the most optimal way to govern the public sector in general.

2.1 WHAT IS 'RESPONSIBLE SCIENCE'?

Within the last couple of decades, a range of new concepts that all propose how science should be done 'more responsibly' have emerged within the field of science governance – including various places in science politics across Europe and the USA. Terms such as '*Responsible Innovation*' (Owen et al. 2013) '*Anticipatory Governance*' (Guston and Sarewitz 2002), '*socially robust science'* (Nowotny, Scott, and Gibbons 2001) and '*upstream public engagement*' (Wilsdon and Willis 2004) have gained momentum within science government literature.[6] In this project, these calls are seen as attempts to steer public science in certain directions. They are seen as representing and articulating a specific '*rationality'* (Gordon 1991; Miller and Rose 1990a, 1-31) that is, they have similar views on what is problematic about the way science is conducted in its current incarnation and how science and science government can be organised differently in order to be more 'responsible'. As such, the calls for 'Responsible Science' are interpreted within a Foucauldian understanding of power and government, an understanding that I will unfold thoroughly in chapter 5. Generally speaking, the calls share the view that there is a need for more external governing of science as a vital supplement to the internal professional ethics that also guide scientific conduct (Braun et al. 2010; Jasanoff 2011). Moreover, they agree that there is a need to enhance scientists' abilities to reflect upon the 'outcomes' of their inventions – that is, the social,

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[6] Throughout this dissertation, I use the Foucauldian notion of 'government' as a broad term for the steering of a specific area. In line with Foucault, I do not consider 'government' something exclusively performed by elected officials, but rather as the general act of 'structuring a possible field of action' (Foucault 1993a: 237). Within public administration theory, some distinguish between 'government' as the exercising of parliamentary rule, while 'governance' is considered a specific government paradigm where power is diffused from central administrations to 'networks' of heterogeneous actors, and the conduct of government is changed from formal rule to 'soft' forms of steering (Rhodes 1996; Torfing & Sørensen 2005). The literature on science politics is usually referred to as 'science governance literature' and I therefore keep that notion.

environmental and ethical consequences of introducing scientific knowledge and technologies into society. This enhanced reflexivity is supposed to come about by different means such as increasing the use of foresight studies, more direct public engagement and interdisciplinary collaborations between scientists and social scientists, with the latter being able to include broader reflections on the outcomes (Macnaghten, Kearnes, and Wynne 2005). The overall aim of this rationality is to achieve the dual goal of increased democracy through public participation and producing innovative technologies through publically legitimated knowledge (Kearnes and Rip 2009). Irwin (2006) even talks about '*the new governance of science*'; thereby indicating that we are currently witnessing a significant shift in the way science is steered. The theme of 'Responsible Science' is thus closely related to the question of what science's role in society should be and what the central tasks of scientists are. Public science is, furthermore, part of public service and thus also entangled in questions about eventual special responsibilities and the general role of the public sector in society.

While the specific features of 'Responsible Science' may be new, the idea that scientists *have* responsibilities is not. Science and scientists' responsibilities have been discussed at least since Weber gave his lecture 'Science as a Vocation' in 1917, where he described an 'Americanisation' of the German university and increased pressures on the scientific profession as free to pursuit a search for '*clarity*' (Weber, Owen, and Strong 2004: 12) Merton famously described the CUDOS: 'Communalism', 'Universalism', 'Disinterestedness' and 'Organised Scepticism' as institutional norms or ideals that guide the scientific profession (Merton 1973). While they have been criticised for being far away from the everyday work of scientists, many working scientists still consider them as ideals, they should try to live up to. The responsibilities of scientists were also a significant theme in the aftermath of the Second World War. Scientific knowledge had played a large role in securing the Allies' victory via the development of the atom bomb. But this invention also basically gave rise to the questioning of science's purpose and values: Was the atom bomb a way of 'helping mankind' or was it the final proof of the profession's irresponsibility? While this was being debated, science, especially basic natural science, enjoyed a 'golden age'. With the USA as frontrunners, the rationale was that if scientists were left alone with plenty of resources, they would eventually lay '*the golden egg*': A brilliant invention that could be applied in industry (Shapin 2009: 72). At the time, this was the foremost responsibility of science. Meanwhile, scientific innovation was continuously being questioned in relation to new technologies that were seen as controversial such as the discovery of the double helix structure of DNA in the fifties. This discovery led to the 'Asilomar Conferences' in the seventies, where the 'responsibility' of scientists and their ability to govern themselves was again on the agenda in relation to the growing field of biotechnology. There have been different interpretations of the conferences; some consider them a defensive strategy enabling the scientists to defend their autonomy, and others consider them a sign of the shift from internal to external governance (Braun et al. 2010). Around the same time, '*Entrepreneurial Science*' (Etzkowitz 2003) and the conception of public researchers and universities as active profit-seekers also gained momentum, while the public sector in the late seventies began a range of reforms that with time also affected the universities by forcing them to compete for funding and shift to the project-based organisation of research (Boden and Nedeva : 57)Public debates about gene research and *in vitro* fertilisation also raised the question of whether science had gone too far in meddling with nature, not least in relation to the birth of Louise Brown in 1978, who was the first child conceived by *in vitro* fertilisation.

Based on this short overview of some of the older important events in relation to the study of public science and its responsibilities, it is clear that the purpose and ethics of science have been up for debate several times and the institutional conditions for conducting science has also changed since the beginning of the twentieth century. The latest shift is thus the renewed calls for 'Responsible Science', all of which have surfaced within the last couple of decades. In the next section, I will describe events that have led to this recent '*problematization*' (Foucault 1993a 229) of the government of science, and I will examine the most recent shifts in science government based on cases from the UK, the USA, the EU and Denmark, thereby also connecting the historical description above with newer developments.

2.1.1 MANDATES FOR NEW FORMS OF GOVERNMENT

The recent calls for 'Responsible Science', as described in the intro to the previous section, share their justifications for more 'responsibility' in an effort to avoid the '*un-intended side effects*' (Beck 1992) of emergent technologies as well as making the area of science subject to (increased) democratic rule by introducing deliberative forms of engagement (Nahuis and Van Lente 2008). Similar calls for more 'responsibility' have also been made in several national political arenas. In relation to these calls, it can be argued that they are fostered as a response to developments and events from the eighties and nineties that have problematized both the role of science in society and how science should be governed. These events have given mandates for new ways of governing science. In what follows, I will describe the developments in the UK, the USA, the EU and Denmark in order to show the similarities in the solutions they propose to the 'problematization' of earlier ways of governing science. By doing this, I argue that we are witnessing a general shift in science government discourse across the Western world; a shift that entails a focus on both participation as an strong value in science government and the goal of adapting to – and succeeding in – the '*global knowledge economy*' (Gorm Hansen 2011).

In the case of the UK, two events are considered central in explaining the shift in science government that began unfolding (and still is) since the turn of the millennium. The first event is the 'Mad Cow Disease (BSE)' case, which occurred at the beginning of the nineties, and the second event is 'GMO Crisis', which occurred in the end of the nineties. In the first case, public authorities and scientists downplayed the risk of human infection from contaminated beef, meanwhile several people subsequently became very ill (Irwin 2006: 304). This engendered serious mistrust in both public authorities and science. This mistrust was further fuelled by an unpredicted public revolt against GM foods which was covered in detail by the British media and which shocked British and European legislators (Shaw 2002: 274)Analysing the content of new British science government initiatives, Irwin (2006) concludes that many 'old' discourses about science-society relations are still being performed: In many ways, science is still seen as an objective expertise that is to be used directly by formal government. However, fresh discourses about science's role in government decisions have also spurred new discourses. They include a larger focus on the citizens' role in science policy. There are intentions to trust the citizens' concerns about science and expertise as legitimate and well grounded. Specific realpolitik initiatives such as '*public participation exercises*', '*consumer research*' and '*openness to public critique*' have been initiated (Irwin 2006: 300). The hope is that such initiatives will establish public confidence in the quality and direction of government decisions, where scientific advice has earlier been the only source of legitimation (Irwin 2006: 300; Horst and Irwin 2010). This shift in the mode of governing can, based on these examples, thus be seen as a move away from a technocratic mode of governing where science and the state are the main actors, toward a form of government where direct participation is considered valuable and where accountability is a means to secure legitimacy. While the shift can be seen as an attempt to avoid incidents such as the 'Mad Cow Disease' case and the GMO crisis, Irwin also suggests that another goal is to establish the UK as a '*powerhouse for innovation*' in the global knowledge economy and, to that end, the focus on participation and heterogeneity is supposed to foster an innovative environment (Irwin 2006: 308). Other authors describing the development of science government in Britain agree on Irwin's analysis of the shifts that have occurred in the ways of governing (Macnaghten, Kearnes, and Wynne 2005; Kearnes et al. 2006), Kearnes et al. (2006) add to Irwin's analysis by suggesting that the government discourse has shifted from being dominated by a purely technical '*risk discourse*' to one that takes into account wider social concerns such as ethics, the environment and general values (Kearnes et al. 2006: 17). Along with Wilsdon and Willis (2004), they also suggest that the shift in science government should not only be understood as a narrow response to the BSE case and the GMO crisis, but also that science government can be seen as the beginning of a tendency for more inclusive and direct forms of government with the potential to revitalise the democratic spirit in the UK in the face of general declining trust and declining participation (Macnaghten, Kearnes & Wynne 2005: 270; Wilsdon, Willis 2004: 14). In that way, the 'new governance of science' is not only considered a shift in the role that science plays in general government, but also a leverage for more democracy via participation and, as Irwin suggests, a way to establish innovation on a national scale.

In the case of the USA, the shifts in science governance are best viewed in the light of '[…] *a well-established culture of technological optimism, but also against a robust tradition of scientific activism and open debate about the social* *impacts of science and technology'* (Jasanoff 2011: 45). While the qualities and side effects of scientific endeavours have thus been up for debate by scientists themselves (notably in relation to the development of the atom bomb and the discovery of the double helix structure of the genome), Macnarthten, Kearnes and Wynne (2005) argue that the increasingly large public investments in science such as the 'Human Genome Project' (from 1990) and the national venture into nanotechnology that began in 2000 needed public support. At the time, the government was in need of public support for these huge investments and therefore a move toward more deliberative forms of science government (ibid.: 275).

The 'Human Genome Project' was the first public science investment where a proportion of the investment (3 %) was set aside for research into the 'Ethical, Legal and Social Implications' (ELSI) of the accompanying scientific pursuits. Furthermore, an ELSI working group had the responsibility of providing the general (but not very defined) 'oversight' of the entire project (Jasanoff 2011: 178). Subsequently, the ELSI programme has been criticised for being an inefficient government tool, as the huge budget for research concerning the implications for society was seldom transformed into policy initiatives with the goal of limiting negative side effects (Fisher 2005: 323). The next big federally funded investment in science, nanotechnology in 2000, was therefore accompanied by a new set of initiatives that not only focused on the ethical and social aspects of new technologies, but also aimed at establishing a link between the government of new technologies and the research into them (Fisher 2005: 324). In their current form, the initiatives are therefore focused on public participation processes in relation to the development of nanotechnology and they also include foresight studies and other exercises that have the following goals:

‘[…] [make] *scientific cultures more self-aware of their own taken-for-granted expectations, visions, and imaginations of the ultimate ends of knowledge, and rendering these more articulated, and thus more socially accountable and resilient.’* (Macnaghten, Kearnes & Wynne 2005: 278)

While the *motivations* for changing science government in the UK are thus different from the USA, the solutions addressing public scepticism about scientific expertise (in the UK) as well as addressing the presumed lack of public support for huge investments in science (in the USA) appear strikingly similar: In both countries more direct public participation in the government processes surrounding science are instated. A technocratic way of governing where science and the state are seen as the most legitimate actors has been problematized and more deliberative forms of governance where the public plays a larger role have been proposed.

The phenomenon of proposing public participation in science government as the solution to various problems for science and government can also be observed in the case of the European Union. Waterton and Wynne (1996) show that, in the past, environmental science has been used as a way to foster cultural integration across European borders, as science was considered ‘neutral’ and ‘objective’ and thus something that could unite different European peoples who were otherwise considered to be very divided due to national interests and national cultures. The project was not a huge success. According to Waterton and Wynne (1996), integration around a common European environmental policy failed because the integration of different groups of people demands a sense of shared values more than a commitment to common, objective facts.

Recently, the approach to science has changed. The European Union has focused on 'Responsible Innovation' in both science- and innovation policies, which entails the double aim of achieving economic growth and the development of the EU in both a democratic and sustainable way (Von Schomberg 2012). Currently, the European Commission is debating whether reflections about how to handle ethical problems, strategies for public science communication and reflections about how to handle social aspects of scientific conduct should be a mandatory part of the grant applications in the next European framework for research (Horizon 2020). These demands are additions to other steering strategies in relation to science such as codes of conduct and the precautionary principle (Owen, Macnaghten, and Stilgoe 2012).

Owen, Macnaghten & Stilgoe (2012) suggest that the most important mandate for the call for 'Responsible Innovation' is the weak European economy in the aftermath of the 2008 financial crisis. The EU sees 'Responsible Innovation' as an important building block in restoring the European economy by uniting business, academia and civil society in innovation processes (: 753). But on top of that motivation, they argue that, philanthropically and philosophically, aims also play a role, even though the justifications for these remain '*unclear*'. However, they may be linked with the aforementioned 'GMO crisis' and other public upheavals about emergent technologies (Owen, Macnaghten & Stilgoe 2012: 756). The EU, and especially the European Commission, seem to be keen on having science work toward both socially acceptable and publicly desirable ends in the hope that this will enhance the European economy and distinguish Europe from its competitors in the global knowledge economy (Owen, Macnaghten & Stilgoe 2012: 753).

Much like the descriptions of the UK and the USA, it seems that the EU is trying to achieve several goals by rendering science (and innovation) more 'responsible'; in this case, this seems to mean being more responsive to the public's wishes. In doing so, the hope is that the EU will overcome its current economic crisis as well as establish responsibility as a signature for its innovation and thereby create a niche that sets it apart in comparison to its global competitors.

While similar solutions to seemingly different problems can be observed in the three previously described regions, Denmark stands out with a different development: It could almost be argued that the development is the 'opposite' to that of the other regions; that is, from participatory forms of government to technocratic ones (Mejlgaard 2009). Denmark has had a long tradition for public engagement in technology development. This culture goes back to a huge social movement that arose in the middle of the 19th century. As part of that social movement, education became a way to empower laypeople to partake in society rather than further specialise the upper classes (Andersen and Jæger 1999). Furthermore, a discourse of appreciation and respect for laypeople's knowledge forms and their right to partake in important decisions was also a part of this movement.[7] In relation to science government, this specific culture has been institutionalised in organs such as the Ethical Council (from 1987)[8] and the Danish Board of Technology (from 1995), which initiated participatory and public dialogue about new technologies (Horst 2008). Most famously, the Board have hosted the widely borrowed and widely discussed '*consensus conferences*', which invited laypeople to develop policy recommendations on new technologies based on their own deliberations and experts' input (Blok 2007).

[7] An important event related to that movement was the abolishment of the monarchy in favour of a constitutional monarchy and representative democracy in 1849.
[8] The Ethical Council was a response to increasing controversies regarding medical, reproductive technologies and now advises the Danish government about ethical concerns in relation to gene- and biotechnology.

But as Horst (2012a) illustrates, the government discourse about public participation in science has recently changed in Denmark. The Danish Board of Technology lost its support from the fiscal budget in 2011. It is now an independent foundation that gets ad hoc support from national and international organs. But even before it lost its permanent public support, activities within the Board were declining. The last consensus conference was held in 2005, without funding from the Danish government. The Danish central government generally seems to have lost interest in democratic deliberations about science (Horst 2012a; Mejlgaard and Stares 2010).

Simultaneously, with the diminishing focus on public participation at the national level, there has been more focus on Danish universities as loci for the growth of the Danish knowledge economy. The Danish university reform from 2003 actually made it mandatory to disseminate knowledge beyond the scientific community while, at the same time, introducing more competition for funding and competition between universities (Gorm-Hansen 2011). Combined, these initiatives have changed the science communication landscape in Denmark; whereas in the past it was embedded in a tradition of democratic participation, it is now focused on the branding of research groups, departments and universities. Moreover, the communication in the past was often fuelled by the individual scientist's interest or feeling of obligation, whereas now it is more professionalised and exercised with the help of professional communication units (Horst 2012a: 104).

So the science government discourse has recently shifted in Denmark as it has in the UK, the USA and the EU. But the shift in Denmark looks different. The science government discourse has changed from public participation being a central part of the government to a discourse whereby communication is tied to inter-organisational competition. This has, in turn, also meant that the former deliberative ideals have been replaced by ideals closer to that of a traditional technocratic steering of science without much incentive for public participation. But as Irwin (2006) demonstrated in relation to the British case, old and newer discourses blend, interact and exist side by side. While the Danish governments (that is, forms of steering) have shifted in their perspectives on the values of participatory governance, Denmark still hosts a fair amount of science engagement activities. But where they were, in the past, supported by the central government, they are now increasingly funded by local universities, private funds or the European Union.

Based on the descriptions of the four regions, it is possible to identify similarities between them. All of them have been through recent changes in science government discourse. While these changes may have been brought about for different reasons in each place, the changes in at least the UK, the USA and the EU have included an increased focus on the participation of laypeople in decision-making related to science. Denmark has had a rich tradition for deliberative science government but has recently re-organised it in such a way that science communication now forms part of the competition for funding, and the government of science has shifted to more technocratic forms – also as a means to enhance Denmark's advantages in the knowledge economy. While this shift is thus different from the others, they all seem to, among other aims, share a focus on science as a means of adapting to the global knowledge economy.

Kearnes and Rip (2009) argue for an emergent 'government landscape' of 'Responsible Science'. Based on their analysis of recent developments in science government in the Western world, they argue that good science government is increasingly conceived of as that, which does not constrain innovation and commercialisation, while different strategies (such as public participation and foresight studies) are invoked in order to reduce eventual negative outcomes. Furthermore, in this interpretation of 'good governance' lies the hope that the dual goal of innovation and democracy can be achieved at the same time (Kearnes, Rip 2009: 23) Their analysis seems fitting in relation to the developments that have occurred in the countries, as described here. While the balance between the goal of innovation and that of increased democracy do vary from country to country, both goals seem to be in place. Furthermore, it also seems as if there is a consensus about the means required to reach these goals: Public participation and further reflection upon outcomes from the scientists. It seems striking that all the countries share the focus on innovation and democracy *and* a focus on participatory processes and reflexivity as a means to get there.

In order to understand this relative homogeneity, I will now look at 'Responsible Science' in the light of shifts in broader government discourse on the most optimal way to govern the public sector as a whole.

2.1.2 RESPONSIBLE SCIENCE AND BROADER GOVERNMENT RATIONALITIES

In this section, I will argue that three government rationalities about the most optimal way to govern the public sector have influenced the content of the recent calls for 'Responsible Science'. By 'government rationality', I mean a shared set of ideas about the most optimal way to govern that has influenced public sector reforms and tangible ways of organising the public sector (Gordon

1991). These three rationalities are the '*Responsive Government*', which has gained momentum since the beginning of the nineties , '*New Public Management'* from the eighties and onwards and the more general influence of a diffuse '*neoliberal*' morality (Shamir 2008).

The rationality of the 'Responsive Government' developed in the USA during the first Clinton administration but has spread and is, for instance, a vital part of the OECD's recommendations for public sector reforms in its member states (OECD 1996). The general concern of this rationality is a lack of democratic accountability in public service (DeLeon 1997: 238) This is a problem that, according to the rationality, can lead to inefficient work on behalf of the public professionals and, hence, declining public trust in government and declining participation in democratic society (DeLeon 1997; Du Gay 2008). As a solution to this, several initiatives to ensure the accountability of public servants have been installed. These consist of formal auditing to ensure transparency. A range of different auditing methods to secure legitimacy has been installed in public service – in relation to this study it is mostly the public engagement exercises as a way of securing legitimacy that are interesting. Justesen and Skærbæk 2010 look at the '*performative effects*' of auditing in the public sector and conclude that they both affect the development of the auditées professional identity as well as their well-being at work – a point that is very relevant in relation to this study's findings. Furthermore, 'governance networks' comprising of several, heterogeneous actors (e.g. scientists, civil servants, members of the public and representatives of industry) have been introduced as capable negotiators of common solutions in a specific area, ensuring accountability throughout the process (Boden and Nedeva : 10). Importantly, the rationality changes the form of accountability in the public sector. In a traditional 'Weberian' public bureaucracy, the accountability is vertical and hierarchical; the civil servant is loyal to her superior and her government and her role is to fulfil the duties that are required of her in respect of having this particular professional role (du Gay 2008: 338). But with ‘Responsive Government’, this accountability shifts to being ensured not only by formal rule and professional ethics, but also through the process of public ‘dialogue’ between heterogeneous actors and direct participation. Many concrete initiatives such as ‘user-driven innovation’ (Jensen 2012) ‘participatory design’ (Kensing and Blomberg 1998), ‘participatory innovation’ (Buur and Matthews 2008), ‘Open Innovation’ (Gassmann 2006) and ‘Collaborative Innovation’ (Sørensen and Torfing 2011) have been developed within the public sector as a result of this rationality. They share a similar focus on the co-production of knowledge, service and commodities by set of heterogeneous actors and a belief in the transformative capacities of dialogue and participation.

It can be argued that the supposed ‘lack’ of accountability that is claimed by proponents of ‘Responsive Government’ is not so much due to the faults of the traditional bureaucracy as they are a result of an erosion of exactly that way of organising the public sector (du Gay 2009). This has happened by re-describing the job of public bureaucrats as a profession of enthusiastic ‘deliverers’ of government policy and ‘entrepreneurs’ of public projects and change rather than formalistic and impersonal protectors of the administration (du Gay 2009: 379). The demands for more direct public participation in science government resemble this call for ‘Responsive Government’. There seems to be a lack of trust in professional ethics and internal control being enough to secure ‘Responsible Science’. Therefore, accountability secured through public participation is proposed in order to ensure that public science both delivers what it promises and that the promises are considered legitimate in the eyes of the public.

Another influence on the demands of 'Responsible Science' comes from the rationality of '*New Public Management*' and the value of public service as '*entrepreneurial*' (du Gay 2008: 336). Since the end of the seventies, both the efficiency and efficacy of large states have been questioned, and the public sector across the Western world has gone through a range of reforms that introduced private-sector-like management to the public sector, including a general re-organisation focused on 'marketization', 'competition' and a commitment to 'consumer-contractor' relationships between the state and its various stakeholders (Boden and Nedeva: 48-49). While proponents of 'Responsible Science' do not explicitly seem to adhere to the ideas of the marketization of science (most of them are more occupied with the democratisation aspect), none of them directly contradict the demands for competition between research organisations or the marketization of research funding. Kearnes and Rip (2009) point to the fact that the methods of enhancing social responsibility in science are usually '*enabling commercialization*' rather than restricting it (Kearnes and Rip 2009: 23), as the foresight studies and participation processes are happening 'before the fact' and thus guide the final technology in a legitimate direction, where no restrictions on commercialisation are needed.

Furthermore, the science government changed that I have described above have as part of their goals the idea of 'Responsible Science' as a driver for the global 'knowledge economy'. 'Entrepreneurial science' – the phenomenon of science actively pursuing close ties with industry in order to commercialise its own inventions – has also been tied to New Public Management. Etzkowitz (2003) argues that entrepreneurial science was originally not necessarily linked to public policies, but rather to innovative individuals and universities and has increasingly been linked in such a manner as a way to increase national competitiveness in the globalised knowledge economy. This has led to an explosion in entrepreneurial activities at public universities (Etzkowitz 2003: 1, 140).[9] Based on these considerations, I argue that certain demands for being 'entrepreneurial' and working under competitive conditions have also inspired the demands of 'Responsible Science'.

The last influence that I will point to is that of a general 'neoliberal moral' (Shamir 2008: 2), which is increasingly pervading both the public and private sectors. Shamir (2008) describes tendencies to marketize new areas of society by introducing competition and markets into the public sector – as also described in relation to 'New Public Management'. In his view, there is a dual motion whereby areas governed by 'the market' are growing, while a specific 'moral' that was formerly associated with the public sphere is also growing in new domains. His primary example is CSR (Shamir 2008: 2). Using the notion of '*responsibilization*', Shamir points to the fact that while 'governance' is dispersed among a range of competing actors in an imaginary 'market', these actors in turn have to assume 'socio-moral' tasks, which have normally been associated with the public sphere (Shamir 2008: 14). But he is sceptical about the democratic potentials in this 'responsibilization' because he sees the market logic as 'winning' the discourse, thereby framing social issues as business opportunities and leaving morality without its usual transcendental and emancipatory abilities (Shamir 2008: 14). In relation to the rationality on 'Responsible Science', I argue that the scientists who are the subjects of this dissertation are also asked to assume 'socio-moral' tasks, such as reflection upon outcomes and civil engagement projects, while at the same time are also

[9] Etzkowitz (2002) also describes this as the 'second academic revolution' (the first starting at the beginning of the 20th century) and suggests the 'Triple Helix Structure' of government-industry-universities as an overarching methodological tool to analyse the current ways of organising science and innovation.

competing on a market – both as sellers of innovations and competing for funding for new projects.

I have described these three rationalities because I want to point to the fact that the developments in public science are not exclusively tied to the public scientist's profession. It is part of a general development in the way that the entire public sector is governed. I find this aspect important because proponents of 'Responsible Science' do not seem to connect their calls for 'social responsibility' with broader tendencies in public administration. The justification for these calls is often a worry that the traditional ethos of scientists (as, for instance, described by Merton 1973) – including such virtues as 'objectivity' and 'disinterestedness' – have unintentionally led to many of the catastrophes that science is partly responsible for, such as devastating environmental damage and lethal weapons (e.g.Beckwith and Geller 1997; Wynne 1994). It remains an open question as to whether public participation and oversight will change that. But it is clear that proponents of 'Responsible Science', both in the science government texts and in the national cases, are inspired by contemporary ideas about the most optimal way to govern the public sector in their suggestions of how to change the way science is organised and conducted. In this dissertation, the three rationalities are therefore used as an inspiration for interpreting the scientists' daily responsibilities. As public science is closely entangled with the rest of the public sector, I see these broad rationalities as theoretical resources that I can use to make sense of the scientists' ideas about their daily responsibilities.

2.2 SCIENCE AS PRACTICE AND SCIENCE AS AN INSTITUTION

In this section, I will examine another strand of literature, namely, the sociology of science, which is used both as an inspiration for the project and as a strand of literature that I refer to throughout the analytical chapters in an iterative process. I have divided the literature into two main bodies: The first studies science as a profession and as work and has recently focused on micro-sociological studies of practice, and the second studies science as a specific institution in society and looks at this institution's influence and role in society as a whole – here the view is more macro-oriented. Both views are important in answering the question of how 'responsibility' is conducted and justified on an everyday basis. Moreover, the sociology of science also provides more in-depth perspectives on the scientific profession and the development of science historically than science government literature and the public administration literature.

The previous section provided an overview of the development of government demands for 'Responsible Science'. Most of the approaches share the view that the public has the capacity to have a say in deciding the direction of science. Furthermore, they share the view that responsible innovation is something that comes out of deliberative processes that involve heterogeneous actors and that the state is trying to reach the dual goal of more innovation and more democratic engagement at the same time. In this section, I will establish how this dissertation takes its own particular stance in relation to these understandings and the role of social responsibility in science and situate it more within the broader literature on the relationships between science and society.

2.2.1 SCIENCE AS PRACTICE

Over the last century, several authors have given their own account of the relationships between science and society. The earliest body of literature, '*the* *first wave of science studies*' (Collins 2014), is concerned with how norms guide scientific conduct. These accounts have focused on the question of what the main purpose of scientific inquiry is and how the scientific profession relates to other areas of society. The most famous of these is perhaps Max Weber's essay 'Science as a Vocation' (Weber, Owen, and Strong 2004) in which the vocation is understood one where scientist pursues questions of interest in a disinterested and open-ended manner. This is combined with teaching, where the same ethos is taught to new generations of scholars (Weber, Owen, and Strong 2004: 6). Important for Weber's work in general is his insistence on '*different orders of life*'. He argues that different rationalities are being performed in different areas of society (du Gay 2009). Morals and norms from one part of society cannot – and indeed should not – be reduced by morals and norms that transcend all of society (Du Gay 2005). Weber thus demonstrates that the scientific profession holds a special ethos that guides those persons (or *personae*) who are familiar with these specific areas of social life. But this ethos cannot and should not be transferred to other aspects of the social.

Merton has later elaborated on these qualities of the scientific profession, most famously in his construction of the CUDOS norms, which are based on interviews with scientists and which he presents as guiding scientific practice. CUDOS stands for '*Communalism*', '*Universalism*', '*Disinterestedness*' and '*Organized Scepticism*' (Merton 1973) and conceives of the scientist's job as comprising strong internal norms that are continuously and consciously taught to newcomers in the field through apprenticeship. These norms have the function of keeping society's values and interests out of scientific conduct, thereby rendering knowledge objective and thus more helpful for society's actors. Merton curiously modelled his descriptions of the scientific profession on Weber's account of the bureaucrat in his doctoral dissertation (Merton 1970;

Lynch 1997) and the two forms of professional ethos do share similarities. In relation to this dissertation, this raises the dual question of how these two kinds of professional ethos overlap and whether a particular form of ‘public scientist’, who works in public society and for the state, can be separated from other forms of scientist, for instance, those working for industry (see 9.2.2 for further analysis). Furthermore, it raises the question of how public scientists at present relate to public demands for further democratisation and public participation in deciding the direction of science.

This ‘first wave of science studies’ was followed by a second wave that radically criticised the focus on norms and especially the work of Merton (Collins 2013). Rather than focusing on the role of *norms* guiding the scientific *profession*, they studied scientific *practice* from a constructivist perspective and especially the practical construction of ‘*facts*’ (Latour and Woolgar 1979; Lynch 1985; e.g. Pinch and Bijker 1987). In each their own way, the texts from the second wave show how values, cultures, interests, pragmatic decisions, organisations and non-human interventions shape both the construction of facts and the construction of the scientific work (Latour 1987; Cetina 2009; Traweek 2009; e.g. Shapin, Schaffer, and Hobbes 1985). So the primary question posed by Weber and Merton addressed how the profession of science was governing itself as a particular, autonomous part of society. For the second wave of science studies, the primary question addresses how facts and society are co-constructed through open-ended processes (also) occurring in laboratories. Some of these studies (Traweek 2009) further examine how these practices in turn shape the overall work of scientists.

2.2.2 INSTITUTIONAL PERSPECTIVES ON SCIENCE

While one body of literature can thus broadly be conceptualised as a sociology of practice, with science as its specific object of study, another can be seen as an institutional sociology that studies the relationship between science as an institution and the rest of society. Two main approaches to this institutional theme can be detected. Moreover, one body of literature argues that science has developed from a closed institution with few relations to society, to an open institution where the boundaries between science and the rest of society are gradually dissolving. Another body argues that science has never been separated from society as such; nevertheless, the whole of the institution has changed historically, with different obligations and relationships to other parts of society emerging.

One body, where the work of Helga Nowotny, Peter Scott and Michael Gibbons has been especially influential (Hessels and Van Lente 2008) argues that science was once an institution with its own rules and norms, which separated it from the rest of society (e.g. Gibbons and others 1994; Nowotny, Scott, and Gibbons 2001; Nowotny, Scott, and Gibbons 2005; Scott 2003). Gradually, this has changed due to a range of different circumstances, notably, that of the evolving 'knowledge economy', where public science has received a special obligation as providers of an educated workforce, as drivers of the knowledge economy and as deliverers of commercial technologies. Their main argument is that science (and society) began with '*Mode 1*', where science was conducted an academic pursuit within strict disciplinary boundaries and with a one-way communication of facts from science to society, which then interpreted and used them (Nowotny, Scott, and Gibbons 2001: 15f). This has now changed to '*Mode 2*', where science is increasingly embedded in society: It is conducted in various places, including outside academia; its direction and qualities are debated in public spheres, '*the agora*', and these changes have in turn meant that science is increasingly contextualised and context-sensitive, that is, always produced in '*a context of application*' (Nowotny, Scott, and Gibbons 2001: 23, 69). This changes even the core epistemological beliefs of science. Where the main concern of scientists in Mode 1 was whether their theories were 'true', that is, understood as corresponding to the phenomenon they described, 'true' now means that they both need to correspond to reality *and* also be '*socially robust*', that is, living up to common, legitimate ideas about society's general development (Scott 2003: 83). The theory of Mode 1 and Mode 2 science has had a great influence on European science politics in the last decade and has, for instance, inspired the composition of the latest frameworks for research (Hessels and Van Lente 2008; Gorm Hansen 2011). Some have even called the theory a '*self-fulfilling prophecy*' (Boden and Nedeva : 11). As it is considered a valid description of contemporary society, policy-makers and funders have increased their focus on '*applicable science*', the importance of '*social robustness*' and the necessity of conducting '*interdisciplinary research*', thereby actively creating Mode 2.

In contrast to this understanding, others argue that science has always been 'part of' society; that is, the demarcation between the institution of science and that of the rest of society is a constructed one, and it takes particular forms in specific periods of time. However, as science and society continuously co-construct each other, they are never separated (see also Jasanoff 2011; Shapin 2009). While science is a specific institution in society where special norms and practices can be studied, it does not make any sense to say that it is 'outside' the rest of society. That being said, the institution of science's role(s) in society has changed historically. Various political and scientific developments have pushed both science and society in new directions – they have co-developed in various ways. Notable historical examples of these kinds of changes are the consequences of 'The Manhattan Project', that is, the development of the atom bomb during WW2. Not only did this invention cause scientists to re-assess the enormity of their impact on society, it also led them to debate the kinds of obligations they had as a profession. Furthermore, the project also heavily influenced the organisation of public science in America after WW2, with the Department of Defence taking a significant role in driving scientific innovation forward in various areas (Shapin 2009: 68). More recent changes can be seen in the aforementioned calls for 'Responsible Science' and the tighter connections between science and industry. The '*entrepreneurial scientist*' has become a powerful actor, who has the means to pursue big, unsolved questions and commercialise them faster than ever before (Rabinow 1996) These turns in the co-production of science and society happen continuously, spurred by different occasions and interpreted differently by various actors. The main question for this body of literature is therefore not whether science has become more or less 'open' in relation to society; rather, the authors argue that it is important to pursue the institutional changes empirically and study their effects.

In his work '*The Scientific Life – a history of a late-modern vocation*', Shapin (2008) suggests that corporate science has taken over many of the features previously associated with academic science, such as having time to do science, an emphasis on curiosity and undertaking open-ended investigations. In contrast, academia is struggling with tight budgets, large administrative burdens, temporary employment and a general lack of funding (Shapin 2009: 327ff). In my interpretation, his assessment of this development seems to be that since science and society have always been interdependent, we should not worry much about the recent developments in the direction of a more marketized science. The scientific profession, which is the focus of this study, will (also) survive this increasing commercialisation and marketization; nevertheless, it will also be transformed by this development.

Other authors have criticised this point of view for being too optimistic on behalf of the scientific profession and the general conditions needed for producing scientific knowledge (Herrnstein Smith 2009; Lave, Mirowski, and Randalls 2010). They argue that just because science-society relationships and the conditions for public scientists are in constant flux does not mean that we should not look into these specific conditions and also assess them and their consequences from various critical perspectives. They agree that the working conditions for public scientists at present are indeed marked by the features that Shapin (2009) describes, but they believe he is taking this development too lightly. The pressure on public science is grave for both the scientists who have to endure the conditions of 'a late-modern vocation' as well as for society itself, which is losing not only opportunities for innovation, but also the democratic potential of having the bulk of its knowledge production within the public realm.

In line with the second body of literature, this dissertation is based on the premise that science-society relations are co-productive and changing, depending on the different institutional contexts within which they operate. At the same time, and in agreement with Shapin's (2009) critics, this project is primarily occupied with looking at how a specific institutional condition – that of 'Responsible Science', as described above – is actually influencing daily work practices in public laboratories. It is therefore necessary to undertake more descriptive, and indeed appraising, studies of the effects of 'Responsible Science' on scientific conduct. Moreover, it is especially important to undertake studies of the fate and role of 'public science', that is, science conducted at public universities with some government funding. It seems that the question of how scientists should perform their job is changing along with the changing demands for their societal engagement.

In essence, I argue that the demand of assuming ‘responsibility’ and doing ‘Responsible Science’ raises the question of how science should relate to the rest of society and its institutions (democracy, industry, the state and the public). In doing so, the demand also questions the ways that daily work in public science is being done at present. In that way, both the sociology of science that focuses on the *work* and the sociology of science that focuses on the institutions become relevant for this dissertation. The former is relevant because of the focus on how science is conducted on an everyday basis and how norms (going back to the ‘first wave of science studies’) play a role in scientific conduct, and because it also acknowledges that these norms are creatively negotiated in everyday work life with different performative effects as a consequence (see chapter 3 for a description of the dissertation’s methodology). The latter is relevant, as I emphasise that the historically described, different institutional relationships between science and society are being performed in daily work as different normative positions in relation to science’s obligations. With that, I also make the argument that it is possible to locate and study ‘*patterns*’ in the way the social is performed (Foucault 1993b 229; Law 1994: 107) and that these ‘patterns’ can both be viewed as stable long-lasting, historical institutions and as rather fragile arrangements that need to be sustained through careful daily conduct in order not to fall apart; it is a question of perspective. This argument will be explained more thoroughly in the next chapter, which looks at this dissertation’s reliance on Michel Foucault and John Law as its main methodological inspiration.

3. METHODOLOGICAL CONSIDERATIONS

'My argument is that empiricism, the assumption that there is a reality about which we can all agree is an effect, rather than a cause. But it is an effect. And, since it is an effect, it is one to which I will not only contribute […] but also seek to take advantage.' (Law 1994:54, original emphasis)

3.1 CONSTRUCTIVIST PERSPECTIVES

In the previous sections, I have discussed my research theme in relation to science government theory, public administration theory and the sociology of science. The following chapter outlines the methodological choices that have informed this project. As such, this chapter is intended to tie together the preceding chapter on theory and expand the presented logic of inquiry by discussing its potential in relation to the empirical material and my engagement in the field. This dissertation is situated within a '*constructivist*' perspective. By that, I emphasise that creation of organisation, roles and materiality is a collective process that takes place in networks of heterogeneous actors (Law 1994: 17). Here, I specify my two (constructivist) perspectives on the material: First, Foucauldian-inspired studies of government and 'government rationalities', and, second, studies of 'modes of orderings' (Law 1994) and micro-sociological studies of science. While I do not argue whether the two are directly commensurable, I do argue that they can be used at different levels of the analysis and that the choice of the two perspectives is, furthermore, not totally eclectic; that is, they can enter into dialogue as they share some basic assumptions. In this chapter, I will specify the concepts I use in the analyses, as well as their theoretical heritage, I have used to analyse my material. I will also discuss the limitations of each concept's explanatory power and the subsequent choice to use various approaches in different parts of this dissertation

The foundational assumption is that the theoretical perspectives, the empirical material and my personal interests have resulted in special 'stories' consisting of the material, which could have been different if the mixture had been of another kind (Tryggestad, Justesen, and Mouritsen 2013; Law 1994: 15). The research process thus involves the active construction of results in negotiation with (more or less) engaged participants, dominant discourses, a variety of theories and the methods at hand. This does not mean, however (as some of the kind, participating scientists from the labs might suspect), that the results are just arbitrary or subjective. In line with Law (1994), I argue that:

'"Real" stories are also modes of ordering. But stories too, tell of real histories, real actions and real people. They are a part of, a way of talking of, some of the ordering patterns in the recursive fields of the social.' (Law 1994: 71)

Here, Law argues that reality is an effect. 'Stories', both the ones I construct with the present text and the ones that I see and listen to while doing fieldwork, have effects; they push reality in certain directions. They alter the reality in which they are performed and are also altered with it. Their truth-value is thus not based on their ability to represent the real world as correctly as possible, but also their ability to alter the real world.

The following sections are devoted to explaining the main empirical focus of this dissertation, namely, the practicing scientists' own understandings of responsibility in their daily work. Thereafter, I conceptualise 'responsibility' within a Foucauldian government framework and present the concepts of 'government rationality' and 'political rationalities' (Miller and Rose 1990b; Gordon 1991) 'modes of ordering' (Law 1994), 'conduct' (e.g. Weber 1956) and 'justifications' (Boltanski and Thévenot 2006) as the 'tool' used to structure and analyse the material. As such, I do not use a very uniform framework for the analysis. Rather, I borrow concepts from different authors and use them to build a specific story about scientists and their responsibilities under current government conditions. Despite that, I will still argue that the methodological approach should not be considered as eclectic. The concepts all share a constructivist approach to the social. Furthermore, all of them share similar conceptions of the 'individual' as decentralised and underline the importance of looking at 'action' as a phenomenon dependent of local context, expectations to the situation at hand and the roles of the assembled 'actors' (for lack of a better term).

3.2 FINDING EMPIRICAL FOCUS

This study is part of a bigger grant under the headline 'Social Responsibility in Science'[10], which has as its foremost aim to develop empirical knowledge about what current demands for 'responsible science' mean for the conduct, management and governing of public science in Denmark, the UK and the USA. This dissertation is the culmination of studying how bench scientists make sense of demands for responsibility based on ethnographic studies in Denmark and the USA. It was determined from the beginning that three ethnographic case studies (also including the UK) would be too much for a single PhD project.

Within this framework, the empirical focus of this project developed from a curious paradox apparent in science government literature. Science government literature stands on the shoulders of micro-sociological studies of science that have forcefully demonstrated how science and society are co-constructed and

[10] The project is funded by the 'Det Frie Forskningsråd – Samfund og Erhverv' and managed by Professor and Head of Department Maja Horst. The project consists of a subproject A that studies the connections between government discourses and research management and this project, subproject B, that studies how bench scientists make sense of demands for responsibility.

that academia is far from being a distant ‘Ivory Tower’ with few relations to ‘society’ (Pinch and Bijker 1987; Latour 1987). Despite this heritage, the assumption of much science government literature in the field of ‘Responsible Science’ still seems to be that scientists need the appropriate education if they are to include ‘social’ values in their scientific conduct, as their profession is solely characterised by an instrumental rationality (Fisher 2007; Macnaghten, Kearnes, and Wynne 2005, 268-291; Guston and Sarewitz 2002). Furthermore, few reflections about the political aspects of the demands for ‘Responsible Science’ have been made within the field (for exceptions see Kearnes and Rip 2009; van Oudheusden 2014). This lack of emphasis on the political aspects of ‘Responsible Science’ is also reflected in the surprisingly few (albeit growing) studies of how ‘social responsibility’ is conducted in practice and the performative effects thereof (for exceptions, see Fortun and Fortun 2005; Parkhill et al. 2013; McCarthy and Kelty 2010). This dissertation adds to the body of knowledge that conceives of ‘Responsible Science’ as a political phenomenon and also considers this phenomenon an empirical object rather than a normative concept.

The curiosity surrounding the science government literature is further spurred by the development of the empirical studies. Here, the scientists often reacted with surprise and alienation in relation to the theme, as one of them pointed out during the pilot studies: ‘as I’m sure you will find out, nothing really responsible goes on here.’ Otherwise, they politely told me that being ‘responsible’ demanded too much work and bureaucracy and they did not have time for that. At the same time, however, they seemed rather ‘socially’ engaged: They participate in various ‘outreach’ activities; they are deeply concerned about global problems such as hunger and infectious diseases, which they try to find solutions to via research; and they are also engaged in various political projects that criticise big business and current government. Furthermore, they also reflect a lot upon their job, the (shifting) roles of science in society and the current political conditions in science. This contradiction of alienation, resistance and engagement further propelled my interest of understanding 'responsibility' as a multifaceted (political) and empirical phenomenon.

Therefore, the empirical interest gradually grew into a focus on the scientists' own understanding of 'responsibility' in relation to their jobs, without a pre-understanding of whether these responsibilities could be understood as 'social' or not. The object of research then concerns what it is exactly that is considered 'responsible' in scientific work, how this 'responsibility' is justified and how the scientists then do different forms of 'Responsible Science'.

3.3 CONCEPTUALISING THE OBJECT OF RESEARCH

The object of research is thus the scientists' own understandings of their responsibilities and how these are realised. This study is conducted in two arenas: As a document study of understandings of 'responsible conduct' as they are articulated in scientific journals and as an ethnographic study of the daily responsibilities in two synthetic biology laboratories in Denmark and the USA (more details about both arenas in the next chapter on Methods).

The objective of this section is to show the translations from empirical focus to coherent theoretical concept, making it possible to trace and study understandings of responsibility within the empirical material. This study is situated within the area of laboratory studies, which takes a particular interest in how specific institutional settings influence the practical, scientific work (Lynch 1997). The understandings are examined in two different arenas – laboratories and texts – and two different frameworks have inspired the studies. The document study has been carried out as a study of ‘political rationalities’ (Rose & Miller 1990). The idea of ‘political rationalities’ is inspired by Foucault’s understanding of power as a ‘*conduct of conduct and a management of possibilities*’ (Foucault 1993b: 138). The focus is on showing how different understandings of proper scientific conduct are articulated in scientific journals as a way of informing the readers about the ideal way of behaving as a scientist in the scientific world. Thereby, the rationalities attempt to manage the scientists’ possibilities for action. With this analysis, I also emphasise how the ideas of ‘Responsible Science’, as described in the theory section (2.1), are one among several competing ideas about responsibility. I also use the term ‘government rationality’ (Gordon 1991). The two concepts are closely related but the concept ’political rationalities’ is used to study how plural rationalities make a certain area of the social contested and open for change. ’Government rationalities’ are used to show stability; how a specific rationality has agency in an area and influence behaviour. I consider demands for ‘Responsible Science’ as a government rationality. But in chapter five, where I show how different ideas about responsibility compete, I call all of them ‘political rationalities’.

Foucault mainly studied overarching historical ‘epistemes’ in Western thought systems. Therefore, the concept of ‘political rationalities’ is well suited to studying the contestations articulations of responsible conduct as they appear in journal papers over fifty years. However, the concept is less suited to capturing the unstable and shifting negotiations over meaning and action as they take place over the course of a day in the two contemporary laboratories.

I therefore turn to John Law's concept 'modes of ordering' from his book 'Organizing Modernity' to frame the ethnographic laboratory studies. Using this framework, I also move from the very coherent understandings of responsible conduct in the texts to the more fuzzy and unstable understandings of daily responsibilities and how to realise them in an organisational context (see 3.3.5 for further description of 'organisation'). 'Organizing Modernity' is in my interpretation both an organisational ethnography of science management as well as a description of how large social institutions such as 'modernity' are continuously (re)created by local action. Similarly, I use the concept 'modes of ordering' (making it into 'modes of responsibility') for two things: First, to show how the scientists' perceptions of responsibility organise the way they go about their daily work in the laboratories; and, second, to show how different institutions related to the role of science in society are thereby also constructed and negotiated on an everyday basis. The two entry points for studying 'Responsible Science' are described in the following two sections.

3.3.1 FOUCAULDIAN GOVERNMENT STUDIES

The first perspective that is relevant for my general view of the subject of 'Responsible Science' is the late studies by Foucault, namely, his studies of rule in modern Western societies. Foucault describes a 'new' power form that radically expanded with sciences such as economics, statistics and the first biosciences at the beginning of the 18th century. This power form was different from the earlier 'sovereign' rule. He differentiates between the 'sovereign' state and 'government' (the new power form) as follows:

'*In contrast to sovereignty, government has as its purpose not to act of government itself, but the welfare of the population, the improvement of its condition, the increase of its wealth, longevity, health, etc.; and the means that* *the government uses to attain these ends are themselves all in some sense immanent to the population; it is the population itself, on which government will act either directly through large scale campaigns, or indirectly through techniques that will make possible, without the full awareness of people, the stimulation of birth rates, the directing of the flow of population into certain regions or activities, etc.*' (Foucault 2003a: 241)

'Government' should not be understood as the parliament or other formal actor. 'Government', which is different from sovereignty, is a specific mode of ruling that has the prosperity of the population and the healthiness of the individual as its primary goal. It does not come from a single identifiable origin, but rather grows out of complex networks of initiatives made by scientific, cultural, economic and social initiatives in the attempt to guide people and the population in certain directions rather than others (Foucault 2003a). The shaping of legitimate action thus comes from multiple sources and is taken up by the state itself, so, as Foucault writes, there is a 'governmentalization' of the state happening – one which is still in progress (Foucault 2003a: 244). 'Ruling' is not exclusively performed by the state, but rather always appears in a context where a network of formal rules, accountancy practices, expertise and available technologies make some forms of conduct more legitimate than others (Dean 2013: 36).

Stemming from these observations about the characteristics of Foucauldian understandings of power and rule also comes the realisation that norms play a significant role in guiding in specific ways what is legitimate and what is not in a given situation (Dean 2013: 35). From this also follows the idea that power is not about the direct articulation of what is 'allowed' and what is 'prohibited', but is rather about shaping the actual space in which it is possible to regard some forms of conduct as legitimate (and thereby something that is 'allowed') and others as illegitimate (and thus may be unimaginable, illegal or just ill regarded). Moreover, government decides which areas of life and society are subject to rule and which could be considered autonomous. In that way, this view of power also demonstrates how lines between that which is autonomous and that which is subject to rule are in themselves contingent and a product of political struggles. Following this understanding of power, I will now look at the concepts of 'problematizations' and what they mean in the context of 'government'.

Regarding the concept of 'Responsible Science', I consider it a specific attempt to administrate the area of science by guiding the conduct in specific directions. As established in the previous chapter, 'Responsible Science' is considered a solution to a range of policy problems. The ways to go about realising the ideals of 'Responsible Science' are through the interaction of a broad range of different actors – citizens, scientists, politicians, social researchers, etc. – who are supposed to collaborate to make the outcomes of science more democratic, equitable and sustainable (Owen et al. 2014). The 'Responsible Science's' inherent confidence in the transformative capacities of participatory democracy as a vital supplement to laws and directions emphasises the trust in norms as being capable of altering behaviour among scientists and measuring improvements.

From the perspective of 'Responsible Science' the institution of science as it includes a 'problem' of responsibility, as the outcomes are supposedly not living up to the expectations of the public's views and wishes. In a Foucauldian perspective, this shift can be seen as a '*problematization*' of the hitherto internal governance of science through professional norms as described by Weber and Merton. A problematization should be understood as the emergence of intensified discussions and propositions about the steering of a specific area of the social because the preceding ideas on how to govern have come under scrutiny (Foucault 2003b: 229-30). The governing of the institution of science has thus been fundamentally problematized and is, as such, being presented as something that calls for the altering of the norms, which are supposedly guiding the field for now. In stating this, it is important to underline that the problematization of the internal governance of science is not a new thing. As previously mentioned, the institution of science has been under scrutiny several times, for instance, in relation to the development of the atom bomb and the discovery and use of recombinant DNA, the GMO crisis and, recently, the financial crisis. All these incidents have formed part of the destabilisation of the public's trust in science's ability to govern itself responsibly as well as in the traditional laws and regulations that have failed to prevent these catastrophes. But as Owen et al. (2013) describe, the features of 'Responsible Science' are a fairly new way of describing a solution to the aforementioned problems, and a concept such as 'Responsible Innovation' has not been mentioned directly, at least in Brussels, before 2011. But the history of the overall, demand of responsibility is longer: '[t]*he last half-a century has forced a re-evaluation of this contract* [between science and society]' (Owen et al. 2013: 31).

'Responsible Science' is an idea that promises to combine the dual goal of growth and democracy and which also gives specific guidelines as to how this responsibility is going to come about, namely, through more participation, deliberation, reflection and anticipation (Owen et al. 2013: 38). In this dissertation, 'Responsible Science' is conceived of as a specific 'rationality' (Gordon 1991: Miller and Rose 1990). Elaborating on Foucault's work on power, Miller and Rose propose that one can use the concept of 'political rationality' to study the exercise of modern government by looking at the following:

'[…] *the changing discursive fields within which the exercise of power is conceptualized; the moral justifications for particular ways of exercising power by diverse authorities; notions of the appropriate forms, objects and limits of politics, and conceptions of the proper distribution of such tasks among secular, spiritual, military and familial sectors.'* (Rose & Miller 1992: 175)

In the analysis, I will use this concept in chapter 5, whereby I identify four different 'political rationalities' based on a document study of how ideas about responsibility are articulated in scientific journals. More info about the analytical strategy is provided in the chapter.

3.3.2 MODES OF ORDERING

'Modes of ordering' are patterns of local achievements of order that are made up of networks of material and non-material components, such as machines, talk, ideals, imaginations and space (Law 1994: 110). The orders are never complete or pure: You will, for instance, never find a pure form of 'bureaucracy' – not even in the French central administration. There will always be traces, resistance and other competing orders at play. Related to this point is that orders are recurrent, that is, they are open-ended processes that are self-generating:

'*The social is a set of processes, of transformations. These are moving, acting, interacting. They are generating themselves. Perhaps we can impute patterns in these movements. But here's the trick, the crucial and most difficult move that we need to make. We need to say that the patterns, the channels down which they flow, are not different in kind from whatever it is that are channeled by* *them…The social world is this remarkably emergent phenomenon: in its processes it shapes its own flows. Movement and the organization of movement are not different.*' (Law 1994: 15, *original emphasis)*

The 'modes of ordering' share some similarities to 'government rationalities' and 'political rationalities'. They are also productive in creating ordering effects and create differences between right and wrong, hierarchy, boundaries, etc., and Law also ascertains that the modes are inspired by Foucault's writings (e.g. Law 1994: 111). However, he also claims that his views on how modes of orderings interact and change in 'real' organisational life and his considerations of the role of materiality are more elaborated than those of Foucault (Law 1994: 22f), even though he later calls them 'mini-discourses' (Law 2009). The modes also share some similarities with Weber's different kinds of 'Lebenswelt' (life world), as each order generates its own irreducible ideals, justifications, rationales and, indeed, conducts. In that way, Law tries to strike a balance between descriptions that neither favour structure nor agency as the explanation for behaviour. He does this by emphasising the relative instability of orderings, while insisting that it is possible to impute patterns to the networks of the social. The crucial point is, though, that these patterns are not different from the actual actions that are considered parts of the pattern (Law 1994: 112, see also quote above). In that way, Law is also focused on the 'performative' aspects of the modes; that is, how they have the power to generate certain forms of effects, which in turn also alter the modes themselves.

All in all, 'government rationalities' and 'modes of ordering' are used to make scientists' understandings of 'Responsible Science' into an object of study. The former emphasises the political and regulative character of the different ideas about responsible conduct. It can be used as a guide to pinpoint the content, difference and demarcations between different ways of thinking about responsibility. The latter is more suited to looking at the practical efforts entailed in living up to the daily responsibilities associated with scientific worlds, while simultaneously constructing different understandings of science's role in society. In the next two sections, I will describe how I trace 'government rationalities' and 'modes of orderings' in my empirical material by looking for 'justifications' of 'conduct'.

3.3.3 CONDUCT

Given that I have framed the object of research as 'government rationalities' and 'modes of orderings', I have also chosen a way of understanding agency that at once recognises that there are patterns in the various ways we act, albeit with the understanding that these patterns are acquired and sustained through very specific processes of learning, mimicking and repetition. Thereby, our actions are not a mindless expression of an inevitable macro-structure (McFall, Du Gay, and Carter 2008: 29). The concept of 'conduct' has been of much interest for sociologists since the 'founding' fathers Emile Durkheim and Max Weber first wrote about it. The notion itself refers to the manner in which people behave in particular circumstances, occasions or contexts. The concept thus emphasises the roles that individuals assume in different circumstances, for instance, a policeman acts differently in a situation at work than he does in his spare time. There is an expectation of different forms of conduct, depending on different situations and, at the same time, different expectations of different and recognisable roles in society, such as the police, mothers, fathers, and bosses or – as is the case in this dissertation – scientists. In that way, studying conduct is both about paying attention to the specific acts of people in specific contexts and, at the same time, how these acts are learned and sustained through rituals, organisation and categorisations (McFall, Du Gay, and Carter 2008: 51).

With this understanding of conduct also follows the fact that the concept is not used to explain individual behaviour based on structural, 'macro-social' explanations, nor is too much faith put in the individual's ability to act freely and determine his or her own actions. 'Conduct' is used to assist the researcher in gaining insight into a third position on agency: One where agency is of a distributed nature. Rather than having the individual or a structure as the focus of study, certain forms of conduct are in focus, as well as the fact that these conducts are learned abilities and have developed into special capacities and deportments (Mcfall, du Gay and Carter 2008: 6). Agency is thus looked upon as something that constitutes people in different situations, rather than something the individual possesses and which is expressed in actions. Several authors that are central to this dissertation have been interested in studying conduct. Max Weber, both in his 'Vocation Lectures' on science and politics (Weber 2004) and his study of the 'The Protestant Ethic and the Spirit of Capitalism' (Weber 1956), shows how particular spheres of society (e.g. puritan Lutheran, the political and scientific professions) during specific periods of time use particular techniques and practices to connect the ideal behaviour – the norm – with an inner consciousness (du Gay 2000). In that way, he underlines how certain practices – such as that of hard labour and ascetic lifestyle among pietistic Lutherans – constitute and sustain specific ways of thinking about the meaning of life and consciousness, rather than the other way around. This basic view about the relations between practice and individuality is also the basis for this dissertation: Conduct is looked upon as recognisable patterns of practices, which appear in certain circumstances and spaces and which make up people and groups in particular contexts.

Foucault was also interested in conduct in relation to his studies of power and was in fact, in his later works, inspired by Weber, but he focused more explicitly on the elements of power inherent in the concept of conduct. In relation to modern ruling, which we touched upon above, he claimed that 'the exercise of power is a "conduct of conducts" and a management of possibilities' (Foucault 2003b: 138). His argument is that the exercise of modern government is characterised by the structuring of possible fields of action. Fields of action shape the freedom of actors to act by rendering some choices of behaviour and thinking (conduct) legitimate and right rather than others. For Foucault, conduct then becomes significant as the way that socialisation itself is constructed and how it takes particular forms according to time and context (Dean 1996: 217). He studies socialisation by looking at the mundane assemblages of moral, physical and administrative elements that make up the organising of space, time, good and bad (Hunter 1996: 147).

John Law, in his book 'Organizing Modernity' (1994) on research management, is also preoccupied with something akin to the concept of 'conduct', although he prefers to talk about 'agency' (e.g. Law 1994: 74). However, my argument here is that his notion of agency is closely related to the ideas of conduct as Weber and Foucault describe them. Law also readily admits that both of them have been inspirational for his book (Law 1994: 66f and 95). Law considers 'agency' as local and contingent effects of what he calls 'modes of orderings' (e.g. Law 1994: 75). Modes of orderings are patterns of local achievements of order that are made up of networks of material and non-material components, such as machines, talk, ideas and space (Law 1994: 110). The orders are never complete or pure (you will, for instance, never find a pure 'bureaucracy'; there will always be traces, resistance and other competing orders at play). Related to that point is that orders are recurrent, that is, they are open-ended processes that are self-generating:

'The social is a set of processes, of transformations. These are moving, acting, interacting. They are generating themselves. Perhaps we can impute patterns in these movements. But here's the trick, the crucial and most difficult move that we need to make. We need to say that the patterns, the channels down which they flow, are not different in kind from whatever it is that are channelled by them…The social world is this remarkable emergent phenomenon: in its processes it shapes its own flows. Movement and the organization of movement are not different.' (Law 1994: 15, original emphasis)

The modes of orderings share some similarities to Foucault's notion on 'discourse', as they also have ordering effects and create differences between right and wrong, hierarchy, boundaries, etc. (Law 1994: 111). But Law claims that his views on how modes of orderings interact and change and his considerations of the role of materiality are more elaborated than those of Foucault (Law 1994: 22f). They also share some similarities with Weber's different types of 'Lebenswelt', as each order generates its own irreducible ideals, justifications, rationales and, indeed, conducts. Another similarity is the relationship between agency and the construction of meaning. Like Weber, Law also sees a close relationship between actions and meaning creation, where grand narratives do not presuppose local action. Rather, the repetition and insistence of certain actions instate certain worldviews and discourses. Law's focus, however, is not only on the construction of big, historically lasting constructions such as 'capitalism', 'Protestantism' or 'science'. He also focuses on the flux and instability of local pools of orders, as they can be studied in, for instance, research organisations. Law calls these contingent pools of order ‘modes of orderings’ to indicate that they are a special way of understanding and organising in the world, depending on the present orientation. In that way, Law tries to balance between explanations that either favour structure or agency by emphasising the relative instability of orderings, but at the same time insisting that it is possible to impute patterns to the networks of the social (Law 1994: 112).

The individual as an explanatory reason for action is rejected in favour of a decentred subject, whose actions are an effect of a particular ordering attempt (Law 1994: 74). By looking at ‘ordering’ attempts, Law also touches upon the notion of authority and power, as the modes have the ability to guide agency in certain directions, while at the same time also re-establish and strengthen the particular order. Above, I present conduct as a way by which persons in particular circumstances take responsibility for behaving according to the expectations dictated by a specific situation. In my interpretation, this is similar to Law’s understanding of his concept ‘agency’.

This dissertation has been inspired by all three authors, and their different areas of interests in relation to ‘conduct’ will be explored in different chapters. In chapter 5, I have used a Foucauldian framework to analyse articulations of responsible conduct of science as they appear in science journals from 1960 to now. Here the notion of conduct has been important in two senses; First it is seen as the proper way of scientists of addressing a precarious subject: by writing about it in scientific journals, because that is how scientists usually communicate; by writing and in journals (or books), so it is a way of conducting their job as they use to do. The other sense in which it gives meaning to talk about conduct is in relation to the journals themselves: Here conduct is seen as the process of assembling and mediate meaning through a range of different technologies (the tangible papers, the journal databases, the search systems, the letters, etc.). In that way, the papers become a way of managing the conduct of others by inscribe the ideals of how science should relate to society in media, which spread easily both locally and globally.

In the chapters 7, 8 and 9, where I describe how different ‘orders of responsibility’ appeared in the daily work of the two research organisations, I have relied on the ways that Law describes agency. He sees it as being generated by different orders, where irreducible logics and reasons are performed and negotiated. In one of the chapters, 7, I describe a mode of ordering that is quite similar to that which Weber describes as ‘the scientific vocation’ (Weber 1994). But I have also added two other modes that reflect later developments of the scientific work, namely, that of ‘entrepreneurial science’ (Etzkowitz 2003) and that of ‘Responsible science’ (e.g. Nowotny, Scott, and Gibbons 2001; Guston 2000; Owen et al. 2013)

3.3.4 JUSTIFICATIONS

Inspired by Law’s (1994) emphasis on how different pools of orders are enacted and negotiated in local organisational contexts, I want to look at the diversity of responsibilities that are performed in the labs and how these different responsibilities are negotiated and ranked in relation to the daily work. Similarly, I want to see which different worldviews are invoked in order to explain science’s general responsibilities toward society as they are carved out in the journal papers. In order to grasp this diversity, I turn to the concept of ‘justifications’, which has surfaced in various academic fields such as sociology, philosophy, economic sociology, organisation studies, institutional theory, etc (e.g. Boltanski and Thévenot 2006; Stark 2000; Jagd 2011). Interest in the notion has mostly been spurred by Boltanski and Thévenot's influential work (1987/2006) 'On Justification'. The work is considered an alternative to French critical theory, especially that of Pierre Bourdieu, because of the latter's tendency to emphasise social structures over actors' capabilities to act (Jagd 2011: 345). Instead, the authors argue for a 'pragmatic sociology' (Boltanski and Thévenot 1999), which emphasises the possibilities for the (decentred) subject's ability to criticise, manipulate and resist power relations in specific situations by drawing on and ranking different regimes of justification (Boltanski & Thévenot 2006). The authors identify six different 'modes of justification': the civic, the industrial, the market, the fame, the creative and the domestic. Furthermore, they suggest that disputes in social life can be analysed as clashes between these incompatible 'modes of justification', where actors draw on different criteria for evaluating the situation (Boltanski & Thévenot 1999). In this dissertation, I use a general understanding of the subject's ability to creatively use different justifications in specific situations to make sense of specific choices and meanings. I also use the idea that there exist different 'regimes of justifications' – different commonly acknowledged ways of understanding the world and how to act within it. I use this idea when I assert that the different 'modes of responsibilities' encompass different ideas about science's role in society. However, I do not use the content of the six specific regimes that Boltanski and Thévenot describe to explain the scientists' justifications. While I am sure that this would have also provided interesting perspectives, I have let the scientists construct their own justifications and left it at that.

Connecting science and technology studies with specific organisational concerns, John Law also touches upon the phenomena that connect a specific, difficult situation with a more general idea about how to solve it. He gives this phenomenon different labels throughout his book, for instance, 'justifications' (Law 1994: 53) or 'problems' (Law 1994: 111). He sees this phenomenon as yet another effect of ordering attempts:

'*They* [modes] *may generate and embody a characteristic set of* ***problems****. Here the issue concerns patterns in the relationship between what is on the one hand, and what might or should be on the other. It is the gap between these that defines the problem (and so a need for resources).*' (Law 1994: 111, original emphasis)

Law thus studies patterns in the gaps between how the world is at the moment and how it should ideally be – what connects the gap between these two are thus specific forms of justification that refer to specific and different forms of worldview. Similar to the above descriptions, Law's idea is that there are different rationalities that are well known and which the actors can use as a resource for dictating action in specific situations.

While different in scope and disciplinary origin, the different approaches to the notion of justification seem to circulate around the existence and construction of competing rationalities in the social. They share the view that actors are active in choosing different worldviews when justifying actions, while they, at the same time, agree that the worldviews are not necessarily infinite in number. There are certain recognisable, historically institutionalised grounds for making your point in ways recognised as meaningful (Law 2009: 149). In that way, the framework for justifications relates to the work of Weber, as he insists that there are different types of 'Lebenwelt', which inhabit different forms of rationalisation irreducible to each other (Weber 1956; du Gay 2000). With this also follows that 'justifications' should be understood as more than mere 'motivations' for a specific action or opinion. The justifications entail different worldviews, different ideas of what is right and wrong and different ideas about the individual or organisation's role in society. This also becomes clear in the analytical chapters, where each of the 'modes of responsibility' also entails a specific view of science's role in society and the specific character of that society (for a similar view on science's construction of 'society' see Strathern 2003).

In this dissertation, the justifications are used throughout the analysis. The use of justifications mirrors the ambition to connect the diffuse demands for 'Responsible Science' with the mundane practical work that is done in the research laboratories. How do these two entities connect or disconnect? How are the calls recognised, embraced or resisted by scientists, and how do they (if at all) implement the calls in the specific tasks they have to do?

In the first analytical chapter (chapter 5), I look at descriptions of responsible conduct as they are portrayed in scientific journals from 1960 to 2010. I illustrate that these descriptions are based on specific justifications about the role of government: Whether they justify responsible conduct with a focus on internal or external steering as most responsible; and whether they consider the process of doing science or the outcome of scientific work as the crucial object of steering for 'Responsible Science'. In the subsequent three analytical chapters (7, 8, 9), I look at the daily scientific work of two public biotech laboratories. Here, I describe what the scientists consider to be their main responsibilities, how they justify these responsibilities and how they conduct themselves in order to meet these responsibilities. I use the findings from chapter 5 as a mirror, whereby I compare the daily responsibilities and the way the scientists go about their work with the highly idealised ideas about proper scientific conduct.

3.3.5 ORGANISATIONAL CONSEQUENCES

In each of the ethnographic analytical chapters, I also describe how the mode generates some specific shared ways of imagining time and fact-making, the boundaries of the lab, the world 'outside', their professional role and the role of science in general in society. I also indicate that the responsibilities generate both negative and positive feelings among the employees; the responsibilities are at times a joy and at other times frustrating. Law calls all these consequences these the 'patterned effects of modes' (Law 1994: 83), and that is how I also consider them. I want to stress that while these patterned effects may be performed in several places, the exact way they are performed is tightly connected to a local context as well. At the same time, I also emphasise how the scientists reproduce and uphold more general ideas about science's role in society through their daily conduct. I have therefore chosen to call the patterned effects, for 'organisational consequences', because it is the consequences of a range of matters for people that work together in pursuit of more or less the same goals and who share some similar understandings of their place of work and of the work they do. I do know that this term also has connotations to various concepts in organisation theory, but these are not used here.

3.4 ANALYTICAL STRATEGY FOR CHAPTER 7-9[11]

In the analysis of the ethnographic material, 'modes of responsibilities' are used to describe the three different forms of responsibility that the scientists have: the

[11] The document study, where I identify 'political rationalities' is a journal paper, which was published together with Maja Horst in 2014. Therefore I have not described the analytical strategy for that part of the analysis here. It can be found in relation to the analysis in chapter 5. As it is a published paper, I am afraid that some of the points from the theoretical and methodological section will be repeated.

‘truth’, the ‘business’ and the ‘public’ (see chapter 6,7,8). The modes are inspired by John Law’s ‘modes of orderings’ (1994) and each ‘mode’ entails a specific (patterned) way of understanding and acting in the world: What is seen as right and wrong, the role of the scientist, the role of science in society and a range of other issues that render meaning to the scientists in a given situation.

The construction of the modes was done by assembling descriptions of actions from the field notes that were similar to each other and described common ways of behaving in specific situations, also called ‘conduct’.[12] Similarly, I assembled justifications from field notes and interviews that were alike. I also (especially from the STIR inventions) assembled statements that compared a way of doing things with a way of justifying them. I did so in order to establish connections between conducts and justifications. I do realise that the connections between ‘conducts’ and ‘justifications’ are a construction. This is because, firstly, actors make sense and thus justifications for their actions in hindsight as, among others, Karl Weick has described (Weick, Sutcliffe, and Obstfeld 2005). In that way, the actors’ connections are a meaningful reconstruction of their ways of behaving and the accompanying motives, but not necessarily identical with what took place in the specific situation – which was probably much more ‘*messy*’, as John Law would say (Law 2004). Secondly, I, the researcher, also construct the connections between conducts and justifications on the basis of the informants’ descriptions and my theoretical understandings. Thereby, I also influence how these connections are constructed. In a similar way to the informants, I also create meaningful stories about responsibility, which makes sense in relation to the goal of making a dissertation. Given that I maintain (following Law 1994: 109) that both the actors and I draw on well-known general stories about science’s role in society in order to construct a sense of meaning and

[12] This coding work was done based on field notes and transcriptions of field notes in the data coding program Nvivo.

responsibility in specific situations, I think it is valuable to describe the connections between conducts and justifications. Not as a 'correspondence' description of 'reality', but rather as a way of suggesting that *how* the scientists make sense of their work and *how* they conceive of the relation between how they act and why they do so is an important clue as to *how* the scientists adapt to current demands for 'responsibility' in science. As John Law writes about his own research aim:

'[…] *one of the points that **I** tell is that **how** the Laboratory members tell stories, **how** they formulate their past, is an important clue to a much more general issue: how it is that they would like to order the organization in a much wider range of circumstances; and how it is that the organization is being performed and embodied in a wide range of circumstances.*' (Law 1994: 19, original emphasis)

In relation to actually constructing and distinguishing the different modes, it was necessary – besides using 'conduct' and 'justification' – to look at some of the other descriptions that Law gives of the modes in order to get inspiration and guidance. John Law (1994) also describes '*shared stories*' in the labs, that is, shared ways of thinking about the laboratory's past, present and future, its boundaries, internal content and external environment, which help the lab to maintain itself (Law 1994: 54). I have not explicitly worked with the concept in the presentation of the analysis, however, during the construction of the modes, I became aware of working with both conducts and justifications that seemed to be 'shared stories' – either in both labs or within one of them. I want to describe modes as shared, as something most people in the laboratories will recognise in contrast to more private ideas and positions.

Furthermore, Law also describes '*ordering effects*', which is in his words actually the only observable parts of the modes themselves, as they *are* literally the sum of the '*recursive patterned effects of the networks of the social*' (Law 1994: 15). Law describes that the modes may generate '*patterns of deletion*'; that is, the modes tend to put some aspects of the social in the background, while others are empowered and therefore very visible (Law 1994: 111). He also states that modes generate and perform

'…distributions, defining or embodying a characteristic approach to what might, does or should pass from whom to what under what circumstances' (Law 1994: 111).

In my work on constructing the modes, I also looked out for these kinds of patterned effects in my material. I sorted them according to how some entities, for instance, 'the scientific profession', were extremely visible and active in some situations, while being utterly absent in others. And I distinguished between the different ways of constructing knowledge that the scientists told me about; at times, they spoke of public science as a stable institution that disseminates knowledge to a 'society' made up of huge institutions such as the '*state*', '*industry*' and '*the public*'. Other times, they spoke about how knowledge is sold on a market where they compete with other laboratories. Again, the task was to group these different stories that I saw in my material, since I treated them as effects of a specific way of looking at the responsibility the scientists have.

These groups of shared stories, ways of doing their work, justifications for their actions and the ordering effects were then combined and distinguished from each other in order to make up each of the modes. All of these elements did not just fit together nicely like pieces of a puzzle. Therefore, I chose justifications as the most important part of the mode in order to be able to distinguish them from each other. If the justifications for different forms of behaviour or more general effects were the same, then they belonged in the same mode, even though the effects looked very different. If it was not possible for me to construct all elements, that is, conduct, justification, shared stories and ordering effects, then I did not consider the data material consistent enough to support the construction and description of a mode. In that way, the different components also worked as a way of triangulating my analysis: if all the elements were not in place, I refrained from further description and these 'half-baked' modes are thus not part of the analysis. In the end, I ended up with three 'modes of responsibility', whereby each generated some specific ways of doing the daily work, each argued in different ways for the reasons for this specific responsibility and each had some general consequences for how the labs were organised.

4. METHODS

In this section, the practical aspects of this study will be discussed. This chapter is divided into three sections. The first section looks at the cases and outlines why the specific arenas, text and laboratories in this study were chosen and, in relation to the laboratories, why I chose to compare laboratories in Denmark and the USA, as well as why I chose synthetic biology. The second section concentrates on the methods used to study responsible science empirically. It explains the use of document studies, observations and interviews and provides an overview of the data collection for this project. I also discuss some of the challenges I faced, especially in relation to the ethnographic studies and the collaboration with the scientists. Finally, I describe the analytical strategies for the document study (chapter 5) and the ethnographic studies (chapter 7-9) respectively.

4.1 CASES

This study is a case study of how scientists understand and conduct responsibility in relation to their work. I have used two cases as the loci for the investigation of this question. The first case is a document study of how scientists articulate responsible scientific conduct, and the second case is a laboratory study of two laboratories working within the field of synthetic biology. In the next two sections, I will account for why these two cases were chosen and why I look at them as 'cases'. As my point of departure, I use Abbott's definition of cases as:

'[…] *fuzzy realities with autonomously defined, complex properties – and* […] *as engaged in a perpetual dialogue with their environment, a dialogue of action and constraint that we call plot*.' (Abbott 1992: 65)

Based on that definition, I see cases as a defined and delimited field of study, where the properties of the cases can be studied as if they were acting independently of their surroundings. But in reality, these boundaries are constructed (by the researcher), and it is important to notice that the environment also gives meaning to the case. By using the terms '*fuzzy*' and '*complex*', Abbott also underlines the importance of a case being big enough to expose contradictions, different layers and different themes, which can be unfolded by the researcher. In the articulation of this study as a 'case study' also lies the assumption that the '*fuzzy reality with autonomously defined, complex properties*' is an example of something that can be generalised beyond the case itself (Abbott 199266; Ragin 1992: 8). I will return to these possibilities for generalisations, based on the findings, in the concluding discussion (10). Here, I will describe my cases.

4.1.1 THE DOCUMENT STUDY

The document study comprises the study of an archive of 263 papers about the responsible conduct of science written between 1960 and 2010 (see chapter 5 for further descriptions of the construction and analysis of the archive). Initially, the document study was intended to be a more 'traditional' literature review of the 'state of the art' of the pre-supposed field of 'social responsibility in science'. But the literature searches soon showed that rather than one or several delimited 'fields' detailing this subject, there was a heterogeneous cacophony of voices coming from many disciplines, with each arguing for their conception of 'Responsible Science' and how it can be realised.

Therefore, the 'literature review' was turned into an analysis of different 'political rationalities' that attempt to steer the conduct of science in certain directions; the journal papers are thereby considered data rather than background and theory. In this dissertation, the document study is considered a 'case' on contemporary ideals about responsible conduct in science. The contemporary ideals are located by studying a specific archive (263 journal papers) that was assembled as representations of understandings of responsible conduct in general. Each paper articulates an idealised picture of how scientists should behave and how the institutions of science are supposed to be regulated if we are to ever reach these ideals. The analytical task was to identify these articulations and find patterns (political rationalities) in their argumentations. Based on the sample of texts, four rationalities about the responsible conduct of science in general are presented. The argument for generalising from 263 journal papers to a statement about 'science' more broadly is that the archive had representations from a diverse range of disciplines and voices. The analysis is presented as the first analytical chapter of this dissertation (5).

4.1.2 COMPARING DENMARK AND USA

Moving from the document studies to the ethnographic studies, the task formulated in the PhD grant was to make a qualitative study of two research organisations that worked in the fields of advanced biotech in Denmark and the USA.

Besides the very practical aspect of starting with Denmark because it is my home country, Denmark in itself represents an interesting place to conduct studies of 'Responsible Science', as it has a rich tradition for science communication that has also been the inspiration for some of the recent ideas about including social values in scientific conduct. As I also described in greater detail in the theory section, Denmark is now putting a stronger focus on organisational communication and inter-organisational competition in the way that science communication is performed. In that way, Denmark is almost moving in the opposite direction of many other Western countries at the moment (Mejlgaard 2009). The USA is also interesting for several reasons, first and foremost, because they are considered global front-runners in the development of science and science government – a fact already noted by Weber in 1917 (Weber, Owen, and Strong 2004). While the structure of the US scientific system and its relationship to the state, industry and the public has been a source of inspiration for many other countries, public science and the university system is still very much different from the Danish model. The US government has recently developed additional outreach and engagement projects in relation to their strategic stake on novel technologies, but while they are very far in their scientific endeavours and strategic government of science, they are not as experienced in outreach and engagement as Denmark (Jasanoff 2011).

From the beginning, there were two reasons for the comparative aspect of this study. First, studying two laboratories is a way to become more aware of the local, unique traits of each laboratory. It is also a way to make myself ponder over aspects that I may otherwise overlook, because I may take them for granted as institutional aspects of science rather than local aspects. Seeing that things can be otherwise in another laboratory was supposed to make me more sensitive toward my data material (for similar considerations in relation to comparative laboratory studies, see Traweek 2009; Knorr-Cetina 1999). The other idea was that the countries' respective political cultures in relation to science's role in society would influence how the scientists think about and conduct 'Responsible Science' (as suggested by, for instance, Irwin 2006; Irwin and Horst 2010). As I will discuss in 'Observations' (4.2.1), this second idea proved difficult to establish in relation to my material, whereas the former idea about sensitivity was very fruitful in terms of being much more sensitive during the actual observations. It is easier to study an interaction as something special and noteworthy when it can be compared to another place where things are handled differently.

4.1.3 SYNTHETIC BIOLOGY AND LABORATORIES

The grant proposal also suggested different scientific disciplines such as nanotechnology and synthetic biology as possible empirical focuses for the study. I chose synthetic biology because it is a very new field (even newer than nanotechnology), where a lot of promises for its potential uses are already being made by scientists, potential funders and politicians. At the same time, others advocate caution and consider the discipline controversial because it combines biology and engineering, thereby creating new organic material that nature has never nurtured and which could also have potential unknown side effects for both humans and nature.

Because the field is itself so much in the making, it was my expectation that the scientists working with synthetic biology would have reflected upon the directions of research and the questions they wanted to pursue, as there are still so many uncertainties surrounding the field's potential. The last reason for choosing this area was the expectation that the controversies surrounding the field since its emergence would have familiarised the informants with themes such as ethics and public scepticism, thereby enabling the scientists to elaborate considerations and give examples from practice. All in all, my initial thoughts were that I would end up with two laboratories, one Danish and one American, and I would compare the way they related to themes of 'Responsible Science' and see if I could link their different practices to their different political cultures. Inspired by Flyvbjerg (Flyvbjerg 1991), the case would then be a comparative best-case study. The different political cultures in Denmark and the USA would together be the point of departure for the comparison, and I would highlight the differences based on this theoretical premise. It would be a 'best-case' methodology because I expected (in both cases) to find organisations where people already relate to demands for 'Responsible Science'.

The two laboratories where I made the most elaborate field studies[13],(which I will call Gyro Gearloose and Curious George respectively from now on) find it important to include values and ethical questions in their work – each in their own way. Curious George has worked with GM plants for many years and they have learned the hard way that funding can disappear due to lack of public support (see also the description of the so called 'GMO crisis' in 2.1.1). They do not want a situation like the one that occurred around the turn of the millennium to happen again, and they work hard to obtain public support for their work. In the USA, I, helped by my American supervisor, chose a lab working on the development of an advanced diagnostic device that is able to distinguish between a wide range of different diseases. They are also developing new vaccines for, for instance, certain types of cancer. In comparison with the Danish lab, the American lab seemed much more focused on developing readymade technologies, but at that point, I was not sure if that was more a way of branding themselves than an actual ambition. Nor did I at that point regard the different scientific aims as being of great importance, but this perception has changed, as I will demonstrate in the analysis.

[13] I did two pilot studies in Denmark before I decided to continue my observations at Curious George. The other lab was also working with synthetic biology. While it was also engaged in different outreach activities, the lab in itself was much smaller and solely focused on one project: to create a synthetic living cell. At Curious George, they were part of many different projects and faced different forms of problems depending on the content of the projects. I was interested in this diversity and the opportunity to interview many different people at different levels in the laboratory.

4.2 FIELD WORK

Moving away from the considerations regarding the case studies, the theme of the following sections relates to the different engagements with the field in the ethnographic study (the method for the document study is presented in 5.2). I used three methods of collecting data in the laboratories: observations, 'STIR' interventions and interviews. An overview of the different engagements is provided after the descriptions of each method.

4.2.1 OBSERVATIONS

In the beginning, I considered the observations a supplement to the interviews I was going to conduct toward the end of my stay. The theme of 'social responsibility' in science was something I was sure would need to be discussed with the scientists in order for me to understand, because, at first glance, it did not seem to be something that I could extract from the observations. However, it was important for me to know something about the laboratories, the people and the science well before the interviews so we could talk about responsibility in connection to their everyday work.

But after a while in the field, I discovered that the observations were more valuable than just well researched background information. They provide valuable insights in their own right, not least when it comes to negotiations about the labs' values and the consequences for how they prioritise tasks – themes that are discussed at both meetings and among colleagues during the day. The values are also revealed in newspaper clippings about important topics or cartoons hanging on the scientists' doors or in the mails that circulate on the labs' common email lists. Other aspects that point to what the scientists consider (and do not consider) their responsibilities also emerge in relation to the physical arrangements of the labs. For instance, I noticed the difference between the perimeter and the middle of Curious George. At the perimeter (closest to the canteen, farthest away from the labs) sit the PR personnel. The walls are covered in promotions, newspaper clippings, calls for papers, and conference posters, all of them set up to spell SYNBIO in capital letters. But the term disappears completely when you enter the wet labs, the heart of the laboratory and the place where experiments are done. Here, the writing on the walls is about security and safety, what the flasks contain and which temperature the fridge has to be at. Gradually, the observations acquired the same status as the interviews: Valuable insights into how various understandings of responsibility matter for the scientists' daily work, their division of labour, and how responsibility can create a sense of belonging to the lab and the scientific profession.

The observations also helped me rethink the research theme, especially regarding the comparison between the two political cultures. It was as if the connections between the scientists' daily work and abstract political cultures were quite difficult to make, however, other interesting connections stood out. There were many striking similarities between the two laboratories: The colours were very much the same (grey, white), and they told similar stories about the importance of accuracy and carefulness, about measuring correctly and re-counting the flasks. Their public events also shared similarities. There were evenings where people were invited into the lab and scientists explained their field. There were meetings in public buildings, where scientists and the lay public discussed new scientific findings or ethical controversies. At the beginning, it was more surprising that similar types of contradictions and conflicts appeared in both labs. At times, even though I was actually in the same organisation, it felt more as if I was in different organisations over the course of one day than it had done when I went from Denmark to the USA. Sometimes, for instance, the scientists in both labs believed the public to be ignorant; at other times, they were thought of as knowledgeable. Sometimes, they seemed absolutely confident about their future results and predicted scientific revolutions; at other times, they advised caution and carefulness in relation to results that were already published. Second, both laboratories also shared a sense of responsibility toward the organisation and toward the public, but these two concerns were performed differently at each lab. The observations of the shared recognition of norms of responsibility, as well as the two labs' different reactions to them, meant that I included other forms of comparison in the study apart from just the difference between the two labs. I also started to compare different responsibilities and different situations independently of where they took place.

The role of the ethnographer has been discussed at length in much seminal literature on ethnography, and the roles vary from the strictly observant to the fully participating member (De Certeau 1998; Neyland 2007; e.g. Law 1994) I decided to play a rather active role based on the very practical consideration that I was quite nervous about being in the field and activity makes me less nervous. I tried to act as a sort of consultant or confidant (dependent on my impression and relationship with the person) and encouraged them to discuss work conditions, problems with experiments, ethical considerations and other themes that they found important from day to day. I read white papers on synthetic biology and the most cited papers from each lab in order to understand the basics of their field. This was based on the thought that to take highly skilled experts' time and then seem oblivious about their field of competence does not engender any trust, respect or will to cooperate. While I would not in any way claim that I became a favourite confidante or very important to any of the employees, I did over time build up trusting relations with many of them, especially the junior scientists and especially in the Danish lab. This also occurred to a certain degree in the American lab, but due to language barriers and a far more formal tone of communication among the employees, the strategy did not work as well.

In ethnographic fieldwork, some of the biggest challenges can be figuring out how to move around the organisations, how to be informed about important events and how to handle the great amount of time one spends wandering aimlessly around the hallways, all the while being unable to shake the feeling that everyone thinks you are peculiar (Neyland 2008). In 'Organizing Modernity', John Law, for instance, describes how he developed a habit of striding quickly through the buildings with a couple of books under his arm in order to look preoccupied, while he was in fact just going around in big circles (Law 1994: 44). Barbara Czarniawska comments that it feels as if whenever anything important happens in the organisation, then she is always somewhere else (Czarniawska 1997). In order to counteract at least some of these feelings of ridiculousness and ignorance, I decided to use a 'shadowing' strategy, whereby I sometimes followed a single researcher for half a day. The method had several advantages for me: First, I could ask this individual to explain things to me along the way and we could engage in conversation. Second, the method enabled me to move about easily in the organisation, provided that the scientist moved as well, and granted me entry to events that were important so long as I went with the scientist. I followed twelve scientists around each lab, making sure to schedule appointments with them in advance so I knew I had something to do when I arrived (although they forgot about me once in a while and disappeared on holiday or to conferences). The strategy also had the advantage that I got to know many of them quite well and could move from asking the same questions to engaging in discussions that developed from one shadowing session to the next. I continued this strategy for most of my stay at both labs, but as I became more familiar with the labs, I supplemented the shadowing strategy with observations of various meetings, presentations and some of the public engagement activities that they participated in.

4.2.2 STIR

During the observation studies, I also introduced the STIR interventions (Fisher 2007). I added this method to the observations and interviews because I wanted to explore the justifications for mundane decisions made by the scientists in the labs. The idea was that I could persuade the scientists to explicitly construct a connection between their daily work and some more general values.

The STIR protocol was developed by Erik Fisher as a tool '*to more reflexively attend to the integration of technical and social considerations*' (Fisher 2007: 158) The initial idea is that social scientists, via the repetitive use of this specific interview protocol can make scientists reflect further upon their own scientific practice, the values inherent in that practice and perhaps even change that practice. In that way, STIR forms in my interpretation part of the government rationality, Responsible Science' that I intend to study. Therefore, I did not use the STIR protocol as a way to enhance reflexive capacities. But I found the interview guide and the repetitive engagement (several times a week) with the same scientists very useful, because it makes it easier to combine everyday practice with general reflections on values.[14]

[14] The protocol consists of four phases, namely: Opportunity, considerations, alternatives and outcomes (Fisher 2007), which I translated into four questions, namely: **Opportunity:** *What have you been doing today?* **Considerations:** *Did you face a dilemma during the day? If so, what did you chose to do?* **Alternatives:** *What do you think would have happened if you had chosen otherwise?* **Outcomes:** *Who cares about the decision and why?*

4.2.3 INTERVIEWS

By the end of each stay in the labs, I had conducted interviews with a selected portion of the scientists, lab technicians and PIs. These were used to help make sense of the observations that I had conducted throughout the previous months.

The informants were chosen based on the impressions I got from the observations. Moreover, informants were selected based on the premise that there should be informants occupying different positions in the labs; that is, both juniors and seniors. I also wanted as many different points of view as possible in order to explore as many understandings about responsibility as possible and thereby gain broad and multifaceted material in which to look for patterns. Therefore, the informants also consisted of people whom I had heard express qualitatively different opinions about their daily responsibilities and science's role in society.

Interestingly, the scientists at Curious George were very curious about how I selected my informants. They were afraid, on my behalf, that I would not be able to get a 'representative' picture of their opinions because they could see that I had chosen specific persons. One of them commented that I seemed to be more interested in very vocal people and encouraged me to include the more silent ones as well. Furthermore, some of these scientists seemed quite curious about what was going on during the interviews, which took place behind closed doors. I got the feeling that being part of the project was considered a good thing – perhaps because they knew that management approved of the study and the engagements. Therefore, I ended up with many more interviews from Curious George than I had originally intended: I could never refuse to interview people who actually want to participate.

The interviews consisted of three overall themes: I asked about their daily tasks and inquired about specific situations from the observations that I wanted to understand; I asked about their motivation for working in public science and their responsibilities in relation to their specific position; and, finally, I asked them to relate to common ideals about science's responsibilities, including the four political rationalities from the document study.

4.3 OVERVIEW OF FIELD ENGAGEMENTS

OBSERVATIONS

Place	Pilot 1	Pilot 2	Curious George 1	Curious George 2	Gyro Gearloose
Duration	1 week (2011)	1 week (2011)	6 weeks (2011)	4 weeks (2012)	10 weeks (2012)
Persons shadowed	3	3	9+	10+	10+
Positions	PhD student, postdoc, full professor/PI	PhD student, postdoc	Lab technician, research assistant, PhD student, associate professor, PI, project coordinator	PhD student, associate professor, PI, full professor, group manager	PhD student, assistant professor, PI, lab manager, full professor, Master student, postdoc

STIR INTERVENTIONS

Place	Gyro Gearloose	Curious George
Duration	6 weeks	6 weeks
Participants	4 participants	4 participants
Missed out (numbers indicate how many STIR interviews did not occur during the 6 weeks)	One person one time	Three persons, one time each

INTERVIEWS

Pilot 1	3 Interviews
Pilot 2	3 Interviews
Curious George 1	8 interviews
Curious George 2	11 interviews
Gyro Gearloose	9 interviews
All in all:	34 interviews (between 40 minutes and 2 hours)

SEMINARS

By the end of my fieldwork, I had conducted two seminars, one at Gyro Gearloose and one at Curious George respectively. Here, I presented initial findings and discussed them with the participating scientists. Field notes done by Sarah Davies and Emil Husted.

4.4. CONSIDERATIONS ABOUT THE KNOWLEDGE PRODUCTION

4.4.1 WHO ENTERS MATTERS – IDENTITY WORK

During the field studies at the labs, it became clear that I had to partake in what Emma Bell (1999) labels '*identity work*' for '*impression management*' (Bell 1999: 17 see also; Goffman, Jacobsen, and Kristiansen 2004). In order to obtain the knowledge that I was looking for, it proved important that I appeared as someone whom the scientists could legitimately confide in (Baarts 2007). In that relation, the informants' perceptions of my identities such as 'female', 'non-scientist', 'appreciative', and 'junior' affected the knowledge created. In this section, I will account for this 'identity work' that I did, especially in relation to being female and coming from a business school, in order to be regarded as a legitimate receiver of information

My physical appearance mattered for the engagements at Curious George. I am relatively young, relatively tall and relatively female. Even though all of those identities already exist at Curious George, the combination of all three does seem to attract attention. One of the scientists, that I did not know, commented loudly that it was obvious that I was not used to the hard lab-work, since I saw it fitting to wear high-heeled boots at work. I also found out that I was referred to as Cecilie '*with the big, brown eyes',* and there were other incidents where it became clear that my physical appearance had been a theme of discussion in the lab. I enjoyed conversations about fashion and clothes with some of the female scientists during lunch breaks. It works very well as an icebreaker, however, the other scientists still teased us for pursuing such "ridiculous" subjects and I thereby definitely (if not before) acquired an identity as 'female' by doing so – an identity that did not always seem to be as respected as the male one. At least it seems that 'female' connotes 'less scientific' (an observation, frequently made by feminist science studies (e.g. Reinharz and Davidman 1992). At Gyro Gearloose, the tone of the entire lab is much more formal and gender is not on the agenda, neither mine, nor theirs. Once, someone questioned the fact that I was pursuing a PhD degree while my husband was 'only' finishing his Master's degree. Apparently, he did not consider that the correct role distribution. But those who overheard the remark looked extremely embarrassed and the room fell silent. Gender is clearly not seen as a legitimate measurement for appropriate behaviour at Gyro Gearloose.

However, at Curious George it was perhaps my affiliation with 'Copenhagen Business School' – or '*Money School*' as they called it – that proved to be a greater legitimacy problem than my gender. As we shall see in the analysis (both chapter 7 and 9), business is not considered a serious science in the same way as the biosciences, nor is the pursuit of profit considered very noble. While neither of these issues form a particularly big part of the present research project, just the fact that I had an office on the CBS campus was a point of curiosity and suspicion regarding my abilities among some of the scientists. Women from CBS (the students) were also referred to as '*dolls*'. Sometimes, they mentioned that it was people like me (studying business) who could figure out how to make money on their technologies – but they clearly did not consider that to be an advantage. At Gyro Gearloose, I got almost the opposite reception. There, especially the lab director seemed disappointed that I could not say more about market potentials and ethics, as he considered those my subject. He told me to be more daring and actually give some recommendations – very much in line with his philosophy that knowledge for the sake of knowledge was of no use.

My gender (at Curious George) and my academic affiliation (at both labs) were certainly the two most pervasive obstacles in relation to being considered a legitimate knowledge seeker in the labs. And most of the time, neither of these were a problem. But there were times when I got the impression that I had to manage my identity carefully in order to be considered an individual whom they wanted to share their knowledge with. While it may have been an advantage that I was an outsider and thereby more 'safe' to confide in, it was also important for them that they found me capable of handling that knowledge and a worthy partner of conversation – and it is here that the impression management is important.

These identity negotiations vary from situation to situation. But in relation to gender and academic affiliation, I often tried to compensate for my 'disadvantages' with humour and other traits that were valued – strategies also described by Bell (1999: 23) and Baarts (2007). I also took care to underline that I had understood the science the scientists told me about (not that I always did), by referring to similar experiments or asking about certain topics that I remembered and had read about. I provided knowledge about science politics and explained certain logics in that system in order to show that I also possessed specialised knowledge. I also made fun of my 'business background' or provided ironic comments to distance myself from CBS. Furthermore, I repeated that I was not doing business studies at Curious George, however, at Gyro Gearloose, I did discuss the development of bio-business, the medical sector, FDA and the like with the PIs to show that I was well-informed and interested in their work. However, I never volunteered any specific advice. All of this may seem ridiculous and perhaps quite self-absorbed. But as Bell (1999) suggests:

'*Though the ethnographer's working role within the research setting is, to some extent, predetermined by ascribed characteristics, such as gender, which constrain data collection and colour narrator perspective, these impressions are* *highly negotiable within the research process. The researcher is practically capable of utilizing social skill to exercise a degree of agency (Giddens, 1984) in defining a working role and shaping relationships with organizational members.*' *(Bell 1999: 32,* original reference*)*

To compensate for characteristics that, at times, were not seen as favourable in relation to being considered legitimate, I used my '*social skills*' to change the scientists' impression of me and move attention away from the business school. But an important part of balancing this 'impression management' is also to remember personal integrity throughout these situations. I am actually quite proud of working at CBS – and I am *certainly* proud of being relatively female. Therefore, I also employed other strategies that were more offensive than defensive. After some of the incidents at Curious George, I started to wear even higher heels, more make-up and more feminine outfits than I usually do. This is also impression management – it is just another strategy. By doing so, I both wanted to assert that women, of course, have a legitimate place in academia as equal partners in all kinds of conversations, and, at the same time, to use the reactions evoked to gain more knowledge about the culture.

Most importantly, at both labs I was eager to disassociate myself from the field that I am in fact a member of, namely, that of visiting laboratory ethnographers. While the scientists were positive and welcoming, they had tried having ethnographers around before and therefore seemed tired of discussions involving the same topics, such as 'for and against GMO' or 'for and against vaccines and diagnostic devices' (the two labs respectively). It seemed important that I come up with new angles on my project about 'responsibility' if I wanted their attention and confidence. Their abilities to reflect about their own role in society, combined with annoyance with laboratory ethnographers ended up as an inspiration for studying the scientists' own ideas about responsibility instead of only 'looking' for ideas about responsibility that resembled those in the 'Responsible Science' literature.

4.5 OVERVIEW OF THE ANALYTICAL CHAPTERS

In this section I will, before moving on to the proper analysis, present an overview of the structure of the analysis.

In chapter 5, I present the results of the document analysis. I describe four different political rationalities that are constructed from the study of an archive of scientific journal texts. The first, the '*Demarcation Rationality*' advocates for a strict separation of science and society; science can only objectively describe and thereby help society if it is 'outside' society. The '*Reflexivity Rationality*' sees science's responsibilities as being inspired by society's many problems – not least the ones science has created – in its priority of projects and research goals. The '*Contribution Rationality*' does not share the two previous rationalities' faith in science's ability to govern itself responsibly and therefore suggests the firm external control of science, in order for it to live up to society's demands for innovation and democracy. Finally, the '*Integration Rationality*' also articulates the external governance of science as being necessary, but sees dialogue and collaboration between science and societal actors during the research process as the way to reach a more responsible outcome.

In the next chapters, I move from the study of documents to the ethnographic study of two laboratories. In a small prelude, I introduce the two laboratories before moving on to the descriptions of three different modes of responsibility.

In chapter 7, I present the first mode of responsibility, namely, that of Vocation. This mode generates three different forms of conduct: checking, repeating and criticising. The scientists justify these ways of going about their work with the responsibility they have for the 'truth' – a phenomenon they continuously have to take care of by doing their work in a specific way. This mode also generates some more general consequences for the scientists and the way they consider their work and their role in society. They regard themselves as being part of a large, international scientific community and do not regard the specific laboratory where they work as being of particular importance. They consider time an endless resource, because working meticulously with the experiments is valued more highly than finishing a project. This also has to do with their understandings of a 'fact' as a phenomenon entailing extreme caution on their part in not predicting too much without a lot of evidence to back it up. They consider themselves a part of a society where 'science' is a stable institution that delivers (at times relevant) knowledge to other stable institutions, namely, that of the state, the industry and the public. They also closely relate this relationship with the part of their work they label 'scientific', that is, doing experiments, analyses and writing papers. This is their main job as scientists, and therefor the responsibility for the 'truth' also holds the highest value.

In chapter 8, I describe the second mode of responsibility, namely, that of 'Oikos'. This mode generates three different conducts: investing, saving and maintaining. The scientists justify these ways of doing their work with the responsibility they have for the 'business'; that is, the laboratory's survival. In order to make sure that the lab and its members will be there in the future, they have to take care of their resources. This mode also generates some specific consequences for the entire lab and the way they consider their role in society. Instead of being part of a worldwide community, they are now much more attached to their organisation and consider themselves in competition with other organisations. Scientists are no longer members of the same large group and can even be ‘enemies’. Their perception of time has also changed: Where before the scientists valued working hard to perfect their results over a long period of time, they are, in this mode, much more aware of their resources being finite and they need to do as much work as they can with as few resources as possible. Their view of facts has also changed: Where Vocation generated a carefulness about bold predictions and faith in results, the scientists are now much more bold and believe they can prove their own hypotheses. It seems as if a certain anxiety is also traceable among the scientists; they are aware of their temporary employment and the risks of not being funded again. They consider science’s role in society as one where scientists are sellers of knowledge on a market, but they never know whether anyone will buy their knowledge, thereby ensuring their survival. While the scientists are perhaps not that fond of this part of their work, the three forms of conduct do take up much of their time, since they are not able to live up to their other responsibilities without a lab, that is, the business.

Finally, in chapter 9, I describe the mode of Citizenship. Three forms of conduct are connected to this mode, namely, ‘speaking about science’, ‘listening to the public’ and ‘simply doing research’. They justify these conducts with their responsibility for conducting science that is legitimate in the eyes of the public. Science’s role in society is now considered part of the public sector and has to live up to certain standards of transparency and legitimacy in the public’s eyes. This also generates the consequence that the scientists engage themselves in activist-like activities, where they take an active stance in how science should be conducted and used. On the other hand, it also engenders a certain sense of ‘basic science guilt’ among those scientists who cannot readily state how their scientific findings can be translated into usable technologies. In contrast to the two previous modes, Citizenship does not generate a clear understanding of time and fact-making; perhaps because the mode is relatively new and no general understandings of these areas have been established.

In each chapter, the mode of responsibility is described as if they were 'pure', that is, as if the other two modes did not exist and the scientists only made choices and decisions based on one responsibility. In that way, it is almost as if I describe three different organisations in the three chapters, but it is the same two labs being used as the basis for all three chapters. In the scientists' everyday work, the three forms of responsibility are constantly mixed and they are weighted against each other in specific situations. Throughout all the chapters, I also show examples of how the responsibilities are weighted against each other. I comment on the difficulties in understanding and prioritising between the different responsibilities in specific situations, and I discuss this as being one of the biggest challenges in relation to the realisation of 'responsible science' in practice.

5. MAPPING SOCIAL RESPONSIBILITY IN SCIENCE

One of the characteristics of the '*new governance of science*' (Irwin 2006; Guston and Sarewitz 2002) is that the ability of science to govern itself in a responsible way has been fundamentally problematized (Braun et. al. 2010; Jasanoff 2011)[15]. Along with this problematization has come a new set of sensibilities and demands for more deliberative forms of governance (e.g. Irwin 2006; Kearnes & Rip 2009). The theme of social responsibility of science, or even 'Responsible Research and Innovation' (von Schomberg 2011), has gained momentum within both policy and academic discourse (Guston & Sarewitz, 2002; Fisher & Mahajan 2006; Owen et al 2009; Stilgoe 2012; Sutcliffe 2011). In particular, notions of the social responsibility of science are evolving in soft law and international settings, yet there is no unifying definition of what this term means (Davies & Horst, 2012). Rather, the notion of social responsibility of science can be seen as political: It is open for contestation about how it should be defined and interpreted, and each of these interpretations has consequences for the governance of science, i.e., the way science is regulated and practiced (Foucault 2003a).

The notion that science has a responsibility towards society, however, is not new. Scientists have a long tradition of discussing their responsibilities as a balance between their professional autonomy and their general moral responsibility as human beings (Douglas 2003). Nevertheless, there are no clear connections between scientists' own discussions and that of policy makers and scholars of the new scientific governance, although it should be recognized that the literature on social responsibility 'in practice' is growing (e.g. Mccarthy & Kelty 2010, Phelps & Fisher 2011, Stilgoe, Owen & Macnagten 2013, Davies, Glerup & Horst, forthcoming). This paper contributes to the understanding of

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the relationship between scientists' discussions and that of policy makers and other actors by mapping the overall landscape of ideas about social responsibility in science as it can be found in academic journals. It takes as its point of departure the position that both scientists and scholars of new scientific governance are addressing the same '*problematization*' of science, namely how it should be governed – or govern itself – in a responsible way. In doing so, all voices addressing this issue are viewed as 'political' because they contribute to the shaping of ideals about how science is to be performed and regulated. The paper, therefore, makes no a priori distinctions among different types of voices. Rather, it deliberately treats all voices equally in order to investigate the governance effects of these discussions.

Mapping discussions on social responsibility in academic journals, we have employed a Foucauldian analysis of governance to understand how a particular conceptualization of responsibility implies a political rationality, i.e., a particular form of governance of science. The analysis identifies four different political rationalities. They differ according to whether they advocate internal or external regulation of science and whether they are focused on regulation of the process or the outcomes of science. They all imply, however, that a particular relationship between science and society is necessary in order for science to be responsible and also that scientists need to conduct their science within the structure of this relationship in order for their practice to be legitimate and proper.

5.1. RESPONSIBILITY AS A POLITICAL RATIONALITY

Debates about the social responsibility of science are far from new (Shapin 2009). While many events, important for these debates, have occurred, we restrict our brief discussion to two. First, the Manhattan Project – the development of the atomic bomb in the United States during World War II – led a number of scientists to discuss the purpose of their occupation (Rhodes 2012). Shapin (2009, 65) describes how scientists in this context moved their discussions into the public sphere by debating their moral obligations in relation to the development and use of nuclear bombs. These discussions are continued in present debates about dual use (McLeish and Nightingale 2007), but also more broadly in the discussion of scientists' responsibilities for the use of the outcomes of their science. In 1973 and 1975, the Asilomar Conferences on recombinant DNA brought together scientists – and importantly some non-scientists – who gathered to discuss potential hazards in connection with the discovery of restriction enzymes and the incipient field of gene technology. These discussions inspired new governmental organizations such as ethics committees in a number of countries and instigated more general discussions of scientists' abilities to govern themselves responsibly (Braun et. al. 2010). The case of Asilomar demonstrates the contested nature of such activities: Is it a successful story about the will to self-governance, a discussion led by a small elite of scientists, or a story of how scientists were forced to further regulation by outside actors (Barinaga 2000)? Regardless of how one interprets the event, Asilomar epitomizes the question of science's ability to govern itself.

While the discussions about the relation between the scientific profession and society are thus not new, the debate following the Asilomar conferences marked a shift in the character of the problems discussed and the kind of actors who might have a legitimate say about them. In a Foucauldian perspective, this shift can be seen as a 'problematization' of the hitherto internal governance of science through professional norms. A problematization should be understood as the emergence of intensified discussions and propositions about the steering of a specific area of the social, because the preceding ideas on how to govern have come under scrutiny (Foucault 2003a, 229-30). In this instance, what had come under renewed scrutiny in the second part of the twentieth century was science's ability to govern itself responsibly, its strong connections with the state and its weak links to the public, and its traditional morality as basis for state regulation of new technologies (Braun et. al. 2010, 512). In more recent years, discussions of science and social responsibility have also been connected to more general, theoretical debates about governance. Some consider the demands for responsibility an instance of a growing market-embedded morality spurred by neoliberalism (Shamir 2008); whereas, others are worried that questions of ethics and responsibility are neglected in favour of market considerations (Hellström 2003). The discussions of how responsibility in science relate to broader societal developments further underscores its more prominent place on the agenda as a governance-problem.

Our use of the term 'governance' follows Foucault's argument that the exercise of modern government is characterized by the structuring of possible fields of action (Foucault 2003b, 138). Fields of action shape the freedom of actors to act by rendering some choices of behaviour and thinking (rather than others) legitimate and right – what Foucault (2003b, 138) has referred to as 'the conduct of conduct'. First, it implies a move away from a state-centered view of 'government' as something that is conducted by specific individuals or classes of individuals with a pre-defined set of interests. Rather, governance should be understood as attempts to shape individual and collective behaviour performed not by individuals or classes of individuals, but by rationalities or discourses (Foucault 1976/1998, 94, Foucault 2003b, 128). Hence, modern governance is understood as complex multitudes of language, agencies and technologies that seek to administer the lives of others (Foucault 2003a, 237). Second, it implies that studies of modern governance should focus on how these multitudes make different fields of action possible by turning our attention to the study of 'political rationalities' (Rose and Miller 1992) – 'the changing discursive fields within which the exercise of power is *conceptualised*, *the moral justifications* for particular ways of exercising power by diverse authorities, notions of the appropriate forms, objects and limits of politics, *and conceptions of the proper distribution of such tasks* among secular, spiritual, military and familial sectors' (Rose & Miller 1992: 175, our emphasis). In order to study modern governance, we thus have to study how different forms of rationalities about responsibility in science are conceptualized, how they are justified, and to whom the practice of responsibility is distributed.

We pursue this agenda by studying articulations about the social responsibilities of science as they appear in academic journals. As a medium for professional and normative discussions about the role of science, such journals are an important venue for discussions about this issue, and due to the intensity and breadth of viewpoints they present, the analysis can serve as an indicator of the current, more general perspectives on the social responsibility of science. Using empirical material from academic journals accentuates the fact that the governance of science is, to a large degree, structured by the profession's own norms and standards. Merton emphasized this point when he described the norms of communalism, universalism, disinterestedness, and organized scepticism based on interviews with various scientists (Merton 1973). Sociologists of science have subsequently criticized these CUDOS norms for not capturing what goes on in the daily work-life of scientists (e.g., Lynch 1997). In our view, such criticism should not be taken to imply that the CUDOS norms are irrelevant, but rather that they are insufficient as an account of the practices of science.

Departing from our focus on the conduct of conduct, norms – and among them norms similar to what Merton described – have an important influence on ideas about the objectives of science and the purpose of the scientific profession. Rather than study scientific norms of professional conduct as a good or bad description of scientific practice, we are studying their performative effects for the governance of science. In this way, the discussions of social responsibility of science as they take place in academic journals are a governance technology, part of 'the complex of mundane programmes, calculations, techniques, apparatuses, documents and procedures through which authorities seek to embody and give effect to governmental ambitions' (Rose & Miller 1992: 175). The journals are the specific, material technology (albeit not very 'high-tech') that allows arguments about responsibility in science to be circulated and read by those who are considered as the objects of governance, namely, scientists, science scholars and science policy actors. It is thus a specific 'apparatus' that shapes conduct and ways of thinking towards a more responsible practice (Miller & Rose 1990: 8).

This is not to say that political rationalities on social responsibility in science are a straightforward phenomenon to study. As Foucault points out, modern governance can have a plurality of specific aims, and these aims can be ambiguous and even contradictory and yet still work on the same object of steering (Foucault 2003a: 237). There can, in other words, be different political rationalities at play at the same time. They overlap, contradict and supplement each other continuously. Furthermore, arguments about enhancing the responsibility of science are pervasive, and they flow in many directions with circular forms. No a priori distinction between arguments coming from 'within science' or from 'outside science' can be made, as this distinction is itself an effect of political struggles.

On this basis, our task in this paper is to map the different political rationalities of social responsibility in science as they appear in contributions to academic journals. We have done that by studying how the 'problem' of social responsibility in science is articulated in various journal papers by asking of a sample of such papers the following three questions:

- How is the specific problem (or problems) about lack of responsibility in science articulated?
- What are the central aspects of science (or its relation to society) that need to be changed according to each articulation?
- What kinds of solutions to the problems are imagined in these articulations and how are these solutions supposed to be put into place?

Through these questions, we want to carve out how spaces of action are constructed as legitimate for scientists and how these spaces differ from each other and overlap. In the next section we will go further into the description of how we constructed an archive that made it possible to map political rationalities about social responsibility and investigate their implied effects on the governance of science.

5.2 BUILDING AN ARCHIVE OF POLITICAL RATIONALITIES ON RESPONSIBILITY

The first step of a Foucauldian analysis is to define and delineate the archive in which to study political rationalities (Foucault 1972/2010). Based on initial readings of a large number of papers, we identified 13 keywords central for the responsibility of science:

- Science policy
- publishing
- public participation
- Research misconduct
- Social responsibility
- responsibility
- upstream public engagement
- risk management
- Ethics
- environmental impact assessment
- science
- moral and ethical aspects
- Public opinion

We combined the keywords into different search strings and the resulting searches yielded approximately 1000 articles.[16] From this collection we then selected all papers that explicitly stated normative ideals about the governance and responsibility of science, i.e., statements about the purpose of science and directions for how science should be regulated or steered towards this purpose.[17] For each of the remaining 263 papers, we summarized the answers to the three research questions above and extracted illustrative quotations.

The papers in our sample are diverse and range from editorials addressing problems such as fraud in laboratory research to journal articles on how science can become more innovative. The sample also includes critical papers on science's damaging side effects and how they can be avoided, as well as debate pieces on how the direction of science could be subject to more democratic decision-making. Some papers are focused solely on the development of their own discipline, while others are concerned with the entire scientific community. Some papers are written by scientists and others by scholars in the humanities and social sciences with opinions about the development of science. In the analysis, we do not distinguish a priori between different speech positions, and so we have not differentiated between papers written by scientists and non-scientists. Rather, we have treated all voices equally in order to understand the total sum of possible articulations of the responsibility of scientists. The archive is therefore quite diverse and the positions plentiful. Nevertheless, all the papers offer answers to our three research questions and, in this way, they all point to a political rationality with implications for how science is governed.

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[16] The searches were conducted in the three databases, Scopus, EBSCO Host and SAGE. Based on the content of the databases, our searches go from 1960 to 2011 and they were done within all disciplinary areas. The selection of papers from the end of the nineties and onwards is noteworthy larger compared to the amount of earlier dated papers. There could be many reasons for this; a growing interest in the phenomenon; more publications in general, etc. We believe that this difference in size is also due to the fact that papers have increasingly been available online automatically, whereas older papers are not necessarily so.

[17] Papers were excluded either, because they did not address the governance efforts at science or scientists (but rather the media or citizens), or they were too descriptive. Many papers were descriptive empirical studies about scientists' engagement in society, for instance studies of how scientists and citizens interact in certain public participation exercises. In a more all-encompassing Foucauldian analysis of regimes of knowledge these should also have been included, but for this limited study we chose to focus on the normative papers.

5.3 FOUR KINDS OF RESPONSIBILITY

Based upon our close readings of the texts, we have identified four different political rationalities, which can be described using a 2x2 matrix. The first dimension describes whether regulation of science should be internal or external; the second dimension describes whether issues of responsibility relate to the process or the outcomes of science. Figure 1 illustrates the four rationalities and their relationship to each other. The Reflexivity and Demarcation rationalities both advocate internal regulation of science but, while the Reflexivity rationality insists that the responsibility of science is to strive for outcomes that can work as solutions to society's problems, the Demarcation rationality aims for a total separation between science and society in order to prevent social norms and values from biasing the otherwise objective production of knowledge. The Contribution and Integration rationalities both point to a need for the external regulation of science, but they also differ in their focus on whether responsibility relates to the outcome of science, as the Contribution rationale proposes, or its process, as the Integration rationale suggests.

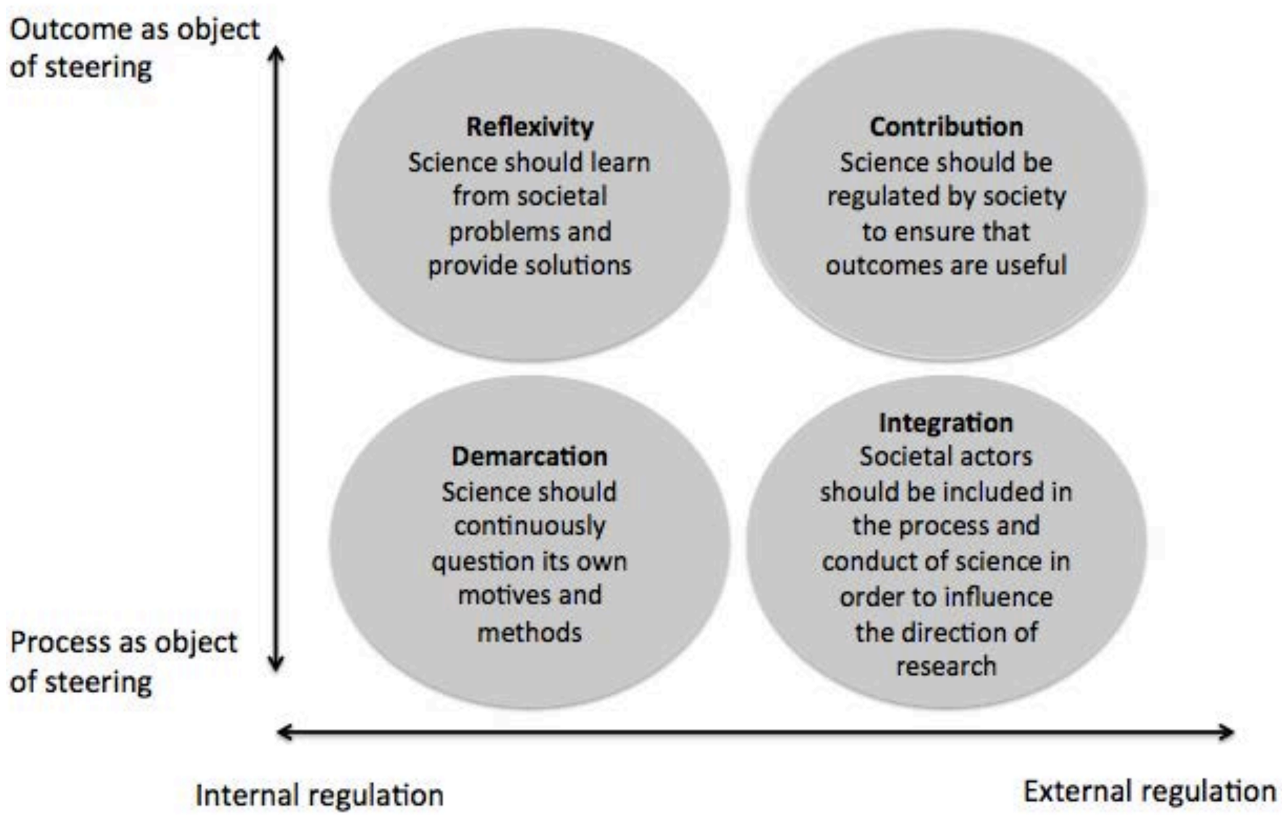


Figure 1: An overview of the four political rationalities. They are arranged in relation to whether they advocate internal or external regulation of science and if they propose the process of science or the outcome of science as their object of steering.

In the following sections, we will describe each of these political rationalities in more detail using illustrative quotations from the papers in our archive. We have chosen the quotations as poignant examples of the patterns found in the analysis, but they should not be understood as ‘evidence’ in their own right outside of the analytical context. Within the Foucauldian analysis, it is not the individual texts but rather the patterns found in specific articulations – in this case the arguments – presented in the texts that matter. When including references to similar arguments in other papers, however, we have used the conventional way of referencing the entire paper rather than trying to reference the specific argument. It should also be noted that these four rationalities are found throughout the archive. As such, the findings are robust in describing a recurrent pattern. However, as Foucault has also pointed out, different rationalities overlap and intersect even though they can seem contradictory. Our description of these four rationalities should therefore be understood as an analytical construct that points to some patterns in the archive, while not offering justice to the full complexity that is also at play.

5.3.1 THE DEMARCATION RATIONALITY

The Demarcation rationality often commences from a pride in science's endeavours, and it considers the job: 'a noble and exciting calling; those who take part in it are fortunate' (Danforth and Schoenhoff 1992: 351). Basically, science is a honourable profession, but unfortunately it is increasingly tormented by fraud and misconduct, which threatens its ability to do good for 'the people' (Danforth and Schoenhoff 1992: 355). This rationality articulates two interrelated problems as the reason for increasing fraud and misconduct in science. First, it worries about the increasing pressure for results and publications demanded of scientists, for instance: 'The number of papers to consider is increasing rapidly, but the space [in journals] available for publication is not keeping pace' (Brice and Bligh 2005: 84). These pressing conditions make fraud and misconduct more tempting for scientists: 'Academic advancement, 'publish or perish', as well as prestige, are other important driving forces [for publishing scientific papers]. Finally, there are many financial benefits (direct and indirect) in publishing such as promotion and further research funding. Many of these forces can lead to ethical lapses.' ((Anderson and Boden 2008: 155); (Caelleigh 1991; Evered and Lazar 1995; Anderson and Shultz 2003; Editorial 2006; PLoS Medicine Editors 2010).

Second, it expresses the fear that increasing incidents of fraud and misconduct will lead to an increasing public mistrust in the capability of science to contribute positively to society. This fear of declining public trust is expressed in Claxton

(2005a, 27), for example, when he states that 'all scientists must be aware of the potential for fraud so that science can continue to pursue truth and serve mankind' (Schmaus 1983; Danforth and Schoenhoff 1992; Whitbeck 1995; Fleischmann 2008; Frankel 2000; Caveman 2002; Illes et al. 2010). Public trust is often described as important for maintaining science as the institution that drives 'mankind' forward, and instances of fraud and misconduct fostered by increasing pressure to produce results are viewed as threats to this trust.

According to this rationality, the solution, i.e., the way to secure the responsibility of science, is to install: 'a moral code that the vast majority of scientists embrace' (Caelleigh 2003: 225) where only strict scientific methods are seen as legitimate (Schmaus 1983; Smith 1998; Heitman and Bulger 2005; Wolpe 2006; Wager 2011; Perlis and Shannon 2012). The reinforcement of such a 'code' is exemplified in the 'Responsible Conduct of Research' education program for young researchers, which should cover 'almost every domain of scientific activity: data management, conflicts of interest, authorship, publication, peer-review, collaboration, mentoring and misconduct' (Roland 2007: 707). A number of scientific actors and structures are enrolled as responsible for fostering this strict culture. The most frequently mentioned are: the scientific advisor, who has the duty to transfer these professional norms to the next generation of scientists (Brice and Bligh 2005; Roland 2007; Seiler et al. 2011); the review system and the journal editor, which have the final responsibility for declining manuscripts due to fraud (e.g., Pittenger 2003; Cain 1999; Slesser and Qureshi 2009); and ethical guidelines, which prescribe good scientific practice and authorship in both disciplinary societies and journals (Caelleigh 2003; Davidoff and Batalden 2005; Claxton 2005; Perry and Mittelmark 2008; Poff 2009).

These actors and structures are all articulated as legitimate means to creating a moral culture that can enforce strict scientific methods. Their legitimacy is described as being rooted in the fact that scientists are all part of the same profession: 'Members of a scientific discipline, like other professional groups, are bound together by similar aspirations, values and training and enter into a community of common purpose' (Frankel 2000: 216). This membership is contrasted to a whole range of outsiders to science who should refrain from instilling or enforcing proper conduct: 'Regulations imposed from outside science cannot promote the kind of atmosphere necessary to ensure ethical practices. An ethical climate must be fostered from within the scientific community' (Frankel 2000: 216; (Rothman and Poole 1985; Wolpert 2005; Marchant and Pope 2009; Martinson, Anderson, and DeVries 2005).

In this way, the Demarcation rationality articulates science as a profession that should have a high level of autonomy from other actors: Outsiders to the scientific realm should not interfere with the discussions about scientists' responsibility and how to achieve it. But the profession itself ought to employ a number of techniques to install a specific kind of responsibility to be honest and objective in every single individual scientist. So the profession's freedom from interference from external actors is articulated as dependent on the internal establishment of a strong professional culture. This internal control system should constantly monitor the members of the scientific profession by scrutinizing methods and results and by socializing aspiring scientists into the system. Only by assuring that each individual scientist is rigorous, honest, transparent and not influenced by society's interest in her work, is it possible to maintain proper responsibility within science (Edsall 1975; Bulger and Heitman 2007; Vollmer 2007; Evans 2010; Wager 2011).

Interestingly, the demarcation between science and society also points to decisions that scientists should not feel responsible for: 'There is a real danger in asking scientists to be more socially responsible - the history of eugenics alone should show at least some of the dangers. For, by asking scientists to be socially responsible, in terms other than the obligations already discussed [to be rigorous and honest in relation to your results], would be to give power to a group who are neither trained nor competent to exert it. It was not for scientists to decide whether or not to build a[n atomic] bomb . Nor will it be for scientists and doctors alone to decide whether or not to introduce genes into the germ lines.' (Wolpert 1989: 943) see also (Rothman and Poole 1985; Fisher 2003; Nüsslin and Hendee 2008; Stieb 2008). In such articulations, the effort to construct a strong internal commitment to truth simultaneously exempts the scientific profession from taking responsibility for decisions on the direction of society, even though the scientists' own insights or inventions are used in these decisions.

5.3.2 THE REFLEXIVITY RATIONALITY

Where the Demarcation rationality clearly praises science as a honourable profession, the Reflexivity rationality is more ambiguous in its appraisal. It acknowledges that science has solved many big problems for society, but it also worries that scientists do not assume responsibility for the wrongs that modern science has also produced: 'Scientists can no longer get away with accepting credit for the glorious achievements of science, but must also respect some of the responsibility for the misapplications of science' (Brouwer 1994: 193); see also (Vessuri 2002; Waelbers 2009; Schuurbiers, Osseweijer, and Kinderlerer 2009). The 'no longer' in the above quotation indicates that the Reflexivity rationality describes a change in the scientific profession. Perhaps it was possible in some distant past to be disinterested and not care for the consequences of scientific developments, but: 'a transformation…has taken place in science since the Second World War whereby [science] can be said to be a social institution and not something engaged in by disinterested seekers after the truth' (Forge 2000: 348) (Ziman 1998; Liska 2004; Waelbers 2009; Schuurbiers, Osseweijer, and Kinderlerer 2009).

Whether scientists appreciate this development or not, they need to assume the responsibility that comes with being a 'social institution'. But, the Reflexivity rationality claims, scientists have not done so to a proper extent and they need to realize this challenge in order to be responsible: 'Attention needs to be paid to recent changes in the research context: the principles of good scientific conduct themselves may need to be revisited and the capacity to address moral issues within research cultures should be addressed' (Schuurbiers, Osseweijer, and Kinderlerer 2009: 230). The strong commitment to the use of rigorous scientific methods, praised by the Demarcation rationality, is not seen to be nearly enough to assure that scientists have acted responsibly. In fact, quite the opposite is seen as true, since science is considered to be a vital part of many societal catastrophes by the Reflexivity rationality. New technologies are not only creating progress, but they also cause bad side-effects, which are largely ignored by science (Cournand 1977; Sassower 1996; Koepsell 2010). What scientists are missing in order to be socially responsible is articulated as a kind of self-awareness, an ability to foresee the consequences of their own practice. For example, Sweeney (2006: 458) considers the lack of reflexive thinking with regard to nanotechnology: 'What appears to be missing at the present time is a clearly articulated prognosis of the potential global social benefits and harms that may develop from further scientific and technological advances'. As long as scientists are not considering how their own practices are affecting society, science cannot be understood as socially responsible (Watson 1974; Studer and Chubin 1977; Strydom 1999; Nicholas 1999; Forge 2000; Beckwith and Huang 2005).

The solution to this lack of attention is to improve scientists' self-awareness and their ability to incorporate such considerations in their research: 'as previously stated, "To act responsibly," means "to act in an inquiring and reflective way"….One has to try to understand how, in the specific situation, the human actors and the technologies mediate with each other, influencing the eventual outcome' (Waelbers 2009: 62. See also Smith 1992; Rotblat 1999; Wing 2003). Scientists thus need to make an effort to foresee – or at least discuss – how their research affects their surroundings. They need to be able to look at their own role as part of a bigger society, where actions have consequences at other places and other times. In other words, scientists need to increase their reflexivity, even though it might not be an easy task: 'While I agree that it is difficult to predict all the consequences of new scientific discoveries, I also have to stress that we scientists are seldom asked to reflect on the long-range effects of our work' (Brouwer 1994: 193).

The Reflexivity rationality describes various techniques intended to help scientists get better at reflecting on their own practices. These techniques are focused on making scientists aware of their own values and motivations, as well as making them reflect on the possible outcomes of their scientific inquiries. This rationality often articulates faith in education as a means to make scientists more reflexive: 'what science education now requires is "metascience", a discipline that extends beyond conventional philosophy and ethics to include the social and humanistic aspects of the scientific enterprise. For example, students need to learn about the…societal responsibilities of research scientists, and to rehearse in advance some of the moral dilemmas that they are likely to meet" (Ziman 2001: 165. See also Kirman 1992; Ernst 2003; Peiffer, Hugenschmidt, and Laurienti 2011). It also points to other methods whereby scientists should adapt a specific attitude towards their work, such as 'co-responsibility' (Strydom 1999), 'social role responsibility' (Waelbers 2009), or 'strong objectivity' (Wing 2003) – terms that all cover the development of more awareness of values, interests, and the consequences for society. But there are seldom any specific guidelines as to how scientists can adapt these attitudes in practice.

Compared to the Demarcation rationality, the Reflexivity rationality adds responsibility to the practice of individual scientists: Scientists not only need to perform science according to the highest standards of quality, but they should also be able to oversee and reflect on the consequences of their own practice. In this way, reflexivity appears to become a sort of add-on: 'Researchers still have a responsibility to produce 'good science' – in two senses: science that is as 'truthful' as the semantic, philosophical and ideological confusions surrounding that word allow; and science that is 'socially responsible' (Scott 2003: 84 see also Pimple 2002). So not only do scientists have a responsibility of finding truths about the world, but they also have a responsibility for assessing if their science is good or does good in society. This rationality indicates a purpose for scientists beyond the one proposed in the Demarcation rationality. The Reflexivity rationality sees part of scientists' task to be attentive to society and its problems – not the least of which are problems that science may itself have caused.

5.3.3 THE CONTRIBUTION RATIONALITY

The Contribution rationality articulates science as a societal institution akin to the healthcare and the education systems. It is part of society and serves certain societal goals: 'Clearly, the aims of science, particularly in the case of the biomedical sciences, are closely linked to certain ethical, social, or political goals' (De Melo-Martín 2008: 39 see also (Brown and Guston 2009; Swierstra and Jelsma 2006; Sandler 2007; Allyse 2010). In this rationality a particular vision of what is good for society is inherent in the specific goals that science pursues. According to the Contribution rationality, it is therefore paramount that society has a decisive role in shaping these visions and goals and that scientists see themselves as working to produce a valuable contribution to society.

The arguments of this rationality center on two societal prescriptions that should guide scientists. The first is that science should be *innovative* and contribute with knowledge and technologies in order to improve national and regional growth. For instance, Heitor advocates stronger European universities in order to improve competitiveness: ‘There are...challenges that still remain in this reform movement to adapt higher education in Europe to the global landscape and to improve funding for R&D’ (Heitor 2008: 611). He suggests that universities adapt models from the financial sector and become more ‘responsive’ to societal needs, as this responsiveness is a precondition for the adaption to a global knowledge economy (Heitor 2008: 609) see also (Etzkowitz et al. 2000; Weed and Mckeown 2003; Beesley 2003; Nature editors 2009).

The second societal demand articulated by this rationality is that of *democracy*. Democracy is here understood to mean that scientists’ activities should be in line with expressed public preferences and that experts’ conduct should be subject to public scrutiny: ‘in a democracy a sceptical and questioning attitude towards experts of all kinds is a thoroughly healthy thing’ (Durant 1999, 317. See also Abraham and Davis 2005; Bubela et al. 2009; Cho and Relman 2010).

Independent of whether the rationality addresses the demand of innovation or that of democracy – or both at the same time – the rationality is concerned with the ‘goals’ (De Melo-Martín 2008) or ‘purposes’ (Rappert 2003: 467) of science. The Contribution rationality measures science’s ability to be innovative and democratic by looking at what comes out of the laboratories – the results and applications. If the knowledge and technologies are out of line with society’s preferences and do not create growth, science has not lived up to its responsibilities. Therefore, following this rationality, scientists can be perceived as a sort of public servant working to materialize the objectives of society in their knowledge production*:* ‘Scientists should conceive of themselves as artisans working for the public good, whose efforts are directed toward an ideal of well-ordered science; and this ideal of well-ordered science should be understood in a global and democratic fashion' (Kitcher 2004: 331).

According to the Contribution rationality, the current problems with responsibility in science is that scientists do not see themselves as these 'artisans working for the public good'. Instead, they consider themselves and their work as separated from society and pursue irrelevant and perhaps dangerous paths that are not beneficial for anyone: 'Curiosity-based research may provide new knowledge, but what can one now do with that knowledge — of what use is it?' (Beesley 2003: 1529). According to this rationality this question – of what use is this knowledge? – should be forefront in all scientists' work, but it is not. Rather, scientists tend to be 'retreating to the safety of the ivory tower' (Drenth 2006: 15 see also Swierstra and Jelsma 2006; Taylor 2009; Werner-Felmayer 2010). Scientists have been allowed to be cut-off from criticism and public inquiry, but this allowance needs to change: 'Knowledge, as Francis Bacon famously observed, is power. If today's enormous scientific-knowledge-that-is-also-enormous-power is to be harnessed democratically, it is essential that it should be subjected to close and careful public scrutiny' (Durant 1999: 317. See also Abraham 2003; Abraham and Davis 2005; Drenth 2006; Brown and Guston 2009; De Melo-Martín 2008). Scientists' results are thus too important to be left to the scientists, but scientists are, seemingly, unwilling to be under public control.

To change this problem, the Contribution rationality proposes to enhance outside (societal) control over scientists. According to this rationality, the scientists do not have the ability to become more responsible by themselves (von Hippel 1978; Redman and Caplan 2005; Bates et al. 2010; Underwood 2009; Sandler 2007). Therefore someone outside the system needs to intervene. In line with the Demarcation rationality, the Contribution rationality argues for more control within science, to avoid fraud and misconduct. Contrary to the former, the Contribution rationality articulates a strong need for external control. As with other public servants, i.e., doctors and teachers, the misuse of professional status should not be tolerated and, in fact, needs to be punished: 'Lack of criminal sanctions for scientific misconduct appears to create an elite class of persons who are exempt from punishment for cheating, stealing and outright lying' (Redman and Caplan 2005: 248. See also Lancet Editors 1996; Andersen 1999; Riis 2001; Bosch 2010; Miller 2011). Various kinds of external controls with results must therefore form part of scientists' work life.

The Contribution rationality also articulates a more general need for public scrutiny of the directions of scientific inquiries. The rationality calls for improved governance in a range of areas such as potential patent possibilities, industrial potential, environmental harm, and (as in the following quote) the risk of 'dual use': 'A clear normative articulation of acceptable and unacceptable behaviour would therefore contribute towards improved governance. Currently there is a lack of international criminalization of individual activity in relation to biological weapons production that might allow actors to rationalize their choices' (McLeish and Nightingale 2007: 1649). Thus, the rationality demands public control from different kinds of external bodies in order to assure that scientists are more innovative, that they collaborate more with industry, and that in general they aim to fulfil the public's stated preferences when aiming for various outcomes of science (see also von Hippel 1978; Baylis and Robert 2006; Drenth 2006; Bubela et al. 2009; Underwood 2009; Taylor 2009).

In the Contribution rationality, scientists have a responsibility to deliver results that are wanted by society. Science should not just pursue knowledge for the sake of curiosity, but it needs to contribute to society. The purpose of science is to be at society's service and scientists need to be focused on this. Since scientists cannot, however, be expected to do so on their own accord, their conduct needs to be overseen by non-scientific actors, who are perceived to be more able to sustain responsibility.

5.3.4 THE INTEGRATION RATIONALITY

The fourth of our identified rationalities is similar to the Contribution rationality in so far as it articulates that science is supposed to be firmly rooted within society, but it does so in a different way. The Integration rationality is centered around the vision, that actors from science and society need to work together as equal partners in order to produce better results: 'The exposure of citizens, public interest representatives and scientific experts to each others' perspectives might contribute to a transformation of how these different participants define their interests or take account of others' interests' (Abraham and Sheppard 1997: 163). Here, scientists are conceived of as actors with special experiences who, in collaboration with other actors, can develop solutions for society (see also Ball 2002; Nowotny 2005; Roco 2006; Elshtain 2008; Horton 2010; Vogt, Baird, and Robinson 2007). It is the collaboration between different actors that is crucial in this rationality. While the Contribution rationality is focused on the outcome (that science lives up to the objectives of innovation and democracy), the Integration rationality does not seem to have a fixed societal objective. Rather, the goals for science and society should come out of a process in which scientific and societal actors agree on the preferred objectives together. Whereas, democracy is also a strong value in this rationality; it is the process of discussing, rather than public oversight and control that is seen as crucial.

In line with the previous two rationalities, the arguments of the Integration rationality take as their point of departure the negative side-effects of science when describing the main problem in current scientific conduct. Science is articulated as producing outcomes that are unsustainable and controversial, because scientists work without a thought for the wider societal implications of the knowledge and technologies they create: ‘Given the environment in science, scientists, on the whole, are unlikely to participate in soul-searching over the consequences of their work. Those who argue for the social implications of their findings may do so without any framework for thinking about the consequences’ (Beckwith and Geller 1997: 147. See also Beggins 1978; Cohen and Gotlieb 1989; Schuurbiers et al. 2009; Thorpe 2004; Woollard 2006). According to this rationality scientists, do not themselves engage with other actors and thus do not experience ‘a transformation’ (Abraham and Sheppard 1997: 163) that could lead scientific developments in more substantially democratic directions. Rather than pointing to a lack of internal considerations (Reflexivity) or a lack of external control (Contribution) as the key reason for these problems, this rationality points to the lack of integration between science and other actors in society as the main problem to be corrected.

In the Integration rationality, the solution is to enhance the dialogue between scientists and other actors in order to develop a new kind of ‘integrative’ responsibility that can transgress today’s very specialized society: ‘Genuine responsibility is not to be found in the compartmentalized roles of the professional, expert, scientist, government official or career politician. It is, rather, in the cracks between such specialized roles that the basis for an integrative sense of human responsibility is to be found.’ (Thorpe 2004: 79; Steckler 1973; Ritterbush 1977; Schrag et al. 2003; Rip 2009; Mikulak 2011; Roco et al. 2011). Within this rationality, responsibility is thus an outcome of a process in which several different actors meet and together learn and change based on common deliberation. Consequently, science needs other actors – lots of other actors – to become responsible. Scientists need to be challenged continuously by different view-points so they can integrate them into the development of knowledge and technologies. Within this rationality, several techniques are proposed to expose scientists to society's values: Scientists need training in philosophy and social science during their education in order to be more aware of the importance and works of norms and values; scientists need to engage in various public-participation exercises, in which emerging technologies are discussed with citizens (Miah 2005); scientists should be better at communicating with public media (Fischer 1999; Evans 1999); scientists need to develop their methods in cooperation with those whom they study, so they can develop knowledge together (MacKenzie, McDowell, and Pittaway 2007; Hugman, Pittaway, and Bartolomei 2011). Scientists need to engage with these various other perspectives to jointly and collaboratively find a way forward.

The Integration rationality articulates as a crucial point that these dialogues should be on-going and happen 'before the fact', that is, before knowledge and technologies are finalized and implemented in society, at which time it becomes more difficult to change their properties and the way they affect their surroundings. Instead, it is important that various perspectives on emerging technologies are surfaced, while they are still in the making, so they can be integrated as development is on-going: 'Public engagement processes should be established early so that stakeholders who will bear the risks and benefits of synthetic biology have the opportunity for meaningful input into the trajectory of this field. To be meaningful, public engagement must recognize that some avenues of research will not be acceptable and some products may be prevented from reaching the market' (Bubela, Hagen, and Einsiedel 2012: 136; Schuurbiers et al. 2009; van der Burg and van Gorp 2005; Roco 2006; Fisher 2007).

According to the Integration rationality the main responsibility of scientists is to develop knowledge that is aligned with society's norms and values, having first realized that these norms and values should be contextually identified. The rationality sees scientists as a special kind of citizen, one who possess specialized knowledge that can be used to develop society in better directions. But this is only possible if science opens up and allows social concerns to form part of the scientific process, instead of focusing only on technical aspects. In this way, the Integration rationality diverges from the Contribution rationality by articulating that knowledge production is collaboration among different actors and responsibility is something that develops through this collaborative process.

5.4 CONCLUSIONS

Analysing 263 contributions to academic journals, we have identified four different rationalities of the social responsibility of science. Each of these rationalities articulates a specific way of defining problems, as well as a specific way of legitimizing certain political steering mechanisms as solutions to these problems. The analysis illustrates a wide variety of views on how science and the scientific profession should be governed. The proposals range from strict self-governance and autonomy to a very integrative view in which a large group of different stakeholders from outside the scientific system should be involved in the conduct of science. The Demarcation and Reflexivity rationalities both articulate scepticism towards external involvement; whereas, the Contribution and Integration rationalities embrace external regulation, albeit in different forms. In this way, we could argue that the four rationalities seem to stretch over a continuum from an idea of no involvement at all from society, to an idea of radical involvement from society in which citizens, social scientists and other actors literally enter the laboratory and co-develop knowledge.

A different pattern emerges if we focus on the object of steering. The Demarcation rationality focuses on optimizing the scientific process before-the-fact by installing a strict moral code among the scientists that should be focused on honesty and accuracy in their work. In this way, it resembles the Integration rationality, which also has as its object of steering the scientific process before-the-fact. Even though the two rationalities are radically different with regard to their choice of internal or external regulation, they are focused on the process of science as the crucial object of steering in order to make science responsible. In contrast, the Reflexivity and Contribution rationalities share a focus on the outcome of science as the important object of steering, although they differ in terms of the steering mechanisms they want to use. While the Reflexivity rationality describes how scientists themselves should use society's problems as an inspirational framework that guides their research, the Contribution rationality advocates firm, external control and guidelines from society that scientists should be compelled to follow.

When looking at the four rationalities in their totally, we can also reflect on the distribution of morality or ethics in each rationality. On one hand, the Demarcation and the Reflexivity rationalities articulate science as a fundamentally 'good' institution that has an in-built capacity to know how to serve society best. On the other hand, the Contribution and the Integration rationalities point to society as a necessary source of moral knowledge about how to develop 'a good society' from which science needs to learn. This perspective strongly reverberates with social studies of science and technology that advocate further involvement of philosophers and social scientists as teachers of responsibility in scientific processes (e.g. Macnaghten et.al. 2005, Fisher 2007, Flipse et.al 2012).

Following Foucault (2003a) we do not consider the four political rationalities as mutually exclusive or strictly separated. From a very general perspective, they all stress the need to regulate the relationship between science and society. Even in the Demarcation rationality, society plays a large part as that which has to be excluded. The definition of a boundary (or an integration) between science and society is therefore an overall shared problematization in the four rationalities. In this way, the analysis portrays the current situation of the profession of science as one in which it is impossible to *not* ‘have a relationship’ with society. Rather, the main political question is how to define and regulate this relationship.

The identification of these four rationalities serves as a map of contemporary ideas on science governance. Like any other map, it excludes a lot of shades and details, and we might have enlarged certain differences in order to make the map intelligible. However, the map is intended as a reference point for directions and the understanding of differences, and as such it is useful as an overview of directions in the governance of science. It is clear that there is no uniform agreement about what social responsibility of science is and should be. It is equally clear that the definition of this concept is inherently political – in the Foucauldian sense of structuring fields of action. Similar to Braun et al. (2010), our analysis demonstrates that a particular definition of responsibility also implies a particular understanding of the proper conduct of science, sustaining the description of some forms of practice as responsible and others as irresponsible. Following from this perspective is also the realization that an argument for more responsibility in science is not a way of dealing with the fact that science has political consequences – it is itself a political statement.

However, our map does not make us any wiser about the practice of science and the relationship between what scientists actually do in their laboratories and the normative and political statements about proper conduct of science investigated in this paper. As seminal laboratory ethnographies (e.g., Bloor 1976; Latour and Woolgar 1979; Knorr-Cetina 1999) have shown, scientific norms and scientific practice are at times worlds apart. We therefore propose further studies of this relationship – in particular of the ways in which scientists are influenced – if at all – by these political rationalities in their daily practices of making facts in the laboratory and making organizations when doing research management. It would be interesting to investigate whether there are more or different rationalities at play in these forms for practice. It is also pertinent to explore how these proposals of responsible conduct of science are played out in connection with such mundane organizational concerns as next year's budget, a debate piece in yesterday's paper, a failed experiment, or the need for a new coffee machine in the canteen.[18]

[18] We would like to thank Emil Husted for great assistance with the building of the archive and development of the method. Furthermore, we want to thank Alan Irwin, Erik Fisher and Paul du Gay for reading earlier drafts and provide useful comments.

6. PRELUDE TO VOCATION, OIKOS AND CITIZENSHIP

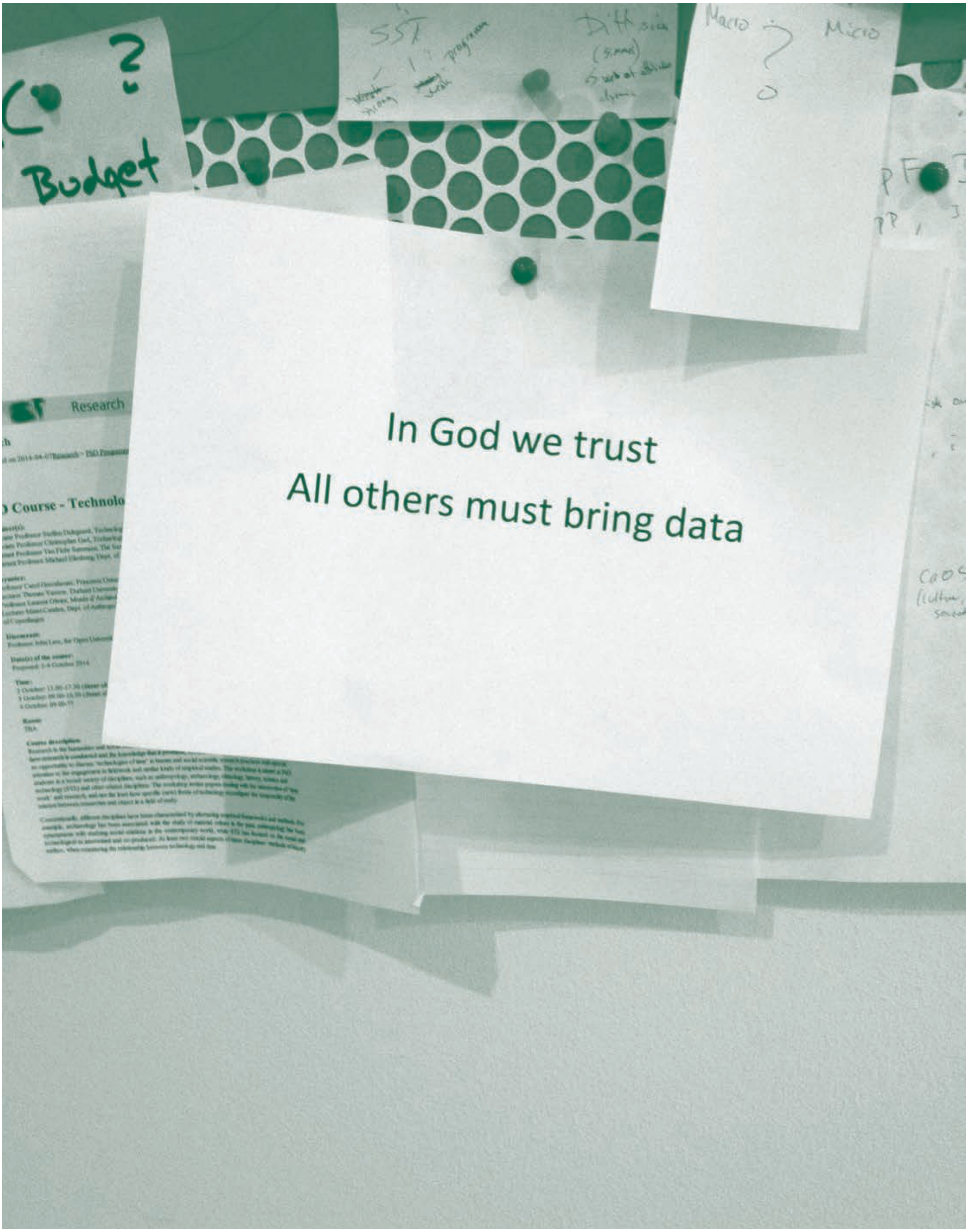
In the next three chapters, I will present an analysis of what scientists consider to be their specific, daily responsibilities, why they consider them to be so and how they try to live up to these. The analysis is based on the ethnographic studies of two research organisations, Curious George and Gyro Gearloose, but, as I will touch upon in the concluding discussion (chapter 10), I believe that the results can be useful in other research contexts as well.

Each chapter presents a specific 'mode of responsibility'; that is, a specific way of perceiving and conducting a responsibility, which is shared among the observed scientists and which generates some specific understandings of the general organisation and its surroundings (see also 3.3.2). I have described each mode as if it were 'pure' (Law 1994: 6f). That means that each chapter is written as if the scientists only had *one* responsibility and everything they did was connected to this one responsibility. This is of course not how their daily work life looks in reality. I identified three different responsibilities (it is possible there are others that I did not detect). These three were constantly performed and negotiated. In every chapter, there is a section near the end in which I try to demonstrate how the different modes are negotiated and where I have no ambition of being 'pure'. This allows us to see how these modes contradict each other and create difficult situations. Their separation is an analytical construct that I have made (inspired by Law 1994), in order to be able to describe each of them in detail and also to demonstrate that inherent in each of them is a complex set of worldviews, understandings and practices that are all interconnected. Finally, I also want to emphasise their differences and it seems like these differences stand out more clearly if I describe them in their entirety, one by one. In doing so, it almost seems (and that was how it felt) that the three modes are descriptions of three entirely different laboratories, even though it is a mixture of the same two laboratories that I describe all the way through.

The two laboratories are called Curious George and Gyro Gearloose. They have received these names because these figures, both of which are cartoon characters, remind me of them. Both laboratories are working in the field of synthetic biology. Curious George is located in Denmark and is part of one of the big universities there. It has changed name a couple of times due to mergers and reforms but is, in any case, quite old. Most of the staff occupy temporary positions or hold their own temporary grants. There are a few permanent positions, but they are so few vis-à-vis the numbers of employees that it does not even seem like the scientists compete for them. All of the scientists are occupied with plant chemistry in one way or another, mainly gene modification of some type. The scientists take pride in being curious, doing '*solid basic research with a product on the horizon*', as one of them puts it, and they also explore '*where no one has been before'*. At the same time, Curious George has a long tradition of having close relationships with its users, such as plant breeders and foresters. This identity seems – as I will demonstrate – quite important for their ideas about responsibility and that is why I have named it Curious George.

Gyro Gearloose is located in the USA. It is a fairly new laboratory and part of a fairly new initiative at the university, to which it is attached. A range of laboratories has been established under the same roof and Gyro Gearloose is one of them. They are independent of faculty, they rely 100% on external funding and they are assembled under the mission of making 'translational research' in the field of advanced biotech; that is, connecting basic research with revolutionary, tangible technologies. Gyro Gearloose takes that mission very seriously. The lab director explains that they are '*inventors'* and that they continuously strive to live up to such a designation by trying to invent technologies that sound impossible; for instance, a vaccine that will cure cancer – one of their goals on their webpage. They are so forward-thinking that they also meet much resistance in the scientific community, but that only seems to make them more determined in their pursuits. As with Curious George, the identity that is connected to the form of science they do seems important for the way in which they consider their responsibilities. They have therefore been given the name Gyro Gearloose, after one of the most famous inventors ever.

7. VOCATION


In God we trust
All others must bring data
Budget
Research
Macro
Micro

This chapter presents the first 'mode of responsibility', which I have chosen to call 'Vocation'. The description of a '*scientific vocation*' is by no means a new one. Weber described it as early as 1917. Many of the noteworthy, ensuing works on the scientific profession have at least partly been comments on Weber's classic lecture and essay (e.g. Merton 1970, Merton 1973, Rabinow 1996, Shapin 2009, Law 1994).

Of those, perhaps Merton has been most occupied with describing the scientific vocation as a responsibility to society, as reflected in the 'CUDOS' norms – Communitarianism, Universalism, Disinterestedness and Organized Scepticism – which he derived from interviews with scientists (Merton 1973). As mentioned in the theory section, the norms were later criticised by the '*second wave of science studies*' (Collins 2014) for being too idealised and far removed from practice. As we shall see in later chapters, there are other norms that also guide conduct in scientific work. But in relation to duty and responsibility connected to the scientific profession, ideals very similar to the CUDOS norms and the idea of a 'disciplined pursuit of knowledge' play a huge role – both as ideals that the scientists strive to live up to and as a practical, embodied way of conducting one's work on a daily basis. Law (1994) describes what the vocational, performing agent 'scientist' (or persona) looks like. While he focuses on how these responsibilities are learned and conducted locally rather than describing them as an institution (in contrast to Merton), the two share the focus on the combination of exploration and conservatism:

'[…][t]*he scientific agent is told of and performed as a puzzle-solver who seeks solutions that are both creative and conservative* […]. *Distinctively it [the mode] speaks of the importance of the roles that are played in this by the body and the eye, of the tacit knowledge acquired during the course of professional training, which comes to shape both perception and action. . And it tells of the need, but also the* *difficulty in incorporating these ways of seeing and doing into the body of the person*.' (Law 1994: 81, original emphasis)

I have chosen the name 'Vocation' for this mode to underline the close connections with earlier descriptions of scientific conduct. As Law suggests, the mode combines the conservative and the creative. It does so by sticking to some very strict norms about what good scientific work process should look like on the one hand and emphasising the possibilities of new discoveries on the other. It is also suggestive of the hard work on mind and body that a practitioner has to perform in order to live up to the dictates of this 'vocation'. It is very much this work – and the results – that I explore in this chapter. As 'responsibility' is the overarching theme of this study, the way I look at the scientists' conduct will continually be connected to how they see this way of doing their job in relation to their idea of the role of science in society.

Besides sharing similarities with Weber, Merton and Law's analyses, this mode also shares similarities with the 'Demarcation Rationality' and the 'Reflexivity Rationality', which I described in the previous chapter 5, a point I will return to at the end of this chapter. Despite the fact that the observations in this chapter thus may not seem very original, they are nonetheless important because the scientists consider the development and adherence to them as their foremost professional responsibility. They are also important, because aspects of this conduct are not seen as having value by some of the proponents of 'Responsible Science'. Specifically, the idea of 'Disinterestedness', to deploy Merton's vocabulary, is seen as counterproductive to pursuing the goal of a more 'responsible' way of conducting science. Some of the texts from the archive I analysed in chapter x, considered this neutral 'technical-rational' thinking of scientists as one of the reasons why more external regulation was necessary (e.g. Fisher 2005; Owen et al. 2013; Schuurbiers and Fisher 2009). The ideals of Vocation are not considered very compatible in regard to responsibilities such as democratising science and concerns for society's general development. These tensions will be discussed toward the end of this chapter. First, however, I will show how Vocation generates specific forms of conducts and justifications.

7.1 CONDUCT

I'm entering one of the small PhD offices, where three students are crammed together with big, square desks, heavy bookcases, battered bikers' helmets, a bunch of jackets and scarfs, humming computers and potted plants in various degrees of erosion. Miriam looks up from her screen as I come in. 'Oh hi', she says, 'Do you care to come and take a look?' She waves at two grey images on her screen. I approach. 'Can you see anything here?' she asks and points at the upper image. I squint at the almost entirely mono-coloured picture: 'Uhm, perhaps I can see some faint darker grey lines here in the middle?' I volunteer. 'Good', Miriam sighs relieved, 'If you can see them as well, then perhaps they are really there, but I guess I should do the whole thing again, just to make sure.'

This story is from one of my last visits to Curious George. By that time, I was not even that surprised anymore about being asked a question related to their specific and highly specialised work. That happened more often than expected, considering that I knew little about the technical specificities of each of the scientists' projects. But in both laboratories they would ask whether I could see 'something' on a screen; or whether I had an opinion about the best way to do this or that; or whether I thought their methods were correct. But compared to how much they asked each other, my ration of questions was far from impressive. During a workday, I would observe people constantly moving away from their screen or laboratory desk to ask someone else their opinion or advice. Juniors approached seniors. Seniors approached other seniors. Juniors and seniors approached people with special expertise: For instance, the guy who had the most experience with a specific machine, or the woman who had conducted this or that experiment many times before. One of the conducts that I will focus on in this chapter is that of ‘checking’.

Another conduct is ‘repetition’. In the story above, Miriam concludes that she probably has to redo the whole experiment again. This is also something that has taken up much space in my field notes: Scientists doing the same procedure again and again and again. I have encountered people recounting their flasks, re-writing the ciphers, re-examining their infected plants or mice, re-inspecting that infected entities were separated from non-infected ones, checking up on all ingredients of an experiment and re-readings of papers. This I will also return to in due course.

Finally, we move on to the last form of conduct: being critical of methods and results. Returning once again to the story above, Miriam seems all in all quite critical about the existence of the darker grey lines in her images. She wants me to confirm them. She is not sure ‘*if they were really there*’ and she wants to do the experiment that led to them again ‘*just to make sure*’. This is not about Miriam being particularly insecure about her own abilities; quite the contrary, Miriam seems quite self-confident. It is a particular, stance in relation to their work that people adopt. People question their own results and motives, scrutinise their own actions and place disclaimers at the beginning of texts. And perhaps they will, as part of this, also repeat their own experiments and check up with colleagues. This third aspect will also be studied later in this chapter.

In the story about Miriam three different forms of conduct that I attribute to the order of Vocation are identified. The three conducts are closely interwoven – as they are in the story about Miriam. In fact, perhaps the scientists would not distinguish between them, as I do. They are in that respect analytical constructs, which I have assembled in order to show characteristics of the mode Vocation. In the following sections, I will return to each of them and describe them in more detail. Then I will connect the conduct to specific justifications for why this particular form of behaviour is desirable. Finally, I will show that Vocation has specific effects on the overall organisation of the work in the two labs notably how it generates specific ideas about the scientific profession and the role of science in society.

7.1.1 CHECKING

The encounter with Miriam and her possible faint grey lines is not the only time I have observed people checking up on results with colleagues.

One of the juniors at Gyro Gearloose sits solemnly in front of her screen. She is going through numerous power point slides. On them are colour diagrams that shift from ice blue to orange and deep red. The blue colours indicate that the test shows a healthy person, the orange and red ones that there is a risk of contamination with valley fever[19]*. 'I'm going through the presentation for tomorrow', she tells me. 'I'm presenting for the group in order to make sure that my results are correct.' The next day, at the presentation, they go through the details of her experiments and her results. All aspects seem to get turned inside out: 'Why use milk as buffer?'*[20] *asks one of the attendants at some point – he is not trained in lab work but in advanced statistics, however, all questions seem welcome.*

This is business as usual. In both labs, people present their work and their progress internally; sometimes to their closest colleagues, sometimes to the entire laboratory. They do this not just because others might find it interesting or that it could spur new collaborations with colleagues, although these are important aspects as well, but also

[19] Valley fever is an infectious disease caused by a fungus endemic in the Southwestern part of USA and Northern Mexico.
[20] 'Buffer' is a dissolution used in chemical experiments, in which it is important to keep the pH-value constant.

to check that all the results are 'correct' and that the experiments have been conducted as they should. Some of these meetings are quite informal. They are agreed upon by a specific group of colleagues. Perhaps the presentation forms part of a meeting, where other items are also discussed, perhaps it is part of doctoral supervision, if it involves assessing students' results. At other times, the presentations are more formal. Both labs have a weekly seminar (at Gyro Gearloose even several) where the scientists take turns presenting their results. At Curious George, the juniors talk a lot about these presentations, which are called 'CRAMS'. When it is their turn to do a CRAMS, they talk about how well people have done at the last CRAMS, or they gossip about those who have made a bad impression with their research – be it juniors or seniors. The way the juniors talk about this seminar made me think about a rite of passage. It is something testing and significant that they all have to go through. There are risks of both shame and failure, but also of glory and fame. And the juniors all seem equally unsure about how their own presentation will go beforehand. Of course, the glory and fame – which is perhaps not as great as it sometimes sounds – does not only come from having correct results. I suppose they should also present some interesting and original results to obtain that. Furthermore, they probably also have to do it in an arresting way. Shame, on the other hand, originates from sloppy work and thus incorrect results. It is quite evident that the juniors fear that their work will be looked upon that way. The seniors never talk about CRAMS in that way. They are supposedly quite used to being scrutinised in this manner and therefore do not fear any shame. It is my impression that the presentations also work as a way to teach newcomers (juniors and other newly-employed people) about both the quality standards at the lab and how checking should be done. The seminars at Gyro Gearloose and Curious George therefore work as important places to learn about proper conduct in the mode of Vocation. As a sort of side effect, they also distribute (at least temporarily) local glory or shame to those who present, at least at the junior level. Shame, as I will also show in some of the coming sections, is inherent to the mode as a way of regulating individual behaviour.

The exposure to criticism and collegial scrutinising of results and methods starts early on. I attended the final seminar for a bachelor student, who had done part of her lab training at Gyro Gearloose. She had brought homemade cake, she squirmed and smiled more than the students I have seen and there were fewer people in the audience than at the normal seminars (only her two supervisors, her two friends and me), but otherwise the scene was the same. She made a PowerPoint presentation of her experiments and analysis. The supervisors went through her experiments and results and asked about all the methods, how it was done and how she got the results. Afterwards, we ate her homemade cake and chatted. When the scientists are further along in their careers, there may be more audience members and the cake is replaced with champagne (or just black coffee if it is an ordinary seminar), but the ritual is basically the same from bachelor level onwards. These seminars are one of the ways that the scientists check up on each other. But there is a lot of informal 'every-day' checking going on as well. The first story with Miriam and the faint grey lines illustrates this point well.

I'm sitting in one of the wet labs, where Jack is working. He has just complained that he has made a mistake and has to redo some of his work again. His supervisor comes in and looks at the progression. He examines the three Petri dishes on the table closely. 'This is not what it's supposed to be,' he says and opens one of them and sniffs. 'Here, you can smell it.' He hands over the Petri dish to Jack. 'Can you detect that sour thing? That's mucus. It's contaminated.' They laugh, but Jack still looks annoyed as his supervisor leaves. He turns to me: 'Do you know what mucus is?,' I shake my head. 'It's snot,' he states, flatly. 'I've sneezed into the goddamn Petri dishes. Now I have to redo even more work…'

I often encountered this supervisor/student relationship. Good supervisors come around and check up on their students' work. They have more experience with experimental work; they can distinguish snot from E. coli simply by the smell. Supervisors often just show up, see what their students are up to and then lead them in the right direction in very tangible ways. Those supervisors who are considered '*good supervisors*' check up in a calm and respectful way. When I followed the seniors, who often sit in their offices, juniors would constantly come in and ask questions about everything from sentence constructions in papers to the status on chemical supplies. As one of the seniors, Sandra, at Gyro Gearloose explains to me:

'I learned a lot from one of the seniors in the lab, where I was a graduate student. That's why I really try to support and advise her [a bachelor student, ed.] – *and also help you.*[21] *How else should you guys learn your way around?'*

However, this is not a relation reserved exclusively for the supervisors and students. It is a way of going about your work that I encountered at all levels. At Curious George, many of the scientists share an office, often two to a room. As I sat there, I saw how the scientists constantly ask each other for advice, make each other check numbers or formulations in their papers. At times, checking up is extended beyond the members of the lab as when the members are at conferences or external discussants are invited to present or assess PhD students, associate professor applications, etc. One of the scientists at Curious George told me that she at times writes to the authors of published papers and inquires about their methods – and that she has kept all her own notes from her lab work in the eventuality that somebody may write to her. Something she both hopes for and dreads, because on the one hand

[21] I often got the impression that the seniors considered me as yet another young pup, whose 'up-bringing' as a valuable member of academia was their responsibility as well.

it would be an honour if somebody were that interested, but on the other she is afraid that her notes are too sloppy.

Those ways aside, the most common way of checking is the self-monitoring, which the scientists constantly exercise.

Today I'm following Catherine around. We are in the basement of the building where the plants for her next experiment are kept in a cool, dark room. She is going to infect half of the plants with mildew fungus and half of them will remain untouched. She explains the process –partly to herself, partly to me – during the process of infecting the plants. 'Infected plants to the right, healthy plants to the left.' She repeats this phrase many times. 'I really have to remember,' she explains to me, 'it's so important that they are not put together. They look the same with and without infection at this stage, and they have to be in the same climate chamber after infection to ensure the same light regimen.' She continues the work. 'They are to the right,' she reassures herself, as she puts another potted and infected plant on a rack. She writes it down in her lab notes so she can remember for later. Then she counts the plants on the two racks. It is as it should be: Half of them to the right, half of them to the left. She still looks a little concerned as we leave the room and she casts glances back at the two filled racks.

Much of the checking is actually mostly directed at the scientists themselves. They are very careful in everything they do and constantly just look again. In that way, it takes immense concentration to do fairly mundane tasks such as filling ten flasks with liquid or measuring the amount of buffer correctly.

Annie is sitting in front of her screen as I approach. She shows me some graphs that she is currently working on. According to her, they do not look as they should. She is *going to go through all the numbers to check up – or otherwise do the statistics all over again, because something seems wrong. She wants to check whether it is the samples or the techniques that are causing the problems.*

The scientists, first and foremost, check up on their own procedures and results to see whether they seem reasonable. They go through their papers many times before they show them to others. They check the statistics, the measurements, the way they have positioned their samples, the timer on the fridge, the oven or a third device, and they write the measurements down, so they can return and check on their former selves. This constant checking up on their own behaviour seems to take a lot of intense work:

I have chosen to assemble a range of observations under the heading of 'checking'. One could also interpret some of the examples as 'helping each other'. The good supervisors help their students in the lab; scientists are given the opportunities to practice presentations at internal seminars; good colleagues help each other write papers and handle the complex and stubborn machines. I consider both interpretations quite valid. It depends on the angle of research. Davies and Horst(In review) point out that the notion of 'care' in scientific work has been neglected. They suggest that the feminine connotations to such notions as 'care' have made it difficult to focus on such interpretations in a world dominated by (traditionally masculine) discourses about 'objectivity' and formal rules. Their paper shows, in contrast, how research management is also very much about 'caring' for the research group and 'caring' for the juniors (Davies and Horst In review). Agreeing with Davies and Horst, I do not suggest that the 'checking' should be understood in a way that excludes friendships, wishes to help a colleague or care for inexperienced PhD students. Rather, I suggest that 'care' and governance in the form of checking up are not mutually exclusive. They can, in fact, be two sides of the same coin.

The reason that I have chosen to focus on the more disciplining aspects of the examples is that the examples resonate with the descriptions of responsible conducts as I encountered them during my work mapping social responsibility in science, as shown in chapter 5. Many of the texts[22], which I categorised as '*Demarcation Rationality*', state that internal control with methods and results are vital. As described, the scientists consider it paramount that science is autonomous, but governed by strong, internal ideals so as to ensure integrity. Here, they often name the daily, mundane processes such as peer review, supervision, presentations and collegial supervision of work as ways of enforcing these ideals. Seen in this light, the many encounters that I have described, (also) become instances of checking up on fraud and laxity.

7.1.2 REPEATING

I will now return to the story about the faint grey lines from the beginning of the chapter. Besides checking, that story also illustrated another vocational aspect, namely, the repetitive character of the scientists' conduct. Miriam was still not completely sure that the grey lines really were in place, so she wanted to do the experiment again '*just to make sure*'. I have often observed the scientists repeating their actions. People do the same experiments over and over again and they check and re-check the single elements of the experiments again and again. I once helped one of the scientists give numbers to 1080 small plastic flasks that she was going to use for her next experiment. We counted and counted and counted. And we re-counted the numbers four or five times. And I am sure that she did some additional re-counting when I was not present. I got the impression that she thought I was a bit careless with numbers. Often the repetition also takes place because the scientists suspect that

[22] I often claim that in the overall picture, most areas of the sciences are represented in each of the political rationalities, but I must admit that feminist theory is quite absent from the Demarcation Rationality.

something may be wrong (as in the re-counting of the 1080 flasks), or because they have actually discovered that 'something' is indeed wrong:

Josh tells me about an experiment he is currently doing. He has redone it six times and each time something has gone wrong. In one instance the enzyme, which was supposed to cut the gene, did not do so precisely enough. Another time, he did not succeed in getting the whole gene transplanted into an E. coli bacterium: 'To optimise these processes…It takes a week every time, or a week and a half. And every time, you have to do the whole thing again,' he sighs.

The scientists '*optimise*' their work all the time. And that optimisation demands the repetition of what they have done. They often express quite a lot of frustration about this aspect of their work. They complain about an experiment they have worked on for weeks because small things keep going wrong. Or they are grumpy because they simply do not know what is wrong, but they have for some reason not been able to get the same (good) results as the first time they tried it. Repetition can take place for a lot of reasons, and despite the scientists' frustration, it takes up much of their workday.

Angela is pacing back and forth between one of the fridges and her desk in the wet lab. She is finding materials for an experiment. 'It's really routine today,' she tells me. 'I've done this experiment a hundred times before. It was even part of my Master thesis and I've published it. But our partners in China have not been able to post-test it. I don't know what the fuck they have done wrong. But anyway, now I'm just doing it one more time to show them that it's correct.'

While verifying may not be the ideal way of doing science according to Karl Popper, this happens quite a lot on an everyday basis. Other labs are unable to confirm experiments and repetition is needed. There could be various reasons why work has to be repeated, and various actors who demand it: The scientists themselves, supervisors, reviewers or external partners at other institutions. But no matter who it is, back the scientists go into the lab and try again. As Clark comments in relation to the many, repeated experiments, only half jokingly:

'I believe that there is perhaps some kind of brain imbalance in scientists. We miss the receptor in the brain that normally is responsible for the transmission of frustration.'

But they do actually complain a lot, and they do seem frustrated. Perhaps they use these kinds of stories about '*scientists*' not being frustrated to keep themselves from falling apart from frustration: In that interpretation, a good scientist is one who just starts again, patient and without complaints. In my notes, my initial thoughts in relation to that comment were that another explanation could be that the scientists lack the brain receptor responsible for quitting something, even though it makes them extremely annoyed. In other words: They appear extremely stubborn rather than eternally cheerful.

Repeating work is considered a quality. I have mostly observed that attitude in relation to experiments and journal paper writing, but not exclusively. At times, the attitude will also surface in relation to other tasks. For instance, Mike told me, how he tries to educate his juniors about science communication. He told me that he wants his students to be as rigorous with articles for the general public as they are with writing papers for scientific journals:

'I ask them [the PhD students about a claim in their text]*: "Is this true"? And the students answer "no, but nobody knows". To that, I point out that "you know" and*

then I make them sit down and rewrite the thing over and over again, until it is exactly true. In the end, they spend as much time with science communication as they do with experiments and papers.'

Repeating work in order to refine it until it is 'true' is, in general, considered a value. That also became evident when they at times asked me about my methods. They would question how I validated my observations if I was not able to repeat them. Or they would comment that it seemed weird that I could conclude anything, when they (themselves) are not separated from the rest of the world and are fed with the same inputs several times. These comments always made me squirm. On the one hand, I did not want to go into big scientific theoretical discussions about knowledge and how to produce it. I did not want to discuss my own field and its flaws or their field and its shortcomings, because we are very far from each other, and I did not want conflict. Another thing is that when I was embedded in a field where different ontological and epistemological ideas are present, I found it difficult to remember or defend the social scientific stances. I think my insecurity and forgetfulness of my own scientific heritage testifies to the pervasiveness of the ideas about accurate repetition as a way to make results truer. It is part of a strong scientific stance, which is also well known outside the labs. When I faced it in daily practice, the confidence and security with which they posed questions about my methods left me mumbling and uncertain in comparison. Observing and interviewing seemed so careless all of a sudden.

Repetition is an integrated part of the scientists' daily tasks and, at the same time, they also consider it a valuable aspect of their behaviour. They repeat things in order to make sure that they are as good as they can be; '*in order to make sure*', as Miriam said, or even to secure that something is '*true*', as Mike demanded of his students doing science communication.

7.1.3 BEING CRITICAL

The last form of conduct connected to Vocation is that of being critical. As already mentioned in the introduction, the three forms of conduct are closely related. 'Checking up' can also be seen as a way of being critical by controlling how things are done, and the scientists often repeat their experiments in order to avoid future criticism. But while 'being critical' is not a singular act like repeating an experiment, I will still argue that it is an active attitude, something that one takes upon oneself in different situations.

For the last time, we return to the story about Miriam and the faint grey lines. She told me that she was not sure whether those lines were '*really there*'. The scientists are very critical about their ability to judge their own work. In their view, faint grey lines on a screen could be so many other things than the result they are looking for. Or it could be pure luck that what they are looking for is there the first time they try it. I have encountered this critical attitude towards their own results many times during my fieldwork. People doubt their own results; they try to pose as many critical questions to their findings as possible and turn and twist their hypotheses in order to find weaknesses. In general, I get the impression that the scientists are quite hard on themselves. They assess everything they do themselves with a very critical eye, constantly scrutinising their own actions and motives. At times, they have actually expressed that they want to be a little less critical, because all that scepticism frustrates them:

Christian is preparing an experiment with some plants. He has done the experiment several times before, but is once again checking that the results are correct. 'I get so irritated with myself sometimes,' he complains: 'I mean, I've done this before, I KNOW how it is. But then I get insecure again. I really wish I could just say "it is like *that" or "it is not like that", but I just can't bring myself to do that. I need to make sure over and over again.'*

This irritation due to not knowing whether something '*is like that*' or '*not like that*' is one I have often encountered among the juniors – but never among the seniors. This difference between the insecurity among juniors and security among seniors provides a solid basis for studying a critical attitude as an acquired ability. I have observed many of the juniors getting very frustrated in several situations similar to this one. They often feel that they should be more self-assured about the correctness of their own results. But on the other hand, they cannot do it because they do not trust himself. Furthermore, they do not think that trusting results excessively is *right*. As one of the other juniors comments on another occasion:

'I believe that we [him and his colleagues] *could be a little more critical about results. I'm actually a little disappointed about the scientific world in that regard. I thought there would be more focus on being critical about* [our own] *results.'*

He often feels that his supervisors only look at the final results and not at the notes, the numbers and the calculations that precede the results. My impression is that he considers it to be an overly-trusting attitude, which does not sit well with his idea about how critical one should be in academia. So, on the one hand, he is frustrated with his own reactions because he cannot bring himself to be less critical about his own results, and he therefore progresses slowly. On the other hand, he is disappointed in the '*scientific world*' because it does not live up to his conception of a critical standard. Another junior tells that he feels that those among his junior colleagues who are, in his view, less critical about their own work somehow seem to do better professionally, being more at the centre of management's attention and publishing faster. These mixed feelings are typical among the juniors. I have encountered them in almost all my interviews. The juniors feel insecure regardless of whether they '*do well*' and publish or have not done so yet. The difference between them is mostly in how they cope with these work conditions. Some of them become frustrated and develop a critical attitude toward the institution of science or the organisation they work in. Others somehow come to terms with the condition that the critical attitude is always contextualised in relation to other concerns and that this condition does not necessarily make the results less '*true*'.

Among the seniors, the theme is hardly discussed – at least not as a personal challenge. Most of them do emphasise that a critical attitude is an important skill to teach their juniors. But frustration at their own results or not knowing when their work is good enough never comes up. My interpretation of this junior-frustration and senior-silence is that the scientists gain more embodied and less analytical knowledge about the right dosage of scepticism during their career. This dosage is something that is hard to teach to the new generations of scientists because it is about a certain *Fingerspitzengefühl*, which only comes with experience. It is a tacit skill that they develop over the years by being embedded in the scientific system. They learn it by making revisions for submitted papers, by being rejected or published, by making presentations or patents and by having followed another senior's work when starting out in the system or by supervising younger students. I suspect that the seniors have learned (though they do not say so explicitly) that the appropriate amount of criticism is also dependent on culture and they accept that this culture does not mean that what they do is wrong or not true. Linda, one of the seniors, comments in an interview:

'To conduct experiments and be a scientist, that is to be critical, especially about your own results.'

So, in her opinion, a scientist *is* a person who is critical, especially about his or her own results; it is not just something you *do*, it is something you '*are*'. So in that way of talking about it, the critical attitude becomes an identity, something you possess if you are a real scientist. This is, in my interpretation, a little different from the juniors' ways of talking about it. Miriam, for instance, says that she thinks '*it is important to be careful in saying that "everything is good over here" [that their own results are good], because nature has its intricate way of sneaking around your theories*.' The difference in the two comments is that for the senior, critical is something one *is* if one is a scientist. For Miriam, a junior, it is something one needs to *become* by practising being careful in trusting oneself. It is not something she takes for granted as a trait; it is an attitude that only comes through careful practice.

Despite my impression that the seniors have a nuanced and embodied understanding of how one is critical in the right way, it is passed on in a very abstract way. Many of the seniors whom I have interviewed underline the importance of teaching their students to have a critical attitude toward their own work. One of them even has a laminated poster in his office stating '*In God we trust; all others must bring data*', and I think it is there in honour of his students more than for himself. This way of talking about the need to put forward data as a universal obligation (for all but God) that is beyond time and space is typical of how this responsibility is passed on from seniors to juniors. But the seniors also try to teach the juniors about 'the truth' in much more tangible ways. As I told earlier, then good supervisors give specific advice on how to move on with experiments or they tell their juniors that something is true enough. But it is difficult for the juniors to learn. They are so used to being critical that knowing when something is good enough becomes very difficult. The difference between the juniors' and the seniors' attitude toward a critical attitude indicates that it is an ability, of which the finer nuances the scientists acquire over the years, but that it is an extremely difficult skill to learn.

7.1.4 SUMMING UP

The three different conducts of checking, repeating and being critical are all valued ways of working. They are passed from seniors to juniors, even though the juniors seem to have difficulties learning how they should be translated into practice. In the next section, I will illustrate how the scientists justify these forms of conduct as essential for maintaining a specific role for science in society as the profession that guards ‘the truth’. First, however, I will comment on the findings so far in relation to the concept ‘conduct’.

I have used the notion of ‘conduct’ to describe some specific ways of going about daily work. This notion comes with a welcome blurring of the lines between thinking and doing or ideals and practice. The idea is not that scientists have an a priori ideal about being guardians of ‘truth’. Instead, I argue that the relationship between ideals and practice is recurrent and historical. The scientists’ way of doing their work has been repeated over time. It has also been connected to an idea about ‘truth’ as something that one can discover ‘out there’, and certain forms of working have, over time, become the way that ‘the truth’ is best demonstrated. This is similar to the way Weber describes how a pietistic lifestyle paved the way to both heaven and (over time) wealth for Northern European Protestants and instated a certain ‘Capitalist Spirit’, where hard work and a modest lifestyle were valued (Weber 1956). The point about the relationship between mundane practices and ideal is also one that Shapin & Schaffer (1985) demonstrate very well in their account of ‘Leviathan and the Air-pump', where the public ‘demonstration’ of an experiment in front of an assembly of ‘gentlemen’ secured the experiment’s validity. It does not mean that the scientists do not deviate from this ideal or do a lot of other things: In the next chapter, Oikos, I will, for instance, demonstrate that they also cut a lot of corners and do not always worry as much about the truth. But that there are limits to Vocation does not mean that it does not exist. As Law (1994) asserts:

'*Pools of order* […] *do not last for long. They are pretty limited. And they are the product, the outcome, or the effect, of a lot of work – work that may occasionally be more or less successfully hidden behind an appearance of ordered simplicity.*' (Law 1994: 5)

So the 'simplicity' of Vocation, the way it makes the checking, repeating and being critical look coherent and in the form of ideals is the result of the continued work of different students, rules, supervisors, sayings like '*In God we trust; all others must bring data*', and lots of other agents that hold the mode together and perform it – until it is disrupted. But they *do* also work like this, and they think of it as a huge responsibility to do so. In the next section, we will take a closer look at how they justify these ways of working with ideas about a specific role for science in society.

7.2 JUSTIFICATIONS

In the coming sections, I will look at the scientists' justifications for the duty to check up, repeat actions and be critical. The question is which kinds of worldviews and ideas about science's role in society justify these ways of doing the job.

7.2.1 TRUTH

Scientists rarely feel the need to justify why they find it so important to conduct their work the way they do. It seems to be something *everybody* knows, and they therefore see no reason to bring it up over lunch. Boltanski and Thevenot (1999) suggest that justifications surface in '*critical moments*', that is, in disputes; moments where two or more parties discover that they do not share the same viewpoint and need to draw on a specific moral order so as to justify their opinion (1999: 359). Such moments about these conducts seldom arose. Therefore, I tried to construct a '*critical moment*' by comparing the empirical material from Gyro Gearloose and Curious George. But that did not help much, as the two labs, to a large extent, share the understanding of responsibilities within the mode of Vocation.[23] In the end, my best strategy was to create '*critical moments*' in the labs. I did so by using the STIR interviews (see also 4.2.2) as opportunities to make the scientists connect some of their everyday, routine tasks with specific values. I did that by constantly asking '*why do you do that*' and '*why could it not be otherwise?*'. In this example from an interview with Annie from Gyro Gearloose, we are talking about her job in general and which responsibilities follow from being employed as a graduate student:

Annie: *'Well, as a graduate student and as part of my PhD, we have to get a thesis definition or outline approved, basically. And a couple of aims – a couple of things we have to accomplish through those three or four year periods. One responsibility, of course, is working towards accomplishing those. Right, the second responsibility is being honest while doing that.'*
Me: *'Mmmm, why honest? In which way?'*
Annie: *'Honest as in, you know, if there are data that maybe contradict general thinking, then it should still be presented as it is instead of any manipulation. Because we have to make our opinions based on real data, it should not be the other way around. So, that's what I mean about it: You have to be honest about what it is that you are doing.'*

So Annie is talking about the importance of being 'honest' in what she does. She contradicts this honesty with 'manipulation' and explains that 'honest' means that she has to base her opinions on 'real data'. The excerpt is about two forms of

[23] I will return to these similarities later in the chapter.

honesty: It is both about the responsibility to look for a truth – '*real data*' out there in the world or under the microscope – and the responsibility to speak the truth and thereby be 'honest' as a scientist should be. The commitment for scientists to be honest, meaning basing one's work and words on 'real' data, will probably not come as a surprise to many. The scientific profession is commonly known for having high standards for honesty, despite lapses here and there. The interesting thing is to see how the responsibility for being honest is turned into hard work in the laboratories – both hard work with the materials in the repeated experiments and hard work with the scientists themselves in internalising these responsibilities as a tacit skill. One of the scientists from Curious George who justifies her way of working in a similar manner, articulates the connection between conduct and worldview here:

'I have a big responsibility for doing it [the research] *properly and noting it down properly. Be meticulous, I think. Both because I really don't want any wrong results going out – in that way it is also in a way a responsibility for the group – you don't want to do, like a "Milena-thing"*[24]. [...]. *Unconsciously, you can by accident be a little lazy with your stuff and think, "Well, it's probably* [going to be] *the same* [result]." *I mean, not thinking the problem through properly. But I really think you have a responsibility for doing that [thinking the problem through]* [...]. *I mean, so your data are reproducible and what you say is correct. That's also why I'm probably one of those who check* [my data] *an extra time – rather once too often than the opposite – before I say anything about my numbers. I know that especially one of my supervisors is irritated about that... he thinks I should really move on and that I have already proven my point once* [...]. *But, I mean, damn it, what if... I mean, I would really, really be ashamed about that.'*

[24] The scientist is referring to "the Penkowa Case" about Danish scientist Milena Penkowa, who was recently convicted of very serious scientific and financial fraud. The case received a lot of media attention because she had been known as a young talent with a very promising career in front of her.

This quote illustrates the great seriousness that is apparent in relation to the mode of Vocation. She takes her responsibility to both find and speak the truth very seriously – so seriously, in fact, that she does a lot of '*extra*' checking (at least her supervisor thinks) in order to ensure what she says '*about* […] *numbers'*. It also illustrates how her daily conduct (in this instance checking) is related to the great responsibility she feels for her general research being '*done properly*', that is, so her data are '*reproducible*' and '*correct*'. The responsibility for that is so great that one can almost feel how terrified she is that she might accidentally not live up to this responsibility. In addition, she also explains that refraining from this responsibility for finding the truth through meticulous work and speaking the truth about the data would leave her extremely '*ashamed*'. So the double responsibility for finding the truth and being honest comes up again as in the interview with Annie. In general, this is the responsibility the scientists articulated – more or less directly – in relation to Vocation. Mike, from my first pilot study, says it very directly when he compares journalists and science:

Mike, the PI, and I are chatting over our respective lunch boxes. Mike asks if I can really secure full anonymity, because their field is so specialised that people with just a little knowledge about it would recognise them immediately. He comments, taking another piece of vegetable pie, that he knows journalists have a tendency to 'twist things' and he doesn't want that. I assure him that I am not in journalism, but in social science and say that I want to tell 'complex stories about the lab'. Mike looks annoyed and says, 'Don't tell me that you want to tell complex stories, tell me that you want to find the truth.'

While this story is a comical account of a PhD student without much experience in how to address people outside her own field (I do not even know what on earth I meant by '*complex stories*'), I do believe that I actually managed to create '*a critical* *moment*' (Boltanski & Thévenot 1999: 359), even if it was not exactly on purpose. Mike indirectly compares journalists with scientists. Whereas the former have a tendency to '*twist things*', and are thereby not honest, he is looking for a commitment to the '*truth*'. In his view, journalism, as a profession, does not have – or does not take seriously – the obligation one has to the truth. Again, the reference to '*truth*' is double in that it is both the act of '*find*[ing] *the truth*' and speaking the truth, the latter in contrast to journalists, who '*twist things'*. It seems as if he thinks this obligation is something that sets the scientific profession apart from most other jobs. He mentions this special obligation to find the truth and tell the truth in several contexts, emphasising that scientists are special due to responsibility. It is also Mike who – as I showed earlier (7.2.1) – encourages his juniors to tell '*the truth*' when making science communication, a skill that takes time to acquire.

In quite subtle ways, '*the truth*' surfaces here and there in my conversations with many of the scientists, but the responsibility for the truth is still not expressed very clearly. I mostly encounter these subtle references to 'truth' when they compare themselves with other professions or groups. There are, for instance, many expressions about politicians or the public's lack of understanding of true science at Curious George; this is seen as a reason for those types to stay out of decision-making about, for instance, GMO on open fields. In an interview with Clark from Curious George, I present the four rationalities from chapter x for him one by one, so he can comment with his personal opinion. I present the '*Reflexivity Rationality*' for him in a very shortened version:

Me: *'Number two states that scientists are supposed to solve problems for society, but they should decide for themselves what they want to solve, when and why.'*
Clark: *'That's very pragmatic. I do think this is correct to some degree. Yes, we are problem-solvers, and yes, we do make that decision on priority.'*

Me: *'Do you think that is the way it should be?'*
Clark: *'Yes, because we are the ones who are best informed about what problems there are. No one else might be better informed to make that decision, unless you are a politician, who will adjust things from different perspectives – to say it carefully.'*

Clark considers scientists the best decision-makers in regard to societal problems that should be solved, as they are the '*best informed*'. He contrasts this with politicians, who '*adjust things from different perspectives*'. Based on that quote – and Clark's general scepticism toward politicians and their opinions – I interpret this as yet another manifestation of the difference between the scientific profession and other professions, notably, that of the politician. Scientists can and should evaluate what is best for society; because they have access to the truth about the world, they are '*the best informed'*. Politicians, on the other hand, evaluate from '*different perspectives – to say it carefully.'* They do not have the same obligation to speak truthfully nor the true knowledge needed to make the right kind of decisions. This distinction between science and politics is one I often heard among the scientists. Politicians evaluate things from a different perspective than scientists. Sometimes they scorn the political profession for being too focused on personal power (again in contrast to scientists), at other times they do not judge the different responsibilities as such, but just state that scientists have distinct responsibilities 'to do the science', whereas politicians and government share a responsibility for ethics and legislation:

'It's the state's responsibility to legislate in an area, and it is the Ethical council's responsibility to keep a check on the ethics, and it's the Agency of Science's and Innovation's responsibility to take care of the economic aspect of the public science and then it's my responsibility to actually do the science.'

If the subject becomes more contextualised than the very abstract descriptions that I have shown above, the scientists find it harder to explain their responsibility for the truth and how they are going to handle it in a specific research situation. The following is an excerpt from my interview with Tim from Curious George. We talk about the reason he chose his research theme – the effect of climate change on specific crops. First, he tells me that he is quite content that the subject is relevant to small farmers from developing countries and how they should adapt to climate change. But after a little while, he adds some considerations:

Tim: *'When you do research, you shouldn't think too much about what it should end with – at least not if you want special results for that reason, and if you think too much about social responsibility, it might be that you are pushed too much in specific directions, which you perhaps wouldn't have been if you didn't have those general thoughts.'*
Me: *'Would that matter?'*
Tim: *'Yes, I actually think so, but it depends if you can do it… matter-of-factly, or how should I put it, I mean, that [if] you don't influence the results, then it wouldn't do any harm that you had that thought in the back of your head.'*

Tim points to the problematic in thinking '*too much about what it should end in*', because that means that one would perhaps manipulate the results in order to reach certain ends. The important point for Tim is that the scientists should separate what they want from the specific way they work with their data. In our interview, that is his main argumentation for being careful when working with '*social responsibility*'. Tim separates some ways of thinking about social responsibility from others. It is okay if the scientists think about it '*matter-of-factly*', where they '*don't influence the results*'. This '*matter-of-fact*' way of thinking is a bulwark against letting your own interests influence the results. But by making this boundary, he also establishes a possibility for thinking about outcomes – as he did himself in relation to his choice of PhD subject – as long as this thinking is not directly influencing the core research itself: '*the results*'. So Tim establishes a boundary between the general visions for a project and the daily laboratory work. This is a form of boundary work that I have often met. It becomes a tangible way of handling the responsibility to find the truth, speak the truth and still have an ambition about solving some kind of problem.

I often heard arguments similar to this. The scientists find it of the greatest importance that their '*results*' are not influenced by any desires. But they make a distinction between '*results*' and their research project in general. It is okay – and can even be a very good thing – that the ideas and framework are inspired by social challenges, but the core of their job, the experiments, need to be kept pure and value-free, so that the results do not get '*turned in specific directions*'. They should speak for themselves '*without influence*'. As an extra point, it is also noteworthy that the scientists often have difficulties handling the close entanglements between the pairs of basic research/applied research and internal/external regulation of science. Sometimes an argument about science's noble character due to its responsibility for the truth becomes a defence of their right to do basic science. In a similar way, applied research at times means the same as external regulation of science. This is a theme I will return to in the section 'Basic Science Guilt' (9.3.3).

All in all, I will argue that the scientists do feel a responsibility for 'the truth' – a responsibility that is tied to their professional norms and conduct. Their job is to find the truth and speak the truth without their own prejudices or interests being involved. Therefore, they should also be careful not to involve themselves in something in which their interests would be influenced in certain directions, such as 'social responsibility', because that would affect both their ability to find the truth and speak it. But the scientists do see it as their job to take care of the truth by striving to find it and be honest. In my interpretation, this they consider to be theirs and science's role in society: to be the caretakers or guardians of the truth.

7.2.2 'COMMUNITY' AS PROXY FOR 'TRUTH'

As mentioned in the previous section, the responsibility for the truth is something that is seldom directly spoken about by the scientists. This is partly because they all seemed to agree upon this responsibility for the truth, and partly based on my impression, because most of them (perhaps with the exception of Mike) consider it a bit silly to talk about something as pretentious and abstract as 'the truth'. They often argue for something a little more tangible and specific, for instance, in the STIR interviews (see 4.2.2). The last question, "*Who cares about your choice?*" often prompted answers such as '*my supervisor*', '*my boss*', '*other scientists*', '*the reviewers*', or '*my field*'. The scientists consider these people as the ones who would be interested in whether they have made the right choices in the lab or not. They are also the people whom the scientists would be afraid to face if they had not lived up to the standards required of them. In the following, it is one of the scientists pondering over the case of Milena Penkowa:

'Poor girl, I mean, she must be so sorry for what she has done. How is she ever going to be able to face her family, her colleagues or her boss after what she has done? It's one of my big fears that I by accident/ could end up like he due to an unfocused moment...'

'*Family*', '*Colleagues*' and '*Bosses*' are the three groups that he would be afraid to face if he had not done his work properly. Those are the people he fears would judge him and perhaps also the people who compel him to check up on his experiments an inordinate number of times and to be very meticulous about his notes and very critical about his own results. My interpretation is that he feels accountable towards them. He considers them as having a legitimate right to shame him and make him feel '*sorry for what she has done*', that is, if he had committed fraud like Milena Penkowa – even if it was not on purpose. The scientist believes that he has to answer to them.

What all those groups have in common (besides '*family*' from the latter quote) is that they are all fellow members of the scientific profession. So besides having a responsibility for the truth, Vocation is also about being accountable to the other members of the same profession. Sometimes when they talk about how they would feel unable to face their colleagues if they were caught perpetrating fraud, an image comes to mind of an enormous group of white coated people peeking out from everywhere: behind a curtain, through the laboratory windows, over the scientist's shoulder, and so on, while they scientist is working. This is because I get the general impression that the scientific community is a unit that they always (in this mode) feel accountable to.

So in a way, it is hard to tell how exactly they justify their conduct. My field notes and transcriptions point in two directions. Is it as guardians of the truth, where they are the one profession in society with that obligation, or is it as a responsibility to live up to some internal norms for rigorous work stated by fellow colleagues? I am not sure, and of course the two are not mutually exclusive. But I will suggest that the scientific community works as a '*proxy*' for '*the truth*'. '*The truth*' is an abstract and quirky entity, about which it can be hard to talk explicitly, as illustrated when the scientists have to explain themselves to the visiting ethnographer. Hence, it is easier and less abstract for the scientists to say that they are afraid of what their boss and colleagues would do if they were not living up to the expectations for proper conduct in academia. Or perhaps the two are seen as so entangled that they do not distinguish between the scientific community and 'the truth': The truth is something they have to care for, and they see their colleagues as fellow guarantors of that truth.

I argue that they have a responsibility *for* the truth; to take care of it by finding it and speaking it. But when they talk about the 'scientific community', it is about whom they feel responsible *to* (Douglas 2013: 2). In the first instance, 'responsibility for' means what they believe they have an obligation to take care of; in the second instance, it is about whom they are accountable to and believe they have to *answer to*. I believe this makes a difference. The first indicates that the scientists have to take care of something (the truth), whereas the second points more to those who will punish them, mainly by making them feel shameful if they do not guard the truth.

7.2.3 SUMMING UP

The justifications that are performed in Vocation are about the professional responsibilities that scientists have to live up to. Vocation is performed as a worldview where science is one among many professions, but the only one (at least compared to journalism and politics) that has a commitment to take care of the truth – their responsibility thus plays a unique role in society. The responsibility is twofold, as it is both about a commitment to find the truth and a commitment to be honest – speak the truth. This responsibility obliges them to work carefully and meticulously and helps them avoid being excessively bold in their statements. But it is difficult to bring this justification to the surface because the scientists seldom talk about 'truth'. First, they seem to agree, also across the labs, that this is the way of the world and therefore they do not need to discuss it at work. Second, my impression is that, while most of them agree that they have a responsibility for the truth, they have difficulties expressing this, as it seems rather strange, abstract and pretentious. Therefore, they use different members of the scientific community as 'proxies' for 'the truth'. They argue that they need to conduct their work according to the highest principles, as they are accountable to their scientific peers. The fact that they use the scientific community as a proxy for the truth does not make the scientific community any less important; it is very clear that they consider the scientific community a group whose ideals and expectations they have an obligation to live up to.

7.3 ORGANISATIONAL CONSEQUENCES

In the two previous sections of this chapter, I have shown the different conducts Vocation performs. I have also demonstrated how the scientists justify their ways of working with a specific responsibility to and for 'the truth'; a responsibility that is solely associated to their profession in society. In this section, I will illustrate how the Vocation mode also creates some specific ways of organising the scientific work in general.[25]

'So the way I think of them, these modes of ordering ***tell*** *of the character of agency, the nature of organizational relations, how it is that interorganizational relations should properly be ordered, and how machines should be. […]. I'm saying, then, that they are imputable ordering arrangements, expressions, suggestions, possibilities or resources.' (Law 1994: 20, original emphasis)*

What to make of a statement that explains the qualities of modes in such a way? Does it even make us any wiser about their nature? Certainly John Law is avoiding being too specific when he describes what modes of ordering are. In accordance with a 'modest sociology' (e.g. Law 1994: 2), he only tentatively suggests definitions, and they are usually garnished with several reservations. However, I still believe that there are some features that can be drawn from these tentative suggestions, which will be productive for the discussions about how to govern science such that it will be deemed 'responsible'. While Law conducts microsociological studies of the Darebury laboratory, his ambition is to say something about the nature of the social and how it

[25] For my specific take on 'organisational consequences' see also chapter 2.

is accomplished (Law 1994: 39). In my reading, his claim is that this is done through the continuous organising of everyday (work)life in recognisable patterns, where narratives, for instance, about the '*character of agency*', '*the nature of organizations*' and '*how machines should be'* are used as resources to steer action in what would have otherwise been an incomprehensible '*mess*' (Law and Singleton 2005: 2). As this is done, the narratives, the agency, the nature of organisations and so forth are also reproduced, altered and maintained. This I have already touched upon in the theoretical chapter.

What I want to dwell on here is the question of what exactly is being ordered. John Law offers a connection between science and technology studies and organization studies. But 'Organizing Modernity' is in my reading less about the mundane work of organising daily laboratory work in itself and more about how macro-structures and (even) grand narratives of modernity (which is also indicative from the book's title) are constructed through the exercise of mundane, daily organising. In that understanding, the daily laboratory work is both used to show how local agency constructs macro-structures as well as to argue that the social is thus made up of contingent and fragile ordering arrangements, held together on an everyday basis. In this reading, 'organising' does not primarily refer to how work is distributed, how different sections compete or how new work procedures are implemented – all themes that 'classical' organisation studies address. But they are used to say something about how 'society' itself is ordered. Where John Law's ambition was to reveal how 'modernity' itself is made up of these fragile orders, mine are smaller.

I will use this interpretation of 'Organizing Modernity' to say something about Vocation (and the other orders in the next two chapters) that goes beyond the two laboratories I have studied. I will to some extent consider the performative effects of Vocation in the two laboratories. However, I will mostly focus on Vocation as a general order that asserts which role science should play in society in order to be 'responsible' – not just in the two laboratories, but also beyond. Modes make up science's role in society through local conducts in a specific way. This role has some special characteristics, which are different from what science's role is considered to be in the next two chapters. These characteristics are my focus in the remainder of this chapter.

Law makes a '*check-list*' of the '*patterning effects*', which he believes the modes generate (Law 1994: 110). I will use some of the key descriptions from this check-list to describe which patterning effects I believe the modes perform. I will also add some 'patterned effects' that I have observed myself. In the theory section, I already described one of these patterning effects, namely, what he calls '*problems*' (see 3.3.4), and I described that this was very similar to the notion of '*justifications*'. In this section, I will take a closer look at the effects that he talks about as 'deletion' and 'distribution' (Law 1994: 111), but I will also look at the understanding of 'time and truth' and 'boundary work'. In the next chapters, Oikos and Citizenship I will also describe these patterned effects. Some of them will relate to the same subject, some of them will be entirely different. It is so, because each mode makes certain parts of the social very visible, while others become of less significance (Law 1994: 110).

In my reading of Law, the modes are created and sustained locally but may be so stable that they can endure over time and space – as do the four modes he describes. Therefore, the description of the mode Vocation at Curious George and Gyro Gearloose can also be seen as broader ideas of science's responsibilities in society. I see Law's 'check-list' as a way to understand how modes regulate behaviour, similar to the way I described how government rationalities work in a macro-perspective. They create a space for possible actions by rendering some forms of action legitimate and others not so. Whereas I described how the political rationalities did so by quite actively stating guidelines for proper conduct (Rose and Miller 1992: 175), the modes of ordering do not necessarily explicitly prescribe proper conduct, but rather construct space for agency via a range of different tactics, which I will now discuss in relation to Vocation.

This section about organisational consequences is divided into four themes. First I will demonstrate how Vocation generates a focus on science as a *profession* and a *community*, while the importance of the single labs are downplayed. Second I will show that a consequence of checking, repeating and being critical is that it dramatically reduces the speed of work on a daily basis, making the process of doing science very slow. Third, I will display how Vocation continuously sets the profession of science apart from other professions thereby protecting its autonomy. This is also connected to fourth theme, namely the scientists' idea about science's role in society as one of several stable, demarcated institutions that work on improving life for mankind.

After these sections I will explicitly look at the differences in how the mode is performed in the two laboratories. Here, I assert that the mode of Vocation is quite strong, as the differences between the laboratories are few, despite the fact that they are situated on two different continents and do not work with the same scientific questions. Finally, I will demonstrate the mode of Vocation is not 'pure', as the related conducts and justifications are negotiated with other concerns in the daily work of the two laboratories.

7.3.1 DELETING THE ORGANISATION, FOREGROUNDING THE PROFESSION

Law asserts that orders '*may embody and perform relatively consistent patterns of* ***deletion***' (Law 1994: 111, original emphasis). That means that the performance of a particular order tends to put some objects, entities or ways of thinking in the foreground, while other ways are more in the background. In relation to the mode of Vocation, I did experience that some aspects of scientific life were in the foreground, while others were almost absent. Most striking was that the relation between the individual and the scientific community seemed of great importance, while entities such as the local lab, the exact university or national state seemed unimportant and were seldom mentioned. In my interpretation, it is this relationship that is considered vital if the responsibility for the truth is to be maintained.

I base this interpretation on several factors. First, the fear of shame, of which the juniors are especially aware (see 7.1.3 and 7.2.1). They are worried by the idea of not living up to the responsibility and thereby being exposed as sloppy or – even worse – cheating scientists. My impression is that this fear is very individual. It is something they fear for themselves, but not for their colleagues. And the fear is seldom articulated other places than in the one-to-one interviews with them. They fear being shamed as individual scientists; it is not something they fear on behalf of their research group or organisation. They consider neglecting the responsibility for the truth to be something between them and the scientific community. As one of them says, then he would be afraid of what his family, colleagues and bosses would say if he had committed fraud – even by accident. So closer relations are seen as specific embodiments of all those who would judge his character, but in his opinion, they would not share in his guilt. Fraud and bad work are solely considered his responsibility and a lapse in his moral character. While ‘the Penkowa Case’ did expose some considerations about both the University of Copenhagen’s role and the role of Milena Penkowa’s closest colleagues, this has not seemed to affect the belief among the scientists that not living up to the standards is a matter between them and the community.

The foregrounding of the individual and the community on the one hand and the deletion of the organisation on the other also appear in other situations. As shown in earlier sections, the scientists often mention the 'community' as their stakeholders, and they say '*we*' when they talk or write about their results and research rather than naming single authors or a specific position. The '*we*' does not refer to their own lab or their group as the '*we*' I will discuss in the next chapter. Instead, they are speaking on behalf of science. In that way, there is the single individual who speaks (writes) and then the community backing up the statement. They also make clear the connections between their professional identity as 'scientists' and their responsibility for the truth, while the membership of a particular organisation or group is of no importance in that matter. As one of the scientists put it: '*To be a scientist, that is to be critical*.' And a large part of the learning process in academia is about learning to understand and embody this responsibility in practice. This is what separates them from other professions, notably, those of '*politicians*' and '*managers*'. Other identities than that of the professional are again 'deleted', using Law's term; it is not about their organisation, their university, their nationality or their research group. The profession stands out, while the lab disappears. This point will be further elaborated on in the next two chapters. In those, I will illustrate how the professional community fades into the background and entities such as the research organisation and shared citizenship become visible instead.

The point about professional identity is a clue to explaining how the responsibility for the truth becomes a way of regulating science. The responsibility for the truth and the conduct that supports it are seen as the most central part of the scientific profession. Compared with the other two modes, I would argue that this is 'the strongest' mode, and it is so in two senses. First, it is a very strong institution; I have already mentioned how it performs across time and space. Second, it is also the most persistent mode within the two organisations. The struggles of adapting to it for the juniors create some of the most serious personal crises I have witnessed. The seniors also confirm that this responsibility is the foremost value to teach the juniors. The other responsibilities, which I describe in the next chapters, are seen second to this and considered 'political' conditions that they should learn to 'navigate' in. From the perspective of Vocation, these are seen more as annoying work conditions that they unfortunately need to adapt to, not as something that defines or is the core of their profession.

In a way, it seems almost commonsensical: The responsibility for the truth is performed through meticulous work with materials and text, where checking, repeating and being critical is vital. Basically, the scientists consider this their job: This is what they do and are supposed to do – otherwise they would not even be 'scientists'. But the other responsibilities, that for the organisation and that for the public, are a little different; they could (if they were free to do so) choose not to be responsible for those parts of their work, but they would still be scientists. However, it does not work the other way around. They cannot quit the responsibility for the truth but still be responsible for, say, the organisation's economy or for their findings being turned into products and still call themselves scientists. Doing that would make them administrators or innovation consultants or a third category, but they would not be scientists anymore. Without the responsibility for the truth, the profession disappears as well. This may seem almost trivial; of course, scientists are not scientists if they do not research something. But as I will describe in the next chapters, it is not necessarily so in everyday work, when a multitude of other responsibilities start to pile up and the time to perform the responsibility for the truth becomes limited. The responsibility for the 'truth' is thus closely related with the aspects for the job, that the scientists consider as their core tasks. Therefore it is also the most stable.

7.3.2 SLOW SCIENCE

Besides creating a sense of community Vocation also installs a certain idea about *time* in the labs. No matter when or where I looked, all the scientists were very busy, but their respective business seemed to be of different kinds. My argument here is that the idea about time and how the scientists should relate to the organisation's understanding of time is dependent on the mode of responsibility.

As a consequence of the central conducts in Vocation, repeating, checking and being critical, it takes a long time to conduct science, and the scientists work a lot. Curious George was situated on one of my regular cycling routes through Copenhagen. When I cycled passed in the evening, lights could usually be seen in several windows facing the route, and I am sure it would have been the same had I passed Gyro Gearloose after dark. People drop in over the weekend to check up on their experiments – or they simply stay the night in the lab to monitor their experiments. On the days I follow Annie from Gyro Gearloose, she often politely dismisses me at lunch, as she does not have more time for me: '*You should eat, that's important',* she teaches me, but when I ask if we could have that important lunch together, she refuses because she does not have the time. She rarely sits down for lunch. She just quickly goes outside the wet lab (food is prohibited in the wet lab for safety reasons) and eats a bun or a roll, while pacing impatiently back and forth in front of the huge windows, which separate the lab from the hallways. The juniors tell me about seniors who answer their e-mails in the middle of the night and seniors tell me about juniors who '*spend their life in the lab*'. According to most of the scientists, they work on weekends and bring work home with them in the evenings. People have supplies of food and snacks in their drawers for workdays that last longer than the closest delis' opening hours. Here and there, pizza menus are pinned to boards. Time and work is also a favoured subject of talk and discussion among the scientists. In the following, it is one of the Gyro Gearloose scientists who tells an anecdote about time management:

At an evening get-together at Gyro Gearloose, one of the graduate students tells the closest dinner guests that he has heard from a friend that the laboratories in Europe close everyday at 5:00 p.m. and are closed over the weekend, which makes it impossible to conduct time-consuming experiments. He considers this closing policy 'completely crazy', albeit not, apparently, unlikely.

While I can testify that the laboratories do not close at 5:00 p.m., nor over the weekends (at least not in Denmark), the story is interesting, because it is clear how work outside normal business hours is seen as so normal that weekend closing sounds extravagant and odd. In fact, it sounds so '*crazy*' in the ears of the graduate student that it is something only ancient, decadent Europeans could pull off. The story illustrates that the scientists consider it an utmost necessity to have a lab that is always open and a great amount of time in order to be able to do their work properly. A related point, which will be examined in Oikos, is that the scientist also implies a difference in work morality and in what it takes to create results in Europe and the USA (see 8.1.2). The tale about laboratories closing at 5:00 p.m. is considered so exotic and absurd that it is appropriate entertainment at a dinner party. But that story is just one among many stories about work hours. The scientists complain about the lack of economic awards for the long work hours compared with that of industry. They explain that they never see their families, and especially about the lack of attention their children receive and that the latter are always picked up late from kindergarten. Casual remarks about being stuck at work on a Friday evening are common, as are stories about reading papers while the children watch (too many, according to the stories) films on their iPads.

At Gyro Gearloose, juniors with children are rare; in fact, I never met one. At Curious George, the juniors are older than at Gyro Gearloose (due to different educational systems and cultures), and there are thus more juniors with children there. The PhD students with children complain that those without children could produce better dissertations because they have more time to do so. The PhD students without children, on the other hand, claim that they are less structured and do not accomplish as much during a workday as the parenting students do: '*I do the same work in 12 hours that they do in six'*, as one of them says.

It is hard to come across a work place anywhere where people gossip about how little they do, how easy their job is, and how much time they have on their hands. Even if that were the case, few would admit to it. Hard work, and a lot of it, is generally considered a virtue for better or worse. In many workplaces, people work long hours, have a lot of assignments and bring work home with them, as they do in the two laboratories. In that way, the two labs resemble most other contemporary work places. My argument is that hard work and long hours are ways to perform Vocation.[26] The scientists associate a large workload with true results. As Catherine from Curious George wrote on Facebook as a subtext to a picture of a precipitous curve: '*Hello Good-looking! Priceless to get such a beautiful peak, when you have optimised for three days*.' Catherine connects a good result, '*such a beautiful peak'*, with very intense and meticulous work, that is, she '*optimised for three days*'. So in my interpretation, the endurance of long workdays – and talk about them – is a way for the scientists to be good at their job and also show it. Living up to the expectations of finding and speaking the truth is done through the very slow, but careful work of materials. As such, working long hours is a way for the scientists to show themselves and the world that they are great at their job.

[26] As I will go more in depth with this in the coming chapters, other concerns contributed to the long workdays as well, such as the hope of career advancement, competitions for jobs and after-hours voluntary science communication work.

So the idea is that the scientists work long hours because they repeat, they check and they are being critical. By doing this, they also live up to what is expected of them in the mode, and therefore this becomes a sign that one is a good scientist. I have chosen to call this phenomenon '*Slow Science*' because it reminds me of the '*Slow Food Movement*', where quality, meticulous work with the basic ingredients and many hours in the kitchen is considered the best way to make delicious, healthy food. Slow Science is a sign of quality because repetition, checking and scepticism slow progress down immensely. The next story is a good example of how Slow Science is cherished.

At a late dinner party, some of the scientists are discussing the latest developments at Curious George. This morning, there has been a big breakthrough in the experiments. While breakthroughs are always welcome, this one was particularly welcome. The previous year, the lab was surprisingly excluded from a big collaboration – perhaps (the rumours are plentiful) because they did not deliver fast enough and the partners did not believe in them anymore. But now they have the results that prove their hypothesis. And they no longer have to share them with their former partners. The participants at the dinner party are quite satisfied with this development. This will teach the former partners a lesson and show them how good science should be done. 'Slow and steady wins the race,' one of them comments.

The scientists are proud that they did not deviate from good scientific norms in order to produce faster results. They are proud that they have done solid work, which one can trust, and find it suspicious that the other collaborators were so interested in fast results. To them, that is a sign of rotten professional character. Good scientific work is about never taking shortcuts, even though it may cost partners and money. So at Curious George, they have chosen not to deviate from their scientific standards, despite the fact that they lost a partner due to their 'slow' work. But this is turned around and seen as a victory (especially because they now actually have the results), as it demonstrates that Curious George is willing to sacrifice its own resources to maintain professional responsibility.

All the scientists seem to share the view that the work is so complex and difficult that they are bound to have many failures and few successes. Repeated efforts are considered a sign of seriousness and devotion, whereas instant success with experiments is just mediocre '*luck*'. Repetition and failure is as such a vital part of being a (good) scientist. The earlier quote from Clark about brain imbalances comes to mind again: '*I believe that there perhaps is some kind of brain imbalance in scientists. We miss the receptor in the brain that normally is responsible for the transmission of frustration.'* If we look at this quote again, it is possible to interpret it as a statement of professional pride: Scientists are a particularly tough species, who, in contrast to everybody else, can endure an environment where disappointment and boring, meticulous work is the name of the game and the rewards are few. Working long hours is a consequence of making high quality science. The longer the scientists stay in the lab, or the longer they work on a paper, or the more time they spend on a grant proposal, the more convincing they are as dedicated scientists. In that way, the mode of Vocation contributes to fostering a work climate where working long hours are expected and appreciated and where quick results are not necessarily considered a virtue.

As a result of Vocation, time becomes perceived as, on the one hand, an endless resource – as there should ideally always be time to make another experiment – and, on the other, something that should be well spent; there is room for repeating the experiments, checking again and receiving critical comments from colleagues. But there is no room for breaks, going home early, parties or chatting. Taking one's time while still working hard throughout the day (and possibly night) is recognised as the way to live up to the Vocation. As a consequence of Vocation, the daily work of science is organised as a never-ending exploration of possibilities, albeit one that should be conducted in a concentrated and focused manner. I will return to the differences between this understanding of time and the one in Oikos in the next chapter.

7.3.3 BOUNDARY WORK: SCIENCE VERSUS OTHER PROFESSIONS

Vocation also generates a specific way of organising the profession outwardly in relation to questions of regulation. Here, I will argue that the scientists also use differences in responsibility actively to set them apart from other professions. It is an instance of '*boundary work*' (Gieryn 1983). Gieryn argues that 'science' as a specific form of knowledge and practice is a construction that is performed and maintained by scientists (Gieryn 1983: 781). He further argues that 'boundary work' is '*ideological*' and serves the purpose of '*enlarg*[ing] *the material and symbolic resources of scientists or to defend professional autonomy*' (Gieryn 1983: 782). This 'boundary work' can be observed in the two labs. They often compare themselves with other professions in ways that flatter themselves more than the occupation to which they are compared. Sometimes, the work does not seem to have a particular strategic goal, such as professional autonomy; it sounds more like a repeated statement of something everybody already knows. This is, for instance, the case in relation to the course in 'bio-business', which Miriam attended at the local business school.

She is not particularly impressed with the curriculum or the teaching. After one of the classes, she scowls: 'Those management types [the teachers] *always share the same banal points; you might as well have a chat with your mother.'*

In Miriam's interpretation, 'management types', be it researchers studying management (some of the teachers running the course) or professional managers do not add much of value. They only share '*banal points*' and do not possess much more knowledge or expertise than the average layperson. My impression is, based on this experience and the PhD students' disdain for the course in general, that they do not consider it real knowledge in line with the norms of their own Vocation. It is not scientific. This may also have something to do with the fact that this course is about earning money on medical inventions. Despite the fact that some of the PhD students will end up doing exactly that, and have voluntarily enrolled themselves in the course, they consider business a lower pursuit in life; something that '*management types*' tend to do, not scientists. Scientists take care of '*the truth*' and that is not an occupation they are supposed to earn money on. It also seems as if they equate making money with being a bit banal. Having a business is not something that demands highly specialised knowledge (your mother could advise you on it) in contrast to doing science, which demands skill, brains and patience (for more stories and elaboration in relation to the bio-business course, see Citizenship). This difference between science (as in the natural sciences) and other forms of research are often pointed out by some of the scientists. In the following, it is Henry, the lab director at Gyro Gearloose, who comments:

'The real goal, if you have a real goal, is… you know you [scientists] *are trying to find out if this hypothesis is true, and you do actually have some sort of real grounding, where in social science, my view is that social science would say, "Well,* […] *we all agree this is the right thing to do", but two years later,* [social scientists] *can all agree that this is the wrong thing to do.'*

So scientists have a '*real grounding*' because they are looking for a long-standing, solid fact, whereas social scientists can change their opinion every second year.

Again, this sets the natural sciences apart because they have that obligation to take care of the solid fact, something that not even other members of academia can claim. So there is much talk about how the responsibility for the truth sets the scientific profession apart from other professions. But at times, they also – as Gieryn (1983) suggests – use this special obligation as an argument for professional autonomy, especially in relation to public science's ability to deliver innovation and growth. Miriam from Curious George is preoccupied with the differences between private industry and public science:

'It's a source of annoyance when you follow the public debate and you listen to companies who complain that we [public scientists] *are not innovative enough and* [they say things like] *"If our company got a better tax agreement, then we could get all the world's scientists up here." Well, that's not true, because you can see how the companies cut and cut in their Research and Development departments, and they focus on 'perfume-big-sellers' where they wrap their old innovations in new wrapping paper and then that's what they make their money on. And fair enough, if that's what they want, but then don't come crying and say that Danish scientists are not ready to take the big chances in science and innovation.'*

Miriam complains that public science is pressured by industry in the media. In this view, public science is being accused of not being '*innovative*' enough or ready to take '*big chances*' in science and innovation. In my interpretation, she tries to negotiate some space to do science as she wants to do it, by firing some of the same claims back at industry: They are not nearly '*innovative*' enough, as they do not actually contribute with new inventions, they just make some small adjustments to known technologies and sell them as something new. The implicit argument is that this is in stark contrast to public scientists, who actually do real science, which is understood as discovering important stuff by '*taking big chances*'. The difference between industry and public science is often up for debate in different contexts. The scientists position themselves as different because they do not have to worry about the bottom line. In the following, it is Simon from Curious George:

'You asked what qualifications are needed in order to work at a place like this [...]*, you need flair for – and find delight in – being occupied with things that are politically decided and politically oriented. And by that, I contrast it to things being decided by black numbers on the bottom line. And that is a difference in ways of thinking. If you only want black numbers on the bottom line and want that to control your research, then you're an industrial scientist – or you should consider becoming it. I'm not saying that this affects you all the time, but there are some connections in some kind of way. As a public scientist, you can see it in the way that you adapt your applications to the calls, but it doesn't affect your research. You adjust the application, even though you research the same subjects as always – and that is a talent as well.'*

First, Simon asserts that being a public scientist demands that one work with the political conditions and in a system whose directions are decided politically. He contrasts being regulated at the political system with that of being regulated by the market – '*black numbers on the bottom line*'. He adds that it takes a special kind of person to be in public science, someone who does not want to be regulated by numbers, but rather something else (which he does not explicate). In my interpretation, this unmentioned something else is the 'truth'. Being regulated by the political system does not '*affect research*' because only what is true or not true do that. But it takes a special '*talent*' to write applications and still not let it affect your daily work. Again, I consider it as an instance of boundary work. This is because Simon contrasts being regulated by the market with that of being regulated by the political system. But the trick in the political system is that the scientists can still maintain their responsibility for 'the truth' – as long as they know just how to write the right things in their applications.

Considering the two last quotes, Miriam's and Simon's, together, they present a picture of science in competition with industry, one where public scientists need to assert their particular virtues in contrast to those framing private science, and thus underscore their right to certain forms of autonomy. Turner (1980) comments in his analysis of Victorian public science that, '*the pursuit of public science has involved lobbying various non-scientific elites* […] *and defining the position of scientists vis-à-vis other rival intellectual or social elites, such as the clergy'* (Turner 1980: 590)

Whereas the clergy has a less prominent role in relation to these cases now, I consider 'industry' to be a '*rival*' that puts pressure on public science to perform in a specific way and in relation to different principles, most notably those of '*the bottom line*'. Gieryn (1983) comments that boundary work is locally constituted and strategic in its endeavours (: 784). Here, the strategy seems to be that the scientists distinguish themselves from industrial science in order to maintain their autonomy. The scientists try to construct a space for themselves where they can pursue their scholarly interests and get funding despite the pressures they feel from industry as well as from those university reforms that have forced public research organisations to mimic private industry, with competition as a basic incentive (Shore and Wright 2000) They do that by referring to two principles; namely, by actively stating that they are '*ready to take big chances*' while, at the same time, being grounded in responsibility for the truth, where the research '*is not affected*' despite different political priorities.

As such, they use boundary work to distance themselves from what they feel most pressured by, namely, industry and being regulated by financialisation. But this boundary work also has consequences internally. The extra emphasis on the scientific profession as noble adds to the pressure that the juniors feel for doing a good job:

As part of the closing seminar I gave at Curious George, I had included anonymous quotes from my interviews that I found telling about the nature of that organization. The idea was that the scientists should discuss whether they recognised the situations or values that were described in the quotes. It was a way for me to 'test' whether some of the impressions I got from individual scientists during interviews were recognised among the rest of the group. One of the quotes expressed disappointment in the research environment in terms of prioritising fast publications over self-criticism. When I reached the slide with that particular quote, the ambience of the approximately 40 scientists grew uncomfortable. People whispered and looked worried. Only one of the PIs spoke up. He was troubled by the quote and said that 'something should be done' if this was a general impression. He maintained that scientific rigour was one of the most important issues for the lab. The juniors mostly looked at their shoes. Afterwards, one of the juniors approached me. He was worried that it was his quote [it was not] *that I presented. If it was his, he stated he felt that he had shared more with me during the interview than he should have. Furthermore, Canute, the head of the section, was quite worried and wanted to know if that quote was a single incident or a more general impression. He really wanted the juniors to understand that the importance of honesty and scientific rigour was above everything else. Nothing should change that, not even demands for fast publications.*

It took some time to sort things out. I promised the PhD student that he could read his interview through and withdraw all the statements he was not comfortable with. Meanwhile, several of the juniors approached me with comments about recognising 'their' quotes. None of them were right, however. I also spent some time explaining my interpretation of the situation and the quote to Canute and a couple of other PIs.

As soon as they got the interpretation, they were not worried anymore and even agreed. Roughly the same interpretation as I put forward here. As I have already described in 'being critical' (7.1.3), the juniors feel that it is quite difficult to learn the appropriate level of criticism. They simply have difficulties in knowing when something is 'true' enough. Usually, they are more rigorous and critical – just to be on the safe side – than the seniors think necessary. Looking at this phenomenon again in relation to boundary work, another dimension can be added to this picture of insecurity. Not only is the responsibility for the truth something they need to learn and contextualise in specific situations. At the same time, they need to maintain that they are noble, trustworthy and deserve to do autonomous science. The grand and shared stories about scientific virtues are not necessarily compatible with ordinary organisational work life to someone just starting out on a scientific career. But as the juniors *have learned* to be honest and respect 'the truth' since the beginning of their university lives, they do not look favourably at the specific everyday setting they are part of.

So on the one hand, the responsibility to be precise sets the scientists apart as a special profession, which takes responsibility for the truth and not just having 'black numbers on the bottom line'. On the other hand, being a member of this special profession also feeds the insecurity related to the standards for good work among the junior staff. The mode of Vocation does not only produce rigorous work, an understanding of time as precious resource and an identity as a noble professional, it also produces a large amount of insecurity for those learning to conduct themselves in this system.

7.3.4 DISTRIBUTING RESPONSIBILITY BETWEEN INSTITUTIONS

Another 'patterning effect' that John Law describes is that of 'distributing'. He says that modes:

'…generate and perform distributions, defining or embodying a characteristic approach to what might, does or should pass from whom to what under what circumstances' (Law 1994: 119 original emphasis).

This effect is something I have observed being performed as part of Vocation. In this case, the objects that are to be distributed are different forms of responsibility and the actors between whom the responsibilities should be distributed are the state's different institutions, namely, those of science and politics.

The exact conceptions of the best forms of distribution vary a little. At times, the scientists believe that both the responsibility for the truth and for deciding how to act on that knowledge should be bestowed upon them. This is, for instance, the case where Clark states that he (and scientists in general) are in the optimal position to make decisions concerning how society's big problems should be handled, as they (scientists) are 'better informed' about the problems. He also acknowledges that politicians could partake in that responsibility, but he is still sceptical because he does not think they have sufficient knowledge to do so. At other times, the distribution between state actors looks different. The scientists still have the responsibility for 'the truth', but they refrain from taking the responsibility for what is to be done with their knowledge. For instance, Annie explains, as I showed, that it is her responsibility to state in the papers that her findings could be used to construct biological weapons, but it is the politicians and other actors' responsibility to figure out what needs to be done in relation to that risk. While the scientists in both examples believe they have the responsibility for the truth, it varies if they believe that this responsibility also grants them responsibility for how society should act as a consequence.

Either way, the way the scientists consider the distribution of responsibility between science and politics shares some similarities with the way Shapin (2009) conceives of the relations between science and the state after the Second World War and in the first decade of the Cold War. Shapin explains that scientists had never been as entangled in societal matters as after the Hiroshima bombings, which led to ambivalence about the merits of the scientific profession. Some celebrated the development of the atomic bomb as a victory for both science and democracy – without it the war in the Pacific Ocean would have continued. Others were deeply ashamed that science had played a decisive part in developing such a lethal technology; others again saw it as being out of the profession's hands. They might have developed the technology, but it was not their choice as how to use it (Shapin 2009: 65). While this debate went on – also among scientists far from experimental physics – the conditions for conducting science had never been more favourable in Shapin's opinion. He describes how public science received almost limitless amounts of funding, both for basic and applied science. This also meant a fast expanding job market for academics. Furthermore, science had, via its engagement in the wars, gained access to political circles, where they found themselves wielding power in terms of advising on directions for both scientific exploration and national strategies in areas such as growth and military (Shapin 2009: 64).

In my interpretation, this way of both debating and arranging the role of science in society is mirrored in the ways which Gyro Gearloose and Curious George see the ideal distribution of power between science and politics. As scientists, they have the responsibility for the truth, they should have quite a lot of freedom to take care of that responsibility, and, perhaps, also the power to dictate politics based on their findings. Either way, knowledge is channelled from science to politics and not so much the other way around. Moreover, it is the distribution of responsibility between the state and public science that is in focus. 'The public', for instance, who will be very apparent in chapter 8, is not given any sort of responsibility here; nor is 'industry', which actually played a huge role in the development of scientific work in the post-war years (Shapin 2009: 128). This is solely about the relationship between science and the state.

This way of thinking about the distribution of responsibilities may be highly idealised (also compared to the actual situation after WWII, despite the favourable conditions). Conveniently, it also grants the profession of science the very privileged position of both being able to take or not take responsibility for further outcomes. But it is still interesting how the ideas about distribution of responsibilities, to some extent, are also an institution with historical roots, which can be traced back to the decades after WWII. In my interpretation, these distributions of responsibilities between public actors are the way that Vocation constructs science's role in society. Society is considered as 'made up' of rather stable institutions with distinct responsibilities. Science is supposed to deliver advanced and new knowledge – also basic science – and a work-force to these other institutions. But in order to do so, they need a good amount of funding and relatively free hands. I will compare this view of science's role in society with those expressed in Oikos and Citizenship in the next chapters. Here I will move on to the last considerations about Vocation, before the conclusion. The first of them shows how Vocation's responsibilities are weighted against other concerns in the two labs and is as such not pure. The second takes up considers the differences in how Vocation is performed across the two labs. The two sections are therefore also ways of addressing the questions of methodology and generalisations.

7.4. NEGOTIATING VOCATION

In the previous sections of this chapter, I have described Vocation as if it were a 'pure' mode, where nothing interfered. But, of course, it is not quite so simple. As

Law comments, modes are never 'pure', but rather constantly disturbed by resistance and competing concerns (Law 1994: 111). Therefore, I have devoted this section to clarifying how the mode of Vocation is constantly moderated and demarcated in daily work at the two labs. In this section, I examine how the responsibility for the truth is negotiated and at times set aside in favour of other concerns, such as economic issues or public scepticism.

I'm overhearing Canute explain why they do not plant GMO in open fields, but rather only in securely closed tanks. I have heard the argument many times before. It usually goes like this: 'I have decided that we won't plant any form of GM plants out in the open. Despite the fact that we actually could – it's not illegal. But I just don't want to bother with the hassle and the debate. I don't think it's dangerous at all, but there's just too much opposition and I have given in.'

Most of the scientists at Curious George believe that the question of whether GMO can be planted in open fields should be assessed from crop to crop; that is, it depends on the crop and its qualities. They also think that the public's resistance to GMO is rather silly. But they have followed the debate on GMO, and they do not want to face the kind of public resistance that they met around the turn of the current millennium. So they follow what they believe to be the public's desire and only harvest their plants from tanks, despite the fact they might be able to do interesting experiments if they could plant it outside. This is an example of the scientists actually listening to the public. Although they do not agree and they even find the idea of resistance to be highly irrational, they acknowledge it as legitimate and something that needs to be followed if they want to gain and preserve public trust, which is vital for their funding. So the ideal of striving for the best possible science (for instance, using fields as the laboratory) is set aside in favour of the concern for public – and thereby economic – support. Vocation 'looses' the negotiation. This concern for public support comes up once in a while. In the following, it is Benson from Curious George:

During an interview, Benson tells me about a science contest he has recently participated in as a supervisor for a group of students: 'We didn't do that well, the experiments didn't go well and the ideas were not good enough. But I still got a lot of praise for the participation. The story about the participation in the contest went to the media and my bosses are glad because I have contributed to showing society the importance of our field and that we need money.'

Benson is not very proud of the work he did in the science contest. Evaluated from the perspective of Vocation, '*the experiments didn't go well and the ideas were not good enough*'. He is therefore also a little surprised – and almost disappointed it seems – that he has received so much praise for the job. His '*bosses are glad*', however, he had expected them not to be due to the poor experiment work. But they have evaluated his performance based on a different criteria: the work might not have been the best but that is not of great importance, as they got media attention and were able to show '*society*' both that they are an important field and that resources are needed to pursue their important work. From the perspective of Vocation, these criteria are not that significant. But there are other concerns in the lab. Those people – such as the happy bosses – who are preoccupied with themes such as society's acceptance of synthetic biology and the lab's resources evaluate Benson's participation in another, much more positive, light. For Benson, who is a junior and still learning to navigate in public science, this is confusing and a little disappointing, as he sees the responsibility for the truth above all – even the organisation's survival. In this instance, negotiations are about different criteria for evaluating a specific situation. At times, the criteria from Vocation are set aside because other concerns are seen as just as important – or more important – than responsibility for the truth.

Different actors, here Benson and his bosses, evaluate the same situation based on different criteria of evaluation: they justify the work from different perspectives. At another occasion, one of the seniors explains to me that, from management's perspective, Benson has done a great job. He has succeeded in promoting Synthetic Biology in the media. He has managed to get a group of young undergraduates engaged in biochemistry and they had actually ended up with some results. No one (at the senior level) would expect the same standards from undergraduates and young PhDs as from experienced scientists. The manager underline that there are times where the responsibility for the truth is set aside because other concerns are just as important. So in this instance, different positions in the organisation evaluate a specific situation differently. But there are many different forms of negotiation going on all the time. In the excerpt below, it is Canute who speaks about funding opportunities:

During an interview, Canute tells me about a presentation they did for a group of potential funders. They were in competition with other labs, which also did presentations. The PI had emphasised that their group should be 'modest' and 'realistic' about their ideas and the potentials. They should not promise the funders that their work would surely lead to big inventions or very specific technologies. He thought it was important that they presented themselves as serious and reliable scientists who did not try to spice their talk up with too many loose promises. In the end, when they got their evaluation from the funders, it stated that the science seemed very solid and genuine, but that they were too modest and should have been braver in their predictions, therefore they did not get the money. Canute is disappointed. He thinks that all these different evaluation criteria make science 'a really complex system to work in.' It also makes the job as manager quite hard because it is difficult to give the right advice or teach the younger generations how to navigate in *academia. The criteria for what is considered good work are unpredictable and shift fast.*

I often encountered descriptions like this. The scientists find it difficult to manage the different evaluation criteria that arise in relation to their work. In some ways, Benson and Canute's stories are quite similar, despite Canute being senior, Benson being junior and the 'competitions' being very different. But in both stories, they expect to be evaluated on other criteria than what turns out to be the case. They see it as science's responsibility to produce steady, reliable knowledge; they should be '*modest and realistic*', in Canute's words, have good ideas and provide good experiments. But then they fail because that is not what others expect of them. In Canute's story, they do not receive the funding because they (apparently) should have been '*brave*' and – at least – express hope for some fantastic results. But this is contrary to Vocation, where scepticism is considered a virtue and excessive confidence in the results not so. In that way, Canute would have neglected what he considers his responsibility had he been bolder in his predictions.

Both Benson and Canute do, at times, justify their actions with other ideas than the responsibility for the truth. Also, they can both act extremely strategically or choose not to, depending on the situation at hand. Despite this, they are very confused and considers the different criteria a challenge to handle. Not just because they failed to read the situation correctly and conduct themselves accordingly, but also because the work it takes to jump between different criteria for evaluation – and neglect those they find important – is really frustrating. Canute, who is considered really good at '*all the political stuff*', as his colleagues say, often complains about the '*very complex system*'. Even for him, it is extremely difficult to work in an environment where it is very unclear as to which kinds of responsibilities they need to live up. Furthermore, he is concerned about the juniors, like Benson, and how they are supposed to survive in this system. He is very preoccupied with the idea that he wants to educate '*decent human beings*'. But he doubts whether this is possible nowadays, as the scientists have to fight for the jobs and face a system that is too complex to 'navigate in' – and stay decent, that is. This discussion about decency and the conditions for conducting 'responsible science' will be taken up and developed several times throughout the next chapters.

Annie is looking through her lab book as I arrive. She is scribbling additional notes in the margin and seems occupied: 'I'm going to run a sample that compares diseased people with normal people,' she tells me. This is a big addition to the experiments she has already done in order to show the efficiency of a new diagnostic method. In what she calls 'standard studies' in the field, such an addition would not be necessary. But as the scientific statement is quite bold, she needs very convincing data, she tells me. If they cannot convince the scientific community about the method and publish the paper, it is going to be difficult to raise further funding for the project.

In many ways, this situation resembles many others that Vocation generates. It illustrates the decision to do additional experiments in order to convince the scientific community about the truth of a claim. But the difference lies in the justification. In the previous sections, I have argued that Vocation generates certain forms of conduct that are justified with the responsibility that the scientists share for the 'truth'. But in this situation, Annie's justification does not refer to the responsibility for the truth, but rather to the concern for future funding. She needs to repeat, check and be even more critical than usual, because without really strong truth claims, the lab will probably loose opportunities for further funding in that area. The choice to run additional experiments becomes a means to reach the goal of sustaining the lab with more resources. It seems that the conduct that I have described as 'typical' for

Vocation is not necessarily always so. In this situation, these forms of conduct are used strategically to reach another goal than caring for the truth, namely, caring for the lab's economy. Without a lab, the scientists cannot perform their Vocation.

The situation with Annie is not the only one where I have witnessed such a combination between conduct and justification. Once in a while the scientists tell me that they have to redo experiments they consider '*routine*', even though they believe they have already proven their point. But some of their partners have not been able to repeat them and get the same results and the partners need results in order to keep the funding flowing. This is especially the case at Gyro Gearloose, where they work with a method, immunosignaturing[27], which has qualities that are contested in the scientific community. Therefore, the scientists often need to do additional experiments to prove their point for journal reviewers and foundations. Journal reviewers and foundations are also equally important for the scientists: If they cannot convince their scientific peers about their claims, they will not publish anything, and without publications, no further funding. Below is another example of this kind of negotiation from Gyro Gearloose:

Annie gives a presentation for the group. She is showing how a new diagnostic method can detect and distinguish various infectious diseases quite quickly. There are questions from the group as the presentation goes along. One asks Annie why she has not used immunosignaturing as a method for the device. Emma, the PI, who is among the audience, breaks in and answers on behalf of Annie: 'Well, frankly, the funders don't like immunosignaturing, so we have chosen to do it both ways.'

[27] Immunosignaturing is a method of diagnosing diseases by studying which kind of antidotes the body develops in order to fight the disease. Each disease provokes a unique set of antidotes and can thereby be used to find and distinguish different diseases from each other.

Most of Gyro Gearloose's work is built on a belief in the qualities of immunosignaturing. But they are met with opposition from research funders and other scientific environments. These actors do not believe as strongly in the method and do not think that the data the lab has provided so far is convincing enough. So what should Gyro Gearloose do when they want to explore the possibilities with immunosignaturing, but funding opportunities are slim? Should the lab succumb to their funders' demands and use a more traditional method to detect the diseases – or should they stand their ground because they think that their evidence is strong enough? In this instance, they choose a third way. The funders have made it quite clear in the contract: They want a functioning diagnostic device that can be used to detect specific infections for a specific group of people who are at risk of being infected with a broad range of diseases. The PI decides that the group will both develop the device based on more 'traditional' diagnostic methods as well as make experiments proving they could use immunosignaturing – perhaps even with better results. It probably means that the scientists have to work twice as hard for the same money, but at least they get the hope of future funding in return.

This story illustrates how the order of Vocation interweaves with other concerns in intricate ways. Both immunosignaturing and the other method live up to the standards of being '*true*'. Based on careful work with materials, Gyro Gearloose has come to the conclusion that there is more potential in immunosignaturing, as it diagnoses faster and perhaps more precisely so. In order to receive funding, they agree on the funders' conditions, but also move along with their own work. Ideally, they would like funding for immunosignaturing no questions asked, but their scientific claim is not strong enough (yet). They need to continue to build up evidence and make as many 'true' statements about immunosignaturing as possible in order for the scientific community, and thus their (potential) funders, to follow. However, to do that, they have to pass through more traditional diagnostic methods in order to receive funding.

In contrast to the previous case, this is not a straightforward story about the mode of Vocation being set aside because of a concern for the lab's economy. Vocation is still performed regardless of whether the scientists choose one method or the other. It is a conflict between different 'truths', which turns out to have implications for Gyro Gearloose's economy. They still conduct their work in the same way: they repeat, they check, they are critical. The external resistance against immunosignaturing actually makes them do even *more* checking and repeating. At a glance, one could interpret this story as an instance where economic 'strategy' is prioritised over 'truth-making'. While I believe this happens at times, it is not the case here. Irrespective of whether the scientists work with immunosignaturing or the other method, they strive to make the results as accurate as possible. They make truth claims about diagnostic methods that are more renowned in the scientific community and among the funders. But they also use the funding to advance their 'own' truth: Immunosignaturing is a better way of diagnosing. The relationship between the different concerns is complicated in this instance. They still strive to do rigorous work; they also strive to advance a niche where they believe their laboratory is strong, namely, the field of immunosignaturing. But in order to do that, they need to make a 'detour' (Latour 1987: 116) around a more well known form of diagnosing. The responsibility connected with Vocation is never neglected. However, another concern has appeared and the two move alongside each other: namely, that of maintaining the organisation (a responsibility I will return to thoroughly in the next chapter x). They see it both as their responsibility to make rigorous work and advance the organisation's speciality. And the one responsibility supports, in this instance, the other.

As I write this, I am reminded of the two-headed Janus figure, which Bruno Latour deploys in 'Science in Action' (1987). Here, he uses this figure to show how facts become stabilised over time. The two heads cannot see each other and offer different stories about a 'fact'. The head looking to the left says, '*When things are true they hold*', while the one facing right says, '*When things hold they start becoming true*' (Latour 1987: 12). So the right expresses the idea that when and if others are convinced about a phenomenon, it will be taken up and used in various ways; the fact will be cited, and it can be used as the foundation for the production of other truths – it '*starts becoming true*'. However, it takes a lot of work to work on this truth; it takes additional experiments, more papers and several checks, and, all the while, the scientists fear the money running out. If no one will ever be convinced about the merits of immunosignaturing, then the fact becomes more and more of an artefact, an idea forgotten in a drawer somewhere, never cited (or only few times) and never used (Latour 1987: 12f). The story would be that the scientists at Gyro Gearloose never managed to get it right; immunosignaturing was not the 'true' way of diagnosing and therefore the idea never caught 'hold', except among a few stubborn believers at Gyro Gearloose's.

Having been studying Vocation in action in general, I have mostly seen the right Janus head speaking. The scientists have constantly told me that they need to do more work for a fact to become true (though they never describe it in those exact words). In the above story from Gyro Gearloose, in contrast, somehow the two heads speak at once. On the one hand, the scientists really believe that immunosignaturing is the way forward in diagnosing – they believe that to be a truth as the left head says. It even seems that the more others try to discourage them from that precarious road, the more convinced they become. They believe, as Latour puts it, that they have '*stumbled over the right structure*' (1987: 13). In other words: '*When immunosignaturing is true, it holds*'. But many others are not equally convinced and this perforce makes them do more work; they need to make immunosignaturing '*hold*' more securely. Therefore, the right Janus head also appears and the lab still does additional experiments and succumbs to other methods because the scientists are concerned about the organisation's economy and possible future.

In Latour's account, the figure of the Janus head illustrates how a successful fact develops over time as more and more allies start to support it, until finally – as if by magic – the rhetoric turns and the fact becomes the explanation for other factors instead of just being the result of careful work (Latour 1987: 14). In relation to the negotiations of different concerns, I will argue that the two Janus heads can also be viewed as representing different responsibilities: In Vocation, the right head tends to speak and the insecurity of the validity of a fact makes the scientists work more carefully with their materials, always being critical about results. But in the next chapter, Oikos, where the responsibility for the organisation's survival is in focus, the right Janus head is almost silenced in favour of the left: '*When things are true, they hold*'. Suddenly, the scientists become much more confident in the truthfulness of their predictions and fight for them, even without checking and repeating. Scepticism reigns as a consequence of Vocation; faith as a consequence of Oikos. I will return to that point in the next chapter.

Summing this section up, it is clear that the mode of Vocation is not the only form of responsibility that the scientists have to deal with at work. Other concerns such as that for the lab's economic stability continue to play a vital role. At times, these shifts frustrate the scientists because they turn academia into a 'complex' work environment, where decisions are difficult to make, as the expectations are difficult to read. At other times, the different concerns reinforce each other instead of excluding each other – as we saw in the last story from Gyro Gearloose. As such, this factor does not make the work environment less 'complex'; it still takes a lot of skill and time to turn the complexities into opportunities. But the scientists do find ways to take advantage of this complexity, rather than becoming hapless victims.

7.5 DIFFERENCES BETWEEN THE TWO LABS

In this section, I will take a closer look at the differences between places. While their justifications remained identical, on closer inspection the way the scientists at Curious George and Gyro Gearloose reflected on how to fulfil their responsibilities differed slightly. However, these differences are not great. In fact, this mode generates some strikingly similar organisations. The next two modes expose greater local differences. It is interesting, how the differences between the *modes* seem bigger than the differences between the labs. This testifies to the relative stability of especially the mode of Vocation.

At Curious George, the importance of honesty and rigorous work is often openly emphasised and reinforced, for instance, the laminated poster stating, '*In God we trust; all others must bring data*', as described above. Management and the seniors are very preoccupied with teaching their juniors about the values of honesty and the responsibility they have for finding and telling the truth – though they never express it in so many words. Many of the STIR interviews are about considerations related to different choices about how to proceed with work and the choice between an easier solution and a more difficult one. The choice usually falls on the difficult solution, with the justification that it will make the claim much stronger. It only dawned on me exactly how important, as well as difficult, it is for the scientists at Curious George to handle this responsibility for the truth when I left Curious George for Gyro Gearloose. At Curious George, the subject is both somewhat delicate and, at the same time, highly valued. I could see this at the final seminar at Curious George, where the presentation of the quote about being in doubt as to whether they are critical enough created high suspense and conflict, as I described in 7.3.3.

This is in stark contrast to Gyro Gearloose. It is not that they do not care about their responsibility for the truth, indeed they do. But it is not a delicate subject, nor anything they talk much about. They never discuss it at meetings and it seldom surfaces as an issue in the individual interviews either. The only times it does is when the scientists want to do me a favour and very patiently explain the basic norms of science, as I am considered an outsider; for example, Annie, who tells me about her responsibilities as a scientist:

'And to be truly honest with you, I also have the responsibility to present my results as honestly as possible, so people pursuing this research can use them – including the things I do not succeed in.'

The scientists at Gyro Gearloose tell me about the importance of '*honesty'* in the same way that they patiently teach me about the double-blinded peer review, the importance of citations or the logics of Web of Science. They never tell me about it as a personal issue: something difficult, which they need to learn or cope with. They tell me about it as an inherent part of the job, part of the natural prerequisites for being a scientist, and they usually explain it in very general terms – not as something that is (or is not) an important issue for them personally. Most of the time, that is just the way it is. Period.

The only instance where the responsibility for the truth is explicitly up for discussion is in relation to authorship. All Gyro Gearloose's projects are interdisciplinary, understood in the sense that scientists representing different disciplines are assembled in order to solve a specific problem; for instance, how to develop a cancer vaccine.

Different specialists such as immunologists, molecular biologists, biochemical specialists and statisticians would be united in that pursuit. But Henry, the PI, told me about the difficulties associated with these collaborations. It becomes, for instance, very difficult to distribute the responsibility (and honour) for the work across papers, as the different disciplines have different traditions. Some disciplines cite the main author first, some last. It is also difficult to assess how much each scientist has contributed to a paper and whether it is enough to attribute her or him authorship. Given that they come from different disciplines, they may have contributed with something crucial, but still not enough to become an author. Henry told me that these issues cause conflicts at times, although I never experienced that during my stay. He said that the solution to these problems are to always apply the 'Vancouver Protocol' of authorship.[28] He even claimed that, as long as they are followed, such conflicts would never arise. The Vancouver Protocol states that authorship credit should solely be distributed based on 's*ubstantial contributions*' to the following three aspects of a paper:

> 1) Conception and design, or analysis and interpretation of data; and to
> 2) drafting the article or revising it critically for important intellectual content; and on
> 3) final approval of the version to be published
> (see footnote 28 for source).

The Vancouver Protocol for authorship reflects the ideas of proper conduct from Vocation. It emphasises the duty to work meticulously on the paper itself, by stating that scientists need to be part of both the analytical part, the writing part and the critical assessment of the result in order to be 'an author'. They also emphasise the

[28] The Vancouver Protocol was first drafted in 1978 by a group of editors from medical journals, who wanted to standardise the formats for submitting manuscript, inclusive bibliography styles and authorship. The standards are recognised internationally in a broad range of scientific fields. The group has continued its work with standards and ethics and its recommendations are continuously published. From: http://www.research.mq.edu.au/about/research_@_macquarie/policies,_procedures_and_conduct/documents/Vancouver.pdf 26.07.2014

obligation to check, repeat and be critical, by stating that one should be part of ‘revising it critically’ and giving it ‘final approval’. In that way, proper conduct in how to act as a scientist is reflected in the construction of a paper; what a scientist needs to do in general is also what they need to do in the specific case of being acknowledged as authors. While Henry believes that adhering to these principles can solve conflicts over authorship in interdisciplinary collaborations, I remain a little sceptical. I think the principles are rather abstract and leave room for interpretation, not least the notion of ‘*substantial contribution*’. How do you establish that something is – or is not – a substantial contribution? In any case, it is one of the few occasions where the responsibility for the truth is articulated as some sort of problem at Gyro Gearloose. And even here, it seems that they quickly silence a potential conflict by reference to general standards.

It is difficult to answer the question of why the responsibility for the truth is such a contentious subject at Curious George but not so much at Gyro Gearloose. One explanation is that ‘the Penkowa Case’ (see footnote no 24), seems to be omnipresent at Curious George as an example of the worst form of disgrace of both a single scientist and science itself. Almost everyone I talked to at Curious George mentioned it in detail; she had put not only herself, but also the entire medical science community in bad standing internationally. And that her scientific fraud was also garnished with financial fraud and the nasty rumours that she had slept with both the president of the university to avoid prosecution and the minister of science in order to win the prestigious ‘Elite Scientist Prize’ given to excellent scientists by the Ministry of Science. In that way, it seems as if the scientists made her scientific fraud more plausible by the fact that she had rotten morals in general, or perhaps the other way around; that lapses in the responsibility for the truth lead to a moral free fall, ending in financial crimes and promiscuity.

Some of them expressed, as I mentioned, a great fear of ending like her. Not that it seems like any of them are on their way. It is more as if her failure vividly evoked their biggest nightmare: One where they had involuntarily somehow been discovered engaging in considerable scientific and financial fraud. Their descriptions remind me of people's general fear of experiencing a moment of insanity that makes them jump off a bridge or out in front of a train; I believe Freud calls it the 'Death Drive'. It is quite unlikely, but being unlikely perhaps makes it all the scarier. Milena Penkowa embodied their fears of what happens if the responsibility for the truth was neglected and the scientific community stopped supporting them. They have witnessed a professional suicide. That subtle threat of being disgraced and excommunicated from the scientific world has a face at Curious George; it is more real because it happened very recently – and it did not look pleasant. The scientists at Gyro Gearloose do not have such a figure to relate to. For them, the responsibility seems much more a general norm, which they of course have to live up to; however, the idea of not doing so and the consequences thereof seem so far away that they do not give it extra thought – at least not to me.

But perhaps the phenomenon of Milena Penkowa cannot alone account for why the responsibility for the truth is such a precarious topic at Curious George but not so at Gyro Gearloose. An additional interpretation could be that the scientists in each place have quite different ideas about what kind of science they are doing, and this factor has consequences for their attitude toward the responsibility for the truth. I will argue that it is about two different modes, really: Basic science and applied science (which have been described thoroughly elsewhere in the literature, so I will not do so here). Even though the sociology of science has provided accounts of how these are quite blurred in practice (e.g. Gibbons and others 1994) they still have quite an influence on how the scientists consider their professional roles and the work they do. At Gyro Gearloose, they mostly seem to be project workers, who develop different parts of a machine. None of them can construct all the pieces themselves, and they need each other in order to realise the end product. When I asked people at Gyro Gearloose what they were currently doing, their answers were something in the line of: '*I'm working at the cancer vaccine' or 'I'm part of the group working with the diagnostic device'*, whereas people at Curious George answered, '*I'm synthesising this or that gene'* or '*I'm looking for the right buffer for my next experiment'*. The answers at Gyro Gearloose indicate that it is the end product that is in focus, while the answers at Curious George are about the process of doing the work. In that way, the foremost problem at Gyro Gearloose is perhaps not '*Is this true?',* but rather '*Does it work?*' – and if it does work, then it is probably true. As I heard Henry comment at a meeting: '*if it's good enough for a product, then it's good enough for a paper'*. The logic seems to be that if they can make their methods function as they want them to, then what they have done must reflect a reality – otherwise the technologies will not work.

At Curious George, they define themselves as '*basic scientists*' and are sceptical about the idea of being inventors of products – they want to understand the world, and it is an extra bonus if their understandings can be used for something practical. In that way, the question '*Is it true?*' becomes much more prevalent than '*Does it work?*' They cannot test whether their technologies 'work'; instead, they are left to check whether they are still 'true' the next time they perform the experiment. '*Is it true?*' has the effect of the constant questioning of one's own and one's colleague's abilities to be guardians of the truth and thereby a rightful member of the scientific profession.

But despite these differences in how much the scientists reflect about their responsibility for the truth and the personal fear of not living up to it, they are in total agreement about *having* that responsibility. As I mentioned at the beginning of the section, this mode is by far the one where the differences are the slightest between the two labs. This, I believe, says something about how strong Vocation is in science. It can be observed regardless of whether I walk into a lab in the States or in Denmark. It performs regardless of whether the scientists are juniors or seniors – they just become more self-assured over the years. Based on the analysis of the journal papers in the last chapter, it also seems that these norms are pervasive across disciplines. If we look at the literature on the scientific profession, Weber describes norms similar to those described in Vocation in 1917 and by Merton in 1973. It is quite a strong and persistent mode, seen both from a historical perspective and across places. In the theory section, I briefly described trends in science government within the last hundred years and how the role of science has shifted in connection with the purpose of government (2.1). In these different periods, the pursuit of the 'truth' regardless of whether it was for the sake of curiosity, as a means for efficient fighting in war or as a motor for national growth has always been the central part of the job. The problems arise, when other concerns start to make it difficult to actually perform the research and thereby guarding the truth. Responsibilities such as those to commercialize or 'engage' with citizens start to loose their meaning if they are not done on the basis of actually doing a lot of research.

In my interpretation, this is why this mode is so strong and so uniform; it has been the key responsibility for scientists historically, and even if other responsibilities have come and gone, this one has persisted. I will continue the discussion about the pervasiveness of the modes in the next two chapters and in the final discussion.

7.6 VOCATION IN A 'SOCIOLOGY OF SCIENCE' CONTEXT

I began this chapter by asserting that descriptions of the scientific profession as a vocation had been done by several authors. I also identified political rationalities akin to that of Vocation in chapter 7. Now I will return to these former descriptions and consider the mode of Vocation in the light of these writings. First, I will discuss Vocation in relation to the political rationalities from chapter 5. Second, I will discuss the findings in light of Merton's CUDOS norms, Weber's 'Science as a Vocation' and the descriptions of Vocation in 'Organizing Modernity' (Law 1994).

In chapter 5, I identified two political rationalities that bear similarities to Vocation, namely, the '*Demarcation Rationality*' and '*Reflexivity Rationality*'. What these two, first and foremost, have in common with Vocation is the insistence on science's right to autonomy. The Demarcation Rationality articulates this autonomy as a precondition for making science work: if 'society' – understood as interests and other motives besides truth seeking – is not kept out of scientific conduct, science is simply not responsible – nor is it really science. The articulations of the Reflexivity Rationalities are more pragmatic in so far as they identify science's main responsibility as solving society's problems. Thus, a need to engage with society is acknowledged, but the right to decide which problems they want to engage with and how remains.

The descriptions of Vocation are very similar to these two rationalities. The scientists consider themselves as having the responsibility to find truths about the world and solve problems. The checking, repeating and being critical can be understood as very practical attempts to keep 'society' out of the scientific work. By checking, repeating and being critical the scientists try to make sure that 'society' in form of issues as personal bias, a wrong measurement or a badly cleaned flask influence the experiments and the following conclusions. The connections between the responsibility for the truth and the way the scientists try to live up to this standard are seldom made explicit in the daily laboratory work the way they are in the texts from the previous chapter. Helping 'mankind' and similar statements are rarely made in connection to the mundane everyday tasks; these procedures are just part of everyday work life. Both laboratories recognise the responsibility to address societal problems, though they do so in slightly different ways. Curious George considers the conduct of what they see as ‘basic science’ as a way to engage in society, whereas Gyro Gearloose has a strong emphasis on providing tangible technologies that answer more immediate and known needs in cancer treatment and diagnostics. In that way, both ideas from the Demarcation Rationality and the Reflexivity Rationality are interwoven in the ways that the two laboratories try to live up to their perceived responsibilities. However, there is quite a lot of room for adapting to and interpreting the ideas about the responsible conduct of science as presented by the two rationalities. So the mode and the rationalities share the emphasis on autonomy and internal regulation of science as a condition for being responsible.

But the findings from the previous chapter suggest that Demarcation and Reflexivity differ in their understanding of the *process* of scientific conduct or the *outcomes* of scientific investigations as those that should be ‘responsible’ (see 5.4). In my interpretation, the focus of Vocation is entirely on the process. The checking, repeating and criticising are all aspects that seek to address the way scientific investigations are conducted, but it only relates to the process of doing science, not the wider consequences of their findings. The scientists do seem to find it amiable and meaningful to work on subjects that they believe improves aspects of society, but the connection between responsibility and their daily work mainly concerns the process.

Based on interviews with scientists, Merton developed the CUDOS norms as a description of the general norms that guide scientific work. The scientists in the two laboratories often make claims and conduct themselves in ways reminiscent of the CUDOS norms. At least they underline the importance of the scientific community and as I have shown they are indeed also very critical about their own work. They are also very careful to remain ‘disinterested’ and not letting their personal interests interfere with their experimental work.

But there is also another similarity. Merton described the norms guiding science without much concern for the contemporary institutional conditions that made these norms possible at that specific time and place in history. In the descriptions, he does not write anything about the setting these norms are working in; the research group, the university or the science-based firm. Neither does Vocation generate any talk about the setting. The scientists describe their responsibility for the ‘truth’ as a general and natural phenomenon that will look the same, no matter the context and conditions. In my interpretation, this is because the scientists exclude these conditions on purpose; In the same way that ‘society’ should be kept out of scientific work, so should external conditions be excluded from their ideas about responsibility, as responsibility is a question about personal morals and integrity. Neither the scientists nor the CUDOS norms seem to take into consideration the external context in which the scientists have these obligations; namely, that of being affiliated to a university that also has a significant role in society, being part of a group or an organisation, and working under (perhaps) changing institutional conditions. All these aspects of scientific work are excluded and only the individual’s inner morals and the profession’s standards seem to play a role for their understanding of – and also performance of – responsibility in this mode.

In that respect, both the scientists in the laboratories and Merton’s CUDOS norms differ from Weber’s descriptions of the scientific Vocation. Weber starts out by describing the conditions for conducting science (comparing Germany and America) at public universities; that is, the hierarchies and the job’s core tasks. Furthermore, he describes the changing role of scientists working from Humboldtian ideals to science as a job and career, a development that Weber does not embrace wholeheartedly

(Weber, Owen, and Strong 2004: 4) He combines these conditions with the inner vocation, the need for passion for the tedious processes and the contributions science can make in bringing 'clarity' (but not truth!), if not to society, then at least to the scholar himself:

'This brings us to the last contribution that science can make in the service of clarity, and at the same time we reach its limits. We can and should tell you that the meaning of this and that practical stance can be inferred consistently, and hence also honestly, from this or that ultimate fundamental ideological position.' (Weber 2004: 26, emphasis added)

In Weber's insistence on the institutional conditions as an inherent part of the vocation and his insistence on 'clarity' rather than truth as a goal for scientific inquiry, his description varies quite a bit from that of Merton and indeed the scientists at Gyro Gearloose and Curious George. While I believe that the differences between Merton and Weber can be attributed to differences in theoretical standpoints[29], the case is different in relation to the scientists in the two laboratories. I believe these differences have something to do with the theme of 'responsibility' and the way I asked them about their jobs. They also spoke about 'passion' for the job, the creativity involved and what it meat to be part of a group, a public institution and a university. But they did not connect these aspects with their meticulous work with materials or their responsibility for being 'honest'. Neither did they seem to believe that 'clarity' was enough; they were, as shown, very preoccupied with the responsibility for taking care of 'the truth' as an inherent part of their profession.

[29] Merton was quite inspired by Weber in his doctoral dissertation (1970) but his descriptions of the scientists were (according to Lynch 1997) based on the descriptions of the 'bureaucrat' rather than the Scientific Vocation.

Law's descriptions of 'Vocation' embrace both the aspect of abiding to strict standards for how science should be conducted and the creativity, both of which are ensured through the adherence to these standards (Law 1994: 81). His focus is on how Vocation is about skill; how to successfully internalise the very conservative rules and the very explorative potentials in the job and make them into a *Fingerspitzengefühl*, something the scientists just *know* how to do, instead of knowing it theoretically. While this focus on skill is perhaps not something the scientists talk that much about, it is a big aspect of Vocation: I have describe how the young scientists struggle to learn this very delicate combination and how they at times suffer in doing so. In that way, Law's ideas also relate to the responsibility in Vocation, because they do need to learn to balance and integrate these two aspects of their work in order to be 'scientists'. That being said, they do not seem to talk much about the creativity involved in their job in relation to the theme of 'responsibility'. They do articulate the opportunity to discover something great or invent something that addresses a huge challenge, but not as a responsibility, more as something they hope to do.

So some aspects about seminal descriptions of the scientific Vocations can be recognised in the way that the scientists consider their responsibilities. But the specific theme of 'responsibility' makes the mode of Vocation a little different from earlier descriptions. The focus on process and adhering to strict norms for good work are in the foreground, whereas aspects such as 'creativity' and 'clarity' and the context in which the scientists do their work are in the background. However, some of these aspects are seen as significant in the two next modes that I describe. I will therefore now turn to the chapter called 'Oikos'.

7.7. CONCLUSIONS

In this chapter, I have shown how the scientists share a certain responsibility for the 'truth' and how they exercise specific forms of conducts to honour and uphold that responsibility. I have also shown how guarding the truth is a skill that it takes time and practice to learn and that the juniors struggle to learn so. I have argued that the scientists look at the responsibility for the 'truth' as closely related with what they consider to be their central job, namely that of doing research: conducting experiments and writing papers. Therefore they also consider this responsibility to be the most important one, compared to the two other responsibilities that I will look at in the next chapter. I have argued that the way the scientists tell about this responsibility is closely tied to an understanding of science's role in society, where science is one among several stable institutions. Its job is to distribute new knowledge and a good work-force to the other institutions, but this demands that the scientists are left free to be creative and have substantial funding and time on their hands. Therefore the scientists fight to be as autonomous as possible. This idea about science's role in society brings thoughts of the conditions for American science in their Golden Age, right after World War II, where scientists were generously funded and had much freedom to do 'basic research' in the hope that it would eventually lead to revolutionary technologies (Shapin 2009). This specific way of organising science is thus still performed in Vocation.

In relation to the question of governance, it is clear that Vocation is connected to strict self-governance through norms for good scientific behaviour; that is behaviour such as checking, repeating and criticising and that these norms are uphold through apprenticeship, where the juniors learn the skills from the seniors. Society does as such not 'speak back' (Nowotny 1999) in this mode, but that does not mean that the scientists see themselves as 'outside' society or exempt from norms about good behaviour. But their norms are closely related to the core task of doing research and deliver knowledge to the other stable institutions in society. This is in itself quite a big job and takes up a lot of time. Therefore they are also careful to protect their autonomy.

In that way this mode can also be seen as in contrast to the newer demands for 'Responsible Science' as the scientists do not embrace the ideas of external governance and further responsibilities for the 'social' aspects of science. But this is not necessarily an argument that simply relies on technical-instrument rationality. Instead it relies on the idea of a 'society' with different specialized tasks and where the scientists believe that politicians will take care of these concerns. Furthermore, it relies on the concern for their everyday worklife, where they are already fairly busy in having the responsibility for the truth. While the scientists are at times frustrated with the many failed experiments and the time they need to spend to get a good result, it is a part of their work that they never question – they are even proud that they are able to go through such hardships in order to guard the truth. It sets them apart fro other professions. Adding to this responsibility can be burdensome and takes up quite a lot of time. A point that I will return to in the coming chapters.

8. OIKOS


LUK ET
STINKSKAB
– FÅ RÅD TIL
EN PH.D.-FLAB
Kom med i bevægelsen
for et energirigtigt KU
Grøn Gerning

In this chapter, I will describe a different mode of responsibility than that of Vocation. I have chosen to call it 'Oikos', and, from this perspective, the labs all of a sudden look very different from how they appeared in Vocation. People, work and things are arranged acceding to another responsibility, and, consequently, it is as if I have entered two completely different labs than Curious George and Gyro Gearloose, though the observations took place at the same time and place as those of Vocation. Suddenly, as I will demonstrate, severe scepticism is replaced with firm beliefs; time has become a scarce resource; and the juniors are left to do 'science', while the seniors do 'politics'.

Oikos is the Greek term for '*household*', and the prefix 'eco-' in, for example, economy and ecology, stems from it.[30] In Greece, Oikos referred to the entire household, including men, women, children, slaves, the possessions and animals within it, as well as the property itself. While the male head might have had the responsibility for managing the estate in its entirety, everybody had a contributory function. I have chosen this name because this mode has the responsibility for 'the business' as its core. By 'business' I mean each lab's procedures in maintaining resources to keep it alive and well. In order to meet that responsibility, the scientists *do* certain things that remind me of the chores we all have to do in a household so that it does not fall apart or get taken over by somebody else. We have to clean the bathroom, take care of the kitchen equipment and turn off the lights before we leave. We also have to make sure to pay the rent and lay money aside for future, unknown expenses as well as save up money for things that will improve and maintain the home, such as a dishwasher or a weekly cleaning service. If that is not enough, we also have to work or invest in order to have an income. Furthermore, we have to make sure that all the members of the household are doing reasonably well and are

[30] http://global.britannica.com/EBchecked/topic/178273/ecology 07-06-2014

doing their part in maintaining it. The things that are expected vary, but they are united by the responsibility for keeping the home economy healthy.

In some ways, Oikos shares similarities with seminal descriptions of '*the entrepreneurial scientist*' (Etzkowitz 1998). The entrepreneurial activities of scientists are, as such, not a new phenomenon. There are examples of scientists making businesses out of their inventions as early as the 17th century in Germany, but, before the 1970's, these activities did not alter the laboratories or the scientific profession in any serious way (Etzkowitz 1998: 824). However, within the last four decades, a

'web of relationships has grown up among academics, university originated start-ups and larger firms. Often the same academic scientists are involved in both types of companies, managing a diversified portfolio of industrial interactions' (Etzkowitz 1998: 423).

These developments have created a new figure, 'the entrepreneurial scientist', who is an academic and a businessman at the same time: One who seeks to take commercial advantage of his or her innovations. According to Etzkowitz (1998), this has also changed the organisation of research. The boundaries between industry and academic science are dissolving as academic science is done in R&D departments, and industry both contracts university researchers and funds their studies. As more universities take on these collaborations, which are also encouraged by public university reforms, the competition for funding between universities has expanded (Shore and Wright 2004: 58). In turn, tenured positions are set aside in favour of project employment based on external funding. This makes the academic job market more dynamic and changing. These tendencies started in biotech but have expanded to various parts of the universities, mostly the natural sciences. While the rise of 'entrepreneurial science' as a structure is described here, Etzkowitz does not say as much about the behaviour of the 'entrepreneurial scientist', though he does mention a 'cognitive shift' in the way the scientists see science's role in society (Etzkowitz 1998: 829). He claims that the disciplined pursuit of knowledge (Vocation) is supplemented with an attitude that acknowledges science for its commercial uses as much as for its intellectual properties.

Law describes 'enterprise' – one of the modes of orderings – as a specific attitude among the scientists, which also shares similarities with my findings:

'So this [enterprise] *is an ordering mode about agency, about how people are, or how they should be. But it is necessarily, at the same time, an ordering practice that has to do with structure too.* […] [T]*he fragments of structure add up to a set of opportunities, a set of resources. For if they are not treated as an opportunity, then the agent is no longer acting responsibly; instead she is in the process of retreating from the proper performance of agency; she is undermining her very status as an agent. So, like the other modes of ordering, enterprise is a morality tale as well as a description.'* (Law 1994: 75-76)

The entrepreneurial mode is thus one where the conditions for doing science, '*the structure*', should be seen as opportunities and resources, rather than limits and annoyances. They should be creatively used by the scientists to make their lab thrive. If this is not the attitude, one is not even living up to one's responsibility, one is not even 'entrepreneurial' anymore. Law's description of entrepreneurship as a certain moral comes close to the descriptions of Oikos in this chapter. Oikos is about having some tough conditions and attempting to make the best of them. Even attempting to turn them into an advantage for the lab, so that it may do better, and they will be able to hire more people or buy new equipment. However, it is also different. While entrepreneurship is definitely needed as a skill to make the labs survive, this mode of responsibility is not identical to 'enterprise'. There is more to it: Oikos is not only about the responsibility to seize opportunities, it is also about the daily care of the lab, where both juniors, machines and buildings are handled carefully, so they can continue contributing to the business's economy.

As in the previous chapter, the description of Oikos is structured as follows. First, I will identify three forms of conduct, which I believe to be typical of this mode, namely, '*investing*', '*saving*' and '*maintaining*'. Second, I will carve out the justification for these three forms of behaviour, namely, the responsibility for the 'business'. Third, follows the descriptions of the organisational consequences of this responsibility and how it is negotiated in relation to other concerns and how the performance of the mode differs between the two labs. Last, I discuss the findings in relation to seminal literature about entrepreneurial science and public reforms of the universities.

8.1 CONDUCT

In this section, I will describe three forms of conduct, '*investing*', '*saving*' and '*maintaining*', which are pertinent to Oikos. Though they may look very different, all three of them have at their core a care for the organisation. They all constitute ways of going about daily work that are considered necessary if each lab is to survive. In that way, the three forms of conduct are also ways of dealing with the current conditions for doing scientific work and actively relate to the lab's surroundings. It varies according to whether the scientists consider themselves as being victims of powerful structures or as being able to change the way things are done. I will relate to this topic during the descriptions.

8.1.1 INVESTING

This form of conduct is the one that I first noticed, mainly because there is so much talk about it. The scientists often mention the efforts they put into securing the survival of the lab in the future. They explain that they have invested time in writing new research applications, have invested money in new machines for future experiments or have hired new PhD students, of whom they have great expectations. Some of these explanations have developed into anecdotes that are shared among the scientists. These stories are about their great efforts and the achievements or failures that followed. One of the most popular stories at Curious George is about how the lab – or Canute, as the story often goes – landed the biggest national public grant in Denmark. When Canute tells the story himself, it goes something like this:

Professor Canute and his group had applied for one of the biggest national grants and had used a ton of time on the application process. After submitting the application, Canute was asked to come in for a meeting at the Vice Chancellors office. Another PI from a nanotechnology lab had also submitted a proposal for the same national grant was also invited. The Vice Chancellor was very straightforward:

'I see the term synthetic biology in both your applications,' he stated. 'If you can develop a common project around synthetic biology, we shall forward your application to the granting agency' Professor Canute and the other PI agreed to give it a try. Professor Canute recalls the process: 'We sat down and worked for two months. We come from different disciplinary backgrounds and it took a lot of time to truly understand each other's theories and methods. At times, we had to go back to basic high school chemistry and biology in order to reach a mutual understanding. But in the end, we developed a common framework with synthetic biology as a common denominator. And subsequently, we got the grant.'

Due to professor Canute's efforts, the lab got a large amount of resources to hire new staff and buy new machinery. They also received new research goals to achieve, new methods they had to learn, new administrative chores and new partners in other labs. The laboratory grew due to the new grant. Synthetic biology also became an entity they had to relate to and, not least, actually do, which I will tell more about later (8.3.4). Curious George, and not least Canute, thus invested a considerable amount of work hours and manpower in writing the application and starting a meaningful collaboration with the other lab. In return, they got resources, new research agendas and new collaborations. But this influx of resources needs to be quite stable. An expansion of the lab needs to be maintained. As of this writing, the grant is still running (five years all in all) but Canute has started to invest his time in new opportunities:

'Where's Canute?' I ask as I haven't seen him around for a couple of days.

'Oh, he is in Brussels', Inez, the communication manager replies, 'He is trying to persuade the EU Commission to give synthetic biology a prominent role in the next Framework Programme for Research.'[31]

Now that they had worked on the National Grant for a couple of years, it was time to see how the knowledge, the machines and the personnel they had gained could be reinvested. Canute had gone to the EU capital to promote synthetic biology as an important solution to a range of societal challenges. The framework had not been made in its final form yet. Therefore, it was important to influence how it was framed, that is, which disciplines and challenges were mentioned and how, so the chances of actually answering in a meaningful way on the proposals would be easier later on.

But Canute does not solely focus on synthetic biology during his visits. He also talks about 'social responsibility' in science. His lab worked together with a philosopher and some social scientists during the previous project. They focused on 'ethics' in connection with the development of synthetic organic materials and democratic legitimacy in science. Canute now tries to promote the idea that considerations about social aspects of scientific development should be included as part of the calls.

In the next chapter, I will take a closer look at concerns for the responsibility the scientists feel for 'the public'. But in this chapter, the concern is for the business, and Canute is investing some of the lab's gained 'capital', namely, experience in working with social responsibility, to enhance Curious George's chances in the next round of investments. By suggesting that concerns for social responsibility are emphasised in the next framework for research, his own business gets a strategic advantage in the

[31] Horizon 2020, the new EU framework for research focuses explicitly on science's response to Europe's great challenges such as innovation, growth, climate change, sustainability and democratic inclusion. From www. http://ec.europa.eu/programmes/horizon2020/en/h2020-sections 20.08.14

application process as they already have experience in that area. So the knowledge from working with synthetic biology and the experience with social responsibility is reinvested in an attempt to land a new grant.

Landing grants is a big issue at both Curious George and Gyro Gearloose. At Curious George, 75% of the lab's budget is based on external funding. The last 25% is granted by the university as payment for the teaching the department partakes in. At Gyro Gearloose they rely 100% on external grants. During my visit there, I heard a rumour that the square feet of all the labs in the building are allocated based on the annual budget of each lab. So not only are the labs shrinking or expanding in manpower depending on the local economy, but the actual space to do the work follows suit. Though I heard the rumour from several sources, I never had it confirmed, so I am not sure whether it is true. But even if it is not, it is telling for the way that many of the scientists look at their workplace – as a place where everything is uncertain, even the ground they stand on.

Investing time in opportunities for grants matter because they are essential for both the scientists' own job opportunities and the lab's survival. These are interdependent. Therefore, many of the employees at the lab spend a considerable amount of their work time on applications in one way or another. They invest time in writing grant applications, meeting with representatives from funding agencies or gathering potential manpower for a new group. Mostly, it is the seniors who invest their time in such a fashion. Not only do they work to secure their own salary but also to secure new positions and new equipment. Some of the PhD students and postdocs tell me about that kind of work to, albeit on a smaller scale. They mostly invest time in securing their next position, either by writing grant applications or cultivating good relations with the seniors, who may let them participate in the next project.

The question of how to land a grant is a topic of constant discussion among the scientists. Where to invest? Which foundations or councils are considered low-risk, high-gain? Should all the resources be placed in one unique fund or is it better to spread the risk and apply for several smaller funds? The question of the *quality* of the investment is also up for debate: Is there an optimal way of phrasing their words, formulating the texts and interacting with potential stakeholders? Perhaps this is the most discussed issue. New research ideas are assessed on their ability to generate funding. The scientists discuss new grants in relation to their ability to relate to immediate societal challenges among the funding agencies, such as '*cancer*' or '*climate change*'. Or they go into areas of research that are similarly topical such as '*metabolism*' or '*enzymes*'. If their idea does not relate to a popular theme directly, they discuss how it could be shaped in order to do so, either by rhetorical means or by changing the research idea slightly so it is more in line with what they expect funding agencies to go for. The following excerpt is an example of this kind of discussion from Curious George. The upcoming deadlines for relevant funding are on the agenda at a meeting among the PIs:

One of the PIs needs a new TRM machine in order to proceed with his work. The one they already have in the lab is almost worn out. He considers applying for money from the Lionburg Foundation, an agency that was founded by an international brewing company. The others think it is a great idea. 'Remember to mention hops in the application,' one of the other PIs comments. 'Lionburg wants to develop sweet beer, so they can convince women to drink more beer, so hops are the new black.'

By establishing a connection between the new TRM machine, the prospects for brewing based on hops, and the lab's expertise in cereals, the PIs expect to increase their chances of getting the grant, even though research in hops is not on the immediate research agenda. A few minutes after the exchange about the TRM machine, the PIs discuss options with regard to the Halleys Foundation as their deadline approaches. They are not really sure what to apply for but: '*Remember not to mention anything about applications* [applied science]. *They are only interested in basic science'*, one of the PI reminds the others. So in one of the stories, it is all about the application potentials. The job is to make reliable connections between a TRM machine and beers for women, but without promising to look at the challenges related to beers for women. In the other story, it is paramount to disconnect their research from any kind of applied science. In some instances, the scientists at Curious George try to convince the funding agencies about their merits in applied science, whereas at other times they emphasise their expertise basic science. It depends on what they need and what they can get. The scientists often tell me that '*this is how the system works*', and they do not see any way of changing that. But they can learn how to 'navigate' it. Simon, one of the PIs from the meeting, explains about navigating in an interview afterward:

'The secret is to write an application in a way where you meet the politicians – or those who distribute the money – so they hear what they want to hear and you get to study what you want to study. Those two things do not necessarily need to be the same, but the secret, as a scientist, is to answer their questions in the applications, and then you must, five years later, explain why you, by the way, couldn't solve the problems. And there is always a good explanation for that. And I also believe that they [those who distribute the money] *know that this is how it works. I certainly hope that they know that, because otherwise they are pretty naïve and I don't necessarily consider them to be that.'*

Interpreting Simon's statement, it seems as if the relationship between funders and those receiving funds is staged and everybody takes on particular roles – one part hears what they want to hear and the other part tries to attune the application to what is expected. Then they go their separate ways and both parts do what they had planned to do anyway for the next five years. But according to the PI, both the scientists and the funders know that this is how it works – otherwise they are '*pretty naïve'*. So my interpretation of Simon's quote is that this is about a specific attitude to your job. The scientists at Curious George agree to partake in a staged performance where they have to say or write certain things at the right time – mention potential applications or not, for instance – in order to receive money so they '*get to study what they want to study*'. The scientists consider this a system they cannot change: This is just the way things are, '*…this is how it works*' and you have to play by the rules of that system in order to get as much out of it as possible. In the previous chapter (7), the scientists asserted that while they do not particularly like the funding system, it does '*not affect their research*'. I cannot judge whether Simon's statement is true or not. I know that while most of the scientists also find the funding system very complex and at times confusing, not all of them will agree that it is just a staged performance. But Simon's considerations reveal the extent of the efforts the scientists do in order to '*study what they want to study*'. While the scientists at Curious George at least feel that they move in directions they want to, the investment of time and skills in reaching that point takes up much of their workday.

The scientists at Gyro Gearloose also invest a considerable amount of work hours in applications. But the way they go about their investments is a little different. While they also consider how they can tap into different popular themes and areas of research, they claim that they simply refuse to take part in a staged performance, where they either make too many compromises or say that they want to study something other than what they intend to study. As Henry says: '*We actually mean what we say*'. This has something to do with the way they set their goals '*backward*': They start with a goal they want to reach, e.g. a prophylactic cancer vaccine, and then they work backward and try to see what kind of knowledge they need to reach that goal. They therefore only apply for grants where this strategy is possible, either by making a contract where they are hired to invent a very specific technology, or grants where they can explore a wider range of potentialities, albeit taking their point of departure in the final technology.

While their approaches to investment differ, the two labs do share the idea of neither having much power in the relationship with the funding agencies nor in the general structure for obtaining resources. The golden era, when the researchers had a stable influx of money, is over, and they now see themselves as small players in a big market with fierce competition. They can learn to '*navigate*' in the system, as they constantly say, but they do not believe that they can change the rules of the system. Law (1994) talks about '*agency lost*' (Law 1994: 60) in relation to research management. This concept covers the feeling of being a victim to structures that cannot be conquered but only managed to the best of one's ability. In relation to funding, the scientists generally feel like victims of a structure. But as I will illustrate later on, they also have possibilities to '*regain agency*' (Law 1994: 61), it just takes a lot of work.

Observing the scientists invest reminded me of the '*Credibility Circle*' that Woolgar and Latour described in 'Laboratory Life' (1979). The 'Credibility Circle' shows what a scientist needs to do in order to 'move' forward in a scientific field. They demonstrate how recognition in the scientific field can be translated into a grant (money). The money is then invested in equipment that can generate data, which the scientists (if they are good) can translate into arguments and articles. These articles can then be exchanged for recognition, which lays the ground for yet another (and perhaps bigger) grant; hence the circle (Latour and Woolgar 1979: 201) In this understanding, the scientific work is seen as an economy, where knowledge and recognition can be exchanged for money and grants. The investment of time and skills in getting new grants reminds me of this economic circle. In this interpretation, the endeavour is quite strategic: The scientists know that the grants presuppose knowledge and recognition and they try to build up an 'economy' for themselves. But the scientists are not only doing this work in order to make a move in the scientific field, they are also doing it because they have a responsibility for and to their place of work. They need to supply the lab with resources so it can survive and thrive. As in most economic theory, the scientific agent in Latour and Woolgar's circle is self-interested; the theory is about the advancement of a scientific fact and personal recognition. But my argument, which I will describe in the Justification section, is that the scientists are not necessarily thinking so much in individual terms as in organisational terms when they invest.

8.1.2 SAVING

Investing time, skills and knowledge in the next project is not the only form of conduct that Oikos performs; the conduct of saving is just as important. I noticed a lot of concern for the resources that the scientists handle in their daily work. They are careful to return chemicals to the correct shelves so others can use the leftovers; they think about how much water they use; they are painfully aware of the prices of all the ingredients in their experiments; and they handle their expensive machinery with kid gloves. Once, when I followed Sandra around, she complained about the computer programs being old. '*But I guess I'm spoiled', *she hurried to add; *'[w]e had exceptionally good funding at my last job'*. Complaints about the labs' equipment were otherwise quite rare and awareness of using the lab's equipment with consideration was generally high.

I'm following Miriam around the halls. She is gathering different kinds of materials for her next experiment. 'I really can't screw up this experiment,' she tells me, 'it's too expensive. These two babies,' she shows me two bottles each the size of the *perfume samples they hand out for free in department stores, 'cost around 100.000 grand each.' She notices my sceptically raised eyebrows. 'Oh yeah, well perhaps not that much, but they* ***are*** *really expensive, so I need to get the experiment right in order not to waste them.'*

In the last mode, redoing experiments showed dedication and the right kind of attitude to the work, however, in Oikos, things have been turned upside down. Now an experiment should preferably produce the wanted results the first time around because the scientists feel obliged not to use too many of the lab's resources. The responsibility for the truth from Vocation has been replaced with a concern for the lab's resources. That does not mean that Miriam will not repeat the experiment if the experiment goes wrong. But she will probably have to argue her case and underline the importance of that specific experiment in relation to her overall studies. She will also have to consult both her supervisor and the very strict purchasing department. Another concern is now all of a sudden of the greatest importance, a concern that was invisible in Vocation. The scientists do their utmost to reuse where they can. They consider new ways by which experiments could be more efficient and thereby cheaper to conduct. Their meticulous way of considering efficiency is similar to the meticulous work conduct observed in Vocation; however, their responsibility is not to find the truth, but rather to make the resources last. They invest in machines that reduce costs in energy and time, and they remind each other of best lab practices, which save energy and money.

This concern for materials is not only confined to their current experiments, it pervades the way they think about working as scientists in general. Concern for not wasting their materials also comes through when the scientists imagine what will become important in their future work lives. Not wasting resources is, for instance, a great concern in relation to the new laboratories that the university is building for Curious George and other areas of the science faculty:

Curious George will be moving to new buildings within the next couple of years. But the buildings are not finished yet. In fact, they are still just sketches on the architects' drawing boards. I follow Simon, who is engaged in the building project, to a meeting with the architects. They discuss the design of the new laboratories. First, how much it will cost to make a triple security system in order to avoid experiments and materials getting ruined in the event of a major electricity cut. They find it difficult to assess if it is worth the money. Who knows what the risk is and how much they would eventually loose? Then they move on to the general lighting of the laboratories. Simon insists on LED lighting because it is so much cheaper than other forms of lighting. 'Even Barney [referring to one of the other scientists] *is finally convinced that LED is the way forward and it actually works,' he says to the architects, 'so go for it.'*

As the new buildings are being planned, the scientists try to implement structures in them that will hinder the waste of money, time and materials. They attempt to estimate how much their future work is worth and how much protection against potential breakdowns can be obtained in return for the huge price of the building. They also suggest what they believe to be the cheapest and most modern form of lighting, namely, LED lighting. It is interesting how the scientists stretch their responsibility to include savings in the future. They try to assess what they will need and what scenarios could possibly occur: For instance, a breakdown of both the general electricity system and one of their local power stations at the same time. These discussions between architects and scientists actually remind me of those public engagement exercises on emergent technologies that I have observed as part of my studies and in earlier jobs. And it strikes me how fragile the decisions are. All the actors involved in the building try to imagine the best solutions for the future based on their personal and professional knowledge and values. Furthermore, they try to foresee future catastrophes for the lab and think of how to avoid them. They also suggest state-of-the-art technologies such as LED lighting because its energy saving, but it seems such a small detail compared to the entire project (though I am sure they have gone through tons of other details at meetings when I was not present). Their deliberations about the future therefore seem quite fragile and insecure to me. However, in the same way that public deliberation about emerging technologies perhaps says less about what the future will bring than about present values (Borup et al. 2006), the discussions here say something about what is an important value for the lab right now. That is, the foremost concern is how to save resources and make sure their work is never wasted.

The same kind of awareness is true of how they spend time. The scientists often check smartphones and watches in order to assess whether more tasks can be squeezed in before the day is over. Catherine brings tubes home with her after hours so she can number them after the kids have been tucked in. It is '*much more efficient*', she tells me, to do the routine work at home instead of at the lab. The scientists often have strategies like that for saving time. They calculate whether they are able to run another sample while another part of the experiment is cooling down, or whether they can stir a couple of ingredients before the next meeting begins.

Alan has two jobs at Gyro Gearloose. He is developing a treatment for acne and he works as a lab technician. He tells me how he plans his workday, moving between his own experiments and helping the others. It takes careful planning. He has to strike a balance whereby he can move his own project along at a reasonable pace because he needs to be done by the summer, when he will start grad school. At the same time, he also has to do the work as a lab technician for the other scientists. He carefully puts *some time aside for each experiment and he often has several tasks to attend to at the same time. The trick is to assess the amount of time for each task and make them fit together, he says. He is becoming much better at that recently, he tells me. In the beginning, he 'wasted' a lot of time because he was not that experienced in handling each machine and in attending many tasks simultaneously.*

Reducing time '*waste*' is considered just as important as reducing material waste. This demands that the scientists have a good feeling of how their materials behave and how to operate the different machines. Often something goes wrong and the scientists have to start all over – as described in Vocation. But in contrast to Vocation, the scientists are suddenly very concerned with wasting time and curse when they have to start all over again. The missing brain receptor that Clark describes in Vocation (7) – the one whose absence makes the scientists immune to frustration – is suddenly very much present in their brains.

The same considerations about time are present in relation to the longer-term perspectives of their work. Not only do the scientists carefully manage their time on an everyday basis, they also do so in relation to an overall assessment of how much work they can do before the money runs out, before the project finishes or before the PhD stipend is used. How much can they accomplish in the time that is left? Can they attend the next important conference with a paper presentation or is that too much? They do not distinguish between time and materials in this mode: Both are considered finite resources, which they need to handle respectfully. As Sandra from Gyro Gearloose answers to the STIR question about facing a choice during the day:

'Uhh, yeah. Just figuring out which of the different projects to work on… So… I needed to move the re-immunizations along, but I have been waiting to talk to Emma [the PI on the projects] about possible ways to move along. Like, [I] *just don't want*

to plough ahead with that and then have her say 'why did you do that?' And then we have wasted time immunizing mice and wasted materials […]. *So you are basically left with two choices; either work on the array data from last week or finishing characterizing the* […]'

While the technicalities of this statement may be difficult to comprehend for an outsider to Sandra's work, a general concern comes through; how to manage time and materials in relation to the research projects is something she has to think about everyday. She does not want to '*waste time immunizing mice and waste materials*' if things can be done more efficiently. Time and materials are the two most precious resources not to waste and the scientists are careful not to. So careful, in fact, that Sandra thinks that the question of how to move along is a management decision and not just her own choice. Management does seem to have a big say in how to spend the resources, not least because the financial limits are often set by the PIs of each project. In the mode of Vocation, I described a situation where Annie from Gyro Gearloose presented her work to the others in the group (see the full story in 7.4). Someone from the audience asked why she had not used the diagnostic method called 'immunosignaturing' in the method she developed. The PI cut in and told the audience how the funding agency had set the use of a more traditional method as a condition for the grant. Gyro Gearloose had accepted that but decided to move the project along with both the traditional method *and* the immunosignaturing method. They therefore developed both a technology for their funders as well as their own line of research.

In connection with the mode of Vocation, I clarified that these two methods are a result of a negotiation between a concern for the organisation's finances and a concern for advancing a certain type of truth – that of the efficiency of immunosignaturing. Looking at this story from the perspective of Oikos, the story takes another twist: Gyro Gearloose applies for money to perform a specific form of research, namely, developing a diagnostic device based on a specific method. They receive the money and move along, not only with that assignment, but also an assignment where they develop a device based on immunosignaturing. However, this means that they have to make two projects for the price of one in terms of manpower, materials and time. Seen in that light, the importance of saving becomes very apparent. They need to be very meticulous when undertaking an assignment that is larger than the funders imagined it would be. The resources, be it the scientists themselves, time or the materials, are spent carefully, but creatively. And to make ends meet, it is deemed important that the PIs be involved in the decisions on how to move forward. The scientists believe that the PIs should decide the right balance between different priorities. I will go into more depth on the role of the PIs in Oikos in the sections '*Oikos and Hierarchy*' (8.3.1) and '*Fast Science*' (8.3.2). But in connection with this story, I want to emphasise that this part of being 'entrepreneurial' is discussed to a lesser degree in the literature (for exceptions, see Gorm-Hansen 2011). The competition for funds demands that more work is done for less, especially if the laboratories want to '*regain agency*' (Law 1994: 61) and move in a specific direction with their science. Earlier, I showed that the scientists believe that reliance on external funding does not affect the way they do their research, but the amount of work and 'navigation' they need to do in order to pursue their own scientific interests becomes apparent.

The interpretation of another story from Vocation also takes a twist if seen from the perspective of Oikos. I described a scientist entertaining a dinner party with a story about 'European' laboratories closing after 5:00 p.m. and being closed on weekends. Something they considered quite strange (see 7.3.2 for the full story). In the previous chapter, I interpreted this story as part of an understanding of time that is performed in Vocation: Scientists need a large amount of time to work with their materials if they are to handle the truth responsibly. However, in Oikos it can also be interpreted as an articulation of the competitive advantages that the USA (according to the story-teller) obviously has. This is because the USA has realised that it is necessary to engage the workforce over the weekend and after hours if they are supposed to do high quality science on limited resources. The 'Europe' that he portrays in the story does not have these conditions – or perhaps they do science under quite different circumstances since they can afford to close their labs.

All in all, '*saving*' reveals another attitude to conducting science than that of '*investing*'. The scientists invest their knowledge and skills in new projects and develop strategies to study what they want to study, while they still sufficiently answer the funders' calls. But they also manage the resources they already have very carefully. It seems they have this feeling that there is never enough money and they therefore have the responsibility of saving where they can. This point makes even more sense if it is connected to the way that the scientists prioritise projects and work. In order to accomplish what they want, they sometimes have to do both the work the funders ask of them and the work they want to do – for the same amount of money and during the same period of time. While this may send their research in the direction they favour, it also means that both time and materials are even more limited.

This is another side of the '*entrepreneurial scientist*', as described by Etzkowitz (1998). While the 'entrepreneurial scientist' can be described as perceiving commercial as well as scientific potential in her scientific work, it is perhaps also someone who has learned to adapt to circumstances where resources are very limited. As Law points out, then, the entrepreneurial scientist is also someone who never fails to see an opportunity, even in the instances where it may be disguised as a constraint (1994: 61). '*Entrepreneurial*' in that sense does not refer as much to the commercial side of science as to the way they manage to turn the conditions for doing their work into an opportunity. In that sense, the way the scientists 'save' makes sense. They do so because funds are limited and temporary, but also because saving time and resources opens up new possibilities and perhaps even the possibility to do the science they wish. All it takes is extra work.

8.1.3 MAINTAINING

The last form of conduct that I want to touch upon in this chapter is that of '*maintaining*'. Again, the analogy goes to a home where the roof, the pipes and the kitchen equipment need maintaining; but so do the relationships between the members of the household and the relationships with entities on friendly terms with the household (stakeholders). In a similar vein, the scientists maintain their lab carefully in many ways.

I will start with the equipment. Before entering a lab for the first time, I had some highly futuristic ideas about what a lab looked like: shiny white machines and fluorescent lights shining out from under the doors. In reality, the labs look much more rattled. Some of the machines are quite old, some of them just look old; in any case, they all look used and not very shiny. Here and there, they are fixed with gaffer tape or a dishcloth is wrapped around a pipe. Some of them are probably really advanced, but this is hard to believe because they look almost homemade, with their plastic-pipes and flasks attached together by tape. The computers are not always up to date and run quite slowly.

I noticed how many of the scientists are responsible for a specific machine. Many of them are experts in using a particular machine and they each keep their respective machine running. To do this, they sometimes need to use a small trick for when the machine will not start or when it makes other mistakes. They know where it usually leaks and they remember to put a bucket under it before they turn it on. They are the scientists the others call if something goes wrong. Some of them may even build their career on their knowledge of a certain machine because its maintenance is vital for the entire lab's experiments and this reliance on their expertise grants them a position. In relation to some of the machines, the maintenance is a shared responsibility. At Curious George, they have a rotation for the cleaning and maintenance of machines. Even the espresso machine in the kitchen is part of it, though I doubt anyone can build a career on that.

As with many of the other items in the labs, the machines are ridiculously expensive and it is therefore important that they are preserved for as long as possible. At the same time, they can be quite fragile and delicate, and it takes a great amount of knowledge to know how they work. I was fascinated by how practical that work sometimes is. When a scientist has been summoned to fix a machine, she will resolutely hit the machine on the top and suddenly it will start up after it has been stubbornly silent for hours. This underlines the point about how having very specific knowledge about a certain machine is very important for its maintenance.

Another item, of which the PIs take especially great care, is that of their 'group'. A group is a collection of people who are working together on the same project and usually also on the same grant under the direction of the PI. Simon from Curious George underlines that, as a PI, '[w]*e have an organisational responsibility for our group's wellbeing.'* The maintenance of a cordial ambience among the group members seems to be of great importance. Almost all the PIs I speak to mention that as their biggest responsibility. Here, it is Clark who ascertains that maintaining a 'happy' group is a necessary skill as a senior in academia:

'…social skills I think, are really, really important. You have to be able to maintain a happy group. That's the only way. I think this is the last part of experience [you get through training]*, and I have seen a number of models, so I had the chance to learn how to do it and how not to do it* […]*.'*

He continues by underlining how maintaining a 'happy group' is about its composition, something I hear often:

'You have to make sure that there is a good balance of people […] *I think, especially in Denmark, social interaction is really, really important. Thus, you really actively foster* [a good environment] *and then it works and we don't pick people* [here] *because of their excellence but because they fit well into the team.'*

So the PIs spend a considerable amount of their time being managers of 'people' and 'groups' and they feel a responsibility to compose these 'groups' such that they 'work' in the right way. This means that they, at least at Curious George, in addition to a potential applicant's academic virtues also make the selection based on his or her ability to contribute to a cordial ambience among the group. This is not only important in order to accomplish the goals that have been set in the grants, but also, as Canute explains, because '*we try to make decent people out of our students.*' The juniors are seen as a special responsibility. It is not just for the sake of the organisation that the group needs to function well, but also for the sake of the juniors, because they are encountering the academic workplace for the first time and need to learn 'good behaviour' as well as experience that academia can be a decent place.

Most of the PIs tell me about the great responsibility they feel for the group, especially the most junior members. Simon from Curious George compares it to being a good host: not only does he need to be good at inviting the right people in –

those who will benefit from each other – but also at serving the right food and playing the right music, so all of the guests will enjoy the evening. Curiously, most of the PIs at both labs claim they are not particularly good at managing '*people*'. They consider it a personal skill but not one they possess themselves – or they believe they have too little time to actually manage people. I am not sure why. Perhaps, it is again the old myth about scientists not being good with people; or, perhaps, it is a constant headache, whereby they always feel behind as a result of time constraints. It is hard to assess whether everyone thrives in an environment, and also difficult to do something about.

Maintaining the lab is about maintaining the relations between the different people working there. The PIs say that this has not historically been a priority in academia but that it is increasingly becoming so because they all work together so much. Henry from Gyro Gearloose explained that he tries to teach the juniors in the lab about the managing of groups, as nobody ever told him during his academic upbringing. He was quite surprised to learn about the importance of maintaining a good group when he became manager. And the younger scientists seem to be aware of this fact as well and add it to skills they need to possess. It is not only about being dedicated to science or being very good at it, they should also commit to the lab and the work environment itself. For the juniors, the main thing is, perhaps, not maintaining a good group environment, though most of them are happy to contribute, but rather thinking of it as a way to get a job. Miriam speaks about the employment process she went through:

'I think that the most important skills were that I am sociable, that's how it felt anyway, because I don't have the most impressive CV, so to speak. And also that I'm laborious, perhaps, in any case, I think that my old employers told them [Curious

George] *as much, when they called them – and also that I'm ready to embrace changes and I'm good with sudden changes, and that I'm willing to travel…'*

All of the juniors are quite interested in maintaining their job, as all of them are employed in temporary positions. They are eager to enhance their chances of further employment by also being easy to work with. I do not consider this interest in maintaining a job as part of Oikos because it cannot be justified as a responsibility for the local lab; it is much more an individual interest. The PIs work to maintain a really good group; the juniors work to maintain their employability. While both perspectives are understandable, the behaviour of the juniors is not necessarily a consequence of a responsibility for the lab's wellbeing, but rather for their own job situation.

The last item will be taken up in relation to maintaining the scientists' responsibility for their external relations. By '*external relations*', I am mainly referring to people in the different foundations, but figures such as '*the public*' or '*partners*' in the private sector also come up once in a while.

During an interview, Charlie tells me about some data Gyro Gearloose needed for a paper – without it, the editors refused to publish. The problem was that the lab did not possess these data and were not able to make them. Charlie ended up contacting a guy they know somewhat from a German company. He was really in doubt as to whether it was too much to ask – nor did he know if a company would give their data to a public paper. But the guy did, and he was of course listed as co-author on the paper, Charlie underlines.

Stories about external relations are usually about some sort of exchange, such as the one about data above. Gyro Gearloose asks for data and they are able to provide authorship in return. While it is difficult for me to assess whether authorship and the data are of equal worth in the exchange, the stories about external relations are usually about exchanges. However, they are also about the limits of normal conduct in relation to actors outside the lab and about going beyond that limit. Charlie is not sure whether he can contact a man from a private company and ask for data, but he does so anyway and Gyro Gearloose gets what they need. This is often the case in stories about external relations; it is about transgressing some rules or structures in order to reach a goal that benefits the organisation:

During an interview I ask Simon about his collaborations with the perfume industry. I can't help but think that it must be a very different feeling [admittedly, I think of it as less noble] *to work on developing pheromones for perfumes compared to developing cancer treatments, despite the fact that both technologies stem from the same research project. But Simon tells me that I should try to look at things from another angle. It was the perfume industry that contacted him and gave enough funding for him to hire a PhD student and a postdoc to work on the project. So all in all, he now gets much more knowledge about the cancer treatments by working on the pheromones.*

This is a very typical way of both seeing and doing things in Oikos. The scientists see opportunities and subsequently take them; as Law concedes: '*if they* [conditions] *are not treated as an opportunity, then the agent is no longer acting responsibly; instead she is in the process of retreating from the proper performance of agency.*' (Law 1994: 116). So maintaining can also be about taking chances and doing something unexpectedly, which changes the structures and enables the scientists to work with what they want. Working with the perfume industry turns out to be a way to maintain the cancer research, and so the scientists go for it.

'Maintaining' is about looking after those parts of the lab that the scientists find most important: Machines, people, groups and external relations. It is both about learning the skills related to taking care of machines and people as well as making all of them do what is expected of them. These two things are gained through experience, irrespective of whether it is about machines or people. At times, it is also about bending norms and rules in order to reach a goal that will benefit the organisation. These maintenance activities are connected by the fact that they are continuous tasks that need to be followed up once in a while. They are never really complete, they always need to be returned to – just as I return to my dripping kitchen tap and fix it once in a while.

8.1.4 SUMMING UP

Oikos generates some patterns of behaviour that can be compared to the work we all (ideally) do to take care of our own home; namely, spending our resources with care (saving), looking after our equipment and our relations (maintaining) and investing our time and resources in order to secure a necessary income in the future (investing). Throughout this section on conduct, I have claimed that these forms of behaviour are generated by a specific responsibility that the scientists feel for fostering the laboratory they work in. I will try to argue this point based on empirical data in the next section. In conclusion, I will say that in comparison to Vocation, the scientists do not seem to enjoy the work needed to maintain the lab in the same way they seemed to enjoy the checking, repeating and being critical. It seems that they all believe that these forms of conduct are necessary but mostly as prerequisites for getting back to what they really want to do; namely, experiments and writing papers. While these actions can also be seen as necessary in order to maintain the lab (without citations, no grants – such is the logic of the credibility circle (Latour and Woolgar 1979: 201), the scientists seem to enjoy them much more. They consider them as inherent parts of their job, while, as I will show, the conducts here –

especially that of investing – are considered mere '*politics*', which they want to escape.

8.2 JUSTIFICATIONS

In the previous chapter, Vocation, it turned out that it is actually difficult to pin down what it is exactly the scientists feel responsible for. My interpretation is that it is 'the truth', but that most of them seem to consider that notion a little too grand to articulate. Furthermore, the responsibility for the truth is so inherent to their idea about their professional identity as 'scientists' that its nature or importance rarely comes up as an issue. It turns out that some of the same difficulties are present in relation to Oikos. Over time, I get the impression that the scientists feel they share a responsibility with their colleagues for their workplace. That includes the physical surroundings, their colleagues and the shared fate of the lab. But it is difficult to point to because, rather than talk about it, they conduct themselves in ways that suggest it: they apply for grants, they help their juniors, they fix the old machines, they clean the fridges, and engage in any other kind of activity that shows they care for the place. It seems too self-evident to address.

Another thing is that it is difficult to pin down what exactly they feel responsible for. Surely, it has something to do with their colleagues, it has something to do with the future and it has something to do with the physical place they work at every day. But all of these terms are slippery. At times, their 'colleagues' are the people in their research group – perhaps two or six other people. Sometimes it refers to everyone at the universities where the two laboratories are situated. At other times, their 'colleagues' are the people in the same department, the same section or the same centre. It is difficult to say for sure. The same ambiguity appears when they talk about the future of their workplace. The future for whom? Sometimes it is the next generation of researchers, sometimes it is the university, sometimes it is the future of their group and its research, and sometimes it is the entire scientific community. And, of course, defining the boundaries of their workplace is also difficult. The lab is at once part of the university, the physical wet lab, or part of a department (whose name changed twice while I was at Curious George due to structural reforms at the university). It means that I can never be entirely sure what they are referring to when they do, for once, voice a responsibility for colleagues, time and place.

Therefore, it is evident that the assembled conducts and their shared justifications have been defined by me. 'The business' is thus to be considered as an analytical construct made from the very many, more or less, explicit entities that the scientists exhibit concern for. For instance, '*the group*', '*students*', '*other employees*', '*the machinery*', '*my supervisor*', '*my group leader*', '*my colleagues*', '*this place*', and so on. So when I, based on the remaining paragraphs in this section, claim that the scientists feel a responsibility for '*the business*', it does not mean that they necessarily share a clear picture of what that '*business*' is. Their imagination of '*the business*' is fragmented and the scientists' conception of it depends on the situation at hand. The conundrum of being certain that there is a common denominator but being unsure about what it is exactly clearly illustrates that the mode is an analytical construct. The scientists probably have a much more fine-tuned idea about this responsibility than I do, and, perhaps, they are also able to distinguish between the different responsibilities that I have assembled here as one. But despite these reservations, I will still conceive of it as a coherent phenomenon that can describe what they feel responsible for.

8.2.1 THE BUSINESS

First, I turn to Canute and his thoughts about research management:

During an interview, Canute reflects on what it takes to be a good manager. He says that one of the crucial things for him is to refrain from being the last - and thereby main - author on the papers of the young promising investigators he has recruited and that he never co-authors manuscripts to which he has not made a significant contribution in order to secure himself a high h-index or citation number.[32] *He argues that good Post doctoral fellows and PhD students will never return to his lab after they have graduated if he does such things.*

Plentiful reasons can be given for refraining from becoming the main author. Canute could also argue (he does so on other occasions) that letting his students be the main author will help move their career forward or that he has actually not done enough work to deserve main authorship. But the reason he gives above is neither about him nor his students directly. It is about the future of the lab. His argument is that if he is not a fair and, perhaps, nice supervisor, this will reflect back on the workplace and good people will not return to work there. Canute is, in other words, investing in future employees by treating them well at the beginning of their career. For Canute, the missing authorship is an investment in the future lab. If he focuses too much on his own career, that is, the '*h-index*' and '*citation numbers*', the lab will potentially suffer the consequences later on. And in this situation, he puts the lab's future above his own career.

Simon from Curious George has just signed a patent agreement together with one of the juniors on three new inventions. He sits at his desk humming to himself as I enter

32 The citation number is an expression for how many times a particular paper has been cited by other authors. The h-index is an index that attempts to measure both the productivity and impact of the published work of a scientist or scholar. The index is based on the set of the scientist's most cited papers and the number of citations that they have received in other publications

his office. He tells me that the deal is done and now he 'just sits and waits for all the gifts that will pour in from the others as a thank you for the good work.'

At a glance, this story is primarily about a single scientist. He is happy with a job well done and awaits the recognition. But, as many of the scientists have assured me during the fieldwork, patents are good news for the entire lab in general. While the money may never benefit others than the individual scientist, the lab gets recognition from patents. '*Think about MIT,*' as one of them explains to me. '*Everybody knows that they are the best. Otherwise, how can they obtain that many patents*'? So if scientists patent their inventions, it signals that they do valuable work and that reflects back on the whole environment. The logic seems to be: The more patents the lab has, the better it must be. In this light, it makes sense that Simon awaits champagne, chocolate and flowers from his colleagues. Besides helping his own career, his work contributes to creating a good reputation for the entire lab – perhaps even for the university.

Responsibility for the business surfaces here and there in the organisation. There are, for instance, plenty of posters in the wet labs reminding people to clean up after themselves so others can use the equipment, or reminding them not to use more materials or chemicals than necessary so there will be enough for everyone. Of the posters, my favourite – from Curious George – states: '*Close a fume cupboard, afford a PhD dude'*[33]. While the poster is part of a big campaign for a '*green university*', the justification for the plea does not refer to concerns for CO_2 emissions. Rather, the reasoning goes that careful use of the fume cupboards will make it possible to hire more people. It is, of course, easy to argue that it is very uncertain whether money saved from the consistent closing of fume cupboards will go directly to funding new

[33] A slogan that is more catchy in Danish, where it rhymes: 'Luk et stinkskab/få råd til en PhD-flab'.

PhD staff. It would at best be difficult to ascertain connections between specific energy saving practices and a rise in staff recruitment. But that is not the point. The poster refers to something that everybody knows and understands: Money is a scarce resource and the contribution each individual makes will benefit the entire organisation.

In all of the stories, the arguments refer to a shared responsibility for the place the scientists work. They do not, as in Vocation, refer to the entire scientific community, but rather to a 'we' that is closer to home, so to speak. The stories all describe a responsibility for the immediate surroundings: the colleagues, the young students that are educated in the lab, the physical surroundings they work in. However, I still need to argue why I have chosen to call this 'we' 'the business'. I will attempt to move forward with that argument now. In order to do so, I return to Simon's comparisons of research groups and good parties, segments of which I described further above:

'[…] *you've got to hope that some of the people you have hired have the ability to make things work. I mean, for instance, if I'm throwing a party, then I also have to rely on that they* [the guests] *will make a good melting pot that works and that the party turns into a cosy place, because that's one of the things you can actually say: if the social frame is right and the chemistry between the group members is good, well, then there is also a bigger chance that your impact will be much greater.*'

What I find interesting in this quote, besides the nice party analogy, is Simon's focus on the relationship between the people's synergy, '*the chemistry between the group members*', and the result, '*the impact*', they are going to create. While I believe that Simon finds the wellbeing of the group important, what really matters here is '*impact*': what they create together. Something more than the group itself is at play.

They need to move forward, to obtain results together. The group and what the group is hopefully creating, namely, great impact, are interdependent. Without the group, this would be difficult, and without the impact the group is of less importance. So while there is a responsibility to take care of the people within the lab, this concern always goes hand in hand with the goal to perform. The scientists are not assembled to have a great party; they are assembled to accomplish great things.

At Gyro Gearloose, Henry, the PI and lab director, calls all of his employees for a meeting in the big auditorium. Henry wants new ideas for research projects. And he wants all of the attendees to contribute with ideas. He asks openly if they have any ideas for 'a new big research project'. He thinks that the lab has developed cutting edge technologies on the grants they already have but that they now need to move further ahead in two ways; both by applying at the biggest and most prestigious agency, namely NIH[34]*, and by trying to combine the technologies they have invented and thereby create some new extraordinary ones. None of the scientists answer the call for 'a big idea' right away, but there are lots of questions and people seem more attentive than usual.*

The meeting is held during a time of insecurity at Gyro Gearloose. A number of people (lab technicians) have been laid off, and the annual '*state of the lab*' speech that Henry gives every spring has been postponed this year. Rumour has it that Henry does not want to give it until they are sure about a big research application that is being assessed at the moment. He wants to present some good news. In the mean time, they still need to move on and come up with ideas for '*a new big research project*', which they can invest their time in. The story is interesting because Henry addresses the lab as that which needs to move forward. While this demands that the

[34]The National Institutes of Health (NIH), is a part of the USA. Department of Health and Human Services and functions as the nation's medical research agency. From www.nih.gov 25.08.14

scientists also develop new science, this is not the focus. The argument goes that all the assembled scientists need to move forward, and, in order to do that, they need to develop new science. The situation can be interpreted in different ways. Seen in light of the current crisis, which has necessitated making people redundant, the story can be interpreted as something close to a pessimistic prophecy: If the scientists do not come up with good ideas that can generate an income, then the lab cannot survive and they will all loose their jobs in the long run. If it is interpreted in a more optimistic light, it can be viewed as a compliment to all the present scientists: They have accomplished so much that they are now good enough to compete for funding in the most prestigious funding agency in the nation. No matter which version, the justification for action remains the same: Together, the assembled employees and Henry are a unit that should be taken care of, and this will happen if they '*move forward*'.

In the previous section (8.2), I claim that the justification for why the scientists '*invest*', '*save*' and '*maintain*' lies in the responsibility they feel for 'the business'. Based on the stories, I can begin to argue why. The scientists seem to both recognise and put forward the idea that they are part of a common unit, whose survival and wellbeing is a reason for action in its own right. In the story about Henry and the other scientists from Gyro Gearloose, they are assembled to discuss how 'they' – the lab – are going to proceed from their current position. In Henry's eyes, they need to make some bold investments with their knowledge: They should go to the most prestigious investor, NIH, and they should invest everything they already know in a new technology that cleverly combines all the previous ones. But the ultimate reason for that, more than the wonder of such a technology, is that it will move them all '*forward*' from where they are now. In the above interview excerpt with Simon, he tells me how the better the group, the greater the '*impact*'. I do not interpret this as them creating a more exciting type of science, necessarily – perhaps that is self-evident for the scientists. But more than doing great science, this is about a group having a great '*impact*', something that will benefit all of them in the long run. This is also based on the great importance of funding, which is influenced by one's 'impact factor'[35]. Referring back to the credibility circle: A great '*impact*' can be exchanged for new grants and new positions. Besides relating to exciting results, '*impact*' refers to how much influence the scientists' findings will get; the prestigiousness of the journals they may be published in; whether it will be possible to patent their knowledge; and whether the findings will reach the attention of the media and the general public. In both Simon's party analogy and the meeting at Gyro Gearloose, I see traits that remind me of a '*business*': What matters is to create growth for the '*business*'. This demands investments, as the story from Gyro Gearloose indicates, and also a good composition of human capital, which Simon underlines in his quote. While the two labs can still produce a good product – or rather good science – the important thing is not the scientific results being true, as I show in Vocation, but rather the movement and, indeed, the survival of the 'business': The labs and the inhabitants therein. Clark from Curious George also emphasises this responsibility:

'*I think the department has the idea that for the department a publication is really good. For the department, patents are really good. For the department, it is really good if I get public exposure so stuff gets put on the back page – or even in articles – of newspapers. So publicity and of course money* [is important].'

[35] The journal Impact Factor is the average number of times articles from the journal published in the past two years have been cited in the JCR year. The Impact Factor is calculated by dividing the number of citations in the Journal Citation Reports year by the total number of articles published in the two previous years. But the scientists also use 'impact' almost as slang for how much influence (funding, media attention, citations, etc) they expect as response to work they have done

Clark talks about a local entity (the department), of which he is a member and where it is therefore considered of importance that he take a responsibility for striving after publicity and money. Oikos seems to generate the idea of science's role being one where scientists are members of a local 'business' and therefore have a responsibility to keep that business going, preferably, with a surplus for new investments. They do so by investing, saving, maintaining and by making a contribution that has an '*impact*'. In an interview with Henry from Gyro Gearloose, this responsibility to '*contribute*' is also underlined. He tells me how they differ from other labs by being focused on a common '*project*'. He even literally compares the lab with a '*business*':

'*We* [management] *say: "You may think of yourself as a biochemist, but we do not necessarily think of you as that. We think of you as a contributor to the project." And so, if we* [...], *say, need someone to work on cell biology over here on this different thing,* [then] *people have to move* [because] *we cannot afford not to. And I think this is good. It is more like* **business** *that way: You do what you have to do. And I think that's good, because people in academia tend to be... it relaxes them more, you know.* [But here] *they have to move* [...] *we have had people who did not necessarily like that* [...], *as one of them said: "I want to know that I can come in and do the same thing everyday, you know. I want to know that I'm secure* [...]*". And I don't blame them, but that's not what we do here.*'

So Henry actually compares Gyro Gearloose to '*academia*' as if they were something else, namely, more of a '*business*'. In his eyes, this means there is more dynamism at their workplace, whereby the scientists have to '*move*' and should not think of themselves in terms of their discipline but in terms of what they '*contribute*' to the overall '*project*'. This is contrasted with '*academia*', a place that '*relaxes*' people and enables them to do the '*same thing everyday*'. So Henry takes pride in running a place that looks like a business in so far as it is more dynamic and more focused on a common project. At the same time, there is also a dual focus on the economy *and* the project – or even product – both of which have to constantly be thought of together. People have to '*move*' and do something besides their own expertise, such as biochemistry, because the 'business' '*cannot afford*' them doing only biochemistry if cell biology is needed. The responsibility for the business comes before their disciplinary roots, and the economy and the scientific project are closely entangled. In this quote, the contrast to Vocation's justifications is made very explicit. In Henry's interpretation, academia's hallmark is not the pursuit of truth, but instead a traditional way of thinking and a lack of concern for a common project. Business breaks these habits by focusing the scientists' energies on the creation of a project; that is, integrating the economy and the scientific pursuits and thereby shaking the scientists out of their usual roles.

8.2.2 SUMMING UP

In the previous section, I have argued that the scientists justify the conducts '*investing*', '*saving*' and '*maintaining*' with their shared responsibility to keep '*the business*' running. By '*business*' I mean the local entity that the scientists consider themselves members of and where their contributions and their budgets are closely entangled. '*The business*' is abstract and ambiguous: The scientists do not speak about exactly the same thing when they refer to it. At times, it is the research group, at other times the department, their centre or the whole university. But despite these different references, the central point is that it is a place they consider themselves members of and where its survival – and hopefully success – is considered a justification for action. Responsibility for '*the business*' makes the scientists look at their findings with a different point of view compared to that of 'Vocation'. It is not only the truthfulness of a statement that matters; now its ability to create 'impact' and be used for further investments is considered vital. Therefore, other entities that do not seem significant in Vocation suddenly become so; namely, patents, publicity and human capital.

In the following sections, I will show some of the overall effects of this responsibility on the everyday work life of the scientists. As in the previous chapter, I will also demonstrate that Oikos has limits. Like Vocation, it is negotiated and rejected continuously. Finally, I will also discuss this mode in relation to both notions of the 'entrepreneurial scientist' and recent public science reforms.

8.3 ORGANISATIONAL CONSEQUENCES

From the perspective of Oikos, Curious George and Gyro Gearloose look very different than they do in Vocation. A very idealised description of work life in the labs seen from the perspective of Vocation would go like this: The two labs are inhabited by scientists who take their time to check up on their plants and cells, constantly question their previous actions and next steps, and do not really pay more attention to their closest colleagues than they do to colleagues across the globe. The juniors are in training among older professors and slowly learn the art of taking care of the truth, a skill they struggle with immensely and which makes them forget their family and laundry. Over lunch, the scientists discuss current catastrophes that threaten mankind and make subtle complaints about the fact that neither politicians nor the media would listen to their advice.

The laboratories look radically different in the mode of Oikos, as I will examine in this chapter. Scepticism is replaced with a firm belief in the lab's abilities to create usable results, and the training of young scientists-to-be is replaced with a division of labour where the juniors conduct experiments and write papers while the seniors write grant applications and do 'politics'. The slow and meticulous work as seen in Vocation is replaced by a constant awareness of time as a finite resource. The uncertainty related to how the truth is taken care of is replaced by a worry over the lab's future and the personal job prospects.

In this chapter, I will examine the consequences of Oikos for how work is organised in the two labs. First, I will show how Oikos relates to the hierarchy in the organisations; how the responsibility for the business is congruent with the formal hierarchy – but also how this is, at times, transcended. Second, I will turn to the question of how the scientists perceive the relation between time and work and how time becomes a crucial issue. Third, I will describe how the scepticism in Vocation is put aside for what I consider a strong belief in the labs' abilities. Last, I will study how the organisation is very much in the foreground, while the profession – a significant factor in Vocation – fades into the background. Finally, I turn to the limits of Oikos and the differences between the two labs.

8.3.1 OIKOS AND HIERARCHY

An interesting aspect of Oikos is how different tasks for maintaining the business are distributed across scientific ranks. The simple rule of thumb is that the more senior one is, the more responsibility one has for the business. Simon from Curious George describes elements of the different positions:

'Well, if you as a PhD student have to be educated to become a scientist, then you have to concentrate on that small corner of your desk and nothing else for three years, while as a postdoc, you also have to take responsibility for managing the PhD students. As an assistant and associate professor, you all of a sudden have to implement new elements in the education [Bachelor and Master level], *and it is*

especially here that it starts to get "political", when you talk about sharing teaching across departments and research groups and what have you...'

Simon often explains to me how the work becomes more 'political' as the scientists move up the ranks. In Simon's interpretation, '*political*' is basically all the aspects of the job that are not directly related to experiments, writing papers or the actual teaching of students - whereas the management of teaching, for instance, is a '*political*' subject.

Most of the scientists seem to share this idea that their responsibility for the business is limited to them doing the work demanded by their funders. The PhD students and postdocs, for instance, usually describe their tasks in terms of the questions they need to answer within a limited period of time. As Annie from Gyro Gearloose puts it:

'My responsibility is to fulfil the requirements of this grant. My job is done when I have fulfilled those.'

They do not feel that it is their responsibility to take part in investing time in grand grant applications, nor are they asked to do so. This seems to be an explicit and conscious decision in the labs. In order for them to concentrate on their research, the PhD students should be free of obligations or tasks that are not relevant. Simon remembers his own period as a junior:

'I always had the feeling that a lot of things were going on that I could then read about in the paper the next day. Things I was not necessarily a part of... [...]. *I remember discussing it with Canute; that it was really nice to be exempted from those things. Because there is a lot of politics and a great need to take a stance in relation to things that are not necessarily related to your present science. If our research is to*

get somewhere, well, then those who are actually doing the scientific work need to have time available to actually do it.'

In Simon's account, responsibility for the business also includes the obligation to exclude the juniors from the world of 'politics'. This is not only done so they have time to be '*educated to become a scientist*' – a logic I have already touched upon in the previous chapter – but also a way of separating several tasks, all of which need to be done. The job of doing 'politics' is so large that it takes up a lot of time and energy; therefore, the seniors also have a responsibility to create a politics-free vacuum, where it is actually possible to do science. There has to be '*time available to actually do it*', the science, that is. Otherwise, they all agree that the business will never '*get somewhere*'. Curious George and Gyro Gearloose share the idea that the juniors have a special responsibility to do the science, not only for themselves, but also for the sake of the business, whereas the more 'senior' they become, the more responsibility they get for negotiating 'politics'. At times, however, what it is the scientists actually mean by 'politics' can be a little blurry. Sometimes, it seems that it is just all the work they do not want to do. At other times, it seems as if 'politics' is what is happening when other parties such as university management do not agree with the lab's decisions. 'Politics' can also be all the footwork that is needed in order to obtain a grant. And finally 'politics' is about interacting with the media regarding stories that relate to the science done in the labs.

But while the scientists speak about a certain division of labour, this division is now changing due to the labs' two directors, Canute and Henry. They agree that the juniors should not have too many obligations besides doing science, but they also consider it important that they get a clearer idea of '*how the system works*', as Henry puts it. While Simon cheerfully recalled above that he was omitted from being told a lot of information before he found out about it in the newspapers, the two directors believe it should be different for the new generations. Henry tells me that it is an explicit goal for him to teach his students about the system that they work in:

'*No one told me about these things* [university politics] *when I was a graduate student. But I think it's important that you know all of these things and you learn how to navigate in this system. That's also why I have let my future students follow the mail correspondence between me and the management about their education. Because it concerns them. And because they need to learn how to deal with issues like that.*'

'*These things*' refer to, a controversy about a new graduate programme that Henry has initiated but which university management is not in favour of and therefore wants to close down, even though several students from across the world have already been accepted. Henry does not think that the prospective students should intervene. That is his job, as he is the manager; however, the students should learn that university is not free of politics and it is important to be able to navigate this system. In a similar vein, Canute also wants his juniors to be aware of the many aspects of working in academia besides doing research. Curious George has been working with GMO for many years and they experienced the 'GMO crisis' in the late nineties, when suddenly all the funding for anything to do with gene modification rapidly dried up (see theory section 2.1.1). He wants his students to know that these things may occur and that they can severely affect their prospects for work. He says they should learn how to '*navigate*', a term they all use a lot. He discusses the GMO crisis and similar issues with his students at meetings and supervision sessions, and it is his explicit goal that the juniors develop an attitude and a stance in relation to these issues. Not necessarily the same stance as his own, but *a* stance.

However, perhaps the juniors are learning to navigate in the academic system without the help of the two directors. In any case, the juniors need to adapt to a work environment where the tenured positions are scarce, and where they need to rapidly learn how to secure funding for their own jobs. As Linda, one of the seniors from Curious George, observes during an interview:

'[...] *we don't have many tenured positions, I mean, in this section we only have three permanent scientific positions and you know yourself how many we are, so especially for the young ones, this is a period of insecurity, because they need to look two years ahead and perform all the time.*'

The juniors need to '*look ahead*' and '*perform*' if they want a chance to continue in the scientific system – either by acquiring funding independently or by being chosen as a member of a large research grant. So in a way, the juniors also need to do 'politics', even though the seniors may think the juniors' involvement is limited compared to their own. But most of them really want to move to a more secure position – or at least a position where they are not dependent on temporary funding from their colleagues. A permanent position may be a pipe dream, but most of them tell me that they would like to manage their own grant. As Simon says:

'*If you are capable of maintaining your own salary, then it is of course really nice* [to work here]. *But if you have to begin to rely on others' applications, then it starts to become a problem...*'

Simon considers it '*a problem*' if he has to rely on others' money. If he does that, then he also has to give up the benefits he has come to enjoy as PI, such as the freedom to formulate his own research questions (as long as he can fund them) and the right to supervise PhD students. As there are very few permanent positions in both of the labs, most of the scientists are either funded as part of a bigger grant or have received grants and have the position of PI. This means that they have the same obligations as the tenured scientists in terms of project management, PhD supervising and teaching, but they do not have the benefits that a tenured position gives in terms of job security. Therefore, they constantly have to be on the lookout for money if they want to ensure they keep their office and lab space.

After about three weeks at Gyro Gearloose, I start to notice a rather tense atmosphere in the lab. People whisper about the 'budget' and there are rumours about redundancies and missed grant opportunities. One of the scientists confirms that something is indeed amiss:

'*...a number of people in the whole building* [not just Gyro Gearloose, but the whole centre] *has been let go over the last week because it is just that time of the year with the budget.*'

Apparently, there is not enough money for them on next year's budget. Everyone seems worried that they are next. Sandra, who is new in the lab comments:

'*I think as I look back at the almost three months I have been here, I have got a lot done, so that really feels good. Because, you know, there has been a bit of disruption in the lab recently with... we had one of our techs let go...*'

So not only is she glad because she is progressing with the experiments. She is also glad that she has '*got a lot done*' because that puts her in a less vulnerable position if more redundancies are necessary. She also tells me how she has reoriented her career moves within the last few years, as science is a tough 'business' to be in:

'[...] *I'm trained for the daily stuff and then the end goal... I always kinda picked topics that interested me and now I like... I'm trying to get more towards what is going to help keep me employed. And it's more infectious diseases and cancer and things that there's funding for – or should be funding for.*'

She tried to apply for her own funding for some years before she got to Gyro Gearloose but never succeeded. Earlier, she was more interested in '*basic science*', but the current conditions for funding has turned her attention towards '*infectious diseases and cancer*'. While Sandra is one of the senior scientists, she is not employed on a permanent basis. She therefore always ensures that her work is done and that she accomplishes a lot so management will '*keep her involved*' in her own as well as other projects in the lab. Sandra has therefore learned to 'navigate' the academic system under the current conditions. While she never complains or blames anyone, she does seem to be in a difficult position, that is, dependent on others' contemporary grants. It is understandable that many of the scientists ascertain that they would like to become PIs, as there is a certain degree of freedom connected to it.

So at the bottom of the hierarchy, there are the graduate students, postdocs and non-tenured scientists who are all responsible for conducting science, while those higher up in the hierarchy do 'politics'. But while the bottom does not have a formal responsibility for investing in new grants and negotiating university politics, they still need to make a substantial contribution in order to be noticed by management and perhaps also apply for their own funding.

At the bottom of the hierarchy, there are many scientists employed in temporary positions, and these positions are becoming more widespread in the system as permanent positions disappear. Therefore, taking care of the business is increasingly about making sure that there is a steady inflow of funding to sustain the single scientist's career and keep the business staffed. While those scientists who have been in the system for some time seem quite confident that they will be able to continue to fund themselves, the younger ones are much more anxious about their prospects and do not necessarily consider a career in academia as a wise choice in terms of job security and stability. Even among the more experienced members of the business, the constant focus on funding is an annoyance. Simon comments:

'What makes the difference between being employed in a permanent position than in the position that I have now is really based on whether the organisation gives you room to make mistakes once in a while. And it doesn't do so in relation to my current position. I mean, I can't have three years where I don't get money in the bank, so to speak, because then I'm out of this system – and you can do that if you're in a permanent position. Not that I'm saying it's popular, but if you got a track record and they [management] *know that once in a while some really great ideas are coming from you, then they will let you do it. I mean, I'm sure that Holger Beck*[36] *has had some periods in his life where he did not roll around in money, and it's seldom that there are scientists who have constantly had a lot of money, but the big elephants in biological research have perhaps always had it, because they have been great at following the political trends* [...].*'*

Simon has been quite adept in the art of getting funding for both himself and several juniors. Despite this, my interpretation is that he asks for a little more space to '*make mistakes*', which gives him room to manoeuvre and time to think more about the

[36] Holger Beck is a renowned Danish theoretical physicist, who has recently retired from his position as professor at Copenhagen University.

research without constantly having to worry. He cannot work at the lab for three years without providing funding via new grants, because otherwise he will be forced '*out of the system*'. This worry and need for room to actually conduct research is one I recognise in both Gyro Gearloose and Curious George. It pervades the mode of Oikos, from the juniors to the seniors, and they spend much of their workday both working on maintaining the business and worrying about the business's future.

These descriptions remind me once again of '*agency lost*' as described by Law (1994: 61). The scientists believe themselves to be victims of powerful structures, where they need to 'navigate' carefully in order to both maintain the business and keep their own jobs – two concerns that are closely entangled. The demand for external funding keeps the scientists on their toes and leaves no room for mistakes; if they are unable to obtain funding for the business, then they will soon be out of a job themselves.

But as Law also describes, there are some 'cowboys' (Law 1994: 62); that is, a few individuals who are seemingly capable of favourably utilising these conditions and regaining at least some agency by managing their opportunities in creative ways. Given that Law is quite sceptical about the notion of 'individual', he considers 'cowboys' to be results of orders, namely, that of '*enterprise*' and '*charisma*' (the latter is inspired by Weber's ideal types of managers). As such, these 'cowboys' are not the individual conceived of as an essential personality, but rather are a lucky set of circumstances that transforms certain individuals into a 'cowboy' in certain situations, while in other situations they may still be victims. The most illustrative example from my material of such a 'cowboy' is Canute from Curious George. Two aspects turn him into a cowboy in my view: First, that the other scientists perceive of him being in this position and continuously re-articulate him as such, and second that he has taken it upon himself to be the primary force in moving the business of

Curious George forward – and thereby also taking some of that burden from the others.

The first aspect is one that I mainly encounter among all the other employees at Curious George. Several scientists call Canute their 'mentor' and mention him as one of their main reasons for working at Curious George. He is also described as being 'extremely good at politics'. Furthermore, people tell me that they would not be in their current position if it were not for Canute. For example, they state:

'I only got this job because of professor Canute, he found the money somewhere'; or, *'The lab would not be this vibrant without Canute around. He makes things happen';* or, *'Professor Canute is pretty special and very good with strategy and politics.'*

The juniors are in awe of him and describe him in almost mythical terms sometimes. They speculate about who has the potential to become 'the next Canute'; that is, someone who can make a lab into a vibrant and prosperous place. They are a little pessimistic about the prospects, as they cannot see how anyone can replace Canute. At a PhD graduation party, one of the parents of the newly graduated students even sang a tribute to Canute and everyone joined in on the refrain. So the myth surrounding Canute is sustained through a continuous re-articulation of his merits and talents (at other times, it is his more quirky side and brightly coloured socks that garner recognition from the students).

In my interpretation, this mythologising stems from Canute having taken an extra, and rather unique, responsibility to look after the business. He has been extremely skilled at landing grants, for instance, the biggest national grant. He is adept at foreseeing new tendencies and has recently been the driving force behind the lab's involvement in 'Responsible Science'. He is also convinced that science communication is becoming increasingly important and has hired new staff, including a 'Head of Communication', to integrate this into the scientists' daily work.

The remarkable ability to both 'do politics' and sustain the lab is of great significance to the scientists. In my interpretation, the scientists have so much confidence in Canute that their daily worries about the future of the business evaporate somewhat. By taking this responsibility upon himself, Canute gives the others, not least the juniors, the chance to feel unburdened and enables them to concentrate on their research tasks. Even though I think that this responsibility is actually borne by a lot of people in reality, Canute's exceptional skill does seem to assure the others that the lab and their jobs will be there in the morning.

In that way, Canute is made into 'a cowboy' who somehow transcends the structure that the rest of the scientists at times feel disempowered by. His often-successful efforts to supply the lab with money are not only seen as a result of his official rank (lab director), but also as a result of his special personality. He possesses charisma and he never seems to fail in his responsibility to treat new demands as opportunities.

All in all, the responsibility to take care of the business is shared across different positions. The juniors are, on the one hand, mostly expected to do research while the rest are busy doing 'politics'; somebody needs to do the research because this is a prerequisite for sustaining the business. On the other hand, the juniors also need to partake in 'politics' if they want to secure a career for themselves in academia; they need to find money for their next project or need to work hard to be considered as candidates for a job in a big grant. Most of the scientists are employed in provisional positions secured by external funds. This condition makes all of them invest much of their workday in their future projects. It also makes them anxious about the future, as there is no room for error. Without money, they will soon disappear from academia.

A few scientists seem to have a special talent for 'politics' and they take it upon themselves to take care of the business. This also grants them a special status among their employees, because they unburden the bench scientists of some of the responsibility. So it seems that the responsibility for taking care of the business is in line with the hierarchy; however, most of the positions also include the daunting responsibility of securing their own jobs and that of the others.

8.3.2 FAST SCIENCE

Another consequence of Oikos is the shared perception of time. In Vocation, I describe time as being considered a precious but limitless resource and that spending time on experiments is considered a sign of quality. The responsibility for the business turns this view of time upside down. Suddenly, time is a scarce resource because they have temporary grants and have to compete with other businesses:

I'm entering Angela's office. She sits by her desk and writes eagerly. She tells me there is a rumour that another lab has results similar to theirs in the pipeline, and they now need to publish fast so her research group will be credited for the finding.

This story underlines how the responsibility, and with it the meaning of time, has shifted from Vocation to Oikos. Vocation emphasises the fellow scientific community and the scientists are considered one big group. But Oikos emphasises the responsibility for the business, of which the scientists are members, and the fact that they are in competition with other labs. They therefore need to publish before the competition and their time is thereby suddenly limited. But the time limit is not just an issue in relation to the competition with other labs. It is also a question of micro-managing throughout the workday, as the scientists need to divide their time between different projects:

Alan has just finished his Bachelor degree. He had an idea for the treatment of acne based on the methods they use at Gyro Gearloose. The director found the idea intriguing and he hired Alan as a part-time lab technician, and part-time researcher on the acne treatment project. As a lab technician, he has to help the other scientists prepare and run experiments. As a researcher, he has to move his own project along and hopefully finish it before he starts grad school at another university after the summer holidays. He finds it difficult to balance these two tasks and is behind with his own studies. I ask him whether he thinks that the other scientists and the lab director are interested in him actually finishing the acne project and he could therefore ask to be unburdened of some of his other tasks? 'Well, yes…' he answers hesitantly, 'but perhaps they [the other scientists and the lab director] *are more interested in getting the cancer vaccine out, where I'm helping out. So if that gets out very fast and I get my work done, but not as fast as we all had expected, then I believe that they will think it's okay.'*

Alan is at times frustrated at having to balance his own scientific project and his chores for the other scientists. But given that he is sure the lab will prioritise getting their work on the cancer vaccine published over his project on acne, he cannot do much about it. Therefore, all he can do is work hard and carefully move between the different tasks in order not to waste time, because the clock is ticking and the end of summer – and thereby the deadline for his project – are fast approaching. The feeling of having a lot of work and little time is shared among the scientists and across the labs.

It is, for instance, addressed in a joke between two PhD students, whom I overhear at Curious George. One of them has caught a cold and is afraid that he will become too ill to attend work in the next couple of days. '*Don't fall ill, you don't have time for it,*' the other one jokingly replies to his worries. Jokes about '*slaving life away*' in the lab and it not being possible to fall ill if one wants to finish on time are common among the students. But it also reflects the concern they have about the amount of time available vis-à-vis the work they are expected to do. They mostly consider this discrepancy between time and the amount of work as a personal problem and not something that has anything to do with the general expectations related to being a scientist. Most of them just consider themselves to be too unstructured in their work to get through the PhD without it being a struggle against time. They seldom connect it to the official amount of time allocated to PhD work or to expectations from management. This idea of time management as a personal problem can, for instance, be seen in a story about Miriam, where she complains that she is bad at assembling materials and organising experiments in an efficient manner. She considers herself 'slow' compared to her colleagues:

'You know, I'm always running around the hallways looking for stuff and people. Good scientists, they are organised, they can think ahead, and get all the things they need in one round. I spend a lot of time in here – a lot more time than the other PhD's, but I don't think that I accomplish more. I'm just not that structured. You should not let yourself be fooled into thinking that those people who are staying in here all the time are the most dedicated or the people who get the most work done. It's more that they are not as structured as the others.'

She blames herself for not having enough time and even considers that to be a sign that she is not a '*good scientist*'. She describes good scientists as '*organised*' and having the ability to '*think ahead and get all the things they need in one round*'. These abilities are somewhat different from the ones that are stressed in Vocation, where being critical and very meticulous are in the foreground. Vocation generates scientists who spend a lot of time on their work, because this aspect indicates that they take their work seriously and are committed to the truth. But Oikos is different.

Here, it is about not having enough time, and therefore time-management becomes an important issue. Being busy is conceived of in another way.

At a PhD graduation party at Curious George, there is a speech about the young graduate. One of the stories is about how he had been sitting in the lab late one Saturday evening. One of the Chinese scientists had come by and had acknowledged the young graduate by saying that he now finally worked 'the Chinese way'.

As I describe in Vocation, the scientists work on weekends a lot. But seen from the lens of Oikos, this work takes on another meaning. In Denmark, many consider China to be Europe's (and especially Denmark's) biggest competitor, as it is perceived of as being full of hardworking, serious people, in contrast to the not so hardworking Danes. Therefore, there is the expectation that Chinese science and innovation will outmanoeuvre the Danes in a few years. I therefore interpret the speech as implying that the young candidate has finally learned to live up to his responsibility of working hard and not wasting time.

The understanding of time in this mode is very much tied to the conduct of 'saving' (see also stories about time as a finite resource here: 8.1.2): time is a scarce resource and should therefore be spent carefully. Spending time in the lab on a Saturday night is, in this mode, not considered as an instance of being meticulous and taking time to work carefully with the data. It is more seen as recognition of the fact that the scientists would be wasting the time of the entire laboratory if they did not work over the weekend to finish their work as fast as possible. The two ways of understanding time generated by Vocation and Oikos co-exist, but they are not reconcilable. Depending on the mode, two different contexts in which to do science are apparent. From the perspective of Vocation, the scientists are responsible for the truth and should thus spend time on getting the most precise results. From the perspective of

Oikos, these professional duties fade into the background. It is more important that the scientists work fast and get the work done, so they can move on to something else for the sake of the business. So the idea of time shifts between the modes.

This focus on time as a scarce resource also sheds light on an analysis that I present in Vocation (7.1.3). In that analysis, the junior scientists worry that their work is not living up to the standards related to the truth. If one considers that analysis again, but with time as a scarce resource, one can see an extra layer of confusion appearing for the junior scientists. Not only do the scientists have to produce something truthful, which they all appreciate takes an enormous amount of time, but they also have to simultaneously treat time as a scarce resource because all the grants and projects are temporary. In that way, the scientists have to weigh concerns for the standards of scientific rigour against time as a scarce resource; the evaluation criteria are thus doubled.

In the chapter on Vocation, I name their understanding of time 'Slow Science' because it reminds me of the Slow Food movement, which equates the slowness with which one prepares a meal with the quality of that meal. Following that logic, I call the understanding of time generated by Oikos 'Fast Science' in reference to fast food. I do realise that the connotations to fast food are not as positive as those to Slow Food. But the fast food scene has improved much recently and they offer healthy and delicious alternatives, so neither fast food nor fast science should necessarily be understood to be of bad quality. But that aside, perhaps the understanding of time in this mode does not deserve as glorious a name. While the scientists do believe they need to take care of the business, they are not necessarily that fond of the focus on time management that Oikos generates. Many of them seem to find it quite frustrating and tiresome to have so little time on their hands and, as Simon mentioned earlier, no '*room for mistakes*'.

Interestingly, Merton has described the time pressure on scientists in the paper 'Priorities in Scientific Discovery: A Chapter in the Sociology of Science' (Merton 1957). Merton describes how controversies about 'who was first' with regard to grand new discoveries have always been an inherent part of science (dating back to Galileo). He attributes these controversies to the '*institutional norms of science*'. These norms remind the single scientist that his role is only fulfilled if he advances mankind's knowledge. To do so, according to Merton, is also the way to garner professional recognition and esteem in the scientific community (Merton 1957: 639) . In some ways, the analysis by Merton also makes sense in relation to 'Fast Science' as described here. But whereas Merton describes the relation between the institution of science and the individual scientist as the locus of analysis (the norms assert pressure on the scientist), Oikos sheds light on how the competition between different *labs* creates the need for priorities in scientific discovery and renders the demand for being 'first' important. Establishing findings and publishing papers are important aspects in the attempt to acquire funding. It thereby becomes extremely relevant as a way to take responsibility for the business. Being first and being fast is, in this interpretation, not only about personal recognition for each scientist but also of vital importance for the business's survival.

8.3.3 STRONG BELIEFS

Another organisational consequence generated by Oikos is that the scientists have to have a strong faith in their assumptions. This is in stark contrast to the critical attitude that was a hallmark of Vocation. However, here the scientists are much more certain that their hypotheses are correct – even before the experiments are done.

In an interview, Annie explains how Gyro Gearloose is moving toward accomplishing the promises they have been giving to the funding agency. She tells me that the grant *was based on three subprojects. Now that the first project has worked out, chances are that the same agency will fund them again. According to Annie, the lab made some bold claims in the proposal. They promised that they could create a diagnostic device that, with just a single drop of blood, can detect and distinguish a range of different infectious diseases. No one has been able to do that before. But at Gyro Gearloose, they have continued to strive toward this goal and believed they would pull it off, and now they have done it. Annie tells me that the funding agency has been a bit sceptical about the science, but now that the scientists have actually developed the method, Annie is quite certain that they can get funding from them again.*

One of the most striking consequences of Oikos is how it diminishes the scientists' doubt about the truthfulness of their own predictions and promises. Vocation generated a doubtfulness that always accompanied the work. But Oikos turns things around. The scientists' scepticism is replaced with a firm belief in their own abilities and assumptions.

I have been invited to join a party that Henry is hosting at his house for the students who are applying for the graduate programme that he and the lab have initiated. As the evening draws to a close, many of the students are assembled in the living room, where Henry tells them about how his career began. He describes how 'no one believed in his ideas' in the beginning. He had applied for a position but was rejected in favour of another scientist. Eventually, she – the other scientist – said no and he was accepted instead, but he had great difficulties in obtaining funding and the lab was filled with machines the other scientist had left behind and which he could not use. In order to obtain money for his own project, he sold all the lab equipment. Eventually, he got a small grant due to a single member on a funding committee. She assured him that she found his ideas 'crazy' but that she was willing to give it a shot.

Throughout the story, Henry emphasises that few people believed in his ideas and he had to work hard to convince people, but he finally did – at least about his initial findings. The scientists at Gyro Gearloose are still met with scepticism from other labs. But Henry is used to opposition, as the story illustrates, and he is also used to believing in his own ideas when others do not. When this is the case, the lab just works harder to prove their results in even more ways.

At Curious George, they also have some beliefs that they withhold despite opposition. The scientists at Curious George are, for instance, quite convinced of the merits of GM crops as a way to solve hunger problems despite great public – and at times scientific – opposition. At the closing seminar I give at Curious George, the scientists and I discuss the difficulties associated with working in a controversial field. Josh, one of the PhD students, raises his hand and comments:

'I don't think it's difficult at all. I mean, I considered this issue before I even started working here. And I've made up my mind. I think it's a good technology. So I really don't think about it anymore.'

Again, this is different from the critical attitude that pervaded Vocation. In that mode, the scientists are perhaps still in favour of GMO, but it is based on their work with the plants, the many papers they have read on the subject, and the fact that they are continuously keeping themselves up to date with the latest developments. Here, in Oikos, Josh states that he has simply stopped considering the subject – even before he started as a PhD student. He believes in what the lab does, period.

Where the critical attitude from Vocation compels the scientists to go back into the laboratory, the strong beliefs generated by Oikos also have a productive effect. The act of believing means that the scientists can do certain things that would have otherwise been difficult: They can apply for funding, make bold claims, publish – or at least try to – and they can patent their findings. When the scientists believe in something, they make decisions based on that fact. For instance, GMO is a good technology, hence I can work here; immunosignaturing will revolutionise diagnosing, hence I should apply for this grant; or this diagnostic device works, hence we should write a paper about it. When the scientists doubt something, they make decisions based on this uncertainty: We have not proved that the diagnostic device works, hence we need to retreat to the laboratory; or I am not sure that I separated the treated plants from the non-treated, hence I have to start this experiment all over.

The belief that one is right about something is often seen in Housekeeping, for instance in this statement by the director: '*We have some amazing results that would make other places green with envy*'. *But it is difficult to convince others. We have to do it slowly, one paper at a time*'. The belief that what the lab finds out is actually correct, is apparent, they have '*amazing results*', but despite this belief, they have troubles making other people believe in what they are doing. The way to change that is through production of papers. So papers are also used as arguments for a statement, which the scientists at Gyro Gealoose have already settled as a fact. They are means to convince those who are not part of Gyro Gearloose that the lab is indeed moving in the right direction and their results are reliable.

Moving in the direction of establishing facts is important because it increases the labs' chances of getting funding. Therefore, convincing facts are of the utmost importance in Oikos. In section (7.4), I discussed how negotiations of Vocation reminded me of the two-headed Janus figure described in Bruno Latour's 'Science in Action' (Latour 1987): One head says '*When things are true, they hold'* and the other head says '*When things hold, they start to become true'*. In the previous chapter, I stated that the scientists are only making statements similar to that of the right head of the Janus figure: '*When things hold, they start to become true*', but now we have suddenly moved to the left head. Now things are true and thus they hold: The diagnostic device works and they can therefore also write a paper about it; the lab has amazing results, thus they just need to convince outsiders; or their ideas are ambitious, thus someone should provide the money. In Latour's account, the two heads of the Janus figure are not just stating opposites; they are also separated in time. When a scientific controversy is not yet settled, the right head talks. In the language of ANT, more actants need to be enrolled in order for the statement to become a settled truth (Latour 1987: 15). But when a controversy is settled, the left head talks. The fact becomes the reasoning behind new actions and new networks can evolve based on this fact (Latour 1987: 14). But in my study, the two heads do not seem to be separated by time; the two heads are in fact speaking at the same time, depending on the mode of responsibility. When the scientists feel responsible for the truth, they tend to be in doubt. They think they need more evidence in order to provide the truth. However, when they are responsible for the business, they tend to be convinced that things are true. The responsibility for the business thus generates a firmer belief in the lab's abilities.

8.3.4 DELETING THE PROFESSION – FOREGROUNDING THE ORGANISATION

As I explained in the theory section Law (1994) describes the modes of ordering as generating specific patterns of the social (: 110). For instance, the modes tend to make certain parts very visible while other aspects are 'deleted'. In the chapter on Vocation, I illustrated that the scientific profession and the scientific community are very visible as a result of the mode Vocation, while the labs as organisations faded into the background. Oikos generates the opposite pattern. The labs become very visible, while the scientific community is, if not entirely deleted from view, at least transformed and less important.

Together with Henry, I'm leaving a meeting that I have just observed. 'You sent me an email yesterday,' I volunteer. 'Oh yes, I did… I did… why did I do that?' he replies, half to himself. 'Oh yeah, now I remember, it was so you could see the kind of opposition we are facing and I have been telling you about. No one believes in our results and people can get very hostile just because the results are so unbelievable and can shake the whole community.' The email was from a scientist from another lab who – in so many words – concluded that Gyro Gearloose's assumptions and results were rubbish and that the lab neither deserved funding nor any praise for its work.

In the mode of Vocation, the scientific community seems to be both an enormous group of colleagues from across the globe and, at the same time, custodians of the truth who make sure fraud is not committed. But essentially the scientists think of themselves as members of a large community. This solidarity is no longer visible in the mode of Oikos. Instead, it seems that the scientific world consists of competing labs that do not always treat each other respectfully. Instead of being a member of a large community, Gyro Gearloose has suddenly gained significance as a smaller community where they believe in specific ideas and are in opposition to the wider scientific community. Oikos generates a 'we' – Henry sent me the mail so I could '*see the kind of opposition **we** are facing*' – that has its foundation in the lab and their shared assumptions.

At Curious George, they are, among other areas, working with synthetic biology. This is also visible in other parts of their buildings. On the communication officer's door, there are lots of comics, small posters, debate pieces and illustrations related to synthetic biology pinned up. Canute and the communication officer write many of the debate pieces; however, in other parts of the lab, synthetic biology is utterly *absent. Synthetic biology is, for instance, never mentioned in the wet labs or in relation to the scientists' daily work tasks. At times, some of them tell me that they do not 'do' synthetic biology at all or that they have always worked with synthetic biology, but it is only recently that it has got that name – and they only use it for strategic reasons. But they go to conferences where synthetic biology is included in the headline, and they also collaborate with another lab that is famous for its exciting results related to synthetic biology.*

In the beginning of my the field work, it annoyed me that I could not get hold of this term and if the scientists actually worked in the field of Synthetic Biology. Did they 'do it' or not? And for strategic reasons or not? But the question about the sincerity is not that important. Synthetic Biology seems to 'do' a lot of things for Curious George. One of the things that Synthetic Biology did for Curious George was to create a 'we' that separated them from other labs. For instance they distinguish themselves from the top-down approaches used at other laboratories and identify themselves with the bottom-up approaches characteristics of Synthetic Biology as expressed in the "share-your-parts" idea. When they work together with other faculties under the Synthetic Biology grant and things are a little difficult to run, they remind themselves that this is part of working with Synthetic Biology that is in its nature '*interdisciplinary'*. Synthetic Biology is also present when they participated in Public Engagement exercises and see that as their duty, because they were part of something '*new*'. The PhD students are at times going to '*synbio lunches*' together, where prominent scientists and industrial people give talks and they can discuss different aspects of the discipline and the developments. At the same time, the scientists get their papers accepted in top synthetic biology journals, Canute is evaluating grants for centres for synthetic biology in Great Britain and the lab gets invited to give plenary lectures at international conferences.

In many ways, it is actually irrelevant in this dissertation to answer the question about whether the scientists actually 'do' synthetic biology, because, no matter what, synthetic biology creates many situations where Curious George performs as a united entity. So in many ways, synthetic biology unites the scientists at Curious George and distinguishes the lab from other labs. My impression is that synthetic biology makes the lab into a distinct entity more than it makes the scientists part of an international scientific community. Synthetic biology plays different roles in maintaining the lab and enabling it to thrive.

The responsibility for the business unites the lab, perhaps not surprisingly, in its core assumptions and distinct scientific field, making it stand out as a unified whole. The wider scientific community, which has a strong presence in Vocation, is more in the background here – or, rather, is not considered as a community but instead as voices from other organisations. This deletion of the profession and foregrounding of the organisation is also an important clues to how the scientists consider science's role in society. Vocation generated a view of science as one among several other stable institutions that had some distinctive responsibilities and together made this society work. In this mode, the scientists do not talk about the distribution of responsibilities between different institutions. Instead they seem to constantly focus on their own lab and its possibilities compared with other labs and their possibilities. Their idea of society has become one of a market populated by many small labs that are in fierce competition for very scarce resources. Big institutions as 'the public' or 'government' disappear – only industry and funding agencies are left as other actors on the markets. And they are only visible in respect of their roles as providers of funding or buyers of knowledge.

8.4. NEGOTIATING OIKOS

In the last couple of sections, I have treated Oikos as if it appeared as a pure mode that was never questioned and that pervaded all of the scientists' work. But, of course, it is not. There has already been an entire chapter devoted to another mode, namely, Vocation, and I will describe yet another one, Citizenship, in the next chapter, therefore Oikos is of course also limited. In this section, I will describe some situations where Oikos is negotiated in relation to other modes. In the last chapter, I described some negotiations used by the scientists to alleviate situations where the performance of Oikos is in conflict with Vocation. In what follows, I will add some more descriptions of these negotiations.

I'm following Alan around. He explains the different projects that he will hopefully have time to do today. As we talk, one of the scientists approaches and asks whether Alan has time to help her with an experiment in the afternoon. 'Of course,' Alan volunteers. Afterwards, I ask him how he prioritises the different projects he is working on. 'Carefully' he answers and laughs. But he continues by explaining that the trick is to make the incubation times fit, so he can work on one project while another is stirring. But it requires him to remember all the things that he is doing and to know the exact reasons behind the steps he has taken. This is difficult at times. He also says that he is a little afraid that his focusing on the many different tasks makes him a little less critical with his own scientific work. If he had more time and fewer things to consider at the same time, he believes he could prepare his experiments more properly and consider more carefully why he chooses as he does.

In this story, Alan is concerned that some of the values that are inherent in the mode of Vocation are neglected because he has the responsibility to move many different projects along at the same time, as this will benefit the business the most. While he considers it his responsibility to do so, he is at the same time afraid that it is actually affecting his ability to make decisions regarding the project with acne. While it does not change his behaviour, if his responsibility for 'the business' seems too burdensome, then it does makes him weary, as he gets the feeling that he is actually neglecting the responsibility that he feels for the truth.

The scientists often question the responsibility for the business because they simply feel that they neither have the time nor the experience to do it. Clark from Curious George explains to me that since he is not really educated to commercialise his findings, he finds it inefficient that he has that responsibility. We continue:

Me: *'Would you like to become better at that aspect of your work?'*
Clark: '*Actually no.'*
Me: '*Why not?'*
Clark: *'It is not really that interesting. I would like to have assistants for that or an office where I state: "This is my technology. Now go and see if you can sell it somewhere".'*

In this quote, Clark is not directly rejecting the idea of commercialising his technologies, but he does not find that aspect of his work particularly interesting, and he also finds it suspicious that he has to do work that he is not educated to do. While I am sure he knows that it is utopic, he would prefer some kind of work division, where others took the responsibility for patents and commercialisations so he can focus on doing science. I interpret this as an instance where the responsibility for the business is challenged by the concern for doing what the scientists consider their main job. Clark considers it his main job to conduct research. That is the task that he is educated to do. The responsibility to do something with what he invents is secondary – not that he is against it, but he believes that these different responsibilities should be kept separate. So the responsibility is rejected on the basis of a special form of division of labour, where scientists invent and others commercialise. While this is not realistic, it shows some of the resistance that the scientists feel for this part of their job.

Another way that the scientists negotiate the responsibility for the business is by stressing that they are exactly *not* businessmen. They are public scientists and public scientists should not care about making a profit in the way businessmen do, because that makes them neglect the concern for those duties they have as public scientists. As Miriam underlines:

'I think it's important that we [public scientists] *stand up and say that we don't want to be a service organ for industry, because then we'll never make any kind of work that matters in the long run and that's an important part of academia's role. Because the way they make science in industry… I fear that it isn't about the big discoveries but just improvements of old stuff, because it's all about the bottom line.'*

In this quote, Miriam distances herself from thinking too much about '*the bottom line',* because that concern will take her focus away from the responsibility she has as a public scientist, namely, that of doing science that is not readily applicable, but that somehow may turn out to be a great and useful discovery later on.

Oikos is negotiated during the scientists' daily work in a range of different ways. In some instances, the responsibility for the business is questioned because the scientists are worried that it will collide with their responsibility for the truth – as Alan and Clark describe in relation to different situations. Here, it is mainly a matter of *time* and how they cannot fulfil both responsibilities to the extent they wish if they are to focus on their research as well. This is something I saw rarely. The responsibility for the business is also contradicted with the responsibility the scientists feel they have as *public* scientists. Therefore, they think that this responsibility for the business sets some limitations for their research and should be reserved for corporate scientists. In the next chapter, Citizenship, I will take a closer look at this distinction between public and private.

8.5 DIFFERENCES BETWEEN THE TWO LABS

The two labs share many similarities in terms of how they construct the responsibility for the business and the way they handle it. The basic responsibility for the business is shared and so are the organisational consequences of this responsibility, such as how the responsibility is shared between different professional positions, how the work conditions make them anxious about the future and how they believe in their own assumptions.

However, their attitudes toward this responsibility vary. At Curious George, they see the work they have to do as necessary, but quite tedious. They view much of the responsibility for taking care of the business as something quite annoying and counterproductive for their work. They consider most of it as something that is demanded of them from the outside, most likely from politicians. On the whole, they consider the part of their job that takes focus away from their core responsibility (developing new scientific insights) an annoyance. In an interview, Clark from Curious George describes the joy he gets from doing scientific work:

'I can develop my own projects. If I stumble across something that sounds interesting, I am absolutely free to follow up on that as long as it fits in the concept. And I have to define the concepts through my interests. This is why I was picked for this job and, since I think that I fit this job very well, there is a very good overlap between what I am supposed to do and what I like.'

Mostly, he finds that he is able to '*develop his own projects*', and he also believes that this is what he has been hired to do: '*This is why I was picked for this job*'. But on the other hand, he also has to do a large amount of work to make '*something that sounds interesting*' into a research project that can be funded and done at Curious George. That part, he finds annoying:

'I think the stretch is sometimes a bit over the top. There are certain keywords from which you get funding. If you don't have those in your application, then you can almost guarantee that the reviewer may not even see it – that it will be kicked out beforehand. So if you want to get money, you have to make the stretch and bring that into the project. And hopefully it will be close enough, so that it includes your interest as well.'

He is less satisfied with this part of the job and also finds it slightly silly, as they have to fit certain terms into their applications in order to get the attention of the funding agencies. His comments on the balance between what the scientists find '*interesting*' and the interests of the funding agencies echo many other comments from employees at Curious George. When I ask about the parts of the job that relate to maintaining the business, all of them point to the quest for funding as a demanding and difficult part of their work. They seem to believe that these conditions are getting worse and that they fill up more of their workday than in the past. They also consider the '*freedom*' to choose interesting patterns of research as an important aspect of their job.

One of the scientists at Curious George explains that he likes the job because there is a lot of freedom to be creative connected to the position. But he thinks this is changing due to the increased reliance on external funding and political demands for more '*relevance*'. He is afraid that this will ruin the fruitful scientific environment, because he believes that the less freedom they have to be creative in their work, the less creative they will become, and thereby not deliver the solutions that society is asking for:

'[…] *I believe* […] *that if you are asked to solve a problem very explicitly, like: "Solve this problem now!", then you will walk down a path where everybody else has* *already been. Then it all turns a little more into mechanical thinking and automatic thinking, because you may also feel that there is a deadline and no time for reflection.'*

So for the scientists at Curious George, the part of Oikos that relates to external funding and political demands for the direction of research is tiresome and perhaps damaging for the scientific results, as it demands time and energy. 'The freedom of research' is an ideal that has been both described (e.g. Merton 1973) and discussed before as a specific relation between the institution of science and the rest of society. In order for science to investigate, improve and criticise society, it is supposed to be kept at arms length from other institutions and pursue directions that it finds to be the most pressing. What the scientists in the above excerpts are describing share some similarities with this principle. But in Oikos, this principle is not thought about as an institutional prerequisite, but as an organisational one: The scientists at Curious George see 'freedom' as being time and space to do what they believe is their primary job, namely, doing research. The rest is thought of as 'politics' or 'bureaucracy', something that leaves less time for their core responsibility. On the other hand, they never question their responsibility to be careful when using materials. It seems that they consider this element as both fair and meaningful. In any case, the materials are never really up for debate. Their concern about not having enough time is a little trickier: While they perhaps do not question that they should not waste time directly, this virtue is closely connected to the temporary projects and the temporary positions that they are critical of.

The attitude toward this responsibility is a little different at Gyro Gearloose. First, it seems as if management is embracing this way of managing science, as it keeps public scientists on their toes and forces them to think creatively and move out of their disciplinary comfort zones – as described in the quote by Henry. The scientists who are not part of management do not speak much with me about these aspects of their work life. I am not sure if this is because I have never really gained their confidence and have remained an outsider - or they in general do not discuss these issues. While they do not voice their opinions much, there seemed to be an anxious ambience in the lab due to recent firings and talk about budgets. But it is not my impression that this necessarily makes the scientists sceptical about the way the lab is organised. It seems that they have accepted that temporary positions and external funding is the way to organise scientific work.

It is difficult to say something very distinct about why the two labs have a different attitude to this responsibility. In Denmark, the great reliance on external funding and the increased competition between labs is relatively new. The university reform from 2003 fully initiated this development. Before that, the universities' economies were to a much larger extent based on stable public funding (Gorm-Hansen 2011). The reform has received much critique from the scientific community in Denmark, and many of the working scientists are fiercely against it. This opposition may be an explanation for the differences between Denmark and the USA. But on the other hand, the USA has been through similar reforms of the funding system. The governance structures of the universities are similar in both countries, even though the universities in the USA are in more competition than they are in Denmark.

Another interpretation is that Gyro Gearloose has a more 'entrepreneurial' spirit than Curious George. Henry talks about himself and the other scientists at the lab as being '*inventors*', and this is perhaps an identity that is different than being a 'scientist'. As inventors they are perhaps much more used to the fact that they need others – investors – to believe in them in order to raise money for their projects. This is in contrast to Gyro Gearloose, where Clark maintains that they are '*really geeks*' and focused on the '*whole process of doing science*'. These differences in how they understand their own jobs may also influence their attitude toward the responsibility for the business. Where the scientists at Curious George do not think of themselves as an entity that has a 'business' to run (the fact that they do is a result of external conditions), the scientists at Gyro Gearloose consider the 'business' a much more integrated part of the lab.

8.6 CONCLUSIONS

In this chapter, I have examined a mode of responsibility that I have chosen to call 'Oikos'. Oikos generates some specific forms of conduct, namely, that of investing, saving and maintaining. The scientists justify these forms of behaviour with the responsibility they have for the survival of 'the business' – the organisation they are a part of. The organisational traits Oikos generates are very different than the ones described in the chapter of Vocation. Oikos generates a concern for time as a scarce resource, which I have called '*Fast Science*'. It also generates a strong belief in the labs' own abilities and assumptions. And it creates an understanding of science role in society as that of a business in competition with other businesses – an understanding that makes the united professional community fade into the background.

As part of the concluding remarks, I will now relate the findings from this chapter with the governance rationalities from chapter 6. Interestingly, this mode is perhaps the farthest away from any of the four rationalities, and perhaps it is because not that many scientists writing about 'responsibility' consider this particular responsibility a desired one. But the rationality that comes closest is in my view the '*Contribution Rationality*'. While the focus in chapter 6 is on how the Contribution Rationality demands the external control of scientists and underlines the importance of 'usable' results, some of the elements are still similar, not least the focus on how to govern science in order to make it ‘contribute’. While external governance is certainly in focus, some of the texts also articulate the need to make laboratories and universities look more like ‘businesses’ and make them compete on a market. The logic of the rationality being that this will drive the scientists away from doing work that is not relevant or usable and toward making a valuable contribution to society. The mode of responsibility that I have described here can be viewed as a result of that way of thinking. While the scientists from Curious George claim that these conditions do not affect the core of their research, they also admit that they have to shape their applications in ways that ‘overlap’ with their funders’ interests. Gyro Gearloose is even more radical in so far as they consciously take their point of departure in a specific tangible ‘need’ or ‘problem’ that they identify in society and try to solve. In that way, they do not feel they have to bend their applications too much in comparison with what they want to research; they simply apply to those funding agencies that have similar interests.

At the beginning of this chapter, I stated that Oikos shared some similarities with the ‘entrepreneurial scientist’ as, for instance, described by Etzkowitz (1983, 1998). While it is true that the scientists at times see the potential for taking commercial advantage of their findings and do frequently take patents, Oikos demonstrates some differences compared to Etzkowitz’s descriptions. Where the ‘entrepreneurial scientist’ in his view has many opportunities in the double role of scientist and businessman or -woman, Oikos in the scientists’ view seems to be mainly generated by external pressures for competition and the market-like structures of the universities – it is a necessity rather than an opportunity. That this perhaps also increases their possibilities for personal economic gain seems to be a smaller part of the scientists’ attitude. They are more concerned with how they can keep their jobs after the current funding runs out and what the lab’s future may look like under the current conditions.

9. CITIZENSHIP

In this chapter, I will describe the third and last ‘mode of responsibility’: the mode of Citizenship. In the previous chapters, I have already discussed how concerns about contributing to the surrounding society at times made it difficult for scientists to live up to what they consider to be responsibility for the truth and to the organisation. But in this chapter, I will illustrate that this concern is indeed a mode in itself. In several ways, this chapter directly addresses the science governance literature on ‘Responsible Science’ (e.g. Owen et al. 2014). First, because this chapter demonstrates that the government rationality does indeed work in the organisations – and I illustrate in detail how. But I also examine how Citizenship differs from ‘Responsible Science’, as it also draws on other contemporary ideals about science and society. Second, because it deals with how concerns for the surrounding society are at once embraced, negotiated and rejected. In that way, it engages with some of the practicable obstacles in realising an ideal about a science ‘*for the people*’ (Owen, Macnaghten, and Stilgoe 2012). Finally, this chapter describes how the abovementioned rationality works in relation to different scientific pursuits and disciplines, thereby questioning whether elements in this rationality should be used as general guidelines in science governance, as has recently been the case in the European Union’s new framework for research, Horizon 2020 (Von Schomberg 2013).

Like the previous two chapters, this chapter begins by exploring a range of conducts in this mode, before moving on to justifications and the organisational consequences. It finishes by relating Citizenship to the two previous modes and the ideals about responsible conduct as we saw them in chapter x.

9.1 CONDUCT

In this section, I will examine three different forms of conduct that are pervasive in Citizenship, namely ‘speaking about science’, ‘listening to the public’ and ‘doing research’. All of the conducts are ways of behaving that are supposed to fulfil the responsibility of contributing something to the public, and all of them are acquired abilities. I observed how they are taught to newcomers in the lab and sustained through different, repeated situations and events. They are also encouraged by management and discussed as part of the daily work of the organization. As in the previous chapters, I want to make it clear that the scientists under discussion here would not necessarily deploy the same distinctions as I have: For instance the distinctions, between the conduct ‘speaking about science’ and that of ‘listening to the public’. But these distinctions bring out some details about the differences between the two labs and their views of the public. While ‘listening to the public’ and ‘speaking about science’ may well take place at the same time and in the same space, I also see them as two different ways of performing the mode of Citizenship. ‘Speaking about science’ does not imply that the scientists intend to listen to their audience, but merely that they consider it their duty to inform a wider public about what they are doing. ‘Listening to the public’ on the other hand implies the expectation that the public has a legitimate say in science, which the scientists actively take their time to listen to. What connect all forms of conduct are the justifications deployed by the scientists, which we will return to after the section on conduct.

9.1.1 SPEAKING ABOUT SCIENCE

The first form of conduct that I want to describe is that of 'speaking about science'. I have chosen to include it because I discovered that this takes up a considerable part of the scientists' work (and spare) time. At the same time, I also believe that both the way they speak about science and the places they do so share some distinctive similarities, which I find interesting to draw out in order to show what the mode of Citizenship is about. First, I will describe what it means to 'speak about science' in the two laboratories.

It turned out, to my surprise, that most of the scientists in the two labs are quite engaged in speaking about their research in various arenas. One of the seniors at Curious George claims that being able to communicate about one's own research is a vital part of the scientists' upbringing:

'What we [the supervisors] *really, really want is that our students understand their project and that they are able to tell about it. I mean all of us have to be able to sit next to our grandmother and explain our project to her, before she falls asleep over dinner. No one has a project that is so difficult that you cannot explain it in three minutes.'*

The conduct of 'speaking about science' is very straightforward. It is about the scientists going around and describing their work to people outside their own field. Some, but not all, of this is formally organised and takes place directly in the work context, as in this instance:

Tonight is 'Public Lab Night', a bi-annual event, where the whole university, including Gyro Gearloose, invites the public inside. On a 'normal' weekday, it is actually quite difficult to get into the lab. Guards are barring the entrance and *everyone needs a special invitation to get in if you are not employed here. To open the doors you either use the iris-recognition device or a personal key-card. At first, I believed that all these precautions were in place to prevent industrial espionage. But it turns out – one of the scientists tells me – that it is supposed to hinder animal activists from getting in: 'you know, they have the whole basement full of laboratory animals, right?'*

But tonight the doors are going to be wide open and people may come from far and wide. The lab is buzzing with the preparations of different exercises and exhibitions, which are on show for the guests. Annie, whom I'm following, fetches a big box from under one of the desks. It contains a vast amount of nuts and bolts and some homemade strips of cardboard and paper, which shine in bright blue, yellow, orange and red colours. They are identical to the multi-coloured diagrams that I have seen her work on when she is preparing presentations of her dissertation, but the diagram has been torn apart into strips. 'This is the kit I use when I'm telling people about the diagnostic device that I'm working on', she says. 'The colour blue in a signature indicates that the individual is healthy, whereas orange, yellow and red indicate the presence of disease. I have made my work into a puzzle, so kids can try to assemble the diagram – it's amazing, even kids, who are five or six years old, can tell a sick person from a healthy person using their color coded signature responses.'

All the other volunteers, who are going to present their research tonight, have tried to come up with similar creative ways of speaking about their projects. There are pieces of cardboard, scissors and post-its lying around the lab. The devices are quite different from the grand machines and small flasks usually in action. So, on the one hand, the lab feels a little unusual. This is not a typical day: All the volunteers for the evening wear the same T-shirt with a big 'ASK ME' sign on the back, the front-doors are unexpectedly open, the guards joke around, and the instruments have shifted from being so alien to me that they elude description to being items I recognise from theme days at primary school. On the other hand, perhaps it is just me who thinks about the difference; the scientists seem to feel at ease with this change of environment. While this day may be a little different than a normal workday, the scientists do not seem confused or in doubt about their task. They have clearly done all of this before. Annie has experimented with the puzzles several times, and she has found a way to tell children about the project in an engaging way. She tells me that she likes the 'Public Lab Nights' because

'You never know if somebody is going to give you some kind of clue to the research. And the kids are funny. They are so eager and not afraid to ask at all.'

Sandra, a newly employed scientist from Gyro Gearloose, comments that South-West University in general seems quite good at 'outreach':

'In my old job, there weren't that many outreach activities or anything, you know, of that stuff. I really think that the president here has done a great job in promoting that kind of thing. I mean, I see all these adverts for science-cafés and stuff and everybody seems involved.'

Interestingly, despite being at a university known for its many outreach activities, several scientists commented that they '*seldom have the opportunity*' to discuss their research. This is a little surprising to me. I have almost been overwhelmed by the number of engagement projects of various sorts they are part of at Gyro Gearloose. Some of it, such as the 'Public Night Lab', is part of the university's overall outreach strategy, and some of it is initiated by the lab itself. There are a lot of activities by which they attempt to tell people about the research they are doing. They frequently give talks at the local Exploratorium, they participate in 'Public Lab Nights', and I have seen their PIs appear at other public engagement events. Nevertheless, they still seem as if they could somehow do more, and they talk about themselves as isolated from other people. I frequently heard scientists express the sentiment that they are outside the rest of society and not engaged enough. This is not only the case at Gyro Gearloose, but also at Curious George. The scientists feel guilty about their lack of engagement and feel they should somehow do more to reach out. My interpretation is that their idea of isolation comes from the image of the lonely scientist in the 'Ivory Tower'. This popular image of the scientist as a lonely, geeky person high up in a shiny, impenetrable tower seemed at times rather strong among the scientists – despite the fact that they should be the first to know that things far from conform to this stereotype Barry, Chandler and Clarke 2001). I believe that the mode of Citizenship is far away from the image of the scientist, alone in his ivory tower. However, it is very telling about the mode that the scientists both reproduce the image of themselves in an ivory tower while not liking this role. It is important for them to get out of that tower, not to be isolated, but to be, rather, engaging and responsive. But I do not think that any number of science cafés and public lab nights would change their views of their profession as being decoupled from the rest of society. The scientists speak a lot about their work in various formalised settings, but they still believe it to be too little compared to how much they feel they *should* engage.

I will return to this matter in greater detail in the section called '*Basic Science Guilt*' (9.3.3). For the purposes here, it is enough to underline the constant insecurity about communication, because it highlights the conduct of 'speaking about science'. Having a bad conscience about not telling the public enough about their research projects indicates that the scientists acknowledge there being expectations to their role, which they then do not believe they fulfil; partly because it clashes with another ideal, namely, that of being distant from society.

At both Gyro Gearloose and Curious George, the act of speaking about their research to wider audiences beyond their scientific community is something one does and something that is considered important. All the scientists I engaged with have told '*laypeople*' about their research in various places. They have been at science festivals, they have told school children about science, they have been at libraries and cafés and talked to interested citizens, they have invited people into their labs on special nights and showed them what they worked with, some have written debate pieces for the national newspapers, and some have appeared on television news once in a while. In my view, these places for 'speaking about science' are interesting in regard to the current focus on public legitimacy. Most of these events occur in places that are recognised as being at the core of public life and general education, or as Nowotny (1993) would put it, the 'agora'. Schools, newspapers, libraries, TV news and academia: All places that have historically been recognised as locations with public access that encourage debate, enlightenment and development (although ideas of who exactly has been considered a member of this 'public' has shifted over time). The performance of this mode in classic public spaces adds to my impression that the scientists consider public legitimacy important. The scientists are obliged to show up in spaces that are made for the public and report about their work – not only so that the public can be enlightened and science debated, but also so that fellow citizens in their community can value and evaluate the scientists' work. This issue will be further discussed in the Justification section.

This formally organised and, by its nature, '*public*' way of speaking about science is just one of the ways that they engage, with another way being more private and informally organised and which often happens in everyday-like situations with only a few people involved. In the following quote, it is one of the seniors, Sandra, at Gyro

Gearloose who describes science communication at the bus stop; she finds it a bit difficult, but tries to make an effort anyway:

'I cannot discuss in a way, you know, where anyone would understand what I'm saying, because it's all about crystallography and in-vitro translations. I just have to put it in different terms, and we don't always have a lot of interaction with society in general, so it's just like neighbours or people I see at the bus – my daughter's bus stop – so I try to explain what I'm doing, the best that I can…'

Many of the scientists talk about communication in the same way: it's all a bit difficult, as they need to translate between their specialised work-language and a different everyday language (and again there is the idea that they '*do not often have a lot of interaction with society'*), but they claim that they make an effort anyway. They speak about all sorts of places where they meet people and discuss their research. Family gatherings, visits to their grandmother, parties at clubs and bars – or at their daughter's bus stop on the way to school. Many of them talk about discussions that are taken up by individuals who are critical of the technologies they work with, be it vaccines, diagnostic devises or GMO. Sandra continues:

'But it's difficult, you know. People have their own ideas about what you are doing or they are very anti-vaccine or whatever […]. *I think that is so weird...*[…] *I have just met them at the bus stop or where-ever, and then you try to say "well, we do vaccines, not only to protect our own health but to our… the help for the elderly people we know or the six-month-old babies we know", or whatever, you know, we are trying to protect society, and it isn't just the drug companies that are trying to rip you off, you know, as I hear a lot.'*

From the quote, it seems that she becomes a little offended that people are against vaccines, as it is her work and she believes that she is doing a good thing. She says that '*we are trying to protect society*' and then people just answer that they are '*anti-vaccine*' or complain to her that it is the drug companies that are trying to '*rip you off*'. Interestingly, she has at other sessions been very understanding about (her own imagination of) the general public's scepticism about science. She finds it reasonable that people are anxious about all the money that gets poured into science compared to how few results they get back (see also 9.2.1). But when it's about her personal work, she becomes more passionate and defensive. This is a reaction I often got from the scientists who discuss their research areas in their spare time. They frequently get the impression that science communication is about defending their own work and position. As Angela from Curious George says:

'I have friends and acquaintances who are really into that eco-wave and who believe that being part of that equals being against everything that could be named mingling with nature in any kind of way. They think [my work] *is a little… But, when I start to actually talk with these people, they are quite easily convinced that what we do here is pretty ok and orderly.'*

Usually, they find their own work '*ok*', and they can explain why it is so important and potentially useful, and they therefore want to defend it. At the same time, the discussions become an opportunity to actually discuss and perhaps convince people about their position. Miriam, for instance, says:

'One of my good friends, for instance, he has begun to send me all these papers about how dangerous GMO can be, because he knows that I'm working with it.'

Me: *'What's your reaction to that?'*

Miriam: *'It's a little "oh bummer, now do I really have to familiarise myself with all that stuff?" But I'm becoming more and more aware of how important it is to discuss these things, because in the last instance, we can see how we make obstacles for ourselves in the future, if we don't do it.'*

I had conducted several interviews with Miriam at this point and had also followed her around for quite some time. Thus, I am not sure whether it has been my own influence that has played a part here, or whether it is the almost two year period she has spent at Curious George, but she has definitely changed her mind about the importance of non-scientists' opinions. In the beginning, she was quite convinced that people who could not see the advantages of GMO were either just not enlightened yet, or stubborn, radical activists. But this attitude has gradually changed. She still believes that GMO is a good solution to a range of different problems, but she has also started to consider her own role as a scientist differently. She has started to think it important to participate in dialogues about emerging GMO technologies, because without it she risks her area disappearing due to lack of public support. What is left is the annoyance that she has to get acquainted with a lot of new material, as this is tiresome and requires extra work. This very practical and indeed onerous aspect of communication is something many of the scientists pointed to.

In many ways, Miriam, Angela and Sandra's stories reflect an attitude that I often encountered: They are a little tired of talking about their research in their spare time, because it is hard to spend a lot of time defending one's job and choice of living – and sometimes people can be downright unpleasant. On the other hand, they keep doing so, and they clearly feel they are obliged to. In Miriam's case, her (new) position is almost an exact replica of the recommendations from recent Science Communication Literature. Several authors (e.g. Kearnes and Rip 2009) have reported that nanotechnology is seen as an opportunity to '*get it right this time*' – in opposition to the controversies following the advancements of GM-technologies. My impression from Miriam and others is that the scientists hope to curb resistance to their field by making the right kind of communication 'in advance'. However, there is more to it than that. Miriam, at least, has changed her impression of her work and the role of science in society. When I, in the same interview, ask her if she thinks it is okay that public attitudes influence, which areas get funding, she replies:

'Yes, I'm convinced it is, and perhaps I wasn't the last time you interviewed me, but I'm increasingly convinced about that. I mean, yes, it's their [the public's] *money and if we can't convince them that we are doing something good, then perhaps it's not the right thing that we are doing. But we also have to convince them that there really can come something good* [out of science] *if they dare take the risk…'*

Miriam's impression of the science policy world has changed. In earlier interviews, she shared the view that the public, in contrast to the scientists, is irrational and therefore should be kept out of decision-making about the direction of science. Now she more considers science policy as an arena where the public has a legitimate say and where everyone, including the scientists, should try to 'convince' each other about the best direction for science and society and negotiate a solution. She therefore pulls herself together and reads her friend's anti-GMO literature, even though it means extra work. I find Miriam's shifting attitude about the public's legitimate right to participate in science policy decisions interesting, as it reflects the contemporary idea about science's (and in general public institutions') role in society; namely, that scientists continuously have to address the public's concerns, because the public has a legitimate voice and right to change the direction of science. In that regard, the public sphere is considered a place where several heterogeneous, but equal, actors *negotiate* opinions and try to convince each other of their merits (Horst 2012b: 128). It therefore becomes the scientists' duty to speak about their research in order to ‘convince’ their peers, the public, that they are working for them. In this also lies a shift in the scientists’ view of the connection between ‘the citizen’ and ‘the state’. The citizen is, in this view, seen as having the right to – through choices and actions – deliver policy objectives (Barnes and Prior 2009: 6). This in turn has consequences for the way the scientists do their job: From the observations so far, it seems that they have to partake in speaking about their science more ‘publically’ in order to obtain and/or deserve public support for public science’s future.

I do not believe in any way that all the scientists whom I met during my fieldwork spoke about research in the same way and with the same expectations. But all the scientists seem to agree that they should be able to – and do frequently – explain their research to laypeople. This does not necessarily mean that they agree that the public should interfere with decision-making on funding. Nor does it mean that they all agree that the public should have legitimate opinions about, say, ethical or safety issues. But as a minimum they agree that it is a part of their job to speak publically about science.

9.1.2 LISTENING TO THE PUBLIC

In this section, I will describe conduct that is closely linked to that of ‘speaking about science’, namely, that of ‘listening to the public’. As I already hinted in the last section, it is important for the scientists to engage with the public, and I further described how this is necessary, as they need to negotiate science policy; it is only by speaking about their research that they can move the public. In this section, however, I will go a step further and argue that the scientists also take particular care to *listen* to the public and incorporate what they hear into their daily work.

A while before I started my fieldwork at Gyro Gearloose, they tested one of their inventions, a diagnostic device, on a group of citizens from the nearby city. The *diagnostic device could monitor the general health of a person from day to day and also 'predict' certain diseases (such as certain forms of Alzheimer's and cancer), even if there would not be an outbreak for years to come. The scientists held meetings with the participants and informed them about the functions of the device, what it could do and the prospects of getting a technology like that on the market. If people were positive or curious, they could become part of the experiments and get a chance to monitor their own health for a period of time. A lot of people volunteered – and many even wanted to continue after the experiment had ended. Some of them came up with the idea of a website, so they could check their health condition online. The scientists were quite surprised by this development; they had not expected people to get so engaged, and they got very busy setting up the webpage. One of the ideas with this experiment was to get new test persons for the technology, which was often difficult. Another one was to get input from people about the problems and prospects of the device, so they could alter the designs and functions of the technology before they tried to commercialise it.*

During my visit, the scientists at Gyro Gearloose often referred to this test as a tremendous success. Not only did they recruit a lot of test persons, they also got valuable input about their diagnostic device, which changed some of the basic features of the technology. In its original form, the diagnostic device measured the amount of antibodies in the blood and, based on that information, estimated the health condition of the person in question. Therefore, everyone who used the device needed to prick him or herself with a needle. In an interview with Charles, who had been part of organising the project, he told me about one of the attendees, who had asked if they could come up with another method because she hated needles. He comments that '*after we got that input from that lady about not wanting to hurt herself, I really believe things got going*'. Due to her comment, the scientists went back to the lab and started thinking about alternative ways of measuring antibodies:

‘*And that* [after the woman questioned the use of needles] *was when we started thinking about doing saliva tests, because it turns out that saliva and blood actually have a lot of the same antibodies. A couple of very specific ones are missing, but most of them are the same.*’

They ended up redesigning the entire device, so it is now based on a saliva test instead of a blood test. They had to sacrifice the identification of ‘*a couple of very specific*’ antibodies in the redesign process, but it seems that they think it was worth it. The scientists often refer to this story when they argue for the value of including laypeople in their research projects. I heard references to ‘*that woman*’ at several meetings and the story was retold to me in several interviews. While she seems to be one of the few individuals (to my knowledge) who has had that great an influence on their work, they use the example to underline that one can never know whether ordinary people may possess knowledge that can change the direction of research and that listening to people is therefore of the utmost importance. As one of the scientists said in an interview:

‘*I think almost everybody here has given a talk at the Science Café at some point, and those attending are people, you know, off the street, and they ask some of the weirdest questions, but they are always pretty good, and it's usually full, so apparently there is a group of people – and it's always different people – that have enough interest to go find out what scientists are doing. And they make suggestions that we have brought back here.*’

Talking to the scientists at Gyro Gearloose, they seem to think that it is valuable to listen to people ‘off the street’ because at some point somebody with valuable input – as the woman with the needle-phobia – will show up. They are (almost literally in this story) looking for the needle in the haystack. They are looking for that one individual who can pose the right question or propose the right solution, and thereby bring the research forward. According to my observations, this individual rarely shows up, but the expectation that she or he will is so embedded among the scientists that they keep on looking.

Here is another example of this way of interacting. It is the day after the 'Public Lab Night', and Annie tells me all about her success with the cardboard puzzles. More than a thousand visitors had been present at the university and she had had many interesting discussions, especially with one of the attendees:

'It [doing science] *is like finding clues to the mystery, and you never know where you will find them, for instance, … So, I was talking to this layperson* [one of the visitors] *about the research that we do here on valley fever, and she said: "Oh, did this fungus*[37] *develop from tuberculosis?" And I asked her why she said that, and she told me that she was a native and had lived here all her life. And in the early 1800s, this place, was a tuberculosis sanatorium, which meant that patients from all over the USA basically travelled here because of the weather, and it helped in treating the tuberculosis, because at that time they did not have the antibacterial for it. So, some people got better and went home, but a good majority of them did not survive and died here*. […]. *Inspired by her, I went into looking at the genomic level – the actual individual genes of the two different pathogens [tuberculosis and valley fever], where one is a bacterium and the other is a fungus… In my head they were not related in any way, but it turned out that a good majority of them* [the genes] *were identical.* […] *So it was talking to a complete layperson about valley fever and her asking me*

[37] Valley fever is caused by spores from a specific fungus, which people inhale. The fungus is found in the South-western region of the USA and Northern Mexico.

this question that sparked this whole area […]. *It's scary because it also confirms that what we do as humans to the environment is actually coming back to you…'*

Annie is trying to understand valley fever and develop a diagnostic device that can detect the disease quickly and all of a sudden '*a layperson*' is giving her clues as to the origin and appearance of the disease because she has local knowledge and can provide '*new clues to the mystery*'. Annie also points to the advantage the woman had in *not* being trained as a scientist. The woman did not consider the fact that tuberculosis is caused by a bacterium, whereas a fungus causes valley fever. She is therefore able to suggest a connection that the scientist finds implausible, but which turns out to be true. The woman, in fact, adds a further dimension to Annie's study, as valley fever can now also be seen as an unintended side effect of human decisions made a hundred years ago. These kinds of engagements, where individuals are helping the scientists move forward, are told and cherished at Gyro Gearloose as something valuable, albeit specific examples of helpful insights are actually quite few. I only ever heard about this example and the experience with the woman who hated needles. But on the other hand, neither did Charlie nor Annie ever think that they knew better or did not need the help and opinions of the public in their work. And I think the fact that the scientists keep participating and sharing stories about the worth of laypeople's input testifies to the significance they ascribe to it as part of their daily work. As Annie commented on another occasion:

'What we do here […] *is "translational research". And because it is translational, because it has repercussions on the public, it is in our interest to truly understand what the real question in their mind is, or what it is that they need a solution for – because if we don't, we will just give them a solution that will not even be able to help them.'*

Even though this might sound too noble to actually be true, it was an argument that I often encountered at Gyro Gearloose. They need to '*understand*' what the public wants, because otherwise they will not be able to provide '*a solution*' nor '*be able to help them*'; they therefore find it important to listen. They do not claim that every input is valuable. As Charlie said '*some of them ask the weirdest questions*', but sometimes there is one individual who helps them progress and changes their perception of what they are doing – and this one individual is the reason that it is worth the trouble. They connect this importance to listening to the specific kind of research they do, namely, what they call '*translational*': research that aims at finding practical uses in basic science. Furthermore, I see this way of listening to the public as reflecting classic American values about the public. It reflects the idea of the seemingly ordinary individual who carries a great idea or potential within that ends up changing the world – or perhaps a research question. This view is also reflected in the way that the lab sees itself (which I also described in Oikos). They are an ordinary lab that has extraordinary ideas, and they go through much hardship in order to realise their great potential.

The way that scientists at Curious George listen to the public is different. It is not focused on individual input from specific laypersons; it is more based on general impressions that they receive from the media, from contacts in the plant and tree industries, from NGOs, and so on. The following quote is from an interview where Clark tells me about his new research idea, based on public demand for organic Christmas trees:

'Well, Christmas trees are quite fragile [due to aphids, a parasite that eats the trees]. *So there is a turnover every year of about 1,5 billion Danish kroner. And the little aphid is the worst pest, and the reason for that is… well, the implication is that a lot of pesticide is used. And the Danish people have started to say: "We would like to get*

organic Christmas trees." But there's a problem: The organic Christmas trees are being eaten up. So there is one very important part of research where I think we could make cross-faculty or cross-departmental collaborations.'

Clark believes that he could modify Christmas trees genetically and thereby create an organic Christmas tree that would be better for the environment, the Danish consumers and for the lab itself. While I personally believe that he is overestimating the Danish consumers' willingness to buy genetically modified Christmas trees, he tries to pick up on a current public issue that he and his team have the expertise to solve. But compared to Gyro Gearloose, he has received this knowledge differently; that is, an individual citizen has not delivered it personally. It is an issue that he has probably picked up on by looking at sale statistics, talked to plant breeders about, or perhaps read about in an article in the morning paper. This is the quite typical way in which scientists at Curious George listen to the public.

They do not talk about single individuals as '*that woman with the needle*' as they do at Gyro Gearloose. They talk about '*the public'* or '*people*' as an abstract entity, and they try to pick up on this shared group's sentiments about the lab's research field, GM plants, or whatever the case may be. They have, for instance, decided that they will never plant any GM plants outside securely closed tanks, because they know that '*the public'* does not like it – not even if it is allowed legally. Internally, the scientists actually believe that this precaution is slightly silly, as they do not consider all GM plants in open fields a great threat to the surrounding eco-systems. But they have accepted that '*the public*' does not accept GM plants in open fields, and thus they obey.

It is quite common that scientists from Curious George express these general ideas about certain things that '*the public'* does or does not accept or has recently changed its mind about. For instance, they share the perception that the public has become more positive toward GM plants over the years. From time to time, they articulate this perception of a public discourse in relation to specific research themes, because they want to emphasise that there is an issue where '*the public*' is concerned. It is not really clear how the scientists know about '*the public's*' perceptions of science, but it is clear that they have an idea about common public discourses on certain subjects; for instance, Catherine comments on the problem of antibiotic-resistant bacteria in science:

'Of course, it's okay that people have an opinion about science. If you, for instance, think about antibiotic resistance: If the public should decide, then we should not use it [antibiotics]*, and that is okay, but I'm just not sure what they think we should use instead?'*

As such, Catherine is okay with '*people*' having a say in the direction in which science might go and how they should work. But she also finds these situations difficult because she cannot rely on a similar idea of a common public opinion about what to do instead. In general, my impression is that the scientists rely quite a lot on their own perception about public opinion and try to react to it as they see fit. At the same time, they find it frustrating because they feel they do not have enough knowledge about what exactly the public opinion is or how it should be engaged with specifically.

It seems that Curious George is unusual in its insistence on the importance of the public. This impression relies on the observation that this attention is something that the scientists have learned over their first period of time at the lab. This can, for instance, be observed from the courses and activities that the juniors are encouraged to partake in when they begin their PhDs. Their manager, Canute, works hard to get approval for as many kinds of courses as possible, including courses on start-ups and how the medical industry works, etc. He has also initiated group meetings for juniors with a philosopher, who works with synthetic biology, so the juniors can discuss ethical aspects of their job with him. However, not all the groups established have turned out to as successes, because the students do not feel they have adequate time to attend them with all their other responsibilities in mind. The students are given the opportunity to attend 'synbio-lunches', where prominent guests (scientists and non-scientists) from all over the world are invited to discuss the future of both synthetic biology and synthetic biologists. One of Canute's students, Catherine, is particularly interested in communication activities. Canute and her arrange for her sabbatical at another science institution, which is mandatory for PhD students, to be held at the Department for Agriculture and Food, where she learned about policy processes and political communication.

So the lab does a lot to teach its juniors about the world outside and the legitimacy of other perspectives on their field. However, my impression is that this way of bringing up other perspectives is relatively new. I get this impression partly from the fact that some of the 'old' seniors do not seem as preoccupied with this aspect of the scientific vocation as the younger ones. Furthermore, the PI, Canute, says himself that '*things have changed the last twenty years and we have got to keep up*'. The department has worked with the gene modification of plants for a many years and they went through the so-called 'GMO crisis' around the turn of the millennium (see also Theory 2.1.1). Genetically modified crops and products became extremely unpopular among the European publics. As a consequence of this public resistance, research funding for GMO dropped dramatically, and none of the scientists at Curious George foresaw this development, which was a financial catastrophe for the field and for the lab. So it is no wonder that listening to the public has become part of their job. They have experienced first-hand how opportunities for research funding have suddenly disappeared due to public unpopularity, and they are now adapting to a situation where the public's opinions can have a great impact on their everyday work. They have thus acquired the ability to listen and to try to be responsive to what they think they hear.

Summing up, both Gyro Gearloose and Curious George consider it their responsibility to listen to the public and they do so, albeit in different ways. At Gyro Gearloose, they are preoccupied with the idea of the single individual, who – in unexpected ways – brings the development of their technologies forward. At Curious George, they view the public as a large, but rather abstract entity which has a legitimate say in debates about the direction of research, despite the fact that they do not always agree with its opinions.

In my interpretation, there is a connection between the kinds of science that the two labs are doing and their ways of listening. At Gyro Gearloose, they try to invent specific products that people can use in their everyday lives and thus potential users' inputs are considered quite valid in that development. At Curious George, they work with a broad range of issues, including what they label '*basic research*', the specific development of crops for developing countries and new forms of medical treatment. None of these are considered to be about individual use; they are more about the general development of science itself, whether it be agriculture, the environment or medical therapy. The individual is not considered a target for any of these technologies. Instead, the technologies are pathways to different emergent, shared futures – for instance, a future where pesticide use is replaced with GM plants. These shifts indicate new visions of the way society should be organised in these areas, and my impression is therefore that the scientists are perceptive of the discourses focusing on more general changes. Of course, one could argue that the diagnostic device's potential could initiate large transformations of society as well; for instance, in relation to the information that health insurance companies demand or in relation to how we plan our lives if we know we are going to die from a fatal disease in ten years time. But this does not seem to be an important issue in relation to how Gyro Gearloose uses input from citizens.

9.1.3 DOING RESEARCH

The last type of conduct that I want to describe, as part of the mode of Citizenship, is quite simple: doing research. Doing research has also been part of the other modes I have described, so the challenge here is to demonstrate how this way of doing research can be viewed as different from the other two.

In the following excerpt from an interview with Annie from Gyro Gearloose, she talks about what she considers her main responsibility as a scientist to be:

Annie: *'I think, quite simply, it is two things: One is of course to understand the problems that surround us, without involving any possible prejudice or personal judgment in making their [the public's] assessment of what is the problem. And secondly, solving the problem.'*
Me: *'For whom are you solving it?'*
Annie: *'For people, which is the reason why I mentioned the importance of understanding the problem appropriately without having your own personal judgment in the process. Truly just understanding what the question is, and what the problem is that these people face. How can I best solve it?'*

As I am doing a project about something as principled as 'social responsibility' in science, I am often faced with the question of whether the scientists have told me stories where they appear nobler and more concerned than they are in practice. This consideration often came up when people offered me very general descriptions of their jobs or offered explanations of what they were doing which closely resembled the CUDOS norms as described by Merton (Merton 1973). I do think Annie does that in the excerpt above, and I have two comments to that: First, this study is a combination of interviews and observation studies, and I have attempted to look at a combination of what people have told me and what they have done. Not in order to make a comparison, but in order to see the combined practices of '*imagining*' and '*doing*' responsibility. '*Conduct*' implies a combination of what one does and how one responds to certain perceived expectations about one's role in a specific situation. Therefore, overly idealised accounts of the scientific work has helped me gain insights into what the scientists consider noble and right and how they connect this with their daily work.

The above quote is an instance of such a question: Does Annie truly listen to people without prejudices? And if so, how is she able to do that? But despite the suspicion that this could prove difficult, I do believe the quote is interesting, as it expresses an ideal of how scientists should do their work. In this quotation, Annie, who works on one of the diagnostic devices, explains that she considers '*solving people's problems*' her main responsibility. These problems are the basis of her work and the job is done when she has solved it. At another point in the interview, she explains that she also sees finishing her doctoral degree as her main responsibility, but that these two responsibilities coincide: Her doctoral degree *is* to develop a diagnostic device that can help patients with symptoms of infectious diseases. In that way, she thinks that her responsibility and her daily work tasks are identical: By developing this device, she is also solving a problem for people. In this quote, it is further interesting to see reminiscences of the Vocation mode. Annie wants to understand people's problems '*without involving any possible prejudice or personal judgment*', which is close to the ideals found in Vocation about not letting personal judgments interfere with work. However, in this instance, they are not used as a way of distancing oneself from society. Quite the contrary, Annie is using her professional values as a way to exclude her own opinions in order to be able to hear what '*people*' are telling her. From an outsider's perspective, this may prove difficult, and, furthermore, it is difficult to see how this interaction with '*people*' has taken place in relation to her choice of research subject. But it is certain that she considers her research problem to be motivated by a problem in society that she wants to solve.

The scientists at Gyro Gearloose organised their research process according to what their final goal – a specific product – is. Henry, the PI at Gyro Gearloose, tells me about this way of doing science in an interview:

'[…] [w]*e start at the top – we actually mean what we say. So when* [other] *people do a grant about cancer,* […] *what they really write is "I am going to study this pathway and I am going to study this – and this may have something to do with cancer". And here we say: "let us figure out the new target – what do we need to do to get to the new target", where most people say "this is what I do and I am going to adjust it to sound like* […] *it will have this implication". And we just work backwards. So we are working backwards from everybody else, which allows us to keep focus, because we really say, "we want to have a prophylactic cancer vaccine, let us go figure out how we have to do it." And we even decided to do the prophylactic cancer vaccine because that was the best solution to the cancer problem – you know it has even gone further up from that. In that sense we organise our centre completely differently, and then the faculty in here*[38]*, which really was unique, we all bought into the big visions, and then we said we will organise ourselves to accomplish what we agreed, we are going to organise ourselves to accomplish the visions, so that is different than having faculty where everybody says "okay, you do this, you do that – we will try to write a report that sounds like we have worked together." You know, it is just pretty typical*

[38] Gyro Gearloose is part of a larger research institute, where several biotech laboratories are assembled under the heading of translational research.

in departments – and most centres actually – but we try to do that differently here, we are still doing it different here.'

In this quote, Henry ties their way of doing science to the specific place: the centre that he and his lab form a part of. And this place is, according to Henry, distinct from other places because they organise their research in relation to their target, and their target is based on '*the best solution*', in this case in relation to '*the cancer problem*'. In his own words, they work '*backwards*' compared with other scientists, because their goal, a specific medical technology, is set from the start, whereas other scientists, in his opinion, start out with research topics that they find interesting. On another occasion, he adds that their lab is more '*closely knitted together*' than other labs because they have been united in order to '*accomplish their visions*'.

During my fieldwork, it has been clearly visible that the scientists at Gyro Gearloose are organised with the end-goal as the default principle. The first question in the STIR interviews is always, 'What are you doing today?'. At Gyro Gearloose, they often replied in the line of '*I'm working on the cancer vaccine' or 'I'm at the subproject 2 on the diagnostic device*'. As many of them are working on several projects, they think it important to identify which project they are working on, rather than – as at Curious George – what they are doing more specifically that day, for example, '*sending off a genome to be sequenced'* or *'preparing for the 24-hour experiment'*. In that way, it is pertinent how Gyro Gearloose identifies with the end product of their research. This is also clear from the many different disciplines that are represented in the lab. While all of them have to accomplish something alone, for instance, a doctoral dissertation, the individual project always forms part of a bigger project, where they each contribute with specialised knowledge in order to realise an advanced technology. On the diagnostic device project, they employ, among others, immunologists, advanced computer statisticians and molecular biologists to cover the different aspects of the technology production. The general organisation of the work is fundamentally based on the output. This output focus also became evident when I overheard a conversation between one of the PIs, Emma, and, Sandra, at Gyro Gearloose:

Sandra bumps into Emma on her way out of the laboratory. She apologises for not attending a meeting in their research group yesterday, but she did not know about it. Emma tells her not to worry; if she wants to, she can be added to the group's email list and get the meeting alerts, but not that much happened yesterday that she needs to know. 'Oh, only one thing that is important', Emma adds, 'I wanted the meeting to go in another direction than last time, where everyone got caught up in this knowledge thing. I feel that it's my role as PI to step in and say, "no, we are doing a cancer vaccine, that is the goal, all the knowledge will come as we go along, for sure…", so we talked about that and I think people are back on track.'

In this story, the specific way of organising the research according to their shared responsibility for the end product becomes apparent. Apparently, the group has got '*caught up in the knowledge thing*' – the theoretical implications of developing a cancer vaccine – but Emma thinks that these considerations are of less importance compared to the vaccine itself, and it is important that the whole group follows suit. Sandra commented after the encounter that it was nice to know the priorities and what her role is. Having worked in other laboratories, she is not used to this way of organising things. She has usually done what she considered to be 'basic research', but she is starting to get an idea about '*what she is doing here*' and '*how she contributes to the main project*'. It seems that Gyro Gearloose's ways of organising the research is one that the scientists have to get used to – that they should not '*get caught up in the knowledge thing*' and should instead focus on how to develop technologies. In comparison, the organisation of research has a different emphasis at Curious George.

Here, the research is not organised in relation to a specific end product in the same way, even though many of their grants have specific goals. However, these goals are not the scientists' primary concern, as one of the professors puts it:

'[…] *our basic essence is science, and then we have to – in order to attract funding – be able to direct it in a direction where it has some kind of relevance for society and those who pay the funding and that is fair enough. But we have to realise that we are fundamentally interested in basic science.*'

While I am not sure that everyone at Curious George would agree that they are '*fundamentally interested in basic science*', the quote does resemble some of the opinions that I often met at Curious George: They have some specific interests they want to pursue, which they then have to twist a bit in order to get the funding needed (see also previous chapter 8). This does not mean that they do not want to give something back to society via their science, but rather that they go about contributing in other ways than by a specific, tangible technology. In the following sections, I will describe some of these other ways of living up to this responsibility.

The scientists at Curious George see themselves as having a big responsibility in relation to choosing which research projects to work on and where to apply for funding. The PI, Canute, explains that they take great care in choosing some research topics and calls for applications over others:

'*We don't even bother with those* [call for applications] *that only range from one to two years and where it's all about a fancy, new concept. Because nothing is going to* *come out of it that anyone can use. It's the long stretch and the high quality that counts. That's where we can make a contribution that matters.'*

According to Canute, one thing that is important is not to be impressed by new, '*fancy concepts*', but rather to be more aware of how you can contribute with solid research of a high quality. Therefore, the lab's strategy is to get their hands on that kind of money, and they seldom apply for anything else. In the same interview, I, in reply to what he says, mention an instance, where the lab actually has gone for short-term grants and where the potential for big findings is limited. Canute replies that he thinks these grants are sometimes necessary in order to keep the lab going. But he believes that this happens very infrequently, max two times the last ten years. With this reply he shifts position in his argumentation: Where at first he explains that they look for opportunities to do excellent research, which somebody could '*use*', he then also admits that they, at times, have to settle for less in order to maintain the lab as a viable economic entity. Thereby, he shifts mode and argues as we have seen in Oikos. But he does not like that he has to settle for '*fancy new concepts*', which are, in his opinion, hollow and useless. They want to contribute with something useful and at Curious George useful means quality or 'research excellence'.

For the scientists at Curious George, quality is a key word. Their work is all about producing very sound, new science, which has potential applications. They see it as their key task to do what Clark, one of the seniors, calls '*the hard, tedious work*', which lays the groundwork for future applications. And they often connect the need for high quality work with the responsibility they believe they have to provide sound (basis for) technologies to the public.

Besides sorting out short-term projects that they consider lacking in seriousness, Curious George also works on a range of projects where they believe they improve the livelihood of people, without giving up the principle of high-quality research. As the lab is, in one way or another, working with plants, they have for many years sought to improve crops for developing countries by genetically modifying them. In several newspaper and Internet articles, Canute explains how his work on improving the cassava plant in order to help with hunger problems in Africa is what really prompted his getting into plant chemistry research. But this is not only Canute's research project. Several scientists have been involved in the project over the years. One of them, Linda, is collaborating with plant breeders in Nigeria and other places where cassava is an important basic nutrition. Funding for the cassava project comes, for the time being, from the state's development aid. In the following, Linda speaks about what she considers to be her responsibility in relation to that research project:

Me: *'So what you're telling me is that you have a responsibility to return the knowledge that you develop about a plant to those who are actually harvesting it?'*
Linda: *'Yes, or, what I'm saying is that there are some work stations in Africa that we deliver knowledge to, because we can't get further – and that wouldn't make sense anyway – but in some way, we need to return it* [the knowledge or technology] *to the place where it* [the plant] *is harvested because it is a tropical plant. And we cannot escape that responsibility, but it demands two parts: both that we hand over the knowledge and that they [the plant breeders] hand over their knowledge. When the State Aid* […] *granted us the money, they also delegated some form of responsibility that the knowledge will end up where it is used, and that is not* [the government's] *responsibility, that is ours, and the government doesn't have to interfere.'*

What I find interesting in relation to this quote is how they tie their relationship to return knowledge to the users with their links to the state. While '*the government does not have to interfere*' with scientists' work, it is not due to an imagined, institutional demarcation of science from society as we saw in Vocation. They do not exclude the government *because* 'society' in general should be kept out of the project in order to secure objectivity; rather, society – here represented by farmers in Africa – is the reason that the research is going on in the first place. In Linda's words, it is because the responsibility between different (public) actors has been settled and 'delegated' such that the government collects money through taxation and then allocates a portion to State Aid, of which State Aid then grants a portion to research in crops in developing countries, and those scientists who have been granted the money then have the responsibility to co-operate with local plant breeders. And the scientists acknowledge this responsibility and work with it without question.

Some might question whether this is 'doing research' as the heading indicated. This 'research' also includes co-operation with plant breeders, traveling to different countries and knowledge exchanges between scientists and plant breeders. But I choose to call all of it 'research', because the scientists do not themselves make any kind of boundary work between the laboratory work and the knowledge exchanges. We have seen them do that in other situations, where only strict laboratory work is seen as 'scientific' (see 8.3.1) and the rest is 'politics'. But in relation to the work with the cassava plant, these boundaries are not in place and the diverse activities are all part of the research process.

This reflection about whether it is 'research' is also tied to the choice of 'conduct' as a way to look at agency. With the use of this concept also follows a focus on *the way* people behave instead of on a single incident (see also theory 3.3.3). So by accounting for the co-operation between State Aid, Curious George and African plant breeders, I describe *the way* the scientists do research. I would only separate the experimental part from the collaborative part if the scientists actively separated them themselves. But in this case – and in the mode of Citizenship – they do not. The same is the case with the next example from Curious George.

Some of the scientists at Curious George are also working with 'open source'.[39] The biotech area is known for its many patents and for the enormous power of the agricultural and medical industries in this area. However, some of the scientists at Curious George actively refrain from taking patents because they want knowledge to be publicly accessible, and they see open source as a path to fair and more effective knowledge building. This is not conduct that everyone follows and it is not official (nor in opposition to) lab policy. In the previous chapter, Oikos, I actually describe the opposite: how some of the employees value patents and how they take patents for the sake of their organisation and hope to receive personal recognition in return. But in this mode, patents are frowned upon at Curious George. Other scientists working with synthetic biology, where open source and public 'biobanks' are popular, have inspired some of the younger scientists from Curious George. Over a dinner among colleagues, Josh even asserts that synthetic biology is about having a specific 'attitude':

'I mean, I've given it a lot of thought lately, and I talked to Miriam [a colleague] *about it as well, and I also want to state something about it in the introduction to my dissertation: synthetic biology is not a discipline per se, it's more of an attitude to doing research, where you have certain ideas about how you approach it; like you're for "open source" and for "interdisciplinarity" and for "applied research". But, I mean, it's not specialised enough and the knowledge is not deep enough for it to be a discipline.' (from memory)*

[39] The term 'open source' refers to something that can be modified because its design is publicly accessible. While it originated in the context of computer software development, the term now covers a social movement that tries to promote publicly accessible knowledge and technologies in many areas, including synthetic biology. There is much debate about open source approaches and synthetic biology. See for instance: http://www.nature.com/news/synthetic-biology-cultural-divide-1.15149

In the previous chapter, Oikos, I describe how the fact that the scientists at Curious George operate under the heading of 'Synthetic Biology' does not seem to matter in their daily laboratory work, but it matters a lot when they are outside the laboratory: when they attend conferences, when the communication officer is preparing a debate piece, or when their PIs go to Brussels to lobby for funding. But when the modes shift, synthetic biology becomes important in another way. In Josh's quote, doing synthetic biology becomes a political statement that implies that some ways of conducting science are better than others. And some of the scientists seem to have embraced these ways of doing their job: They go to open source case competitions, they refuse to take patents themselves, they collaborate with scientists with similar points of view, they refuse to work with specific companies, e.g. Monsanto[40], and so on. Along the same line of thought, the lab has initiated co-operation with a local Do it Yourself bio-lab.[41] They have done this, in the words of Canute,

'To be inspired by their creative spirit, but also to make sure that they are not developing something that could potentially be dangerous.'

Through their choice of collaborators and ways of handling their knowledge, Curious George are making statements about what role they believe biotech should have in society. They look at 'Synthetic Biology' as a driver for more openness in science and science as something that is publicly available and not owned by big business. But as I mentioned earlier, this is not something that all of the scientists at Curious George are aware of. In fact, I would go as far as to say that some of them have little idea of the social movements that are bubbling away in their midst. No one seems to be against it, on the other hand, but whether they include it in their own research is

[40] Monsanto is a multinational agricultural company that has been under severe criticism for creating social and environmental damage due to their genetically modified crops and aggressive business strategies.
[41] DIYbio is a global social movement where people work from the belief that *'biotechnology and greater public understanding about it has the potential to benefit everyone'* (www.diybio.com). Under that headline, various local public laboratories have been set up, where ordinary citizens can experiment with biotech, science's role in society is discussed and various (more or less rebellious) activist activities are organised.

another question. The work with these perspectives is based on the individual scientists' interest and not something that is official 'lab politic' or a part of a carefully planned 'organizational culture'. That being said, it seems that those juniors, who engage in these matters, are considered as great assets for the lab and someone the lab will probably invest in – to use an expression from Oikos.

This description of the ways in which Curious George conducts 'Responsible Science' in this mode is quite different from the description of Gyro Gearloose. In my interpretation, this is due to the different research aims of the two labs. At Curious George, there are many different ways of conducting responsible research: not taking patents, working with plant breeders, making high quality science, and so on. At Gyro Gearloose, they mainly do it in one way: to produce the best medical technologies for the people. At Gyro Gearloose, it seems somewhat easier to align the research task with the responsibility of contributing to the surrounding society. This is, in my view, due to two conditions. First, the knowledge they produce is always tied up to a specific, tangible technology where they are able to imagine who will benefit, and they are able to include the considerations of this imagined end-user into the design of their technology. Second, they work in teams on projects (albeit the same person can be involved in more than one project) where they have a specific role in taking this specific technology forward, usually due to their disciplinary background. In that way, they are better suited to explaining the way they, as individuals and as a lab, contribute to society. At Curious George, on the other hand, they are not '*working backwards*' from an ideal solution and to the research problem. As they focus on '*excellent basic research*', they cannot in the same specific way tie their research to particular problems in society. Despite this, they still work under broad headlines such as 'making cheap and sustainable medicine'. They also want to take the responsibility for giving something back to society seriously, but they do so from other parameters and in other ways. They refuse to spend public money on 'bad science', and they refuse to apply for grants that are too unambitious to respond to. Furthermore, they have many haphazard principles about whom they should work for and in which ways: They would rather work for the government's development aid than for Monsanto and they want their work to be open source.

9.1.4 SUMMING UP

In this section, I have illustrated different forms of conduct in the two labs that form part of the mode Citizenship. I have shown that the scientists engage in a range of activities: '*speaking about science*', '*listening to the public*' and '*doing research*' in ways which give clear and concrete expression to their desire to live up to the role they believe the surrounding society has of them; namely, that they have an obligation to share their knowledge and technologies, not do any harm and spend public money well. They try to live up to these responsibilities in different ways: Both by informing their (fellow) citizens about the science they do; by 'listening to the public' and incorporating the public's ideas into their research; and by 'doing research' in ways where they take their responsibility into account.

While the three forms of conduct are pervasive in the two labs, there are also some differences that I mostly attribute to the differences between what kind of science the two labs do and what they want to get from doing it. Gyro Gearloose is doing 'translational research' and focuses on developing specific solutions to some of the problems they observe in society. Therefore, the ways they conduct themselves are closely linked to this identity as being a 'translational researcher', and their area of research is always coincident with what they want to share with the surrounding society. The challenge for them is therefore in how to push their technologies 'out' into society. Curious George has a more diverse research agenda, where they try to develop both high quality basic research as well as organic products to use in medicine and agriculture. This research identity that is both about being a 'basic scientist' but also about securing the basis for sound (plant) technologies for future use makes the ways of engaging with society more diverse and not as shared among all the members of the lab, as it was at Gyro Gearloose. Some of the engagements are closely tied to their technologies, such as the GM cassava plant. Others are about attitudes related to how science should be conducted, that is, in an open source environment and always of the highest possible quality.

In these sections, I have shown that the scientists believe they have responsibilities they need to live up to, and I have followed their attempts so to do. In the next section, 'Justifications', I will go one step further and look even more closely at 'why' the scientists believe they have this responsibility. In this way, I will attempt to connect their conduct with some specific types of shared worldviews about science's role in society, which I found to be pervasive in this mode.

9.2 JUSTIFICATIONS

In this section, I will account for how the scientists justify the responsibility to share knowledge and technologies with the surrounding society. The section about conduct has already stated some of the scientists' reasons for why they had this responsibility. They said they need to listen to the public's recommendations in order to align their research with what the public wants; they said the public has a legitimate right to intervene in their research; they see the public as a group of individuals of whom some may be of assistance in the research process; and so on. However, this section about justifications will, as the ones in the two previous chapters, go more in depth with the reasoning behind the feeling of responsibility. My argument is that the mode of Citizenship performs a specific version of the role of science in society and the scientists' duties follow from this. This worldview is not – as I will also show – very unique to the scientists. They draw on known government rationalities on the ideal organisation of the public sector when justifying their conduct. This is an observation already made by Law, when he argues that research managers use well-known ‘grand’ narratives about organisation such as ‘administration’, ‘vocation’ or ‘enterprise’ in their daily work (Law 2009).

As in the previous sections, I will first show that the scientists have some immediate standard comments about why they have this responsibility. In this mode, it is all about ‘tax’ and how they are dependent on public investment and therefore have to return something to society. However, I will also demonstrate that this neat explanation is also perhaps a little too neat and in fact works as a ‘proxy’ for some more elaborate ideas about the role of modern science in contemporary democracies.

9.2.1 TAX AND THE ‘DEMOCRATIC RESPONSIBILITY’

I’m sitting next to Sandra, who is preparing an experiment for tomorrow. She is in a hurry because she is late picking up her kid from kindergarten – ‘I always am’ she complains, smiling. She puts some samples in the oven and rapidly adjusts the time and heat on the keyboard next to it. Then she hurries on. At some point during her tasks, she looks up at me and says, ‘If you have any ideas, you are of course more than welcome to contribute’, ‘Nah,’ I reply, ‘I mean… I would like to, but this is not really my field.’ Sandra then offers, without irony, ‘Oh, but I mean, if you have any great ideas as to how to cure cancer, they are very welcome.’ I think a bit and say, ‘Well, honestly, not that many, but I guess I can tell you something about why the public responds to new ideas about cancer treatments the way they do.’ ‘Oh,’ she says, as she takes off her lab coat, ‘I understand why they are sceptical. They pour so much money into cancer research and still nothing is happening.’

I often encountered the idea among scientists that the public was sceptical due to so much money being spent on science, but with slim returns. Sandra’s example with cancer research is just one of them. This seems to be a general idea among the scientists about how the public looks at science. They therefore consider it their responsibility to engage and talk about '*the good things we are doing*', as several of them put it. Otherwise, they are afraid that they will loose public support. When I inquired about why their relationship to the public mattered, they usually answered that with tax funding followed a responsibility to do something that mattered to the people, who pay them. As this quote from an interview with Henry from Gyro Gearloose illustrates:

'To ask society to pay taxes to fund your research and then withdrawing from the problems that they are facing is not responsible, I don't think.'

The scientists' understanding seems to be that their work is based on public money and therefore they have a responsibility to show that something valuable is coming out of the public's investments. During an interview, Linda from Curious George even called it '*a failure*' if they do not consider the world outside the university as an important stakeholder in their work, as tax funding is the basis for their existence:

'So, I'm actually saying that, in one way or another – both in education and in research – society is an important actor for us. And if we, as a university, say, "we have nothing to do with you people on the other side", then we have failed completely […] *because it is tax money that sustains us.'*

I heard this argument again and again, irrespective of whether I was in the Danish or American lab. Every time I asked directly about why engagement mattered, tax funding would be the answer. Here, it is Christian from Curious George who ponders the responsibilities in relation to his own PhD project:

'I think it would be totally crazy to distance myself from society. I know that there are people here [at the lab] *who isolate themselves a lot, but I also believe that the taxpayers' money is wasted on your education if the topic is only interesting to yourself.'*

I never really encountered those people who were isolated – but perhaps they were isolated to such a degree that I did not even notice them in the lab. Those who did not isolate themselves and whom I met seemed to share Christian's opinion. Taxes, tax money, tax income, tax funding, the public investments; these expressions came up when I asked why they thought it so important to engage with citizens in different sorts of ways. It could also be expressed a little differently, as when I asked Simon whether it was a sort of obligation for scientists to speak about their research, and he answered:

'We do [have that obligation], *as it's the public who pays for what we are doing, and that is the democratic responsibility that we have.'*

In his opinion, they have a '*democratic responsibility*' to account for their work. Not necessarily to produce something useful for the public, but to be able to account for their actions as other organisations that are government funded must. So the argument, while still pointing to tax as the important factor, is a little different than the others. While they underline the importance of the subject being relevant in other contexts than the scientific, Simon connects his idea of responsibility with a more general socio-political norm that public organisations should be economically transparent.

So an obligation to return something to those that fund and to be transparent are the immediate answers to the question of why the scientists believe they have a responsibility to return 'useful' knowledge and technologies to society. These answers make sense seen in the light of several recent university reforms across the Western world, which have had as one of their aims to '*increase accountability to the tax-payer*' by encouraging, among other initiatives, '*better responsiveness to user-groups*' (Shore and Wright 2000: 69). As I touched upon in the theory section, this kind of change in public science governance shares similarities with changes in norms and techniques of governance across much of the public sector, where the focus on accountability to users (taxpayers included) and direct participation replace ideals about professional ethics (DeLeon 1997). In that sense, the answers do make sense, as they resonate very well with current policy tendencies.

But as in the other modes of responsibility, I get the impression that this immediate explanation is somehow a reduction or simplification of some more complex ideas about science's role in society. Although I am sure that their explicit justifications are plausible and well motivated, I also think that it is insufficient to adequately describe the reasons that the scientists feel a responsibility to engage with society. I will argue that 'tax' works as a proxy for more complex and abstract conceptions of science's relationship to society – in the same way as the scientists also used proxies for 'the truth' in the mode of Vocation.

9.2.2 PUBLIC – AS OPPOSED TO CORPORATE

Taxes aside, one of the most common ways that the public scientists justify their responsibility to contribute something to society is by comparing themselves to the private sector. They explained to me that they are different (and better) because they are part of something that is more than profit optimisation. They believe that they somehow have a responsibility to do 'more', as I will show in the following.

The idea that '*society pays, we deliver'* is widespread among the scientists, despite the fact that it is far from always public money that funds their work. As we saw in Oikos, private foundations and companies also play a role in paying for the research projects (8.1.1). Altogether, the connections and boundaries between private and public are much more complex than the manner in which they are talked about by the scientists, a theme that Gorm-Hansen (2011), among others, has described in depth.

However, I never got the impression that the scientists feel the same responsibility toward their private benefactors as they do toward the public. Sure, often the scientists want to collaborate with industry and they have strategic reasons for aligning themselves with the research themes that are popular among big private funders and corporations. If they collaborate, they do feel obliged to deliver what they have promised in the mutual contract, but not much more. I never heard them say things like '*It's the private sector that funds us, so of course we should give something back to them*', the way they did when we talked about their relations to '*society*' (an entity, which, as a logical consequence must be 'non-private'). If they ever made a remark about how they owed something to their private funders, it was strictly tied to the contract or a personal relationship with the funder. My general impression is that they consider private foundations and private companies more as equals or as business partners. I would go as far as to argue that from my observations, their perception of their relationship with the private sector look exactly like the straightforward way they describe their relationship with society: They pay, we give something back. In comparison to this, my impression is that the feeling of obligation toward the public is far more complex and about more than '*tax*'. When the scientists talk about collaborations with industry, they are eager to disassociate themselves from connections that are '*too close*' – and they often compare their own labs to the private sector's labs in ways that favour public laboratories. When I ask Henry, one of the PIs at Gyro Gearloose, why he does not work in industry, he answers:

'I could never do what I'm doing here in a private laboratory. This will allow me to envision big inventions, you cannot do that when it's all about the bottom line.'

In many of the scientists' opinions, companies are narrow-minded and do not have the resources to think in terms of great inventions or big scale problem-solving in the way that public laboratories are able to. They have to be more concerned about '*the bottom line*'. Many of the PhD students express the same sentiments; they are not that eager to move to industry after they have graduated, even though they know that jobs are available and the salaries are higher. As Miriam says:

'I'm sure that I would not be allowed to do the things I do here in a private company. I mean, in public laboratories you can spend time exploring and using your imagination to come up with solutions that are really creative and great. I don't believe I could do that in, for instance [name of company]. *It would all be about the bottom line and small improvements of existing products. It would take me years to be in a position where I would be allowed to do the things I already do here as a student.'*

Public laboratories invent great things, are creative and come up with impressive 'solutions', whereas private laboratories focus so much on the bottom line and have such limited funds that they cannot do the same. Shapin (2009) actually makes the opposite conclusion in his book on the development of the scientific profession. He explains that public research has become so exposed to competition that big visions and ventures into the unknown have become difficult. On the other hand, private science has begun to invest heavily in their basic research and long-term positions, and a steady inflow of resources make the scientists more 'free' to pursue the goals they favour (Shapin 2009: 328). My empirical material is ambivalent in this regard. As I also mentioned in Oikos, I followed a couple of the PhD students who took a course on 'biobusiness', which took place at Copenhagen Business School – my own university. Prominent managers from the pharmaceutical industry conducted part of the teaching. The students from Curious George were not impressed by the content and the prospects of doing 'biobusiness'. As one of them commented:

'I mean that guy from [name of company] *basically said it: They can't really make money on new products, so they just have to optimise old ones. I mean, he talked about making treatment an 'event' for the patient as their new strategy. Damn me if I ventured into science to make wellness.'*

So on the one hand, the scientists tell me that they have their worst suspicions confirmed when they deal with private corporations; they are all about profit and not about solving big problems. Pharmaceutical companies, for instance, do not invent revolutionary treatments, according to the scientists.

During my fieldwork, one of the PIs got a part-time job as a research director at a multinational company. His experience was that public science has become so optimised and output-focused due to exposure to competition that public science is much more competitive and efficient than the company's science department. He therefore had to fire many of the employees and renew their strategy. On the other hand, I also encountered many more well-functioning collaborations between the public and private sectors, where companies are partly funding PhD students or are delivering much appreciated data to journal papers from experiments. Shapin's (2009) point about public and private science's exchange of roles over the last fifty years may be relevant to some extent. My observations are mixed. What I do find interesting is that a myth about public science persists. Many of the scientists use the distinction between public and private to justify a certain special responsibility that they have as *public* scientists: The responsibility to make grand solutions for society's problems – including solutions to problems that society has not even experienced yet.

Public science is supposed to change the world; they have that responsibility on their shoulders, and that distinguishes them from other kinds of scientists. But while they do favour public research over private, they also seem to have a much more straightforward perception of their relationship to the private sector compared with their relationship to the public. As my main concern is the study of public science, I will not go into further detail with that relationship. I will, however, continue my description of how the scientists see their role as being a part of an imagined 'public' sphere.

9.2.3 A JOB WITH MEANING

The scientists tell me all these things with a certain pride in their voice; they like that they work for public laboratories and are part of something they believe actually makes a positive difference for society – and they justify their choice of job with this positive difference. But the justifications in Citizenship are multi-faceted, and we need to look at further reasoning in order to grasp where they root this responsibility to share their knowledge and technologies. Overall, I get the impression that the scientists share a sense of belonging to public society in this mode. This is both different from Vocation, where they express that society's interests do not affect them, and Oikos, where they mainly belong to an organisation. The first example from 'Not Just a Job' is from an interview with Simon from Curious George:

'It [his job satisfaction] *is about whether I can imagine that there is a product at some point which can be used by someone in a reasonable manner. It doesn't have to be in relation to industrial production, it might as well be… I mean, I did some research where we studied how traditional medicine is used, if it is usable or not. And that kind of knowledge is not industrial per se, but it is very usable for those people who live close by these plants. So it's a little more about that the knowledge which I build up is used by someone else as well.'*

In this quote, Simon establishes a kind of link between his work and the people who will potentially benefit from it. It is important for him to do research that has an impact on other people's lives. In many other encounters, Simon was very preoccupied with strategic work, potentials for industrial collaboration and personal benefits, such as profit and professional acknowledgement from his co-workers. But in this quote he disassociates himself from the importance of industrial production and strategies. Instead, he just talks about the importance of making useful knowledge in general and how that is significant for finding some kind of satisfaction with his job. This search for meaning is shared between Curious George and Gyro Gearloose. In the following, it is Alan from Gyro Gearloose and I who discuss during one of the STIR interviews:

Me: *'So you tell me that you want to create a successful product; what is a "successful product" to you then?'*

Alan: *'Yeah, so in my opinion creating a successful product would be creating a product in which it's probably safer, more efficient and less… ahh more inexpensive for the consumer and for the public. So… and I think that 's how all research should be done, that should be the end goal of how all medical research is happening, or you know, people that are designing drugs. It should be safer, more efficient and cheaper… Otherwise it's just a job, you know.'*

Me: *'Who cares about you trying to create cheaper, safer and… what was it… more efficient treatments?'*

Alan: *'Uh... my boss, I guess, and fellow scientists, and in the end the consumer and the public, of course.'*

Several points from this exchange are interesting in relation to how the scientists consider their role in society. First and foremost, it is interesting that he sees it as his goal to '*create a successful product*' (he is working on a cure against acne). In other modes, a paper might have been published or he might have received a postdoc offer. His answer is similar to those I presented in the section on conduct, called '*Simply doing research*'. The scientists at Gyro Gearloose have a responsibility to deliver something they think people need and he tries – and wants to – live up to this responsibility. He even believes that '*that should be the end goal of how all medical research is happening…*'. So Alan identifies being a medical scientist with creating '*safer, more efficient and more inexpensive*' treatments. And then he adds, '*otherwise it's just a job, you know'*. My interpretation of that last addition to Alan's explanation is that the effort to improve something is what makes the job interesting and meaningful – and thereby, apparently, more than a job.

Interestingly, this consideration is very different from what I presented in Vocation. In that mode, what makes the scientists' work special – and precisely more than an ordinary job – is that the only purpose is the expansion of mankind's knowledge and a satiating of the scientists' own curiosity. So once again, it becomes clear how the purpose of the scientific profession changes depending on the mode. Where the mode of Vocation stressed disinterestedness as part of the condition for doing the job, Citizenship turns things around, as clear outcomes are what make the job meaningful.

A second point that is interesting in relation to the exchange is that the '*consumer*' – despite Alan's wish to help – seems quite abstract for him. The consumer is mentioned after the boss and the scientific community. The latter two are more immediately thought of as stakeholders. The consumer, on the other hand, though still present, is not the figure that Alan is most concerned with at the moment. I often met this kind of reasoning during my fieldwork. The scientists have an idea of some kind of public figure out there, someone for whose sake they work, but who is otherwise not entirely present for them. I encountered the idea of the '*consumer*', an end-user, who could be helped by, or be critical of, a product; '*soldier*' – a professional working for the government – returning from destinations far away and needing medical aid; '*patient*', who needs a special treatment; '*the citizen*' or '*the public*', who either has general needs, such as cheaper medicine, or legitimate worries about the outcome of the scientists' experiments; and '*children*' or '*descendants*', who want to live in a future better than the present. The figure they refer to seems to be dependent on the type of technology or knowledge on which they are working at the moment. If they are working on the development of diagnostic devices, they imagine that it needs both approval from the public authorities and a contract with a medical company, after which it will be sold on the market to the individual '*consumer*'. If they work on plant compounds or nano-discs, they imagine a '*citizen*' or a '*public*' who/which may have legitimate concerns or uses for that knowledge, despite the scientists themselves not being sure about its possible future applications. If they work on cancer treatments, they imagine how introducing more efficient treatments could optimise the healthcare system. So depending on the figure they imagine, they try in many instances to design their knowledge or technology so that it will be useful – or at least not harmful – to the imagined figure. That is a goal for Alan from the previous story as well.

Strangely, it does not seem that important which of these figures they portray in relation to their obligations: '*Citizens*', '*patients*' and '*consumers*' seemed interchangeable. They are all legitimate in terms of someone whom it is deemed worth going to work for. It is difficult for me to understand how it could be so. I have just illustrated how they very clearly distinguish between working for private or public science, but then, on the other hand, they do not (to my knowledge) seem to distinguish between making treatments for soldiers returning from war elsewhere and cancer patients. My best guess is that, when I consider this through the lens of 'Citizenship', all these figures make up well-known archetypes in an everyday imagining of society, and that the scientists therefore consider them as legitimate benefactors of the help they provide as '*public scientists*'; in the same way that it is a teacher's duty to help school children and a doctor's responsibility to treat patients. The scientists just have different stakeholders, depending on the project.

But the scientists often have trouble connecting these rather abstract figures with their daily, tangible work tasks. And who could blame them? These figures seem quite far away from test tubes, enzymes or nano-disks, all of which are their immediate concern. They cannot relate every single experiment to the bigger picture. But they still have these blurry, abstract figures in mind when they justify what they are doing; remember: '*all medical research should be done with the goal of creating cheaper, more efficient and more inexpensive products.*' This is again different from Vocation, where ideas about output and their implications for society are not considered important – almost the contrary: Too much focus on the outcome could lead to ethical lapses. In one of my interviews at Curious George, Walter reflects on his motives for doing research. Walter is usually very focused on classic scientific virtues, as I described them in Vocation, and he tends to find other aspects slightly irrelevant:

Me: *'So what you are telling me is that you believe it's fair that, as the taxpayers pay for your work, you have to deliver something back to them. But would it be more interesting for you if you didn't have this obligation?'*
Walter: *'No, actually I don't think so. As a matter of fact, I think that it's very interesting for your own motivation, if you can see that your work can be used for something besides the pure academic exercise.'*

So even those scientists who are more focused on academic virtues believe that their work '*motivation*' comes from something that is beyond the '*pure academic exercise*' – even if they are afraid, as we saw in Vocation, that it could interfere with their professional judgement. In these situations, they often resort to boundary work (Gieryn 1983). They do so in order to simultaneously embrace the responsibility for being objective with the responsibility for the outcomes of their research. Walter, for instance, thinks it is okay that his '*motivation*' for working on specific projects is related to his obligation to the taxpayers, whereas I am sure he would say otherwise about the specific experiments. Clark from Curious George constructs a similar boundary between what gives you '*satisfaction*' and what is of '*interest*':

'[…] [t]*his* [to know that you work on developing better treatments for diseases] *gives you a lot of satisfaction and of course we work towards the high goal of producing drugs for humans in* […] *a cheap and sustainable way. On the way we often forget that; because the plants produce these compounds for a very specific purpose and if we can address that question it is of course not of any interest for the drug production but exclusive for plant biologists.'*

So there are those aspects of work that give '*satisfaction*' and can be considered a '*high goal*', namely, that of producing cheap and sustainable drugs. This aspect could also interest people outside the scientific community. But then there are the '*compounds*' and their '*specific purpose'*. These are '*exclusively*' interesting for the scientific community itself, the '*plant biologists*'. In their everyday work lives, the scientists sometimes claim that they do not think that much about the '*purpose*', but more about the technicalities, as in how and why plants produce '*compounds'*. In my interpretation, this is because they try to separate a part of their work, which they can call '*scientific*', from the rest of their work, which they apparently do not think of as '*scientific*'. But despite the fact that they do not consider the goals – for instance, producing new medicine – as '*scientific*', they do consider it an important part of their work because it gives motivation and meaning. Otherwise, their work would be '*just a job'*.

In this section I have illustrated other aspects of the justification for why scientists need to share their knowledge and technologies with society. I have examined how it is important for the scientists to find some kind of meaning in their job. They do this by relating what they are doing to abstract figures whom they believe they should help, but who are quite far away from their everyday concerns. Interestingly, though, this understanding of 'meaning' was not seen as significant in either Vocation or Oikos. In Vocation, what rendered the job 'meaningful' was almost the opposite of what gives it meaning in this mode; namely, that there is *no* purpose in what the scientists are doing and that they are precisely not engaged in '*society*'. In Oikos, 'meaning' really signified the will to make the organisation survive, and the fight for this gives a sense of purpose (see 8.2.1). So the question remains: What kind of worldview is this sense of meaning derived from? My first, tentative answer to this question is based on an interpretation of the three previous observations. First, the distance the scientists maintain to private science; second, the commitment to the idea of an abstract figure who gives them motivation and meaning in their job; and third, how this figure is at the same time very distant in their everyday work.

Based on these observations together, my interpretation is that the scientists see themselves as part of an 'imagined community', which possesses specific characteristics (Anderson 2006)[42]. An '*imagined community*' is a shared conception of a large group (so large that you do not have everyday contact with them), who share a sense of belonging, based on adherence to both the same limits of the community and a feeling of fraternity, despite differences in class, gender and social standing (Anderson 2006: 7).

In Citizenship, the scientists imagine a community in which there are different professional roles working for a public (as opposed to the private sector and the market) society. These roles have specialised duties. The scientists' roles in this imagined society are to somehow help with the solving of society's big problems, such as cancer or hunger. But they are still relatively free to decide which problems to solve and how – they therefore do not care that much whether they are working for soldiers or patients. As long as they are recognised by the scientists as fellow members of this imagined community, these figures create a sense of meaning. In the next section, I will further elaborate on the scientists' conceptions of being part of an imagined, public community.

9.2.4 AN ORDINARY SPECIALISED PUBLIC INSTITUTION

[42] In the first edition of his book 'Imagined Communities: Reflections on the Origin and Spread of Nationalism' from 1983, Anderson developed the concept to describe national states and feelings of nationality. The term has later been used in many different contexts, for instance 'Facebook', or the local football team and its fans (and indeed opponents) can be described as imagined communities. My use of the term similarly twists the concept, as I describe how a very small population (one lab, two labs) share an idea about how society in general is composed, how they belong to that society, and, thereby, what their role in that society is.

I'm interviewing Annie from Gyro Gearloose. We are talking about her responsibilities as a PhD student and as a scientist and how they differ from other professions' responsibilities.
I ask her how her responsibilities differ from those of, for instance, lab technicians or security professionals:

Annie: *'They don't, actually.'*
Me: *'Ok, why not?'*
Annie: *'Because we all contribute in the same way* […].*'*
Me: *'Could you give an example of that?'*
Annie: *'So, as I mentioned for my thesis, we have a certain number of goals that have been approved, and I have to work towards meeting those. So I mainly just contribute towards those. But maybe security personnel have the same sort of goals outlined for their job, you know, and they contribute towards completing those. So it isn't very different, except maybe the impact of what we do is different.'*
Me: *'Ok, how so?'*
Annie: *'For example, again in that same example that I gave about the security personnel: What they do is in real time. So their job requires that what they do in that moment is important, and affects maybe the eight hours that they spend. But through this program and through doing this work, the opportunity that we get is to make or create something that is better at diagnosing* […]. *My work is done, you know, as long as I meet my goals* […]. *And I leave here. But then it gets taken and applied in the industry, and it goes into practice, and it actually affects people's diagnoses.'*

This exchange with Annie is a very fine example of the imagined community I described in the previous section. She is part of a community where she has a special role in developing '*something that is better at diagnosing*' in the same way that the security professionals probably also have some special '*goals*' which they work to accomplish. Both professions are united in having duties for the community, but they are specialised in different areas and therefore their duties are different. This focus on being together in having duties, but specialised in specific duties – and how they should be conducted – are very typical for how the scientists justify their responsibility in Citizenship. Annie repeats that worldview at the end of the quote, where she describes an innovation process that ends with the sharing of diagnostic methods with 'people', and where all the steps in that process are taken care of by specialised parts of society: her as a scientist, industry, and the medical sector.

I'm following Simon from Curious George around for the day. He is visiting one of the local high schools to talk about his research. We arrive and are directed to a large, semi-dark auditorium, where more than 150 students are assembled. It's all the students who specialise in science, we are told by one of their teachers. It's Friday morning and most of them look tired and not particularly interested. Some of them have fallen asleep and are slumped over the tables. During Simon's presentation, I can see the door open and close constantly as more students enter. Simon explains that enzymes from the plant "poisonous carrot" can be used for cancer treatment if they are transplanted to moss. And he spices up the talk by explaining that the same technique can also be used for developing pheromones for perfumes. The high school teachers ask polite questions and try to engage their students in the discussions as well but without much success.

Back at the department, Simon comments: 'It's impossible when they are that many, it's early in the day and they are seated in a dark auditorium. It would be strange if they were engaged! And there were a lot of things I did not do because of the conditions. I had brought the moss that smells of perfume, for instance, but I could just feel that it would be in vain to send it around. But when I'm visiting a small amount of students, and I […] *bring that moss… Perhaps they return to their mom* *and dad and they tell them about it and, "God, where did that come from" and, you know, "What!" and "Wow, do they do things like that as well?" You know, to make the public aware of what we are actually running around doing, that's perhaps the most important aspect of participating in the Science Festival.'*

The visit is part of the nationwide annual 'Science Festival', where science is celebrated at primary and high schools across the nation. There are many different activities for children throughout the week. One of the events is a visit from a scientist who talks about his or her research. Simon participates every year; he thinks it is important that the scientists are visible at all stages of education in order to attract new students to plant chemistry. But he also hopes that the stories about what he and his colleagues do will reach a broader public through the younger members of society. As the above story indicates, he believes it is important that the public know about what scientists are doing. In another encounter, he explains that he finds this aspect important, as he and the other scientists are part of a public sector along with other institutions that also have an obligation to serve the public in the best and cheapest way possible.

This is an argument I often hear among the scientists: They say that they are obliged to tell the public about their research because they belong to public institutions, and the public has a legitimate right to know how tax money is spent and what the scientists are working on. In that way, it becomes the act of accounting for their activities that is central for their conception of sharing their knowledge and technologies. In a Danish context, I believe that this feeling of responsibility is perhaps reinforced by the recent so-called Penkowa-case ('Penkowa-sagen' in Danish). Milena Penkowa, a renowned brain scientist, was accused and found guilty of serious scientific fraud and the misuse of public funds in 2011 and 2012. The story received huge media attention both in Denmark and internationally. While this recent incident might have provoked the obligation to account for one's work even more in Denmark, I believe that it has deeper roots. Shore and Wright (2004) describe how a series of university reforms have made '*forced accountability'* a part of everyday life for scientists, in that they now spend more time accounting for what they are doing to diverse stakeholders, such as politicians and the public, than actually doing research (Shore and Wright 2000: 71). The focus of their paper is audit technologies, which is not an explicit part of this dissertation. Therefore, I cannot relate specific audit technologies to the obligation to account for daily work life among the scientists, but I can assert that this feeling of obligation to be accountable is indeed present. The scientists connect it with the idea of being part of a public sector together with other institutions, and all the latter should account for their operations and work because, the scientists reason, they owe their existence to the taxpayers. While this idea and the fairness of it are sometimes questioned, as I will show in the section on negotiations it is far from always being the case. In many instances, it is taken for granted and perhaps even celebrated. As Simon remarks:

'In reality, I think the public knows more about the intelligence agencies than they do about us. [...]. *And other public organisations, such as the elementary schools, are discussed a lot.'*

Public research organisations are just one kind of institution among many, and public institutions are obliged to discuss their work with the public. That is just the way it is. This view is also expressed during the seminar I give for the scientists at Curious George as a conclusion to my stay. We discuss the freedom and obligations that come with the job as a scientist:

I try to provoke the scientists by saying that I have observed them becoming really annoyed when 'ignorant' citizens want to tell them what kind of research they should *do and why. One of the scientists raises her hand and says, 'Well, but school teachers get annoyed too if others tell them how to teach.'*

My interpretation of this exchange is that the scientists want to express that in general scientists are quite ordinary when it comes to discussions about professional autonomy. Other workers in public service, such as schoolteachers, feel the same way, but they do not attribute their frustration at loosing professional freedom to an antiquated idea of a lonesome individual in an Ivory Tower. Instead, they continually negotiate how much and in which ways the rest of society can interfere with their work (though, lately, not with great success). My interpretation is that the woman with the raised hand argues that when scientists get annoyed with the public interfering, it is no different to other public professions having to defend themselves against public critique. It is a way of negotiating freedom and duties in public service. In that way, the habit of referring to '*ignorant publics*' and '*facts*' in this battle may be used because it has historically been a legitimate way of setting boundaries for how much the public has a right to interfere with the scientists' professional opinions.

The last two stories above may seem contradictory at first glance. The first quote promotes the idea of public dialogue and the next defends professional autonomy. But in my view, the two stories express the same kind of worldview. Namely, that the lab and public scientists are ordinary members of public service among many other professions. And they all have specialised duties and freedoms. But the scientists' specialised duties and freedoms go hand in hand with the view that the way science and its responsibilities are governed should be compared with the way other public institutions and their responsibilities are governed – for better or for worse. I will discuss this worldview more thoroughly in the section Negotiations. First, I will draw some conclusions on the justifications that I have presented in the previous sections.

9.2.5 SUMMING UP

In the above stories, the scientists look at their work in a completely different way than they do in both Vocation and Oikos. They consider their job as one among many in an imagined public community. They find it reasonable that their work should be subject to public discussion, and they want to join this debate as well – not least to ensure that they do not lose all their professional autonomy. Seen in that way, 'science communication' becomes something very different from the 'deficit model of science communication' (Irwin and Wynne 1996), which is described in Vocation. Here, scientists feel obligated to inform the public about their work and findings; however, they follow that obligation because they want the public to learn and respect scientific facts. These examples show a different case: The communication is imagined as a vital part of negotiations between the scientists, the public and government. By the means of doing science communication a compromise between the public's wishes and the scientists' freedoms can be reached.

9.3 ORGANISATIONAL CONSEQUENCES

The sense of belonging to society also frames the meaning of the scientists' jobs in a way that is different from how it is rendered in Vocation and Oikos. In Vocation, it is the isolation, absorption and curiosity that make the scientists content. In Oikos, most of them seem to struggle to find positive meaning, but they do find a certain satisfaction in actually having done a job that positively advances their own career or the organisation as a whole. In the Citizenship mode, they find '*motivation*' in helping members of a shared society. In that way, all the straightforward justifications based on 'tax' function as proxies for a more abstract sense of being part of an imagined public community along with other public institutions. In their view, the way of governing their duties and responsibilities should be comparable to these other public institutions. That does not mean that 'tax' is irrelevant at all. Tax is a significant part of the scientists' ways of justifying how they do their work, as they consider tax funding the legitimation of the public's right to interfere in decision-making about science. But even if tax is important, there is more to the justification. The scientists believe they can be compared to other public professions in so far as they have a specialised duty to help the public, as well as the freedom of how to help, which comes from the specialisation and expertise they represent. Therefore, they find it legitimate to fight for their autonomy as other public professions do.

The scientists' conception of being part of a public community is very abstract and does not (necessarily) refer to a particular system in a particular. They are, as public scientists, employed by government and with that comes a set of obligations, but their work does not necessarily have to benefit the citizens of their own country. Depending on the task at hand, their idea of the public they work for changes. As scientists, they do not necessarily work for a specific local or national government. Their work does not necessarily have to benefit citizens with whom they share national or regional borders. As we saw in earlier sections, the abstract figures they feel an obligation to share their knowledge with come in many forms. They can be from anywhere in the world: it can be citizens in general, citizens with special needs or special kinds of citizens. But no matter that these figures are quite abstract, sharing their knowledge and technologies with them is a presumed or imagined responsibility in this mode. Despite the fact that their responsibility to share is not tied to a specific state or a specific citizen, their justification for the responsibility shares similarities with some very specific contemporary ideas about how the public sector should be governed.

These ways of considering the job are interesting seen in the light of the demands for 'Responsive Government', as I described in the theory section (2.1.2). In some ways, the scientists do seem to find it legitimate that they are supposed to *deliver* on research goals decided externally and, furthermore, are held accountable for how they spend their time and money. In addition, the acceptance of the idea that the public does not only have the right to express their opinions about scientific developments, but also has the right to, more or less directly, influence the prioritising of research and how it should be conducted further resonates with that rationality.

We saw this, for instance, when they describe how an important part of the public engagement exercises is to report on reasonably well-spent money and tangible results. And it is certainly clear from the way that Henry from Curious George considers it irresponsible to '*ask for money*' and then not deliver, i.e. not living up to conducting the scientific job responsively. On the other hand, the scientists are frustrated by some of the specific manifestations of this demand, for instance: That they have to justify doing science with no immediate application; that they have to make very strategic considerations about the grant applications rhetoric; that they have to spend a considerable amount of time substantiating their whereabouts instead of conducting research; and how very confusing it can be to adapt to the different demands for accountability in specific situations. The responsibility for the public comes with a range of frustrations that are not just a simple wish for general autonomy, but rather points to specific situations where it is difficult and frustrating to live up to being 'responsible' in this way.

I argue that one of the consequences of the responsibility for the public, as the scientists imagine it, is that the scientists' professional role is recast as, in du Gays words '*entrepreneurs*' of publicly decided projects; that is, managing and conducting government-financed projects (2008). But in some specific situations, this is seriously at odds with the responsibility the scientists feel they have for 'the truth' and the view of the scientific community as reasonably trustworthy in governing themselves responsively. Du Gay (2009) explains that bureaucracy's historically-built experience in, and responsibility for, running a state has been eroded in favour of the responsibility for realising the current government's decisions. This has in turn led to an unstable and contradictory range of public reforms in various areas and resulted in a poorly managed public sector. The question is whether we will see similar developments in the domain of science in the coming years. Given that the internal government of science is now heavily supplemented with demands of being accountable to various shifting governments and public opinions, what will happen to the academic system itself, the quality of science and, not least, the well-being of those working under these conditions? Some of the effects that will be described in this chapter such as 'basic science guilt', the shifts from one fashionable theme to another and the tedious strategic work that goes into keeping research meaningful do perhaps point to some of the problems in excessive responsiveness.

9.3.1 FEEL GOOD SCIENCE?

In the previous two chapters, I have demonstrated how time and truth-making shifts depending on the mode. In Vocation, spending a lot of time on experiments and being almost painstakingly patient is of importance because true science takes time to produce. It is all about 'Slow Science'. In Oikos, it is vital to be quick on your feet, trust one's own ability to produce a needed result and publish before others. Fast Science is appreciated.

It is more difficult to see whether Citizenship generates specific understandings of time and facts. The empirical data is less coherent here, and there is simply less talk about it compared with the other modes. Starting with time, I am not able to describe the generation of a specific all-encompassing idea about the nature of time as I have done with the other modes. I cannot see whether the organisation's shared idea of time generally shifts. It is more as if both the conception of infinite time (to do proper research) and time as a scarce resource are still present, and therefore the central conducts of Citizenship are pressed out to the fringes of the scientists' work time, for instance, to the weekends, to the evenings, or as something the scientists have to do on top of other things. In any case, the time spent on these activities still ends up taking more time than expected. As Catherine comments on the responsibility to participate in one of their annual public engagement events:

'Everybody needs to lend a hand at public lab nights, even though you have to leave your other projects, spend a lot of time on it, and half the weekend cleaning up afterwards and you only get two bottles of wine for it.'

This is an understanding of the relation between engagement activities and time that I come across frequently. The scientists feel obliged to participate, they also want to do it in the way I described in the sections on conduct, and it nearly always ends up taking much more of their time than they had expected. The story about Mike – from Vocation – teaching his students that doing proper science communication takes as much time as writing a scientific paper comes to mind. The juniors are not necessarily aware of how much time these activities take and tend to take them upon themselves as yet another task they can do in the evening or at the weekend. But they end up spending much more time on it. The seniors seem more used to it; for instance, when Henry from Gyro Gearloose comments that he has eventually learned to say no – also to things that matter to him. He has, in line with many of the other seniors, learned that public engagement activities take up much of their time if they agree to too much. It stops being something they do at weekends and during evenings and takes time away from the responsibility for the business and for the truth.

On the other hand, the scientists also tell me that they enjoy doing science communication because it '*feels good*' to communicate about what one does and to discuss it with others besides their colleagues. As one of the scientists from Gyro

Gearloose says: '*I think scientists really want to speak about their work, they just need the opportunity*'. Walter from Gyro Gearloose explains that he has participated in courses about plant chemistry for high school teachers and that the process of making them understand and reflect had been '*very rewarding*'. In section 9.1.1, I also showed that the scientists at times complained or excused themselves for not communicating enough. So there is a curious paradox about time and Citizenship: On the one hand, the scientists are surprised that it takes up as much time (from other responsibilities) as it does, and they learn to say no to some of the activities over time; on the other hand, they like science communication because it gives them an opportunity to talk about their research and they feel guilty for not doing it enough. I am not sure how to interpret these contradictions. I have called this section 'Feel Good Science', in an attempt to mirror 'Slow Science' and 'Fast Science'. I use the analogy of food but I am, at the same time, cautious about using it, as I would like more data to support the story. 'Feel good food' is a recent phenomenon: it is healthy food that is also supposed to be tasty and prepared with care. In addition, it should be made exclusively with fair-trade products, exclusively using organic ingredients or be strictly vegan – or all of the above. It is supposed to make your body, your taste buds as well as your conscience feel good. The analogy works in so far as science communication is something that can put the scientists at ease (although I also showed examples of the opposite in 9.1.1). Furthermore, it is something they need to do: '*everybody needs to lend a hand*', a bit like all of us need to adapt to a healthy diet and are glad when we manage to do so. At the same time, it turns out that it takes a long time to cook and eat in that way, time that could have been spent otherwise. Public engagement is in every way something that is considered to be an inherently good and healthy thing to do but, at the same time, a time-consuming task that they do not consider their main priority.

A similar ambivalence seems to be present in relation to fact-making. As with time, it is difficult to establish whether and how Citizenship generates a certain attitude toward facts. So the comparisons with the previous modes are again tentative. At Curious George, Clark tells me that it gives him '*good satisfaction'* that his research is interesting in the field of medicine. But at the same time, he explains that this satisfaction is something that comes on top of the satisfaction of making something that is interesting. At other times, as I have described throughout the section on justification, the scientists see it as their foremost responsibility to help other people, but how this exactly affects their stance vis-à-vis fact-making is more difficult to say. It often means that they prioritise certain research projects over others. At Gyro Gearloose, they do perhaps also tend to have a certain trust in their hypotheses because they always choose the research areas and methods based on what they consider to be '*the best solution to society's problems*', as Henry puts it when explaining their cancer vaccine project. With this, he also indicates that Gyro Gearloose, via their science, actually has access to knowledge about the most optimal solutions to society's problems – in contrast to many other places. At Gyro Gearloose, they strongly identify with being '*translational scientists*', meaning that they see it as their job to translate their knowledge into tangible technologies; without that responsibility, they would not really be there. At Curious George, most of the scientists are similarly convinced that many problems, for instance, hunger, will be difficult to solve without GMO technologies. So there are some connections between the general directions that their research takes and their opinions about solutions to society's problems. But while it is possible to establish links between problems they want to solve and their research area, it is more difficult to make a detailed account of how this specifically influences the establishment of every single fact they develop. This will demand a more thorough focus on that exact aspect.

When they do produce science whereby the prospects for applicability and use are very apparent, some of the feelings of frustration that I have described disappear. It is as if the scientists find their tasks 100% legitimate and the impression I got of them trying to defend themselves disappears. In that respect, it also becomes 'Feel Good Science' because, by living up to some contemporary demands, they do not have to go around with a bad conscience about public money being badly spent.

It is difficult to say why Citizenship does not generate specific attitudes to time and fact-making as Vocation and Oikos do; or at least none that I observed. Perhaps it is because the responsibility for the public is not considered as central as the one for the truth and the one for the business, and the mode is thereby weaker in its ability to generate different organisational effects. Or perhaps it is because this concern for public opinion is new and shared ideas about time and fact-making have not been established. Perhaps they never will. I am not sure about this particular aspect and can conclude that further empirical studies are necessary in order to conclude securely on this subject.

9.3.2 THE SCIENTISTS AS ACTIVISTS

An interpretation of the scientists' ways of doing their work as a result of this mode, could be labelled as 'activist'. By 'activist', I mean active political, non-party engagement with the aim of changing science's current role in society – or at least the field's role in society. The scientists find it important to take part in debates about the direction in which society is moving, especially if this debate involves their own area of work. Catherine from Curious George feels very strongly about this and comments:

'[…] *you have an obligation to speak up if you can see that somebody tries to manipulate things, because there are many organisations that use* [scientific] *data in* *their work. And now there has been that GMO experiment in France, which was a completely crazy experiment with a completely ridiculous population size. I mean, there were 10 mice and then two of them died and then they continued to work with the remaining eight. And, damn me, you cannot take the liberty to call that a population. And then they publish it in that way they did, I mean, they only gave the data to specially chosen journalists – who only got specially selected data before the article was printed and who knows what. And in an instance like that, I believe that you have a great obligation to help people navigate a bit. The statistics made from those experiments were plain crazy…'*

At the time of the interview, the news reported a recent French experiment where rats were fed GM maize and most of them apparently got severe forms of cancer. Catherine is – as the quote suggests – quite angry about the experiment and the press's handling of the case. She is so angry that he does not even care about getting the facts right. It was experimental rats not mice, and there were actually 200 of them not ten. But the facts are not that important here, as the exaggerations are made because Catherine is angry and, because the whole experiment is completely invalid to her, it does not matter if it were based on 200 rats or ten mice, the quality was so low anyway. She believes that it is '*a completely crazy experiment*' and therefore he has a duty as a scientist to speak up to help laypeople '*navigate a bit*'.

In the examples I have described in the conduct sections (9.1.1 and 9.1.2), most of the scientists' engagements with the public have been planned, carefully prepared and are very much about explaining their own research and its potential implications for society to a lay public. This is different. Here, Catherine reacts to an issue that she considers highly politically potent. The debate about GMO and its unintended side effects has been quite contentious. In the other examples, I have argued that the way the 'speaking about science' is performed could be understood as a specific commitment to democratic society through its practising in society's institutions (libraries, schools, etc.) and by the institution's methods, that is, enlightening dialogue. This example is different. Here, Catherine raises her voice with a specific standpoint, a conscious, political statement. Her political statement is still based on his professional expertise, and she still wants to help non-scientists '*navigate*' the debate. In that way, there is still an element of enlightenment in her activism, but that is not all. She is also just angry and wants to show an '*alternative truth*' (Barry 1999: 75) about GMO: a truth that he thinks she has a special access to based on her professional knowledge; namely, that the experiment was of no use and that GMO has not been proven harmful. In this way, her engagement becomes an engagement on equal footing with other interested parties.

Several scientists have complained that their colleagues, both within the lab and in general, could participate more in debates about the direction of science. For instance, Miriam comments that:

'Where I believe that scientists are failing are that they do not dare say "this is perhaps not the right way forward." We tend to lock ourselves up and say that "they have decided that, so we might follow it as long as I can still do research," instead of speaking up and saying: "Well, is this what we want as a society? Isn't it better that we do some basic science, where we can make really big leaps forward, instead of doing science that is pretty applied in the foreseeable future? Instead, do something that is more far-sighted." But you don't hear that much and interest in those kinds of opinions is also really small.'

Miriam believes that scientists should be far more daring in stating their opinion about what role science should have in society. She is a warm proponent of the view that academia should conduct basic science and leave the applications to industry, but while most scientists may feel the same, she believes that they are too scared of losing their money and work to speak up. And while I did observe scientists being critical about other scientific results, as in the example with Catherine above, I seldom observed the scientists actually criticising science policy in the public media. And this is despite the impressive amount of lobbying and negotiating about research-funding allocations they do in their daily work life (as we saw in the Oikos mode, 8.1.1). One of the only exceptions was actually Catherine As mentioned above, she spent half a year in the Department for Agriculture and Food. After her stay, she created a blog where she commented on current science governance themes – including a very critical debate piece that sharply argued against the statements on GMO made by the Department she had worked for only months before.

'They haven't really been in contact since then'. And of course I'm a little, "oh bummer, did I burn all my bridges?" Because I really liked working there […]. *But then, I mean, I couldn't sit there and watch the minister just turn on a dime and follow all the ecological types…'*

While the scientists at Curious George perhaps seldom speak up in public debates about science governance, many of the scientists do other things that I would characterise as 'activism'. As I also mentioned in an earlier section, they have initiated collaboration with the local Hacker Space on certain projects. They are very much in favour of the Hacker Space's open-source policy and their philosophy about science being to and for the people rather than big business. At the same time, as Canute commented, they are also glad to be able to help and keep an eye on the inventions from the Hacker Space so they do not do anything too risky or dangerous. The stance on patenting is a huge issue in biotech, an area where patents are taken out very frequently and a lot of secrecy prevails. While some of the scientists at Curious George do take patents out (also described in Oikos), Canute has, for instance, decided that he will not take patents on production platforms in order to engage other researchers and industries in improving the platform, which could then be rapidly improved used to e.g. produce numerous drugs each of which could be patented separately. This will speed up the process of generating products using synthetic biology approaches. The international research partners with whom they collaborate have the same sceptical attitude toward broad patents that may serve to hamper development within the patent protected domain. It does not mean that they have totally abandoned them: '*it can be necessary at times*', as Canute comments in a national newspaper. And there is no general policy in the lab on patents, but some of the scientists are quite sceptical.

Where many individuals, in one way or another, are involved in their own 'activist' case at Curious George, this is not the case at Gyro Gearloose. Here, they mainly work on their core tasks, namely, developing their technologies. While I am sure that they have opinions on many different subjects, it seems that direct activism and political involvement are reserved for the PIs. And usually the rest of the staff share the political views held by management. The picture is much more homogeneous. But the PIs do engage in 'activism' of sorts – and in a way that is closely related to their inventions. First and foremost, they are concerned that their medical inventions will never end up benefitting the patients because both the FDA[43] and big medical companies may stall the commercialisation of the inventions. While getting the FDA's approval is mostly about making very rigorous work and proving the safety of a technology, dealing with the pharmaceutical companies is not as easy. Henry, the PI, is really keen on getting the products out where they can actually benefit people; he has given the subject a lot of consideration and has also asked external consultants for help. Based on this, he now tries to avoid big companies when he sells the lab's

[43] In the USA., the FDA is the government agency responsible for protecting public health through the regulation of pharmaceutical drugs and medical devices as well as food safety, tobacco products and dietary supplements.

methods and ideas. He is afraid that big companies are too bureaucratic and will not work hard enough in promoting his technologies, but will rather put them in a drawer somewhere. For the next '*big thing*', he has even decided that he, together with the other co-director of the lab, will make a start-up company to avoid all the traps of working with other companies. While this is going to be a lot of work, he really hopes that it will make a difference for when the products reach the end-user. So participation in debates strategies for making findings publically available and criticism of big companies form part of the scientists' way of going about their work in this mode. It differs whether they act as concerned individuals, as an organisation or as part of a bigger movement.

So the responsibility for the public also prompts the scientists to engage in various activist activities and engage in movements such as the DYI-bio and open-source. Furthermore they also take an active stance against big business. In that way the scientists do seem very 'engaged' in society's problems, where they consider them to be relevant and take an active role in the engagement. But as I will show in the section called 'basic science guilt', they do at times, despite these engagements, feel 'guilty' about the science they actually do and their lack of engagement.

9.3.3 BASIC SCIENCE GUILT

The last organisational consequence that I want to touch describe is one that I have chosen to call 'basic science guilt'. It addresses the feeling, I often met at Curious George that they felt guilty about doing 'basic science' and thus needed to defend the value of it. I consider this feeling of guilt as an effect of demands to be 'responsible' and demands to produce science 'for the people' (Owen, Macnaghten, and Stilgoe 2012). In that way, this section also addresses the effects on the general wellbeing of scientists as an effect of Citizenship.

In an interview with Karen, I ask her to make a drawing of her project and how it relates to her surrounding world. Karen draws a circle in the middle of a piece of paper and names it 'project'.
'It's the middle of the universe, ha ha,' she comments as she draws.
She then draws another circle around the project circle, which she calls 'work' and then half a circle further out, which she names 'friends at work and outside work'. After some thought, she draws a bunch of small bubbles that partly cover the project circle.

'They are my ambitions,' she says, 'you know, career opportunities and self-realisation and all that stuff.'

Karen draws a lot of things on the piece of paper; thereby, the picture becomes more and more elaborated. 'This is fun', she comments. Other research fields are added as well as 'learning computer programming', 'visits to other countries' and 'collaborations with other universities'. She comments that she should perhaps draw some of the things the project should ideally relate to, but doesn't; 'Like family,' she says 'and some friends that I don't see much due to work.' She draws two rectangles in the corners of the piece of paper, far away from the middle of the universe. At last, she seems done. We are silent for a bit, and then I ask:
'Are there other things it should relate to, but doesn't?'
'Reality!' Karen promptly answers.
Me: '*But you just said that… But of course, by "reality", do you mean "usable"?'*
Karen: *'That was what I meant, you know, applicable.'*

This is a very telling story of the way that some of the scientists at Curious George look at their projects. On the one hand, Karen is proud that she is doing what she at another point calls '*an advanced basic science project*', but on the other hand, she believes that '*reality*' is missing from her project and indicates that it should somehow be included. From the drawing though, it seems that there is a lot of 'reality' at play. Her project is connected to all sorts of things; a rather big network has been visualised through the drawing. A network that connects the work she does in the laboratory with various other labs, her current life situation and dreams, colleagues and trips to foreign countries, as well as learning new skills, developing new knowledge and getting a doctorate degree. But besides all that 'reality', Karen still thinks something is missing; namely, that the project does not lead to anything immediately applicable for society. At other times, she defends the right to basic science and says it is important to satisfy human curiosity about how the world functions and that basic science could lead to big leaps in technology someday. But these arguments only underline the impression that something needs to be defended. The idea of doing a science project that does not relate to 'reality' – or is not 'applicable' – is not something the scientists feel they can do without justifying it to themselves – and others. They are ready to defend their position.

This sentiment is expressed quite often at Curious George, but very rarely at Gyro Gearloose. 'Basic science' is an integrated part of Curious George's research ambitions. In contrast, Gyro Gearloose does so little of what they believe to be 'basic research' that they never talk about the lack of potential applications. An exception is Sandra, who just arrived at Gyro Gearloose from another job. She describes needing time to get used to Gyro Gearloose's scientific culture. She liked doing research without thinking about the wider potentials for application: '*I need something to preoccupy my mind with, you know, I just like to solve puzzles,*' she commented once, laughing. She thinks it is important that research is '*exciting*' and '*mind-blowing*' in itself without thinking about potential ways to use it. However, I also get the impression that she thinks it is important to defend this position, as if it were threatened. During the period that I followed her, it seemed that she came to appreciate Gyro Gearloose's way of doing things. In the following excerpt, she includes a lot of other thoughts on what science's main task should be:

'So, I think, you know, if you are asking for grant money, and you get that grant, your role is to fulfil that grant, or at least do something as exciting as that. I mean, you might have an idea for a grant, and when you are working on it, you realise that "this is not going to work out, but I think there is something else that is going to work that is even better or more exciting." And it's just – as long as you are producing something useful, interesting, helpful – whatever.'

Here, she includes '*useful*' and '*helpful*' in her definition of what scientists should try to do, but also adds a '*whatever*'; it should not be a definite condition that it must be '*useful*' or '*interesting*'. In the same interview, she tells me that she has come to like the way things are done at Gyro Gearloose and that it is nice to be organised in such a way that she knows what to do everyday, what her role is and what she can expect from others. On other occasions, she maintains – not in relation to her work but more in general – the position that basic science needs to be defended. She was also one of the few who expressed that opinion at a seminar that I hosted toward the end of my stay at Gyro Gearloose, at which the scientists and I discussed social responsibility and scientists' relation to society. But that does not necessarily mean that she was the only one with that opinion: The director of the lab was present during the seminar and I am not sure that many of the attendees dared express too many sentiments that were against his opinions – at least not in times of crisis, as described in Oikos. And Henry, the lab director, is certainly not a fan of research without any other purpose than satisfying human curiosity. As he comments once, when he tells about his research career to some of the juniors:

'Basic scientists hate me when I say this, but I think it's increasingly wrong to let society fund you and then not deliver something back.'

Henry often talks out against basic research. And in contrast to some of the scientists from Curious George, he is not directly defensive about his way of doing research. But again, his vision of doing things '*backwards*' – from the best possible solution to the required research – also fits very much with current science policy ideas. Gyro Gearloose's way of organising their research backward responds very directly to calls for science 'for the people', as the specific end product always determines what kind of experiments they do.

At Curious George, the end product is far from always determined from the outset of a research project. But neither do they directly oppose the idea of their research ending in applicable technologies. One of the professors, Clark, at Curious George asks me about the difference between their lab and the one I visited in the US. I tell him that I find it interesting that they always focus on their end product, something I definitely find different from Curious George. He considers this a bit and replies:

'It gives me a lot of satisfaction to know that there's a product on the horizon, but I think it's important that it's sound basic research that lays the path … We do basic science, we are lucky that the stuff that we are interested in happens to be of commercial interest as well. I found out only later. I was interested in the more scientific part of the project, and then I found out: Wow, there's a company and they are really keen on collaborating…'

While Clark does perhaps not feel as guilty about doing basic science as Karen in the story above, he does acknowledge that *'a product on the horizon'* is a good motivator. He also acknowledges that it has been significant for his field that their area of research – plant terpenoids – is becoming increasingly '*of commercial interest*' because of its potential usage. The scientists at Gyro Gearloose feel that it gets easier to justify their jobs to people outside the scientific community if some kind of product – even if it is only 'on the horizon' – is part of their research. This way of going about their research – with a '*product on the horizon*' – is very telling of the way they do science at Curious George. They have broad ideas about how their science may change different areas of society, such as the production methods of medicine, energy or new crops. They have a vision of a better society, where their methods or technologies fit in, but do not necessarily create specific products. On their webpage, you can, for instance, read a presentation by one of the research groups in the lab:

'By combining highly sensitive Nano devices with biological samples (e.g. proteins and cells), we are attempting to develop novel biosensors applicable in e.g. the medical and environmental safety sectors. The unique features of nanomaterials are expected to provide novel types of biological information that can revolutionize diagnostics and pharmacological investigations of diseases such as cancer, diabetes and heart disease.' (From Curious George's webpage 28.06.14)

From what I discerned in the field, it takes quite a lot of what the scientists at Curious George call '*basic science*' to come closer to fulfilling the expectation to 'revolutionize diagnostics'. For instance, they need to study the 'unique features of nanomaterials' in great detail, which may take years, and perhaps it turns out not to be the right way to go. Nevertheless, the visions are there and they work toward them. However, they do not seem to think that working toward specific visions for new ways of making medicine is relevant enough. The scientists at Curious George seem to be constantly on the lookout for relevance; they do not necessarily *plan* for their projects to be relevant, but they savour relevance when their research turns out to be so. The scientists at Curious George are good at listing all the potential uses of their knowledge and all their close stakeholders and collaborators. But in contrast to Gyro Gearloose, they never seem to be satisfied with their lists. On the one hand, they at times sound guilty about not being relevant enough, as Catherine comments:

'Sometimes I get a bit frustrated with this job. And this is why I also liked being in the Department (of Agriculture), because sometimes, you just sit in here [in the lab]*, and besides your supervisor, only two or three people in the whole world are going to read what you have done… tops! And it just seems so useless…'*

On the other hand, they also furiously defend the importance of their right to do basic science – usually with the argument that no one can ever know when an application will come from great a discovery. As Clark, who seemed almost angry with me when I asked him why he thought the public needed education in science, says:

'What do people that do basic research say? People that do true basic research have very severe limitations when it comes to selling stuff and really making an application. Should they not be allowed to do that research? Just because the public does not immediately see the value? But it [the research] *clearly has value, because it could be the foundation on which 20 years later positive, additional research will happen. So that would be more of a question for you: "Do you think we should do more basic research?"'*

Suddenly faced with my own question, I stammered an answer that did not make much sense. I was more concerned about how agitated Clark suddenly became. But this was often the case when the topic was 'basic science'. My impression is that the scientists at Curious George feel pressured because they are not sure whether they are ‘allowed’ to do basic research. They feel that it is illegitimate – and they sometimes associate these limited possibilities with increased public participation in science.

And yet at other times, they assert that their own research *is* indeed very relevant, because they work with a plant from which it is easy to generalise to other plants – what they call ‘*a good model plant*’; or because they work with something that results in findings that are of interest to industry or others; or because they work with an important crop. For instance, I overheard this conversation over lunch:

Miriam: *(teasing a little) ‘So, why are you really working with that plant, Catherine? Why is it sooo interesting?’*
Catherine: *‘You know, it’s actually a really important crop. It’s it forms the basic nutrition for a lot of people and it contains several highly important vita…*
Miriam: *‘Wow, wow, wow, calm down, you got me – and you certainly got your intro for the PhD dissertation!’*

A certain schizophrenia in relation to the topic of application is discernable at Curious George. But the almost aggressive defence of basic science, combined with the sudden expressions of doubt about the legitimacy of doing it, gives me the impression that a sort of shared guilt about doing basic science hangs over the lab at times. Not all the time, of course. Perhaps, it is noticeable that Vocations do not generate feelings of guilt, whereas Citizenship provokes it.

These stories have been included because I find it interesting that the stories of responsibility in the mode of Citizenship closely resemble the idea that science needs to be of ‘relevance’. Hessels and van Lente (2009) have looked at the demand for ‘relevance’ in science and analysed what it has come to mean in science politics over the years. They show that ‘relevance’ has been a key term in public science at least since World War II, but the meaning of the word has changed. As funding is increasingly framed as a helping hand for national innovation, the meaning of 'relevance' also changes:

'*Around the turn of the century* [the 20th century], *the funding of university research is increasingly framed as support for the national innovation system. Maintaining a 'healthy innovation system' becomes a central goal of economic policy, which implies that universities deserve support thanks to their central position in this system. In this perspective, however, support for university researchers is accompanied by the expectation that they actively interact with other actors in the innovation system, and contribute to the process of 'valorization', by writing patents or by founding spin-off companies*.' (Hessels, Van Lente, and Smits 2009: 395)

In the chapter 'Vocation' the scientists worried about the validity of their studies. Now they worry if their work is relevant, that is, applicable. In that way, responsibility and relevance become connected. The way 'relevance' is articulated in policy discourse is about how science can put forward knowledge, which can drive national and regional innovation. And when the scientists at Curious George talk about their own projects, they are uncertain about whether their work can really do that. In comparison, the scientists at Gyro Gearloose do not feel guilty (about that, anyway), as they always know exactly how their work is going to feed into the development of a new product.

The chapter on Citizenship has so far shown that the scientists feel a responsibility to account for the public money they spend. The scientists are eager to show the public that they are worth the money spent; that they do actually work hard and that they work to improve our shared living conditions. But one of the ways to prove that is to show that what they do has an innovative potential and can be taken up by industry and start-ups. Some of the science that they do at Curious George does not have these immediate potentials. This does not sit well with the scientists.

I will argue that many of the scientists develop what I call '*basic science guilt*' – which the scientists in the above stories suffer from. I understand and use 'basic science guilt' as the feeling of inadequacy that the scientists get when they do not believe that the work they do is legitimate in the eyes of others. And even though they disagree with that perceived judgment, they still find it difficult to maintain, for themselves, the idea that what they work on everyday is of value. And they feel the need to defend their job. To do '*relevant*' research seems to be a way of returning something, where they can prove that tax-money has been well-spent and in that way somehow give something back. But as both 'relevance' and 'Responsible Science' are quite broad and fragmented phenomena, it is quite difficult to estimate when they are 'relevant' or 'responsible' enough and this leaves room for feelings of guilt and inadequacy. At Gyro Gearloose, where they innovate specific products and can see a clear connection between research and application, this guilt is not apparent. In their case, '*relevance*' and the responsibility to give something back to society converge. But at Curious George the tasks are decentralised, people have different projects and some of it is basic science, and that is why 'basic science guilt' prevails.

Observing how the two labs relate to relevance does engender some questions in relation to my choice of the two laboratories as case sites. At a glance, it might seem as if Gyro Gearloose is just more 'responsible' than Curious George. One may argue that Gyro Gearloose lives up to the standards of 'Responsible Science', as presented in the theory section (2.1). They reflect upon their science and the outcomes; they organise and participate in deliberative exercises with the public; and they are focused on innovation and deliver something very tangible, useful and even 'good' to the public, namely, new diagnostic devices and vaccines. They even develop strategies to avoid the much-criticised pharmaceutical industry (e.g. Sismondo 2008) in order to create a better alternative for the 'customers'. At the same time, they also cherish classic academic virtues and strive to live up to academic standards. Are they more 'responsible' than Curious George? The answer depends on the perspective. If one looks at the two labs in comparison with the ideals put forward as 'Responsible Science' (see 2.1), then Gyro Gearloose is perhaps actually the most responsible one. In many ways, they do live up to the ideals. And I do not believe that Curious George does to the same extent. First, the ideals do not seem to penetrate the organisation to the extent they do at Gyro Gearloose. At Curious George, the scientists do a lot of outreach, but it is not centrally coordinated and the reasons for doing it are not centrally controlled, but rather based on individual interests. Second, the scientists at Curious George seem more critical about public opinion than the scientists at Gyro Gearloose. They are not necessarily convinced that the public should always have a say in deciding which direction science should take, and they do not always consider them helpful partners. They do a lot of work with plants for developing countries and advanced medicine, but on the other hand, they also insist on the right to do basic research without interference from other stakeholders. And sometimes, they even refuse to reflect upon their work. They already did that when they chose their area of research, they claim; or they think that it is only necessary if one works with animals. Seen from that perspective, Gyro Gearloose does seem more responsible. But the question is whether we are looking at responsibility in the correct way?

From another point of view, Curious George is perhaps just as responsible as Gyro Gearloose. Yes, Gyro Gearloose has in many ways included in their work elements that resemble those from 'Responsible Science'. Curious George has done this as well, but perhaps not as much. However, given that we consider 'Responsible Science' to be a specific government rationality (2.1) with specific aims, it should be noted that it therefore encourages certain forms of behaviour and discourages others.

In this case, it is my interpretation that ideals about 'Responsible Science' do contribute to the scientists' feelings of '*basic science guilt*' because of the encouragement to reflect upon outcomes of science and how these interact with society. The demand to reflect is burdensome in the instances where the scientists honestly think that reflections upon their interactions with society are of little importance. I imagine that this demand must leave the nagging thought that they just have not been able to reflect hard enough or they have overlooked something extremely important.

Furthermore, the rationality also implies that a certain amount of legitimacy in the eyes of the public is necessary. This is not a substantial claim; the rationality does not positively articulate what makes an outcome legitimate, as this is supposed to be a result of deliberative processes (Owen et al. 2013: 38). But the positive inclusion of different visions for society in the material construction of new knowledge and technologies is articulated as a goal. However, this may prove difficult to fulfil if the knowledge the scientists are making is not applicable. Owen et al. (2013) do assert that 'Responsible Science' cannot and should not be a standard for all research; they state that the most 'pure' forms of science could be excluded from this framework. But perhaps it is worthwhile to look at the unintended side effects of the ideals of 'Responsible Science'; namely, that they form part of a movement advancing away from the value of basic science and toward an ideal of usable results that are possible to discuss and reflect upon. Guilt about not making applied science is in my view one such unintended side effect.

Curious George certainly do consider which kinds of science they conduct and why. They are very committed to – and often articulate the importance of – making science of the highest, long-term quality; something they can leave behind and which can be of use for future generations. While this may not feed into the national innovation system here and now, nor be functional in any kind of way, it does spring from a responsibility to return something of value to the surrounding society. However, they do not understand responsibility as a demand to be '*responsive*'.

NEGOTIATING CITIZENSHIP

In the previous sections, I have described the mode of responsibility called 'Citizenship' as if it were a '*pure*' mode; that is, an ideal, which is fully realised in the way that the scientists work (Law 1994: 6). But in reality, it is of course not. In the two previous chapters, we have already seen how the scientists also organise their work according to the two other modes, namely, that of Vocation and Oikos. In this section, we will take a closer look at how the scientists negotiate which justifications should form the basis of their decisions and how they, furthermore, refuse to take the responsibilities associated with Citizenship due to other concerns.

One of the most common denials of any kind of responsibility to 'listen to the public' (9.1.2) was made with reference to the idea that if the public received education about science, then they would not oppose scientific advancements. This refusal is clear in an interview with Clark, in which he told me about a seminar held by Curious George's 'in-house' philosopher, who worked with ethics in synthetic biology:

Clark: *'Well, we lost a lot of opportunities* [due to the GMO crisis]. *Absolutely. Stem cell research is going in a similar direction right now. And for very similar reasons, and this is very annoying, because I think that scientists can stop that before it happens. And this is why the synthetic biology programme has initiated a public exchange and communication. And we have a philosopher on board…'*
Me: *'Yeah, Andrew?'*

Clark: *'Yes, Andrew. It was really cool, because he has talked about exactly those things: Public perception of synthetic biology – threats and opportunities seen by the public. And then he explained that when the public is educated about a certain topic, and he asked again about their opinions, they are much more positive. And I was like: "Wow, this is a big change." So it clearly shows that we have some homework to do.'*

I have met Andrew and heard him give talks on other occasions, and I actually cannot imagine that he would ever advocate the viewpoint that scientists should engage with the public in order to avoid public controversies. But that aside, it is interesting how Clark here proposes what looks like a classic '*deficit model for science communication*': One where resistance is seen as an expression of ignorance and communication as a way to remediate this ignorance and eradicate scepticism (Trench 2008: 134). Even though Clark acknowledges the responsibility to communicate with the general public, it is only as a strategy to make people more positive toward a '*certain topic*' and to curb public resistance. From time to time, I met this idea in one way or another. Engagement with the public is done in order to open the way for the type of science the researcher wants to do. In the following, it is Walter who talks about his current research project:

Walter: *'Well, what we try to do now is what we call bio-pesticides, that is: How we use naturally occurring substances from the plants as insecticide or fungicide – or as an alternative to the chemically synthesised pesticides, which we use today.'*
Me: *'Precisely. What you do is, as I understand it, develop something that is supposed to replace something which polluted quite a lot?'*
Walter: *'Yes, and hopefully it also generates a bigger understanding among the public that you can do things in alternative ways by learning from nature.'*

Walter hopes that his work can teach people that we can '*learn from nature*'. But at the same time, he refuses to acknowledge that his research could be seen as controversial in any kind of way or that people could see any kind of ethical problems in their new '*insecticide*' or '*fungicide*'.

'I don't really consider ethical problems because I don't work with animals,' he tells me in the same interview; 'I actually changed research area back in the day because I couldn't handle working with laboratory animals.'

So Walter does think about ethical problems, but not as something that affects his current area of research. He has a classic medical view on research ethics, where it is about not consciously hurting others in the research process – in his case, not even animals. In that way, working with GM plants is simply excluded from ethical concerns and he does not give it much thought – although he does mention that plants actually 'scream' in pain when you cut them, albeit we cannot hear them. When he talks about communication with the public, it is about teaching them that nature can be used in intelligent ways, not about 'listening' or accounting for the work they do.

Another way that the scientists try to moderate the demand for engagement is by referring to mutual 'trust' – that all that engagement and dialogue is not really that necessary because they are, as one of the scientists says, '*basically very reasonable people who do very reasonable things*.' This is an argument that I hear quite often: That the different institutions should trust each other and that they (the scientists) trust other parts of society to do their work, so perhaps people should do the same with their work, as Catherine comments:

'I also think that people have to trust that reasonable things are going on here and at the other science departments… I think so… or I actually don't really know what I

think about it. But I think that you have to trust it. I trust it myself: That they do something reasonable at that faculty they have out there on that island [the Humanistic Faculty in Copenhagen]. *In reality I have no clue about it, but they probably know what they are doing… at least that is what I assume.'*

This is another way of questioning public engagement and the inclusion of citizens in the governing of science; that is, by saying that it is unnecessary, because society needs some kind of basic trust in and between its institutions – not suspicion. I often met this kind of resistance during my fieldwork. I interpret it as another view of democracy than the one expressed in the mode of Citizenship. This one is closer to an idea about a representative democracy with stable basic institutions and a distinct division of labour between them. Here, trust that others are doing their work to the best of their ability is a prerequisite for making society work. Too much interference from other professions or groups is seen as a sign of suspicion, not one of mutual trust and collaboration. In contrast, Citizenship performs a much more deliberative democratic ideal: Through dialogue and co-operation with heterogeneous actors, the scientists and others can together move science and society forward.

The final observation of resistance that I will touch upon is tied to the scientists' actual amount of work. The scientists often seem sceptical about engagement projects because they are afraid it will take time away from doing research. They often experience it as yet another chore they have to do, which they do not consider '*research*'. They coined it '*paperwork*', '*administration*', '*bureaucracy*', '*surveys*', '*all that stuff*', '*outreach*', among many other terms, and they often sound tired saying it. Simon, who actually does a lot of research projects, says:

'I mean sometimes I just get so tired… Then this secretary comes and asks me if I had done this or that survey, and I say "no", because I didn't think it was mandatory, but

there are just all these things and it sometimes just gets too much. I have this cartoon in my office: it's this guy walking out into the hallway, saying "I've solved the riddle of cancer", but people ignore that and tell him that he should sign a lot of papers and register his teaching… and I sometimes just get so ARRRGH, because we have to do all these things and it takes so much time… But I shouldn't say too much, I do a lot of outreach myself…'

This feeling of '*ARRGH*' is one I have met frequently. It is not that the scientists are not sympathetic to the cause of outreach or do not want to participate in all sorts of public engagement projects, but rather that they have a lot of work already and they find it really exhausting to fit more into their work schedule. At the same time, they do not get any formal acknowledgement, salary or promotions by engaging with society. Therefore, it often ends at the bottom of the priority list. This is quite interesting because it reveals some of the very tangible obstacles of making 'Responsible Science' an integrated part of scientific conduct. In their daily work, the scientists find it hard to fit in more responsibilities than they already have. They are in many ways, as I have shown in the previous sections, not even against 'Responsible Science' they perform it themselves. But at the same time, stress and feelings of powerlessness frequently arise in the face of the many responsibilities that the scientists are faced with. For instance, this became apparent in an exchange with one of the scientists from Gyro Gearloose on my first day at the lab, when I introduced myself:

Calvin: *'Well, welcome. So you're a bit like the other* [another ethnographer] *who was here last year. So what's your project about again?'*
Me: *'Uhm, I look at 'Social Responsibility' in science and how you guys work with demands to engage with the public.'*
Calvin: *'Oh, so you're here to look at that publish-or-perish phenomenon.'*

(From memory)

In the beginning, I was not really there to look at '*that publish-or-perish phenomenon*'. I saw that as something very different from ideas about 'Responsible Science'. But during the fieldwork, it became apparent that the scientists do not really distinguish between the many different demands they are subject to – Publish! Engage! Innovate! – and have to deal with in order not to '*perish*'. All of the demands are seen as troublesome and often they are seen as obstacles to doing science, which is still their primary responsibility and without which their job would not exist. These conflicting demands are especially troublesome for the junior scientists: first, because they do not have a permanent position (of which there are fewer and fewer in general) and they therefore try to perform all three responsibilities at once in order to prove their worth; second, because these three different responsibilities are actually difficult to navigate (as we also saw in chapter 7 and 8), and it is difficult to discern which responsibility is actually their *main* responsibility. This is, for instance, the case with Catherine, whom I have mentioned before.

As part of her PhD, Catherine has worked for the Ministry of Agriculture and Foods and has her own blog in one of the national newspapers. But all these activities also mean that she is not as far advanced with her research project as she could be. In an interview she told me how her supervisor, has demanded that she start to 'focus'. She needs to concentrate on research. 'Sometimes he even says it, when he just passes me in the hallways "focus, Catherine, fooooooocus".'

On another occasion, the supervisor comments on Catherine's virtues: 'Catherine is an excellent science communicator as well as an excellent scientist, but she really needs to re-focus on the research. I mean, that is our core job here, and you cannot get a PhD without it.'

The scientists find that balancing between the different responsibilities is difficult. While this is something the juniors find especially tough, as they are new to the job, it is something that everyone recognises and is discouraged by. Not because they do not consider engagement with society important, but because it is simply hard and exhausting to fit all their responsibilities into an ordinary workday.

9.5 DIFFERENCES BETWEEN THE TWO LABS

Of the three modes that I have described, Citizenship is that which exposes the greatest differences between Gyro Gearloose and Curious George. In Curious George, Citizenship is performed as a range of different activities and political stances vis-à-vis the best way to do science. They co-operate with DIY labs; they host public lab nights, where the public can experiment with the gene modifications of plants; they try to pick up on the public's perceptions of GMO and problems where they can help and direct their research; they prepare their juniors to reflect about their research by promoting courses in industry partnerships and ethics; and they participate in public debates about synthetic biology and a range of other relevant topics. At Gyro Gearloose, the picture is much more homogeneous. They also participate in public debates, invite the public into their lab, try to circumvent the pharmaceutical industry and are very conscious about which technologies they want to invent. But their lab is much more tightly united in the common goal of inventing tangible, medical technologies that can help future patients. Therefore, all their ways of engaging with the public are underpinning that common goal.

This is the biggest difference between Gyro Gearloose and Curious George: Curious George is not united in one, unique quest; they have a range of different research activities, many of which they label 'basic science'. In my interpretation, that is also why their engagement activities are much more diverse – they are connected to many different research projects, which are confronted with different problems and challenges. Even though the justifications for why the scientists have a responsibility to share knowledge and technologies with the public are strikingly similar across the two labs, the performance of this responsibility looks very different depending on which kind of science they do. Many studies indicate that the boundaries between 'basic' and 'applied' science are blurry at best and perhaps even non-existent in reality (e.g. Gibbons and others 1994). While this may be so, this study describes how the perception of what kind of science is being done has a big influence on the way that '*Responsible Science'* is conducted – even if the general principles for why engagement with society is necessary are similar.

This difference makes it relevant to ask whether it is realistic or plausible to work with *one* idea about 'Responsible Science'. In Owen et al. (2013), the authors suggest that their framework for Responsible Innovation should be applicable in regard to most forms of science – except perhaps the most theoretical kinds of '*purely descriptive science*' (: 45). But perhaps the concept needs more flexibility in order to work and should be developed in closer co-operation with the scientists working in the field, as they seem to have some very elaborate ideas about which kinds of problems and challenges the exact disciplines and technologies may embody.

Another reason that may account for the difference between the two labs is that 'Responsible Science' – especially the idea that the public has a legitimate right to change the direction of science – is a rather new development. Of course, the idea of the scientists having responsibilities toward society is not new, as I indicated in the Theory Section. But the exact composition of that rationality, where participation and responsiveness in some suitably recondite sense has replaced professional ethics as an ideal in the public sector, is quite new and perhaps not that institutionalised yet. The scientists (and policy makers) are still trying to figure out what 'Responsible Science' is, what it looks like and how to do it. It is, in other words, still at an experimental stage. In comparison, the two other modes, and especially that of Vocation, have longer histories. The rules of conduct in Vocation have become institutionalised through many decades (centuries, almost) of careful teaching, writing and dissemination of ideals related to science and how to reach them. They are – at a glance at least – much more coherent and internalised than those of Citizenship. Those ideals found in Oikos are newer. The marketization of universities has been part of New Public Management reforms, which have gradually gained momentum since the eighties (Boden and Nedeva). These reforms have also heavily targeted the universities' finances, forcing them to undertake frequent structural changes in many of the public science's core duties such as teaching, research, and the education of young scholars (Shore and Wright 2000: 71f). While research areas such as plant chemistry and stem cell research have experienced cuts due to public scepticism, the scientists still seem to be in a phase of getting used to that shift in public policy. They seem more experienced in adapting to the demands for competition for funding. Therefore, the local differences in how to handle the demands of public engagement may be more pronounced than they are in Oikos.

9.6 CITIZENSHIP AND POLITICAL RATIONALITIES

As the last point before the conclusion, I will compare the four political rationalities from chapter 5 with the responsibilities that I have outlined in this chapter. As this is the last chapter, I will also make some considerations about the theoretical connections between the political rationalities and the '*modes of responsibility*'.

As I have already shown in the two previous chapters, these purified visions of what responsible conduct looks like and the everyday work life of scientists differs quite a lot. But that does not mean that there are not links and similarities between the two. Interestingly, the mode of Citizenship performs elements of all the different political rationalities, albeit mostly from the '*Contribution*' and the '*Integration*' rationalities. But elements from both '*Demarcation*' and '*Reflexivity*' are also present.

While one of the virtues of the Demarcation Rationality is to withdraw from society in order not to become entangled in society's interests, another point in the Demarcation Rationality is that it is science's duty to help 'mankind'. In that respect, the insistence of Citizenship that science and technologies need to be shared with the public can be seen as a reflection of that virtue. This does not mean that they are identical. There are, for instance, great differences in the ways scientists are supposed to share their knowledge and technologies in the Demarcation Rationality than in Citizenship. The Demarcation Rationality does not encourage any form of dialogue or mutual understanding. Here, facts are supposed to be delivered in a neutral and objective fashion, with citizens then doing with these facts what they please (but hopefully they follow scientific advice). Another example is the great difference between articulating a wish to help 'mankind', as it is articulated in the Demarcation Rationality, and Citizenship's concern for '*the public*'. Whereas 'mankind' refers to the basic trait of being human without any other defining characteristics, 'the public' is already much more delimited and specific. Compared to mankind, 'public' refers to a much more well-ordered mass of people who are somehow not just human, but also part of a population that is within the limits of governmental regulation, which is very different from mankind. As I indicated earlier in the dissertation, it is not of the utmost importance for the scientists whether the 'public' they seek to help is part and parcel of the same regional, national or supranational area as themselves. In that way, the ideals from Demarcation are mirrored in Citizenship in so far as science is still considered an international profession with universal rules of conduct.

While the kind of public they help is of no great importance to the scientists, it is important for them that they *do* help, as this is a motivating factor in their work. They want to solve big problems and challenges and, in that regard, their conduct mirrors ideas from the Reflexivity Rationality. This is further underlined by their not caring who it is exactly they help, as long as they are in need. I consider this a sort of professional freedom, which was also expressed in the Reflexivity Rationality: Scientists are indeed problem solvers, but they have the power to decide which kinds of problems they would like to solve.

Another aspect that is telling for the Demarcation Rationality is its focus on responsibility as part of the scientific *process*, rather than responsibility as a quality that the outcome produces or possesses. This focus on the process of doing science as a target for steering is also apparent in Citizenship. This is, for instance, apparent in many public engagement activities that take place before the fact in both laboratories – where prospects and disadvantages about emergent technologies are discussed. This is not least the case at Gyro Gearloose, where very specific properties of their technologies are up for debate. But the huge difference is that the Demarcation Rationality does not express any wish to include the 'mankind' it tries to help in the discussion about the research. Instead, strong internal ethics are considered the safety net that ensures responsibility.

In that regard, Citizenship shares more features with the Integration Rationality. The rationality articulates a need for external regulation of science and it also considers the process of doing science as the object of steering. This regulation is typically imagined as happening in deliberative dialogues between interested groups. It makes sense that this political rationality is being performed in Citizenship, as a good part of those academic articles, which formed the basis for the construction of that political rationality, were from the newer body of science governance literature that encourages ideas about Responsible Innovation (Schuurbiers, Osseweijer, and Kinderlerer 2009; Roco et al. 2011)

But one of the surprising aspects of the mode of Citizenship is the huge focus on accountability to the public. The accountability is not tied to the production of new knowledge and technologies *per se*. Rather, the scientists are eager to make sure that the public know that they indeed work hard for the funding they receive. This way of performing comes in fact much closer to aspects of the Contribution Rationality. In that rationality, the firm external control is seen as a way to make the scientists contribute to society. This focus on controlling is not explicitly talked about in Citizenship, but it is striking how they see public engagement events as opportunities to ensure that their reputation as honest, hardworking people is maintained. All in all, the mode of Citizenship embodies elements from all four rationalities, which are mixed and matched in different daily situations. Compared to the other two modes, this is definitely the one in which the most varied elements from the political rationalities are assembled. There could be several reasons for that. One is that Citizenship is the youngest mode, and 'the best' elements from all historically legitimate rationalities on '*Responsible Science*' are patched together – creating huge responsibilities: both making '*true*' statements, '*relevant*' science and incorporate reflections on '*outcomes*' in their work.

Another reason may have to do with the nature of the social. I have constructed four rationalities about the responsible conduct of science based on writings in academic journals. While I can see that there clearly *are* connections between these rationalities and the scientists' daily work, it would be rather surprising to find a 1:1 relationship between ideal and conduct. As Law comments (paraphrasing Foucault), '***the networks of the social carry and instantiate a series of intentional but non-subjective reflexive strategies of social ordering***' (Law 1994: 96, original emphasis).

Law does not thoroughly account for how these 'strategies of social ordering' become connected with daily work, for instance, how one 'chooses' different strategies or why the shifts between different modes happen. But he does assert that '*they come and they go. They are certainly not exhaustive. And they are, of course, defeasible imputations. On the other hand, they are contingent, but coherent reflexive…'* (Law 1994: 96). This is the same kind of observation that I can make about the connections between the political rationalities and the modes of ordering. I can argue that there are indeed similarities, and that there are patterns in the way the scientists do and think about responsibility. And I can see how different resources from the different rationalities are drawn upon in each mode and disappear again. But I cannot account for the exact connections.

What I can say is that there are indeed some huge differences between the descriptions of ideal responsible conduct from the document study and how the scientists handle the responsibilities they face in their everyday work life. The scientists seem to use the different ideals about responsible conduct when they have to make specific decisions in difficult situations. Different types of worldview inform them about different ways of 'taking responsibility' for the situation; how they choose and why they do it creates some very specific ways of going about the scientific job.

9.7 CONCLUSIONS

In this chapter, I have outlined the mode of Citizenship. I have described the way the scientists talk about their research, listen to the public and do research for the public. I have also indicated how they justify this form of behaviour with the idea that public science is a public institution among other public institutions and should – like the others – contribute to 'society'; in doing so, they accept that the public has a legitimate say in the direction of science, even though they at times find this tiresome and frustrating. This view of science's role in society is different than how the role was articulated in the two previous modes. In Oikos 'society' was constructed as a market, where research organisations competed for scarce resources. Vocation generated a view of society as a range of stable institutions with distinct responsibilities. Citizenship also generates the idea of society with different institutions, notably a large public sector where public professionals of various kinds work on improving society. But the lines between the different institutions and the different responsibilities have been blurred compared to Vocation's ideas. Both '*the public*', and '*science*' has responsibilities for the outcomes of science – a responsibility that can be fulfilled through deliberative dialogue and 'engagement'. In that way, Citizenship shares many similarities with the broader demands for 'Responsive Government'. Science also has a responsibility to produce '*relevant*' research *for* the people – something the scientists translate into making 'i*nnovation*'. Thereby they also have a responsibility that Vocation allocated to industry. These dissolving lines between different institutions and their responsibilities make the scientists' confused about what their responsibilities are and how to handle the different forms of demands. Notably they develop resistance and 'basic science guilt' if they do not feel they live up to the demand of being 'relevant'.

I have explained how the scientists justify their conduct in similar ways across the Atlantic, but that they speak, listen and do research rather differently in Citizenship. I attribute these differences to the specifics of the science they do, and I suggest that it would be useful take a closer look at the ideals about ‘Responsible Science’ as these are currently presented in the science governance literature. Increased flexibility and different approaches to what counts as ‘Responsible Science’ may be in order.

Finally, I compared Citizenship with the political rationalities identified in the document study. Curiously it seems as elements from Citizenship can be mirrored in the four different rationalities. It is difficult to assert, why it is so, but one explanation is that Citizenship is a mode that generate several layers of responsibility and thereby many tasks that the scientists have to do: both making ‘true’ statements, ‘relevant’ science and incorporate reflections on ‘outcomes’ in their work. In the next, and concluding, chapter I will follow up on the findings from all the analytical chapters and suggest ideas for further studies.

10. CONCLUDING DISCUSSION

Throughout the previous chapters, I have analysed how 'Responsible Science' is conducted and justified in scientific work, taking my analytical point of departure in the practitioners' own perspectives and experiences. In this concluding discussion, I will sum up this dissertation's stance and findings and discuss these in relation to further theoretical, empirical and practical work, looking particularly at the bearing that the notion of 'responsibility' has on scientific conduct.

In the first analytical chapter, I presented an analysis of four political rationalities in relation to the subject of 'Responsible Science'. The analysis is based on the (Foucauldian-inspired) concept of 'political rationalities' (Rose and Miller 1992), and it showed that four political rationalities related to the responsible conduct of science emerge from the study of scientific journal papers. The '*Demarcation Rationality*', which sees an institutional and practical separation of science and society as a prerequisite for responsible scientific conduct; the '*Reflexivity Rationality*', which argues for an enhancement of reflexivity among scientists, implying that they should use society's problems (also the ones created by science itself) as a point of departure for their inquiries; the '*Contribution Rationality*', which insists on science's duty to live up to the public's expectations; and, finally, the '*Integration Rationality*', which advocates for the integration of different actors' perspectives in the process of doing science in order to enhance its responsibility. While all four are distinct, they also share similarities. Both the Demarcation and Integration Rationalities consider the actual scientific process as the object of steering, whereas the Reflexivity and Usability Rationalities point to the outcome as the object of steering. But the Demarcation and Reflexivity Rationalities both insist on internal steering by the profession itself, while both the Usability and the Integration Rationalities underline the importance of external governance if science is to become responsible. Furthermore, the analysis revealed the wide variety of ideas about 'Responsible

Science' in the scientific field. It thereby underlines that 'Responsible Science' is a conflicted and multifaceted phenomenon.

In the three analytical chapters that followed, I moved the analysis from text to ethnography, that is, I did ethnographic fieldwork of two laboratories, which I named Curious George and Gyro Gearloose. In the first of these chapters, I described (inspired by Law's 'modes of orderings' [1994]) a '*mode of responsibility*', which I called Vocation. Vocation justifies its actions with the responsibility that the scientists have for taking care of the '*truth*'. This is done through their meticulous and rigorous work with materials. The three forms of conduct, '*checking*', '*repeating*' and '*criticising*' are characteristic of this mode. The responsibility for the truth also has consequences for the organisation in terms of how they consider themselves as professionals. In the mode of Vocation, they consider themselves members of a global profession with noble intentions. It also influences their perception of time, whereby the mode generates an idea of time, as something there should be plenty of, so they can keep perfecting their observations. It also means that the faith they have in their own abilities to produce a fact are quite limited; they look at their work with many reservations and a large dose of scepticism. Finally, it also means that they consider themselves members of a society that consists of several stable and demarcated institutions such as the state, the public and the scientific, where each institution is endowed with specific obligations.

In the second ethnographic chapter, I moved from the mode of Vocation to the mode of Oikos. Oikos justifies its actions with the responsibility for the '*business*'; that is, to enable and ensure the survival and prosperity of the group they are members of. This is mainly done by '*investing*', '*saving*' and '*maintaining*'. The responsibility for the business also has some specific consequences for the organisation. The scientists regard themselves as part of a group that is in competition with other laboratories and not part of a global community. Their time has become acutely finite; they have to produce to compete. Their scepticism regarding new potential facts has transformed into a firm belief in their own abilities to prove their hypotheses. They look at society as a variety of organisations – both public and private – that are all competing for funding and struggling for survival on a market of innovation and services.

In the third chapter, I turned from Oikos to Citizenship and, again, the two labs transformed. Citizenship justifies its actions with the responsibility for the '*public*'. By that they mean they have a responsibility to produce a form of science that is legitimate in the eyes of the (taxpaying) public. They do this by '*speaking about science*', '*listening*', and by '*doing research*'. The mode of Citizenship is less coherent than the other two, as the connections between their ideas, their ways of conducting themselves and the organisational consequences do not appear as stable as in the modes of Vocation and Oikos. These modes were easier to compare because many of their organisational consequences appeared to be the opposite of the other. It is not as easy to compare Citizenship with the other two modes, as the organisational consequences do not address themes such as '*fact making*' or '*time*' as coherently as they do. However, one consequence is that they see themselves as 'activists'; as people who have a political cause that they need to convince others about. They also regard themselves as being part of a public sector consisting of a range of different organisations that all have a responsibility to be transparent and do reasonable work for the money they are allocated. This mode also engenders a sense of guilt about doing 'basic science' among the scientists at one of the laboratories, as they have a hard time legitimating for themselves the reasons why they are producing something that cannot immediately be applied outside the lab. At the other laboratory, they seem to live up to the demands of making both commercial technologies and integrating the public's views into their work. On the other hand, the employees seem rather anxious about being dependent on temporary grants and not really having the prospects of job security and a stable income.

The three modes are negotiated in the everyday work. The scientists evaluate whether it is the responsibility for the '*truth*', the '*business*' or the '*public*' that should guide their choice of action in specific situations. A general feeling of frustration about the variety of evaluation criteria, as well as the work needed to live up to these criteria, is registered in both labs, albeit in different ways. The scientists in one lab are eager to do the science they find most interesting and relevant without too much interference, especially from politicians. The scientists at the other lab want to solve big societal challenges but feel inhibited by the resistance from both funders and the scientific community.

In what follows, I will conclude with some more general reflections about the findings and discuss them in relation to the themes of 'science governance'. The rest of the chapter is structured in three parts, drawing conclusions on the *theoretical*, *empirical* and *practical* aspects of the knowledge produced in this dissertation. In the theoretical discussion, I consider this dissertation's contribution to the field of science governance. The focus is primarily on the construction of 'Responsible Science' as an object of study rather than a normative concept. In the *empirical* discussion, I consider the findings in relation to their generalisability and in relation to my role as observer and investigator. In the *practical* discussion, I consider the implications of the findings in relation to further work with 'Responsible Science' and similar concepts at both the political and organisational levels.

10.1 ESTABLISHING RESPONSIBLE SCIENCE AS AN OBJECT OF STUDY

This study contributes theoretically to the small, but growing, field of studies that establish ‘Responsible Science’ as an object of study rather than a normative concept (e.g. Mccarthy and Kelty 2010; Fortun & Fortun 2005). Contributing to this field is important for several reasons: First, because further descriptions of the current conditions for conducting the scientific job are needed as a basis for justifying the calls for more ‘responsibility’ in science; second, because it is important that the content of these calls actually addresses the most pressing problems in current scientific conduct. My impression of the current ‘Responsible Science’ literature (e.g. Scott 2003; Brown and Guston 2009) is that much of it takes its point of departure in the idea of the scientists as actively resisting engagement due to their professional heritage. As a result of this, they resist reflecting further upon legitimacy and outcomes. Through the study of responsibility in practice, this dissertation contributes with some alternatives to this view.

Whereas many studies are focused on evaluating the outcomes of projects that work with ‘Responsible Science’ (Fisher 2007; Schuurbiers 2011), this study contributes with multiple understandings of ‘responsibility’ as they appear among scientists in their everyday work, something that has been called for on several occasions (e.g. Taebi et al. 2014; McCarthy and Kelty 2010). It answers this call in two ways. First, by establishing a typology of ideals of responsible scientific conduct based on the reading of scientific journals. This typology can be used as a starting point and guide for further empirical studies of the phenomenon. Furthermore, the four political rationalities testify to the heterogeneity and conflicts in the understandings of responsibility in science and assert the fact that no point of view on this subject is innocent; the question of ‘Responsible Science’ is inherently political.

Second, the ethnographic part of this study examines which responsibilities the scientists perceive as important to live up to in their everyday work and how they attempt to do so. An important contribution to the development of ‘Responsible Science’ as an object of study is the demonstration of the connections between ‘responsibility’ in practice and different historically situated institutions in society. The scientists’ understanding of their responsibility for ‘the truth’ share similarities with the way Weber describes the scientific profession as a disciplined pursuit of knowledge (Weber, Owen, and Strong 2004). As a way of describing science’s role in society, Vocation also reflects aspects of the ‘science-military’ complex (Shapin 2009) that combined public funding for basic science with a strong, centralised state, and with national industry as users of the knowledge and the workforce. Oikos mirrors the development of an entrepreneurial science (Etzkowitz 1998) that also developed in step with the reforms of the public sector, which emphasised competition between public service providers and a blurring of the boundaries between the public and private sectors (Boden and Nedeva 2004). But where ‘the entrepreneurial scientist’ is described as an individual who has the personal skills and mindset to seize opportunities (e.g. Etzkowitz 1998), this study points to the *need* to be good at seizing opportunities, otherwise the scientists’ ‘business’ may not survive. The scientists do not necessarily find this aspect of their work fruitful for the scientific outcomes or their professional satisfaction. And finally, Citizenship shares similarities with the calls for the ‘Responsive State’ (deLeon 1997), which developed during the nineties. While the demands for more competition and the privatisation of the public sector did not disappear with this call, they were supplemented by calls for direct accountability by users (‘customers’) and ‘the public’ (OECD 1996). Elements from these ways of governing the public sector are seen in many areas of the public sector, where ‘user involvement’ is considered as a solution to the public sector’s apparent lack of democratic accountability. The scientists have difficulties in distinguishing this demand from the responsibility for the business. Both are considered extra elements of their work that draw their attention away from the core task of producing sound research. I have made an overview of the most important elements of the three modes and the inherent ideas about science's role in society, the affect on the perception of the scientific profession and what makes the job meaningful:

	Conduct	*Justification*	*Idea about society*	*Professional Role*	*Meaningful work*
Vocation	- Checking - Repeating - Being Critical	Responsibility for the truth	Stable, autonomous institutions	Specialists	Pursuing knowledge
Oikos	- Investing - Saving - Maintaining	Responsibility for the Business	A highly competitive market	Knowledge-sellers	Creating successful organisations
Citizenship	- Speaking about Science - Listening to the Public - Doing Research	Responsibility for the Public	Institutions with dissolving boundaries and overlapping responsibilities	Activists and specialised public professionals	Helping society and taking active political stances relating to their area of research

I have described the three modes as if they were pure – as if they were performed without resistance or were never negotiated or changed. In daily work life, the three responsibilities are continuously weighted against each other in relation to specific, tangible decisions. The responsibility for the business and for the public are not easily integrated into the daily work. Both are at times considered at odds with the responsibility for the truth. Furthermore, the scientists consider it quite hard work to maintain all three responsibilities at once, even if they consider them meaningful. From a current point of view, it seems as if Vocation and Oikos are the most stable modes. They appear similar across the two labs and address some of the same organisational themes such as time and fact-making. The scientists believe that the responsibility for the business is burdensome, as it takes time and their own jobs are continuously at stake. But without a laboratory, there is no way they can engage in their vocation. So it is a responsibility that is hard to avoid. The question is whether the stability of each mode and their interrelationship will change over time and give rise to new responsibilities.

This study also draws attention to the fact that the scientists' understanding of their responsibilities depends on the kind of science they consider themselves doing. In this respect, two distinctions that mattered for the scientists' understanding of responsibility emerged from the ethnographic studies. First, that the scientists in both labs considered themselves 'public scientists' (as opposed to corporate) and that this professional identity, in being part of public service, meant that their responsibilities were regarded as being both larger and more diffuse than the way they perceived the duties of a corporate scientist. The second distinction appeared when the two labs were compared. Here, it became apparent that Curious George values 'basic science', while Gyro Gearloose takes pride in doing 'translational research', with immediate application in the medical sector. Some studies question the actual difference between basic and applied research, arguing that the lines between them are becoming increasingly blurred as much research, even in its 'basic' stage, is undertaken in the context of application (Nowotny 1999: 250). While this may be true, this study suggests that the idea of a distinction between 'basic science' and 'applied science' still has an effect associated with the kind of responsibility the scientists conceive of as meaningful in relation to their daily work. A tentative suggestion stemming from this is that science and technology studies should not necessarily abandon this way of typologising science. While it in many cases may be difficult to distinguish 'basic science' from 'applied science', the two terms carry a significant cultural meaning for scientists. They are used as a way to underline their special professional identity within the scientific field and give meaning to the work they do and their idea about science's role in society.

10.1.1 THE UNINTENDED SIDE EFFECTS OF RESPONSIBLE SCIENCE

Another discussion pertaining to the theoretical contribution of this project is the fruitfulness of including broader studies regarding the development of public service in Western societies. This study argues that the demands for accountability, responsiveness and public engagement are a specific idea of good government that has affected the entire public sector and many public sector professionals. This argument is not made in order to situate 'science governance' within the general public administration literature. It is made in order to underline the importance for science governance of using insights from literature on the development of public administration. Many of the developments within science governance can be compared to similar developments in other areas of public service such as the school system, the healthcare system and government bureaucracy. In the following paragraphs, I will make some final reflections about the perspectives of 'Responsible Science' considered in relation to the more general tendencies of government.

This study has taken its point of departure in the scientists' own ideas about their responsibilities. But as science is by no means a closed system, their ideas also reflect broader trends within both science governance and the general development of the steering philosophy within public administration. In chapter 5, I identified four government rationalities about 'Responsible Science'. One of the main differences between the rationalities is whether they rely on internal or external governance of science.

The Demarcation and the Reflexivity Rationalities rely on internal governance via professional ethics as a way of steering science. I will argue that the Contribution and Integration Rationalities instead introduce 'accountability' (see 2.1.2) to various external actors as a means of government. With the Contribution Rationality, this accountability should mainly be derived from formal audits conducted by government (and more informally by industry), where the inventions' abilities to contribute to industrial development are measured. In the Integration Rationality, accountability is secured through public dialogue with heterogeneous actors (mainly articulated as 'ordinary citizens', 'industry' and 'NGOs'). The main argument here is that the outcome will be 'responsible' as a consequence of a process of involving various actors in the process. The accountability is thus achieved through the process of dialogue. Looked upon from the perspective of the everyday work in the laboratories, these different ways of governing science are both recognised and acknowledged but, at the same time, found to be rather confusing and time-consuming. The practical work of responding to these demands takes a great effort. Time and energy that would have otherwise been used on what the scientists consider their core task, namely, that of doing research, is spent on addressing these demands. Some of the juniors become confused by the different demands and how to prioritise between responsibilities. The seniors must, to a greater extent, adapt to the circumstances but are frustrated with the amount of time they spend strategically considering the relevant evaluation criteria in specific situations.

Considered in the light of broader tendencies within public administration, this move from professional ethics to external accountability shares many similarities with developments in the rest of the public service, not least in relation to the development in the central bureaucracies. Du Gay (2009) demonstrates how a range of positions within government bureaucracy have changed due to reforms of the public sector within the last four decades in the UK – and similar trends can be observed throughout the Western democracies. These reforms have changed the 'bureaucrat' from being accountable to the state administration to being either loyal to the party in government or directly to the public through diverse governance technologies such as phone surveys or apps. Du Gay problematises this shift by pointing to the lack of focus on history and stability in public service decision-making that this shift entails, because no one is loyal to the public administration itself, but more to the opinions of shifting governments and governing trends (Du Gay 2009: 381). The same worry can be applied to science: What happens if internal ethics are rejected in favour of accountability to external auditing from various actors? What will happen to scientific outcomes and scientific jobs if the responsibility for the 'truth' and 'the scientific community' are diminished and with it the careful work that sustains it? The struggles to live up to the demands of doing 'high quality' research and the 'basic science guilt' at Curious George and the insecurity about the future at Gyro Gearloose are perhaps indicative of what could be seen as (with thanks to Ulrich Beck [1992]) the '*unintended side-effects*' of this way of governing science.

As such, this is not an argument in favour of the scientists retreating to their 'Ivory Tower' – many studies suggest that such a tower has indeed never existed (Strathern 2003). Gorm-Hansen (2011) even suggests that politicians chased the scientists up into the Ivory Tower, so they could make public reforms with the intent of getting them out. But my argument is that studies of how 'the new governance of science' actually operates on an everyday basis are an important basis for assessing the qualities of 'Responsible Science'. Especially regarding the big question of whether this form of governing solves the problems in science that it was developed as a response to. Further studies that compare the consequences of demands for 'public accountability' in other public service areas with those of public science could prove fruitful in relation to the development of 'Responsible Science'. The question that needs further attention is, What happens to both the quality of the scientific findings and the quality of the scientific job in light of the new forms of government?

10.2 GENERALISATIONS AND ROLES

10.2.1 GENERALISATIONS

As I described in the method section (4.1), the two labs that I chose had worked with a range of public engagement projects and were as such not foreign to ideas about '*Responsible Science*'. Their specific lines of research have also caused controversy, which makes them even more aware of the importance of public accountability. The question is how and to what extent the findings from this study can be used in relation to more general reflections. In this section, I will argue in which ways I believe they can.

First, the four government rationalities from chapter 5 and the three modes of responsibility echo well-known historically situated ideas about science's role in society, as discussed in 7.3.4, 8.3.4 and 9.3.3. The ideas about responsibility as they are articulated in this study are, as such, not very original. They are well known from both the history of science and the history of public administration. Thus, the contents of the government rationalities and the daily responsibilities for the truth, the business and the public may be recognised in many laboratories, even though the way in which the scientists relate to them probably varies – just as it varied between Gyro Gearloose and Curious George.

Second, the study showed that the differences between the three different modes of responsibility are perhaps bigger than the differences between the two labs, especially the mode of Vocation, which is performed in very similar ways across the two labs. That it enables such stable conducts and justifications across both disciplines and national borders testifies to the pervasiveness of this mode. There are greater differences in the ways that Oikos and Citizenship are performed in the two labs. In both modes, this seemed to be connected to the different scientific goals each lab pursues and whom they perceive the user of their knowledge to be. In contrast, the difference in national political cultures is difficult to ascertain from the perspective of the working scientists and PIs. In my view, this is not necessarily because the cultures do not have an influence. Rather, it shows some of the limits of my methodological framework. Enabling these differences to stand out would have demanded a framework that was less explorative and more focused on the specific relations between political culture and responsible science. This is another area where more empirical studies could be fruitful.

A third question pertinent to the general conclusions of this study pertains to the observed scientific disciplines. Both laboratories work in the field of biotech and more or less explicitly with synthetic biology. Curious George has moved from working with biochemistry to working with synthetic biology. Gyro Gearloose does not seem to pay much attention to disciplinary boundaries and has a highly interdisciplinary staff working according to the dictum of producing medical technologies based on immunosignaturing. Nevertheless, the focus is still mainly on advanced bio-science. Since the seventies, the field of biotech has been very active in establishing connections with industry and has fostered some of the best-known entrepreneurial scientists, such as Craig Venter. There have also been high political expectations for the field's ability to develop scientific findings into applicable technologies (Jasanoff 2005: 17). At the same time, some of the biotech technologies have been found highly controversial and public support has therefore been fluctuating. On that basis, I consider many of the ways that the labs engage with their external environments as much more active and proactive than, say, many geology or astronomy departments. That being said, I still consider the three modes of responsibility for the truth, the business and the public as rather pervasive within the institutions of science, while the specific ways of relating to these may differ. This consideration is based partly on the finding that public sector developments seem to matter greatly for the scientists' understandings of their responsibility and partly on the journal papers that were the basis for the analysis of the political rationalities on responsibility in science. Scientists from various disciplines covering the natural sciences, the humanities and social sciences authored these articles and the different viewpoints crisscrossed disciplinary affiliations.

10.2.2 IT MATTERS WHO ENTERS

In the method section, I argued that whoever enters the laboratory matters for the final product. In this section, I will make some final reflections about how my position vis-à-vis the informants has had implications for the final product, that is, the knowledge presented in this dissertation.

I had expected the scientists to be quite alienated by 'social responsibility' and perhaps even hold negative views toward such developments. But I gradually discovered that while they did have their reservations, these were usually grounded in the practical problems associated with how to apply these thoughts in their everyday work life. This initial finding led me to reconsider my position in relation to the literature on laboratory engagements. Some of these tend to present the scientists as being rather sceptical about including social concerns in their daily work due to their professional heritage – a position that I also observed and presented in the mode of Vocation. Mike Fortun sums up this attitude in science studies:

'So much of what I read in our disciplines of science studies is marked, or at least tinged, with a resentment […] *toward both scientists and the sciences, as though if they had only been more humanistic, more ethical, more responsible, or had better values to begin with, we wouldn't be faced with the 'implications' that justly preoccupy our attention.'* (Fortun 2005: 164)

So the results that I have presented in this dissertation are also motivated by a conscious decision to present a different story about scientists' everyday work than the one Fortun (2005) describes. A story that is not about the scientists' inability to be '*more humanistic*', '*more ethical*' or '*more responsible*'. Instead, the intention has been to tell a story that is partly focused on the multiplicity and political character of 'Responsible Science' and partly focused on describing the very practical aspects of being a 'responsible' professional. It was also a conscious decision to abandon the explanations of resistance and ways of interacting with society, which I already knew. I therefore decided to look at the empirical material from other angles instead. In that way, the results here are, of course, the result of the many interactions between the scientists and me. It is the scientists who have taught me so much about being responsible. But it is also the wilful decision of the ethnographer to search for and present different stories about scientific work. I could have also written a story that emphasised the more negative aspects of the interactions and given examples of hopeless misunderstandings and even insults between the informants and me. But I chose another approach. In that way, it does matter who enters. As 'Responsible Science' develops as an empirical object, more diverse studies of the phenomenon will hopefully appear, so that the inherent conflicts and facets of the phenomenon become visible.

10.3 FURTHER WORK WITH 'RESPONSIBLE SCIENCE'

The last theme I want to touch upon relates to the practical implications of this study. The empirical findings also establish momentum for considering how to move forward with the framework for 'Responsible Science' at the political level.

First, one of the main points from this study is that scientists reflect quite a bit upon their responsibilities toward society, but it takes a large amount of work to both navigate the complex set of expectations they face and to actually live up to them. The scientists will at times reject their responsibility to, for instance, work on obtaining public accountability for their plans. This is not necessarily because they are oblivious to the importance of doing so, both from a professional and a democratic perspective. In many instances, it is because they simply do not consider it their main responsibility and do not have time for it. If the work with 'Responsible Science' is to be a success in practice in the actual laboratories, it seems important that the theme be very closely interwoven with the scientists' daily work and very closely related to their theme of research, so they can conceive of it as a meaningful endeavour in relation to their other work obligations.

A second point stems from the observation that the kind of science the scientists do matters for the way they perceive of their responsibility. Science is a complex institution with a multitude of different goals and disciplines. Therefore, it seems pertinent that a certain amount of flexibility is introduced into the work with 'Responsible Science'. I showed how some scientists even experience guilt about conducting 'basic science' because they consider it illegitimate not to develop something that can be applied outside the lab. Feeling guilty about one's work is not very productive, neither for the hardworking scientists nor for the scientific work itself. While recent demands for 'Responsible Science' are not solely responsible for this development, it does perhaps enforce the guilt because demands for both legitimacy and commercialisation seem to be inherent in the calls for responsible science (Kearnes & Rip 2009). To counteract these developments, it seems to be a good idea to introduce flexibility into the concept. By that I do not mean a scale going from 'less' to 'more' responsible, which the scientists can choose from. Rather, it seems that they should be able to argue for the responsibility of their actions from a range of different perspectives that make sense in relation their exact niche and eventual end-users.

This leads me to the third point, namely, that it is important that the ideas of 'Responsible Science' make sense in relation to the scientists' daily work. This means that it is acknowledged that work with public engagement takes time and energy from other responsibilities; that the demands to be more 'responsible' are recognisable in relation to the actual work done in the laboratories and – perhaps most significantly – it is important to help with the confusion the scientists feel in relation to the very different evaluation criteria of their work. In that way, the role of the social sciences is perhaps not so much to underline the importance of ethical and social aspects of science, but more to help the scientists navigate and make sense of their many work tasks.

The very last point that I want to make is that it is also important to notice the shift from internal ethics to the external demands for accountability, which is part of the demands for 'Responsible Science'. While the intentions of this shift are very noble, it is still an open question as to whether more participation and inclusion in the government of science will lead to a more responsible, or perhaps even sustainable, science. Only further empirical work with 'Responsible Science' can answer that question.

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