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Epistemologies of Uncertainty

Governing CO$_2$ Capture and Storage Science and Technology

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Thesis presented for the degree of

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within
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Lay summary

This thesis uses perspectives developed in the social science tradition of ‘science and technology studies’ (STS) to consider how experts think about and present uncertainties attached to scientific research, technology development, and the regulation of carbon capture and storage (CCS). CCS has been proposed as a carbon mitigation technology system that may significantly contribute to the decarbonisation of energy generated from fossil fuels, and several research publications and governments have presented it as an important public policy objective. However, research and development is still at very early stages and many uncertainties remain. This thesis asks how experts’ perceptions of key uncertainties compare with the evidence basis presented in regulations and in policies that bear on CCS development. I end with some proposals for rethinking policies and regulations in light of my findings from interviews and analyses of scientific research and regulations. The thesis thereby aims to contribute to STS literature by extending theoretical questions into new empirical material and by making proposals for rethinking how CCS science and technology should be governed in years ahead.
Declaration of own work

\(a\) this PhD thesis has been composed by me, Benjamin Evar;

\(b\) where other individuals contributed to part of the work, this has been clearly indicated at the start of a chapter;

\(c\) work herein has not been submitted for any other degree or professional qualification except as specified.
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List of acronyms and abbreviations

CCGT combined cycle gas turbine
CCS carbon capture and storage
CDM Clean Development Mechanism
CEGB Central Electricity Governing Board
CFC chlorofluorocarbons
CO$_2$ carbon dioxide
DECC Department of Energy and Climate Change (UK)
DOE Department of Energy (US)
DTI Department of Trade and Industry (UK)
EEPR European Energy Programme for Recovery
EMR Energy Market Reform
EOR enhanced oil recovery
EPA Environmental Protection Agency (US)
EPS emissions performance standard
ETS emissions trading scheme
EU COM European Commission
EU ETS European Union Emissions Trading Scheme
FGD flue gas desulphurisation
GCCSI Global CCS Institute
GDP gross domestic product
GHG greenhouse gas
GHGT International Conference on Greenhouse Gas Technologies
Gt gigatonne
GW gigawatt
GWh gigawatt hour
GWP global warming potential
HM Government Her Majesty’s Government (UK)
IEA International Energy Agency
IEAGHG IEA Greenhouse Gas RD&D Programme
IPCC Intergovernmental Panel on Climate Change
IPPC Integrated Pollution Prevention and Control Directive
KLIF Norwegian Climate and Pollution Agency
kWh kilowatt hour
MPE Ministry of Petroleum and Energy (Norway)
MtCO$_2$ million tonnes of CO$_2$
MW megawatt
NDMA n-nitrosodimethylamin
ng/m$^3$ nanograms per cubic metre
NGO non-governmental organisation
NILU Norwegian Institute for Air Research
NIPH Norwegian Institute of Public Health
NMF Norwegian Ministry of Finance
NMTI Norwegian Ministry of Trade and Industry
NOK Norwegian Krona
NOx nitrogen oxides
NPD Norwegian Petroleum Directorate
OECD Organisation for Economic Co-operation and Development
OSPAR Oslo-Paris Convention for the Protection of the Marine Environments of the North-East Atlantic
REACH EU Regulatory Framework for the Management of Chemicals
REV representative elementary value
RCEP Royal Commission on Environmental Pollution
RCT rational choice theory
R&D research and development
SACROC Scurry Area Canyon Reef Operators Committee
SBSTA UNFCCC Subsidiary Body for Scientific and Technological Advice
SCCS Scottish CCS
SCOT social construction of technology
SNA social network analysis
SO₂ sulphur dioxide
SSK sociology of scientific knowledge
SST social shaping of technology
STS science and technology studies
TCM Test Centre Mongstad
tCO₂ tonne of CO₂
UNFCCC United Nations Framework Convention for Climate Change
WWF Worldwide Fund for Nature
ZEP Zero Emissions Platform
Abstract

This thesis progresses from a ‘science and technology studies’ (STS) perspective to consider the ways that expert stakeholders perceive and communicate uncertainties and risks attached to carbon dioxide (CO$_2$) capture and storage (CCS) research and development, and how this compares with policy framings and regulatory requirements. The work largely falls within the constructivist tradition in sociology, but also draws on literature from the philosophy of science and policy-oriented literature on risk and uncertainty.

CCS describes a greenhouse gas (GHG) mitigation technology system that involves the capture, pressurisation, transportation, geological injection and long-term storage of CO$_2$ as an alternative to atmospheric emissions. Only few and relatively small applications exist at the moment and research efforts are ongoing in many countries. The case for developing CCS towards large-scale, commercial deployment has largely been presented as follows since the mid-1990s: climate change mitigation is the developed world’s historical responsibility and must be addressed urgently; chief amongst GHGs is CO$_2$, which makes up more than three quarters of emissions; the vast majority of CO$_2$ is emitted from the combustion and gasification of hydrocarbons – oil, gas and coal – for energy generation; transitioning away from these high-CO$_2$ primary energy sources will likely take several decades at the least; therefore, CO$_2$ capture systems should be designed for power and industrial emissions in developed countries, as well as emerging economies where energy suppliers will continue to construct relatively cheap and well understood high-CO$_2$ generation plants.

The development of large-scale CO$_2$ capture has thus arisen from a concern with engineering a technological system to address a CO$_2$ legacy in the developed world, and a high-CO$_2$ trajectory in developing/emerging countries, rather than on the back of purely scientific curiosity. And the potential for large-scale
development has been presented on the back of a variety of scientific and technical evidence, as well as the urgency of the policy objective and related aims. Research activities, often concentrated around technology demonstration projects, are the primary focus of the first part of this thesis. In the second part I consider the extent to which research has shaped policy developments, and how regulations have subsequently informed a more detailed research agenda.

I follow a ‘grounded theory’ methodology as developed by Glaser and Strauss (1967) and take additional guidance from Glaser’s (1992) response to Strauss’ later writings as well as Charmaz (2006) and Rennie (2000), and use a mix of qualitative and quantitative analytical methods to assess my data. These include information from 60 semi-structured interviews with geoscientists and policy stakeholders; close readings of scientific publications, newspaper articles, policies and regulatory documents; statistical evidence from a small survey; quantitative analysis of newspaper articles; and social network analysis (SNA) of scientific co-authorship networks.

Theory is drawn from STS literature that has been appropriate to address case study materials across each of the 7 substantive chapters. The first section of the thesis considers expert claims, with a focus on geoscience research, and draws on literature from the closely related ‘social shaping of technology’ (SCOT) and ‘sociology of scientific knowledge’ (SSK) programmes, as well as Nancy Cartwright’s philosophy of science. The second half of the thesis draws on the ‘co-production’ framework and Wynne’s (1992) terminology of risk and uncertainty, to assess relations between risk assessment and risk management practices for CCS. I likewise draw on literature from the ‘incrementalist’ tradition in STS to ask whether and how understandings of technology risk, governance and deployment could be improved.

Each chapter presents new empirical material analysed with distinct reference to theories covered in the introduction. Chapter 2 provides a general overview of the history, technology, economics and key regulatory issues associated with
CCS, which will be useful to assess the theoretically driven arguments in subsequent chapters. Chapter 3 draws on the concept of ‘interpretive flexibility’ (Pinch and Bijker 1984) to assess a range of expert perceptions about uncertainties in science, technology and policy, and I develop a substantive explanation, ‘conditional inevitability’, to account for an epistemic tension between expressions of certitude and the simultaneous acknowledgement of several uncertainties. Chapter 4 continues the enquiry into stakeholder perceptions and draws on Haas’ notion of ‘epistemic communities’ (Haas 1992) to assess geoscientists’ work practices. I complement this framing with a close look at how uncertainty is treated in simulation modelling and how conclusions about storage safety are formulated, by drawing on Nancy Cartwright’s philosophy of science (Cartwright 1999) and Paul Edwards’ account of complex system modelling for climate change (Edwards 2010). The chapter shows how shared understandings of adequate evidence and common analytical tools have been leveraged to present relatively bounded and simple conclusions about storage safety, while geoscientists nevertheless recognise a high degree of uncertainty and contingency in analyses and results. Chapter 5 continues the focus on knowledge production in the geosciences and is supported by SNA data of workflow patterns in the Sleipner demonstration project. The analysis shows how a few actors have had a pivotal role in developing insights related to storage safety particularly on the back of seismic monitoring and other data acquired through industry partnerships. I therefore continue the chapter with a deconstruction of how seismic data has been used to make a case for the safety of CO₂ storage, again drawing on Cartwright and others (Glymour 1983) to explain how individual findings are ‘bootstrapped’ when conclusions are formulated. I show how a general case about storage safety has emerged on the back of seismic data from Sleipner as well as a shared understanding among geoscientists of how to account for uncertainties and arrive at probable explanations.

Chapter 6 considers to what extent scientific research has given shape to, and in turn been shaped by, CCS policy and regulations in the EU, drawing on Wynne’s
(1992) terminology of risk and uncertainty as well as legal scholarship (Heyvaert 2011). I conclude that a ‘rational-instrumental’ interpretation of uncertainty and precaution has furnished a compartmentalised understanding of risk assessment and risk management practices. Chapter 7 continues to look at the ways that risk assessment methodologies influence risk management practices through a case study of the Mongstad CCS demonstration project in Norway. I draw on ‘incrementalist’ literature (Lindblom 1979; Woodhouse and Collingridge 1993) to consider alternative conceptualisations of technology development and risk management when expectations clash with scientific uncertainties and criticism. Chapter 8 draws on insights from across STS (Downs 1972; Collingridge and Reeve 1986; Wynne 1992) to create a novel conceptual model that accounts for recent years’ developments in CCS governance. Here I conclude that setbacks and criticisms should be expected when analyses have largely presented CCS as a technical problem rather than a socially contingent system. Following Stirling (2010) I conclude that scientists and policymakers should instead strive to present complexity in their analyses and to engage with wider publics (Yearley 2006) when technical analysis is inseparable from socially mediated indeterminacies (Wynne 1992), to increase the chance of more successful engagement practices (Wynne 2006). The conclusions at the end of the thesis seek to draw out interpretive and instrumental lessons learned throughout.
1. Introduction to the thesis

Background to the work

This thesis arose from my curiosity around the way that experts understand and talk about risks associated with carbon dioxide capture and storage (CCS) and how such perspectives have influenced risk assessments. As time passed I increasingly focused on the connection between such assessments and risk management strategies and I therefore found myself working with a tripartite thematic structure: I wanted to understand how expert beliefs were related to an underlying (scientific) knowledge base; I wanted to ask to what extent these perspectives were reflected in risk assessments for regulation; and I wanted to assess what such regulations and the surrounding policy context implied about, and for, risk management and governance. In order to deal with these different themes each chapter draws on different theories and analytical methods to analyse a selection of independent case study materials. In this introduction I aim to explain how this mixed methods approach has helped me reach a number of new insights about governance of science and technology related to CCS.

My variegated approach arose partly because I have followed a grounded theory methodology, explained in detail towards the end of this introduction, and because I initially conceived of the thesis as several publications. Chapters 2, 3, 6, 7, and 8 thus all draw extensively on research published during the PhD process. The other motivation behind this structure was an early realisation that CCS related governance, as for so many emerging technologies, is based on scientific output that remains at an experimental stage. In order to address major questions about funding and regulation it therefore seemed appropriate

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1 There are numerous definitions of ‘governance’ in use across distinct academic literatures. See for example Lyall and Tait (2005) for a range of understandings and case studies by academics from science and technology studies and policy science. I use the term in its widest sense with respect to science and technology and follow e.g. 6 (2005) in considering mechanisms of control (generally regulations), inducement (taxes and subsidies), influence (of individuals, organisations, arguments and ideas) and coping (reactions and strategies for survival).
to consider how fundamental scientific ideas and findings had shaped the governance agenda, and one possible approach was to ask how a selection of different detailed investigations could support each other in drawing some major theoretical conclusions. These conclusions are both interpretive and instrumental and draw on a variety of literature from the science and technology studies (STS) tradition. *Ultimately, the primary concern of this thesis has been to understand how scientific research is produced and presented by the scientific community, as well as used in and conditioned by technology policies and regulatory frameworks.* I have therefore asked what a wide range and a more specific group of experts (geoscientists) believe about risks and uncertainties that bear on CCS and how they have reached these beliefs. And I have asked how this knowledge and the institutions that they are represented by have informed policy and regulations and helped to shape the governance of CCS among a variety of other social forces.

**Policy background to CCS**

Because this thesis investigates knowledge claims associated with large-scale carbon dioxide (CO₂) mitigation technology systems, the research themes and materials are inextricably linked with a policy momentum for climate change mitigation. At the highest levels of government this takes the form of a mandate for national level climate change mitigation activities as required under the United Nations Framework Convention for Climate Change’s (UNFCCC) Kyoto Protocol introduced in 1997. The Protocol seeks to restrict the emissions of greenhouse gases (GHG) to the atmosphere and in 2009 signatories to the Protocol adopted a target long used to guide European policymaking of limiting an increase in the mean global surface temperature below two degrees Celsius relative to pre-industrial levels. Signatory countries to the Protocol are either allocated national emissions reduction targets or voluntary adopt them (in some cases emissions are allowed to increase to facilitate continued economic development), which may be reached with an agreed mix of domestic reductions and payments in exchange for reductions achieved in other
countries. For a detailed examination of economic and policy implications of the Kyoto Protocol see Carraro (2000) and Nordhaus (2010).

It is within this context that CCS developed as a research theme in GHG mitigation policies during the 1990s. Although individual technology components required for the capture, transport and geological injection of CO₂ had been around for several decades, it was only within the context of global climate change mitigation that momentum was generated to develop a CCS system beyond a speculative research programme. And the case for developing it to maturity has largely been laid out as follows by pro-CCS organisations and supportive policy institutions: climate change mitigation is the developed world's historical responsibility and must be addressed urgently; chief amongst GHGs is CO₂, which makes up more than three quarters of emissions; the vast majority of CO₂ is emitted from the power sector’s reliance on hydrocarbons – oil, gas and coal; some of these emissions may be mitigated by making power generation and usage more efficient, by increasing our reliance on renewable and nuclear energy, and by making lifestyle changes that result in a lower per capita energy demand; however, hydrocarbon combustion will likely not be phased out completely as it is a tried and tested technology, hydrocarbon stores are plentiful for the foreseeable future, they provide relatively cheap sources of energy and help to meet base load energy generation requirements; in addition, society is now so fully locked in to its reliance on hydrocarbons that it is extremely unlikely we will undergo a transition to a low-carbon society.

2 Including the Global CCS Institute, the IEA and the EU Commission.
3 Worldwide, CO₂ emissions constitute around 80% of annually emitted anthropogenic GHGs when compared on a CO₂-equivalent basis of global warming potential (IPCC 2007).
4 Coal combustion alone is responsible for 43% of these (IEA 2010b).
5 The peak oil movement has long maintained that global stores of oil will be depleted within the foreseeable future. Whether or not the reader is inclined towards this position, it has become increasingly clear that coal and gas may function as effective substitutes for oil in years to come, a transformation that would primarily affect emissions from the transport sector. Gas and coal outweigh oil for power generation in most developed and large emerging countries and plentiful supplies are now thought to be available with the rapid growth of the shale gas industry in North America and elsewhere in recent years, and the abundant proven supplies of coal globally (IEA 2010b).
6 ‘Carbon’ and ‘CO₂’ are used interchangeably throughout this dissertation unless otherwise specified.
within a few decades\(^7\); therefore, technologies that capture anthropogenic emissions of carbon are required; these may include systems that capture it from air, soil and from the flue gases of power and industrial plants; however, only technologies in the latter category are deemed to be cost-effective at this time\(^8\), partly because of the relatively high concentration of carbon in the flue gases of combustion plants; therefore, carbon capture systems should be designed for power and industrial emissions in developed countries, as well as emerging economies (China, India, Brazil, etc.) where the energy sectors will continue to rely on high-carbon generation for decades to come\(^9\).

The case for developing large-scale CO\(_2\) capture has thus arisen from a concern with engineering a technological system to address a carbon legacy in the developed world, and a high-carbon trajectory in developing/emerging countries, rather than on the back of purely scientific curiosity. And in political circles CCS has strategically been compared with the Apollo lunar programme (Stoltenberg 2007). But in contrast to a push for entirely novel solutions generated through mission-oriented research, most of the basic engineering designs have been borrowed from well-established technologies. The scientific and technological basis for individual components in CCS, such as storage, similarly draws on a host of knowledge from traditional hydrocarbon activities and from core theoretical concepts and studies in the geosciences. This knowledge has been fundamental to research activities in CO\(_2\) injection and storage demonstration projects and a host of smaller pilot studies around the world.

\(^7\) For an elaboration of this argument see Unruh (2000, 2002), and Unruh and Carillo-Hermosilla (2006).

\(^8\) Avoided deforestation and afforestation as a means of carbon mitigation may be developed at relatively low cost (Kindermann et al. 2008), but face challenges related to long-term stewardship, pressures from commercial land development and inadequate global financial mechanisms.

\(^9\) Coal has an extensive legacy in industrialised nations (Chick 2007) and is increasingly the fuel of choice for base load energy generation in the emerging economies of China and India (IEA 2010b). By 2030 CO\(_2\) emissions from coal-generated energy may therefore increase by as much as 40% compared with current levels under existing climate change policies (Ansolabehere et al. 2007).
The CCS research and policy agendas are the starting points for this thesis’
enquiry into the beliefs of CCS experts at large, geoscientists specifically, and
how reflections on risk in research and technology development present
alternative governance options. These issues are described in more detail in an
overview of the thesis structure below, following a review of the theoretical
literature drawn on to interpret empirical material in each of the chapters.

**Social actors in science and technology**

Several theoretical approaches in sociology and policy science deal with
questions of how and why perceptions about appropriate social policies (for
environmental matters, technology development, etc.) are formed and change.
Academics from the camp of rational choice theory (RCT), which includes most
neo-classical economists, have made several contributions to this area of
inquiry. RCT generally assumes that individuals make decisions with a view to
maximising the utility of an outcome in their favour, and that social policy
should similarly be made with a view to maximising utility for society. This
perspective has been applied in analyses of management decisions (Ansoff
1965), public policy (Ostrom 2007) and wider social relations (Hechter and
Kanazawa 1997). The admittedly broad school of RCT assumes that choices can
be analysed according to the interests that they support. An example of what
this implies for methodological work, is the tendency to reduce actors’
perspectives to quantitative preferences in economic exchanges (Friedman
1953). The reliance on neo-classical economic analysis as a basis for social
policy is testimony to the influence of RCT, and by extension utility theory.
Debates about the desirability of environmental policy initiatives, and more
specifically climate change mitigation, have likewise been dominated by
economic cost-benefit analyses such as the the *Stern Review* on the economics of
climate change (Stern 2006) and the *Garnaut Review* on the impacts of climate
change in Australia (Garnaut 2008).
While all methodological frameworks by necessity employ simplifying parameters to create categories Green and Shapiro (1994) have criticised RCT for applying utility theory too liberally, with scant attention to empirical accuracy. This criticism is hard to avoid where RCT is relied on to analyse macroeconomic and macro-social tendencies and seeks precisely to generalise, in order to inform at the greatest distance. Boudon (2003) has formulated three separate criticisms. Firstly, with questions about complex social phenomena RCT fails to account for differentiated belief systems. In other words, accounts of beliefs underlying actions do not progress beyond the tautology that individuals are motivated by self-interest. Secondly, RCT cannot account for the allure of non-consequentialist prescriptive beliefs, i.e. when individuals perform actions well knowing that no material change will follow. Finally, RCT does not help explain why some opinions exist that arguably cannot be said to spring from self-interest, i.e. where an outcome has no effect whatsoever on the direct material or emotional wellbeing of an individual10 (Boudon 2003). The first of these criticisms may rightly be levelled at normative research that relies on reductive methodological frameworks. Analytic research on the other hand seeks precisely to generalise and turn individual relations into simplified economic preferences for the purpose of choosing between a limited set of policy actions. The second criticism rightly notes that RCT analyses often focus on material wellbeing. However, the implication that a methodological framework in itself can delineate what constitutes a non-consequentialist belief is at odds with interpretive flexibility, a theoretical basis of my thesis as explained below. The third and final criticism is problematic because it again conflicts with interpretive flexibility by implying the existence of a universal parameter for qualifying consequential material changes.

Bearing such criticisms of RCT in mind I have aimed to emphasise empirical accuracy and to remain open to the possibility of a multiplicity of motivating factors and explanations of beliefs. I have therefore been motivated to address

10 Both the second and final criticisms may be seen as variations of the first, namely that RCT fails to account for decisions and beliefs that cannot lead to consequential outcomes in individual wellbeing.
my research questions from the perspective of ‘science and technology studies’ (STS), which embraces a variety of foci and explanatory models about stakeholder perceptions of scientific uncertainty, technology controversies, and the role of science in public policy. Often associated with sociology, STS studies also draw on the philosophy and history of science, anthropology, and economics. Rich case studies are often at the centre of STS research and so are discussions of epistemological implications (and ontological assumptions) when science is asked to play a role in public policy. This thesis incorporates a number of these literatures to construct a multifaceted and coherent argument across the case studies. Chapter 3 relies on sociologically driven insights about belief structures (Pinch and Bijker 1984; Hansson and Bryngelsson 2009) to draw conclusions about expectations for technology development and perspectives on policy certainty amongst a wide set of CCS experts. Chapters 4 and 5 build on this and provide more detailed perspectives on CO₂ storage amongst geoscientists, by drawing on accounts of complex system modelling (Edwards 2010) and scientific concepts according to the philosophy of science (Cartwright 1999). Chapter 6 asks to what extent the regulation of CO₂ storage has followed from these beliefs by considering the implications of risk framings (Wynne 1992). Chapters 7 and 8 ask what happens when uncertain science emerges in contested policy contexts, where support and opposition are unpredictable and advances in research nevertheless remain desirable objectives (Collingridge 1980; Stirling 2010).

The thesis draws on a variety of theories to support the case study materials and care has been taken to ensure that these ideas do not conflict with each other and help to advance understanding around the main questions. While the thesis largely falls within the constructivist tradition in sociology a few sources such as Nancy Cartwright’s the *Dappled World* are not directly included in this broad church, but do not directly conflict with it either.¹¹ I draw on insights from STS that first emerged with the ‘strong programme’ in ‘the sociology of

¹¹ Cartwright in fact is appreciative of sociological accounts of scientific practice for raising challenges to what she calls *fundamentalist tendencies*, which is also the aim of her own philosophy of science.
scientific knowledge’ (SSK), initiated in Britain during the late 1970s. The SSK approach was original in its aim to study the scientific sphere not only as an historical and economic institution, but also as a social enterprise that gave way to the formation and acceptance of ideas rooted in social procedures and patterns (Bloor 1973, 1999). Others extended this idea into detailed studies of scientist and machine interactions using ethnographic research methods (Latour and Woolgar 1979). Such analysis paved the way for social research focused on technology developments and innovations under the ‘social construction of technology’ (SCOT) programme. By dispensing with assumptions that saw technology development as a linear and deterministic outcome of fundamental science and *a priori* demands, SCOT research placed the negotiation of visions at the centre of emerging technologies. Thus, differentiated perspectives on system configurations and end-goals are encapsulated in the term ‘interpretive flexibility’ progressed by, among others, Pinch and Bijker’s (1984) seminal article on the importance of recognising user perceptions to the early development of bicycle designs. Their account shows that the dominant design we know today was not intended from the beginning and instead arose from conflicting ideas about the aim of cycling. Interpretive flexibility suggests that the value – ethical as well as instrumental – of an emerging technology will vary depending on the perspective that we consult and that the final configuration of components will often arise through a lengthy, indeterminate and implicit process of negotiation. This implies that the formative stakeholders are not determined a priori, but rather defined over time. One lesson for emerging technologies is therefore to keep discussions of potential benefits from development open, in order to invite wider participation in shaping dominant designs.

The idea that technological development trajectories are neither determined by obvious social needs nor apparent from fundamental scientific research, also laid the foundation for the ‘social shaping of technology’ (SST) (MacKenzie and Wajcman 1985) programme, which may be described as a broad school of thought that rejects technological and social determinism when attempting to
account for developments in science and technology (Williams and Edge 1996). SST-inspired research widely adopts the challenge to linear accounts as explanatory models for the relationship between science and technology. This has led some scholars to focus on the closure and flexibility implied by different technology development trajectories (Arthur 1989; Collingridge 1980, 1992) while others have focused on expectations attached to emerging technologies (Bazerman 2006; Borup et al. 2006; Lahsen 2005; MacKenzie 1999; Utgikar and Scott 2006; Woolgar 1994), the role of public demonstrations (Collins 1988; Russell et al. 2012; Shapin and Schaffer 1985), and the complexity of social interactions in innovation processes (Geels 2005a, 2005b).

Within these related strands of work there remains a wide scope for debate about ontology, while more agreement exists around the epistemology of scientific research. By proposing that the validity of emerging research and the shaping of technology are mediated through a process of social legitimacy, SSK and SST research give way to what Collins and Yearley (1992) have called ‘methodological relativism’ as a means of investigating epistemology. This proposes that closure in scientific and technological developments is contingent on agreement (explicit or tacit) between actors and that the basis for stability and consensus (or lack thereof) should be investigated as a reflexive social enquiry.

**Common beliefs and epistemologies**

Consistent with the position that scientific practices and technological artefacts are constructed through particular social settings, I have studied geoscientists as a group of people with a common belief system rooted in particular expert knowledge (chapters 4 and 5). In doing so I draw on the work of sociologists who have considered the relevance of shared epistemologies within expert communities (Adler and Haas 1992; Haas 1989, 1992; Knorr Cetina 1999, 2010; Lovell and MacKenzie 2011) under the heading ‘epistemic communities’. Following Haas, a group of individuals exist as a coherent thought community if
they display consensus, either explicitly or implicitly, across four forms of belief. Members must share 1) normative and principled beliefs, 2) causal beliefs, 3) validity standards, and 4) a common policy enterprise. As an example, expert knowledge about climate change can likewise be studied as a socially constructed phenomenon by analysing peer-review processes, scientific judgment, economic valuations, and the role of social science in climate modelling (Yearley 2009).

The Intergovernmental Panel on Climate Change (IPCC), which amongst other publications has released the *Special Report on CCS* (2005), has previously been described as constituting a set of differentiated epistemic communities (Shackley 2001), on the basis that its transnational network of scientists and knowledge-brokers claims political authority and authenticity through recourse to best available science, while working according to a variety of standards and practices. Furthermore, individual participants have been described as motivated by shared causal and normative beliefs around anthropogenic CO₂ emissions and mitigation policy resolutions (Bulkeley and Newell 2010; Shackley and Wynne 1995). The international CCS community has likewise been described as an epistemic community based on content analysis of scientific and general readership publications (Stephens *et al.* 2011) and participant lists from the large biannual International Conference on Greenhouse Gas Technologies (GHGT) (Stephens and Liu 2012). There is no doubt that CCS has attracted people with diverse backgrounds in the sciences and that several industrial actors have thrown their support behind research programmes, which have outwardly received major support from policymakers. However, as Stephens *et al.* acknowledge, analysis of micro networks is desirable to investigate this claim at smaller scales. CO₂ storage scientists may be defined as part of the CCS community and the much larger community of climate scientists, but it is instructive to ask to what extent Haas’ theory holds at smaller levels of organisation and communication where detailed questions about practices, evidence and beliefs arise. This is explored in chapters 4 and 5, which consider the use of, and findings arising from, different modes of inquiry.
in the geosciences – simulation modelling, established theory, and empirical findings from field studies. Following the theoretical literature described above, the aim has been to understand whether selected techniques have come to constitute shared forms of practice that support commonly held beliefs about CO₂ storage and associated risks and uncertainties. I support my analysis with reflections on the implications of a limited set of empirical findings about the risk of accidents in complex technology systems (Perrow 1999, 2007) and the tendency for people to support erroneous assessments (Koriat et al. 1980; Fischhoff et al. 1977; Smil 2000; Utgikar and Scott 2006) first presented in chapter 3.

Scientists’ understandings of the uncertainty that surrounds these forms of knowledge production are therefore fundamental to the thesis. So is the practice of relying on a variety of evidence in making the case for CO₂ storage: the occurrence of natural CO₂ stores has been used to argue that it will stay buried underground for millennia; large geological storage volumes have been located that are thought to provide adequate containment; decadal experience with traditional hydrocarbon extraction is referred to as evidence that injection processes can be controlled; monitoring tools have been used to show that the migration of injected volumes largely conforms with established beliefs; fluid-flow models have been developed to simulate future migration patterns; and geochemical models have been relied on to assess long-term reactions in the geology. These findings constitute the *bricolage* of ideas that are referenced to argue for on-going support of research on, and policies in favour of, CCS.

In analysing a selection of this body of work I draw on Paul Edwards’s (2010) account of the history of climate modelling. Edwards’s work provides a useful basis for comparing the rigour of scientific research and the institutional practices in large collaborative geoscience research projects. I also draw on Nancy Cartwright’s philosophy of science as developed in her later work the *Dappled World* (1999). She is here primarily concerned with the role that
modelling plays alongside other knowledge generating ‘tools’ that mediate between the world and our imperfect understanding of it:

[O]ften we must combine both knowledge and technical know-how from a large number of different fields to produce a model that will agree well enough on the matter we are looking to predict, with the method of combination justified at best very locally...The point is that the claims to knowledge we can defend by our impressive scientific successes do not argue for a unified world of universal order, but rather for a dappled world of mottled objects.

(Cartwright 1999: 10)

To Cartwright, scientific enquiries rely on tools such as models to provide precise descriptions of limited aspects of object dynamics under highly constrained circumstances. And this process, she says, argues

...for the truth of some very concrete, context-constrained claims, the claims we use to describe their behavior and control them. But in all these cases of precise control, we build our circumstances to fit our models. I repeat: that does not show that it must be possible to build a model to fit every circumstance.

(Ibid: 34)

Under idealised model conditions, knowledge about our world follows from assumptions generated in controlled settings. However, it does not follow that we can therefore build reliable and accurate models of all imaginable real world circumstances. Cartwright’s position is to ask that we do not afford a special role for models in science as purveyors of truth about the world. Just as a ‘working day’ is an abstract term often constituted of several very different and often disjointed concrete activities, she suggests that we should understand models as concrete instances of abstract scientific laws.

Others have likewise noted the fundamental role that simplifications and assumed relations for complex interactions have in ordering laboratory model settings (Rouse 2008) where a web of simplified contingencies provides a means of concretising abstract relations and forcing constrained observations
from phenomena (Hacking 1983). As for the authority of models as communicative devices, Yearley (1999) notes that the public at large will tend to evaluate results within a wider social context including trust in policymakers and assessments of social assumptions underlying models. Cartwright and others (Morgan and Morrison 1999) have focused primarily on how models are used by scientists as a ‘tool in the box’ of inquiry along with theory and data collection, to force discrete explanations to arise. To have greater faith in models than this is to espouse a fundamentalism about scientific enquiry and the quality of law-like relations that they portray. And to espouse a fundamentalism about the necessity of law-like relations as revealed through scientific tools of enquiry, is to choose an ontological position that runs counter both to relativism and to realism as defined within this branch of the philosophy of science. Rather, law-like relations about the world only ever hold ceteris paribus to Cartwright.12

What, then, does she suggest as an alternative to understanding the relationship between scientific enquiry, observations of regularly occurring phenomena, and ultimately the definition of any law-like relations? Such relations hold only as the outcome of a ‘nomological machine’: “What is a nomological machine? It is a fixed (enough) arrangement of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operation, give rise to the kind of regular behaviour that we represent in our scientific laws” (Cartwright 1999: 50). Cartwright thereby suggests that observations of phenomena under non-shielded conditions, i.e. in the real world, will yield results that should not be described as perfectly law-like, but rather as observed instances of regularly occurring capacities13 of some entities under particular circumstances. Using the metaphor of moral reasoning, she argues that just as morals are generally not applied as dogmatic laws, we should moderate our view of relations between limited observations and general

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12 This ontological position, or rather Cartwright’s challenge against an ontological position that runs counter to our experience of scientific inquiry, is consistent with much of the literature in SSK and SST and supports the epistemological position implied by methodological relativism.

13 Interchangeably referred to as natures.
descriptions by adopting her idea of capacities. This would allow us to adopt and dispense with descriptions of objects more easily than is possible under the concept of scientific laws.

In some respects this is reminiscent of Kuhn’s model of scientific revolutions (1962). Kuhn also suggests that scientific theories and techniques of inquiry should be open to revision when ‘normal science’ cannot account for new findings. However, Cartwright’s notion of capacities is both more radical and less disruptive to conventional practices. Her view that we should dispense with the concept of laws in favour of capacities facilitates a less cumbersome revision of our ideas. And crucially, she suggests that it accords better with the pattern of discovery in much of modern experimental science, which progresses by ‘bootstrapping’ (Glymour 1975, 1983) individual findings; by suspending scepticism that should logically bear on successive stages of experiments and instead to rely on several contingent assumptions to shore up conclusions. From this experimental approach it follows that models and evidence from other modes of inquiry at best only reveal individual instances of concretised law-like relations. This leads her to applaud efforts in the sociology of science, which suggest that research about scientists and their beliefs should progress as an analysis of conventional practices that arise through social interactions. Closure around beliefs, even those that involve law-like relations, is therefore ultimately a socially contingent phenomenon in Cartwright’s philosophy of science.

This philosophical position has proven a fruitful epistemological basis for social research to address the practices, products and pronouncements of geoscientists engaged with projects where work continues to substantiate a generalisable evidence basis. Results from a few techniques and technologies that use data from the longest running CO₂ storage demonstration site, the Sleipner project in the northern North Sea, have been central in crafting these views. In particular, continuous 3-dimensional seismic monitoring, also known as 4D seismic monitoring, as well as fluid-flow and geochemical modelling, have
been central to these activities. In chapter 5, I show that much of the understanding that arises from such observations emerges from the ‘tacit’ knowledge (Polanyi 1958) of geoscientists trained at making sense of empirical observations in the light of theory. Polanyi’s notion of tacit knowledge stands in contrast to accounts of specialist knowledge as codified ‘prescriptions’ that are open to replication through language. Thus Fodor’s (1968) critique of tacit knowledge, which he argues is crude because it fails to distinguish between an adequate and a skilled performance, and fundamentally wrong because it fails to acknowledge that if an explanation of an act is possible – however long and complicated it may be – it should also be possible to translate it into language and expect that a machine would be able to copy it. This implicitly assumes that machines are or will be available to carry out any task that humans are capable of, a question that goes far beyond this thesis. For the purposes of describing the work of geoscientists, rather than asking whether this can ever be entirely replicated by machines, Polanyi’s concept has proven useful in analysing the ways that tools (theory, as well as models and methods of data collection) are relied on interdependently in arriving at conclusions. These tools and conclusions have also helped to develop a widely accessible imagery of risk that has directly furnished regulatory accounts.

**The social dimensions of risk and uncertainty**

A regulatory account of risk is analysed separately in chapter 6. Here I ask to what extent the scientific understanding and portrayal of uncertainties is acknowledged in the regulation of CCS operations within the EU. The first part of the thesis thereby sets the stage for a focus on the recognition of complex scientific phenomena in regulation and public policy. Funtowicz and Ravetz long ago captured the relevance of this analytical turn in science studies with their notion of ‘post-normal science’ (1993): “To characterize an issue involving risk and the environment, in what we call ‘post-normal science’, we can think of it as one where facts are uncertain, values in dispute, stakes high and decisions urgent” (744). Under such conditions evaluations associated with traditional
science would no longer be adequate as research enters the realm of regulation and policy. Analytical complexity on the one hand and disagreements about social values on the other requires a re-examination of what counts as good science. Since the late 1980s STS scholarship more broadly has addressed the question of what happens when science is relied on to shape public policy through ‘mandated science’ (Salter 1988) and ‘regulatory science’ (Jasanoff 1990b). With their contribution to this debate, Funtowicz and Ravetz’s have noted that the starting point for any enquiry into the production and uses of post-normal science should be a recognition that conclusions supported by technical knowledge are bound to rest on value-laden analysis: “When science is applied to policy issues, it cannot provide certainty for policy recommendations; and the conflicting values in any decision process cannot be ignored even in the problem-solving work itself” (1993: 740). They conceptualise this development with a model that plots scientific uncertainty related to policy questions according to two dimensions. The first dimension defines the amount of ‘systems uncertainty’ while the second captures the scale of the ‘decision stakes’. Applied science is normally carried out where both the systems uncertainty and decision stakes are low. When either is higher, we enter the world of knowledge production in professional consultancy. And if either of the categories is very high, the authors suggest that knowledge production is adequately characterised as “post-normal science”. Under such circumstances they propose that there is an instrumental need for an extended peer community to provide quality assurance of observations to scientists, because generalised definitions of systems interactions fail to account for the complexity of real, often very locally specific, conditions.

Wynne (1992) presents an alternative theoretical model of scientific uncertainty in regulatory practices. As I explain in chapters 4 and 5, within the traditional realm of scientific practice, knowledge production processes are easily justified because the users and areas of application remain circumscribed to a limited social and epistemic circle. Risk analysis for policy purposes has likewise traditionally been defined in terms of objective facts about the physical
world and separated risk management as a distinct, and subjective, category (see e.g. Royal Society 1983, NRC 1983). STS scholars (Jasanoff 1986; Jasanoff 1990b; Irwin and Wynne 1996; 2008b; Van Zwanenberg and Millstone 2005) have responded to this categorisation by pointing out that risk analysis and risk management often overlap, complicating the distinction between facts and values and necessitating hybrid evaluations when decisions about acceptable risks are made in policy fora. Wynne’s model likewise notes that the traditional separation of the two is severely challenged when science is asked to support policy prescriptions. As he points out, even where traditional science involves a high degree of uncertainty, as long as scientists progress reflexively in their work, it is mainly when external commitments are constructed on top that the internal limitations of complex analysis become socially problematic:

_The conventional view is that scientific knowledge and method enthusiastically embrace uncertainties and exhaustively pursue them. This is seriously misleading. It is more accurate to say that scientific knowledge gives prominence to a restricted agenda of defined uncertainties – ones that are tractable - leaving invisible a range of other uncertainties, especially about the boundary conditions of applicability of the existing framework of knowledge to new situations. Thus ignorance is endemic to scientific knowledge, which has to reduce the framework of the known to that which is amenable to its own parochial methods and models. This only becomes a problem when (as is usual) scientific knowledge is misunderstood and is institutionalized in policy making as if this condition did not pervade all competent scientific knowledge._

(Wynne 1992: 115)

Wynne thereby suggests that regulations radically redefine uncertainties and authority when they shift the incidence of a scientific evidence burden _upstream_, away from end-users and towards legal framings. Deferring to principles such as the precautionary principle in environmental risk regulation will therefore not in itself resolve debates pervaded by persistent uncertainties. Rather, as legal scholars have also pointed out (Fisher and Harding 2006) this principle may be interpreted and implemented in many different ways, a view that gains traction when reviewing the variety of cases where it has or could have played a role as a basis for regulation (Harremoës _et al_. 2001).
Along with Funtowicz and Ravetz, Wynne thus questions how we should make good public policy decisions that are based on scientific advice of complex environmental questions. In some crucial respects his framing of post-normality however differs markedly from Funtowicz and Ravetz’s. Where they imply that their twin axes of categorisation hold universally, Wynne instead suggests that different stakeholder groups will bring varying definitions to bear on the interpretation of complexity and the magnitude of the decision stakes involved. As Yearley (2000) notes, Wynne’s insistence on the inclusion of the public in a wider peer-review community is therefore motivated by an opening up of general definitions to embrace complexity, contingency and locally specific knowledge. Moreover, his classification of uncertainties as multiple and simultaneous, many of which cannot be quantitatively derived, implies that the public maintains an important role in extending trust and assigning authority to particular interpretations, institutions and scientists – sometimes even with the aim of helping to improve the quality of assessment tools such as models (Yearley 2006). This is a strongly supported tenet of STS scholarship that likewise supports a wider body of work, including that of cultural theorists (Douglas and Wildavsky 1982; Hulme 2009).

Wynne elaborates on his framing by defining four kinds of uncertainties that bear on scientific practice: ‘risk’, ‘uncertainty’, ‘ignorance’ and ‘indeterminacy’. ‘Risk’ refers to uncertainties with known odds. ‘Uncertainty’ refers to parameters that may be fairly well understood, but for which the odds are unknown. ‘Ignorance’ refers to uncertainties that are highly unknowable and frozen under stylised scientific practices, but increase and become clear when social commitments are based on such knowledge. This may happen when science enters the public realm and e.g. guides technology development. Finally, with ‘indeterminacy’, Wynne refers to the open-ended characteristic of proposed causal chains; that it is unclear where analysis of the natural world ends and analysis for social commitments begin: “Indeterminacy exists in the open-ended question of whether knowledge is adapted to fit the mismatched
realities of application situations, or whether those (technical and social) situations are reshaped to ‘validate’ the knowledge (Wynne 1992: 115).” In other words, do new findings reshape the downstream context of deployment and application, or do demands on knowledge production and expectations for application force confirmations from our scientific activities?

Wynne, similar to Cartwright, thus observes that traditional scientific analysis regularly introduces contingencies and imposes limitations to freeze uncertainties at different levels of complexity to derive results that hold only ceteris paribus. And his problematisation of the uncertainty that affects science in regulatory practices thereby focuses on the role of unconditionality in knowledge production in the face of social commitments. In such cases a clear distinction between facts and values is often questionable – technical analyses may be based on different assumptions about social aims or they may simply involve differentiated interpretations of uncertainties. Stable and consistent beliefs about risks are thus often the exception rather than the norm in post-normal science according to Wynne, even within expert communities.

**The co-production of science and governance**

The question of where science ends and policy begins runs throughout the thesis and is fundamental to all of the material covered. A long tradition in sociology going back to Max Weber has explored relations between different spheres of formally distinct social organisation, and STS scholarship has specifically focused on the distinction of social and epistemic borders between scientific knowledge production and policy (Rothstein et al. 1999; Shackley and Wynne 1995, 1996), and identified the need for more participatory (Stirling 2008a; Wynne 2002) and complex (Stirling 2010) conceptualisations of technology appraisal, partly as innovation processes dependent on social learning (Wenger 2000). The meeting of the technical and the social is similarly fundamental to the co-production model (Jasanoff 1990b, 1996, 2004b), which suggests that scientific research should not be understood as readymade for
experts to bring to bear on policy problems. Neither should regulations, such as safety thresholds and exclusion lists, be seen as independently constituted from scientific practice. Rather, the two are engaged in a more complex interaction. Scientific advice may be mediated by experts through formal committee membership, informal relations with policymakers and through publications. And policymakers may shape scientific agendas directly or indirectly with recourse to funding allocations and regulatory actions.

This is a challenge to some policy models such as technocracy, which suggest that policymaking, regulation and scientific knowledge production are, or ought to be, engaged through clearly demarcated areas of authority (Van Zwanenberg and Millstone 2005). Instead, the co-production model acknowledges that the regulation of emerging science often will arise from close relations between regulators attempting to control some poorly understood impacts associated with an emerging body of evidence, while relying on the expert advice of scientists to guide them. In such cases the line between science and regulation is often blurred and may at times lead to a reliance on alternative forms of peer-reviewed ‘regulatory science’ rather than conventional (‘pure’) science (Jasanoff 1990b). The co-production model is thus a challenge to STS research that does not explicitly comment on the political dimensions of science and technology. Jasanoff has furthermore argued (1996) that even where such a connection would not be assumed to exist, analysts cannot ignore the increasingly politicised identity of science since the middle of the twentieth century (Ezrahi 1990), and the expectation that scientists should and can offer authoritative understandings that can be used to order public policy. Along with Wynne (1992) she therefore asks that we not only pay attention to debates that have raised controversy and resulted in clear winners and losers, but that we also consider the implications of questions in science that on the surface would seem to carry little political weight and re-examine where the boundaries of science end and politics begins.
I deal with the question of boundaries between science and political governance most explicitly in the second part of the thesis in chapters 6, 7 and 8. Here, I have been specifically interested in how understandings of uncertainty and risk are (re)shaped and how information comes to be seen as evidence to support specific policy activities. In chapter 8 I furthermore consider how wider social forces alongside policy measures and regulations have shaped expectations attached to CCS, by drawing on Downs’ framework of the ‘issue-attention cycle’ (1972). This framework proposes that attention in society over time follows a cyclical pattern:

1) **the pre-problem stage**, when “some highly undesirable social condition exists but has not yet captured much public attention” (ibid: 39);

2) **alarmed discovery and euphoric enthusiasm**, when “the public suddenly becomes both aware of and alarmed about the evils of a particular problem. This alarmed discovery is invariably accompanied by euphoric enthusiasm about society's ability to ‘solve this problem’ or ‘do something effective’ within a relatively short time” (ibid: 39);

3) **realising the cost of significant progress**, which consists of a “gradually spreading realisation that the cost of ‘solving’ the problem is very high indeed … doing so would not only take a great deal of money but would also require major sacrifices by large groups in the population” (ibid: 40);

4) **gradual decline of intense public interest**, as people are discouraged by the high costs to themselves of responding, while “others feel positively threatened by thinking about the problem; so they suppress such thoughts. Still others become bored by the issue” (ibid: 40) (and the media lose interest); and
5) **the post-problem stage**, when “an issue that has been replaced at the centre of public concern moves into a prolonged limbo – a twilight realm of lesser attention or spasmodic recurrences of interest” (ibid: 40). Programmes and institutions are often established during earlier stages of the cycle, which persist in the post-problem stage and can have some modest successes towards tackling the original problem. Furthermore, “problems that have gone through the cycle almost always receive a higher average level of attention, public effort, and general concern than those still in the pre-discovery stage” (ibid: 41).

Downs’ framework lends support to a basic premise that runs through much of the scholarship on risk and the natural environment and wider sociological analyses (Beck 1992; Collins and Evans 2007; Stirling 2008a; Wynne 2006): that the sanitisation of multivalency in favour of narrow realist perspectives to inform the governance of technology used in the natural environment, ignores the critical epistemological differences between risk, uncertainty and indeterminacy that may logically give rise to varied meanings and understandings, and may pose a danger to democratic engagement. Such sanitisation may be imposed deliberately or indirectly when social movements such as environmental activists ignore, or find themselves ambivalent about, uncertainties and implications attached to policy advocacy based on scientific reasoning (Yearley 1992). Case studies of industrial pollution and resource use across the world show the difficulty of conclusively defining either as distinct from the other and as necessarily good or bad, when resource extraction might imply a change in pollution levels affecting stakeholder communities in various ways (Yearley 2005). Narrowly designed engagement with alternative policy options, whether by government or NGOs, can thereby dilute reflective engagement with the implications of changing environmental and energy practices and governance options, and ignore the importance of diverse and case-specific policies (Stirling 2007, 2010). In chapter 8 I draw on such insights to explain how the implicit marginalisation of critical voices following a narrow
focus on technical risks helps to explain recent years’ significant changes in expectations and prospects attached to CCS.

While I draw extensively on social theory to describe different perceptions of risk and uncertainty across social arenas, I also ask how divergent understandings of risks attached to CCS are best addressed when science is emerging, highly contingent and asked to play a part in governance decisions. To make these prescriptive points I draw on a body of work in STS known as ‘incrementalist technology policy’. This follows Charles Lindblom’s observations that policymakers often are forced to make decisions while faced with enormous uncertainties (1959, 1979). Waiting for more and better data to become available in order to base decisions on an ideal ‘rational-comprehensive’ approach is often not feasible. As a coping mechanism successful policymaking instead progresses by ‘successive limited comparisons’ of whatever knowledge is at hand. Lindblom’s observations laid the foundation for others to suggest that a similar approach should be followed for science and technology governance attached with a high degree of uncertainty (Collingridge 1980; Collingridge and Reeve 1986; Woodhouse and Collingridge 1993). As with the notion of post-normal science, the question of how to govern appropriately when faced with scientific complexity is again at the heart of the matter and concepts first penned by Collingridge were also used by Funtowicz and Ravetz. Specifically, institutions face increasing ‘error-costs’ (the price of a wrong decision) when technologies are relied on to solve large social problems (such as energy generation) and R&D involves longer lead times. These parameters are central to Collingridge’s definition of the policymaker’s ‘control dilemma’: early risk assessment for the regulation of processes and objects that are poorly understood may lead to onerous constraints on further R&D, but without such assessment and regulation early on, new technologies may be rapidly deployed and become difficult to manage once potentially adverse impacts are better understood.
In the face of possibly heightened scrutiny, incrementalists suggest a gradual trial-and-error learning process, what Collingridge called a ‘fallibilist’ approach. Rather than attempt to foresee and plan for every possible eventuality, social planners should embrace a developmental philosophy centred on flexibility. This involves differentiated small-scale experimentation with multiple options rather than focusing on very limited design options from the beginning. While analytical uncertainty remains, further developments progress with a view to minimising error-costs and optimal development trajectories are those that are most resistant to problems arising. This shifts decisions about the best available options away from predictions of success in emerging technologies and towards management of those that imply increased corrigibility, control, flexibility and insensitivity to error (Collingridge 1980). Crucially, Collingridge et al. do not thereby imply a strictly precautionary approach towards innovations, but call for more context-specific analysis to inform the appropriate weighting of risks and uncertainties.

The discussion above points to the following: that risk assessment activities are influenced by specific knowledge production and communication efforts as well as wider social support, and imply a degree of anticipatory management and evaluation, that shape both research and technology development agendas. This does not necessarily imply a radical relativist ontology. Any science-policy issue that traces its roots to the claim that anthropogenic climate change constitutes an urgent public policy problem necessarily assumes that the findings of the IPCC are scientifically defensible to a degree. It is not within the remit of this thesis to question this claim. Likewise, I do not ask whether scientific research is capable of providing us with novel insights about our world. I take it for granted that it has done so for a long time, as I take seriously attempts to base technology policy on the work of scientists, however tenuous or poorly thought out the links between the two may be at times. Respect for the work of scientists is not incommensurate with a recognition that they carry out their practices in distinct social settings and that their findings may therefore be mistranslated into other social spheres. On the contrary, a respectful
consideration of this work and the people who perform it should acknowledge such circumstances. In the face of ubiquitous uncertainty, particular modes of sensemaking are entertained to reach defensible conclusions and these deserve to be understood in detail, particularly when they carry implications for public policy where other concerns come into play.

It would therefore be simplistic to conclude that science can serve, or ought to serve, policy by enlightening available paths and help governments objectively calculate the most desirable actions. Rather, the co-production model suggests that a far more complex state of affairs is often on display or exists just under the surface of public scrutiny, by pointing to the uncertainty inherent in scientific research, the contestation involved with competing visions of technological development, and the messy and unpredictable arena of representative democracy and judicial review. Science cannot be asked to provide objective ethical and moral waypoints for decisions that bear on the social world. It does not explain how a policymaker ought to weigh decisions in the face of budgetary constraints, uncertainty and indeterminacy. However, distinct communities and social movements can be identified that embody particular and selective modes of reasoning with implications for policy and regulation (Yearley 1992). Likewise, regulators and policymakers can be seen to express particular interpretations of scientific uncertainty and adequate evidence. To the extent that public policy is based on and influences scientific findings, questions should be asked about such premises and their appropriateness for conclusions that bear on social planning. **Ultimately, this thesis is therefore concerned with the question of how we sustain a tenable relationship between science and public policy.**

**Methodology**

**Theory and methods**

Bloor (1973, 1999) and others have suggested that social scientists should strive to observe scientific knowledge production as any other social sphere and
therefore approach it with questions about communal belief systems, practices, social conventions, and regular interactions. Following this basic premise of STS, Collins and Yearley (1992) have suggested that questions about epistemology should be addressed through the lens of ‘methodological relativism’. Methodological relativism implies that closure in scientific and technological development and debate is contingent on agreement (explicit or tacit) between actors. This implies a constructivist perspective that challenges linear accounts of technology development as progressing from scientific discovery, and compels analysts to treat technical development as a matter of consensus (or lack thereof). Addressing stakeholder beliefs through a reflexive social enquiry thereby becomes central to the work of social scientists preoccupied with the development of scientific practices and technological artefacts. Following these tenets of STS I have primarily focused my work on a documents analysis of knowledge generated for and in the science-policy domain, and on in-depth interviews with stakeholders involved in generating expert knowledge about CCS. To ensure that I would carry out my research reflexively, I was early on advised by my supervisors to address beliefs about uncertainty and risk from a number of perspectives and in several communities, to compare my interview data with policies and regulations, and to become acquainted with a body of STS literature focused on risk conceptualisations and the role of expertise in the sciences and in policy.

I have found support and advice for this iterative approach following a ‘grounded theory’ methodology as developed by Glaser and Strauss (1967) and additional guidance from Glaser’s (1992) response to Strauss’ later writings, as well as Charmaz (2006) and Rennie (2000). Grounded theory suggests that research should give way to new substantive theory based on empirical data in case study research. Rather than seek to explicitly test hypotheses, grounded theory is concerned with generating novel insights iteratively. This involves collecting data, considering the explanatory potential of some theories, analysing the material with these in mind, and re-examining its fit with theory in light of continued data collection. While any novel insights may therefore
ultimately accord with existing theoretical accounts, following grounded theory ensures that these are not chosen \textit{a priori}. The constant comparison between data and theory seeks to give rise to explanations that accord better with emerging analytical insights and allow novel perspectives to arise. I have followed this approach by integrating a mix of data types and research methods as relevant additions to the thesis have become clearer over time. This has allowed me to rely on a number of epistemologically similar theories to reassess my conclusions over time rather than test a single theory's fit with all of the data.

My, primarily qualitative, data was collected as a total of 60 semi-structured interviews (listed in annex 1) with a range of CCS expert stakeholders, geoscientists, and stakeholders close to policy circles; close comparative readings of scientific papers; and reviews of policy and regulatory documents. Relevant interviewees were selected in relation to the main focus of the research questions in the different sections of the thesis as outlined in this introduction. The type of data collected, the preferred analytical methods, and my reflections on theory within individual chapters and the thesis as a whole – in short, the methodological components of this work – were developed and refined over time as the specific aims of my thesis evolved. Following a grounded theory methodology supported my aim of generating interdisciplinary research that required me to become familiar with technical and scientific debates, policy options, and theoretically driven questions about representation, equity and sustainability from STS literature.

All interviews for this thesis were recorded and lasted between 20 minutes and 2 hours. Whenever possible, interviews were recorded in person, but a few had to be conducted over the phone either because of resource limitations on extensive travel or because an interviewee had little time to meet in person. Many of the earliest interviews were fully transcribed to record subtle details about phrasing, intonation, emphasis, and differences in reasoning. As more interviews were recorded and I collected written material for my case studies, I
chose not to analyse these more subtle outputs, as is recommended in some forms of grounded theory, and instead to compare the explicit and implicit reasoning employed by interviewees with that used in various publications. Once it was thus clearer what type of information was of greatest relevance to my research questions, full transcriptions gave way to more limited and focused note taking. Interview data has been used to structure the material and discussions presented in chapters 3 through 6, and also informs chapter 8. While chapter 3 relies on statements by a range of expert stakeholders, chapters 4 and 5 focus specifically on geoscientists, while chapter 6 primarily draws on insights from stakeholders within and close to policy circles.

Efforts were made to gather responses to a range of pre-crafted questions informed by readings and to iteratively orient the focus of my reading towards matters that arose in interviews. This meant that geoscientists’ focus on reasoning processes about risk and uncertainty gave way to a detailed comparison of the published literature in chapters 4 and 5, just as policy oriented stakeholders were asked to comment on negotiations and specific wordings in regulations for chapter 6. Many questions were however asked in a very open-ended fashion. This kept the format semi-structured and allowed interviewees to raise distinct concerns and perspectives, which in turn allowed me to challenge later interviewees with more detailed questions.

For chapter 5 I also carried out a social network analysis (SNA) to assess the co-publication patterns of geoscientists. SNA can be considered a hybrid of qualitative and quantitative data analysis that provides insights into patterns of connections between nodes in a network (Scott 2000). A network can consist of all kinds of nodes – individuals, organisations, locations – and their interactions or shared connections. In chapter 5 I provide a detailed overview of data collection and analysis options for SNA as well as an overview of the variety of research questions where it has previously been applied. The SNA material proved to supply helpful additional analysis and illustrative support for ideas about professional identity and network structure that I was investigating.
through interviews. Together with a close reading of scientific publications about CO₂ storage at the Sleipner demonstration project, the SNA data helped to support my hypothesis that geoscientists studying CO₂ storage should be thought of as constituting an epistemic community.

Limited quantitative data was collected and analysed for chapters 3 and 8. Chapter 3 draws on results from a short survey (n=19) with range of CCS expert stakeholders from research, industry and NGOs. In chapter 8 I analyse changing perceptions of CCS over the past few years by drawing on articles in specialised print media.

My decision to focus my data collection primarily on interviews and to critically assess the role of evidence in policy documents and regulations was consistent with my reflections on STS theory and a recognition that stakeholder beliefs can be approached and theorised as socially constructed and contingent. I considered extending the short survey presented in chapter 3, originally collected as part of my MSc research, which would likely have provided me with comprehensive data to analyse statistically. Seeing as I was however focused on attempting to understanding how individuals and social institutions involved in scientific knowledge production and policy formation think about and construct notions of uncertainty, I decided that interviews would provide much richer data about beliefs and reasoning processes.

**Description of the research process**

I first began my PhD by reflecting on my recently completed MSc dissertation, in which I had asked how a range of expert CCS stakeholders thought about several technical, scientific and policy related uncertainties that might affect large-scale development of CCS in the UK. The work had been challenging and enlightening to carry out, partly because I had to travel to meet with people and partly because I considered the perspectives of individuals from different sectors of society who harboured sometimes conflicting hopes for the future of technology development and strategies for climate change mitigation. When I
began my PhD research shortly after submitting the dissertation in the fall of 2009, I set out with an aim to continue focusing on expert stakeholders’ beliefs about uncertainties and risks related to CCS development. By that time I had become aware of the substantial body of STS literature devoted to case studies about expertise, discussions of epistemology in the sciences, the social construction of knowledge, and the co-production of regulations. While I had familiarised myself with sociological literature on expert perceptions and the general technical components in CCS systems in order to craft several interview and survey questions, I soon recognised that a deeper appreciation of STS literature would allow me to address questions about authority, belief structures and social policy more rigorously. As a consequence I spent the greater part of my first year familiarising myself with the different branches of research within STS. I began to think more critically about the rhetoric of risk and uncertainty employed in policy documents and how this compared with what I heard discussed among geoscientists in meetings and conferences, and the tone in scientific publications. Towards the end of my first year I decided that I wanted to focus on the ways that risks and uncertainties were understood and portrayed by (geo)scientists and how this compared with the rhetoric and evidence basis in political and policy domains.

As a starting point for my data collection I reviewed the language of emerging regulations in the EU, the US, the UK and other legal jurisdictions. I also looked closely at the policy landscape and the consideration given to regulatory instruments aimed at promoting the commercial development of CCS, primarily in the UK. Much of this review later informed a book chapter, revised and updated for chapter 2 of this thesis. Half of my PhD funding had been secured from an industrial partner that had funded other work in my research group, Scottish Carbon Capture and Storage (SCCS). The other half was supplied as a competitive scholarship from the University of Edinburgh’s School of Geosciences. The industrial partner had agreed to cover part of my costs on the condition that my work would address regulatory questions with an instrumental value to their commercial activities. And I had agreed to this
knowing that I wanted to continue informing myself about emerging legal frameworks and because I was curious about how experts and areas of expertise might be relied on to inform regulations. Several months into my first year it then emerged that the industrial partner no longer was interested in funding my work. Because my research group now became the official supplier of this half of my funding, I was no longer obligated to focus part of my attention on questions with potential commercial implications. I did however still want my research to address questions that might lead to recommendations with an instrumental value to policymakers as well as questions that might lead expert stakeholders such as geoscientists to reflect on their involvement in generating science for policy fora. At the same time I observed that the STS field had much to contribute to wider debates about representation, equity, policy strategies, sustainability and resource use. And I continued to note how theoretical questions about the nature of expertise and the social construction of knowledge could be addressed by drawing on the interview recordings and documents that I was collecting. Over time I thus found myself increasingly framing my empirical research within wider theoretical debates in STS.

A few months into my second year I revisited my MSc dissertation and rewrote it for a journal publication, aiming to reflect more critically and rigorously on how my findings fit alongside STS literature. This publication, updated and revised to reflect policy developments since 2010 and my continuing findings, is the basis for chapter 3 of this thesis. As I delved into STS literature I became increasingly preoccupied with the ways that domain expertise and technical evidence framed understandings of risk, uncertainty, the interpretation of the law, and policy options. As part of my focus on policy options I collected hundreds of pages of transcripts of debates in UK Parliament, attended a hearing about a specific regulatory mechanism – an emissions performance standard (EPS) – and asked policy oriented stakeholders to comment on its development in interviews. I decided that I wanted to devote a chapter of the thesis to outline the narrative of this policy option and another one to beliefs about CCS readiness strategies adopted by utilities that might face a future
regulatory mandate. Both would draw on discussions about expert advice in Parliamentary hearings, written evidence and case studies on implementation options in other parts of the world.

As the months passed it became clear that I would not see the end of negotiations about the EPS as Parliamentary hearings continued and political manifestoes were revised to reshape the policy foundations for CCS commercialisation. Incentives were now being formulated as part of a grander Energy Market Reform (EMR) where support for CCS would only constitute one part. This meant that a case study of the EPS option would have to account for a much wider set of related discussions attached to the EMR, which would steer my focus away from CCS proper and towards policy negotiation. I therefore decided to discontinue this strand of the research in favour of other work. I also decided to halt my investigation of perceptions about CCS readiness options, as I decided that this would divert my focus on technical and scientific uncertainties in favour of a discussion about commercial interests and policy innovation. These data collection choices consequently meant that I had a broader and less singularly focused narrative about policy developments and evolving political language to relate in my PhD, which I have summarised in chapter 8. In an effort to broadly chart changing perceptions of CCS over the past few years, this chapter also draws on an analysis of perceptions in specialised print media. During this time I had also been collecting written documentation about a controversy unfolding in Norway in relation to the Mongstad CCS demonstration plant. I had hoped to integrate this as part of my material about policy negotiations and perceptions of technical evidence, but as I went deeper in to the case study I eventually had enough material to write an entire chapter about the politicisation of CCS, the negotiation of risk and uncertainty, and the question of risk governance in research, development and deployment (RD&D). This is related in chapter 7.

Throughout this time I continued to collect data on emerging regulations in the UK and the EU. Interviewees from research, industry and NGOs all continued to
raise points during interviews about the rhetoric of CO$_2$ storage risk in the EU CCS Directive and as I reviewed this framework and compared it with STS literature and writings by legal scholars, I honed in on the role of the precautionary principle as a decisive element for interpretation. Following an initial publication that focused on risk perceptions among different stakeholder groups, I revisited the material and asked how the availability and stability of knowledge about CO$_2$ storage was portrayed as risks and uncertainties in the Directive. This now constitutes chapter 6 of the thesis.

My focus on scientific and technical knowledge production so far had been limited to its role in deliberating between regulatory and policy choices. While I had interviewed some geoscientists during the start of my first year, by the beginning of my third year my data collection and analysis were primarily concerned with policy dynamics and regulatory formulations. I was however influenced by the writings of STS scholars who used the co-production model as a frame of investigation, to consider how questions related to social policy could also be based in scientific uncertainties, just as supposedly technical discussions could be structured by social policies and legal principles. Following Wynne's (1992) classification of risk and uncertainty I had attempted, unsuccessfully, to address questions related to liability and precaution as an enquiry into institutionalised perspectives by interviewing regulators in the EU Commission. In the winter of 2010-11 I saw another opportunity to contact regulators as I officially took a two-month leave from the PhD to work on a consulting project about regulatory clarity in CCS development for the Global CCS Institute. During the project I worked with people in the Scottish Government who were sympathetic towards my research aims and saw the value of my speaking with regulators in the EU Commission to understand how they drew on scientific experts for advice. I was both provided with an introduction to one of the individuals who would likely be closest to these discussions and made travel arrangements to accompany a government official to Brussels for a meeting. Neither of these efforts led to an interview and I did not manage to secure any information from regulators about their interactions with scientific experts.
Having been unsuccessful in accessing members of this regulatory forum I decided instead to address the question of scientific advice from the perspectives of scientists and policy-oriented stakeholders who had had relations with regulators over the years. This also presented an opportunity to go into more depth with questions about the science behind CCS, the beliefs of scientists, and the social (and philosophical) significance of scientific assessment tools such as models. Having been surrounded by geoscientists in my research group and listened in on debates during presentations, meetings and conferences, I wanted to revisit a question that I had posed at the start of the PhD and assess how scientific knowledge claims were justified as more or less certain and stable. A considerable body of sociologically driven STS literature deals specifically with the work performed by models, but I was becoming increasingly focused on the philosophical implications of attempting to model an environment that involves a high degree of complexity and several categories of uncertainty. My analysis therefore followed the emphasis on models developed in Cartwright’s later writings (1999) and those of her followers, while reflecting on the findings of sociologically driven STS scholars. Because Wynne’s categorisation of risk and uncertainty derives both from sociological considerations about expertise, stakeholder perceptions and analytical conventions, as well as epistemological reflections about agency, observation and knowledge categorisation, my reflections on the material presented in chapter 6 followed the analysis in chapters 4 and 5 coherently and logically.

With the overlap in theoretical subject matter my analysis in chapters 4 and 5 thus supported the policy-oriented questions pursued in chapter 6. To ensure that my empirical material would likewise support a consistent analysis and because the CCS Directive primarily addresses CO₂ storage – and finally, because it can be argued that long-term storage is the one novelty in CCS systems that does not follow regular practices in the hydrocarbon industry – I decided that it would be insightful to ask how this knowledge was being
produced and how the scientists involved thought about its development and certainty. In the spring of 2012 I therefore attended an annual conference of a network of geoscientists, CO₂GeoNet, to supplement interview and conference notes collected during my first year. I also interviewed several individuals from research groups around the UK. Many of these geoscientists had been given privileged access to data from the Sleipner CO₂ storage demonstration project in the Norwegian North Sea, arguably the world’s most prominent CO₂ storage site. In an effort to ensure that my focus on geoscientists would remain fairly bounded, I therefore decided to look at publications related to the Sleipner project and the network of geoscientists who had been involved in producing this work.

Had I had more time I would have wanted to carry out more interviews with geoscientists. And had I had better access to regulators in the EU Commission I would have wanted to draw on their insights as well. I also planned to collect data about the informal network of geoscientists who informed regulators about storage safety and security. For weeks on and off I attempted to find comprehensive annual lists of network membership in groups such as CO₂GeoNet to track changes in participation over the years.¹⁴ Despite being on friendly terms with conference and network organisers I could not find reliable nor consistent data and therefore abandoned this idea.

**Structure of the thesis**

The focus of the thesis is divided into two themes. The first asks what experts who advise policy and regulatory bodies think about risks and uncertainties related to CCS and how they reach these conclusions. The second sections asks to what extent these beliefs have informed policy and regulation and what other issues have simultaneously been important in defining CCS governance. The

¹⁴ SNA is not a statistical method and therefore does not rely on sampling to assess network structures. Rather, comprehensive data on nodes and connections are used to assess questions such as network density, the centrality of individual nodes, and individual clusters. A more detailed explanation of SNA is provided in chapter 5.
iterative process described in the methodology gave way to different empirical material in each of the chapters, analysed with distinct reference to the theories covered throughout this introduction. Chapter 2 provides a general overview of the history, technology, economics and key regulatory issues associated with CCS. Chapter 3 draws on the concept of interpretive flexibility to assess experts’ beliefs about key uncertainties around science, technology and policy drivers. Chapter 4 analyses beliefs and practices in the geoscience research community associated with CO₂ storage by drawing on the theory of epistemic communities and Cartwright’s philosophy of science. Chapter 5 looks in more detail at co-authorship patterns around and findings arising from the Sleipner CO₂ storage demonstration project, again by drawing on Cartwright as well as Edwards to analyse patterns in knowledge production practices. Chapter 6 considers to what extent scientific research has shaped CO₂ storage policy and regulation in the EU, partly by discussing risk assessment in relation to concepts formulated by Wynne and legal scholars. Chapter 7 continues the focus on uncertainty and precaution by discussing developments at the CCS Test Centre Mongstad in Norway, in the context of incrementalist literature. Chapter 8 draws on insights from Downs, Collingridge and Wynne, and asks how we should conceptualise and progress with the governance of CCS in a highly contested policy context. The conclusions at the end of the thesis present my reflections on the research process and seek to draw out interpretive and instrumental lessons learned.

**Novel contributions**

The thesis contributes to research efforts in STS in a number of ways. Chapter 3 extends a general question in constructivism into the world of UK CCS expertise. Chapter 4 considers beliefs and practices in scientific knowledge production amongst geoscientists studying CO₂ storage, a community that has not been studied in detail before. Chapter 5 uses SNA to complement a qualitative analytical approach that is well established in STS scholarship. Chapters 6 and 7 contribute to a wealth of literature on risk, uncertainty and precaution. And
chapter 8 combines insights from across STS scholarship into a conceptual model, to account for recent years’ developments in CCS governance.

**A note on ethics**

My research was funded by the SCCS at the University of Edinburgh, an organisation committed to producing high quality research on all aspects of CCS, primarily geoscience and engineering. Over the years the SCCS has received funding from the UK’s Research Councils as well as considerable funding from industrial partners committed to seeing CCS develop from a research topic into a commercial activity. Within this institutional context I have been free to pursue the research questions that I wanted, but my own perspective on the desirability of CCS has arguably also been affected by the presence of so many researchers working to address uncertainties in CO₂ capture, transport and storage. During my time as a researcher within the SCCS I have been involved in consultancy work aimed at structuring discussions in regulatory contexts and I have contributed to reports on legal developments and public perceptions of CCS. I am arguably therefore not a neutral observer devoid of institutional commitments.

I have however striven to conduct my research reflexively, to think about my own identity and assumptions concerning CCS, and to provide an honest and rigorous account of my observations and analysis. While I am strongly in favour of carbon mitigation strategies that draw on numerous research areas, I am also convinced of the ethical and instrumental value of a wider and deeper appreciation of social dynamics within science and technology governance. I have therefore sought to gain access to a wide number of interviewees and to participate in conferences to better understand uncertainties and tensions within the CCS arena. Being a member of the SCCS has been incredibly helpful in this regard. I would no doubt have faced more scepticism from interviewees and I would certainly not have had the same ease of access to conferences, had I not been part of the research group. While my membership in the group might
carry a heightened chance of my research outputs being biased towards the beliefs and commitments of scientists and engineers, I believe that it has also helped me gain a very broad and deep understanding of several issues related to CCS governance. I hope that this will be reflected in the quality of this thesis.
2. An introduction to key developments and concepts in CCS: History, technology, economics and law

This chapter has been revised from an earlier version by the same name published in 2012 with Chiara Armeni and Vivian Scott. It introduces several aspects in geology, engineering, economics, policy and regulation that have implications for the governance of CCS on global, regional and national levels. It also provides a short history of developments in the field from the 1970s until the present day. The issues covered here will provide the curious reader with perspectives on several technical issues that are discussed in subsequent chapters.

What is CCS?

CCS is defined as a technology system that combines CO$_2$ capture from flue gas emissions in power generation or industrial operations, with compression and transportation of that CO$_2$ to a suitable geological formation for permanent disposal. While storage of CO$_2$ for subsequent use may be possible, this is not the aim of the technology system, which has been conceptualised as an option to substantially reduce atmospheric emissions from energy generation and carbon-intensive industries. Several governments around the world, including Member States of the EU, Norway, the US, Australia and Japan, have established state-sponsored RD&D programmes dedicated to CCS over the past decade, and energy ministries have expressed hopes that these activities will support widespread deployment by the early 2020s.

The International Energy Agency (IEA) has estimated that for CCS to contribute significantly to climate change mitigation, 100 demonstration projects will need to be deployed by this time, and scaled up to more than 3,000 projects by mid-century to capture around 15% of worldwide CO$_2$ emissions. This will require an investment in the order of trillions of dollars (IEA 2009) together with a very

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fast scale-up of capacity in supply industries. The build-up in CCS capacity may likewise provide a window of time to develop more cost-effective renewable technologies (Brown et al. 2007), and yield significant savings in the costs of climate change mitigation (Stern 2006). Calculations from the IEA suggest that these cost reductions compared with a non-CCS scenario may be as large as 70% by 2050, and that to achieve such savings as much as 10 gigatonnes (Gt) of CO₂ will need to be captured annually at this time, 5.5 Gt from the power sector alone and the rest in carbon intensive industries (IEA 2009). That is the equivalent of more than 2,500 average sized coal-fired power plants.

**A brief history of CCS**

From the broadest perspective, the development of CCS as a climate mitigation technology from conception to the present day has gone through the general (and to some degree overlapping) stages set out in Table 2.1, elaborated in the sections below.

<table>
<thead>
<tr>
<th>Period</th>
<th>Key characteristics</th>
</tr>
</thead>
<tbody>
<tr>
<td>1970s and 1980s</td>
<td>Concept creation and early (isolated) academic research.</td>
</tr>
<tr>
<td>1990s</td>
<td>The mitigation of fossil fuel emissions becomes a major political concern and leads to early research and development on CCS; incentivised by isolated progressive climate policies; first demonstration projects commence.</td>
</tr>
<tr>
<td>2000-2006</td>
<td>Detailed studies of CCS as a potentially large integrated system, initially at national, and then broadening to international, scales; CCS part of official future energy and climate agendas and ambitions, as expressed in government policy.</td>
</tr>
<tr>
<td>2007-2010</td>
<td>Publicly funded demonstration programmes created to attempt to prove and accelerate technology development and deployment and many small-scale pilot projects established.</td>
</tr>
<tr>
<td>2010-present</td>
<td>Initial demonstration funding awards allocated and first demonstration projects under consideration with detailed front-end engineering design; policy expectation that multiple CCS demonstration projects will be in operation around the world sometime after 2015.</td>
</tr>
</tbody>
</table>

Table 2.1: Stages of CCS technology development, 1970 to present day.

The early days

The concept of capturing CO₂ from large point sources and storing it in isolation from the atmosphere was first proposed as a climate mitigation method by Marchetti (1977), albeit with more emphasis on storage in the ocean water column than in subsurface geological reservoirs. While emissions abatement and climate change mitigation using CO₂ capture and storage was a novel concept, it is worth noting that neither the capture of CO₂ from large point sources, nor the injection of CO₂ into the subsurface were unknown practices at the time. Natural gas sweetening operations to separate co-produced CO₂ (vented to the atmosphere) from methane was developed in the 1930s (Bottoms 1930) and the first commercial injection of CO₂ (extracted from a natural reservoir) into a depleting oil field to enhance oil recovery (EOR), commenced in 1972 at the SACROC unit of the Kelly-Snyder Field in Texas, US (Donaldson et al. 1989).

Limited academic research on CCS followed through the 1980s (e.g. Hoffert et al. 1979), and an early project to store CO₂ in the subsurface was initiated in 1986 (Shackley and Gough 2006). CCS was brought further into the mainstream of academic research and government interest in the 1990s, when the mitigation of energy-related CO₂ emissions from the combustion of fossil fuels became a major subject of scientific and political attention. The IEA took an early role in stimulating international research in the 1990s through its Greenhouse Gas RD&D Programme (IEAGHG), which created a biannual conference that became a regular meeting place for stakeholders from research, industry and policy...
circles interested in CCS (Stephens and Liu 2012). Other activities were funded through government and industry platforms such as the CO₂ Capture Project, a collaborative undertaking between oil and gas operators launched in 2000. Such organisations published materials oriented towards policy domains that envisioned a place for CCS as a carbon mitigation strategy alongside renewable technologies and nuclear power.

**The first demonstration projects**

The first CCS demonstration project was developed some years earlier in the mid-1990s. In response to heated political debate that attempted to reconcile a progressive green domestic and international agenda with its role as a major exporter of oil and gas, Norway found itself moving into the forefront of CO₂ mitigation and in 1991 introduced the world’s first tax on CO₂ emissions for the off-shore oil and gas industry (Sumner et al. 2009). The tax meant that storage became economically attractive, the capture costs having already been incurred in gas processing during the preparation of natural gas. This led the large oil and gas operator Statoil to consider injecting separated CO₂ back into the Utsira field. In 1996 this resulted in the creation of the Sleipner injection operation in the Norwegian North Sea, the world’s first large-scale capture and long-term storage project (Statoil 2009a). CO₂ co-produced with methane is here separated using amine absorption in post-combustion, and injected into the Utsira sandstone saline formation above producing gas fields.

The Sleipner platform has operated continuously, capturing and injecting around a million tons of CO₂ per year, with planning underway to connect the facility to additional gas fields in the region. Along with early research into storage capacity conducted under the UK’s Joule II study, the Sleipner project was critical in shaping early convictions among geoscientists involved in CO₂ storage that projects were technically feasible and largely hampered by cost constraints (Holloway 1997).¹⁶

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¹⁶ The significance of the Sleipner project and these data are discussed in detail in chapter 5.
Three further large demonstration projects, defined as injecting in excess of 1 million tonnes of CO₂ (1 MtCO₂) annually, linked to gas processing facilities followed, with a total of four projects in operation globally today. The Weyburn Midale project is currently the ‘world’s largest’, storing 3 MtCO₂ annually and commenced operations in 2000 (PTRC 2011). In 2004, the In Salah gas processing facility in Algeria – operated by BP with partners Sonatrach and Statoil – commenced storage operations, re-injecting 1.2 MtCO₂ annually into the Krechba gas reservoir (In Salah CO₂ 2010). Lastly, the Snøhvit gas processing and CO₂ storage facility in Arctic Norway commenced operations in 2008 (Statoil 2009b). CO₂ is here scrubbed from the gas and injected into a saline formation near the producing fields. For all of these projects, their novelty lies in the explicit storage (i.e. injection and monitoring) of CO₂ for climate change mitigation, rather than simply its separation from other gases. It is worth noting that in the US, several large natural gas processing facilities, e.g. Exxon Mobil’s Labarge facility in Wyoming, sell scrubbed CO₂ for use in EOR operations, but these are not operated or monitored as projects for permanent storage. As CO₂ capture and removal is required during natural gas processing, the incremental costs in these types of CCS project are much lower than they would be in power plants and explains why three of the existing projects are of this type. The exception is the Weyburn Midale project (which utilises CO₂ from a coal gasification plant), but this is formally an EOR project, hence it has an additional revenue stream from oil extraction. It is likewise worth noting that none of these projects are fully integrated CCS demonstrations as they do not include a long-distance pipeline or ship transportation component, but instead use nearby storage sites.

**Recent growth in RD&D**

Since the mid-2000s, CCS related RD&D activities have grown and spread, not least in response to and in anticipation of policy drivers and financing initiatives. This has included the development and operation of a number of small pilot studies, mostly commercially led capture projects (often with a
degree of public financial support) and a lesser number of injection and monitoring projects (GCCSI 2011b; Russell et al. 2012; ZEP 2011a). Many of the capture pilots have been located at sites that may serve larger demonstration facilities at a later date, to test and develop capture technologies in situ on flue streams from coal-fired power plants. In contrast, injection and storage projects generally have made use of CO₂ from natural reservoirs, rather than first capturing it from power plant emissions (for obvious cost reasons).

In addition to the four operational demonstration projects detailed above (Sleipner, Weyburn Midale, In Salah and Snøhvit), by mid-2012 three other forthcoming demonstration projects had made final investment decisions to go forward with construction. These are the Gorgon gas processing facility on Barrow Island in Western Australia, the Boundary Dam coal-fired power plant in Saskatchewan, Canada, and the Kemper County coal gasification power plant in Mississippi, US.¹⁷

**CO₂ capture and transport**

CO₂ capture systems fall into one of three different categories: post-combustion, pre-combustion and oxy-fuel combustion. Post-combustion technologies are the most advanced family of capture applications available and are being planned for most demonstration projects funded by the EU Commission. In post-combustion, CO₂ is captured from a power plant’s flue gas after regular combustion, using amines, chilled ammonia solutions or other separation chemicals (Whitton 2009). A main advantage of this approach is that it enables capture units to be retrofitted onto existing power plants that have sufficient space onsite as well as transport options.

Pre-combustion capture works by first reacting a fuel such as natural gas with air to produce a synthesis gas composed of carbon monoxide and hydrogen. The carbon monoxide is reacted with steam to produce CO₂ and additional

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¹⁷ The Kemper County facility has since met with a regulatory review following cost overruns and the departure of top management (Hallerman 2013a).
hydrogen, after which the CO₂ is separated in an absorption process. This leaves a separate hydrogen-rich stream, which can be used as an input fuel elsewhere, e.g. in a hydrogen-based transport infrastructure. The resulting high concentration of CO₂ produced in pre-combustion capture can be more easily and cheaply separated for storage than is expected to be possible with post-combustion applications (IPCC 2005).

Finally, oxy-fuel capture works by burning coal or gas with purified oxygen supplied by membranes or chemical looping cycles, to produce a flue gas that consists mainly of high concentrations of CO₂ and water vapour together with excess oxygen (IPCC 2005).

For all these options there will be an associated energy penalty, reflecting the increased power needed to operate a capture plant. A summary report published by the IEA (Finkenrath 2011) notes that the net efficiency penalty can be expected to range between 7.5–10.5 percentage points, equivalent to a relative net efficiency penalty range between 15–25%, depending on the type of power plant and its average efficiencies. After capture, the CO₂ gas stream is compressed to between 10 to 100 bar and transported either via pipeline or ship to a geological storage location. Existing pipeline infrastructure used for natural gas and EOR may in some places be used to transport CO₂ to offshore storage fields (IPCC 2005).

**CO₂ storage**

Storage is mainly envisioned to take place in depleted oil and gas fields in the near-term, largely because such structures are relatively well mapped and are known to have contained various hydrocarbons for thousands of years. In the longer term it is argued that larger deep saline aquifers, such as the Utsira formation beneath the Sleipner production well, will be required in areas such as the North Sea region if substantial amounts of CO₂ are to be disposed of (SCCS 2011). The injected CO₂ is stored in the pore spaces of permeable rocks.
such as sandstone, displacing and, to some extent, slowly dissolving in saline water.

Injection is likely to take place at minimum depths of 800 metres, at which point the CO$_2$ exhibits a sharp increase in density and a corresponding decrease in volume. With this increased density it enters a supercritical phase that shares characteristics of both a fluid and a gas. Because CO$_2$ is buoyant relative to saline water at this depth, it is normally argued that geological formations should be overlaid by an impermeable mudstone cap rock. This way, if faults or fractures in the sandstone form migratory pathways, it is argued that the dense mudstone cap rock functions as a final permanent trap. As an alternative, it is possible to inject at depths where CO$_2$ is no longer buoyant, but as depth increases so does the cost of injection and the difficulty of monitoring sites. Other storage options include coal seams and disused mines and caverns, but such sites may not ensure sufficient integrity against leakage. Storage at shallower depths is also possible, but usually onshore where groundwater is stored. This therefore poses a risk of local groundwater contamination and also increases the chance of public concern over other environmental and health risks (Shackley and Gough 2006).

Injected CO$_2$ is thought to become trapped within geological structures via physical and chemical reactions over the course of millennia. In the period immediately following injection, CO$_2$ trapping may be enhanced primarily by the stratigraphic and structural characteristics of the geology. Faults and rock fractures may function as traps under some circumstances while in others these may facilitate upward migration. As explained, this argues for the presence of an impermeable cap rock to prevent migration to the surface. As CO$_2$ rises, it may become trapped in pockets within a formation. This is referred to as hydrodynamic trapping and thought to occur over hundreds to thousands of years as saline fluids migrate slowly over long distances towards the surface. Over this course, the CO$_2$ may slowly dissolve in the surrounding water and become trapped as low concentrations of residual saturation throughout the
Geochemical reactions with surrounding minerals are thought to take place over millennia and as injected CO₂ reacts with the geology some of it may become trapped as stable carbonate minerals (IPCC 2005).

### Cost estimates

In a review of ten cost studies carried out for the IPCC’s *Special Report on CCS*, Rubin, Chen *et al.* (2007) summarise a range of near-term CO₂ avoidance costs for coal- and gas-fired power plants. These findings account for the cost of capture, by far the most expensive part of an operation, and range between $29–51 per tCO₂ (€21–37 per tCO₂) for coal-fired power plants and between $37–74 per tCO₂ (€27–54 per tCO₂) for gas-fired power plants. In their own cost model they include transport and storage costs, which add 4–10% onto the cost of capture, resulting in an average full-chain avoidance cost of $61 per tCO₂ (€44 per tCO₂) for coal-fired power plants and $72 per tCO₂ (€52 per tCO₂) for gas-fired power plants. A higher set of price ranges for CO₂ capture are reflected in an IEA cost summary report (Finkenrath 2011). The CO₂ avoidance cost for capture in coal-fired power plants here ranges between $43–58 per tCO₂ (€31–43 per tCO₂), and for gas-fired power plants is estimated to be $80 per tCO₂ (€58 per tCO₂). The EU Zero Emissions Platform (ZEP) has also completed a cost study, this one based specifically on CCS for the European power sector (ZEP 2011b). The CO₂ avoidance cost ranges for a full-chain system in this study are based on estimates for early commercial experience and concludes that the cost range for coal-fired power plants is between €40–50 per tCO₂ ($55–69 per tCO₂) and between €80–110 per tCO₂ ($110–151 per tCO₂) for gas-fired power plants.

The significant price difference between applications for coal- and gas-fired plants in these studies reflects a relatively high degree of natural gas price fluctuations, as well as a higher per unit capture cost of CO₂ from the relatively low concentrations in natural gas compared with coal. With limited real world

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18 Chapters 4 and 5 present a critical examination of claims associated with these stabilisation processes as well as monitoring and modelling activities.
experience to support such estimates, the robustness of assumptions has been shown to be relatively poor. Even minor adjustments to input assumptions have been shown to result in substantial changes to total project costs and associated long-term deployment prospects (de Coninck et al. 2009). This is important to consider as gas prices have fallen in recent years with the increased extraction from unconventional sources such as shale geology. This has in turn flooded the world with cheap gas, caused a shift towards gas combustion, and lowered the demand and prices for coal (Chazan and Wiesmann 2013). The macroeconomics of energy availability have thereby changed the case for CCS dramatically as gas generation has become even more favoured to coal in many places around the world, challenging the argument for CO$_2$ capture.$^{19}$

Prospective analyses have also considered the potential for long-term cost reductions. In one study Rubin, Yeh et al. (2007) used learning rates$^{20}$ from a number of technologies to argue that early costs could decrease by as much as 40% after the first 100 gigawatts (GW) of installed capacity have become operational. If the IEA’s hopes for CCS hold, this could happen as soon as 2020. However, the history of technology development suggests that poorly coordinated development worldwide may result in little knowledge sharing and slow cost reductions (Neij 1997). A substantial number of demonstration facilities may therefore not be sufficient, without appropriate channels for sharing best practice (van Alphen, Hekkert et al. 2010). Costs have moreover risen for some technologies like flue gas desulphurisation after early large-scale integrated demonstration projects were constructed, due to unforeseen complications (Rubin, Hounshell et al. 2004) as well as limited competition among technology suppliers and the presence of a single dominant buyer (Rai et al. 2009).

$^{19}$This point is based on the relatively low concentrations of CO$_2$ in gas compared with coal. All other things being equal, the unit cost of capital for CO$_2$ capture is thus higher for gas than coal. In addition, there is less incentive to mitigate CO$_2$ from natural gas combustion as continued emissions may still be within a national carbon budget.

$^{20}$The learning rate for a technology is defined as the cost reduction (in %) that results as installed capacity doubles.
Incentives and financing

Financing wide-scale deployment has so far been imagined in the form of a mix of private and public funds from an increased carbon price in the EU Emissions Trading Scheme (EU ETS), government-specific funding for CCS (such as feed-in tariffs, levies and direct demonstration and deployment subsidies), financial transfers to developing countries under the Clean Development Mechanism (CDM), and carbon taxes. Obstacles exist for all of these to be implemented and to support large-scale CCS development. In the case of the EU ETS, which has long been seen as the primary funding mechanism for stimulating RD&D in and deployment of low-carbon technologies within the EU, an adequate price signal has yet to emerge. And a lack of credibility in the ETS’s ability to produce this signal may create perverse incentives for private developers to invest in high-carbon energy generation as the future price of carbon is discounted, in turn further eroding the market’s credibility (Blyth et al. 2007).

In anticipation of the ETS price failing to meet the near-term CO₂ avoidance cost for CCS in power plants, the EU Commission announced plans in 2008 to build 12 demonstration projects by 2015 with public financing from the ZEP (c. €1 billion), the European Energy Programme for Recovery (EEPR) (c. €2.2 billion), and 300 million ETS allowances under the New Entrants Reserve (NER300, valued at c. €2.1 billion at the start of 2013). Most of these funds have so far failed to support the development of demonstration projects (Littlecott et al. 2013) and the limited funding available under the EEPR (€180 million per project) has meant that some successful entrants in the past have declined to pursue projects due to a shortfall from other funding channels. According to the Global CCS Institute, over $20 billion of public funds are expected to be allocated worldwide in the next few years to support RD&D (GCCSI 2011b). However, the allocation process in the EU has been delayed since it was first announced and worldwide several projects are facing reduced public financing commitments.
After years of negotiations, CCS has now been accepted as a mitigation technology eligible for financing under the CDM’s project activities. This opens the door for some countries under the Kyoto Protocol\textsuperscript{21} that have relatively large fossil fuel usage – China and India in particular – to invest resources in installing CCS, supported by these technology specific funds. Preliminary regulations are in place to facilitate this, but at this stage it is unclear what a project application for CDM certification will look like.

Finally, carbon taxes may be combined with revenues from a carbon market in some jurisdictions, but only Norway has so far implemented a tax that is high enough to make CCS technology development cost-competitive with unabated emissions. This tax is, however, restricted to offshore oil and gas extraction rather than covering the entire power sector or other high-carbon industrial installations.

As an alternative to financing mechanisms, a CO$_2$ emissions performance standard (EPS) could be passed on an individual country basis or for a region such as the EU. Such an EPS was first introduced in California in 2007 where the allowable average emissions are just below 500 gCO$_2$ per kWh. This is lower than conventional pulverised coal-fired plants, but higher than most combined cycle gas turbine (CCGT) plants, and therefore does not effectively mandate use of CCS in the near-term. Similar legislation has since been introduced in Oregon, Washington and Montana where it has complemented the voluntary carbon market, the Western Climate Initiative. In the EU, modelling work has suggested that the introduction of an EPS limited at 350 gCO$_2$ per kWh for plants above 100 megawatts (MW) capacity from 2010 onwards, could reduce power plant emissions by a third (Wartman \textit{et al.} 2009). Legal analysis has concluded that such a standard would be admissible as part of existing EU Directives (Client Earth 2008). However, the EU Parliament announced in 2009

\textsuperscript{21} Non-Annex 1 countries are defined as signatories to the Protocol that are not required to meet GHG emissions reduction targets. Instead, such countries are eligible to receive carbon offset funds from Annex-1 countries - required to reduce their own GHG emissions - to help finance carbon mitigation projects.
that an EU-wide EPS would not be passed. It is therefore left to individual Member States to decide whether they want to include a standard as part of a general incentive package for CCS.

**Legal developments under international law**

Global legal efforts to govern and support CCS deployment initially focused on the removal of unwitting legal barriers, particularly with regards to CO₂ storage, and subsequently on the design of dedicated legal and policy frameworks to facilitate capture, transport and storage. Developments in many legal jurisdictions (individual countries as well as regions such as EU) are now increasingly focused on consolidating, refining and implementing such legal and policy decisions (Macrory 2011). An example is the EU where detailed guidance documentation was released in 2011 to address several uncertainties that were flagged in early regulatory formulations.

Early legal questions were concerned with the legality of CCS under international marine legislation. In the absence of a specific provision, the 1996 Protocol to the 1972 London Convention on the Prevention of Marine Pollution by Dumping of Wastes and Other Matter (the London Protocol) and the 1992 OSPAR Convention on the Protection of the Marine Environments of the North-East Atlantic were seen as involuntarily prohibiting CO₂ storage under the sub seabed (Havercroft and Purdy 2007; Hendricks et al. 2005). Over a relatively short time, agreement was reached for amending such treaties (London Protocol 2006; OSPAR Commission 2007a, 2007b, 2007c) to enable offshore storage of CO₂ under specified conditions (Armeni 2011). In 2009, an important amendment to Article 6 of the London Protocol was also adopted to enable the transboundary export of CO₂ (London Protocol 2009). Despite the significance of these amendments, some issues remain. While the amendment to the OSPAR Convention is now entering into force the CO₂ transboundary export amendment to the London Protocol has not been ratified by an adequate number of parties. This means that a prohibition is still in force that restricts
the transportation of CO₂ from one legal jurisdiction to another, an obstacle that will need to be resolved before large integrated pipeline networks can become a reality.

**Legislation in the European Union, Australia and North America**

These international hurdles have, however, not prevented CCS from gaining a primary role within national climate change strategies – mainly within the EU and its Member States, in Australia, and in North America. The adoption of the 2009 EU CCS Directive (EU COM 2009) represents the ‘first comprehensive legal framework for the management of environmental risks related to CCS worldwide’ (Doppelhammer 2011: 93), placing the EU in a leadership position on the regulation of the technology. The Directive mainly governs onshore and offshore CO₂ storage (from exploration to post-closure), but some provisions also cover capture and transport. It establishes a permitting regime and criteria for enabling storage activities in the EU, with special attention to site selection, risk management and monitoring. In case of leakage, the Directive also establishes that the operator is responsible for corrective measures, a surrender of CO₂ allowances under the EU ETS (EU COM 2003)²² and preventive and remedial actions under the Environmental Liability Directive (EU COM 2004). It includes an obligation to provide financial security for stored CO₂ and the transfer of long-term responsibility for site safety to a competent national authority, if the site operator fulfils specified conditions. The Directive also establishes a carbon capture readiness requirement for all combustion plants with a rated electrical output of at least 300 MW. This means that consent to build new combustion plants is subject to an assessment of the technical and economic feasibility of retrofitting CCS in the future, when the technology is proven. If such assessment is satisfactory, sufficient space must be left available at the site for CCS equipment to be installed. Finally, the Directive removes legal barriers to CCS deployment embedded in existing EU laws (including the Water Framework Directive, the Integrated Pollution Prevention and Control

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²² Chapter 6 assesses how the responsibility of operators emanates from a particular interpretation of risk formulated in these regulations.
Directive, the Waste Framework Directive and the Environmental Impact Assessment Directive) to avoid double regulation. All EU Member States were obliged to adopt national legislation implementing the CCS Directive by June 2011. The European Commission has since issued four Guidance Documents to support Member States with the interpretation and implementation process.

Australia has also been a pioneer in the area of CCS regulation, which has developed at various levels of government. An overarching Commonwealth framework regulating offshore CCS (Australian Department of Resources, Energy and Tourism 2011) combined with implementing regulations and state legislation – both onshore in Victoria (Victorian Government 2008), Queensland (Government of Queensland 2009) and South Australia (Government of South Australia 2011), and offshore in Victoria and Western Australia (Government of Western Australia 2008) – is currently in force. However, different approaches and inconsistencies can be observed in the various frameworks (Gibbs 2011).

While the Commonwealth Offshore Act, the Victorian Offshore Act and the South Australia onshore legislation are based upon existing petroleum legislation, both the Victoria and Queensland onshore legislations constitute autonomous regimes. In one case (Western Australia), project-specific legislation has even been enacted to regulate the Gorgon Project. Different approaches to regulation have emerged for particular provisions, such as those governing long-term liabilities, which can either be transferred to the Commonwealth/State or remain with the operator (as in Victoria and Queensland).

The state-of-the-art in CCS law and policy is also quite diverse in North America, which has chiefly been influenced by the experience with EOR. In the absence of a concrete US federal government initiative, the Federal Environmental Protection Agency (EPA) has led regulatory activities for CCS. In 2010, the EPA finalised both the requirements for CO₂ storage under the Safe Drinking Water Act’s Underground Injection Control (UIC) Program, including the development

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23 As of March 2013 only one Member State had yet to communicate its position to the Commission.
of a new class VI of injection wells (EPA 2010a), and the reporting requirements under the Greenhouse Gas Reporting Program for facilities that inject CO₂ underground, with or without permanent storage (EPA 2010b). At state level, extensive legal activity has produced a patchwork of legislation initiatives. These have largely not been coordinated and legal concerns related to transboundary migration and differentiated long-term liability frameworks may therefore need to be addressed further in the near future.

In Canada, the province of Alberta has recently taken on a national leadership role for the policy and regulation of CCS. In 2010, Alberta adopted a series of amendments to existing legislation to clarify the regulatory framework for CCS in its territory (Alberta Minister of Energy 2010). The Act vests the government with the ownership of the pore space for CO₂ storage and authority to issue storage licences and leases. Upon certified site closure, ownership of stored CO₂ is vested in the Crown, which assumes all long-term liabilities (including claims in tort), when the claim is the result of activities carried out as part of an agreement with the government for injecting CO₂ and the operator has complied with all other relevant regulations. A financial contribution to a post-closure stewardship fund is also required to enable the province’s government to cover the costs associated with the transfer (liability as well as monitoring and management of facilities). More recently, the Alberta government adopted regulations on pore space tenure rights (Alberta Minister of Energy 2011); undertook a wide regulatory framework assessment for CCS in order to issue regulations, bringing together international expertise; and proposed the introduction of an EPS and a CCS readiness requirement for new coal-fired power stations. Legal developments can also be observed in the province of Saskatchewan where amendments to existing legislation have recently been passed to regulate CO₂ transport and storage.

Figure 2.1 below illustrates the key stages in regulating a storage project in many of these jurisdictions. There are generally four key stages involved: 
exploration (seeking out and characterising a potential geological formation
suitable for CO₂ storage); operation (during which CO₂ is injected); closure (when injection ceases and the injection well is permanently sealed); and post-closure (when monitoring continues for several years). In most places the site operator is the party responsible and therefore liable for the capture, transport and storage stages during the operational period. After injection ceases, financial requirements are usually placed on the responsible party who will then be liable for any costs arising during a post-operational period, usually between 15 and 50 years. Beyond this point, a national authority is designated as the responsible party under all legal frameworks (ICF International 2012).

![Figure 2.1: The legal stages of a CCS project (IEA 2010a).](image)

**Prospects in emerging economies**

Potentialities and prospects of CCS have also started to be discussed in several emerging economies (Kyoto Protocol non-Annex I countries). The IEA has estimated that, in order to maintain a commitment to reduce emissions by 50% by 2050 compared to 2005 levels (IEA 2008), 50 demonstration projects will be needed by 2020 in non-OECD countries (IEA 2009). In recent years, several of these countries have shown significant interest in the technology and some have begun assessments of their legal regimes to incorporate demonstration activities, which has lead to a definition of such countries as “second generation CCS lawmakers” (GCCSI 2011b: ix).
China has shown particular interest and has established financial arrangements, international RD&D partnerships and capacity collaborations to support six major demonstration projects. South Africa has developed an ambitious roadmap for commercial deployment by 2025 and plans to produce a draft legislative and regulatory framework in the coming years. It is also considering the introduction of a carbon tax and carbon capture ready requirements (Beck et al. 2011). Conversely, India has been more reluctant to commit to CCS, mainly because of cost considerations, competing priorities for poverty eradication, and the risks and uncertainties in technology development (Kapila et al. 2009; Rajamani 2011). However, steps have been taken in the context of international RD&D collaborations, capacity building and financial support. Brazil has likewise been cautious and actively opposed the inclusion of CCS within the CDM. This was partly due to concerns associated with monitoring, environmental and health impacts, liabilities, and the possibility that allowing CCS projects within the CDM “would divert from the central idea of the CDM which is to promote long-term benefits in the direction of low carbon economy towards creating subsidies to enhance fossil fuel production” (SBSTA 2010: 8). Despite such opposition, some international R&D collaborations and capacity building initiatives are being developed in the country. Other emerging economies that have launched initiatives include South Korea, Indonesia and Malaysia (GCCSI 2011b).

**Key legal issues for regulation**

Resolutions in recent years to specific legal definitions have been pivotal in establishing regulatory frameworks for CCS. Particularly important is the qualification of CO₂ as waste or as a commodity. This question has been partially resolved by excluding permanently stored CO₂ from lists of waste materials, where disposal would otherwise be prohibited under international marine dumping legislation, and from the scope of EU waste legislation. Recently, the US EPA has also proposed to exclude CO₂ from legislation governing hazardous
waste, provided that storage complies with rules established under the Safe Drinking Water Act. However, some interpretive issues remain with respect to the treatment of CO₂ under the 1989 Basel convention on the control of transboundary movements of hazardous waste, and the 1991 Bamako convention on the control of transboundary movements of hazardous waste into and within Africa, neither of which mention CO₂ specifically.

Another issue is the permanence of stored CO₂ and the potential consequences of leakage. Leakage from a store ('non-permanence') could result in damage to the environment, human health and physical property, as well as in wider economic losses from price effects on land. Leakage will also have implications for climate change and carbon mitigation efforts would be compromised by any release of stored CO₂ back into the atmosphere. Provisions to ensure permanence and to regulate the consequences of leakage are therefore at the core of legislation – site selection, risk assessment, risk management, and continued monitoring activities are central elements of all regulatory frameworks. So are short- and long-term liability frameworks. Chapter 6 discusses these legal issues in relation to the production and presentation of scientific evidence within the EU CCS Directive.

The IEA has analysed several legal issues and best practices discussed in this chapter across a number of jurisdictions, with a view towards supporting development of a bespoke legal and regulatory framework (IEA 2010a). However, it is important to note that individual jurisdictions are likely to modify such tailormade suggestions to fit their contexts. More detail on recent legal developments across several jurisdictions can be found in the IEA's annual Legal and Regulatory Review publication (2012).
3. Expert perceptions of uncertainties related to CCS in a UK context

This chapter has been revised and updated from an earlier version published in 2011. The chapter begins with a short overview of relevant theoretical literature about expert expectations concerning technology development followed by an examination of the role of coal and CCS in the UK context. Past research findings on expert perceptions are then discussed, followed by a section on research methods. Interview findings and a survey with 19 UK expert stakeholders from industry, research, and non-governmental organisations (NGOs) are then presented and discussed in the context of the theoretical literature. Finally, I propose a theoretical term – conditional inevitability – to capture the form of argument employed by interviewees. This follows the concept of ‘interpretive flexibility’ by acknowledging the tendency of stakeholders to attach particular visions and expectations to emerging technologies, which reflect their interests.

Introduction

As discussed in the previous chapter, it is generally argued within CCS research and advocacy circles that the individual technologies required for large installations have been technically proven from their wide adoption in hydrocarbon systems. However, the absence of experience integrating these components in large-scale CCS systems poses a simple technological challenge to prospective analyses of development capacity such as the IEA’s publications. For CCS to have a significant mitigation impact, the IEA estimates that 100 plants would need to be fitted by 2020 and scaled up to 3,400 projects by 2050, all of which would require an investment of trillions of dollars (IEA 2009b). Given these significant costs and the uncertainty around the state of the technology, some stakeholders have opposed development on grounds of long-term safety and the redirection of resources from renewable energy initiatives (Greenpeace 2008).

Other challenges may be present in the form of public opposition, policy inertia, regulatory hurdles and financing shortages. This chapter explores these issues by asking how a wide range of CCS experts from industry, research groups and NGOs perceive uncertainties in the technology and the emerging policy domain. The policy focus was restricted to the UK and expert perceptions were collected through a short survey and 19 semi-structured interviews in 2009. These data are presented in the context of policy developments around that time and in years since. The UK focus was partly selected because the country has had a specific CCS policy in place since 2009. This proved insightful when comparing the expectations of stakeholders at the time with policy developments in more recent years. In addition, researchers and industry partners in the UK have had a significant involvement with international CCS research since the 1990s. It therefore seemed prudent to place these perspectives in the context of literature on expectations in technology development and to ask to what extent views were shaped by this history.

The findings in this chapter have been analysed following the constructivist theoretical agenda in STS (as set out in the introductory chapter). A constructivist perspective broadly posits that beliefs – in this case about science and technology as a basis for social policy – should be understood as arising from particular social contexts and processes. Rather than attempt to understand how these may be linked to the attainment of maximum benefit or wellbeing (I do not dispute that individuals and organisation alike strive for such things), the aim of such analysis is to understand assumptions and conflicting rationalities. As an example, research within this tradition has considered what drives political and policy changes in relation to environmental regulation over decadal timescales (Inglehart 2008; Gray 2000; Mol 2000). A classical account is Hajer’s (1995) study of the drivers behind legislation for flue-gas desulphurisation (FGD) in the 1980s. Hajer showed that while rival political factions agreed that acid rain could be ameliorated by fitting FGD in power plants and capturing sulphur dioxide (SO₂), disputes arose over differing
interpretations of the underlying scientific evidence basis, as well as the scope for extending emissions control to nitrogen oxides (NOx) from automobile exhaust. While Hajer notes that differing interpretations of evidence arose out of a clear concern with securing the continued support of well-established political factions, he is likewise interested in the matter of conflicting rationalities related to this evidence. In short, how could the same information be attached with very different conclusions about likely outcomes and repercussions to public health and the natural environment? To account for this difference of opinion Hajer suggests that scientific uncertainty facilitated the justification of culturally distinct value positions. Collingridge and Reeve (1986) as well as Jasanoff (1990b) similarly discuss how different interpretations of scientific evidence and risk have supported on-going debates about safety in public policy.

A wider field of empirical research similarly questions the role of rationality in assessments of scientific research and technological development. Slovic (1987) has suggested that subjective decision-making and bias may prove costly as unfamiliar or poorly analysed phenomena are discounted as inconsequential. As an example he points to the partial core meltdown of the Three-Mile-Island nuclear reactor in 1989, where risk management costs subsequently escalated. Other cases of partial nuclear core meltdowns, electricity supply grid failures, and near outbreaks of flu pandemics show that experts draw very different conclusions with regards to risk assessment and risk management (Perrow 1999, 2007). More generally, the tendency to err on the side of positive estimates is confirmed by findings in decision theory where respondents tend to systematically support initial positive statements in the face of contradictory evidence (Koriat et al. 1980; Fischhoff et al. 1977). Inclinations towards positive errors have also been witnessed in forecasts of future energy demand (Smil 2000) and expectations for emerging technologies to meet these, as technological and economic barriers are discounted (Utgikar and Scott 2006). Related to this is Bazerman’s (2006) finding of pervasive positive beliefs attached to the role of technology in mitigating climate change.
At this point it may be instructive to critically assess some of the assumptions underlying the cost studies presented in the previous chapter. Rubin, Taylor et al. (2004) reviewed learning rates for a range of environmental control technologies developed over several decades and suggested that a historically observed rate of 11-12% should apply to future CCS development. Similarly, Rubin, Yeh et al. (2007) argued that early costs could decrease dramatically after the first tranche of installed capacity. These applied learning rates are however in the upper range of those observed for coal power technologies, which lie between 3-13% (Jamasp and Köhler 2007). More damaging to the argument is the history of SO$_2$ mitigation in Europe where costs only declined significantly well after FGD technology had matured (Chalmers et al. 2009). And Rai et al.’s (2009) examination of FGD in the US context shows that significant cost reductions only occurred after the government regulated emissions limits, thereby pushing technology development.\footnote{Rubin, Taylor et al. (2004) do however note the importance of context-specific analysis to guide appropriate innovation policies and the likelihood that learning rates will be inconsistent over several decades as a technology matures.} It may therefore be prudent to look beyond technology learning rates when future trajectories are assessed and to consider how a wider set of stakeholders and social dynamics influence development in a ‘social learning’ process (Wenger 2000). Markusson et al. (2012) have thus suggested that we have much to learn about the potential for CCS development by considering how national characteristics shape trajectories. Similarly, it may be instructive to question the social drivers and framings behind visions of future developments by CCS experts, if simply for the instrumental purpose of assessing their reliability.

Such differentiated beliefs attached to technology (un)certainty and future trajectories are the subject of Donald MacKenzie’s ‘certainty trough’ model (1999). The trough compares three groups’ relative perceptions of uncertainty in complex technologies - developers, users, and outsiders. The model proposes that perceptions of uncertainty are lowest among users – those responsible for
implementing the technology in practice – and highest among outsiders – often sceptical of the technology’s merits. MacKenzie first noticed this relationship while researching beliefs around the targeting technology used in intercontinental ballistic missiles and since then found a similar pattern in perceptions of computer systems safety.

![Figure 3.1](image)

Figure 3.1: The certainty trough after MacKenzie (1999). Reproduced with permission from the author.

Lahsen (2005) has criticised the certainty trough as a simplistic model of group affiliation, partly because it fails to account for the tendency of scientists to express inflated beliefs in their own research. Shackley and Wynne (1995) however found a pattern similar to MacKenzie’s in a study of data flows from global circulation modellers to climate change impact modellers. Other research has documented differing interpretations of climate data stemming from inconsistent approaches to uncertainty across IPCC working groups (Swart et al. 2009). In assessing whether the model is applicable for a particular piece of research, we may therefore take a point from Woolgar (1994) who suggests that the conceptual value of the certainty trough is generalisable in technology innovation. The value of its application will thus depend on the research question and scrutiny of a particular methodology.
The certainty trough has been used in this chapter to inform stakeholder categories and to assess perceptions, and its application will be discussed following a review of the relevant UK policy context and past empirical findings on expert perceptions.

**Energy generation and CCS in the UK**

Historically, coal has played a significant role in the UK energy sector. In 1970 coal accounted for 47% of primary fuel consumption in energy generation. By 2011 this proportion had declined to less than 16% while reliance on natural gas steadily increased from about 5% to about 40% over the same period (DECC 2012). This shift has partly been facilitated by the economics of running flexible gas plants as well as the EU Large Combustion Plant Directive (LCPD) on emissions of SO$_2$ and NO$_x$, which will force 12 gigawatts (GW) of coal capacity to close by 2016 (National Grid 2010). However, the UK continues to be very fossil fuel dependent – in 1970 fossil fuels accounted for 96.5% of all energy generation, and this fell to just 87.5% in 2011 (DECC 2012). Over this period a significant shift has however occurred from coal to natural gas in power generation. This has led to a marked reduction in absolute carbon emissions from 204 MtCO$_2$e in 1990 to 144 MtCO$_2$e in 2011$^{26}$ (DECC 2013a). The construction of gas power stations during the 1990s, with relatively low emissions compared to coal, thus led to a dramatic reduction in carbon emissions intensity in the power sector from nearly 800 gCO$_2$ per kWh in 1990 to just under 500 gCO$_2$ per kWh in 2011 (CCC 2012).

$^{26}$ Data from previous years could not be obtained.
These trends imply that while the intensity of carbon emissions has decreased significantly, the UK continues to face a high carbon lock-in from a preference for hydrocarbons in energy generation (Unruh 2000, 2002). This is a trend that may persist for the coming 20 years regardless of climate change mitigation policies, as combustion levels are largely determined by plant operational lifetimes (Scrase and MacKerron 2009). Markets largely determine short-term plant operations independently of such policies as evidenced by the fallout of lower gas prices in the US following large finds of shale gas. As a result much of the coal produced has been exported at lower prices to Europe where it competes with gas. 2012 has consequently broken with the trend of increasing gas dependence illustrated in figure 3.2, even in the UK where no new coal plants have been built in recent years (Economist 2013).
Where market conditions involve an incentive to shift to relatively high-carbon fuels at a lower cost, some form of regulations or incentives are clearly necessary to ensure a low-carbon trajectory in the power sector. In late 2007 the UK government therefore adopted a policy to fund construction of a demonstration-scale post-combustion capture plant through a competitive bid. The exclusion of other capture technologies was justified on the basis that coal was believed to continue to occupy a central role in emerging economies where existing plants would need to be retrofitted with post-combustion technology in coming years to mitigate emissions (DTI 2007). In the following year wider mitigation legislation followed, such as the passage of the Climate Change Act in 2008, which provided a legal mandate for mitigation with a commitment to reduce domestic emissions by 80% in 2050 relative to 1990 (HM Government 2008a). Also in 2008 the Energy Act introduced a legal regime for geological storage, by designating a gas importation and storage zone (HM Government 2008b). In April 2009 the Department of Energy and Climate Change (DECC) announced the Framework Development for Clean Coal, which mandated all new coal-fired power plants above 300 MW to fit capture technology upon construction – in line with the EU Commission’s capture readiness requirement – and full capture was mandated within five years of the technology becoming technically and economically feasible (DECC 2009). The 2010 Energy Act created a legal support structure for a CCS levy and for non-competitive instruments to be considered by the government (HM Government 2010). Such an instrument was first officially considered in the autumn 2010 consultation on a CO₂ EPS (Energy and Climate Change Committee 2010), since carried forward as part of the wider Electricity Market Reform (EMR).²⁷ In late 2010, as

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²⁷ The EMR includes a number of regulatory and incentive elements to attract investment into the UK’s energy market. Contracts for difference (CFD), which are stable government payments to provide predictable incentives to invest in low-carbon generation. A capacity market to ensure procurement of desired forms of electricity supply. Long-term investment contracts to enable early investment in advance of the CFD coming into force in 2014. Various financing measures to ensure that generators have access to markets. Renewables transitional arrangements for investments under existing phased-out incentive schemes. And finally, an emissions performance standard (EPS) to limit carbon emissions from new fossil fuel power stations (DECC 2013b). The proposed EPS has been set at 450gCO₂ per kWh to mandate CCS on coal-fired power plants while permitting unabated gas plants to be built (DECC 2013c).
only two entrants remained in the CCS competition, E.ON Power withdrew its bid and only a consortium led by Scottish Power’s Longannet coal-fired power station remained. E.ON’s decision came as the government announced that CCS demonstration funding would be reduced from 2 to 1 billion pounds and that the CCS levy would be placed on hold (HM Treasury 2010). In February 2011, then Energy Minister Charles Hendry said that he hoped the Longannet project would be chosen as the competition winner pending agreement with the Scottish Power consortium by summer 2011 (DECC 2011). However, later that year the government decided not to award the project with funds, citing among other issues, higher than anticipated CO2 transportation costs and the unlikely prospect of advancing with contract terms that would be agreeable to the government and the consortium members.

In the wake of the failure to allocate funds a new competition was issued with the government pledging that the 1 billion pounds would be granted to a single winner (HANSARD 2011). In contrast to the earlier effort there would be no limitations on capture solutions and in March 2013 the field of competitor projects was reduced to two very different options. The Peterhead project with post-combustion capture on a gas-fired power station in Scotland, and the White Rose project with oxy-fuel combustion capture on a coal-and biomass-fired power station in northeast England (Hallerman 2013b). A final investment decision for one of these projects is pending following the preparation of detailed front-end engineering design reports.

Over the years a number of organisations supporting CCS have questioned the suitability of official efforts to encourage the growth of an industry in the UK. Following the failure to allocate funds to the Longannet project the Green Alliance (Benton 2012), a non-partisan multi-stakeholder think tank, called for firm decisions on the geographic location, long-term funding procedures and the fit with overall carbon reduction efforts, a call that was later again issued by the Committee on Climate Change, the body officially charged with providing
policy recommendations on mitigation to the government (CCC 2013). It is within the context of these policy developments, and the previous chapter’s explanation of the state-of-play in technology development, that the following section delves into experts’ perceptions of uncertainties in CCS.

Review of studies of expert perceptions of CCS

The literature on lay public perceptions of CCS is extensive considering the short amount of time the technology has been considered as a climate change mitigation option. Experts’ perceptions of uncertainties have received less attention. The following section highlights research findings on EU and US expert perceptions of CCS, which were relevant to the interviews carried out for this study. The findings are not specific to developments that have taken place since 2010 and should not be read as an up-to-date opinion poll. Rather, an attempt has been made to summarise general findings.

Surveys and interviews largely reveal a high degree of optimism around the prospect for deploying CCS on a large scale (de Coninck et al. 2009; Hansson and Bryngelsson 2009; Van Alphen et al. 2007). One UK study found that a large majority of experts believed there will be a significant number of capture installations in place worldwide by mid-century (Gough 2008), an optimism which was echoed in Hansson and Bryngelsson’s separate study of European and North American experts. Respondents expressed a clear understanding of the gap between their forecasts and the current state of the knowledge and explained that their optimism stemmed from the overwhelming advantages of worldwide deployment. The primary advantage of widespread deployment has

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28 Along with the 2008 Climate Change Act comes an understanding that the UK will need to largely decarbonise its power sector by 2030 and in doing so will need to set emissions equivalent to around 235gCO₂ per kWh. The Committee on Climate Change has stated that this overall carbon-intensity target could be accompanied and supported by an EPS specifically applied to power generation to provide confidence that this long-term goal will be met by short-term measures (CCC 2013). Without an overall carbon-intensity target in place, an EPS on power generation could however imply that gas plants should be constructed to meet short-term mitigation targets while jeopardising the long-term aim of decarbonisation.

29 Note that these interviews were carried out in 2009, two years before the first competition came to a halt.
been identified as the possibility of meeting ambitious carbon reduction commitments (de Coninck et al. 2009; Gough 2008; Van Alphen et al. 2007). Another advantage has been envisaged as a moral imperative to enable the continued reliance on cheap coal in developing economies (Hansson and Bryngelsson 2009).

CO₂ transportation has generally been envisaged as a relatively simple challenge, while studies of storage potentials have garnered more scepticism. Hansson and Bryngelsson found that experts questioned official storage estimates within the European Union, scepticism that was mirrored in UK research (de Coninck et al. 2009; Gough 2008). Most respondents in these studies expressed a belief that public pressure would make onshore storage of CO₂ an unlikely option to pursue and that offshore storage in depleted hydrocarbon fields will be more economically feasible than saline aquifers (Gough 2008). Some experts also expressed concern over the possible de-prioritisation of aquifers, pointing out that learning will need to take place here to allow storage in the long term (de Coninck et al. 2009).

Respondents in academia have deemed that projections from frequently referenced sources generally underestimate the costs of development (Gough 2008). Moreover, de Coninck et al. found that respondents believed that minor adjustments to a cost model’s input assumptions resulted in substantial changes to total project costs, and the associated deployment prospects by mid-century. The same study compared cost estimates from three different sources and concluded that the robustness of modelling results was poor. Gough also found little agreement on the additional level of expenditures that CCS may introduce to electricity production. Answers from a survey with 18 responses ranged from a 2% to a 75% premium, with no relation between level of expertise and response. In contrast to this variation in estimates, Huijtjts et al. (2007) found that industry respondents generally believe CCS will become cost-effective compared with renewable technologies in the long term. A few representatives from NGOs have expressed concern that CCS directly competes with renewable
energy technologies over financing (Greenpeace 2008; Huijts et al. 2007). Oppositely, Hansson and Bryngelsson found that some experts believe the high near-term costs of CCS could promote development of cheaper renewable technologies.

In 2008 Gough found that several experts expressed concern over the lack of clarity in regulatory and policy frameworks for CCS. This is a relevant finding if simply compared with the developments in UK policy since 2007.

These studies show that experts have acknowledged the importance of addressing several types of uncertainties simultaneously to ensure the large-scale and widespread deployment of CCS. They also show that a majority of respondents believe that technical uncertainties can be managed, provided that enough resources are made available at early stages of development. In contrast, respondents have tended to say that here is much greater uncertainty associated with the management of development costs, the shape of emerging regulations and the form of policy initiatives.

**Data collection and analysis**

Semi-structured face-to-face interviews were conducted with 19 UK CCS experts from industry, research organisations and NGOs to gather a broad set of anonymous opinions on science, technology and policies. Selection followed two overall criteria. To ensure that respondents had expertise in the field, the selection process followed guidelines by Cooke and Goossens (1999) who advise that respondents should have a highly cited publication in the field, frequently be invited to speak at conferences, or be recommended by peers. Efforts were also made to select as equal a number of respondents from each of the three stakeholder groups as possible.30

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30 Interviews in subsequent chapters did not follow this selection approach, as a similar need for variety did not present itself.
Respondents were asked to state their perceptions of uncertainty for an extensive list of questions. Technical questions covered the full CCS chain – capture, compression, transportation, injection, and storage of CO₂ – and other areas affecting design including efficiency, fluid monitoring, and storage permanence. Policy questions were structured around the issues most relevant for advancing large-scale deployment - regulatory regimes and incentive structures, carbon lock-in, and government aspirations. Respondents were asked to explain their involvement with CCS and how much uncertainty they believed existed around different issues, where such uncertainty stemmed from, and how it should be addressed. The following are examples of fixed questions that were used as starting points to obtain more information:

a) Is technical know-how from capture of flue-gases from power plants and industrial facilities directly transferable to carbon capture?

b) How likely is it that the North Sea provides sufficient capacity for the UK’s storage needs?

c) Is it possible to estimate the likelihood of leakage for a particular storage site?

While efforts were made to gather comments on as many of these pre-crafted questions as possible, each interview resulted in a different focus depending on background knowledge and willingness to comment. Major areas of interest, concerns, and logics that emerged in early interviews were noted and addressed in subsequent interview rounds to develop a common set of categories for analysis.

At the end of each interview, an 11-question fixed survey was conducted to quantitatively test for the presence of a certainty trough. Respondents were asked to rate the uncertainty surrounding each topic on an ordinal point scale, ranging from low (1) to high (4), for a list of issues that included the following (a comprehensive list with statistical results can be found in annex 2):
1) Application of capture technologies to large-scale CCS projects;
2) Reliable transportation options for CO$_2$;
3) CO$_2$ behaviour in the subsurface.

The survey was developed based on the first three interviews to increase the relevance of the questions and respondents were given the option of abstaining if they believed that their knowledge of a subject was inadequate to supply a clear opinion.

MacKenzie's original certainty trough designates stakeholders as developers, users or outsiders relative to a technology (the latter often designated as critics). This exact characterisation proved challenging to replicate for CCS because much of the science and many of the technologies are only now emerging and some individuals may be involved simultaneously with research, development, small-scale deployment and policy change. Respondents were therefore assigned to one of the three groups depending on their primary involvement. An alternative approach could have been to ask respondents to state where they perceived themselves to lie on a spectrum of the three categories.\textsuperscript{31} In this study, I assigned respondents to one of three categories in accordance with MacKenzie's original certainty trough and Woolgar's perspective that the model primarily serves as a heuristic, rather than a universal framework. Users were categorised as those stakeholders mainly engaged with the integration of new technologies into systems already in widespread use in power generation. Developers were categorised as those mainly engaged with the generation of new knowledge specific to CCS - CO$_2$ capture, compression, transportation, injection, and storage options. Outsiders were easier to classify and included stakeholders who mainly produced commentary and critique, and those championing alternative technologies to CCS.

\textsuperscript{31} I am grateful to a reviewer for pointing this methodological option out after I completed the thesis.
Survey responses were compiled in a spreadsheet, categorised according to technology or policy issues, and compared across stakeholder groups. Outputs from the interviews were developed into a substantive theory following grounded theory analysis.

**Survey findings**

Only one of the 19 interviewees chose not to state an opinion on the 11 topics covered in the survey with the resulting response rate at 95%. Developers’, users’ and outsiders’ mean responses for the uncertainty surrounding CCS technology in the UK were 11 (46%), 10 (42%) and 13.13 (55%) respectively, out of a maximum possible rating of 24 (100%). Policy uncertainty was rated consistently higher across the three groups and the views were more clustered – 15.5 (65%), 16 (67%), and 16.88 (70%), respectively. The survey findings on technology uncertainty indicate a certainty trough, with the mean of developers’ perceptions higher than that of users’, while outsiders perceive the highest level of uncertainty. The resulting trough is depicted below in figure 3.3 where responses from each of the three stakeholder groups are indicated by the three dots on the graph.

![Expert Perceptions of Uncertainty in CCS Technology - A CCS Certainty Trough](image)

**Figure 3.3:** The CCS certainty trough.
The robustness of the findings across the three groups was assessed with a Kruskal-Wallis test, a non-parametric alternative to the standard one-way analysis of variance. It tests whether three or more independent samples are derived from the same population by comparing their distributions from the mean (McNabb 2004). The result shows a relatively high probability (0.29) that the null hypothesis is true. In other words, there is a low probability that the distributions of the three means in the CCS technology certainty trough are drawn from three different populations. The survey responses were also analysed with an eta-squared correlation measure to test the proportion of the variance in perceptions explained by the type of expert responding. The resulting value of 0.409 indicates that a high proportion of the variance in perceptions of technology uncertainty is explained by expert type. Reflecting on this alongside the result from the Kruskal-Wallis test above, a larger sample size may be necessary to conclusively say whether expert type has a significant effect on these perceptions. Alternatively, different sample categorisations from those used here may be appropriate in future surveys, possibly by asking respondents to locate themselves on a stakeholder spectrum.

**Interview findings**

**Technology system certitude**

Nearly all interview respondents expressed a high degree of certainty that CCS as an integrated system was well understood and technically feasible to construct on a large scale. One user expressed this sentiment in terms of the existing knowledge in hydrocarbon and coal technologies: “It is all doable, it is just a bit of a new journey for us all” (R4). Similarly, when the separate technology components were conceptualised as part of an interconnected system, most anticipated that there would be a smooth transition in scale-up from demonstration capture facilities to large-scale use in power plants: “If you can capture 300 megawatts you can capture a gigawatt” (R2).

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32 An eta-squared measure is used instead of an R-squared measure when the independent variable (expert type) is nominal and the dependent variable (technology uncertainty) is ordinal. Guidelines for significance follow Cohen (see Leech et al. 2004: 147): small = 0.10, medium = 0.24, large = 0.31.
The system feasibility concept – its compatibility with existing infrastructure for hydrocarbon extraction and coal combustion, and the ease that this implies for scale-up – was a line of reasoning expressed or referred to by nearly all respondents. It was widely believed that existing knowledge and infrastructure components could be relied on for CCS build-up and that depleted hydrocarbon fields could be recycled as CO₂ storage spaces. Based on storage characterisation data from the oil and gas industry, “it’s all pretty routine stuff,” said one developer (R15). Many respondents also agreed that the possibility of leakage from a containment site would be small or unlikely following the logic that past field characterisations for hydrocarbon extraction are appropriate for gauging long-term storage safety.

The urgency identified around climate change also led to a conclusion among many respondents that technology solutions in general are essential to limit CO₂ emissions. One developer expressed this sentiment in the following manner: “The only way you’ll increase what we know now is actually by doing it at a larger scale, and if it doesn’t work then it doesn’t work, and we’ll try something else...if it doesn’t work we can build more windmills” (R13). Rather than considering the level of technical maturity in each case CCS was here compared with wind turbines because both are low-carbon energy technologies. These quotes are illustrative of a logic that emerged from several interviews, namely that the certitude expressed in a future system is conditional on learning from other technologies, the potential for rapid development that this learning implies, and the system's role as a large-scale technical fix. Confidence in a potential CCS system was therefore a logical deduction from two main premises: a) a large capacity for transferring knowledge from hydrocarbon extraction systems, and b) the urgency surrounding climate change. This latter premise is particularly noteworthy when trying to understand perceptions around the likelihood of a future large-scale system development – it reveals a belief that the urgency attached to climate change somehow will have a hand in
making CCS a more feasible option independent of the current state of the technology.

**Extrapolations from analogues**

Several respondents pointed out that the longstanding uses of various capture technologies in power plants prove that these can inform choices on absorption materials and corrosion rates in CO₂ capture. Specific technologies that were noted included FGD and natural gas processing, which were seen as useful starting points for gauging the challenges involved in coal- and gas-fired power plants. The extrapolation from analogous knowledge also extended to the transportation of CO₂. Independent of their affiliation, most respondents quoted the 30 years of experience in transporting CO₂ via pipeline in the US as evidence that a safe and reliable CCS pipeline infrastructure can be constructed. One user summed up this view by saying “a pipeline is a pipeline is a pipeline” (R2).

The possibility of knowledge transfers from other technologies was also expressed in views on storage site selection. As noted above, most respondents believed that existing site characterisations of depleted oil and gas fields from hydrocarbon extraction would enable the selection of locations that could ensure permanent storage. In addition, some expressed that natural CO₂ seepages and accumulations serve as proof that leakage is rarely dangerous to human beings and can be contained so as not to endanger the surrounding ecology. Lessons from what were deemed to be analogous experiences with similar technologies led a majority of respondents to express that the CCS system could be constructed to ensure reliable and large-scale capture, safe transportation, and permanent storage of CO₂.

**Inductive uncertainties**

However, the same respondents who expressed certitude that a CCS system could be managed safely and reliably noted that several specific uncertainties cast doubt on the appropriateness of basing assessments on analogues. As an
example, most agreed that a thorough characterisation of the geology is the single most important step towards reducing uncertainty around storage capacity. It was also largely agreed that the characterisation of one site cannot directly inform the storage capacity of another location, and that the resources required in selecting sites have been underestimated in official figures. Concern was therefore expressed over the lack of suitable data and the difficulty that this implied for assessing the feasibility of project deployment: “The storage solutions have not been created to the same level of rigour and thoroughness as the capture piece, so the storage generally I think is going to be a problem” (R2).

Such responses indicate the presence of an epistemic tension in beliefs. Optimism for the feasibility of a future system was expressed on the back of hydrocarbon technology analogues and the urgency of climate change – a deductively derived certitude – and a concurrent scepticism was based on empirical evidence from CCS-specific research – an inductively derived uncertainty – independent of stakeholder support for the technology. Another example of this tension came up with a respondent identified as a developer, who initially expressed certitude that the knowledge of capture and transportation was sufficient to begin large-scale deployment in the UK by 2020. When questioned about specific processes in the capture technology, this respondent pointed out several uncertainties that would need to be resolved, including solvent stabilities, flue gas contaminants, and amine reuse cycles. This respondent and several others also acknowledged that vast amounts of resources and research from several demonstration projects would be necessary to appreciably reduce technical uncertainties, a set of conditions that most believed would not be met in the UK over the coming decade.

**Addressing technology uncertainties**

None of the respondents provided explicit clarifications on how certitude in the technology system could be maintained alongside such critical uncertainties. An explanation was therefore inferred from perceptions around the support network for CCS. Most agreed that very different vested interests converge in
support of the technology for a variety of reasons. Many also believed that this convergence ensures that large resource allocations will be made available in the future to bridge the resource gap. And, provided enough resources in the form of financing, skilled labour, and demonstration plant experience are made available, respondents said that large-scale and widespread development would be assured. Certitude in the eventual scale-up of the CCS system was thus maintained on grounds that a large and varied network of supporting actors would ensure that critical resources are gathered to remove uncertainties in the technology over the coming decade. It is worth noting the significant uncertainty that was attached to such policy prospects across all stakeholder groups in the survey results.

Following the view that this collection of resources will ensure successful large scale deployment, users and developers were largely optimistic that whichever type of capture technology receives the greatest level of financial support over the coming decade will become the technology of choice in the UK. In contrast, outsiders largely believed that post-combustion capture will be the preferred option, because its end-of-pipe application offers power companies the flexibility to operate plants when electricity prices are highest. Both views share the premise that financial reasoning, rather than developments in science and technology alone, will determine the choice of technology. Both lines of reasoning also imply that economic and political drivers are intimately connected in the technology selection and development process. The economic driver was identified as industry’s support for a technological fix that allows the continuation of a coal or hydrocarbon based business model. The political drivers identified were more varied and serve several agendas including climate change mitigation, energy security, and a tradition of state-sponsored support in the energy sector. One outsider commented that support for CCS in UK energy policy was mediated by a need to appease several constituents: “CCS is the way out, the escape route, the escape hatch” (R8).
Financial support

There was little confidence that the resource gap could be addressed sufficiently solely with funding coming from the EU ETS and other sources in the EU Commission. Industry was likewise not perceived as a willing primary investor because of lacking financial justification for CCS. One industry respondent defined as a user in this study explained the consequences of this scarcity in terms of financial guarantees: “We would not be about to invest many hundreds of millions of dollars in something which would not have a guarantee or something that is close to a guarantee of running” (R2). The implication of this assertion, supported by most other respondents’ comments, is that development of demonstration plants will not take place without some financial justification or guarantee that CCS will play a significant role in the UK energy sector’s future.

The task of providing such resources and guarantees was largely seen as the responsibility of government. Some respondents mentioned that this could initially take the form of a mandate on information sharing, because the UK’s competitive electricity market hinders co-operation and obscures how far along different companies are with technology development. Others indicated that detrimental consequences could arise from a lack of science, technology and policy co-ordination on a European level. These views indicate that governments could be caught in a CCS ‘prisoner’s dilemma’. Since no adequate formal agreements to share the significant financial burden of development between nations were thought to exist there would likewise be little incentive to allocate public funds on national levels.33

Many respondents equated the scale of future financial allocations in the UK with the number of demonstration projects that were seen as desirable in official policy communications, and ultimately the amount of learning and training that would need to take place over the decade. Following the 2009

33 This argument continues to be buttressed by the low price of carbon in the EU ETS, still envisioned as the primary financial mechanism for future large-scale CCS.
policy on CCS, most therefore believed that two to four projects would be built in the UK over this period. Stakeholders identified as developers were however concerned that this would be inadequate to mitigate the technical challenges in scale-up to full capture. While two to four demonstration projects were seen as adequate to produce a few highly skilled experts by the end of the decade, there was concern over the learning required to deploy CCS more widely: “If you did two 300 megawatt projects for 2014 and another two 300 megawatt for 2019 and that was it, then no, you wouldn’t have the human capital ready to do it, for full roll out by the 2020s” (R6). To facilitate this learning, this respondent believed that as many as 15 to 20 demonstration plants would be necessary in the UK over the coming decade.

Executive power

All respondents believed that government’s role would therefore have to extend beyond knowledge sharing and into increased financial support and mandates. Government was seen as having the power to determine the future of CCS by allocating R&D funding, mandating capture in power plants, and setting a floor on the price of carbon. Without efforts of this kind, large-scale and widespread deployment of CCS in the UK was deemed to be an impossible undertaking: “Without government subsidy these projects will not happen” (R14). There was concern over the failure to provide such support to date and a frustration over what was perceived as inertia in political circles. It remains to be seen whether the forthcoming EMR will meet stakeholders’ expectations for comprehensive and aggressive measures, along with the new CCS competition.

This view was shared by many respondents who believed that government had historically shaped energy technology developments by strategically championing specific solutions and would continue to do so. Nuclear power was pointed out by many as the technology of choice in political circles where continued financial support would ensure that it maintained a role as a dominant low-carbon energy generation option. Some noted that the costs of this enterprise were likely similar or greater to the alternative of financing a
large fleet of CCS demonstration projects and constructing a common pipeline infrastructure. Without increased subsidies and a coordinated development strategy most perceived CCS as a lost cause because the market mechanism for carbon had proven to be inadequate:

*It’s clear to me that without something pretty radical on the international scale, and the EU scale, and a level of political commitment that isn’t currently present in all the major economies of the world, the EU ETS is never going to have a price that is high enough to finance [CCS], because the EU ETS is politically constructed and can be politically deconstructed when people’s jobs are at risk.*

(R8)

This comment illustrates the low confidence that most respondents expressed in the EU ETS as a financial support mechanism, and the politically constructed milieu of climate change mitigation. Many saw the UK government’s commitment to emissions mitigation as a transient policy choice subject to vested interests and political pressures. While it was pointed out above that respondents shared a belief that CCS would somehow receive the resources necessary for large-scale and widespread deployment by 2020, this view was challenged by the equally pervasive belief that interest groups have conflicting goals with regards to energy sector developments. Concerns over financial support and development trajectories, compared with a belief that government will find a way to clear the path for large-scale deployment in the coming decade, therefore indicate that the epistemic tension in CCS extends to the policy realm. Most respondents believed that government eventually would drive forward large-scale deployment, but they also acknowledged that the political landscape was unpredictable and not on track to amass enough resources for adequate learning from demonstration plants to meet this goal.

**Executive misunderstandings**

Several respondents cited competing agendas within government as a main cause behind the lack of a clear policy on resource allocation, as illustrated by the following exchange:
Interviewer: Do you think the government's overall position on why they are backing CCS is clear? Do you think their goals are clear?
Respondent: No I think they are confused. They don’t know how much it’s to the UK emissions, how much it’s to set an example for Asia in particular, and how much it's to develop UK industry to provide equipment to Asia. So it’s not really clear.

(R7)

In addition to the absence of clear objectives there was a widespread belief that government had underestimated key risks bearing on development including the severity of climate change, CO₂ capture costs, and the consequences of long-term storage liabilities. Industry respondents noted that such misunderstandings had increased uncertainties and fostered distrust in the government's intention to create policies that would support development of competitive large-scale CCS within the timeframe announced by DECC.

We may alternatively draw on interpretive flexibility when reflecting on why government actors keep discussions of associated benefits open-ended. As global resource flows change and national policies react, it may be a more successful strategy to continue to comment on the widest possible set of benefits associated with CCS development and thereby draw support from a larger group of potential stakeholders, rather than close down discussion and hone in on fewer supporters. In other words, the apparent confusion identified by industry respondents may be seen as a deft political strategy to include more stakeholders in discussions, adjust decisions to suit a constantly evolving policy landscape and attempt to secure the future of CCS.

Discussion
The survey findings show that a certainty trough, albeit statistically questionable, emerges for CCS technology. The comparative distribution of users’, developers’ and outsiders’ perceptions of uncertainty conforms to MacKenzie’s (1999) hypothesis of perceptions around disputed technologies. Reflecting on the statistical results, a larger sample size may however be
necessary in future research to conclusively say whether perceptions of CCS technology uncertainty are determined by expert type. Alternatively, different categorisations from those used here may be appropriate, possibly by asking respondents to locate themselves on a stakeholder spectrum. As Woolgar (1994) suggests for the original certainty trough, the model that emerges from this research functions best as a heuristic device and should therefore not be interpreted as a conclusive mapping of comparative expert beliefs. And Lahsen’s (2005) critique of the certainty trough as a reductive device that simplifies stakeholder identities is a useful reminder that individuals often hold multiple roles – particularly in the case of emerging technologies such as CCS. This is particularly relevant when reflecting on the model in relation to the thesis as a whole, which progresses from the theoretical notion of interpretive flexibility.

As a heuristic device the trough is instructive in illustrating how specialised communities develop socially contingent perspectives on technology development trajectories. Potential implications for reasoning processes of socially constructed beliefs were evident in the interview findings, which identified an epistemic tension between a deductive certitude that large-scale CCS would be developed within the coming decade and would work as intended, and an inductive uncertainty expressed around a number of technology and policy issues. It was inferred that this contradiction or oxymoronic tension was accepted based on a belief that large resource allocations would eventually bridge the knowledge gap necessary to move towards large-scale construction. At the same time, several uncertainties in the technology system and the absence of a strategic financial framework in the UK and across Europe were perceived as barriers to achieve cost reductions. CCS development is thus perceived as a conditional inevitability – eventual deployment is inevitable because it addresses several constituents’ agendas, provided that numerous technology and policy uncertainties are resolved over the coming decade.
This confirms and extends Hansson and Bryngelsson’s (2009) finding of pervasive optimism among CCS experts, which they attribute to interpretive flexibility (Pinch and Bijker 1984) on the part of respondents. In this specific case, interpretive flexibility has been situated in a particular national (UK) and regional (EU) context giving substance to the tensions within and consequences of stakeholder’s views. Bazerman’s (2006) finding that people commonly hold positive illusions about the role of technology in controlling climate change indicates that the discounting of technical uncertainties is not unique to the CCS domain. This also finds support with Utgikar and Scott’s (2006) finding that technological and economic barriers often are discounted when future energy demands are planned. The tendency to err on the side of positive estimates is furthermore confirmed by findings beyond STS literature in decision theory, which has found that people tend to systematically support initial positive statements in the face of contradictory evidence (Koriat et al. 1980; Fischhoff et al. 1977). It is likewise helpful to reflect on the example of nuclear reactor construction in Britain during the 1980s when a suite of reactors were planned, only one of which was completed, as development complexities inflated costs to unacceptable levels and hindered widespread deployment (Chick 2007).

These interview findings indicate that the presence of subjectivity and value-based judgments should be acknowledged when experts are consulted on the state and future of emerging technology, its potential for large-scale deployment, and the financial policies necessary to reach goals. This is particularly clear when developments in UK CCS policy since 2009 are kept in mind as a reminder that development trajectories may very well confound the hopes and analyses of experts, despite their acknowledgement of large uncertainties. Overly optimistic forecasting may arise from the high expectations that technology developers and scientists attach to their own work (Borup et al. 2006; Lahsen 2005). Another possible explanation is that stakeholders may simply express beliefs strategically to further their agendas in the policy domain. Under either circumstance, such positive framing of science
and technology can create fertile ground for coalitions to emerge across stakeholder groups (Hajer 1995; Spinardi and Williams 2005).

This chapter however goes beyond confirming a pervasive optimism among technology development stakeholders, where prior beliefs often are confounded by realities. Conditional inevitability implies that respondents do reflect critically on limitations and challenges, and that they nevertheless decide to accept a certain outcome as a foregone conclusion. A capacity for and reliance on reflexivity is implied by the term, along with a decision to dispense with key challenges that rise up against a vision. A fundamentalism about potentialities is kept afloat in the face of several hurdles perceived by the interviewees themselves. This characterisation of an epistemic tension that interviewees themselves are involved in constructing is perhaps the most novel insight that arises from the chapter.

The following chapter continues with an analysis of beliefs and practices among geoscience researchers studying CO2 storage.
4. Epistemic resources: commitments and analytical conventions among geoscientists

This chapter and the following continue the analysis of expert perceptions of CCS by focusing on the beliefs and epistemologies underlying social networks in interdisciplinary geoscience research on CO\textsubscript{2} storage. Material is drawn from interviews with 27 geoscientists, close readings of several research papers, and conference notes. The chapter firstly situates the data within the literature on ‘epistemic communities’ and secondly assesses evidence standards by drawing on literature from the philosophy of science and a comparison with climate modelling.

Introduction

The previous chapter explored collective beliefs amongst a range of CCS experts within a particular national policy context. This chapter continues to ask about the existence of commonly held beliefs, as well as practices, and concentrates on a specific area of expertise within CCS, namely geoscientists who work on CO\textsubscript{2} storage research. Since their practices are largely conducted within an international setting the focus is not restricted to a single country, but instead draws on interviews with individuals from research institutes around Europe. The primary question driving this chapter is whether it is possible to identify a thought community characterised by shared beliefs that are rooted in common practices. The chapter seeks to contribute to literature that has considered how group perceptions are shaped by shared epistemologies within expert communities (Knorr Cetina 1999, 2010; Lovell and MacKenzie 2011). This focus originates with the hypothesis that CO\textsubscript{2} storage geoscientists can be described as belonging to a coherent ‘epistemic community’ (Adler and Haas 1992; Haas 1989, 1992). The central idea driving the theory of epistemic communities is that individuals across different institutions with expertise in a similar body of work develop a common belief system, which may lead to policy convergence on specific issue areas. While the concept of interpretive flexibility used to order the analysis in the previous chapter is focused on technology
development trajectories, questions about epistemic communities focus on how specific beliefs are rooted in particular bodies of work and methods, and it is therefore a natural starting point when asking about the relation between practices and (consequent) beliefs.

In some cases common practices may constitute just one element alongside others that lead to convergence of expert beliefs. Canan and Reichman’s (2002) analysis of how emission limits were established for ozone depleting substances during the late 1980s attributes the success of bringing science to bear on global chlorofluorocarbon regulations to the strength of human networks, or ‘social capital’, as they call it:

[T]he global ozone community depends on the relative similarity of dispositions, interests, experiences, habits and the social relationships created among its members, sometimes in spite of not having a common epistemology. Thus, human, social, and cultural capital are more important than shared beliefs and common policy interests” (ibid: 35)

Their work is a challenge to explanations of policy change that emanate only from a focus on common expertise and instead they suggest that a kind of advocacy coalition (Sabatier 1988), albeit constituted by individuals with deep levels of similar expertise, was central in banning ozone-depleting substances. Similarly, the mass of different disciplines and organisational commitments that have coalesced in support for CCS deployment makes it a unique social arena in which to study questions about common discourse and wider advocacy as was done in the previous chapter. In this chapter I focus specifically on an area of knowledge production within the geosciences and it has therefore seemed fitting to ask whether commonly held perceptions can also be traced to shared epistemological foundations.

The theory of epistemic communities was the starting point for asking geoscientists to explain how they understand key uncertainties related to CO₂
storage and to understand how these beliefs arise from their research. Canan and Reichman’s point emerged as a related issue after it became clear that geoscientists from industry, academia and research institutes have enjoyed close relations in past years. They have attended the same conferences (Stephens and Liu 2012), worked alongside one another in several research projects, and have co-authored many papers for scientific journals. Paul Edwards’s detailed history of climate modelling (2010) provides a useful backdrop to reflect on these knowledge production activities. While it would have been more specific to the research activities to compare CO₂ storage modelling with a range of variables describing workflow patterns, analytical conventions and reliance on assessment tools in the hydrocarbon industry, Edwards’s book provides a candid and rigorous account supported by multiple insightful interviews carried out over many years. And with the advent of the IPCC in the late 1980s and its subsequent release of four assessment reports reviewing climate science, climate modelling has been extremely well reported in the public domain. Long before this, climate modelling was also carried out in the public domain within universities and national research laboratories. In contrast, hydrocarbon extraction research has been the domain of primarily private-held oil and gas companies with a definite commercial interest in maintaining confidentiality. Because of the ease of access to Edwards’s rigorous account and the role of CCS as a research topic arguably carried out in the public’s interest, the comparison with climate modelling seemed both apt and useful as a way to organise concepts and discuss work processes within CO₂ storage modelling. In the next chapter I compare one category of empirical data collection with its conventional use in oil and gas extraction, again to emphasise accepted conventions in CO₂ storage research. In that specific case sufficient information was supplied in interviewees and written reports to make some insightful comparisons with the hydrocarbon industry’s use of similar technology.

In this chapter I have not sought to explain the variety of models used by researchers or assess the theories underpinning different model environments.
An entire PhD thesis could perhaps be devoted to the analysis of commonality and divergence across model results and to investigate how modellers are grouped into specialised micro-communities. Additional granularity could arguably also be achieved by analysing perspectives according to institutional and sectoral affiliations (primarily research or commercial development) and ask whether beliefs could be explained with reference to interest theory, or RCT. As I explain in the previous chapter, an RCT perspective can no doubt help explain questions related to economic exchanges and interests, but during my data collection it became clear that CO₂ storage researchers have been engaged in common efforts and developed shared perspectives that have transcended such sectoral affiliations. For this reason I also decided not to catalogue the list of different modelling environments, the theoretical assumptions underlying each, and the analytical conventions relied on. While I do point out a few key areas of divergence in thought that may be interpreted as grounds for controversy, the argument of this chapter focuses on the degree of common beliefs among a variety of geoscientists reflecting on modelling, developers as well as users. Following the theory of epistemic communities I have therefore primarily reflected on common beliefs and assessed how this is borne out by conventions in practice.

To understand how beliefs across the community are related to practices and the material produced, I primarily draw on Nancy Cartwright’s (1999) philosophy of science and academics within the same school of thought (Morgan and Morrison 1999; Morrison 2008), and compare the reliance on modelling tools in CO₂ storage to their wider use in the physical sciences. Morgan and Morrison’s contributions can be summarised as follows: models are multifaceted instruments that partially rely on theories and empirical data to both inform us about the world and represent it through interpreted renderings. This means that data collected for modelling purposes often undergoes considerable interpretation prior to being introduced into a modelling environment. In this environment, according to Cartwright and her followers, model renditions function as part of a comprehensive ‘toolbox’ along with other modes of inquiry.
to generate original insights (Bailer-Jones 2008). Rather than contribute to debates about realist and relativist ontologies directly Cartwright has been keen to elucidate the ways that scientists use a range of tools – models, theories, and instruments for data collection and interpretation – to perform explanatory work as a means of addressing uncertainty (Hoefer 2008: 4).

This perspective has proven helpful in analysing how geoscientists work and in assessing how their practices are fundamental to understanding the characteristics of shared epistemologies and even values. In chapter 5 I continue this analysis of work patterns by comparing perspectives that have arisen around modelling conventions with those from a specific CO₂ storage project, the Sleipner project in the Norwegian North Sea.

**Background expertise and identity**

Geoscientists who work with CO₂ storage have arrived into the field from a number of areas of expertise. In interviews many described that they had worked with this issues predominantly or even exclusively for more than a decade and identify themselves as members of a CO₂ storage community. Others did however prefer to shun this label in favour of a broader description as applied geoscientists. They pointed to their reliance on theoretical concepts, analytical methods, and instruments such as monitoring equipment that have been useful for a range of research questions in the geosciences. To people who had worked in a number of different research areas the description of a single problem affiliation thus did not fit with their personal identification. Just as they had moved in to CCS they would be able to migrate into other areas of research: “We just rebrand ourselves, you know, where the fashion suits” (R12).

Many of those who identified as members of a distinct storage research community explained that they still maintained close links with industrial partners from their past positions. Much of CO₂ storage research is likewise dependent on partnerships with industry to obtain detailed proprietary data on
geological formations that may serve as storage complexes in the future. Industry is therefore often involved alongside research institutions in projects that assess the storage capacity and integrity of potential sites, and it may therefore be misleading to think of CO₂ storage researchers as existing within a bounded social world of similar individuals. Rather, the issue area more accurately includes people from several sectors, a point that will be illustrated more clearly in the following chapter. This should not be a surprising notion to those who have attended social gatherings such as meetings and conferences focused on CO₂ storage or to those who have considered the roots of the research theme. As Winskel (2012) notes, CCS generally has grown out of concerns and practices in established industries rather than risen as an independent niche development in research. Similarly, academics who have worked closely with industry partners on research projects have noted the similarity of the research theme to those that are commonly assessed by the oil and gas industry. When asked whether knowledge from the hydrocarbon industry was directly applicable to CO₂ storage research one interviewee with a background in oil and gas and now an academic working on CO₂ storage, explained: “Yes, definitely. It’s just another fluid” (R12). As an example, it was elaborated that a geologist with a background in oil and gas without hesitation could switch to CO₂ storage. Others shared this view in part because many of the research methods that have been developed for the hydrocarbon industry are used in CO₂ storage. Similarly, much of the existing infrastructure for depleted oil and gas fields, such as pipelines and platforms, may be recycled in CCS. Another geoscientist explained that the close links with industry were partly also a result of government funding priorities, which placed a premium on research activities that appeared to have “real value” to industry. Finally, many of the consultancies that have provided technical expertise to government, at least in the UK, have traditionally focused on hydrocarbon extraction.

Why do researchers focus on CO₂ storage when they come from a background in the oil and gas industry that arguably pays better? Firstly, none of the
Interviewees explained that they perceived their involvement with CO$_2$ storage to primarily be a means to making an income. There was instead a clearly voiced commitment on their part to be involved with practical research on climate change mitigation. It may be that these individuals become involved because they have a prior commitment to this issue, or it may be that they adopt a strongly held identity label after they enter the field. When asked to clarify, interviewees would invariably refer to a deep-seated personal commitment about climate change mitigation and a concern that CCS research should be pursued with this aim in mind:

*We are all scientists here. You've gotta remember that we're not doing CCS for fun, for our jobs. We're doing it because we all believe that it's a significant way of reducing CO$_2$ emissions and to avoid anthropogenic climate change. That’s the whole point for doing CCS. So I think that’s why lots of people [in CO$_2$ storage] are quite pro it, because they see it as one of the major things that we can do to limit our CO$_2$ production. And I think there are some people who perhaps don’t know too much about CCS, who might look at it and think ‘well, you’re only just providing jobs for yourself’. But actually, I would say that most scientists really, maybe I’m naive, but I would think that most scientists are probably doing it because, they are doing it for the reasons about climate change, or wanting to limit the effects of man-made climate change, rather than the fact that they might get a research grant. That’s what I believe anyway.*

*(R4)*

To those on the inside, the research is not pursued for the sake of pure knowledge or curiosity. Rather, they see themselves and their peers as driven by ethical concerns with climate change, a concern that clearly frames how they approach the relevance and importance of their work. Research that is predicated on an ethical, even emotional, attachment in a large group setting evidently raises different questions of identity formation to those involved. A geoscientist who had worked on CO$_2$ storage for over two decades, explained:

*I constantly find that I would like to understand these questions from a kind of neutral position, but I don’t really feel that I’m necessarily capable of taking a neutral viewpoint of it, because of these influences...I feel like I’m not really necessarily an advocate myself, or at least I would like not to be an advocate. But I feel like perhaps I probably am.*
The relation to a large network of researchers, engaged with and committed to their work as a potential solution to a commonly accepted public policy problem, may thus crystallise as inadvertent advocacy. And as this interviewee explains, a reflexive position may bring this to the fore, but will not necessarily mitigate it. While specific background expertise will condition the theories and research methods that geoscientists can bring to bear on evaluations of new research questions, ethical or emotional commitments should likewise be kept in mind as a conditioning factor in evaluations of uncertainties, and particularly in evaluations of possible failures.

**Evaluations of failure**

Most controversial of these, at least outside the world of geoscientists, is probably that of a, potentially large-scale, leakage scenario. Geoscientists themselves are generally not very concerned with this, firstly because they attach a small likelihood to it ever happening and secondly because they view it in relation to existing rates of non-mitigated emissions. In interviews, the likelihood of a large-scale event was thought to be negligible, as the pressure required to bring large volumes to the surface would dissipate before leakage could occur. However, if leakage should occur the main conduits were thought to be manmade wells previously drilled for extraction. Opinions differed on whether it was possible to accurately assess the leakage risk from these and whether they could be managed. However, as the following quotation exemplifies, all interviewees largely agreed that leakage events were likely to be of considerably small scales:

*I have so little concern for leakage insofar as it impacts atmosphere CO2 concentrations, that it’s almost irrelevant to me. In other words people are talking about 0.1% per year or .01%, I think it’s much less than that. I think it’s zero, almost zero.*

(R25)
It is noteworthy that this interviewee went beyond a narrow definition of risk (chance X impact) from storage by defining it not only as an amount of CO₂ injected, but rather how this amount impacts atmospheric concentrations. In other words, the context of existing emissions mattered because it created a more specific problem formulation. However, the same respondent also explained that this belief in low rates of failure stemmed from common technical background knowledge in geoscience research:

*I think there’s a fairly clear, single community that I’m a part of, and that we have a certain understanding of how these subsurface systems work, and we think that you can inject carbon dioxide into them and that it will remain there effectively forever...I don’t know of anyone who doesn’t think that actually.*

(R25)

Despite this strong conviction on the part of geoscientists, or rather because of it, many explained that it was vital to communicate more broadly that the risks of leakage were relatively small. One geoscientist who had worked closely with government and industry on data sharing explained that the evidence basis made available by research projects was absolutely important in this regard, whether or not the scientific establishment believed that CO₂ storage would involve a significant risk of leakage. This person pointed out that it was the reputational value of future research that was at stake more than anything as well as the matter of public accountability:

*I mean if it’s done right, it’s extremely unlikely that the CO₂ will come back up again, and there will be many, many safeguards in place, particularly in the very early demonstration examples it will be almost gold plated, to make sure that absolutely nothing can go wrong. Because it will be so important from a regulatory and public perception point of view that things go right. But, it still is important in subsequent projects that it’s done in the right way, because companies are getting money to do it in the first place.*

(R4)

Other interviewees likewise mentioned that several precautions would likely be taken in early projects to minimise the possibility of leakage events. However,
they also pointed to experience with natural gas storage and oil exploration activities as statistical evidence that some leakage events would very likely occur. As mentioned, such concerns were emphatically associated with wellbores rather than cap rocks and specifically ones that had been improperly plugged after extraction activities had ceased. One geoscientist from industry who had long worked on questions of wellbore integrity in extraction activities as well as storage research, had a very strong view on the potential for such failures and expressed that minor leaks from wells had largely received scant attention until BP’s Macondo well in the Gulf of Mexico leaked in 2010: “There is a very dirty secret in the oil and gas community, which is that there is a lack of well integrity for anywhere between 15 and 30% of the existing wellbores. And it’s a dirty secret, no one talks about” (R24). And yet, after studying technical literature on wells for years the same person concluded that the likelihood of a catastrophic leak (defined as an incident that would lead to a loss of life or a major liability payment) from a well was near zero: “You go through it and then, ahh, then what you find out is there’s leak and leak, and I think the first thing that we have safely concluded is that the chances of a catastrophic leak in a CO₂ injector or through an existing well are about zero” (R24). What again stands out in this determination of risk is the contextual framing, the reference to a catastrophic event rather than just any event. As with the interviewee above who placed leakage in the context of impacts on atmospheric emissions, the focus was again drawn towards the matter of scale, of consequences, rather than simply the principle of a leakage event.

This is not to say that disagreements have been entirely absent among geoscientists as to the feasibility of CO₂ storage, but significant criticisms appear only to have been published by people on the margins of the CO₂ storage research community, i.e. researchers who have prepared assessments mostly in response to the work prepared by the institutions and research groups primarily involved in stating the case for CCS over the years. A salient example has been the highly publicised Economides criticism of fundamental assumptions underlying storage capacity assessments (Ehlig-Economides and
Economides 2010). The Economides paper asserted that most assessments had overestimated storage capacities by 5-20 times actual capacity, by assuming that a degree of pressure diffusion would take place. Such diffusion was thought to only occur in systems with an open geological outcrop into water or onto land, thereby facilitating leakage. The study therefore concluded that a far larger storage area would be needed to meet official prospects for CCS, making it an untenable climate mitigation option. Several research institutes involved with storage capacity research in turn responded by pointing out that the Economides paper itself was based on highly stylised conditions that would not be replicated in real world geological systems and involved obvious calculation mistakes. Fluid-flow did not take place as described in the “highly artificial case” of a ‘closed’ geological system where reservoir pressure continued to rise at an unabated rate, thereby significantly reducing prospects of constant injection. Rather than increase the chance of leakage, CO₂ migrating within geological systems would instead facilitate trapping through dissolution and residual trapping as well as mineralisation (Dooley and Davidson 2010; Oldenburg 2010). These response and others turned the Economides’ argument on its head by pointing out that open systems are ubiquitous, introduce uncertainties that could be managed by controlling injection pressure, and would aid long-term storage security. To substantiate this position, it was pointed out that there are many natural occurrences of CO₂ stored in over-pressurised systems. Other responses pointed to the decades of experience with oil production, which had shown that geological systems did not need to be entirely closed to ensure that leakage did not take place (Cavanagh et al. 2010). Rather than learn from such real world experiences, the Economides paper was thought to have relied inappropriately on analogous data: “E&E’s radical extrapolation of the results from a single reservoir to a commentary on regional storage capacity is virtually meaningless as it supposes that just one (very thin) reservoir would be available for storage in the entire geological column” (Chadwick et al. 2010: 4). This response also criticised the Economides paper for misrepresenting results pertaining to pressure developments at the Sleipner storage demonstration project, and explained that monitoring results instead
showed that any pressure increase was of negligible concern to the overburden integrity, ensuring that Sleipner was an appropriate site for long-term storage.

One of the geoscientists who had crafted a response to the Economides paper explained in an interview that the criticisms levelled at conclusions in CO$_2$ storage research were the result of a failure of relevant material to reach the right ears. The response was therefore published in the public, non-peer-reviewed domain to allow greater transparency. The same approach was taken by other respondents and showed the high degree of agreement among high-profile geoscientists working on CO$_2$ storage research. The rapid and detailed responses underscored the fervour within the community to correct what were perceived to be flawed assumptions and analyses. In the words of the respondent above, the Economides paper was simply an instance of “noise” that was swiftly corrected to ensure that everyone in the extended CCS community eventually had the “right” understanding (R25). Rather than leave a chip in the armour of researchers, the Economides incident has instead highlighted the cohesion and agreement among geoscientists who study CO$_2$ storage and their belief in the appropriateness of existing practices.

Responses to the Economides paper and the tendency to frame the possibility of leakage in the context of existing emissions and climate change are indicative of a particular way of unpacking potentially problematic events. Research has been driven by a sense of ethical urgency and unfolded through specific background training and community-wide assumptions about real world complexity. The rest of the chapter will consider how background knowledge and tools from this training shape such possibilities into tangible potentialities by focusing on simulation modelling in particular.

**Sources of knowledge**

At this point it may be helpful to compare some key variables in data collection and analysis for CO$_2$ storage with those in climate change research. Table 4.1
below uses categories that were derived from material discussed by Edwards (2010) in his history of climate modelling and serves as a starting point for contextualising some key issues involved with CO₂ storage modelling that are discussed in more detail throughout the rest of the chapter.

<table>
<thead>
<tr>
<th>Category</th>
<th>Climate modelling</th>
<th>CO₂ storage modelling</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Origins</strong></td>
<td>Research efforts originally supported by military demands for weather prediction and subsequently developed with support from several military, scientific and civilian interests.</td>
<td>Research arose within traditional themes in the geosciences, but originally proposed as a public policy response to climate change. Developed since with data and technologies in large part from the hydrocarbon industry.</td>
</tr>
<tr>
<td><strong>Data sources</strong></td>
<td>Based on <em>reanalysis</em> of data from a variety of sensors spaced over the entire Earth in several media, which are <em>parameterised</em> to fit the model environment and <em>reproduce</em> historic climate.</td>
<td>Based on various <em>analogous</em> data and limited direct data from storage projects. Thus primarily informed by data <em>extrapolations</em> from multiple research efforts that are sometimes <em>reanalysed</em> for corrections and for use in a range of models.</td>
</tr>
<tr>
<td><strong>Scale</strong></td>
<td><em>Global</em> - climate modelling seeks to accurately reproduce global processes such as feedbacks and formulates policy recommendations on this basis.</td>
<td><em>Regional or smaller</em> - CO₂ storage modelling is derived from regional geological maps and smaller scale data on hydrocarbon formations. Research often cites regional heterogeneity as a limiting factor in generalising claims about storage capacity and migration.</td>
</tr>
</tbody>
</table>
State-of-the-art in climate modelling relies on supercomputers to interpret complex feedbacks in enormous datasets that involve inputs from large teams of modellers.

CO₂ storage modelling is conducted within smaller teams and by individuals who use a range of model environments and variables to depict possible phenomena. A geoscientist’s interpretive skills are crucial in selecting and constraining inputs and in analysing outputs.

In both cases it can be argued that modelling efforts are shaped by available data and that outputs represent original data that reshape prior understandings of these data, rather than stable data that are plugged in to given formulae. Edwards argues for this by emphasizing the crucial roles of data re-analysis, parameterisation and reproductionism. Below, I draw on Cartwright (1999) and others to make similar points about the roles of extrapolations from analogues to bootstrap understandings and support scenarios of CO₂ storage.

Similar to CO₂ storage research, climate change modelling has grown out of a number of different activities that have converged on modelling, but it relies on enormous datasets to explore a range of complex feedbacks in a global setting. While interpretive skill is still required to make sense of such data, the role of individual interpretations in deciding on definitions of geological system types and tracking possible migratory pathways, is strikingly obvious in CO₂ storage research. This is to be expected in a field that has few direct observations to go from at the moment and instead relies heavily on understandings extrapolated from other research areas, a theme discussed in the following section.
Analogues and extrapolations

In the previous chapter it was explained how expert stakeholders refer to what is deemed to be analogous knowledge from oil and gas extraction to substantiate their beliefs about the technical potential to construct large-scale CCS systems. Similarly, geoscientists rely on a range of knowledge analogues e.g. natural gas fields to assess potential long-term trapping mechanisms (Gilfillan et al. 2009), and natural stores of CO$_2$ to consider the impacts of seepage (Caramanna et al. 2011) and construct quantifiable data on leakage risks (Roberts et al. 2011). Interviewees would often refer to such analogues as evidence that injected CO$_2$ could be safely contained and one geoscientist pointed out that the existence of million-year-old natural stores is a good indication that injected volumes can likewise be expected to remain underground under similar conditions. However, others pointed out that it is difficult to rely on such analogues to determine what a leakage event from a storage project would look like. While seepage from natural stores served as good analogues for understanding basic migratory routes – CO$_2$ in such cases has migrated upwards along sandstone beds and seeped to the surface following faults and fractures just as injected volumes may – it was pointed out that specific geological conditions would determine such dynamics and that simple generalisations from similar fluids therefore were problematic. As an example, one interviewee explained that natural seepage usually is found in volcanic regions where CO$_2$ will migrate to the surface aided by deep increases in pressure rather than simply following conduits such as fractures and faults. Deep pressure events were thought to be atypical in sedimentary basins like the North Sea and consequently therefore problematic to rely on when assessing likely leakage patterns.

Such caveats are to be expected when field data is relied on to extrapolate conclusions for system dynamics that have relatively little direct supportive evidence. In contrast, Edwards notes that where laboratory based science is used to substantiate claims, highly controlled experiments are relied on to
create stylised circumstances from simplified scenarios that eliminate real-world complexity. A few selected variables can thereby be examined and compared with controls. This strategy alone is however insufficient to conduct research in the geosciences, because the uncertainty attached to individual variables often is too large and complex to model accurately. In Edwards’s words:

There is no ‘control Earth’ that you can hold constant while twisting the dials on a different, experimental Earth, changing carbon dioxide or aerosols or solar input to find out how they interact, or which one affects the climate most, or how much difference a change in one variable might make.

(Edwards 2010: 140)

Instead, the advent of simulation modelling in long-run climate forecasting enabled the creation of a simulated Earth based on a host of observations. A similar limitation exists for CO₂ storage research. Relatively short-run data from hydrocarbon activities (decadal) is therefore used alongside evidence of long-term dynamics in the subsurface (millennial) to substantiate claims about CO₂ storage developments. And while climate forecasting has made use of a much greater variety of data, it is similarly bound by the limitation of attempting to model a large and very complex system by extrapolating from varied observational accounts. The crucial point that holds for both of these research arenas is that modelling efforts have progressed by compiling and cobbling together observations – often re-analysed to fit the model environment – to create original clues that may clash with prior understandings of the system. The rest of the chapter discusses how this is done for CO₂ storage and what it reveals about epistemology in the geosciences.

**Simulations, representations and scenarios**

CO₂ storage research that uses simulation modelling primarily considers fluid migrations and the onset of different trapping mechanisms. In all such efforts variables are introduced into a model environment that attempts to simulate
some dynamics that are thought to be at work in the real world. Most long-term of these, and arguably therefore most uncertain, is the simulation of geochemical reactions. Geoscientists are here primarily interested in delineating trapping scenarios that can be expected to unfold as CO₂ dissolves in brine and reacts with surrounding minerals to form stable isotopes and retard future migrations towards the surface (Johnson et al. 2003). As with fluid-flow models, extrapolations from analogous field observations are central in the construction of these simulations and data on mineral compositions in reservoirs may be drawn from sites that are thought to have similar enough geologies (Czernichowski-Lauriol et al. 2003; Czernichowski-Lauriol et al. 2006; Gaus et al. 2005). Where access to data from a specific site is not possible, representative samples from elsewhere have been used to support claims about the mineral composition of brine in geological formations. Reliance on representative analysis however extends beyond such samples and into the model framework itself. As with other methods, models have been adapted from the hydrocarbon industry and simulations of interactions between CO₂, rock and brine have been adapted from their original purposes in modelling hydrocarbon and water interactions (Torp and Gale 2004).

Adaptations of representative figures and modelling environments are thus central to original modelling work and Edwards describes a similar process of representative weather models in early constructions of climate data. Early geoscience research in emerging fields is necessarily bound to make use of whatever existing analytical foundations are available and rework available measurements to best represent conditions in a local environment. While Edwards’s point that a ‘control Earth’ is unavailable for geoscience research is of course true, comparisons of a different sort are crafted from re-analysed representative data and borrowed model environments. CCS researchers involved in constructing such models have commented on the dearth of site-specific data as a severe limitation on the generalisability of specific findings from modelling (Portier and Rochelle 2005) partly because an accurate understanding of long-term reactions has been thought to require exact data on
mineralogical compositions (Gaus et al. 2005). And this may be seen as an epistemological tension – geoscientists rely on representations while fully acknowledging that this procedure breaks with the need for exact data.34

In lieu of claims about exact interactions such assessments instead function as approximated analogues for the ways that CO₂ could behave over time. They may be thought of as hybrids between simple analogues from observations of e.g. seepage rates from natural stores and physical models that use site-specific data to simulate outcomes in environments that involve relatively few variables – they are both models and analogues, and not fully either. One element used in such procedures is the representative elementary value (REV) of mineral compositions or temperatures in a model. REVs are by definition general statements and may be created from a range of sources including personal communications, theoretical knowledge in the geosciences, averages from other studies, and assumptions based on other types of observations (Johnson et al. 2003). They are creations crafted from multiple sources and they are crucial steps in understanding possible patterns of reactions. While other variables in the model environment do not have the same origins as REVs they go through a similar revision process over time. One geoscientist explained that initial assumptions about variables would be revised in an iterative relationship between initial assumptions (on input types, ranges, constraints, etc.), outputs, and new observations from monitoring activities. Where observations are limited, assumptions will necessarily have to be made about the permeability of geological strata, pressure, temperature, fault continuity, etc. When more detailed observations become available many scenarios of migration, leakage or geochemical reactions can be ruled out by comparing the fit of a model with these observations. A number of geoscientists who had worked with models to assess migration and trapping mechanisms such as geochemical reactions, thus explained that outputs would be highly contingent and were treated as such by

34 While I explained at the start of this chapter that geoscientists in interviews largely are convinced that injected CO₂ will be stored safely over the long term, the point I wish to draw attention to is thus the crucial reliance on extrapolations from representations in supporting this belief.
those who knew them well. Models were seen as spaces open for the inclusion of variables thought to adequately represent the environment in question and their relations could furthermore be manipulated to generate a large range of dynamics. These dynamics were not thought to necessarily reflect the reactions that would definitively take place in the subsurface over time, but instead a range of likely scenarios. Moreover, by simplifying the model environment to include relatively few parameters to explain very complex dynamics over a long timeframe, modellers construct a representative environment to answer the questions that they have primarily aimed to address.

With many of the variables used in modelling long-run interactions often being poorly constrained, simplifications become extremely important and variable interactions are often bounded precisely to investigate very limited selections of reactions. One modelling exercise for the Sleipner CO\(_2\) storage project considered how brine would be displaced from inside a reservoir as injected CO\(_2\) increased the pressure, and purposely ignored potential effects from chemical reactivity (Chadwick et al. 2009). The limitation to direct pressure effects was justified by explaining that the geochemical effects would likely be relatively small in comparison and therefore negligible (Chadwick et al. 2008). A truthful investigation following this approach is one that delivers a usefully simplified account of potential dynamics rather than one that includes every possible variable. The result is a methodology that tests selected hypotheses under idealised conditions rather than reflecting all known complexities.

However, comparisons with observations from monitoring of injected CO\(_2\) have not removed ambiguity from the comparably simpler modelling of fluid-flow, because multiple variable ranges still need to be assumed. One example is the possibility of arriving at similar results about plume extent where the injection point temperature is changed by 10 degrees Celsius and all other variables are held constant (Lindeberg et al. 2009). This may seem trivial, but the consequences for storage capacity are dramatic. At higher temperatures the CO\(_2\) would exhibit very different physical properties: “In particular its density
would be significantly lower, giving a correspondingly larger *in situ* volume...Because of these uncertainties, a modelling solution that uniquely verifies the injected volume has not been obtained” (Chadwick *et al.* 2006: 5-6). Reliance on modelling in complex environments thus involves an acknowledgement that any understanding is based on simplified dynamics that give rise to contingent claims.

Such contingency in the face of simplification is the case particularly where expert elicitation is at the basis of assessments. As one interviewee who had been involved with an extensive risk assessment project explained, there were obvious limitations to thinking of models as deterministic representations of variable interactions. Reliance on expert judgment and statistical techniques to derive ranges (whether or not these are allowed to include the full numerical range for an answer) effectively means that several strategic choices are made to constrain representations long before numbers enter a model environment. In this case the interviewee explained that such choices included the selection of experts, the decision to model only those events considered most likely, attempts to seek agreement on most likely ranges through group discussion, and the reliance on Bayesian analytical techniques to compute statistically likely ranges. Such work also involves a fair amount of creativity after a numerical range is computed, an example of which is the selection of faults in the bedrock that may serve as conduits for fluid migrating towards the reservoir cap rock:

> *So in the case of the faults I kind of limited it to, either it’s open or closed, and it’s in one of three positions from the injection site. So either it’s...close to the injection site, midway from the injection site towards the edge of the formation, and then far away basically. That was just a way that I came up with trying to represent, you know, uncertainty in faults. But there’s many other, I’m quite sure, better ways of doing it as well.*

(R18)

Such choices imply that models of complex phenomena are not relied on by modellers as tools for deterministic knowledge production. As another fluid-flow modeller explained, it was often obvious which types of reactions could be
expected to arise from a model because the variables and constraints were so consciously selected. Of interest were therefore not the exact numbers that were produced, but rather a general understanding of possible scenarios.

Conscious selection of inputs and acknowledgement of the permitting and restricting involved in constraints and interactions, are therefore central to CO$_2$ storage modelling. One team of fluid-flow modellers explained that they worked from the perspective that while some models were very useful all models were however wrong. Not only did a modeller have to decide how to depict variables and constraints. It was simultaneously incredibly difficult to determine where to draw a line between ‘real’ data and ‘noise’ in observations. One coping strategy is presented in Rempel et al. (2011). This publication noted that that there will normally be a dramatic decrease in a reservoir’s pH immediately following injection and from then on a continual flux in levels should be expected. This disequilibrium in pH levels makes it impossible to choose a constant value for model inputs that are thought to be representative of natural systems. As a consequence, data was chosen at a point of equilibrium and subsequently extrapolated to non-equilibrium conditions for runs in multiple model scenarios. This procedure shows how data on precise conditions can be constructed through experimental design and extrapolations that alter baseline values. The aim in such approaches is to construct data that is thought to be more representative of natural conditions and that may require reanalysis as new observations become available. As Edwards explains in his analysis of climate modelling, reanalysis is a common tool that geoscientists employ when direct observation is limited or impossible (see table 4.1).

When results are not well bounded by data from observations they are primarily driven by theoretical understandings. And such understanding necessarily addresses relatively simple questions because complex interactions require detailed, specific and accurate data from observations. Conventions from theoretical insights are thus key factors in shaping perceptions of likely events. According to one geoscientist, background training would tend to leave
people with deeply rooted assumptions unsupported by empirical evidence. In the following interview excerpt he explained how assumptions about active fault extensions and new fractures that arise from pressurised events such as seismic activity, often are misinformed by narrow applications of theoretical conventions:

*I think most geologists have a background where they spend a lot of time in the field looking at rocks and outcrop, and they also spend a lot of time looking at hard rocks, things with very low porosity. And so almost within their intuition, in both situations when you get a fault developing in those two situations, they’ll tend to be just as you described, a pathway for fluid flow. And so, that almost gets caught up in people’s intuition. Even the most famous, you know, well-respected structural geologists in the world, their intuition will often be that when you get a fault moving, you get a fluid gushing up it. And this is an interesting thing in CO2 storage, every diagram you ever see has a big ‘wow’ with people. Whereas, if you look at it statistically, there is not actually any difference between if you look at a reservoir that’s been actively faulted at the moment versus ones that aren’t. There’s statistically no difference in the hydrocarbon distribution.*

(R22)

This interviewee characterised himself as a multidisciplinary geoscientist who had worked on industrial hydrocarbon projects for decades as well as CO2 demonstrations. He explained that during project meetings there was often such a vehement and statistically unjustified belief that new fractures would function as fluid conduits that opinions to the contrary were received as insults to religious dogma. The recognition that geoscientists may be strongly attached to theoretical conventions when analysing questions under new circumstances, raises challenges to the validity of scientific enquiries that are enveloped by significant uncertainties, and to the possibility that conclusions may arise through inductive questioning. One geoscientist whose research group had assessed findings on long-term geochemical reactions, voiced a scepticism in the ability of long-term geochemical modelling to deliver robust results, saying that such methods did not compare favourably with “geological reality”. Actual reaction patterns were thought to be far more complex over the geological timescales considered than could be accommodated in studies. Similarly,
fundamental questions about the presence of certain physical forces have raised concerns about the correct theoretical framework for long-term geochemical modelling (Gaus et al. 2005) and pointed to the difficulty of delivering consistent results from shorter-term fluid-flow modelling, where several different approaches may be valid (Nordbotten et al. 2012).

This points to the paradox in increased demands for accuracy in fluid-flow and geochemical modelling for CO₂ storage: when modellers incorporate more sources of data and changes to variable interactions in order to increase the predictive power of their models, they encounter a compound problem of uncertainty in the process. Increased model complexity may be framed as a response to the desire for certainty around likely timescales of CO₂ stabilisation in reservoirs. But this should also be viewed within the context of the very long timescale of modelled reactions. Even in short-term fluid-flow modelling for hydrocarbon reservoirs where observational data from monitoring is relatively plentiful there will be an implicit discounting of many possible uncertainties involved. Asked about this range of uncertainties, one hydrocarbon modeller explained, “that if these were properly treated, I think people would probably be, everything would collapse. That the uncertainties would be so great that people would just wonder why they were using the whole process in the first place” (R27).

Such concerns are not only based on the number of possible variables, but also grounded in the accumulation of different types of errors into the modelling environment. The potential for errors in modelling has so far focused on the difficulty of selecting realistic and constant input variables, bounding ranges, and accounting for complex interactions. One interviewee who had used seismic data interpretation to guide models explained that another type of error would be introduced even from fairly constrained direct observations, because these nevertheless would require reinterpreting prior to being introduced into a modelling environment. The direct observation that arises from seismic monitoring measures the velocity of a wave signal, however a modeller will be
interested in what this says about the impedance, or effective resistance, of geological strata. This information is obtained in subsequent processing that compares the signal change to generalised (theoretical) knowledge on signal results and expected impedances. I will not go into more detail on the types of transformations and possible errors involved with this activity as the following chapter deals specifically with seismic monitoring. Suffice it to say that the reliance on theory and the role of generalisation in geoscience modelling shows the impossibility of working within a purely inductive logic. Deduction from theory and an assembly of generalised expressions, are key elements in the creation of inexact scenarios of CO₂ storage dynamics.

This is not to say that the research involved with modelling has lacked in methodological rigour. But it does throw into question how much we should rely on models to substantiate strong conclusions about likely fluid-flow patterns and geochemical reactions. Results from simulation models to date face a range of uncertainties and they are not generalisable. They shed little light on outcomes in specific stabilisation processes over time and consequently on the details of CO₂ trapping.

This contrasts with the generalised perspective on trapping mechanisms communicated in high profile reports such as the IPCC’s *Special Report on CCS* (2005). A widely recognised figure³⁵ (figure 4.1 below) lists the onset and contribution of the four main trapping mechanisms as time-dependent events.

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³⁵ See for example use in technical summary reports (Solomon 2007) and adaptation by CO₂CRC (2013) aimed at a wider public.
Figure 4.1: CO₂ trapping mechanisms over time according to IPCC (2005: 208).

The figure effectively conveys that trapping will take place as a result of processes that are presented as known and distinct events that contribute to securing CO₂ over time. While a later section in the report notes that there are uncertainties associated with data collection and analytical methods in simulation modelling, the figure communicates a generalised understanding of long-term trapping mechanisms that has gained traction in the CCS world. Geoscientists may recognise and appreciate the heuristic quality of this image, but it misrepresents the fundamental importance of site-specific characteristics to an accurate assessment of trapping potential over time. In this respect it invites readers to develop a distorted and simplistic understanding of how CO₂ storage security can and cannot be understood as a general phenomenon for analysis. Another example, this time specifically communicated within the

36 “The principal difficulty is that the complex geological models on which the simulation models are based are subject to considerable uncertainties, resulting both from uncertainties in data interpretation and, in some cases, sparse data sets” (IPCC 2005: 229).
geoscience research field, comes from a *Nature Geoscience* commentary that discussed geochemical modelling results from the Sleipner project. The results referred to showed that carbonate minerals in the cap rock would tend to form a seal over the long-term, providing added confirmation to the stabilisation model proposed in the IPCC’s *Special Report* (Bickle 2009). But the conclusion was presented without any caveats or explanations of model assumptions that were present in the original research. These included the use of a calibrated core sample from another location; the simplification to a few representative mineral groups; the removal of complex feedbacks between decreases in porosity and decreases in permeability; and uncertainty surrounding the exact timeframe and extent of the reactive surface area in the cap rock, which may have been an order of magnitude greater than assumed, thereby critically affecting the rate of interactions (Gaus *et al.* 2005). The effect of such translations to other communities is to portray modelling in a decidedly more deterministic light than is warranted by uncertainties in the original work. Where model results may be interpreted as a possible range of long-term future interactions by those close to the science, simplified descriptions instead relate a story of well-bounded systems where long-term interactions lead to foreseeable results; findings that are utilised to deliver a generalised message about CO₂ storage security rather than critically present reactions. This is perhaps to be expected not only because outsiders may have little understanding or patience for lengthy explanations of limitations and contingencies, but also because effective communication of findings to a wider audience arguably involves a degree of simplification.

As for the ways that models are constructed and relied on by modellers, this resonates with much of Cartwright’s work and the work of academics who follow her: models function as generators of original perspectives from imperfect data. Although painstaking attempts are made in the majority of cases to precisely define and constrain the parameters included, this does not mean that the model framework and the results presented are relied on as finite representations of the question being addressed. Rather, with a multitude of
assumptions that can credibly be defined and constrained in a variety of ways, modelling results for CO₂ storage largely should be seen as guideposts for understanding general potentialities. Geoscientists (who construct and rely on models) interviewed in this study were thought to relate to findings in this manner, which is not to say that most believed it was impossible to realistically portray complex interactions. But there was an acknowledgement on the part of these geoscientists that models functioned as heuristic devices that were at best able to make probabilistically true claims as opposed to deterministically true ones, and that this acknowledgment was important in efforts to better characterise reactions.37 This reflection also emerges from alternative ways of interpreting monitoring results at Sleipner in more recent modelling efforts of the site. Specifically, Hesse and Woods (2010) suggested that the trapping introduced by interspersed shale layers has generally been interpreted as a reduced vertical permeability in models, but this leads to a misrepresentation of the CO₂ plume dispersal and the amount trapped during buoyant rise. Instead they suggest an alternative approach to modelling that represents this effect without distorting other features. Their model thus reconfigures the available data to better represent the reaction of primary interest.

**Bootstrapping**

What does this say about the epistemology of modelling complex phenomena in the geosciences such as CO₂ storage? Cartwright draws on Glymour (1975, 1983) in noting the centrality of ‘bootstrapping’ contingent findings in modern scientific enquiry to support further conclusions: where emerging evidence is

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37 I have not sought to investigate the spread of opinions on whether a model heuristic can be relied on as probabilistically true in all cases for CO₂ storage. Whether we believe that scientific investigation is ever able to produce probabilistic truth claims is fundamentally an ontological question about our capacity to represent reality in all its complexity. As I mention in the introduction to this thesis, I will not deal directly with ontological debates about the realism or relativism of scientific investigation. I am rather more concerned with the role that tools such as simulation modelling have in structuring a culture of investigation. Practically speaking results can be presented as probabilistic claims simply by calculating a sensitivity analysis from the range of parameters identified in the model. Modellers commented in interviews that this had been achieved by relying on Bayesian analysis for data ranges gathered through expert elicitation and wider data collection methods.
contingent on imperfect information and poorly understood interactions composed of several variables, many are simply held constant in a web of assumptions. This supports what geoscientists themselves say about models, and individual components introduced into models, as spaces of highly contingent assumptions that often are constantly open for revision and reanalysis. It also means that there is a tension between geoscientists’ reflexivity and recognition of contingency in analysis and results on the one hand, and the tendency for findings to become increasingly perceived and portrayed as robust and valid in wider stakeholder circles, such as in regulatory documentation, when the same analytical framings and assumptions are consistently applied to address questions.38

One area of research that some geoscientists noted would likely be extremely important in mediating between their approach to uncertainties and requirements for accurate data in CO₂ storage regulations, concerned the development of ‘key performance indicators’ from fluid-flow modelling results. Where poorly limited empirical data is relied on in models as a basis for evaluating later developments at a site, it was seen as crucial to understand exactly what counted as a major irregularity to the regulator. There is an implicit negotiation of relevant knowledge involved in such efforts that challenges a conventional view of science as produced by observations that are self-evidently germane to the case at hand, partly because of limitations from direct data observations, and because of a substantial reliance on analogous information and general theory. Moreover, this need for regulatory knowledge challenges the idea that scientific methodologies are independent from regulatory concerns and problem framings (Jasanoff 1990b). Although many of the geoscientists interviewed saw a tension between their methodologies and the way that regulators tended to understand uncertainties and drew implications for risk assessment, CO₂ storage research has in many respects not been produced separately from such perspectives, but instead in clear response

38 The perceived robustness of results and their presentation within wider stakeholder circles is addressed in chapter 6.
to them. The notion of generating key performance metrics from modelling to use in evaluations of evolving site security is one example of the scientific agenda responding to regulatory concerns, and doing it by bootstrapping findings to provide conclusions that address risk assessment frameworks.\footnote{I discuss regulatory risk assessment for CO$_2$ storage in detail in chapter 6.} Although the geoscientists interviewed were sceptical that it would be possible to provide exactly the quantifiable measures desired by regulators, they were confident that it would be possible to gather qualitative evidence to show that sites were conforming to model predictions. Such statements were supported by confidence in the knowledge underpinning fluid-flow models, and they present a picture of scientists acutely selecting what is and what is not worthwhile in their limited data – that is, bootstrapping as defined by Cartwright and Glymour is inherent to their methodologies.

Support for the production of findings that arise from what are perceived to be well-understood processes to underpin conclusions on site stability, should therefore not be seen as a challenge to beliefs in long-term site instability. In the same sense that very different modelling approaches can generate valid yet unmatched results through bootstrapping, beliefs in evolving site stability and instability may simultaneously be entertained. When used in regulatory circles, science may however be required to deliver conclusions with a greater degree of certainty to sanitise challenging conclusions. This clashes with geoscientists’ understandings of uncertainty, as pointed out by one interviewee who was scathing of regulatory demands for uncertainty quantification and warned about extrapolating from limited findings as a basis for commenting on the security of storage generally. Such conclusions would have to be formulated on the basis of what limited data was available and wider claims should not be forced from them.

Cartwright’s points about continuous bootstrapping in modelling hinges on the use of steps that are justifiable to scientists, and this is key to understanding the
reliance on analogous data in the production of scenarios. Models function as one tool among many others in assessing the potential evolution towards site stability in CO₂ storage. Geoscientists differ in their assessments of the usefulness of results that are highly uncertain and contingent, but they are able to view these within the context of a range of related theory and empirical findings to weigh the usefulness of results. Theirs is a working epistemology that reshapes uncertain factors and contingencies at different levels through constant bootstrapping, where relatively uncertain results may be hinged on other findings to construct more certainty, possibly as temporary placeholders until more accurate direct observations become available. Among geoscientists involved with CO₂ storage these pieces are joined to support a set of general beliefs about storage safety, as outlined at the start of this chapter. That these beliefs involve a reliance on several assumptions does not escape their notice and should not be seen as a weakness, but rather as fundamental to an experimental agenda that rests on a variety of data and analysis.

Discussions between geoscientists around storage capacity and site security are likewise based on an eclectic reliance on ideas and empirical evidence mirrored in publications. In one discussion over pressure developments in a former natural gas extraction reservoir, geoscientists entertained a number of different hypotheses based on a variety of data – pressure diffusivity caused by connections with surrounding reservoirs (site specific geological characteristics); the range of pressure responses to changing gas volumes (theoretical insights from physics); and the possibility that the observed phenomena were caused by nearby well extraction activities (local operational characteristics). The only way that all of these hypotheses could be considered simultaneously was by having people with different backgrounds informed by R&D and operational experience present. This point is also clear from interviews, but perhaps most lucid in the production of data for and from simulation modelling where geoscientists, at least implicitly, need to believe that such efforts contribute to a better understanding of CO₂ storage despite the
presence of enormous uncertainties; that modelling is another voice in the room to draw on in refining hypotheses.

When such knowledge is drawn on to inform operational practices this does however change the perspective from which appropriateness and suitability are judged. One geoscientist involved in several monitoring research programmes over the years voiced doubts about whether it was possible to determine if the monitoring tools used by geoscientists were fit for purpose as they had not been proven extensively through a focus on leakage at CO$_2$ injection sites. Another geoscientist echoed this point and explained that it was a real challenge to prove a negative (no-leak situation) as well as proving that a site was performing as expected over time, when only limited data was available to construct baseline conditions in a fluid-flow model at the initiation of injection. Counterfactual evidence presents an inferential problem common to environmental regulation, and deviations of relevant metrics over time are often chosen as indicative parameters. In this case, it was seen as crucial that operators and regulators agree on accepted deviations over time from alternative site performance metrics, a process that could further be complicated by the turnover of site maintenance and liabilities to governmental authorities after the injection and post-closure periods. The problem was explained as follows by a geoscientist working on wellbore engineering in industry:

> Then there is the slow leak issue, and slow leaks are very common. Now, where I think that the industry is not clear yet, or the community is not clear yet rather, is when is it acceptable? How should we detect it? What type of sensitivity should we look at? And, when should we remediate and how?

(R24)

There was thus acceptance on the part of geoscientists that undesirable outcomes should be expected to occur and that pre-defined quantitative metrics to guide risk acceptance would therefore be desirable. Rather than provide certainty that CO$_2$ storage could be managed as a risk-free operation, these
views show a concern with an acknowledgement of uncertainty in future operations.

Discussion

This acceptance of uncertainty in future operations may be seen as arising partly from an acknowledgement that research on CO₂ storage to a large extent has progressed by way of assembling a range of analogues rather than primarily from direct observations at demonstration sites. It is however also important to recall that many of the geoscientists involved have a past in hydrocarbon extraction and that this leads them to see operational activities as subject to a significant degree of uncertainty. One geoscientist who had taken part in drilling wells for hydrocarbon extraction expressed how reflections on such activities contrasted with understandings that arose from research that was primarily driven by theoretical insights:

I am afraid I do have total respect for the idea that we don't really know what's going on underground. We think we do and I think geologists, one of the failings of geologists as a general rule, is we tend to overestimate how much we know about what's going on underground. And why I say that, is because I've drilled wells before, and you'd be amazed be how much belief people start to have in what they think is going on underground, because after a year or two of planning a well or planning something, you tend to get transfixed on quite specific models. And actually, it is alarming when you actually drill a well, and find out that everything everyone said and everything anyone's told you, is wrong. And so I think we psychologically, I think geologists, tend to be a bit deterministic. They tend to have quite fixed ideas of what the subsurface looks like. They tend to become married to models, and I think they tend to reinforce those models by talking to fellow geologists who create the same ones, or like the same ones.

(R14)

This raises a key point about the epistemology involved with simulation modelling. When Cartwright echoes Glymour and argues that scientists often must progress with research by bootstrapping their results to reach defensible conclusions, she also notes that this speaks volumes about the standardisation of tools at their disposal. In modelling, this surfaces as a tendency to rely on
prescriptive formats that explore interactions under highly stylised conditions – limited variables are available to express a number of complex phenomena such as permeability, which may be expressed in several equally valid ways. As Cartwright notes, this is a practice that fits the circumstances under investigation to the rules of a model environment, rather than the complexity of real-world dynamics fundamentally constructing the workings of a model (1999: 34). While Edwards does not rely on the term bootstrapping, I hold that his account of climate modelling evinces a methodology that has been constructed in much the same way; that is, understanding has progressed by way of reproduction rather than explanation of every possible observed phenomenon. He refers to Eric Winsberg’s view that simulation modelling, which observes and simulates processes at the same time, does not aim to explain weather data. Rather than test theories about weather developments climate models reproduce observed phenomena within the bounds of distinct theoretical assumptions about variable interactions. Edwards refers to modelling efforts that seek to recreate phenomena from a mix of observed empirical and semi-empirical (parameter and analogue) data as ‘reproductionism’:

Reproductionism seeks to simulate a phenomenon, regardless of scale, using whatever combination of theory, data, and ‘semi-empirical’ parameters may be required. It’s a ‘whatever works’ approach – a ‘Pasteur’s Quadrant’ method that balances practical, here-and-now needs for something that can count as data, right up alongside rigorous physics.

(Edwards 2010: 281)

He quotes Sergio Sismondo:

Simulations and their components are evaluated on a variety of fronts, revolving around fidelity to either theory or material; assumptions are evaluated as close enough to the truth, or unimportant enough not to mislead; approximations are judged as not introducing too much error; the computing tools are judged for their transparency; graphics systems and techniques are expected to show salient properties and relationships.
While the terms are different, this gives credence to the philosophical position of bootstrapping as a basis for scientific practice, and accurately summarises how modelling efforts in CO₂ storage have been carried out and used by geoscientists in performing explanatory work. What Edwards points out is that this type of modelling is predicated on best available guesses being based on assumptions of useful material and accurate enough model environments in order to progress arguments a little further, sometimes diverging, opening up spaces for refutation and later possibly converging again on stable argumentative positions. It is a perspective on data assimilation, analysis and the purposes of modelling that finds a basis in the philosophy of Cartwright and her followers – a perspective which is also seen to operate within the CO₂ storage community where modelling generates new ways of grounding a wide variety of observations, in a process that contributes to understandings alongside observation and theory: “Distinguishing evaluation and confirmation from validation or verification helps to clarify the proper role of models in forecasting climatic change: not as absolute truth claims or predictions, but as heuristically valuable simulations or projections” (Edwards 2010: 352). What is seen as heuristically valuable to the scientists involved – following Cartwright – is information that helps furnish models with data and address some fundamental research questions, while acknowledging that assumptions underlie analytical work, rather than perfectly accounting for all possible variables and dynamics. This is not a willy-nilly selection and disregard of data. Rather, it is a clear preference for generating and analysing information that fits given parameters.

In seeking to increase confirmation about CO₂ storage security, model results concerning shorter-term stabilisation of fluid-flow or longer-term geochemical reactions provide a heuristic that is evaluated alongside other data. Validation and verification cannot be expected as there is far too much uncertainty and in any case no control scenario, in the sense described by Edwards, to use as a
baseline. What instead is attempted is an imperfect reconstitution of background conditions to compare with future developments. Data is made to fit in the model environment rather than the model environment perfectly accommodating all observable variables as they occur in the real world. The strength of bootstrapping thus lies in the expedient means of progressing scientific explanation, rather than attempting to explain all observable phenomena. To the extent that we can accept this epistemology, and what it implies about rigour, models on CO₂ storage clearly have much to tell us about possible scenarios in storage security. They are central elements in current evaluations within the CO₂ storage community where they help to support commonly held beliefs that long-term storage can be accomplished in a responsible and safe manner. Alongside background theory and a range of observations they constitute a critical element in this emerging research field.

While the conceptual procedures are clearly similar across climate modelling and CO₂ storage, the former distinguishes itself by its relative maturity and higher degree of critical awareness among a wide field of stakeholders. Compared with climate modelling, CO₂ storage is also analysed in relatively simple frameworks and those who rely on such results to assess the feasibility of storage sites would be remiss not to acknowledge the incipience of the field and the caution that this should imply when prescribing recommendations about storage activities and policies. Insights from the philosophy of science about the role of models can thus be severely limited and generalised if they are not presented along with a critical examination of methodologies and practitioner beliefs. While there are clearly conceptual and analytical similarities between more mature fields such as climate modelling and emerging ones such as CO₂ storage, policy stakeholders who rely on model results to inform their assessment should ask critical questions about the relative development of a research field rather than simply look to social cohesion as a measure of reliability. Yearley (1999) notes that the public at large will tend to evaluate models within a wider social context including trust in policymakers, comparisons with locally derived knowledge, and assessments
of social assumptions underlying results. When models promise to deliver prescriptions for policy or other users, earlier engagement with these stakeholders in the design of socially mediated parameters may therefore increase the robustness and the authority of results. Climate change modelling here offers an important lesson because sceptics continue to voice a great deal of furore around results, arguably because the field is so mature and has developed a high degree of complexity. Attention to analytical conventions and what their usage implies about simplifications and accuracy ought to be a critical ingredient in arenas aiming to draw up policy-related conclusions from CO$_2$ storage research, simply to anticipate points of potential controversy. And the enrolment of potential critics and users at earlier stages of design may increase the acceptance of findings.

Much of this chapter has been devoted to explaining how a variety of data has been used and reconfigured as input variables for models. The following chapter continues the discussion about information adequacy and analytical conventions by considering just one type of empirical data, seismic monitoring observations, and how this has been used to construct understandings of site security in the context of the Sleipner CO$_2$ storage demonstration project.
5. CO$_2$ storage knowledge infrastructures: seismic monitoring in the Sleipner project

This chapter examines the knowledge production infrastructure of the Sleipner CO$_2$ storage demonstration project in the Norwegian North Sea with a particular focus on the role of 3D seismic observations. Material is again drawn from interviews with 27 geoscientists, close readings of several research papers, and conference notes, as well as a few interviews with policy oriented stakeholders. I first provide an overview of the Sleipner project and analyse the publication network using ‘social network analysis’ (SNA). Second, I again draw on literature from the philosophy of science to assess the role of observations in supporting understandings amongst geoscientists.

Introduction

The primary concern of this chapter is to understand how research related to long-term CO$_2$ storage, arising specifically from seismic monitoring, is produced and presented by the geoscience community and what this reveals about shared practices and epistemologies. As in the previous chapter, I draw on interviews with geoscientists that have focused on group identification, the appropriateness of common practices, and beliefs about uncertainty. A mapping and detailed reading of publications focused on the Sleipner CO$_2$ storage demonstration project in Norway provide a bounded context for the interviews and situate perspectives within specific project aims and uncertainties. In this way the discussion draws on specific questions, approaches and learnings from a project.

As discussed in the previous chapter, geoscientists regularly draw on a range of evidence – models, established theory, and empirical findings from site examinations – to understand potential outcomes. Such techniques and the practitioners and papers attached to them will evince different understandings and treatments of uncertainties. They have also been used alongside one another in large research projects to define operational risks and uncertainties.
in specific locations and contexts. They have thus been used both to substantiate a general knowledge base and an understanding of specific uncertainties at projects such as Sleipner. In contrast to a push for entirely novel solutions much of this understanding has been established on the back of existing techniques and technologies such as seismic monitoring in deep geological formations, which is commonly used in the oil and gas industry. This chapter builds on the previous one by analysing how evidence from the interpretation of seismic monitoring has emerged and been used to substantiate beliefs, primarily about storage security, and what this says about the role of uncertainties in geoscientists’ analyses of complex systems. I first consider the history and community structure of geoscientists involved with Sleipner research by looking at co-publication activities. I then critically evaluate key uncertainties attached to instrumentation in CO₂ storage monitoring and ask how scientists perceive these conditioning factors and limitations within their work. Finally, I discuss the robustness of the data generated from one prominent observational technique, the interpretation of seismic monitoring data, and how it should be understood within the epistemological context of Cartwright’s methodological toolbox of science, Glymour’s notion of bootstrapping evidence towards conclusions and wider STS scholarship.

**The Sleipner project**

As explained in the previous chapter, research on CO₂ storage arises from a number of different knowledge production activities. Modelling, whether of fluid-flow or geochemistry, has had a central role in collecting a variety of these insights and has presented them as related elements within the frames of models. These creative assemblages of extrapolations from analogous data have been fundamental to the consolidation of research activities and to the definition of common reference points for conclusions about safe storage both within the specialised community and outwith. This is not to say that such efforts have forever defined and constrained the types of data that may be assimilated into models – for instance, new research projects continue to
involve other monitoring techniques to widen the scope of data. However, certain technologies and techniques have clearly been relatively prominent in efforts to provide evidence of the safety of storage, and data from seismic monitoring has stood out as a particularly important analytical component both within scientific circles and beyond. More specifically, insights from repeat 3-dimensional seismic monitoring, also known as 4D seismic, have formed on the back of large datasets acquired from industrial partners active in hydrocarbon exploration in the North Sea region, to substantiate an argument that CO₂ injection here has been and can continue to be undertaken while ensuring long-term storage and stability within the geology. While I will mention other empirical techniques that have similarly been important in creating a varied evidence basis, an in-depth focus will only be provided for 4D seismic monitoring below.

Probably the most emblematic undertaking in seismic monitoring for CO₂ storage has been the nearly two-decades long collaboration on research linked to the Sleipner injection and storage demonstration project in the Norwegian North Sea. Research efforts linked to this project have given birth to numerous scientific publications and produced one of the first best practice manuals on CO₂ storage, first published in 2003 and extensively updated in 2008, and these outputs are in many circles regarded as strong indications that CO₂ storage should be thought of as a safe undertaking. Evidence from Sleipner stands as a pillar in the argument that CCS should be deployed as a carbon mitigation option and it is therefore pertinent to ask how specific tools and research findings have lent support to this claim. Specifically, what are the findings from seismic monitoring, how are they supported by the technology and the analytical techniques, what are the attendant uncertainties in the instrumentation and interpretation of observations, how do geoscientists make sense of their findings given these uncertainties, and to what extent do these observations give rise to generalisable insights about CO₂ storage?
Research activities linked directly to the Sleipner project began in 1997 with a workshop organised by the IEAGHG to help foster an international research agenda. The main objective of this activity was seen as the formation of a resource on CO₂ injection and storage that future projects worldwide could draw on and plans were therefore expressed to involve as many partners as possible from the worlds of research and industry. As a result of this workshop the long running SACS (Saline Aquifer CO₂ Storage) project was born. Although the involvement of the IEAGHG at this early and formative stage was not reflected in most publications that arose from the project the group was instrumental in organising early work relations, securing financial support from the EU Commission and disseminating results to CO₂ storage research groups worldwide (Berger et al. 2003: i). SACS pioneered CO₂ storage research in a number of areas and was the first project to compress and inject CO₂ underground beneath a storage platform and use field data from a site to evaluate predictions from simulation modelling. The injection location was chosen based on the presence of a large natural gas extraction operation already operating on the same site above the Utsira geological formation. Geological characteristics and the relatively large size of the formation were moreover deemed to be suitable for the eventual stabilisation and the long-term storage of injected CO₂ (Torp and Gale 2004). Seismic imaging could be obtained every few years to track the presence and movement of CO₂ and such observations could be compared with a range of other outputs from the project, which was conceived as a multi-disciplinary undertaking from the start and included research in geology, geochemistry, geophysics and reservoir simulation. This has meant that a variety of kinds of findings linked to the SACS project have been published in years since.

After the SACS project and the follow-up SACS2 project were completed in the early 2000s, research was continued under the CO₂STORE network, which expanded the focus to consider storage capacity and integrity in locations in Denmark and Germany. Individuals and organisations originally involved in SACS have gone on to be central contributors in more expansive research
networks such as CO₂GeoNet founded in 2004, which has formally advised the EU Commission on CO₂ storage and continues to function as an umbrella organisation that hosts an annual meeting for a number of linked EU research projects. Early research at Sleipner and the Best Practice Manual that followed from these labours were thereby central in establishing a network of geoscientists engaged with CO₂ storage research. Reliance on partner organisations in industry to supply proprietary data has also led to close relations with the hydrocarbon extraction industry. And the continued affiliation with the EU Commission, although in later years not in a formal advisory role, has ensured that findings have been communicated to relevant legal and funding bodies.

Sleipner related research has thus had a formative role in the creation of a CO₂ storage research network in Europe. Following a tradition of inquiry by STS scholars into the importance of research demonstrations (Borup et al. 2006; Collins 1988; Shapin and Schaffer 1985), also asked of CCS demonstration specifically (Russell et al. 2012), it is therefore apt to ask what exactly has been learned from these activities, what their legacy has been in later years’ research and how they have helped to shape a particular perspective on the suitability of CO₂ storage. This may firstly be addressed by looking at a set of the core publications produced and their popularity within research circles. A search was therefore conducted through Web of Science in September of 2012 to locate Sleipner related articles published from 1996 onwards. The search was conducted based on fairly strict criteria to limit results as much as possible to publications that primarily focused on Sleipner related data (as explained in annex 3). This yielded 24 results published between 1999 and 2012 and this set of articles is henceforth referred to as the Sleipner ‘core-set’.

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Figure 5.1: Rise in citations of the 24 Sleipner ‘core-set’ publications, 2003-2011.

The figure above depicts the gradual rise in citations of the 24 Sleipner core-set publications from 1 citation in 2003 and rising to 83 citations in 2011. This may partly be seen as evidence of the increasing relevance of CO₂ storage research to CCS activities worldwide. However, it also reflects an intense interest in a few key findings. Topics at the top of the citations list include geochemical modelling (Gaus et al. 2005; Portier and Rochelle 2005), discussions of 3D seismic monitoring (Bickle et al. 2007; Carcione et al. 2006; Chadwick et al. 2006) and fluid-flow modelling (Bickle et al. 2007; Zhou et al. 2010; Chadwick et al. 2009). Out of a total of 289 citations over this period, 187 were of just 3 articles (Gaus et al. 2005; Bickle et al. 2007; Portier and Rochelle 2005) published between 2005 and 2007, all of which discuss modelling results. Where the average number of citations in 2011 was 3.5, these 3 articles were cited 25, 11 and 9 times, respectively. While citations of other articles have steadily increased over this time the prominence of modelling activities is clear.

Citation statistics provide a window into the increasing prominence of research activities linked to Sleipner. We can also analyse such publications with a view towards understanding some of the social dynamics involved in knowledge production, as is attempted in the following section.
Social network analysis of Sleipner publications

Journal publications linked to the Sleipner project have emerged in collaborative efforts between many individuals and organisations some of which have been involved since the early days of the first SACS project. Exactly who has been linked to these efforts and what their relative importance has been can be addressed through a structured analysis of publication trends using social network analysis (SNA). SNA is an analytical method that allows the depiction of actual networks of exchange or imagined connections based on defined criteria of commonality. Data is structured around individual nodes (actors, organisations, events, etc.) that are connected by lines. Connections may take the form of any number of regularly occurring forms of interaction and the choice is driven by the research question and the availability of comprehensive and bounded data. Past research has focused on macro-scale mappings of citation patterns across the sciences (Leydesdorff and Rafols 2012), co-authorship patterns between sectors of society (Leydesdorff and Sun 2009), resource exchange in policy networks (Dowding 1995; Knoke 1994; Marsh and Rhodes 1992), knowledge transfer across governance bodies (Bodin and Crona 2009), implications of different strengths of connections on community organisation and workflow patterns (Granovetter 1973, 1983), and very localised questions about employment searches (Granovetter 1974). The wide number of applications available for SNA and the breadth of possible input data make it a helpful structural tool to examine questions about social organisation that are rooted in structuralist, constructivist or rational choice ontologies alike.

Figure 5.2 below depicts the co-authorship network that arises from the Sleipner core-set search. One publication was added to this list because it appeared in a previous search some months earlier and was judged to be part of the core-set based on its content. Each node in the figure represents an author

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40 Boait et al. 2012.
and each line represents a single instance of co-authorship. Some publications had as few as one author, others as many as nine. In total, the network includes 70 unique authors. Using Excel, publication data was cleaned in a spreadsheet and arranged in a 70x70 matrix that included all authors on both the horizontal and vertical axes. An algorithm was created and used to search for all instances of co-authorship between individuals. The co-authorship matrix was then imported into Ucinet for Windows (Borgatti et al. 2002), formatted for the programme and extracted to an associated programme, Netdraw (Borgatti 2002), to create the figure. The network is based on type 2 relational data - that is, relations are undirected (co-authorship assumes equal involvement by the individuals in the relationship rather than a directed flow of information from one to another) and valued (or weighted, by the number of publications) to graphically represent the degree of individual involvement in the network as a function of authorship activity. Since inclusion in the network has been based on whether or not an individual has published in a relevant field it can be considered to be positional – that is, a specific role configuration is at the basis of selection – rather than reputational – in which membership is based on relevance according to others in the network (Scott 2000: chapter 3). This approach was chosen in order to generate a network that could be justified as being well bounded.41

Node colours have here been selected to reflect institutional affiliations. The size of each node has been selected to reflect its degree centrality in the network. SNA uses several different types of centrality scores and the degree measures the number of lines connecting one node to others. As Scott explains, degree centrality is a measure of the extent to which an agent is “in the thick of things” (Scott 2000: 83).42 Such a measure is useful when looking at co-

41 SNA is not a statistical tool and the method assumes that networks are comprehensive. If a few nodes are missing the results will be distorted, as highly connected individuals may be unaccounted for. This limitation is resolved by choosing networks that are easily bounded and by collecting data on all nodes and their links. For a fuller discussion of the limitations of SNA see Scott (2000).
42 The degree of a node is equal to the total number of other nodes that it is connected to via a line.
authorship networks, as it will show whether highly productive individuals are present. In this case it is clear that they are and that they are primarily associated with one institution, the British Geological Survey (BGS) depicted here in grey. This is also borne out in interviews with geoscientists at the BGS and elsewhere who noted their close connections with industry actors (Statoil, in yellow) and their contributions to CO₂ monitoring research since the 1990s. The main component in the network also shows that individuals at the BGS have been important in linking research across institutions such as Cambridge University in the UK (pink), public research institutes in France including the Office of Geological and Mining Resources (black) and the Institute of Petroleum (light green), as well as the Danish Geological Survey (grass-green). Beyond this main component there are several relatively poorly connected nodes on the periphery. Where they are connected to others in small clusters, these individuals have primarily published with authors based within their own countries (the US, Japan, Italy and Germany) and referred to data from Sleipner as part of a wider argument on simulation modelling for CO₂ storage. In a substantive sense their work has therefore also been peripheral to Sleipner research.
Attempts to collect complete data on informal relations between geoscientists – for example from their participation in various research networks over the years such as CO₂ GeoNet – were abandoned on grounds of poor data availability. Annual membership lists for such networks could not be obtained to track changes in participation despite being on friendly terms with conference and network organisers and the idea was therefore abandoned. As an alternative, the original Sleipner core-set was expanded via a snowballing technique to map a much wider collection of affiliated publications. Selection was based on reference lists from the core-set publications with none selected before 1996 (the year CO₂ injection began at Sleipner). More specifically, the collection of additional publications followed three limiting criteria to select research that

- appeared to arise from knowledge accumulated directly from Sleipner;
- was classified within geology or geophysics; and
- primarily discussed evidence from seismic monitoring.

The collection criteria for the ‘reference additions’ network thus restricted the focus and the resulting institutional representation to reflect a concentration in seismic monitoring. This choice was driven by a curiosity around which institutional connections had been instrumental in carrying out the work for a particularly influential evidence basis at Sleipner, as explained at the start of the chapter. The network below includes 197 individual authors and is based on 115 publications. Nodes have been coloured according to countries to show the international setting in which Sleipner research has been conducted.

![Network Diagram](image)

**Figure 5.3:** Sleipner core-set and reference additions co-authorship network. Nodes represent individual authors. Lines indicate co-authorship. Colours are coded by country affiliations, see legend below.

**Selected countries, figure 5.3**
- Red = Norway
- Dark blue = UK
- Pink = France
- Black = US
- Dark grey = Netherlands
A final snowballing effort was made to exhaust the co-authorship network by consulting publication lists online (SINTEF 2012) and a published thesis (Nicoll 2011) for additional material. Selection was based on the following criteria:

- publications were selected based on keywords in the title ('Sleipner', 'Utsira', or 'SACS') or their appearance on the SACS project list online;
- selection was extended beyond publicly available peer-reviewed journal papers to include technical project reports. This methodological choice captures the importance of a contributor within the network as some results will appear in several publications.

The aim with these criteria was to generate a network that was as exhaustive as possible for Sleipner specific material. The network therefore also goes beyond the previous focus on seismic monitoring and includes more disciplines to consider cross-disciplinary connections. The resulting network below includes 296 individual authors and is based on 215 publications. In figure 5.4 below nodes have been coloured according to affiliation types to show the multi-institutional setting in which Sleipner research has been conducted.
Figure 5.4: Sleipner core-set, reference additions and manually selected additions co-authorship network. Nodes represent individual authors. Lines indicate co-authorship. Colours are coded by affiliation types, see legend below.

<table>
<thead>
<tr>
<th>Selected affiliation types, figure 5.4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Red = Industry</td>
</tr>
<tr>
<td>Blue = University</td>
</tr>
<tr>
<td>Green = Research Institute</td>
</tr>
<tr>
<td>Brown = Regulator</td>
</tr>
</tbody>
</table>

It is clear from this extended network that a structure is maintained as the search is expanded to include more publications – a main component is surrounded by peripheral authors (with relatively low degrees). In order to assess the core relations in knowledge production, figure 5.5 below has been restricted to this main component. Node colours have been coded by institutional affiliations and nodes sizes by degree centrality. It is clear from the figure that the BGS has a relatively high number of productive individuals within a network dominated by various national research institutes from across Europe.
Figure 5.5: Main component of the Sleipner core-set, reference additions and manually selected additions co-authorship network. Nodes represent individual authors and have been sized according to degree centrality. Lines indicate co-authorship. Colours are coded by institutional affiliations, see legend below.

<table>
<thead>
<tr>
<th>Selected institutional affiliations and associated countries, figure 5.5</th>
</tr>
</thead>
<tbody>
<tr>
<td>Yellow = British Geological Survey (BGS)</td>
</tr>
<tr>
<td>Pink = Netherlands Organisation for Applied Scientific Research (TNO)</td>
</tr>
<tr>
<td>Red = Statoil, Norway</td>
</tr>
<tr>
<td>Navy Blue = Foundation for Scientific and Industrial Research (SINTEF), Norway</td>
</tr>
<tr>
<td>Dark Green = Office of Geological and Mining Resources (BRGM), France</td>
</tr>
<tr>
<td>Teal = Danish Geological Survey (GEUS)</td>
</tr>
</tbody>
</table>

Can we graph the relative importance of different kinds of actors and knowledge outputs in connecting researchers across disciplines? This has been attempted with the graph below, which is limited to those authors with at least two research collaborations within the network so as to include only nodes that function as links between others. Relative importance in connecting others across a network can be addressed by considering the *betweenness centrality* of nodes (Freeman 1979). The betweenness of a node measures the extent to which a particular node lies ‘between’ various others. The measure is built
around the idea of ‘local dependency’, which suggests that a node $x$ is dependent upon another node $y$ if the path (shortest number of lines) that connects $x$ to other nodes in the network passes through $y$ (Scott 2000: 87). A node of relatively low degree centrality (in this case, relatively few co-authorship connections) may nevertheless play an important intermediary role by acting as a ‘gatekeeper’ in a knowledge-based network, and occupy a central position that connects several other actors through research collaborations. Oppositely, a node with a high degree centrality may have a low betweenness if for example the majority of its co-authorship relations are with just a few poorly connected individuals in the network. For the Sleipner network, betweenness is a useful measure to look at as it may show whether particular kinds of research outputs have been instrumental in connecting actors across different research foci. This is true in the case of e.g. Doughty who has co-authored only two publications in the extended set – and therefore has a low degree centrality – but is strategically positioned as a connection point between several authors’ work. However, overall for this network, as for most networks in general, different centrality measures are very highly correlated$^{43}$, meaning that authors with a high degree centrality score would also tend to be central in all other respects, including betweenness (for equations of these measures, correlations, and statistical tests see annex 4).

To more clearly graph the betweenness centrality of authors, both the relative network position and node size have been coded according to betweenness in figure 5.6 below. A higher vertical position and greater node size thus indicate a higher betweenness centrality.

$^{43}$ Cronbach’s alpha = 0.95. See explanation and full results in annex 4.
Figure 5.6: Betweenness centrality in the main component of the Sleipner core-set, reference additions and manually selected additions co-authorship network. Nodes represent individual authors. Lines indicate co-authorship. Higher vertical position and greater node size both indicate a higher betweenness centrality.

<table>
<thead>
<tr>
<th>Main research area of high degree actors, figure 5.6</th>
</tr>
</thead>
<tbody>
<tr>
<td>Chadwick: 4D seismic monitoring</td>
</tr>
<tr>
<td>Czernichowski-Lauriol: geochemoical simulation modelling</td>
</tr>
<tr>
<td>Pruess: fluid-flow and geochemoical simulation modelling</td>
</tr>
<tr>
<td>Arts: 4D seismic monitoring</td>
</tr>
<tr>
<td>Doughty: fluid-flow simulation modelling</td>
</tr>
<tr>
<td>Pearce: geochemoical simulation modelling</td>
</tr>
<tr>
<td>Lindeberg: 4D seismic monitoring</td>
</tr>
<tr>
<td>Eiken: 4D seismic monitoring</td>
</tr>
<tr>
<td>Zweiglel: reservoir geology</td>
</tr>
</tbody>
</table>

A regression analysis was considered as a means to measure the strength of ties and the betweenness centrality as a function of primary research foci for all authors in the network. This was not attempted because a main research focus could not be established in many cases. What figure 5.6 above shows qualitatively is that there has been a pronounced tendency for a few actors to strongly connect others’ work in the Sleipner research network. This has led to
a relatively dense network structure, which was calculated by comparing the observed number of mean ties connecting actors with a null hypothesis of just 2 ties. The null hypothesis thus assumes that actors function as mere bridges rather than well connected nodes able to receive and transmit a great deal of information. The observed mean number of ties was 6 and the result was highly statistically significant (see annex 4 for statistics). The tendency towards centralisation around key individuals and a relatively high density was kept in mind for the interviews. Three of the more central actors above were thus interviewed in order to obtain information on regular relations with partners in research and industry as well as their beliefs about commonly held opinions among geoscientists.

**Analysing network tendencies**

The SNA findings on Sleipner research publications indicate a relatively strong tendency to collaborate amongst geoscientists from academia, research institutes and industry, the centrality of a few authors and institutions, and the importance of relatively few types of research to knowledge production efforts. CCS systems are still maturing and a dominant technological approach has not emerged yet. However, knowledge production efforts have centred on a few particular components in order to generate an understanding of storage security or integrity. This has emerged as a rising level of research activity that has connected various actors within specific projects and across common agendas. Following Edwards’s terminology, these activities, supported by the analysis in the previous chapter, may be seen as constituting an emerging CCS ‘knowledge infrastructure’: “Here is a definition: *Knowledge infrastructures comprise robust networks of people, artifacts, and institutions that generate, share, and maintain specific knowledge about the human and natural worlds*” (Edwards 2010: 17). These elements are all captured by regular knowledge sharing at annual meetings such as CO₂GeoNet; the relatively dense formal networking facilitated through co-publication activities across institutions and countries; and a dominant understanding among geoscientists that particular
kinds of theoretical insights, data and analyses (deep geological monitoring, simulation modelling, and reliance on geological theory to interpret data) are needed to make sense of uncertainties pertaining to storage security (as indicated by the centrality of actors in the Sleipner network with these primary research foci as well as the discussion in the previous chapter). Underlying these activities is an implicit, at times explicit in publications and at conferences, assumption that a public policy consensus exists amongst geoscientists: *CCS is a scientifically feasible option for climate change mitigation.*

However, much of this research agenda is still emerging and projects are far from having coalesced on a single monitoring strategy. Increasingly the tone is set by calls for a suite of technologies to monitor the surface as well as the deep subsurface. The appropriate combination of options is thought to differ according to geological characteristics, and additional technologies and methods from other research areas and industrial applications are still being explored and discussed in conferences. In this respect CO₂ monitoring and simulation modelling may mimic efforts in the early days of climate modelling when numerous approaches were being entertained simultaneously in what Edwards calls a ‘coordinated diversity’: “Modelers compared their models and shared them, but no single system design took hold. If anything, GARP [model] planners resisted this” (250). This is not to say that there are no dominant options currently in use. As I show in a later section of this chapter, seismic monitoring has emerged as a key technology partly buoyed by findings that have emerged from the Sleipner research network.

The international setting shows that the science has been produced by, and not merely discussed within, a body of collaborators across Europe and beyond. Theoretically, therefore, biases due to cultural differences would be less severe in this type of collaboration compared with work emerging from a single
country context or within a single institution. On the other hand, because the Sleipner project and data produced from it is the property of Statoil it is appropriate to ask whether researchers believe that they have been able to formulate reliable conclusions based on it. This question can be addressed both with respect to the possibility that data were withheld, as well as whether the practices involved with the production and analysis of data have fit the purposes of CO₂ storage research. Interviewees generally exhibited a high degree of trust in the reliability of the data from Sleipner. Even if data were withheld temporarily, many stated that it would eventually see the light of day. This is not to say that other stakeholders and the public at large necessarily will or should trust that an industry partner will bring to light eventualities that may impact the security and safety of CO₂ storage. The general public may trust scientists to influence companies such as Statoil to release any pertinent information, but as Wynne (1992, 2002) and Yearley (2006) point out, people will evaluate the degree of uncertainties and the decision stakes differently, depending on their background expertise and history of relations with specific organisations and interests. The control, confidentiality and restricted release of knowledge – supposedly generated and analysed in the public’s interest in mitigating climate change – therefore inevitably presents an ethical conundrum to those who possess it and to policymakers who commission and rely on such research, not least because empirical studies have documented the normality of erroneous assessments by expert stakeholders. Slovic (1987) raises the problem of relying on a constrained group of experts’ assessments because biases will lead to unfamiliar phenomena being under-analysed. Perrow’s (1999, 2007) thesis of the normality of accidents affecting complex technological systems relates how experts often draw varied and erroneous conclusions about vulnerability and appropriate risk assessment and risk management procedures. More generally, decision theory research shows that respondents tend to err on the side of positive estimates and systematically

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44 Publications aimed at international journals must of course pass through a peer-review process that subjects the work to the critical perspectives of an international audience, but not all work is aimed at such international outlets.
support their initial positive statements, even in the face of contradictory evidence (Koriat et al. 1980; Fischhoff et al. 1977). Inclinations towards positive errors have likewise been witnessed in forecasts of future energy demand (Smil 2000) and expectations for emerging technologies to meet these, as technological and economic barriers are discounted (Utgikar and Scott 2006). Given this body of work it is therefore appropriate to question the control of the data that supports limited analyses of CO$_2$ storage safety and security and to consider the instrumental value of bringing a wider set of perspectives to bear on technical analyses that imply radical changes to social policy.

With the empirical material I present in this chapter I am however primarily concerned with the somewhat more subtle aspects of epistemic reliability and trust in light of the institutional dependence and workflow density observed in the SNA graphs above. This brings back the primary question pursued in the previous chapter – *how have common practices conditioned research processes, outputs and beliefs?* – only now repositioned within the context of a specific project (Sleipner) and a specific technology (seismic monitoring). And within this context it is impossible to ignore the overwhelming presence of knowledge and practices that have originated in the hydrocarbon extraction industry, and which have decisively influenced geoscientists’ choices of techniques as well as their work patterns:

> It’s a very large machine. It’s a very expensive and well-funded industry. You have to remember this is more than 50 years of oil funding. That’s where all of this has come from, every single bit of it. So, it’s been funded like a defence industry, so it’s incredibly well-funded. And so it’s much more sophisticated than probably most areas of Earth science. And probably most areas of engineering, cause there’s been so much money poured into it for decades. So, that’s why I’m saying that people who work in one part like reservoir modelling, will not necessarily know what the rest of the chain is, that’s led up to that.

(R3)

This quotation from a geoscientist underscores both the role of embedded industrial practices, and by extension the reliance on industry to support
research efforts, as well as the proliferation of differentiated workflows. It is a message that compares well with a history traced by Edwards in his book (A Vast Machine) on the origins of meteorology and climatology to major strategic resource concerns emanating in particular from the US military. Following the landing at Normandy in 1944, which at the time erroneously was thought to have been informed by accurate weather forecasting, the military became increasingly preoccupied with predicting weather patterns. During the Cold War hopes were also pinned on the development of technologies to control the weather for warfare purposes. As a result of these military aims, funding to the US Weather Bureau increased significantly. In 1963, the Limited Test Ban Treaty was signed between the US and the USSR, in part because it was argued that improved monitoring techniques would be able to detect the origin of radioactive fallout from nuclear test bombings. In this way military concerns again spurred the development of monitoring and modelling techniques. Significant resources were thus poured into atmospheric monitoring and simulation modelling, which enabled the development of complex weather and climate models. Reanalysis of the available data to reproduce increasingly accurate climatic patterns, by extension abetted more research on climate change. The reliance on key technologies and techniques from the hydrocarbon industry such as seismic monitoring reflects a similar reliance on knowledge supported by strategic resource management, the “large machine” in this case being the industrial infrastructure that has abetted a CO₂ storage knowledge infrastructure.

Another geoscientist explained that 3D seismic monitoring had proven very cost-effective for industrial purposes and gained the status of a central technique for reservoir characterisation. The increasing reliance on seismic monitoring by industry has enabled geoscientists to obtain older datasets originally shot to search for and define hydrocarbon reserves in smaller blocks of data, and reanalyse them with a view towards characterising site integrity and fluid-flow patterns. The SACS project for example also obtained much of its repeat seismic data from past industrial exploration and characterisation
efforts. Other projects have followed this model and designed research agendas together with industry, or at least included them on advisory boards, thereby gaining direct access to large and proprietary datasets. One geoscientist noted that this was also seen as a beneficial arrangement for industry partners who gained access to a relatively affordable workforce, because research projects often receive a large amount of their funding from the EU Commission. The same person commented that partnerships based on access to proprietary data moreover allowed industry to retain control of information flowing into the public domain. Other interviewees explained that such arrangements lent more credibility to research efforts because non-industrial partners may be perceived by the public to be less partial or biased. On occasion these arrangements have meant that geoscientists have had to take care to only share information publicly that industry partners have consented to. One geoscientist thus recounted that an industrial partner had been very selective in choosing its collaborators and had prohibited certain results from being published, although it was unclear whether the findings were thought to have a significant bearing on understandings of storage security. According to some policy-oriented geoscientists the protection of intellectual property rights has been compounded by political pressures, particularly so in the UK where industrial consortia have competed for government funding. Others explained that seismic data used in research projects often were some years old and arrived pre-processed by industry, which reduced concerns around sensitive data reaching the public domain. While geoscientists may understand and accept such confidentiality and restricted data releases, possibly because of their respect for the commercial interests of industry partners and their own involvement in the hydrocarbon industry, this should lead us to question its appropriateness. In contrast, climate science is generated and widely shared and critiqued with an aim to ensuring that the public at large will trust that scientists represent their interests. While the SNA graphs above clearly show that geoscientists have collaborated to release a wealth of research about CO₂ storage into the public domain, the commercial interest in sanitising and
controlling the underlying data remains a concern that should be discussed in relation to its public interest.

Beyond this dense and international inter-institutional collaboration there has been significant sharing of analytical methods and findings across a range of related projects. This is clear from discussions at conferences such as the annual CO$_2$GeoNet and the biannual GHGT (attendees here include representatives from the wider world of CCS research). The IEAGHG has been particularly involved with the organisation, communication and dissemination of research on CO$_2$ storage over the years. In addition to its involvement with organising the first SACS project, it has coordinated reviews of state-of-the-art in new research and established external peer-review processes. These activities have fostered technical papers written within and across eight distinct research networks, many of which meet annually to share new findings. Some of the people involved with organising these processes and events for several years have been called upon to supply technical advice to national authorities. One person who had been involved with these activities for several years explained that regulators would draw on technical reviews prepared by the IEAGHG and similar organisations rather than constantly commissioning new research. As an example, one research project had created a pool of knowledge around the monitoring of stored CO$_2$ that geoscientists who communicated extensively with regulators could draw upon when they provided advice. The same person explained that the IEAGHG had taken part in UK delegations to international regulatory and policy fora that included the BGS and other stakeholders. This was the case with the renegotiation of the OSPAR Convention and the London Convention, and other country delegations similarly involved individuals from their national research institutes such as the French BRGM. The interviewee went on to name individuals at the BGS who had been notable for their involvement, partly because of their grasp of technical storage issues and partly because they were skilful at working with scientific questions in policy and regulatory contexts. Legislative amendments such as the allowance in the London Convention to store CO$_2$ under the sub seabed, were
thus perceived as an outcome that was highly dependent on the quality of technical advisers and the skill of negotiators: “So my dream team, in any particular negotiation, was with a BGS person and an environmental lawyer” (R37). At the same time the importance of inclusive technical workshops was stressed as a key determinant in securing the inclusion of CCS within the UNFCCC: “The sitting together in the drafting groups, the working groups, that’s the most important aspect. And that’s what’s been lacking a lot in the UNFCCC” (R37). The use of a technical workshop on CO2 storage at the UNFCCC negotiation in Durban in 2011 was thus seen as an important procedural change that helped convey geoscientists’ understandings of storage security and leakage potential to negotiators. In previous rounds of negotiations geoscientists had not been present and representatives from industry such as the CCSA and the GCCSI had therefore been swamped with requests from regulators wanting to understand more about CO2 storage. The interviewee explained that researchers from non-industry bodies had therefore been in high demand at these events so that more objective advice would be presented. In separate interviews with geoscientists at the BGS and the Dutch TNO, people recounted how they had been in scientific advisory roles for many years, both formally as members of technical panels and committees, and informally through irregular email exchanges and phone conversations with policymakers and regulators. The knowledge infrastructure for CO2 storage, and CCS generally, thus expands far beyond co-authorship patterns in individual projects, and is expressed as general advice in publications aimed at governance arenas, facilitated through informal relations with regulators, and structured in formal networks of technical advice.

In the national context of the UK several geoscientists who had provided technical advice to government explained that they believed the government, specifically DECC, was well informed about minute technical issues related to CCS and storage in particular. From the perspectives of these seasoned advisors, government was thought to have an appropriate understanding of scientific and technical matters as a result of close relations with consultancies
and national research institutes. According to one academic with an extensive background in the hydrocarbon industry and who had maintained a consultancy position advising government, this understanding was increasingly clear in detailed conversations with DECC over the years: “So, I think that the guys in the Office of Carbon Capture and Storage are extremely well informed” (R12). This did contrast with the view of one geoscientists who said that knowledge of CO₂ monitoring was extremely limited in DECC – and who explained that reservoir engineering was in contrast very well understood following years of extensive experience with hydrocarbon extraction in UK waters. Similarly, those who claimed to have insight into relations between the UK government and consultancies explained that the organisations with an extensive background in hydrocarbon extraction recently also had migrated towards advisory roles for CCS. The point of appropriate analogous experience thus again arises as it did in the previous chapter with respect to geoscientists’ background training and their move into CO₂ storage research. The wider set of stakeholder involvement with CCS in recent years, also observable in the rising numbers of organisations present at the biannual GHGT conference (Stephens and Liu 2012), shows how the development and expansion of a knowledge infrastructure has partly been abetted by the inclusion of actors primarily focused on hydrocarbon extraction. In this respect relations with industry stakeholders go beyond a one-way reliance on data and technology migration. The perception that understandings from hydrocarbon extraction are directly applicable to CO₂ storage has meant that established stakeholders from industry have been able to enter the CCS arena and arguably expand their advisory relations. Geoscientists in interviews indicate that this trend is justifiable in the face of the kinds of knowledge that are thought to be applicable to CO₂ storage. Furthermore, it has been one element that has been important in establishing a sense among geoscientists, at least in the UK, that government has been suitably briefed and is transparent about their aims and methods in regulating CO₂ storage. This contrasts with CCS stakeholders’ perceptions of UK government policy aims as explained in chapter 3, which have been perceived as unclear and poorly defined.
The rest of this chapter will dwell on technical issues related to one component of the knowledge infrastructure, seismic monitoring. I will explain how geoscientists who have been involved with the Sleipner network understand its role in developing knowledge for CO₂ storage, compare it to its original use in informing hydrocarbon extraction activities and discuss findings in the light of some key uncertainties.

**Seismic surveying of geological formations**

Seismic monitoring techniques are used when characterising subsurface formations and dynamics by measuring the travel time of seismic energy emitted from surface shots, such as explosions or dropped weights, through the subsurface to motion sensors on land (geophones) or in water (hydrophones). In the subsurface, energy travels in wavefronts from the shot point and arrival at the sensors is measured as a ray path of waves emanating from the shot. Seismic energy is either refracted (bent) when it encounters a layer with a different density or reflected at such interfaces. These processes will take place simultaneously during a single survey – when seismic rays strike a density contrast a portion of the energy is refracted into the underlying layer and the remainder is reflected back towards the surface. While similar equipment is used to obtain these two types of observations, seismic reflection uses analytical techniques that enhance the vertically reflected wavefronts and it is generally more applicable to deeper observations, where it shows density contrasts in greater detail. It is also considerably more expensive than refraction surveying.

Both techniques record density contrast observations as functions of ray path travel times, which are subsequently reanalysed to generate density contrast observations as functions of depth. Contrasts are measured as the difference in acoustic impedance from one density to another. In order to arrive at the depth function the data undergoes several stages of processing that eventually result in a 2D or 3D imaged structures (3D seismic surveying is considerably more
resource intensive). The density contrasts are here usually depicted as differently coloured layers of rock throughout a geological section, corresponding to the relative change in acoustic impedance. All of the processing performed up to this point is usually the product of machine automation at which point a geoscientist, usually with some formal training in geology, will begin to interpret the images and define characteristics thought to be germane to particular research questions. In hydrocarbon recovery, analysis may focus on identifying structural traps that could contain oil and gas. In CO₂ injection focus is often directed at identifying structural characteristics that aid or impede fluid migration such as faults and fractures. Because information on densities from seismic ray paths is known relative to previous densities that the wavefront has travelled through, an accurate understanding of structural characteristics is supported by a detailed understanding of a site's stratigraphy - the change in rock layers from e.g. clay to sandstone to granite - as a basis for subdividing seismic sections into sequences that are interpreted as "the seismic expression of genetically related sedimentary sequences" (Kearney et al. 2002: 82). This produces a representation of successive depositional sequences of geologically similar strata that terminate against geologically dissimilar and adjacent strata, observed as distinct seismic sequences.

Assemblage of information on stratigraphical characteristics by design seeks to generalise available information into clusters of data on mineralogical compositions in regional geological formations. Such simplifications are common where detailed information on bulk rock compositions is unavailable from local boreholes. Localised samples can provide site-specific information on likely grain-to-pore compositions of nearby or representative areas. Alternatively, the travel times of seismic wavefronts in a rock volume are compared with generalised data on density contrast changes to assess the likely changes in stratigraphical characteristics throughout the site. A ray's partition into refracted and reflected parts is determined by the contrast in acoustic impedance. If, for example, the geological layer under investigation is largely homogeneous, the ray will continue to travel through it until it encounters a
density contrast of noticeably different acoustic impedance, whereupon part of the ray will be reflected back to the sensors. Figure 5.7 below provides a simplified explanation of this process in a marine environment.

![Diagram of a marine towed streamer seismic survey with the raypaths that result from a single shot by an airgun into a streamer containing 5 hydrophones.](Image)

*Figure 5.7: Simplified marine towed seismic survey with the incident and reflected raypaths that result from a single shot by an airgun into a towed streamer containing 5 hydrophones. $V$ represents the wave velocity, $\rho$ represents the density of the rock. Part of the wave is reflected to the hydrophone at the first reflector (noticeable change in acoustic impedance) and part of it is refracted through the rock. Wikimedia Commons (2012).*

Seismic reflection surveying commonly involves the following steps after data from a survey has been collected: removing noise; determining the wave function and reflectivity function (for all the different geological layers that have been observed in the survey); determining a velocity function to convert the data from time to depth; and finally, determining the acoustic impedances of the formations (Kearey et al. 2002: 49). The resolution of images following these processes and analyses can be severely limited. This partly occurs because the
remaining higher frequencies from a continuously reflected raypath are absorbed with increasing depth, resulting in reduced vertical and horizontal resolutions. The resolution may also be reduced as a result of subsequent processing such as noise filtering. In 3D seismic surveying three sensors record data in all directions, as opposed to 2D seismic surveys that use only vertical sensors as depicted in figure 5.7. A fourth dimension may be added by monitoring changes in a reservoir over time and thereby generate 4D seismic surveys. Several issues may subtly affect the accuracy of measurements over time, including the use of different kinds of geophones spaced at slightly different locations, as well as seasonal changes in the water table. Background noises from nearby activities such as hydrocarbon operations may also be recorded along with the seismic signal. Noise sources and levels may change over time, necessitating different filtering processes. These means of data retrieval and processing illustrate how seismic surveying is a technique used to produce relative information, which is subsequently situated in specific contexts by processing it and comparing it with generalised information from geological charts and borehole data on mineralogical properties. Reliance on prior information, both theoretical and site specific, is thus crucial to the way that geophysical monitoring information is translated into understandings of geological properties.

**Seismic monitoring in hydrocarbon recovery**

4D seismic surveying has been used as a source of data to aid understanding of reservoir fluid-flow and provide information to increase oil and gas recovery in the hydrocarbon industry since the 1990s. The development of relatively expensive permanent 3D seismic survey arrays over hydrocarbon fields has often made economic sense as the cost of conducting a survey (typically less than 1 million pounds) is often much less than the cost of drilling an observation well (sometimes tens of millions of pounds depending on depth) and marginal relative to the cost of developing hydrocarbon extraction installations. Detailed seismic assessments are often seen as a basic
requirement to inform future well drilling in enhanced oil recovery operations (see figure 5.8 below). And spatial measurements of reservoir-scale changes during production were early on described as complementary to traditional engineering pressure and temperature measurements from extraction wells (Lumley and Behrens 1998). Since the late 1990s, 4D seismic has become recognised as a source of valuable data for reservoir monitoring (Gosselin et al. 2003), as a means to help identify additional reserves (Kretz et al. 2004), and provide “invaluable information” to increase recovery rates (Macbeth and Al-Maskeri 2006) from fields worldwide (Behrens et al. 2002). This is achieved by updating simulation models with seismic 4D data and comparing past simulations with observed production data in a ‘history matching’ exercise. Regular surveys thereby show changes in fluids within the reservoir as a function of time and can help provide more accurate information on fundamental properties in comparison with a 3D baseline survey. The reliance on repeated surveys is estimated to have increased oil and gas recovery targets worldwide from around 50% to around 70% of available stores (Calvert 2005), and in individual cases is reported to have increased targets to over 90% (Staples et al. 2002).

An additional advantage of repeated 3D seismic surveys is that regular distortion effects can be noticed in subsequent observations, filtered from past surveys, and used to update simulation models. Such effects are distinct from random noise, which does not exhibit a repeatable pattern and therefore can be more difficult to detect. Kretz et al. (2004) reviewed several papers and identified two general approaches to the use of 4D seismic data in modelling environments. The first approach uses seismic data qualitatively by matching the visual information generated from changes in production with developments in saturation, pressure and temperature. The second approach uses inverted 4D data to quantitatively point out changes in fluid saturation, impedance, and elastic properties between surveys. The inversion process is defined as a minimisation problem that measures the mismatch between observed production data and simulated fluid-flow data. A number of studies
have thus assumed that changes in saturation and pressure can be estimated directly from the 4D data (Tura and Lumley 1999; Meadows 2001) by computationally distinguishing the individual effects of saturation and pressure in the seismic signal (Landrø 2001). However, 4D seismic data does not provide absolute nor distinct values on these properties (Lumley and Behrens 1998) and is therefore generally seen as complementary to traditional pressure and temperature data gathered from wells (Calvert 2005). Such measures are used to anchor seismic estimates for fluid-flow models (Macbeth and Al-Maskeri 2006), to continuously update past interpretations of reservoir simulations for better history matching (Staples et al. 2002), and allow geophysicists to ‘honor’ all available data (Dong and Oliver 2003; R27).

![Diagram](image)

Figure 5.8: 4D seismic data in simulation model workflow. Adapted from explanations in Blonk et al. (2000), Calvert (2005), Gosselin et al. (2003), Kretz et al. (2004), and Lumley and Behrens (1998).

Data comparisons are additionally thought to be useful in reducing distortion effects as even small changes in seismic shot and receiver positions may lead to significant changes in the location of distortions, thereby reducing the ability to successfully match surveys (Calvert 2005). One study has noted that a 10-metre change in shot position more than doubles the non-repeatability of signals and a 20-metre difference triples it (Landrø 1999). Gosselin et al. (2003) summed up
the compound effect of these uncertainties by concluding that acquisition problems, survey distance from the static 3D model, and suboptimal calibration of the inverted seismic signal with well logs all lead to significant limitations in application. According to one interviewee who had been involved with operations at the majority of oil and gas fields in the North Sea over the past twenty years, the benefits of 4D seismic technology were however clearly manifest in its increasing role in assisting with the mapping of remaining concentrations in nearly depleted fields. Such benefits were particularly obvious when compared with the limitations of other modelling approaches informed primarily with data on structural geology. As another geoscientist with a background in industrial applications of seismology explained: “It just was a complete revelation I think, and it’s something in academia we are only just getting to grips with” (R15). The benefits of seismic monitoring are now so widely recognised that it is a standard component in most hydrocarbon recovery practices. Geoscientists who work with subsurface research are thus very familiar with seismic technology and analytical techniques, a familiarity that is also observed among geoscientists working with CO₂ storage where seismic occupies a central role in fluid-flow monitoring, in part because of its application in the Sleipner project.

**Seismic monitoring in the Sleipner project**

Geoscientists explain the migration of seismic monitoring from hydrocarbon recovery activities into CCS as a matter of natural course. According to one geophysicist it was simply another instance of technologies and methods refined by the oil and gas industry unsurprisingly being applied to CO₂ storage: “I can’t really think of anything that’s really specific for CO₂ storage – that couldn’t be used in the hydrocarbon industry, put it that way” (R3). The same interviewee believed that seismic monitoring was a particularly obvious choice of technology in most instances because of its presumed wide range of applications. This interviewee therefore explained that no other remote sensing monitoring technology was thought to provide similarly detailed and reliable
information on the volumetric characteristics of geological structures or the presence of faults and fractures. Similar or better quality information was only thought to be available by drilling far more expensive observation wells and obtaining direct pressure and temperature readings. Another interviewee explained that good quality seismic imaging was moreover fundamental to create baseline data that could inform the reliance on other remote sensing monitoring technologies, such as gravity surveys and electromagnetic monitoring (R5). In the opinions of many of the geophysicists interviewed the importance of seismic monitoring as a component in CO₂ storage could therefore not be overestimated.

Much of this praise for seismic monitoring has come on the back of findings in the Sleipner project. Scientific publications that discuss Sleipner often include repeat 3D seismic images and figures of sections showing layers of CO₂ accumulating within the Utsira Formation. These snapshots of the geology also make their way into presentations both to other geoscientists and to the public at large and serve a very definite purpose in communication. What is so distinctive about the 4D Sleipner images to geoscientists is the clearly observable presence of several small, horizontal discontinuities within the formation, which is largely thought to be composed of sandstone. These discontinuous layers were not present on the original survey before injection took place and only became visible following repeat monitoring of the injected CO₂. These surveys showed that the bulk of the volume was accumulating within nine ‘pockets’, extending laterally from the injection point. It has since been surmised that the lateral discontinuities are highly impermeable shale layers impeding the upward migration of CO₂ within the formation. Besides this surprising heterogeneity what has also caught the attention of geoscientists is that the relatively thin accumulations would not be visible if not for the increased compressibility afforded by the shales. As will be described in more detail below, observations from seismic monitoring are limited by wave frequency limitations. In practice this means that to image any change in acoustic impedance a minimum relative change in density is required. While
the accumulations have been relatively thin, the high compressibility afforded by the shales has increased the density and thereby enhanced the reflectivity of the CO₂, which stands out clearly in subsequent surveys (Arts et al. 2003). The discovery of the previously unknown shale layers led to revisions in understandings of the formation geology as well as beliefs about the likely migration routes and potential leakage scenarios. And geoscientists have pointed to this to argue that large CO₂ injection experiments are necessary before we can ever begin to understand the suitability of long-term injection and storage at presumptive sites. This is in stark contrast to the way that monitoring technologies have traditionally been used in hydrocarbon recovery. As one interviewee explained, the heterogeneity revealed in surveys came as a surprise partly because there was no oil and gas production data to draw on from the same location:

_We are working our way backwards from oil and gas of course, where there you start producing and you learn a lot from your production already, and here we start in some kind of virgin area, inject something, and that works like a contrast fluid, which we use in medicine for example, and it illuminated the area that is affected by the CO₂. And we learn more about that area, but as soon as you go out of that area we are a little bit more in the dark._

(R5)

The 4D seismic surveys thus led geoscientists to revise their earlier assumptions about migration rates and the extent of the CO₂ plume. By 2001, the 4.36 Mt of CO₂ injected were observed within 1.3 km of the injection point, whereas simulation models had projected that a similar amount would rise to the top of the formation where it would be trapped underneath a large continuous cap rock and extend 9 km from here. The shales have therefore been pointed to as an important geological feature in delaying the wider dispersal of injected CO₂, at least for some decades. Besides this direct stratigraphical trapping, the shales are expected to encourage greater dissolution of CO₂ and to promote geochemical reactions, both of which are thought to increase long-term trapping (Chadwick et al. 2003). 3D seismic
images have thereby played centre stage in the development of certainty around the suitability of future injections at Sleipner. They are likewise a centrepiece in the argument that CO₂ injection generally can be conducted safely by pointing out that observations at a specific project support a general understanding of trapping and security. The Sleipner 4D seismic images (see figure 5.9 below) have thereby had an important role in cementing the notion that different trapping mechanisms render the CO₂ immobile over time, as for example explained at length in the IPCC Special Report.

Figure 5.9: Vertical sections (above) and plan view (below) of the CO₂ plume injected at the Sleipner field imaged with repeat 3D seismic surveys, 1994-2008. Vertical sections show individual shale layers illuminated by what are thought to be thin concentrations of CO₂ underneath. Above, a ‘velocity pushdown’ effect – discussed further on in this chapter – noticeably depresses deeper reflections in later surveys. Below, plan view shows the outward growth of the CO₂ plume from the injection point (warmer colours denote higher concentrations). Akhurst (2012).

As Chadwick et al. (2003) and others have reflected, the use of 3D seismic data before and during injection revealed until then unknown features at a relatively well-understood saline reservoir. With the discovery of the shale layers and an adjacent sandbank several assumptions about static and dynamic processes have been revised including the CO₂ migration rate, plume extent, dissolution rate, geochemical reaction patterns, cap rock pressurisation and ultimately the effective trapping potential and uncertainties associated with leakage. One
interviewee explained that the results from seismic monitoring had confirmed to geoscientists that injected CO₂ was behaving “generally as expected” (R4). Such reasoning however appears to miss the crucial importance of revisions following the findings. While in the case of Sleipner the discovery of the shales was thought to confirm the general notion of increased stability over time, it also highlighted that many uncertainties impinge on the possibility of making general statements about future ‘security’ at a site. What the experience with seismic at Sleipner has underlined is that migration and trapping processes are extremely dependent on site-specific empirical findings. Rather than being proof of simple dynamics at work this is a finding that reveals a surprising degree of complexity unaccounted for in previous studies.

In contexts beyond the circle of geoscientists who have been directly involved with CO₂ storage, the geological heterogeneity revealed by the seismic images and the uncertainties associated with the technology are rarely raised. As one geoscientist explained, the images have been a very powerful means of communicating the notion that CO₂ storage is safe:

*I must admit you know, you don’t realise how much you know about these kinds of things. It sort of seeps into you over the years. And so, what 4D seismic is perhaps, is very graphic, in that you can show images that people can more or less understand. Especially, you know, if it’s couched in terms of, I don’t know, ultrasound scans, or something like that, you know. People can say, oh yeah, we can see this change and this is the CO₂ migrating in this direction, whatever. But of course, there aren’t many people, especially among the general public, who are in a position to ask awkward questions about it, you know. So, I think it has it’s advantages as a means of communication perhaps, because as I say, it is quite nice the way it sort of appears.*

(R6)

Geoscientists have been clear in pointing out that site-specific characteristics ultimately determine what types of monitoring technologies are appropriate. However, as the quote above points out, it is difficult for both committed insiders and sceptical outsiders to assess the adequacy of technical options generally and the reliance on seismic monitoring at Sleipner specifically. The
perception of some that seismic monitoring has been a fundamentally successful choice at Sleipner can however be critically assessed by examining the knowledge that has been produced in its wake in light of uncertainties raised in interviews with geoscientists.

**Seismic monitoring and data analysis in context**

From the start seismic monitoring was seen as a complementary technique to assessing developments at Sleipner. It was thought that limited repeat surveys should be conducted every few years and compared with other data from the site. As it turned out, many more surveys became available in subsequent years than were planned for. A geoscientist who was involved with the SACS project and subsequent research efforts explained that the availability of relatively frequent surveys did not follow from a predetermined research agenda, but rather from a series of *ad hoc* analyses. An initial 3D baseline survey was acquired in 1994 two years before injection began. The first follow-up survey was shot in 1999 and paid for by the EU as part of the SACS project. A survey in 2001 was funded under the CO₂STORE project. Then, however, data acquisitions arose as a result of the strategic location of the injection point above a natural gas field: the 2002, 2004 and 2006 surveys were obtained from industry efforts to image the gas field, and as it happened the injected CO₂ was also visible in these images. As an example of close industry cooperation, the 2006 survey received partial funding from the EU Commission to widen the scope and ensure that an extended area was covered to image the CO₂ plume. A similar pattern was repeated in later years: the 2008 survey again imaged the gas field and was later acquired for on-going Sleipner research, and a 2010 survey of the gas field was again extended slightly with research funding to cover more of the plume. The surveys were therefore primarily shot to image the availability of gas, which any knowledge about the CO₂ plume was attendant to. The frequent succession of images suggested that a methodology of 'continuous monitoring' was at the core of the research when in fact the availability of images arose from fortuitous circumstances. Not only have CO₂
storage geoscientists thereby been beholden to technologies and techniques that have originated with hydrocarbon research. They have also, at least at Sleipner, depended directly on the operational practices of industry to obtain relevant data.

The link with industrial practices is also reflected in what additional sources of data have been feasible to obtain for research efforts. In the case of Sleipner this is most clearly visible in the absence of well logs, a matter that was discussed since the start of the SACS project. During an early project meeting in 1998 the need for an observation well was discussed in relation to a range of measurements that would support insights on changes from baseline monitoring. Basic information was thought to include data on CO₂ saturations within the reservoir and an observation well was deemed “...the only way to look at saturation distribution in gas and dissolved water (IEAGHG 1998: 9).” Likewise, an observation well was deemed necessary to monitor pressure and temperature induced effects on the overlying shale cap rock and to monitor the integrity of nearby wellbores. Pressure monitoring and core samples would also be important in validating the physics underlying simulation models and the development of a detailed monitoring baseline was therefore thought to be a critical priority (ibid: 10). In the context of available monitoring options a dedicated observation well was however deemed to be the main cost component requiring an unspecified “large investment” (ibid: 21), and it quickly became clear that drilling a permanent observation well would be prohibitively expensive (Arts et al. 2000), with costs thought to be in the range of 55-70 million NOK, around 6-9 million EUR (Torp and Gale 2004).

In addition to the seismic baseline data and in lieu of an observation well located at the injection site, the reservoir model was informed by well logs from around 300 other wells, 30 of which were located within 20 km of the injection site. These supplied, among other information, reservoir core materials and three pressure readings. Two of these readings were from wells near the Sleipner field and one was from the Brage field located around 250 km to the
North (Berger et al. 2003: 5, 27). The data were largely deemed to be of very high quality and the well logs were thought to vastly exceed requirements to adequately characterise the reservoir and assess its storage capacity. The similarity between porewater samples taken from these wells and those obtained from others at a later stage was furthermore seen as evidence that characteristics likely were uniform throughout the entire formation (Portier and Rochelle 2005). The well logs were also used to model potential migration pathways, which were significantly revised in light of the CO₂ visibly ponding underneath the shale layers.

To update these features in the reservoir model at a later point, logs from nearby wells were again thought to be necessary, this time to assess the continuity of the thin shales throughout the reservoir, which could not be clearly resolved from the baseline 3D seismic data. The injection well itself could in theory be used to collect follow-up information, but its near-horizontal drill direction proved to be a barrier to collecting information throughout the thickness of the reservoir. Lacking well logs from the injection site, the detailed images from repeat time-lapse seismic monitoring were therefore seen to be even more important in accurately characterising the reservoir and fluid flow patterns. Surprisingly small gaseous accumulations (~1m thickness) far below the normal detectability limit of 7m had elicited a clear response due to the increased compressibility afforded by the shales, an effect that was thought to improve detection of future leakage events: “It is exactly this major effect on the time-lapse seismic signal of relatively thin CO₂ accumulations that has built our confidence that any major leakage into the overlying cap rock succession would have been detected. So far, no changes in the overburden have been observed in the Sleipner case” (Berger et al. 2003: 17). Rather than see pressure monitoring as a fundamental monitoring tool as has traditionally been the case in hydrocarbon recovery, the highly permeable sandstone, large pore volume, and the shallow domal structure of the formation were noted as characteristics that ensured a high storage capacity and minimal gas column accumulations throughout. Consequently, only a relatively small pressure build-up would
likely take place and be “far below the estimated limits to avoid mechanical failure or gas penetration through undisturbed cap rock” (Torp and Gale 2004).

Arguments against an observation well that initially pointed to the high cost thereby increasingly emphasised information generated from seismic observations in light of these site characteristics. It is noteworthy that the far higher cost of drilling an observation well, acknowledged during the start of the project, was again recognised as the standard approach in related injection activities including acid gas injection and natural gas storage (Berger et al. 2003: v). However, for the purposes of a research context concerned with the application of existing technologies to develop limited scientific insights, seismic monitoring was hailed as a major success that was both appropriate and sufficient to meet the aims: “The SACS project has identified repeat-seismic surveying as its technically preferred and cost effective monitoring option offshore. However, it is uncertain whether regulatory bodies in Europe, and the general public, would have sufficient confidence in repeat-seismic surveying alone, particularly for the first such storage operations...It is possible that, in early commercial CO₂ storage operations in Europe, a combination of observation and seismic monitoring may be required, although this will undoubtedly raise the cost of monitoring CO₂ storage operations considerably” (Berger et al. 2003: v). While considerable enthusiasm and conviction was expressed over these findings within the research project, it was likewise observed that the principal reliance on repeat seismic monitoring to characterise changes in the reservoir would perhaps not be sufficient to other stakeholders.

Rather than dwell on the limitations of this methodology compared with the use of seismic monitoring in traditional hydrocarbon recovery, the project emphasised that time-lapse seismic monitoring was a key technology to inform simulation modelling: “Thus the SACS local reservoir model has demonstrated that if a well does not exist at, or very close to, the injection site, as at Sleipner, the initial calibration of the physical conditions and reservoir model may not be
ideal. However, if good quality 4D seismic data is available, the reservoir simulation can still be history matched to the seismic interpretation” (Berger et al. 2003: 28). Following the general description in the previous chapter, a range of information could be obtained by extrapolating figures from the seismic observations. The perceived success of seismic monitoring at Sleipner thereby also owed much to *synthetically derived* pressure and temperature estimates at the injection point.

The confidence expressed in the principal reliance on 4D seismic as a tool to detect potential leakage – beyond its utility in informing a reservoir model, mapping general migration patterns, and estimating the storage capacity – is striking. It is surprising because of the focus during geoscientist conferences such as CO₂GeoNet, in research project meetings, and in interviews with monitoring specialists on a comparative monitoring methodology using multiple surface-based technologies to detect background fluxes as a baseline to assess presumed leakage. And it is surprising because the relevant measure of leakage potential at the time was thought to be a variable extrapolated from seismic data – the synthetically derived pressure change in the overburden – rather than direct data on pressure changes or measurements of CO₂ fluxes at the surface. As the 2003 version of the Best Practice Manual explained, the enthusiasm over seismic was partly due to the monitoring context: an early and limited offshore research project rather than a full-scale operation on land near settled communities. With limited data to inform reservoir models, seismic observations were transformed from changes in *acoustic impedance* into *pressure measurements* and *permeabilities throughout the reservoir*. The ability to extrapolate such data points to inform simulation models, underscores the perceived success of repeat seismic monitoring at Sleipner.

When a more extensive version of the Best Practice Manual was published in 2008 a decidedly more cautious tone was used to discuss the 4D seismic surveys. For instance, detection of regular noise patterns was mentioned as an important factor in attempts to match subsequent surveys, which had not been
carried out in the exact location where the baseline was shot. As pointed out
above this imposes a limitation on accuracy that also affects traditional
applications in hydrocarbon extraction activities. However, the absence of logs
from an observation well at the injection site led to additional uncertainties in
determining changes in fluid pressure and saturations. Hence geoscientists now
raised more notes of caution concerning features that could and could not be
detected:

> It is important to stress the need for caution in this type of analysis. Seismic
detectability depends crucially on the nature of the CO₂ accumulation. Small thick accumulations in porous strata would tend to be readily
detectable. Conversely, distributed leakage fluxes through low
permeability strata may be difficult to detect with conventional seismic
techniques. Similarly, leakage along a fault within low permeability rocks
would be difficult to detect. Fluxes of CO₂ such as these may well be
associated with changes in fluid pressure, in which case shear-wave seismic
data is likely to prove useful as a detection tool. Saturations are also
uncertain. The mass estimates given above assume quite high CO₂
saturations within the small accumulations; CO₂ at lower saturations can
give comparable reflectivity, resulting in even lower detection thresholds.

(Chadwick et al. 2008: 198)

The possibility of thin migrations and leakages along faults in low permeability
strata where accumulations would not lead to a strong seismic response, were
raised alongside uncertainties related to saturation levels in the observed CO₂
accumulations. The accuracy of previous insights about storage integrity arising
from seismic monitoring was not doubted, but the precision with which they
could be stated in the absence of well-based monitoring was now more clearly
raised. The report thus pointed to other research projects that had
incorporated well-based monitoring techniques to derive more constrained
results on saturation and other variables (ibid: 220). Some of the sources of
these uncertainties are raised in the following section.
Instrumentation uncertainties

Reliance on seismic monitoring as a key technology to furnish simulation models with a range of data points returns us to the description of data extrapolations in the previous chapter. Indeed, Edwards’s account of how information has been gathered and analysed for inclusion in climate models again serves as a helpful comparison for thinking about the conventions and limitations involved with seismic modelling in the Sleipner research. Following the initiation of global weather and climate prediction efforts in the 1960s, Edwards notes that early global cooperation laid out two separate tasks for numerical modelling: a ‘test-type experiment’ to assess the ability of models to predict past data on global circulation patterns, and a ‘fact-finding type experiment’ that aimed to provide more accurate descriptions of these past patterns using long-term data and thereby show the failings of models when more detail was incorporated. These two general tasks led to various kinds of experimentations with global circulation models to uncover what they could explain about past climatic changes and to assess how the predictive power of models could be improved. Secondly, Edwards notes that these improvements were derived from information retrieved from a host of monitoring equipment such as satellites. Most observations were transformed into other information before they were introduced into the model environment and were therefore not referred to as data but various ‘signals’. These signals were compared with primary data points, such as temperature measurements, before they were allowed to inform the model environment (Edwards 2010: chapter 9). A similarly supportive role has applied to seismic observations at Sleipner, which are a source of relative information on geophysical changes. These ‘signals’ have subsequently been transformed into time dependent migration data and assessments of site security through a comparison with primary information from limited well logs and knowledge of the regional geology. The relative limits on data in CO₂ storage research compared with climate modelling have however also meant that seismic observations have served as primary data points in their own right – interviews with geoscientists and conclusions
presented in the SACS Best Practice Manual show that the visuals produced have been interpreted as convincing information that injection has progressed in a safe manner.

In light of the hydrocarbon industry's traditional reliance on seismic information as a complement to well observations and the limited availability of local pressure and temperature readings at Sleipner, how much can and should we rely on these visuals as primary data to inform our understanding of reservoir developments? As described in the previous chapter, the production of predictions about plume extent from simulation models can be insensitive to large changes in temperature. One implication has been that the saturation level of CO$_2$ in the lateral migrations has not been uniquely determined, and with this the effective storage capacity of the reservoir.$^{45}$ Another issue pointed to by a geoscientist with extensive experience in industrial applications of seismic, was the implication for detailed analyses based on seismic timelapse observations: “You know, the signal that you get is so...You know, I think that just a change in the temperature of the water can account for 50% of the 4D signal difference that you are getting. So you have to be really careful to repeat things” (R22). This interviewee far from dismissed the utility of repeat seismic surveys and had relied on them extensively both in CO$_2$ injection and hydrocarbon extraction activities. The point was simply that a conscientious use of timelapse seismic observations was based on a detailed understanding of site conditions, a point similarly raised by many other interviewees.

Along with a fundamental understanding of reservoir geology, constrained temperature measurements are arguably a cornerstone of such basic site information. As pointed out by a geoscientist who had studied the Sleipner project in detail, later years had seen a convergence on an injection point temperature of 37 degrees Celsius whereas earlier publications had quoted a

$^{45}$ The effective storage capacity is highly dependent on the saturation of CO$_2$ in the reservoir. If saturation levels are relatively high, following a lower reservoir temperature and pressure, the same plume will contain more CO$_2$. A higher temperature and pressure conversely implies lower saturations of CO$_2$. 

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range. The interviewee explained that the convergence on a single number was not properly substantiated by findings from Sleipner and should have been derived from pore-water samples and temperature gauges at the injection point. A review of several publications confirmed that measurements were instead best guesses based on seismic observations, and personal communications from Statoil about the injection point temperature at the start of the operation. Geoscientists who have been involved with Sleipner research since the SACS project have made a similar point. In a publication they have explained that poorly constrained reservoir temperature measurements from the start of injection activities should be considered when interpreting seismic images and that related claims about exact plume observations therefore should be treated with caution. The reliance on low and high temperatures in models could both produce the observed plume reflectivity and velocity pushdown (see figure 5.9 above), and the 37 degrees Celsius number was poorly constrained and based on a single well measurement from a different depth than the injection point. Temperature measurements of saline water produced from the injection site have however supported that number (Boait et al. 2012).

The absence of temperature measurements from a local well – and the uncertainty this implies for the matching of timelapse images as well as the determination of CO₂ saturations and storage capacity – was raised in an interview with a geoscientist who had been involved with the Sleipner project since its inception. These circumstances were not seen as a limitation to obtaining an accurate assessment of storage and leakage potential, but did mean that any determination of leakage based principally on observations from seismic monitoring should not be treated as precise and finite values. Specifically, it was thought that leaks smaller than 2,000 tons would be below the seismic detection limit. Given the seismic imaging available to date and the assumption of a lower than expected threshold of detectability following the discovery of the shale layers, the interviewee was however confident of the site’s containment characteristics: “So I think you can be, you know, adequately satisfied that it’s not getting into the overburden” (R7). The interviewee
likewise explained that the effect of the velocity pushdown, observed as a downwards shift in the centre of the plume observation (see figure 5.9 above), could easily be accounted for by dividing each of the shale layers by the number of overlying reflections (thought to represent the accumulated CO₂) to obtain an accurate estimate of the total injected volume. These calculations have resulted in an average pushdown effect per layer that has subsequently been used to correct storage assessments. This reanalysis brings to mind Edwards’s explanation of how older data were time and again reanalysed to fit changing assumptions in climate models and to account for slight variations in instrumentation. Volumetric reanalysis of layers in the Utsira Formation has likewise been based on the assumption that the velocity pushdown is an artificial element introduced by variations in instrumentation produced by slight variations in survey locations following the 1994 baseline. While the pushdown has largely been interpreted as a technical artefact it is noteworthy that the literature still raises uncertainties about its fundamental properties: “The key issue is whether the observed dimming of lower horizons is an artefact of seismic imaging, or whether it is real and caused by penetration of CO₂ into the impermeable mudstones” (Boait et al. 2012: 18). In other words, the mudstones, or shale layers, may be more permeable than is assumed in most models. This has significant implications for the determination of the average reservoir permeability used to calculate potential migratory pathways in fluid-flow models. Geoscientists who acknowledge this uncertainty nevertheless rely on the simple division explained above to assess injected volumes as this corresponds with the belief that shale layers function as flow barriers. The ‘artefact’ interpretation of the velocity pushdown may therefore be seen as predicated on the need for an account that can be operationalised in model calculations to remove uncertainty attached to the magnitude of injected volumes, rather than an account originating from the basic question of how seismic technology would produce observations under specific conditions. In other words, as an interpretation that supports aims to account more definitively for the injected volume rather than by a strict observational account.
Such strategic choices are common among geoscientists who rely on seismic monitoring to assess reservoir dynamics. Another geoscientist explained that it was common to have a trade-off between high-resolution imaging and random noise. A high-frequency survey might theoretically image resolutions down to 2 metres thickness, but acoustic interference could easily degrade the signal and reduce the final image quality below that of a low-frequency survey shot under similar conditions. Another seismologist explained that seismic monitoring is limited by its ability to observe and locate small changes in acoustic impedance, and sometimes poor at distinguishing between these and larger ones. An example that was also referred to in other interviews was the difficulty of distinguishing between low and high gas volumes: a seismic signal will often not show a detectable difference in gas saturations above 5% of the total volume. This limitation is also discussed in findings based on seismic monitoring at Sleipner, which note that it becomes very difficult to distinguish the degree of CO₂ saturation above just a few per cent (Carcione et al. 2006). Another seismologist explained that the minimum detection was closer to 40%, but it became clear from interviews that this threshold was very dependent on the ability to match surveys with the original baseline. Again, the quality of the baseline measurement and the role of instrumentation in accurately matching developments in subsequent reanalysis, became clear. In addition to the uncertainties that arise when matching subsequent surveys and determining saturations exactly, interviewees also explained that reservoir layers thinner than 50 metres would produce images with ‘averaged’ characteristics for the entire thickness rather than detailed images of small changes in impedance. One geoscientist working with seismic interpretations was explicit in explaining what this entailed for detailed assessments in images: “Seismic data isn’t a true representation of geology. You know, this [section] isn’t a sandstone bed, this is probably a series of sandstone beds. Because your maximum vertical resolution is somewhere in the region of 50 to a 100 metres” (R13).

46 Meaning that a 5% saturation may be observed as a 100% saturation in a given volume.
In interpreting seismic images such limitations all interact to obscure the detail of exact dynamics. Changes may be due to fluid migrations within highly saturated thin layers or low saturation changes across several layers: “So you are gonna see a change in the seismic properties. Just again, it’s very difficult to tell with seismics whether it’s a large change in a thin layer, or a smaller change in a sort of stack of layers. So you wouldn’t be able to discriminate that very well” (R3). This interviewee elaborated on the statement by explaining that in cases where injection commenced into a number of thin layers, all of different rock types, it would be difficult to differentiate each of these using seismic observations and to accurately characterise CO₂ migrations. In other words, it would be difficult to determine if CO₂ was moving in particular directions because of fluid conduits. Another seismologist explained that the difficulty of detecting small changes over a large area meant that seismic monitoring would only be suitable in picking up large migratory changes on the order of several tons of CO₂ (R5). Given the range of alternative monitoring techniques available, this seismologist and others explained that seismic monitoring would only be useful in accurately monitoring changes across individual geological structures with a thickness of at least ten metres, or in some cases much thicker structures depending on the effect of signal attenuation from the top of a reservoir (R13). These compound uncertainties led one geoscientist who had been closely involved with seismic research at Sleipner to state that calculations of CO₂ volumes would easily involve a 30% uncertainty range (R5). That had to be considered along with the significant cost of seismic, which could constitute a severe limitation to the viability of future projects: “And some people are very pushy on the seismics. I think that in the end that could kill maybe even CCS, because it adds to the costs again. I think we need to use it where it is appropriate” (R5). Another geoscientist also referred to these uncertainties as reasons for scepticism about the benefits of seismic generally, particular in the context of the relatively high cost of surveys and the time required to properly analyse images. The practicality of seismic images would also often be
constrained by the type of local geology and the volume of gas in a storage complex (R2).

The presence of several uncertainties affecting interpretations of seismic images – changes in temperature, matching and reanalysing subsequent surveys, image artefacts, determining saturations, and distinguishing thin layer changes – all complicate the accurate assessment of fluid-flow dynamics. Knowledge claims around CO₂ storage are often presented as conclusions that take into account a range of observations and extrapolated knowledge made by people who are finely tuned to the need for clear and simple statements about safety in the face of a variety of uncertainties. This is perhaps true of all scientific observation, but the point does not seem to be very well understood outside the circle of geoscientists working with seismic monitoring. In the arena of climate monitoring and modelling there is arguably a larger audience that understands the basic difference between weather and climate; that one is a long-term trend of the other, which involves a range of uncertainties that impact final assessments. There is arguably also a widespread understanding that an enormous amount of data from many different sources is used to support projections of future climatic changes, however different the conclusions that people draw about the role and significance of uncertainties in final estimates may be.⁴⁷ In the world of CCS, as opposed to CO₂ storage, very often there are no caveats at all attached to remarks that we understand subsurface dynamics at demonstration sites such as Sleipner. Perhaps this is because seismic imaging is seen as such a well-established assessment tool following its importance in the hydrocarbon industry. According to a number of interviewees, reliance on seismic had revolutionised hydrocarbon extraction activities over the past decade because, as one seismologist explained, it is the only monitoring method that can provide an image of a reservoir at depth. But in that arena of application, observations are commonly supported by a range of

⁴⁷ As exemplified by the criticism and distrust of climate scientists following the so-called “Climategate” furore.
other data, most notably from wellbore measurements of temperature and pressure.

In the light of such uncertainties, how do geoscientists interpret the images produced from seismic surveys? A number of studies have considered the ways that geologists rely on prior information from theory and about specific sites to interpret information and sometimes arrive at very conflicting conclusions (Baddeley et al. 2004; Wood and Curtis 2004; Bond et al. 2007; Polson and Curtis 2010). Seismic imaging constitutes only one source of information that will be interpreted in the light of this range of prior experiences and theory. As one seismologist explained, such assessments showed that geological interpretation is “as much an art as it is a science” (R3). Such statements could be seen as a way to deflect attention from the question of how generalisable the skill of seismic interpretation truly is when contrasting conclusions often arise. But it is also acknowledged that a variety of trained geoscientists bring different perspectives to bear on the task of interpretation: structural geologists are often involved at earlier stages of interpretation to trace and define individual strata while geophysicists will normally be involved at a later stage to assess what fluids are present. To control for differences in interpretation, a geoscientist with a wealth of industry experience explained that computers therefore increasingly handle interpretations as fully automated processes. However, it was also clear from other interviews that geoscientists skilled in seismic interpretation continue to have a very significant role. Such procedural steps thereby act as control measures to increase the reliability of final interpretations.

Some interviewees showed how geological strata were demarcated and features, such as potential faults, were delineated by scrupulously peering at images for hours. Experience in what signs to look for as plausible signifiers of key characteristics in the geology as well as a background in theory, were thought to be crucial elements in arriving at accurate interpretations. And rather than function as an alternative to individual interpreters, one
seismologist explained how automated processes were the starting point for geologists. According to this interviewee, computer automations were only a backbone for more accurate predictions informed by geoscientists, partly because they tended to produce artificial geological features in images where limited information was available in between individual reflections. While a seismic interpreter would understand that such blocks of data could simply signify an absence of information, computer automations often accounted for these portions by ascribing geological features to them, such as faults. One geoscientist therefore explained that the combined best guesses by computers and interpreters worked iteratively to produce more reliable data. Asked how a seismic specialist would know when an automation process had produced an unreal account to be corrected, this interviewee explained that the computer would produce a feature that looked “stupid” to the trained eye, or that “doesn’t behave the way it should in the subsurface” (R13). It is clear from this description that geologists do not simply use seismic images as finite analyses of reservoir dynamics delivered as automated ‘prescriptions’. Rather, they draw on a cache of understanding from experiences of how geological structures should appear and about the dynamics that can be expected to take place in the subsurface to interpret observations and derive new knowledge from them. Seismic observations both complement such ‘tacit’ knowledge (Polanyi 1958) and are directly modified by it, in ways that are recognised as legitimate sensemaking processes by geoscientists who share a common background in training and subject matter focus. Although hardly ever discussed explicitly, the iterative relationship between different kinds of knowledge and analyses in such procedural steps, is therefore crucial to understanding how conclusions about CO₂ storage integrity and safety are arrived at and formulated.

**How robust is knowledge from Sleipner?**

Up until the introduction of numerical weather prediction, Edwards explains that weather analysis was an interpretive undertaking that involved a combination of mathematics, graphical techniques and pattern recognition
While general research questions about CO₂ storage, specifically simulation modelling as described in the previous chapter, may resemble activities currently being undertaken for climate modelling, the work is largely progressing at a much more fundamental stage similar to early weather and climate prediction. This is clear from the smaller model environments, the variety of ways in which information about basic variables such as reservoir permeabilities are treated, the uncertainties that scientists themselves acknowledge around fundamental phenomena observed in seismic imaging, and the still limited reliance on integrated monitoring approaches. Geoscientists’ reliance on tacit knowledge to inform interpretations and assess long-term storage safety is central in this undertaking. The logical necessity underlying reasoning attached to the presence of the lateral shales is a highly visual example of this tacit understanding and an indication that CO₂ storage research finds itself at a comparably early stage of development, where knowledge derived from theory plays a fundamental role in interpretation.

This raises the question of how robust we should assume the knowledge from 4D seismic monitoring at Sleipner has been, partly because of the absence of data from local observation wells. Seismic has clearly been a powerful source of illustrative information about layer dynamics and the images have been represented as evidence of CO₂ steadily accumulating in a safe manner. However, significant uncertainties of different kinds are attached to assessments of how it has been accumulating, partly because saturations in these pockets are unknown, and because small and dispersed migrations may be accumulating elsewhere underneath the seismic detection limit (Bickle et al. 2007). As suggested in this chapter, we might therefore reflect on how seismic data has traditionally been relied on in the hydrocarbon industry as for example described by one geoscientist who had used the technology to identify features in exploration activities: “So for example in the oil industry you wouldn’t go running off to the boss and go right, I found a five metre thick reservoir based on a seismic resolution of 50 metres, because you can spend 50 million pounds drilling a well and find out that it’s not there. It's just a, it doesn't actually show
Uncertainties in imaging are acknowledged as a basic limitation in the reliance on seismic to guide expensive activities such as well drilling. On its own, seismic delivers a very general understanding of a reservoir, a characteristic that geophysicists will sometimes refer to as the ‘fuzzy’ property of seismic datasets. Successive images will be encumbered by changes in signal strength for multiple reasons – and with this variability in signal strength follows the inclusion of random noise that is often difficult to filter from images. Where scarce additional observations are available to support seismic data, a fundamental epistemological question therefore arises: how direct do observations need to be to support conclusions? Commenting on the lateral CO₂ migrations at Sleipner, a seismologist and simulation modeller explained how knowledge of the thin and interspersed shale layers was obtained in the light of limitations that affect seismic images:

respondant: [On] the seismic image, you cannot see these very thin layers. And it doesn’t matter whether it is 3D or 4D. You are never going to see these layers, unless they are acting as a barrier to your fluid flow.
interviewer: How do you actually see them then?
respondant: Well, you don’t.
interviewer: So you infer?
respondant: You have to infer them.
interviewer: From?
respondant: Well, from production.

(“Production” here refers to the extraction or injection of fluids in hydrocarbon activities or CO₂ storage. According to this interviewee, eventual validation of geological features is thus often dependent on readings and activities associated with wellbores. However, even in circumstances where such observations are available and used to inform simulation models, determining volumes and migration patterns is not a straightforward activity. Rather, the interviewee above explained that experience with the treatment of uncertainties in industrial modelling of hydrocarbon flows, showed a poor correlation between models and reality: “I really think at the moment the uncertainty analyses done

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in the industry are pretty poor, and if anything they tend to be overoptimistic” (R15). Limitations in uncertainty analyses were perceived to arise from simplified approximations, such as assumptions of linear relations between various variables used to inform history matching in fluid-flow models. However, flow and pressure dynamics are often highly nonlinear\(^{48}\) and the interviewee explained that such conditions mostly do not inform models, with a resulting poor robustness in final simulations. Rather than being well defined by simple approximations, the outcomes of multiple variables interacting at any site were instead described as being highly unpredictable. Under such circumstances the robustness of model simulations was perceived to improve only with the availability of more complete sampling at a specific site – a process that the interviewee explained would yield data that in turn could be incredibly complex to introduce into a model environment. The availability of more direct data observations therefore does not mean that models become better informed or that results become more robust. Separately from data acquisition, complexity arises as a limitation on reproduction in the modelling environment.

Another geoscientists with a history in the hydrocarbon industry explained that the presence of errors and uncertainties in seismic processing and interpretation meant that analysts ideally should rely on multiple scenarios rather than single images:

*Geologists have to eventually say, well let’s drill the well here. We’ve got to present a model. We’ve got to get investment. We’ve got to reassure people. We’ve got to draw a line under this. So we can’t keep saying ‘what if?’*

*So, geologists are too deterministic and what we really need to do in any 3D seismic investigation, is somehow to factor in the errors. And the errors come from all sorts of different sources. From the acquisition, from our processing, so there’s a whole number of points at which errors creep in. You end up with an image, you only get one image, you don’t get ten*

\(^{48}\) A simple example of this that affects CO\(_2\) storage is the sudden increase in the density of CO\(_2\) experienced at around 800 metres’ depth.
images. Actually, what you really needed is ten images, by the way. We could have processed it all these different ways and if you do, then everything has an error bar on it, and each fault has an error bar on it. The position of it has an error bar on it. The image has an error bar on it. How the hell will you get that across [in models], we've never really cracked it. So our images are very deterministic. You get one image, one seismic image.

(R14)

Because of the acknowledged limitations of any single technology and the relatively high costs of timelapse seismic, several experts in hydrocarbon and environmental monitoring techniques explained that it was increasingly seen as necessary that projects should make use of a suite of options simultaneously. One academic with an extensive background in industry noted that,

[j]t's probably the Cinderella area of carbon capture and storage at the moment, is what will be required for long-term cost-effective monitoring that can give you both a comprehensive overview of what might be going on, plus some early indication of what's going wrong. And that is a big research area.

(R12)

This perspective was echoed in large research meetings where geoscientists would discuss the merits of different monitoring technologies. Each option was seen to have its own merits and drawbacks and while early work at demonstration sites such as Sleipner had relied extensively on just a few options, future efforts would likely draw on findings obtained from several technologies to satisfy regulations such as the EU CCS Directive. Another geoscientist was both positive about the utility of images produced at Sleipner and warned that characterisation of the main volume of injected CO$_2$ was not useful in addressing whether leakage had taken place, because of the extrapolations required in converting seismic data to inputs in models and the possibility that some accumulations might be beneath the detectable limit. Concern over the absence of surface monitoring techniques was specifically echoed in research meetings partly because measurements of natural background seepage would be required as a baseline to determine any future
site leakage. Judging from the focus in current research projects this perspective appears to now have become a standard inclusion in monitoring agendas.

Geoscientists reach conclusions with the aid of several assumptions when faced with a multitude of uncertainties in their data, and their analyses thereby introduce additional levels of uncertainty into findings. As explained by some modellers during interviews, one cause of such uncertainty is the assumption that variable interactions are determined by relatively simple (linear) relations, rather than more complex (nonlinear) ones. Another is the erosion of accuracy over multiple data processing steps. A third is the tendency of expert communities to arrive at self-reinforcing conclusions in the face of institutionalised interpretations. Such uncertainties may be acknowledged as inevitable components in comprehensive analyses and may nevertheless be too complex to include in decisions based on analytical tools such as models. An interviewee with years of experience in the hydrocarbon industry explained that this is partly because decisions in industrial settings commonly are carried out by quantitatively risking different scenarios. However, where only little quantifiable data is available, other information often will not take its place in scenarios to influence formal decision making processes. Model environments are thereby inherently and differently biased towards particular kinds of information and treatments of uncertainty. While they may become more robust by matching predictions with production data, this does not necessarily open up discussions about how to account for unknown unknowns, also referred to as ‘ignorance’ in Wynne’s (1992) terminology. As shown in this chapter and in the previous one, the limitations and uncertainties underpinning models are directly acknowledged by geoscientists who will comment on a range of these in interviews. Bootstrapping is central to the methodology of producing bounded conclusions in publications concerned with the geological storage of CO$_2$. The category of ignorance however introduces a challenge to the suitability of this methodology: ignorance is not incorporated as an element in analysis precisely because it does not give rise to data. The robustness of information about
environmental risks acquired in the presence of uncertainty and ignorance is therefore largely a socially mediated decision about adequate evidence, and scientists’ acknowledgement of ignorance in models should give rise to discussions about the appropriate presentation of data and the transparency of methodologies – both among geoscientists and outward to the broader public. Ignorance therefore adds to the need for a discussion of the social parameters involved with scientific inquiry already present where bootstrapping is central to inquiry.

**Discussion**

The Utsira Formation has been seen by geoscientists and portrayed to the wider public as a uniquely ‘successful’ site to test the safe storage of CO₂. Geoscientists readily acknowledge that its unique characteristics – an extensive, highly permeable and shallow sandstone reservoir covered by a thick mudstone cap rock in a relatively well-understood geological region – will help show that CO₂ storage is a safe undertaking. The absence of any clear evidence of leakage could be considered a success for the demonstration impetus because it helps alleviate concerns of immediate or catastrophic leakage scenarios. But it may also be considered a stumbling block in establishing the relevance of potential leakage scenarios, owing to the conscious efforts of geoscientists and industry partners to continue relying on a best-case geological reservoir. While the migration patterns observed in seismic images may be used as evidence of storage integrity, they have not so much confirmed prior model results as they have shown the complexity involved in forecasting the dynamics in large and highly complex systems.

Research to date has concluded that no leakage has been detected with seismic monitoring and that assumptions in fluid-flow models, most notably the average impermeability of the reservoir, have been relatively conservative. It has however not shown that the monitoring methodology has been fit for the purpose of detecting any leakage. Seismic reflectivity has very clearly been a
useful technology for monitoring CO₂ injections at Sleipner and for gaining a better understanding of reservoir characteristics, and it is regularly recommended as a technology to assess injected volumes, storage capacity, track fluid pressure fronts, and identify large potential leaks (Meadows and Cole 2013). While seismic monitoring undoubtedly has revealed surprising findings at Sleipner, the question of what constitutes adequate leakage monitoring is a topic of at least three separate and large EU research projects. Understandings of appropriate monitoring of CO₂ injection and storage are thus evolving to emphasise reliance on a range of technologies and techniques to provide convincing evidence of safe containment. In a very instrumental sense then, assessments and definitions of safety and security are seen to continually involved within the community of specialist geoscientists. The data and opinions presented in this chapter should therefore be seen as a momentary snapshot of perceptions and social reasoning processes about the state of the knowledge, rather than a stable and cemented analysis of how analytical tools are definitively brought to bear on a complex and evolving set of research questions.

History matching at Sleipner using data from 4D seismic monitoring has a legacy in traditional oil and gas activities, but in that setting well logs of local pressure and temperature data are generally seen as fundamental to the accurate interpretation of seismic responses. If the success story of Sleipner is taken for granted at this stage, the lesson to derive from this history is that geoscientists have taken bold steps to operationalise limited data in new ways, albeit with existing techniques, to address novel questions and produce data that has provided additional support for the argument that CO₂ can be stored securely in the long term. A less charitable interpretation is also possible: that pressure readings ought to have been obtained by drilling a local (and expensive) observation well; that geoscientists have played down the challenge to theoretical understandings and community consensus of discovering the shale layers, and what this discovery implies for the uncertainty and stability ascribed to knowledge claims concerning long-term storage; and that
geoscientists should point to the high degree of uncertainty and ignorance (using Wynne’s terminology) that this discovery implies for the state of knowledge about CO$_2$ storage.

As it stands, the more charitable interpretation has largely held its ground in both academic and grey literature published since the SACS project. What is commonly left out of descriptions of this research agenda is the accumulated understanding that geoscientists use to interpret their data – specifically, the received body of knowledge of what it is empirically possible to observe while looking at fluid-flow in geological strata – and how this is transformed in subsequent analytical steps to derive tractable conclusions often aimed at policy stakeholders. Different kinds of knowledge have been interdependent in the formation of interpretations to support claims about storage integrity, and such claims have been presented as tenable positions with reference to the possible realities envisioned by theory, and by a need to provide relatively simple explanations of complex phenomena. This is what Cartwright (1999) refers to as the simplifying act involved in drawing conclusions from e.g. modelling results obtained under highly stylised conditions. Ceteris paribus conditions, at least implicitly, are assumed to guide conclusions towards a least-complicated answer. This would appear to invoke the principle of Occam’s Razor and give way to the simplest possible explanation. However, as Cartwright and others have noted, simplicity in modelling environments arises from the forced economy of our explanatory tools, not from attempts to use the simplest known explanation. Her perspective prompts us to see that scientists rely heavily on theoretical insights to guide their explanatory work, whether or not these theories are truthful in a realist sense: “Inference to the best explanation makes sense when one is inferring to the most probable cause but not when one is inferring to the alleged truth of a fundamental equation” (Hoefer 2008: 4). In other words, inference to the best explanation is not a problem within scientific circles insofar as we acknowledge that it involves simplifying acts that hinder a more truthful understanding of complex phenomena. As I explain in the following chapter, the adoption of such work in other fora where it is used to
support various social commitments does however force us to reflect more 
deeply on how we define knowledge categories and characterise uncertainties, 
and whose interpretations of either we assign credibility to. As I will suggest, 
such questions are not necessarily better or more easily addressed downstream 
from the point of knowledge production by force of regulations.

Cartwright’s work does suggest that scientific practice should adopt a more case 
sensitive and granular reasoning process that would ideally give rise to 
explanations that are less generalised and theoretically encumbered, based 
more specifically on empirically observable particulars, to acknowledge the 
dappled capacities of real world phenomena, and thereby dispense with 
dogmatic formulations. Ulrich Gähde (2008) has suggested that we add to 
Cartwright’s explanation of how formulaic accounts crowd out richer and more 
specific descriptions, by acknowledging that selective cognitive choices filter the 
range of possible explanations even at the earliest stages of data collection and 
preparation for model environments – including the effects of parameterisation 
of data in Edwards’s (2010) account. Gähde’s point thus evokes the centrality of 
tacit knowledge at the earliest stages of research (Polanyi 1958). This is also 
witnessed in CO₂ storage research when geoscientists draw on established 
practices, selected observations and theory, to guide their assessments of 
integrity and safety; assessments that in part are based on some fundamental 
notions of how gases and fluids under pressure in geological strata ought to 
behave. And that involves piecing together what might take place from 
idealised settings as a basis for formulating guesses about likely behaviours in 
non-idealised dynamics. From Cartwright’s perspective, drawing on such 
knowledge is perfectly acceptable and crucial to enquiry, but we risk 
misrepresenting the complexity of phenomena when we present them as 
simplistic and formulaic relations. Rather, scientists should be clearer in 
acknowledging that they are studying the capacities of imperfectly understood 
objects through the lens of idealised experimental work. This framing does not 
subtract from the repeatability of experiments. Instead it suggests that stylised 
experiments are very limited glances at behaviours that, through repeated
experimentation, might exhibit law-like regularities. Cartwright’s perspective thus presents a severe challenge to the repeatability and generalisability of science far beyond the world of CO₂ storage. It is a concern echoed by STS scholars (Shackley and Wynne 1995, 1996; Wynne 1992; Yearley 1999, 2000) who have also stressed the importance of more nuanced analyses that bring a wider set of stakeholder perspectives to bear on early analysis, even where these views may be classified as non-expert (Stirling 2008a; Wynne 2002; Yearley 2006).

Research outputs related to Sleipner have at times been formulated as cautious statements based on limited local observations. But another view has also arisen from this work, namely that we can make fairly strong conclusions about the integrity and safety of storage as a whole on the basis of this limited data. And beyond the world of geoscientists the data has arguably taken on a decidedly more rigid form as evidence in policymaking circles. The concentration of expertise among a fairly specialised and cohesive community of scientists, which has responded as one to criticisms about underlying theoretical assumptions (as portrayed by the Economides incident in the previous chapter) shows the power of a well established community in defining the terms of a technical discussion. It also presents a refinement to the theory of epistemic communities, namely that expert bodies implicitly refer to their concentration of skills and epistemic cohesion to impose a technical perspective in policy discussions, without having to address perspectives on wider social implications attached to a debate. The possible implications of such sanitisation of multivalency (Wynne 1992, 2006), in this case implicitly introduced by the dominance of a cohesive epistemic community, is discussed in the following chapter, which considers how the Sleipner project has been used alongside other information as an evidence basis to support policies for and the regulation of CCS. Drawing on the material presented in this chapter and the previous one, I ask what geoscience has been expected to deliver to public policy and what this says about prevailing understandings of risk, uncertainty and analytical adequacy beyond scientific circles.
6. Governing CO₂ storage in the EU

This chapter draws partly on content discussed in a previous publication as well as subsequent interviews and additional analysis of documentations. It follows the previous chapter by discussing key uncertainties related to CO₂ storage, and analyses how evidence has been presented in the context of EU policy and the CCS Directive. Material is drawn from publications by the EU Commission and the EU Parliament as well as interviews with 17 policy stakeholders and a few geoscientists. The material is situated within STS literature focused on risk, uncertainty and indeterminacy in ‘post-normal science’.

Introduction

Twenty years ago Wynne noted the increasing tendency to invoke preventive measures in environmental risk regulation. Legally formalised as the precautionary principle, the reasoning departs from ‘end-of-pipe’ controls in favour of moving the burden of technical proof further upstream and may be applied across a range of public policy problems (Harremoës et al. 2001). However, specifying that assessments should consider how potential harms are best prevented involves a fundamental shift in the understanding of knowledge production, rather than a minor adjustment. With the locus of evidence directed at assessing potentialities, Wynne argues that questions arising from the social dimensions of application fundamentally reshape a knowledge base: “The different social premises which that shift implies also open up the possible reshaping of the natural categories and classifications on which that scientific knowledge is constructed” (Wynne 1992: 112).

Traditional risk assessment becomes problematic in the context of environmentally ‘extensive’ technology systems because of the complexity of interactions with the natural environment. More often than not parameters of interest to analysts cannot be directly measured and ‘surrogate variables’ are

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therefore used to construct ‘composite variables’. As Edwards has noted, investigations in the geosciences generally fall within this category, and from the previous two chapters it is clear that CO₂ storage research in some cases has relied on very limited observations, often extrapolated from analogous data, to construct representative values for assessing the safety of long-term operations. This workflow pattern is entirely normal within the sciences; as Cartwright and others explain, it is a feature of how investigations commonly arrive at bounded conclusions e.g. through modelling. Wynne notes that it is only when external social commitments, incapable of acknowledging the intrinsic limitations of scientists’ epistemologies, are constructed on top of such findings that problems truly begin to arise: “This only becomes a problem when (as is usual) scientific knowledge is misunderstood and is institutionalized in policy making as if this condition did not pervade all competent scientific knowledge” (ibid: 115).

While it is entirely necessary to examine how models that underlie scientific knowledge can be improved, Wynne notes that this is not enough. Rather, we should invite more scrutiny of how public policy is constructed on top of such knowledge. Jasanoff has similarly stressed the importance of recognising how science is used to buoy public policy and in turn shaped by external agendas (1990b), often in ways that are indicative of culturally distinct regulatory patterns and particular interests that claim authority to specialised knowledge (Jasanoff 1990a; Rothstein et al. 1999), a relationship that is captured in the ‘co-production model’ (Jasanoff 1996, 2004b). It is at the interface of science and public policy that knowledge may sometimes assume an unconditional character by discounting not only uncertainty and ignorance, but what Wynne calls ‘indeterminacy’. He defines this as the meeting point of technical knowledge with the demands of application, where an open-ended question arises “of whether knowledge is adapted to fit the mismatched realities of application situations, or whether those (technical and social) situations are reshaped to ‘validate’ the knowledge” (Wynne 1992: 115). As an example, he points to the 1986 Chernobyl nuclear accident and the subsequent radioactive fallout over Cumbria and North Wales as a case study of contested scientific assessments of
human exposure. Following the review of an older study, government scientists assumed that human contamination would depend on a physical route of exposure to radioactive material in soils. Environmentalist critics however refuted this explanation and new research ultimately showed that sheep grazing on the hillsides constituted another and previously undefined exposure pathway for humans who ate the slaughtered meat. This transformed the understanding of harm from a purely physical phenomenon to a physicochemical phenomenon dependent on socially contingent activities (ibid: 120-2).

Indeterminacy is thus fundamentally different from risk, where odds of specific impacts are known, as well as uncertainty and ignorance, where main impacts may be understood but magnitudes and odds are unclear. Indeterminacy is instead introduced where contingent social behaviour leads to outcomes that cannot be captured in upfront technical analysis. Following the co-production model we may add that topics addressed through scientific research often are guided by demands for comparative and quantitative assessments of risks, and that the knowledge produced is stripped of its conditional characteristics precisely because public policy demands a simple interpretation of evidence to inform its responses.

What the ‘facts’ of interest are is thus a function of where we direct the focus of investigation and for what purpose. Epistemological boundaries are determined in simulation modelling exercises often for the practical reason of limiting results to include those that are thought to be most important in affecting storage dynamics and leakage potentialities. Instrumentation supports the creation of specific and limited kinds of information and defines how data are adapted to the modelling environment. Investigations at Sleipner have thus aimed at clarifying storage integrity and safety principally through perspectives about fluid migration and what this implies for pressure developments, by drawing on a theoretical knowledge base such as reasoning about ‘open’ aquifers to support assessments of storage capacity and the
relative safety of existing wellbores. These analytical choices are supported by reasoning about the most likely types of failure events identified by geoscientists e.g. in group elicitations and emanate from ways of knowing the natural world in the geosciences and for specific contexts. They have however also formed a basis for the regulation of injection and storage activities more widely. And as Wynne points out, such external commitments may in turn transform understandings about the relevant technical and scientific facts. Shifting the burden of proof away from socially contingent end-points towards upstream technology assessments therefore does not necessarily mitigate uncertainty. Rather, it redefines what knowledge we believe is necessary to define potentialities of harms and their likelihood according to normative rules. When such normative rules influence epistemological choices in scientific investigations, the divide between science and policy about the natural environment is effectively dissolved. This process is discussed below in the context of geoscience research, and CCS policy and regulation.

I have attempted to interview many of the relevant stakeholders for this chapter who would be knowledgeable about, or have influenced the terminology and definitions of, risks and uncertainties in CCS policy and regulation. As I mention in the introductory chapter to the thesis, my discussion would likely have benefitted greatly had I been able to interview regulators and/or had access to advisory meetings between regulators and geoscientists. Despite my best attempts to acquire such data this was unfortunately not possible. While I have made efforts to rigorously analyse my material, the account I provide in this chapter may therefore not be a definitive assessment of how risks and uncertainties related to CO₂ storage have been interpreted within the science-policy context of the EU Commission and the development of the CCS Directive.

**CO₂ storage in EU policy**

When geoscientists explain that they have confidence in the safety of CO₂ storage they implicitly define boundaries of analysis and the treatment of
uncertainties for a variety of information. Sometimes these are made to support
generalised assessments by scientists and stakeholders in public policy arenas.
The IEAGHG (and over the last decade also the IEA) has been particularly
present as a supporter of numerous assessments concerning CCS and CO₂
storage since the 1990s. Roadmaps have been drawn up (IEA 2009) to assess
gaps in technical, legal and social understandings to support commercial
development, and more recently the lack of financing for demonstration plants
has been commented on in the context of the long-term, global goals for CCS
(described in chapter 2). CCS has been defined as an important addition to
climate change mitigation efforts and the science of CO₂ storage has been
definitively stamped as adequate for the purposes of proving operational safety:
“To date, four large-scale CCS projects have carried out sufficient monitoring to
provide confidence that injected CO₂ is permanently retained” (IEA 2013: 63;
emphases added). Along with the IPCC the IEA has communicated an
understanding to a wider audience beyond geoscientists that CO₂ storage should
be thought of as a safe undertaking for purposes of regulation, on the basis of
specific scientific assessments.

The reliance on scientific research to construct a case for the promotion of CO₂
storage is also witnessed in institutions that employ regulatory mechanisms
such as the EU Commission. The rhetorical development in documents issued
by the Commission since the publication of the IPCC Special Report in 2005
shows an increasing recognition of CCS as a vital public policy aim, partly based
on research from Sleipner. In 2005 the Commission released the report
Winning the Battle against Global Climate Change (EU COM 2005), which
affirmed that anthropogenic climate change was taking place and called for
measures to operationalise its aim to limit the rise in the global mean surface
temperature to 2 degrees Celsius. CCS was noted as an explicit option alongside
other technologies, all of which required increased investment in energy related
R&D. This investment priority was likewise perceived as a means to ensure that
the EU remained a competitive region for technology development, and
equipment manufacturers for carbon storage were specifically mentioned as
potentially benefitting from R&D investments and funding from an emissions trading scheme (ETS). In 2006 the Commission released the *Report of Working Group 3: Carbon Capture and Geological Storage (CCS)*, which recommended developing a comprehensive regulatory framework for CO₂ storage, partly to ensure that CCS would be eligible to receive ETS funds. All technology components in the CCS chain were described as having largely been proven and these elements could therefore be deployed together now at industrial scale. Sleipner was singled out as an EU sponsored research project, which would require further resources to provide assurances of long-term storage integrity in years to come. This follows the explanation of a geoscientist who was closely involved with informing regulators in the UK and EU for many years, that the seismic images from Sleipner and the many associated publications in peer-reviewed journals had been very influential in convincing policymakers that storage could be undertaken safely. The same document referred to figures on long-term leakage risk from the IPCC *Special Report* as an evidence basis: “The IPCC has estimated that the fraction retained in appropriately selected and managed geological reservoirs is very likely to exceed 99% over 100 years and is likely to exceed 99% over 1000 years” (EU COM 2006: 5).

A policy stakeholder who was involved in the early days of EU regulations described the *Special Report* as the first comprehensive assessment of CCS under working conditions that was instrumental in laying a foundation for the monitoring of injection and storage activities. The preparation of the report also influenced IPCC guidance on accounting standards that had begun in 2004 when the BGS was asked to provide the UK government with a study of how CO₂ storage sites could be monitored. According to this stakeholder and interviewees from the BGS, this request led the BGS to become involved with an *ad hoc* advisory group focused on implementing CCS in the EU ETS. When existing international carbon accounting standards were reviewed it became clear that they would not work for CO₂ storage, and in 2006 a CCS specific chapter was therefore prepared for the IPCC’s updated GHG inventory guidelines by geoscientists (from the BGS and elsewhere).
A policy stakeholder explained that the IEAGHG had attempted to publish a complementary report on monitoring performance by drawing on work in the CO₂GeoNet research network, but that it was not possible to accurately quantify parameters to the levels hoped for. Two geoscientists explained that the chapter instead drew mostly on the IPCC *Special Report*, the EU ETS accounting structure and the work previously prepared for the UK government. According to the policy stakeholder, the resulting methodology was original in advocating a measurement based approach following a site characterisation and modelling:

(a) Properly and thoroughly characterise the geology of the storage site and surrounding strata;
(b) Model the injection of CO₂ into the storage reservoir and the future behaviour of the storage system;
(c) Monitor the storage system;
(d) Use the results of the monitoring to validate and/or update the models of the storage system.

(Holloway *et al.* 2006: 13-4)

This methodology has clear parallels with the workflow in the Sleipner research and the *Best Practice Manual* that arose from the SACS project. Another policy stakeholder explained that the formalisation of this methodology allowed the CO₂ storage community to provide scientifically supported advice to policymakers on storage safety, because the procedures emphasised the acquisition of evidence from monitoring and modelling that showed the injected CO₂ had been securely trapped. Following these guidelines, an operator is held responsible for injected carbon as an accountable stock similar to emissions tied to industrial facilities under the ETS. A leakage event is thus treated similarly to emissions above those allotted under a national carbon budget – operators surrender an equivalent volume – which paved the way for ETS allowances to help finance CO₂ storage. Because such accounting would require precise numbers, the inventory guidelines had to address how uncertainty arising from instrumentation and modelling should be weighed in final estimates:
Uncertainty in the emissions estimates will depend on the precision of the monitoring techniques used to verify and measure any emissions and the modelling used to predict leakage from the storage site. The concept of percentage uncertainties may not be applicable for this sector and therefore confidence intervals and/or probability curves could be given. (Holloway et al. 2006: 18)

The guidelines pointed to the difficulty in relying on deterministic estimates when accounting for leakage and instead recommended that assessments should be based on probabilistic estimates. This recommendation followed from assessments that had given rise to the use of the 99% figure in the Special Report, which referenced results from two long-term simulations based on the Weyburn Midale EOR project (Walton et al. 2005; Zhou et al. 2005). These studies considered possible release scenarios on a 5,000-year timescale using different modelling approaches. Zhou et al. produced a deterministic simulation while Walton et al. produced both a deterministic base case and a probability distribution function (PDF) to support a probabilistic risk assessment (PRA) methodology. Inputs and ranges for both deterministic and PRA modelling environments may be informed by discussion amongst experts and limited to those figures thought to represent the most likely value(s). This follows standard practice in the emerging science of CO$_2$ storage and some of the figures in scientific publications discussed in previous chapters were similarly obtained from industry partners and bounded through expert discussions. Zhou et al. found that the likely cumulative leakage via all existing wells was less than 0.001% at the end of EOR activities, and while Walton et al.’s deterministic simulation found a higher amount of leakage over the same time horizon, the PRA results appear to have directly substantiated the conclusions in the Special Report.$^{50}$ That is, CO$_2$ retained in the geosphere was very likely (95% confidence interval in IPCC terminology) to exceed 99% over 100 years and likely (67% confidence interval in IPCC terminology) to exceed 99% over 1,000 years

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$^{50}$ I acknowledge that it is very likely these publications were discussed among geoscientists who may have brought other evidence to bear on the question before deciding on the wording. However, I only have limited accounts of decision making during IPCC discussions available from my interview data.
(Walton et al. 2005: 697). Similar to the description provided in chapter 4, these simulations were based on limited data and a number of assumptions for long-term interactions and Walton et al.’s use of PDFs underscores the intention to inform a range of probabilistic leakage scenarios. Following Cartwright such results should be viewed as momentary glimpses into dynamics based on limited data analysed under highly stylised conditions.

Nevertheless, the 99% figure in particular appears to have created a basis for a generalised quantitative statement about CO₂ storage security. A coordinating lead author to the Special Report described its inclusion as the outcome of an expert group assessment: “We felt that we had to say something about leakage and fraction retained” (R43). Another lead author explained that Canadian representatives had been influential in ensuring that the figure was used in the report. Since then, its role had changed in CCS regulation debates and it was thought that the figure had assumed the status of a criterion for storage sites rather than an “impression that it is feasible to have an effective risk management of CO₂ storage sites” (R35). This perspective finds support in a later section to the Special Report, which expressly explained that “[t]here is insufficient experience in monitoring CCS projects to allow conclusions to be drawn on physical leakage rates” (IPCC 2005: 371). In line with this caveat, an industry stakeholder believed that the 99% figure had been widely misunderstood in regulatory circles to mean that 1% of injected CO₂ would leak. This was perceived as a misinterpretation of risks from storage operations as leakage would most likely not occur as a continuous event, but instead follow a well blowout as a single constrained event, dependent on pressure build-up rather than injected volumes: “So to talk about leakage rates is totally fallacious” (R28). In other words, this interviewee was concerned that communicating the risk of leakage as a rate of the injected CO₂ misrepresented its dependence on pressure build-up in a geological store.⁵¹

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⁵¹ Following the discussion of the Economides incident in chapter 4, there is evidence to suggest that geoscientists involved with CO₂ storage research would largely agree with this statement.
The transition from statements based on highly constrained data and assumptions, arguably cast in terms of uncertainty, towards generalised scenarios utilised as precise risk profiles, however appears to have been relatively seamless. As Wynne states, we should perhaps not be surprised at this seamlessness simply because the rhetorical requirements attached to risk and uncertainty in science and public policy differ so markedly from one another. Whether probabilistic or deterministic, the language of risk implies that potential hazards and odds are known. Uncertainty however acknowledges that potentially influential factors remain unknown, and possibly unknowable (ignorance), until a time when external commitments force them to become recognisable. The quantitative formulation of storage integrity in the *Special Report* and policies in which the 99% figure has been directly quoted, have provided a decidedly risk-based framing of phenomena related to storage integrity and can be traced throughout the EU's regulatory language.

**CO₂ storage in EU regulations**

This shift in rhetoric and the changing evidentiary context for monitoring and modelling is observed in the transition from scientific practices to regulation. Arguably, the best example of this is the EU CCS Directive, introduced into law in 2009 and since transposed into law by most EU Member States. The Directive primarily regulates activities related to the injection and storage of CO₂ and supersedes several existing regulations that might have otherwise barred storage. The legal basis for CCS in the EU is thereby extremely specific to a limited set of activities and the annexes to the law likewise provide detailed descriptions of the technologies and dynamics that should be considered when characterising storage sites and updating flow models. Interviewees explained that information exchange between policymakers and geoscientists in the development of the Directive and earlier regulatory activities such as the OSPAR Convention and the London Protocol, in large part took place through informal communications. A geoscientist from industry who had provided expert advice to the Commission as well as national authorities, explained that informal
workshops with regulators often allowed intense discussions between regulatory stakeholders, research scientists and industry representatives, specifically because note taking often was prohibited and specific references would remain absent in subsequent reports. This anonymity allowed industry representatives to share conclusions from internal research without disclosing data to competitors. It is worth noting that the research geoscientists interviewed for this thesis who had been directly involved with informing the Directive, believed that the final wording's openness towards a range of techniques and the absence of technology prescription allowed the Directive to strike a balance between procedural recommendations and flexibility for operators. As an example, a policy stakeholder involved in drafting subsequent Guidance Documents released in 2011 explained that these underwent a second drafting process when Member State representatives voiced concerns that earlier versions had been too prescriptive. In this respect the EU regulations followed recommendations in the IPCC inventory guidelines and methodologies in demonstration projects such as Sleipner closely, by allowing a range of monitoring and modelling approaches, as long as fundamental questions about storage integrity and safety were being addressed in a manner that was deemed to be adequate.

The Commission presented a draft version of the Directive in 2008 and it was subsequently debated in Parliament where several amendments were proposed. Some of these emphasised the importance of enabling mechanisms to support technology development alongside regulatory oversight, and included recommendations that Member States should support infrastructure development and financing. This was most clearly displayed in the call for financial mechanisms to support the development of 12 demonstration projects by 2015. Later, this was formalised into the New Entrants Reserve 300 million CO\textsubscript{2} allowances (NER300). The parliamentary rapporteur for both debates on the Directive and the NER300 funding mechanism recalled in an interview that a balance between regulation and development had been a critical aim at the time:
I wanted to make sure that there was a system in place, which would allow CCS to develop...I didn’t want a regulatory mechanism that was too cumbersome. That said, I wanted Commission oversight, not necessarily final approval [of projects], but Commission oversight, and of course that’s how we put it in the end, because the Commission could issue an adverse opinion. And although that was not necessarily binding upon the Member State, in practice the public relations effect of an adverse opinion would destroy the CCS project.

(R44)

Apart from funding mechanisms such as the NER300, financing was primarily envisioned to arrive via the ETS as allowances for stored carbon. Monitoring and modelling practices therefore had to comply with existing regulations for carbon abatement accounting, and stipulated that a leakage event would lead to the surrender of allowances of an equal amount. Questions of what would count as convincing evidence of leakage and who should bear the burden of proof were therefore central to concerns over long-term liabilities, some of which were clarified in the four subsequent Guidance Documents released in 2011.

Differences in wording with potentially large consequences for the management of storage sites, were however already evident in the proposed parliamentary amendments to the Directive in 2008. An example was the suggestion to replace a criterion that storage sites should exhibit “no significant risk of leakage” with the amendment that a site should only be proposed for storage if “no risk of leakage is anticipated that could have a negative impact on human health or the environment” (Davies 2008: 7). This proposal would move the regulatory act from being incident to any evidence of leakage, towards evidence of leakage that could be considered an impairment of the surrounding environment. The rapporteur explained that all such proposals at the time should be viewed in the light of the relative support that existed for climate change mitigation. This meant that there was broad support for strong Commission oversight and authority in enabling projects, and in terminating them in case of poor performance:
You have to remember that it was a slightly different climate at the time. The climate at the time was very much pro-CCS, it was pro doing things about CO2. And I would have felt uncomfortable if the legislation had given a government that could not be entirely trusted, [permission] to authorise the underground storage of CO2, without submission of documentation to the Commission, and for the Commission to have the opportunity to pass, to assess, whether or not the process of assessing the site and the evidence about this site seemed to be reasonable.

(R44)

The interviewee explained that some interest groups, notably environmental NGOs, had displayed considerable resistance towards CCS, but that it was generally acknowledged as a balanced suggestion among stakeholders to maintain regulatory oversight for demonstrations. This is also reflected in the absence of radical amendments to the wording of recital 33 (previously 26), whereby transfer of responsibility for a storage site to a government authority can only take place “if and when all available evidence indicates that the stored CO2 will be completely and permanently contained”. Recital 35 in the final version of the Directive furthermore requires that monitoring activities should be intensified if leakages or other irregularities are identified and that former operators will be required to pay for such activities in case they are shown to be at fault. Lastly, recital 36 mandates that financial provisions should be in place to ensure that a former operator surrenders carbon allowances as determined by the ETS price. The amendments submitted by the Parliamentary group both supported and strengthened this argument (Davies 2008: 11-12). In practice this meant that a consistently strict interpretation of operators’ responsibilities was supported throughout the adoption process and reaffirmed by the proposed Parliamentary amendments:

The legislation provides for a private operator to pass responsibility to a Member State for the very long term storage of CO2, but only after there is near absolute certainty that the possibility of leakage has been reduced to zero.

The IPCC envisages leakage rates of no more than 1 per cent every 1,000 years, a period four times longer than the entire history of industrial civilisation. Nonetheless, in the opinion of the Rapporteur, any leakage
that can be anticipated to have a negative impact on human health or the environment is unacceptable.

The Norwegian Government reports that after 10 years of injection operations at Sleipner beneath the North Sea there has not only been no leakage but no migration outside the limits predicted. With the passing of time stored CO₂ becomes more stable and leakages even more improbable. (ibid: 41)

It is noteworthy that the 99% figure and Sleipner were now employed not only to point out that CO₂ storage was a generally safe activity, but also to emphasise that leaks of any magnitude that could be thought to have a negative impact on the environment would be unacceptable. In this respect the Directive went beyond the knowledge gained from specific sites and beyond the general statements of probabilistic risk assessments, with an assertive value statement that demanded a complete ban on leakage. This is also borne out in the final Directive’s requirements for reservoir monitoring and modelling activities before responsibility for long-term stewardship can be transferred from an operator. Sections (a) and (c) of article 18(2) largely follow procedures that can be recognised in Sleipner and IPCC documentation by requiring that operators at a minimum demonstrate the following:

\[(a) \text{ the conformity of the actual behaviour of the injected CO}_2\text{ with the modelled behaviour};\]
\[(b) \text{ the absence of any detectable leakage};\]
\[(c) \text{ that the storage site is evolving towards a situation of long-term stability.}\]

(EU COM 2009: 18(2))

Section (b) however states that any detectable leakage is unacceptable. Much of the debate around CO₂ storage risk in Europe has been in response to this perspective, the retention of certain responsibilities with operators after site closure, and the possibility that an unknowable cost will be incurred in case of leakage. For example, in 2011 the UK’s Carbon Capture and Storage Association (CCSA), an interest group strongly supportive of CCS, raised the concern that phenomena for which the odds and impacts could not be clearly defined were
perceived as major operational threats to prospective operators: “It is the unspecified, unknown quantity, unknown duration risks that are of most concern” (CCSA 2011: 2).

The insistence on no detectable leakage might partly be seen as a response to public concerns with scenarios that projected and permitted leakage (Ha-Doung and Loisel 2009). And it certainly follows comments made by EU Parliamentarians during debates about the Directive that leakage would be unacceptable (Davies 2008). It also follows the accounting measure adopted in response to the Directive becoming integrated with the ETS where existing monitoring guidelines demand that any leakage would need to be quantified and an equal amount of emissions allowances surrendered (EU COM 2011a). As the policy stakeholder quoted above explained, the ETS became a central negotiating piece with factions set against CCS demanding that allowances should only be made available after storage:

*That was to buy off opposition from people who said we could be spending all this money, and CCS is an unproven technology. We could be handing out money to developers and we were going to see nothing of consequence as a result. So, OK, we say you only get your money when the CO₂ is permanently stored. Well, that sorted out the short-term political problems, but it doesn’t work because developers need money up-front.*

(R44)

An interviewee from industry likewise explained that support for ETS allowances was brokered as a process of “political horse trading” (R42). Payment of ETS allowances following injection strengthened the case for additional funding mechanisms such as the NER300 to support development costs upfront. It also clarified that injected CO₂ would have to be accounted for precisely.52

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52 I am not arguing that policymakers seriously considered allowing developers to inject CO₂ without accounting for it as precisely as possible. Rather, that the counterfactual of proving a no-leak situation implies a peculiar relationship between scientific evidence and emerging regulations. By
This therefore presented accounting of injected volumes as a key regulatory question rather than a concern for geoscientists to resolve exclusively within their community. The Guidance Documents to the Directive in turn recognised that technical uncertainties would bear on an accurate estimation of leakage and therefore opened the possibility of relying on site specific performance measures related to plume development, threshold values for corrective measures, baseline measurement values, etc. This approach was also suggested for the reservoir as a whole to assess leakage risk: “It may also prove useful to develop overall performance measures and standards for the entire monitoring scheme in terms that probability is X% of detecting a leak of Y tonnes per year or more within a time period of Z days or less” (EU COM 2011a: 105).

According to an interviewee involved with the drafting process, the Guidance Documents could thus build on wording in the Directive but could not significantly alter their intention. Nor were the documents prescriptive. This meant that proposals such as the one above for a risk assessment methodology that followed the probabilistic risk assessment presented within the IPCC Special Report and inventory guidelines, were simply options. And while the approach above might delineate trigger points for regulatory actions, it would not change the Directive’s zero tolerance towards leakage.

**Regulation and precaution**

Wynne has suggested that regulations aimed at anticipating adverse outcomes by relying on quantifiable technical parameters, by design ignore uncertainties that arise from use in complex social contexts. Instead of mitigating uncertainties such precaution instead ignores the crucial role of indeterminacies:

[Indeterminacies] are not merely lack of definition in a determinate cause-effect system; the relationship between upstream commitments and downstream outcomes is a combination of genuine constraints which are

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virtue of the definitions applied in the Directive, the matter of accounting techniques thus necessarily became a regulatory concern.
laid down in determinate fashion, and real open-endedness in the sense that outcomes depend on how intermediate actors will behave.

(Wynne 1992: 117)

He thereby frames technical debates within a concern about their social implications rather than distinguishing between the two as facts on one side and values on the other. Wynne’s framing should be seen as a response to a tradition of defining risk analysis in terms of objective facts about the physical world that separated risk management as a distinct, and subjective, category (see e.g. Royal Society 1983, NRC 1983). Along with the work of other STS scholars (Irwin and Wynne 1996; Jasanoff 1986; Jasanoff 1990b; Stirling 2008b; Van Zwanenberg and Millstone 2005) his model points out that risk analysis and risk management often overlap, complicating a clear distinction between facts and values and necessitating hybrid evaluations when decisions about acceptable risks are made in policy fora. Following this observation, the tolerance level towards leakage developed in the Directive and the Guidance Documents can thus be seen as an objectively managed criterion, independent of uncertainties associated with the science underlying CO₂ storage as well as the social context of development and use.

Fisher and Harding (2006) have likewise observed a general tendency to resort to a ‘rational-instrumental’ ideal in environmental risk management in policy circles that emphasises ‘objective’ interpretations of risk. According to Heyvaert (2010) this is the dominant mode of conceptualising risk within EU legislation and ensures that regulatory methodologies are geared towards risk in terms of actionable ‘facts’ and based on a strict separation between fact-finding and decision-making. The standardisation of legislative procedures is also evident in the EU Commission’s Communication on the Precautionary Principle which conceptualises risk management as a process that is distinct

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There is no single definition of the principle that is consistently used in European Community law. The following is one workable definition:
and separate from scientific risk assessment: “The precautionary principle, which is essentially used by decision makers in the management of risk, should not be confused with the element of caution that scientists apply in their assessment of scientific data” (EU COM 2000: 3). Fisher and Harding have contrasted this delineation of a fault line between scientific risk assessment and regulatory risk management in the EU Commission, with an alternative interpretation of the precautionary principle found in guidance documentation for Australian environmental policymakers (Deville and Harding 1997). They refer to this as a ‘deliberative-constitutive’ interpretation, which does not divide risk assessment and risk management into separate scientific and political processes, whereas “[f]or the Commission [environmental and public health] problems are inherently manageable by methodologies and thus what is required is an institutional framework that ensures decision makers carry out pre-ordained tasks” (Fisher and Harding 2006: 22-3). These pre-ordained tasks focus on risk management, leaving risk assessment to the scientific domain.

These two contrasting interpretations of how to implement the precautionary principle therefore function as different theoretical models for the public administration of risk, and by extension the admission of uncertainty and indeterminacy into assessments used to inform management practices. The rational-instrumental analysis of the precautionary principle sheds light on the framing of leakage risks employed in the CCS Directive, a framing that has implications for several aspects of operators’ long-term liabilities:

- Operators assume strict liability for leaks during the operational period and public liability coverage commences only once operations have been turned over to a competent authority.

Where, following an assessment of available scientific information, there are reasonable grounds for concern for the possibility of adverse effects but scientific uncertainty persists, provisional risk management measures based on a broad cost/benefit analysis whereby priority will be given to human health and the environment, necessary to ensure the chosen high level of protection in the Community and proportionate to this level of protection, may be adopted, pending further scientific information for a more comprehensive risk assessment, without having to wait until the reality and seriousness of those adverse effects become fully apparent.

(Von Schomberg 2006: 37)
- The site operator must monitor stored CO\textsubscript{2} annually to ensure that no leakage or “significant irregularities” have occurred. Corrective measures must be taken and the operator must surrender EU ETS allowances totalling the amount of leaked CO\textsubscript{2}. To quantify the amount of leaked CO\textsubscript{2} the operator will need to refer to the last date when no emissions to the water column were reported or in some circumstances the date on which injection commenced (EU COM 2011a: 119). This raises the uncertainty of how very small leaks that may previously have been undetected over several decades should be accounted for as well as questions about the implications of evolving baseline calculations and monitoring options.

- Operators will need to provide financial security to cover the cost of corrective measures and the surrender of ETS allowances. These “[a]mounts should not be adjusted by multiplying with an estimated probability to calculate an expected value” (EU COM 2011b: 17). This means that financial security in case of leakage should be available for the full amount of injected CO\textsubscript{2} multiplied by the current market price of allowances in the ETS. This effectively places an unknowable financial burden on operators who have been injecting CO\textsubscript{2} for several years, particularly if the baseline for a no-leak scenario is drawn at the start of operations.\textsuperscript{54}

It is therefore perhaps surprising to hear from an influential industry stakeholder that the expectation of zero leakage from sites was championed by industry:

*We’ve had to fight hard actually in the industry to get the concept of there being no leakage, because nobody before would believe that there could be*

\textsuperscript{54} Insurance schemes would appear to be ideally placed to mitigate the financial risk associated with leakage, but as of yet no schemes have been made available to prospective operators aside from coverage for operational activities. Besides, as an industry stakeholder explained, large oil and gas companies would be most likely to enter storage operations and these companies often have stronger balance sheets than insurers. They are therefore ideally placed to insure their own operations (as opposed to storage), as is often the case for hydrocarbon extraction.
zero leakage, and you've got to say that there will be to all material effects, zero leakage. Because, a lot of people have talked about, well let's have a nominal 1% or 0.5%, but if you have 1% of leakage, it means the store gets emptied in a hundred years, well no because it has a sort of exponential decay, but on the other hand it does mean that eventually all the store will leak out. And that's not gonna happen. So, you can't have some kind of arbitrary percentage leakage, cause it's just as wrong as saying zero.

(R28)

The interviewee thus stressed the artificiality of anticipating any leakage scenarios as simple risk events with well-defined magnitudes and odds. Rather, support for a zero leakage rate was perceived as a favourable strategy by this interviewee at the time because future developments in large part depended on regulations being in place that needed support from stakeholders opposed to CCS. To consider the zero leakage provision as an obstacle to commercial development is therefore to revisit it in the light of political consensus on CCS development in the EU having been secured; prospective developers may rightly argue that it increases monitoring and insurance costs, but it was likewise viewed as a necessary sacrifice by industry at the time to move negotiations along. The parliamentary rapporteur for the Directive explained that at the time of its drafting, prospective operators had thus shown considerably less concern with the liability provisions because expectations for CCS development were different:

They [prospective operators] were not [frustrated with the wording] at the time. At the time people thought that the legislation was a fair balance and that problems would be resolved as we worked through them. It's different with the carbon price down and less emphasis on much less political direction of certainty about developing low carbon [technologies], having a low carbon energy strategy.

(R44)

An interviewee who was involved in drafting the Guidance Documents in 2011 explained that prospective operators now were much more concerned about the clarification of the liability provisions: “The big worry was, you know, that [requirements for] financial security would be way too high and that CCS would
never take off. That was the biggest contention” (R38). An interviewee from industry was more blunt in criticising policymakers for resistance towards their concerns, even at the time that the Directive was being drafted: “The original language of GD4 [Guidance Document covering long-term liability] was appalling. It actually, if anything, made the original Directive worse, if that’s possible. What we ended up with was a fudge, which basically said, Member States deal with it” (R42). According to this interviewee, the effect of the storage liability provision was to significantly erode the willingness of industry to develop storage projects.

On the other hand, a research geoscientist explained that he was glad that leakage monitoring and accounting had made its way into the Directive in connection with the ETS as these provisions likely would increase accountability. It is noteworthy that this interviewee acknowledged that liabilities of an unknowable magnitude could be imposed on operators and that this could make them reluctant to develop projects. As a management option, an interviewee who had worked on several legal frameworks for waste streams explained that financial security could be constructed as a pooled fund for several projects and reduce individual payments by sharing liabilities. This would obviate the need for third party insurers and emulate mechanisms for other waste storage schemes. A similar approach has been proposed for CO₂ storage risk management in the US (CCSReg Project 2009) where liability pooling is likewise common for events that may adversely impact the natural environment. Other research has found willingness among insurers to cover part of the costs associated with the surrender of ETS allowances (ClimateWise 2012). Such options continue to raise questions about the incidence of residual unquantifiable costs and the influence of regional disparities in governance styles amongst Member States that could introduce varying degrees of acceptability for operator fault and liability, complicating the situation for international storage schemes.
The Directive’s compartmentalisation of risk management decisions related to financial security from risk assessments of CO₂ storage, is thus not the only imaginable regulatory outcome. Leakage potentialities could have been interpreted probabilistically with the understanding that small amounts would be permitted to leak if sites were properly monitored and mitigated. Probability calculations as a basis for assessing insurance costs are standard practice for environmental pollutants covered under the EU’s Environmental Liability Directive (ELD) (Bio Intelligence Service 2008). An alternative model is also available in the Commission’s proposal for the regulation of the offshore hydrocarbon industry (EU COM 2011c). This requires “a systematic assessment of the likelihood of hazardous events and their consequences” and follows the risk calculation approach in the ELD, “so that the risks of major accidents to people, the environment, and offshore assets are acceptable” (Article 3(4)). Such formulations do not remove uncertainties from an analysis, but with less technically deterministic wording the incidence of the evidence burden is moved further downstream from scientific assessment and beyond strict precaution in regulations, opening up the possibility of a debate about the role of management practices in risk analysis. Socially mediated indeterminacies are thus definitively drawn more directly into analysis and risk management strategies move closer to a deliberative-constitutive interpretation of precaution.

The co-production of geoscience research and governance

The discussion above shows that CO₂ storage regulations have both been informed by scientific findings and in turn given shape to a research agenda that has increased the focus on baseline and near-surface monitoring. An example of this dynamic was the 2011 meeting of the IEAGHG CO₂ storage monitoring network, a scientific group that meets annually to discuss new results and

55 ‘Acceptable’ is defined in the following way in the proposal: “‘acceptable’ shall mean: rendering a risk of a major accident tolerable to the furthest extent beyond which no significant reduction of the risk is derived from the input of further time, resources or cost” (60). The question of course remains how this definition is weighed in individual cases.
methodologies. The meeting agenda was structured around the three main requirements for transfer of responsibility in the Directive: that the behaviour of the injected CO₂ conforms with models; that there is no detectable leakage; and that the storage site is evolving towards a situation of long-term stability. Much of the discussion focussed on the difficulty of meeting the Directive’s instruction to quantify leakage and speakers listed several reasons that near-surface detection techniques should be employed even where it might be difficult to cover the full extent of a storage complex or provide good background data for baseline measurements (IEAGHG 2011: 4). Geoscientists were thereby responding to regulations with a research agenda that would employ new technologies and techniques to answer questions and concerns posed by regulators. This was in contrast to earlier research emerging from projects such as Sleipner that had informed and structured regulatory categories. Geoscientists had since then also provided advice during the OSPAR Convention and London Protocol negotiations, and many of these same people maintained informal advisory relations with EU regulators and consultants when the Directive was drafted. Geoscientists were thereby central in informing regulations that laid the basis for negotiations in the EU Parliament and publications such as the Guidance Documents. And with provisions in place that required precisely quantified data on e.g. containment and leakage, geoscientists in turn responded to these technical criteria through new research agendas. Authority to determine a regulatory agenda now rested more definitively with the Commission and the CCS Directive, and geoscientists maintained a role in developing responses rather than defining the terms of the risk analysis framework.

Many of these criteria arose from negotiations with a range of stakeholders rather than just regulators. One industry representative provided an example of how opinions among stakeholders differed with respect to appropriate risk management. Several NGOs had been unhappy with an early proposal that operators should maintain responsibility for a site for a ten-year post-closure period and instead pushed for a 20-year period to ensure that stores would be
“twice as safe” (R34). The same respondent thought that this was however a misinterpretation of how the risk of a leakage event should be conceptualised – rather than rely on a universal temporal standard it was proposed that management practices should follow risk quantification informed by site-specific geological and fluid-flow characteristics.

Reliance on directly observable characteristics to inform a risk model was in turn seen as an unlikely methodology by a geoscientist who described geological interpretation as a highly uncertain process that involved inherently unquantifiable parameters. This interviewee saw little hope of knowing what developments would take place when several poorly constrained parameters interacted and was therefore sceptical about the possibility of accurately quantifying risks. Reliance on a range of performance measures as suggested in Guidance Document 2 was seen as a more fruitful methodology, but this was in turn complicated by the absence of comparative knowledge on failure events. In lieu of direct experience with failure management it would therefore be difficult to decide which characteristics were important to pay attention to in prospective sensitivity analyses: “That’s what I mean, is it an appropriate measure? A measure has been proposed from the top down, so to speak, rather than the bottom up” (R16). The demand for such measures was thought to come primarily from regulators wanting to quantify leakage liability, a concern that clashed with geoscientists’ understanding of operational uncertainties: “They are not looking at it from our [geoscience] perspective, [which] is that we would endeavour to make it as uncertain as possible. Again, a non-quantifiable scale” (R16). This follows the analysis in the previous chapters – while it may be possible to provide quantified information using models, such outputs often involve tacit judgments about relevant parameters, adequate precision and possible extrapolations. When regulations move the incidence of an evidence burden further upstream by demanding technical performance measures, these data are increasingly removed from uncertainties that arise in real world application. In this way geoscientists found themselves increasingly responding
to the rhetoric and concepts of a regulatory agenda rather then setting the terms of an analysis within the practices of their community.

While indeterminacy is rarely raised as a factor in scientific uncertainty analysis, instrumental limitations and their implications for precision are, and a number of geoscientists commented that CO₂ storage regulations were often more aspirational than based on technical realities in subsurface monitoring. A geoscientist explained that the Directive’s provisions on leakage, which he and his colleagues had been closely involved in advising, included some “funny wording”: “There’s one of these, what is it called, the total and permanent containment must be guaranteed. Well you can’t prove that, it’s an aspiration, but you can’t prove that” (R7). Others who had worked with regulators explained that the anticipation of risks could involve problematic disconnects when compared with the practices of geoscientists: “I think it is a problem...that there is a disconnect. Either regulators or legislators are often trying to write laws that are inconsistent and not, um, what’s the word, well they are inconsistent with the way that science and reality is, and they are not...they just don’t work with our understandings” (R25). This interviewee mentioned a legislative context outside the EU where 99% of injected CO₂ should be accounted for. Although he was convinced that the vast majority would remain stored he explained that instrument limitations made it impossible to meet the required level of precision: “We know that it’s all down there, but again quantitatively demonstrating that using remote sensing, geophysical imaging, well monitoring, you name it, that’s not possible. And yet, you know it’s still there. That’s a disconnect” (R25). A research geoscientist who had provided policymakers with advice on several occasions took a similar perspective and was worried that regulations were being drafted too prescriptively when few well-founded predictions could be made about storage integrity generally. This was contrasted with risk assessments for oil and gas extraction and radioactive waste storage where provisions for predictability often are based on probabilistic scenarios of containment over time. The interviewee thought that
a discussion about storage scenarios would yield a more realistic understanding of how scientific findings could inform risk management:

So you don’t know what the permeability, the ease of flow, of CO₂ through the reservoir is. You’ve got a probability function there, that some of the CO₂ will move through very fast and other parts of the CO₂ will move through different parts of the reservoir much more slowly. So that’s an inherent variability in the system. Which then, if I’m asked a question of how fast will the CO₂ move through, that then effectively means I’ve got an uncertainty about how fast the CO₂ will move through. Because some of it will move fast and other parts of it will move slow. So I can’t say ten years or a hundred years. I can then say that 1% of it will move through in ten years. 99% of it will move through in 10,000 years. That’s the type of answer you’ll have to give.

(R17)

This explanation evokes the IPCC’s 99% figure by stressing the role of uncertainty in technical risk assessments. While indeterminacy was not directly recognised as a factor in risk assessments that could determine risk management, it was indirectly recognised in geoscientists’ perceptions that EU regulations had developed an independent perspective. A geoscientist from industry who had advised national regulators and the EU Commission on leakage from injection wells commented on the differences in interpretation across institutions:

respondent: And I have to admit that dealing with Norwegian regulators was constructive in my mind. Dealing with the European Commission has been very frustrating.

interviewer: How so?

respondant: I don’t think we managed to communicate. Namely, we are living in different worlds and that’s fine. There’s been no bridge between those two worlds. It’s been, we’re doing this. Ah no, you don’t understand, we’re doing this, we’re doing this. Ok, well just do this (laughs). What can I say? You know if you don’t want to listen I’m going to stop talking. So it’s been very frustrating.

(R24)
The interviewee did believe that a compromise on adequate evidence would eventually be reached as more time was devoted to interpreting the impact of legal provisions on operations. Yet past dealings with regulators had proven to be disheartening and there had been little willingness to work with industry to evolve expectations for storage operations beyond the zero leakage provision towards keeping leaks “as low as reasonably practical”. Such concerns highlight a contrast in the cultural perspective on risk among geoscientists compared with that developed in EU regulations.

While many interviewees were concerned that a zero leakage perspective in the Directive would restrict the development of storage projects, an alternative explanation was voiced by an industry stakeholder – namely that the provision could lead to inadequate monitoring unless regulators would be closely involved in developing comprehensive monitoring programmes. The interviewee was speaking from experience dealing with oil and gas operators that had implemented poor monitoring programmes to reduce the chance of detecting leaks. A proposed alternative was a ‘no consequences’ provision, which would accept that relatively small leaks would remain undetectable. The choice of regulatory style was thus perceived as introducing indeterminacy by altering social response mechanisms to data collection and evidence construction.

**Discussion**

The Sleipner project has had a significant influence on arguments for CCS, both in policy arenas and in other scientific research projects. A publication outlining the Snøhvit demonstration project has for example referenced it as proof of concept for CO₂ storage: “The technology for such storage of CO₂ is considered proven by the CO₂ storage at the Sleipner field in the North Sea” (Maldal and Tappel 2004: 1403). Similar statements have been made in EU policy documents and are reinforced by technical risk assessments such as the IPCC’s 99% figure, which presents leakage as a knowable consequence rather than an
uncertainty (or as ignorance). These perspectives have legitimated the coupling of CO₂ storage with the ETS, which in turn has necessitated a risk-based quantification of leakage. Collingridge and Reeve have famously stated that translations of scientific output into policy inevitably are challenged by the two worlds moving at different paces and presenting different needs (1986). Statements by geoscientists concerned that regulatory requirements do not follow what it is scientifically possible to prove to a sufficiently accurate degree, could be read as an indication that evidence in time will catch up with regulatory expectations:

At present, the state of the art in geomechanical modelling, and in linking geomechanical models with geophysical observations, is probably not sufficiently advanced to fulfil the requirement that ‘the conformity of the actual behaviour of the injected CO₂ with the modelled behaviour’ (E.U. Parliament and Council, 2009) could be rigorously demonstrated in a manner analogous to reservoir modelling of CO₂ distribution and 4D seismic observations. Nevertheless, we anticipate that with more detailed and advanced geomechanical models, and a more rigorous method for predicting seismicity based on geomechanical models, further advances will be made.

(Verdon et al. 2011:151)

As the previous chapters show, such perspectives acknowledge that limitations to precision are introduced by a range of uncertainties, but that accurate risk assessments can nevertheless be developed according to standards agreed within the scientific community. Following Wynne’s (1992) framing, geoscientists have tended to present knowledge of risks as a matter of acquiring increasingly refined technical understandings of complex systems without accounting for the limitations to quantitative analysis implied by ignorance and the indeterminacy that is introduced when technical systems are employed in a social context. EU regulations in turn have presented a rational-instrumental ideal of risk management in storage projects that effectively does not acknowledge technical limitations in monitoring data and modelling practices. This leads to an impression that risk assessment is a purely objective activity, while social aspects are only introduced in management practices. The gap in
rhetoric between regulations, and the practices and perspectives of geoscientists, reinforces the impression that regulatory and scientific dynamics have unfolded separately from one another. However, both have clearly given shape to the other through scientifically driven evidence development and the construction of a research agenda informed by regulations. To present one as separate and distinct from the other therefore obscures the effects that management concepts and practices will have on assessment methodologies and vice versa. Analyses of stakeholder negotiations in regulatory science fora have furthermore shown that the formulation of standardised tests, selection of critical thresholds, and decisions about what should count as acceptable evidence must be understood as contingent on culturally specific modes of reasoning (Jasanoff 1990a; Rothstein et al. 1999).

CO₂ storage arguably involves fewer uncertainties than climate change science, but there are parallels in societal demands for risk estimates of both. Following Douglas and Wildavsky (1982), Hulme (2009) notes that debates around the risks of climate change involve large uncertainties that cannot be approached solely as discussions about objective physical phenomena. Rather, risk assessments and risk management approaches should be seen as socially constructed phenomena. Quantitative assessments necessarily draw boundaries around the precision and uncertainty of monitoring instruments and modelling practices. This blackboxes socially mediated indeterminacy, which is only amplified by precautionary regulations that may restrict allowances for uncertainty. Neither the regulations nor the science acknowledge that indeterminacy arises when these two modes of understanding meet. This has given rise to expectations that are incommensurate with the ways that geoscientists conceptualise uncertainties, and methodologies that have closed down questions about wider understandings of uncertainties, rather than explored their implications for interpretations of safety.
Various perspectives on risk acceptance that could be drawn on to inform regulations of management practices for CCS are witnessed in e.g. research on public understandings of safety and risk (Bradbury et al. 2009; Hammond and Shackley 2010). Recent years have seen protests from local citizens’ groups against storage projects, which have been concerned that leakage would present a significant health risk, damages to local ecosystems and a drop in local land values (Bradbury 2012). Recent focus group research carried out in the UK has also concluded that public stakeholders tend to have relatively little trust in official messages about CCS communicated by industry and government and tend to think that the risks surrounding the technology are unclear (Howell et al. 2012). Such voices and their implications for projects both introduce and emphasise ways that indeterminacy can be understood as integral to a thorough analysis of uncertainties (Stirling 2008a, 2008b; Yearley 2000). Technically focused risk assessments miss the point if social pressures mount and require that monitoring and modelling activities should change to accommodate views that have been left unaccounted for. Examples of such triggers may include the recent discovery of a large fracture near the Sleipner injection point (Schaps 2012). Although this will not necessarily function as a conduit for CO₂ it may very well increase monitoring costs if regulators or members of the wider public become convinced that more could be done to assess leakage risks.

The wording regarding leakage in the Directive and its connection with the ETS has profoundly impacted understandings of risk and reshaped the research agenda on CCS. As it stands, a regulatory impetus towards precaution has prevailed in shaping the language of CO₂ storage risk and geoscientists have subsequently found themselves responding to a conceptual framing far removed from their own. As discussed in the previous chapters, moving the incidence of an evidence burden towards the domain of scientific knowledge production will not in itself invite discussion of unquantifiable uncertainties and ignorance. Placing the evidence burden within regulations will likewise not necessarily resolve questions about appropriate definitions of risks or engage stakeholders in a debate about indeterminacy. For either to occur, efforts at
inclusive engagement are necessary that will involve discussions about the implications of hybrid risk assessment and risk management practices.

The following chapter presents a case study of how the discovery of indeterminacies have affected the development of the CCS Test Centre Mongstad in Norway, where concerns about a public health risk associated with CO₂ capture have reshaped research agendas.
7. Technology management in the face of scientific uncertainty: A case-study of the CCS Test Centre Mongstad

This chapter has been revised from an earlier version published with Simon Shackley in 2012. The chapter continues the discussion of how risk, uncertainty and indeterminacy may influence risk assessment and risk management approaches by developing a case study of the CCS Test Centre Mongstad (TCM) near Bergen, Norway. It begins with an overview of concepts from STS literature on ‘incrementalism’, a short account of the political context for climate change mitigation in Norway, and a summary of budget increases and delays at TCM. The emergence of a health concern related to CO$_2$ capture is thereafter covered, with a focus on the implications of differing interpretations of scientific uncertainty. It ends with a discussion of how insights from the literature on incrementalism in technology policy may benefit uncertainty management. Data is drawn from policy documents, regulations and news articles.

Introduction

The literature on incrementalism is rooted in the policy science tradition with Charles Lindblom’s work. Lindblom observed that the ‘rational-comprehensive’ or ‘synoptic’ ideal – in which policymakers examine and compare all possible options before deciding on an optimal approach – was a cumbersome prescription and an inaccurate description of how decision-making takes place under uncertainty and under pressure to establish agreement between political factions. He explained that observers of the policy process instead are well aware that decision-making in such cases follows a set of ‘successive limited comparisons’, in which choices are made incrementally to reach agreement on the basis of imperfect assessments. Aims may be well-defined, but are open to re-evaluation as new information emerges. And rather than assume that there

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is a linear progression from high-level aims through policies and regulations as the rational-comprehensive method does, a test of ‘good’ policy under the method of successive limited comparisons is more modest. It simply asks that competing interests can agree on discrete steps rather than final objectives: “In an important sense, therefore, it is not irrational for an administrator to defend a policy as good without being able to specify what it is good for” (Lindblom 1959: 84).

Lindblom’s description of the policy process was subsequently applied in the science and technology studies tradition by neo-incrementalist academics such as Collingridge (1980, 1990, 1992), Collingridge and Reeve (1986), Woodhouse (Weiss and Woodhouse 1992; Woodhouse and Collingridge 1993) and Morone (Morone and Woodhouse 1986) as a prescriptive concept for the management of ‘big science and technology’ projects. When the cost of committing errors is relatively large, rather than adopt strategies for technology development that aim to meet pre-determined objectives, these academics have suggested that technology selection and application should follow the path of successive limited comparisons. Such advice follows Lindblom’s observations on successful policy development, formulated as ‘disjointed incrementalism’ in his follow-up article from 1979:

1. Limitation of analysis to a few somewhat familiar policy alternatives;
2. Adjustment of objectives in light of the policies potentially available, rather than considering ends in the abstract;
3. More preoccupation with ills to be remedied than positive goals to be sought;
4. A sequence of trials, errors, and revised trials;
5. Exploration of only some, not all, of the important possible consequences of a considered alternative; and

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57 Ishii and Langhelle (2011) have similarly noted that the rational-comprehensive ideal obscures the reality of environmental policy integration, including integrated policies on CCS.
58 Here quoted from Woodhouse and Collingridge (1993).
6. Fragmentation of analytical work to many partisan participants in policy making.

Applied to technology development, these guidelines recommend that early analysis is limited in scope; that end-goals are updated by information from early trials; that such trials do not seek to perfect a pre-selected solution, but instead consider alternative routes to solve a problem; that trials are continuous and therefore relatively small in size; that all possible risks are not exhaustively analysed in trials; and that different actors are provided with a chance to inform analysis, so that the solution(s) arrived at have an increased chance of being implemented.

These prescriptions are discussed in relation to the TCM case study towards the end of this chapter. Development of the TCM has followed in the footsteps of Norway’s past endeavours into CCS technology piloting and demonstration, begun in 1996 with the Sleipner offshore CO$_2$ separation and storage project, and continued with the Snøhvit and Kårstø projects, as well as the now cancelled Halten facility. The TCM is distinguished from similar projects worldwide as one of few that aims to separate CO$_2$ from several types of flue streams including a gas-powered cogeneration plant and an oil refinery. In 2010, an inquiry was launched into the health risks posed by airborne degraded amine components, a by-product in the post-combustion capture process. This chapter addresses the source of this concern, its political and scientific context, and the implications for technology development. The chapter also explains how strategic technology management could be informed by insights from the neo-incrementalist school.

With a focus on CO$_2$ capture in this chapter I steer away from the previous analysis of science and regulations surrounding storage. Following my explanation in the methodology, this choice was made as material was found that presented itself well for an analysis with relevance to my theoretical research aim of charting debates about indeterminacy in relation to scientific
evidence presented in regulatory fora. Similar material on CO₂ storage further downstream from debates about regulatory frameworks was not found. Since component configurations for CCS systems are still not decided and a single dominant design does not yet exist, it seemed relevant to ask how risk assessment and risk management procedures have been impacted in a research and development setting using the material available. My findings can therefore not be generalised to all capture developments or CCS systems, but instead provide a case specific account of how technical knowledge presented in a social setting has altered previous risk conceptualisations.

The political resolve behind CCS in Norway

Norway’s investment in CCS research has served as an exemplar to members of the European Union and internationally since StatOil began storing CO₂ from natural gas extracted at the North Sea’s Sleipner field in 1996. As described in chapter 5, a breadth of related geoscience research activities have helped to ensure that Sleipner has achieved recognition as a successful storage project. And Norway’s extended network of CCS activities and supportive stakeholders (Karimi et al. 2012) has placed it among the top countries in CCS knowledge production. These activities can partly be interpreted as an attempt to manage the country’s reputation as a leader in sustainable development in spite of the economy’s significant reliance on oil and gas production. While Norwegian domestic electricity is almost exclusively generated from hydroelectric dams, oil and gas exports in 2009 accounted for nearly a quarter of the gross national product (22%) and nearly half of all income from exports (47%). Estimates by the government indicate that such supplies will remain plentiful in decades to come as new fields are developed (NPD 2010).

At the same time that the country is committed to its economic reliance on oil and gas exports, a strong political commitment to climate change mitigation has developed since the early 1990s. In 1991 a CO₂ tax was levied on mineral products and the hydrocarbon extraction industry, which applies to 68% of CO₂
emissions. A national emissions trading scheme (ETS) was first launched in 2005 and merged with the EU ETS in 2008. Also in 2008 a national carbon reduction framework committed Norway to achieve zero net emissions of CO$_2$ by 2030, partly by investing in afforestation projects in developing countries and partly by delivering efficiency measures to domestic infrastructure. This framework also supported development of CCS technology domestically. A special CCS reserve for new gas-fired power plants was also provided in the country’s ETS allowances, which followed the implementation of an emissions performance standard (EPS) in 2000 on new gas-fired power plants (IEA 2011a).

These policy instruments indicate that CCS occupies a central role in Norwegian energy policy by helping to legitimise continued hydrocarbon extraction and export. In a study of the Norwegian political environment, Andreas Tjernshaugen and Oluf Langhelle suggest that CCS has emerged “as an important compromise solution to the country’s climate policy dilemma” (Tjernshaugen and Langhelle 2009: 111). They also point to a number of incidents since the later 1990s, which indicate that strategic technology development largely has been left to stakeholders outside the political system, despite the government owning 70% of StatOil – the Nordic region’s largest oil and gas operator (NMTI 2006). This in turn has resulted in politicians often being caught by surprise when a compromise solution on technology development has failed.

As an example, in 1998 Norsk Hydro (now StatOil) announced that a pre-combustion CO$_2$ capture plant would be built in the wake of political controversy over intentions to commence gas delivery from the continental shelf for domestic use. The CCS plan was shelved the following year and rebranded as a long-term research project. However, it soon became a basis for demands to use best available technology to capture 90% of emissions from the intended gas plant. Since such an option was not available, but had instead been
based on projections of potential CO2 capture, the controversy escalated and led to the resignation of the Prime Minister in 2000.

In 2006, under the Stoltenberg government, CCS again arose as a key compromise solution between industry and environmental groups and plans were formed to develop the TCM by 2012 as a stepping-stone to large-scale capture. The centre would be charged with developing conceptual studies of CO2 capture for an oil refinery and a cogeneration plant, and testing capture technologies through laboratory work and pilot studies. The Mongstad location was chosen for the many emission points connected to the refinery including a catalytic cracker plant, Norway’s largest single source of CO2 emissions.

In what follows, an account of rising cost estimates and delays at Mongstad will be discussed along with the emergence of a public health concern. These uncertainties showcase how technical systems applied in a social context give rise to indeterminacies that affect understandings of fundamental science, which may in turn affect public policy.

**Mongstad budget and timeline**

At the time of its announcement in 2006, Prime Minister Jens Stoltenberg called the TCM Norway’s “moon landing”, referring to the 1960s space race:

> Our vision is that within seven years we must have in place the technology that makes it possible to clean the emissions of greenhouse gases. There will be an important breakthrough for reducing emissions in Norway, and when we succeed, I think the world will follow. This is a major project for the country. It is our moon landing.

*(Stoltenberg 2007)*

To cement its support for the project, the government initially agreed to take ownership of 80% of the TCM (since reduced to 75% with the inclusion of

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59 Translated from the original text in Norwegian.
additional partners) through its company Gassnova, while StatOil agreed to fund the remaining 20% and to manage day-to-day operations (MPE 2011).

Designs aimed for the plant to capture 100,000 tons of CO₂ annually from 2012 for five years as a basis for a potential investment decision for a full-scale plant that would capture 2 million tons annually. The intention was to perform small-scale trials with a chilled ammonia solution by Alstom at the catalytic cracker plant and an amine solution by Aker Clean Carbon at the combined heat and power plant. Results from these tests would constitute the evidence basis for a feasibility study of a full-scale plant (MPE 2007). StatOil would cover any cost increases beyond the budget at the time of an investment decision for full-scale capture. However, until this point the government would act as the principal funder of research and development at TCM. Early estimates in 2006 suggested that costs would amount to 1,200 million NOK (€150 million), while the large-scale demonstration plant was estimated to cost around 25 billion NOK (€3.2 billion).

Changes to these estimates were first made in 2008 when the government budget announced that the TCM would likely cost closer to 2,000 million NOK rather than the original estimate of 1,200 million NOK. Separately, additional planning and preparation costs towards full-scale capture between 2010 and 2012 were estimated to reach 1,500 million NOK (NMF 2009). The plans were now also officially delayed and 2009 was deemed to become the most costly year for development. The budget therefore suggested to more than double the allocation for 2009 from 920 to 1,882 million NOK. Cost reductions from further development of amine technology were also curtailed and now estimated to be between only 0 and 10%, far below what was projected by Aker in 2006 (MPE 2009b).

Despite the projection of significant developments in 2009, allocations for the year were subsequently reduced from the proposed 1,882 million NOK back to just 900 million NOK. Allocations towards full-scale planning and preparation
were also reduced by 320 million NOK, citing technology and judicial complexities that would require the development of new solutions in later years (MPE 2009a). Finally, the total cost estimate increased, now to 5,200 million NOK (MPE 2009b). In April 2010 the government concluded that the original 2012 deadline was no longer prudent and instead moved it forward to 2014 (NMF 2010). The year after, in 2011, the deadline was again moved forward two years to 2016 and the total cost estimate now stood at 6,452 million NOK (MPE 2011).

These significant changes did not go unnoticed in the Norwegian press. The environmental NGO Bellona, which has long been a strong supporter of CCS, responded with particularly sharp criticism to the delays and cost increases. Bellona was very critical of the April 2010 announcement to delay the investment decision for large-scale capture, with one leader calling it “the worst [environmental policy scandal] in my ten years in the Norwegian green policy debate” (Fouche 2010). Following another round of budget changes, Bellona called for StatOil to make all information pursuant to decision-making at Mongstad public. The NGO also stated that StatOil was creating “unnecessary uncertainty” around an issue that researchers as well as Aker Clean Carbon believed was manageable (Kristensen and Tveit 2010). The topic of this uncertainty will be discussed below.

**CO₂ capture technology choices**

In response to this criticism, StatOil and Gassnova explained that the revised cost estimates and development trajectories for TCM arose from a need to address knowledge gaps around the public health risk of the proposed capture technology. A 2008 TCM impact assessment pointed out that a number of research projects had been initiated to address such risks, including the health related effects of atmospheric amine emissions and any degraded components (TCM 2008). These uncertainties were commented on extensively in StatOil’s TCM Master Plan released in February 2009 (StatoilHydro 2009), which noted...
that collaborative research projects were addressing the health risks posed by amine degradation. Because post-combustion amine capture was considered to be the most mature technology available based on evidence from other gas separation and CCS pilot projects, the report concluded that only post-combustion technologies, and specifically amine and chilled ammonia capture, were advanced and promising enough to take forward in a qualification phase. However, the report also noted that the TCM would test amines in a relatively novel fashion, in which some of the solution would be allowed to escape to the atmosphere following the CO₂ absorption/desorption cycle.

Pre-combustion and oxy-fuel technologies had been ruled out already in 2006 because of their relative complexity compared with an end-of-pipe solution, and were now ruled out for the TCM project going forward, as both were deemed to cause significantly more disruption and reconstruction of existing and new plants than post-combustion capture would. It is noteworthy that this reasoning was maintained despite the TCM’s goals to specifically “identify, develop, test and qualify possible technological solutions” and to “reduce costs and risks associated with building and operating full-scale CO₂ capture”⁶⁰ (MPE 2009b). Pre-qualification of specific technologies would therefore arguably go against the grain of the project’s aims. Amines were furthermore noted as being particularly promising because of their use in the Sleipner and Snøhvit projects, but neither of these projects emits a comparable amount of compounds to the atmosphere. The same report noted that the CO₂ captured from flue gases at Mongstad would likely contain nitrogen oxides and oxygen and that these compounds could contribute to the formation of secondary chemicals such as nitrosamines and nitramines, formed during the degradation of amines. Both nitrosamines and nitramines, it was noted, were known to be carcinogenic to animals and humans, and their potency and longevity in the natural environment was uncertain.

⁶⁰ Translated from the original text in Norwegian.
In light of these uncertainties, the StatOil Master Plan therefore proposed two development trajectory options. Under the first one, CO₂ would be captured from the cogeneration plant using available amine solutions with the aim of the fastest possible start-up. The refinery’s catalytic cracker plant would be constructed in a second phase based on the gradual development of the chilled ammonia technology. Because of a CO₂ concentration three times greater than in the cogeneration plant, capture at the cracker plant was thought to be able to reach upwards of 85%. This would make the less pollution sensitive chilled ammonia solution more appropriate than an amine-based technology. The second option proposed the simultaneous development of technologies for the two plants based on a phased technology plan, with a minimum of one and a half years of successful operation of the TCM before scaling up to full capture. It is noteworthy that neither of these plans proposed to test a suite of technologies in addition to the amine and chilled ammonia solutions, despite previously acknowledging that both would present clear challenges to scale-up, and that amine development would possibly include some health risks.

**Amines and uncertain health risks**

An early environmental risk screening study by the Norwegian Institute for Air Research (NILU) from March 2009 (Knudsen et al. 2009) also noted the uncertainties attached to the formation of nitrosamines and nitrarnines from amine degradation. In an attempt to quantify this risk the report referred to a safe exposure threshold: “It was discovered that the long-term risk threshold for exposure of the general population by nitrosamines through inhalation is 4 ng/m³ (nanograms per cubic metre) nitrosamines in air, corresponding to a 10⁻⁶ [or one in a million] lifetime cancer risk” (ibid: 14). Based on this figure the report noted that, “the risk related to the studied amines themselves seems to be sufficiently low” (ibid: 15). A reference was not provided for the source of this threshold.
In September 2010 the TCM consortium filed an application for an emission permit with the Climate and Pollution Agency (KLIF) (TCM 2010). The report commented that amine solutions were known to release carcinogenic substances, principal of which were nitrosamines, during CO₂ capture and subsequent amine degradation. Because safe exposure levels for nitrosamines did not exist under Norwegian law, the application referred to a US Environmental Protection Agency safe exposure limit of 0.07 ng/m³ (EPA/IRIS 2011), corresponding to a 10⁻⁶ lifetime cancer risk, for N-Nitrosodimethylamin (NDMA), a relatively potent nitrosamine. NDMA was thus used as a reference chemical to compare with release scenarios for other nitrosamines and nitramines. The TCM report noted that the use of this exposure threshold followed the precautionary principle as NDMA, being a potent carcinogen, had a relatively low acceptance value and none of the solvents and emissions in the calculated scenarios would exceed this limit. A set of worst-case scenarios were developed from this limit, which showed that nitrosamines would only constitute a health problem if very strict precaution was assumed and models were supported by highly conservative assumptions.

The numbers and scenarios in this application appear to have been based on a NILU report prepared one month earlier in August 2010 (Flatlandsmo Berglen et al. 2010). That report noted that all uncertain parameters in the emissions scenarios were scaled to the worst possible values, including the potential for degraded amines to form nitrosamines. For example, a high level of 10% nitrosamine formation had been assumed following amine degradation along with the possibility of direct emissions of nitrosamines when amines reacted with nitrous oxides. This produced a worst-case annual concentration of 0.11 ng/m³. In comparison, two scenarios that relied on parameters thought to be more likely produced nitrosamine concentrations between 0.008 and 0.029 ng/m³, far below the EPA limit of 0.07 ng/m³. As the NILU report noted: “Worst case emission using feed gas from CHP (over a year) may exceed the EPA/IRIS limit value. All other combinations of scenario and feed gas are calculated to be below the limit value” (ibid: 61).
The NILU report also pointed to knowledge gaps around formation rates of nitrosamines. While 2% and 10% values were assumed in the release scenarios, the report acknowledged that the toxicology literature used values ranging from near zero to 30%. This uncertainty in addition to others, led to the conclusion that it would be problematic to rely on a worst-case scenario study of nitrosamine emissions. In other words, the known uncertainties were too great to construct a set of reliable scenarios as a basis for informed decision-making.

**Uncertain health risks investigated**

In November 2010 KLIF published a letter in response to a request from the Ministry of the Environment, which had asked that the health risks of amine emissions be reviewed more thoroughly. KLIF’s letter pointed out that both the TCM impact assessment from May 2008 and StatOil’s Master Plan from February 2009 had noted that there were significant knowledge gaps associated with the health risks of amine emissions. The letter also noted that revised estimates were now necessary following new evidence on amine formation published by the IEAGHG; because the August 2010 report had erroneously assumed that the US EPA figures were recently updated, but in fact dated back to 1986; and because worst-case scenario calculations were likely to overestimate the risk. However, KLIF also acknowledged that there were no EU reference numbers available for safe exposure levels of nitrosamine and nitramine emissions into air and that the EPA figures would therefore continue to be valid (Jablonska 2010).

In a follow-up letter from March 1, 2011 KLIF noted that new evidence from StatOil indicated that nitrosamines constituted a much lower health risk than previous studies had assumed. This was based on new evidence showing that nitrosamine formation from degraded amines was far lower than the worst-case scenario assumption of 10% (in actuality 0.6-1.1%) and evidence showing that
nitrosamines would not be long-lived in the atmosphere. Atmospheric emissions of nitrosamines were therefore deemed to constitute a less severe health risk than previously thought. However, the letter also noted that StatOil maintained that there was significant uncertainty attached to the formation, degradation, and health risks of nitramines (as opposed to nitrosamines). Although Aker Clean Carbon, the developer of the amine solution, had previously claimed that flue gas from the plant could be purified to eliminate both nitrosamines and nitramines, no evidence had been submitted to support this claim. KLIF therefore asked the Norwegian Institute of Public Health (NIPH) to prepare an evaluation of the health risks associated with nitramines (Jablonska 2011).

A summary of evidence prepared for the NIPH report and released in April 2011 again referred to NDMA as a reference chemical for evaluating safe exposure levels to other nitrosamines as well as nitramines, because of its relatively high potency. The assessment also included a table of competing safe exposure levels developed by other national and international institutes of health. NIPH remarked that a figure from the Canadian Federal health department (Health Canada) on safe exposure to NDMA in drinking water, was likely more realistic than the US EPA figure, because the “US EPA risk estimate seems conservative based on the available data” (Låg et al. 2011: 23). The Health Canada figure was subsequently converted to an equivalent concentration in air using the EU Regulatory Framework for the Management of Chemicals (REACH), and yielded a 0.313 ng/m³ safety threshold for nitrosamines in air, corresponding to a $10^{-6}$ lifetime cancer risk. The report also drew on an inhalation study of NDMA to conclude that the safe exposure level should not exceed 0.3 ng/m³, more than four times higher than the US EPA figure of 0.07 ng/m³. Despite StatOil’s reservations, NIPH also maintained that NDMA was an appropriate measure for evaluating safe exposure to nitramines, because nitramines in general were thought to be less potent carcinogens than nitrosamines (ibid).
It is noteworthy that all of the safe exposure figures mentioned in the report had been available for several years. Little in the way of new data were thus brought into the discussion to argue for the application of a different safe exposure level, and no reference was made to on-going research projects under the TCM consortium investigating the likely emission levels of amines and derivative compounds. The decision to refer to a far higher safe exposure level therefore appears to have been based on a revised interpretation of appropriate precaution rather than the availability of new and compelling evidence.

**Strategy revision and political fallout**

To address uncertainties surrounding the potential health risks, the 2011 TCM budget introduced the possibility of testing alternative technologies alongside the amine and chilled ammonia solutions and estimated that this further technology qualification phase would cost 2,400 million NOK (MPE 2010, 2011). In early March 2011 the Ministry of Petroleum and Energy prepared a comprehensive report on Norway’s involvement with full-scale CO₂ capture with specific mention of the TCM. In one section of this report StatOil noted that health risks associated with amines now appeared to be greater than in 2009 when the Master Plan was submitted to the government. The company therefore recommended a three-year conceptual study and maturation phase to test alternative technologies, and to ensure that at least one option would qualify for full-scale capture. This should be followed by a two-year planning and preparation phase before making an investment decision in 2016 (MPE 2011).

StatOil’s suggestion thus ran counter to the company’s evaluations from 2008 and 2009 that only the two pre-selected capture technologies should be matured. The report estimated that the additional qualification phase would increase the 6,500 million NOK cost estimate with an additional 2,900 million NOK (rather than the 2,400 million NOK proposed in the government’s budget). With a total projected cost now at 9,400 million NOK, the cost estimate had
increased nearly eight times since first being announced in 2006. Bellona again voiced its criticism and was reported saying that StatOil was using concerns over nitramines as an excuse to delay its action on CO₂ processing for other reasons (Bjørnestad 2011). The following day the chairman of Bellona Europa said in an press release that “StatOil is stabbing CCS in the back” and that uncertain health risks related to nitramines were an insufficient reason for postponing the investment decision until 2016 (Saether 2011). The view was shared by SINTEF, the largest independent research organisation in Scandinavia, which said that the uncertainty around health risks could be addressed without postponing the investment decision (Røkke 2011).

StatOil responded to the criticism by calling Bellona’s claims “unacceptable”, summarily denied misinforming the Ministry of Petroleum and Energy, and instead referred to the reliance on worst-case scenarios to inform decisions (CCJ 2011b). A few days later Bellona again wrote that developments at Mongstad were being “needlessly postponed”. The group questioned the validity of a test sample used to inform the TCM emission permit application and asked that the results from an amine degradation test referred to in the NIPH evidence basis, should instead be used in the application. Bellona also referred to the KLIF letter from earlier that month, which noted that the scientific basis StatOil had used for dimethylamine emissions was incorrect, according to a press release from Aker Clean Carbon (Aker 2011; CCJ 2011a).

Why StatOil had not considered revising its development strategy at an earlier stage when these same health related uncertainties were already present in the 2009 Master Plan, even in the 2008 impact assessment, was not specified. While StatOil maintained that uncertainties were now greater than previously assumed there appears to be no published material substantiating this claim. In fact, the NIPH had since, in its April 2011 review, assessed that the health risks associated with nitramine and nitrosamine emissions were less grave than previously thought. It is therefore unclear why there was more cause in 2009
for excluding a suite of alternative technologies by use of a narrow selection process than there was in 2011.

This confusion was noted by members of the Parliament’s Energy and Environment Committee in a June 2010 hearing, well before the NIPH assessment was completed. Committee members pointed out that the government had been made aware of uncertainties related to the health risks of amine emissions as early as 2008. They were therefore unsure why such considerations were not taken into account at the time. The Committee also noted that StatOil had used the Master Plan to argue both for the project’s viability in 2009 and the need to delay the project in its 2011 assessment (The Norwegian Parliament 2010). The opposition Christian Democratic Party noted that the Petroleum and Energy Minister had waited until the previous month to inform Parliament that the project would be delayed and would face cost overruns, information that StatOil and Gassnova had already informed him of in 2009 (Sandelson 2010b). Following the hearing the Christian Democratic Party therefore proposed a motion of no-confidence in the Minister that was supported by the other opposition parties (Sandelson 2010a), and the Minister officially resigned in March 2011.

A further twist to the political story followed after the hearing, in December 2010, when the newspaper Aftonbladet obtained documents from the online media organisation Wikileaks, which reported that StatOil representatives had informed US authorities of expected delays at Mongstad two years prior to the hearing. According to these notes, the directors of StatOil chose to “distance themselves from government politicians that have publicly announced the completion date for Mongstad has been set to 2014, dismiss political guidelines, and state the project will only be continued if it makes business sense” (Tancau 2010). Responding to the leaks, the opposition Progress Party’s energy and environmental policy spokesperson said he believed that “StatOil never intended to realise ‘the moonlanding’ ” (ibid).
The NIPH report became the topic of intense debate again in September 2011, when the new Petroleum and Energy Minister Ola Borten Moe announced that he would not be bringing plans for full-scale capture forward because of concerns over health risks. The Environment Minister was quick to announce that these views were not representative of the government and Bellona criticised Moe for ignoring “the facts on the table” as presented in the NIPH report (Bellona 2011).

**Discussion**

These references to continued scientific uncertainty and the controversy over transparency in government indicate that developments at Mongstad have been embroiled in disagreements centering on competing interpretations of scientific uncertainty and the proper relationship between technology and policy. Tjernshaugen and Langhelle (2009) suggest that CCS has arisen over time as a ‘political glue’ to bind different parties together in the Norwegian government. What this chapter suggests based on the Mongstad case, is that scientific claims that are used to support technological development trajectories with policy implications cannot be understood separately from political maneuvering. Following the previous chapter, interpretations of precaution must be understood as valued statements about presumed levels of adequate evidence in the context of public policy objectives. In this light it is not surprising that the NDMA analogue for nitrosamines and nitramines was presented variously as evidence that exposure was safe to the public and that knowledge of safety thresholds remained uncertain.

Bellona’s criticisms were admittedly stated from the position that rapid development of CCS would be favourable to the interests of the Norwegian climate change agenda and would be in line with the original intentions of the TCM consortium. All the while StatOil maintained a position that was arguably more precautionary with respect to evidence on public health risks. StatOil’s concerns also introduced more uncertainty into the future of the TCM project.
with the higher budget projections and longer construction timeline. If these changed views were based on genuine concern with the health risks posed by amine degradation, could an alternative development strategy have led to a greater recognition of scientific uncertainty and indeterminacies at an earlier stage? And what would such an alternative look like?

One suggestion is at hand in the form of a neo-incrementalist approach to decision-making. While some large infrastructure projects, such as the NASA space programme, may need to be developed on a very large scale from the outset (Schulman 1980), neo-incrementalist writers have observed that a process of small-scale trial and error has largely been more successful in developing new technologies, particularly when the supporting science has been subject to significant uncertainties about risks related to the natural environment. The incrementalist approach outlined in the introduction was criticised over the years by scholars in policy science who interpreted Lindblom’s ideas as conservative (because he calls for small steps in development), lacking goal-orientation (because end-goals are revised with information from continuous trials), and opposed to the use of analysis as a basis for decision-making (Howlett and Migone 2011). Such criticism was largely refuted as misinterpretations of the theory by neo-incrementalists who endeavored to apply Lindblom’s formulation beyond policy science to decision-making in science and technology management (Woodhouse and Collingridge 1993). In this later form, incrementalism was re-defined as “a series of evaluative processes aiding decision-making by limiting analysis to familiar policies and to a subset of possible consequences, employing a trial-and-error approach, and tending towards problem remediation rather than positive goal attainment” (Howlett and Migone 2011: 59).

As an example of this application, Collingridge (1980) noted that learning from nuclear reactor construction was slow to take off in the 1960s, as governments chose to build very large reactors, which took several decades to finish and required large advance security payments by operators. Over the same
timeframe several smaller and safer reactors could instead have been constructed, which would have advanced learning for larger projects at a later stage. Other studies of the US Space endeavour (Byerly and Brunner 1989), and military research and development (Collingridge 1990) have observed “that learning is slow and costly when partisans do not press for initial precautions to head off unbearable errors, flexibility to allow error correction, and deliberate preparation for learning from experience” (Woodhouse and Collingridge 1993: 142).

In light of the material on Mongstad discussed in this chapter, what does this literature suggest for the management of scientific uncertainties and indeterminacies? At first glance Norway’s early political commitment to become a world leader in CCS translated into an expensive and complex set of public policy goals. The early decision to pre-select capture technologies at Mongstad was based on only limited experience with similar technologies in different settings – in which significantly fewer amines were emitted. This approach appears to have focused on end-goals driven primarily by upfront analysis, rather than continued input from small-scale trials to identify competing options. As CCS is clearly an emerging technology system there is little well-established information available to inform analysis, casting doubt on the soundness of such an approach. While both the amine and chilled ammonia solutions were known to work in CO₂ capture systems the application to a setting where much greater amounts of amines would be emitted, heightened concerns related to public exposure. Neo-incrementalism would instead suggest that several small-scale trials with a breadth of technologies from the start, should have informed uncertainties arising in implementation, prior to making decisions on the most appropriate technologies.

This approach would not eliminate indeterminacy from arising where capture technology is used in a social context, and re-evaluations of health risks could therefore arise at later times and in other settings. For example, an opinion published by the Scottish Environmental Protection Agency (SEPA) explained
that the 0.3 ng/m³ threshold for total emissions of nitrosamines and nitramines based on the NDMA reference, could not be adopted as a benchmark in the UK because of methodological differences in assessment standards for carcinogenicity compared with Norway (SEPA 2013). Neo-incrementalism in fact acknowledges that all possible uncertainties cannot be delineated upfront to guide technology selection and therefore advocates early testing in many small trials as a means to understand only some of these, and to allow continued learning from step-wise development to influence design decisions.

The government’s announcement in 2006 of a deadline for the investment decision for a full-scale capture plant may likewise be seen as a poor political decision with implications for planning arrangements and political embarrassment. Instead of announcing specific dates and using pre-qualified technologies in an attempt to meet these, planning could have accommodated uncertainty in technology assessment by acknowledging that real-world trials introduce new and unforeseen perspectives. Such an approach would acknowledge that scientific inquiry hardly ever delivers the exact answers that policymakers want to defend their decisions, because the required evidence often is based on very specific studies, with data collected over decades, and often relies on questionable research ethics (such as direct human exposure to toxic samples) (Jasanoff et al. 1990b; van Zwanenberg and Millstone 2005; Collingridge and Reeve 1986).

The strategy employed at Mongstad also missed an opportunity to diversify learning for European CO₂ capture technologies. Rather than attempt to implement an option that was relatively well understood, the TCM could have explicitly acknowledged that CCS development in Europe is coordinated as a regional project with a policy to fund 12 demonstration projects by 2015. And since post-combustion CO₂ capture has been the dominant approach taken by projects that are part of the EU’s Zero Emissions Platform for CCS (ZEP 2011a), the Norwegian approach could have aimed to consider alternative options from the outset and contribute to a wider understanding of uncertainties.
Announcements of firm deadlines are of course common practice in political arenas and politicians often make grand claims that science and technology will be applied to resolve public policy problems, without commenting on the state of the underlying research. The reference to three different safe exposure levels for NDMA in assessment reports from 2008 to 2011 certainly shows how continued scientific uncertainty was used to argue for opposed strategic decisions, in what was from the beginning a highly politicised research project. In his analysis of the political framing of desulphurisation debates during the 1980s, Maarten Hajer has similarly suggested that technology development was intimately connected with political agendas. Coalitions emphasised and argued against specific scientific findings with clear implications for emissions regulation and technology development, partly to support political agendas, but also as a result of different approaches to the interpretation of scientific uncertainty. As Tjernshaugen and Langhelle have suggested, CCS development in Norway has already been embroiled in an overtly political context. However, acknowledging that technology development is to some extent framed by politics and subject to interest-based maneuvering, does not necessarily lead to the conclusion that strategic decision-making is doomed to follow the winds of a dominant political discourse. Perceptions of risk and uncertainty are constructed by a much larger social fabric that accommodates more than just politics, and it is therefore pertinent to ask questions about how to best manage these in light of their implications for wider social and technical processes.

StatOil's decision to change their view on precaution may likewise have been politically motivated or simply based on a genuine concern with health risks. Either way, it illustrates how scientific uncertainty can be used to support widely different positions on precaution with radical implications for RD&D. Neo-incrementalism would suggest that decision-making could have been improved with a more consistent interpretation of uncertainty throughout and reliance on differentiated small-scale trials, as opposed to strict guidance by the precautionary principle. Wildavsky (1988) has suggested that despite attempts
to spread understanding that trials most often involve errors, public policies often pursue decisions as if trial without error were possible. In many cases this involves a strict interpretation of precaution, what Wildavsky calls “no trials without prior guarantees against error” (ibid: 23). The fundamental problem for public policy is that this position implies a paradox: that we could somehow a priori know what amounts as adequate evidence of a guarantee. Allowance for error should instead be granted more openly to thereby contain failure to the outcomes of small trials, and develop practices that have been based on wider experience with alternative options.

In an EU legal context this would carry implications for the interpretation of the principle of proportionality, and in the case of CCS this principle should be concerned with the effects of unmitigated climate change in addition to potential risks arising in technology development. In their book Averting Catastrophe: Strategies for Regulating Risky Technologies, Morone and Woodhouse, suggest: “The ‘How safe?’ questions that have become so much the focus of concern are matters of fine tuning; they may be important in the long run, but they are relatively minor compared to the major risks that still remain unaddressed” (1986: 173). This chapter suggests that while the carcinogenicity of degraded amine emissions may be known to lie somewhere between the thresholds of $10^{-5}$ and $10^{-6}$, certitude is an elusive if not impossible aim in applied science, and decision-makers may therefore do well to contemplate the use of amine emissions for CO$_2$ capture in small trials over relatively short periods of time to accumulate learning, compared with associated long-term concerns. These would include the risks of unabated climate change, CO$_2$ storage in geological formations over long periods of time, and possibly others. What exactly could be of relevance in such a comparison and how to weigh each concern is a question, as in the previous chapter, that should lead to engagement with a wider circle of stakeholders and discussions about the implications of hybrid risk assessment and risk management practices.
Increasing the resources available to projects will not automatically lead to the resolution of indeterminacies as is clear from the case of Mongstad, where the majority of costs have been subsidised by the Norwegian government, and which thereby presents an alternative to the general dearth of public funding for CCS worldwide. Despite this surplus of funds, the government now risks the same political embarrassment that befell the political establishment in the late 1990s when it failed to fit CCS to gas plants. The country’s ‘success story’ with the Sleipner and Snøhvit projects is referred to in much of the support material for CCS globally and it is therefore telling that major delays and cost increases are being faced at a project that was slated to be another ‘first-of-a-kind’.

The resolve to restrict tests to the amine and chilled ammonia solutions from the beginning may have arisen from a poor evidence basis at the time. Crucially, and very relevant to this analysis, the project also appears to have been steered by a rational-comprehensive approach to uncertainty management. In a conference publication the Mongstad developers noted that the technology qualification approach, following guidelines developed by DNV, was premised on an approach of comprehensive upfront risk identification (de Koeijer et al. 2011). In a section covering “failure mode identification and risk ranking”, these guidelines stated: “The objective of this step is to identify all relevant failure modes and threats of concern for the elements defined as new technology in the technology assessment and, for each, judge the associated risks.” And as part of this process “[a]ll potential failure modes or threats are then identified and their respective risks are ranked by assigning a probability class and a consequence class based on previous experience and expert judgments” (Myhrvold et al. 2009: 1531). Assessing risks by quantifying technical parameters upfront is clearly an important component of information gathering, but strict reliance on such information demonstrates a neglect of the implications introduced by indeterminacy (Wynne 1992) and easily simplifies the role of complexities that bear on analysis (Stirling 2008b, 2010). A neo-incrementalist methodology would have focused more on comparing limited upfront information with the outcomes of trial and error testing before deciding
on a development strategy. The DNV guidelines did prioritise small-scale trials as a basis for large-scale build-up, as do traditional approaches to risk identification in chemical engineering, but the adherence to a synoptic ideal of risk assessment is a problematic premise for learning processes in emerging technologies. This agrees with Ishii and Langhelle’s point that decisions for an integrated carbon capture and storage policy “cannot be judged a priori, but it may be achieved over time through successive iterations of policy processes and practices” (Ishii and Langhelle 2011: 359).

The TCM project now serves as an example of the increased scrutiny of scientific evidence and technical feasibility that may follow as more CCS projects are developed. The following chapter considers how wider developments in recent years have let other uncertainties come to light that have altered perceptions of the viability and desirability of CCS.
8. Revisiting the issue-attention cycle: A conceptual model of the political dynamics of decarbonisation

This chapter has been revised and updated from an earlier version published with Simon Shackley in 2012. The chapter considers how support for CCS has faltered in more recent years following changes in political dynamics and feasibility assessments. This dynamic is explained in terms of Downs’ ‘issue-attention cycle’, which is modified with perspectives from STS scholarship to better explain the context of CCS public policy. The chapter ends with an original conceptual model of the cycle informed by theoretical and empirical content discussed throughout the thesis. Data is drawn primarily from policy documents, news articles and a few interviews.

Introduction

CCS has enjoyed something of a meteoric rise in attention within climate and energy-policy circles since about 2003 in selected industrialised countries and some emerging economies. Prior to 2000, CCS was a fairly esoteric subject and received relatively little funding in comparison with alternative, carbon mitigation and energy generation technologies. A variety of indicators can be used to measure the shift in attention since that time, including publications in popular and specialist media, mentions in government speeches, changes in R&D funding levels, as well as new policies and regulatory measures. Such indicators reveal that while attention to CCS on the whole has been rising, key developments since 2008 also show an increasing lack of confidence that the technology system will deliver on mitigation hopes, within the timeframe proposed by policymakers. Supporters of CCS are keenly aware of these changes and industry spokespeople such as Jeff Chapman, Chief Executive of the CCSA, in 2011 noted that the lack of commitment from public policy was eroding the prospects for mitigation:

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CCS has not been advancing as quickly as it needs to. CCS technology is proven, but the policy and incentives framework is uncertain. Without greater certainty, investors, backed by pension funds and global money markets that will finance CCS, (it) will not deliver.

(Chapman 2011a)

This chapter looks at the challenges that have been mounted against the argument that CCS is a viable public policy and the decreasing confidence in its ability to deliver on stated promises. The dynamic is explained with a revised model of Anthony Downs’ issue-attention cycle (Downs 1972) and quantitative evidence from articles published in a specialist media outlet, ENDS, from 2008 to 2011.

**Issue-Attention Cycle**

![Diagram of the Issue-Attention Cycle](image)

Figure 8.1: The Issue-Attention Cycle. Downs (1972) and modified by O’Riordan (2009).
Downs’ issue attention cycle suggests that attention to ‘ecological matters’ over time follows a cyclical pattern (figure 8.1 above; a detailed description is also available in chapter 1) and that invisible environmental problems would be more susceptible to the erosion of attention over time. Climate change arguably falls into a category of environmental problems that are difficult to experience directly and climate change mitigation is one example where public attention has waned significantly since 2009. While some scholars have stressed the emergence of environmental concern as a long-term trend during the last half-century (Inglehart 2008; Mol et al. 2009), others have maintained that the issue-attention cycle is a better analytical construct for understanding such changes. Tim O’Riordan, for example, writes that:

*Downs placed his finger on the political pulse when he noted that reactionary forces would look critically at the mounting political and economic consequences of measures, and would lobby to rein in the emerging but hesitant political enthusiasm. This did, of course, happen in the early 1970s and still occurs every day, even in the modern era.*

(O’Riordan 2009: 312-3)

Consequently, O’Riordan finds little evidence that the environmental movement has had a pervasive influence on the political and economic *status quo*, four decades after Downs’ account. While the model refers primarily to the public at large as the group that bestows or withholds attention in debates, the ‘lay public’ is rarely involved directly in technology governance, but rather participates via political representatives and by lending support to campaigns orchestrated by non-governmental organisations, newspapers, and so on. To assess how beliefs in the prospects of CCS have changed in recent years it is therefore more appropriate to consider the tone and perceptions within a wider governance community. Following the focus in this thesis, the chapter therefore uses Downs’ model to assess wider information flows in the CCS science and technology governance arena where representative interest groups influence policy framings and regulatory responses. The modified cycle thereby considers attention dynamics within a *governance community at large* towards a
technological option aimed at addressing, and itself possibly introducing, environmental problems. Other authors have made similar points by proposing a ‘political’ cycle (Howlett 1997; Howlett et al. 2009), in which attention originates in the political leadership and later catches public attention. They also note the importance of exogenous events such as energy crises, affecting attention and policy outcomes.

Since its publication, Downs’ model has received both praise for its usefulness as a conceptual framework and criticism for its lack of a rigorous empirical basis. Attempts to statistically test for the hypothesised relationship between public attention and government (in)action have found only a weak correlation, whether the time period under consideration was limited to a decade (Peters and Hogwood 1985) or a month (Howlett 1997). The dependent variable in these studies, government change, was defined either loosely as government initiations and changes, or regulatory announcements (e.g. as recorded in the Hansard system in the UK), and purposefully excluded budgetary amendments and publications of policies. While such data limitations may be desirable to test for changes related to established technologies, a mix of media receptivity, public acceptance, cost estimates, budget announcements, policy developments and regulatory changes should be considered for CCS, as it is only now emerging as a technology system. Constraining analysis to regulatory changes and government initiations of e.g. R&D programs would miss much of the relevant activity.

A related criticism of Downs’ analysis is that it implies a static system, with the issue-attention cycle functioning as a political thermostat, preventing any proposed change that would likely have a substantial effect from taking place. Such a functionalist type explanation finds change hard to explain and O’Riordan proposes some sources of change that might move things beyond the confines of figure 8.1, including the possibility that natural systems are near to reaching global tipping points, acknowledgement of the vulnerability of socio-economic systems to such changes, and changing debates on definitions of
welfare. ‘Disruptive technologies’ – technologies that do not fit within the (currently) dominant socio-economic frame of society – are another potential agent of change that can re-structure the prospects for policy innovation.

The rise of CCS in policy arenas

During the mid-2000’s, dramatic decarbonisation, by way of alternative technologies, appeared to be an unstoppable policy, political and economic issue in government legislation, reports, political and planning statements, as well as in the media. Relevant interests seemed to overlap sufficiently and were backed by an emerging scientific agenda and technological assessments, to the extent that CCS appeared to be well on its way as a favoured techno-policy choice in the climate change and energy communities in several countries, and generally within the EU and North America. Some countries were earlier to embrace it than others (see table 8.1 below) perhaps because of the importance of fossil fuel supplies, the salience of climate change as a political issue and/or their technological capabilities for developing a CCS value-chain.

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Table 8.1: Time of political engagement with CCS. Adapted from Langhelle and Meadowcroft (2009) and updated for the period 2008-2012.

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62 Japan does not appear in Langhelle and Meadowcroft’s original list of countries with early engagement.
As chapter 3 argues, coalitions in support of CCS continue to exist and are supported by common ideas about the appropriateness of the technology system in addressing a range of public policy objectives. Yet wider support beyond committed actors in research, technology development and policy arenas can dissolve fairly rapidly as societal circumstances change and interest wanes. There is evidence to suggest that many of the countries in table 8.1 have shifted from ‘alarmed discovery’ to ‘realising the costs’ in the issue-attention cycle, and in some cases even towards a gradual decline in prospects. As an example, projects have failed to materialise, met with opposition, or faced very high budget increases in all of the countries listed.

CCS rose rapidly on the policy-agendas of several countries from the mid-1990s to the late 2000s. The main CCS ‘champion’ in many national contexts from the early to mid-1990s had been the national geological surveys, which identified assessment of CO₂ storage as an important function of, and opportunity for, publicly funded research and advisory organisations. A key document in the EU and UK context was the Joule II report on geological CO₂ storage capacity published in 1996 (Holloway 1997), in which several prominent European national geological surveys participated. Also in 1996, Statoil initiated the Sleipner project, which since inspired a number of commentators and organisations in the field to suggest that large-scale storage would be possible (see chapters 5 and 6). A few other large projects followed suite in the late 1990s to mid-2000s (Weyburn-Midale, In Salah, Snøhvit) along with a larger number of smaller projects (e.g. CRUST K12-B, Netherlands; Mountaineer, US; SECARB, US; Zama, Canada) (Shackley and Gough 2006). Project announcements and updates since became easier to review with institutions such as the IEAGHG and the GCCSI tracking worldwide activities. In the 2000s, CCS became widely evaluated by the energy, environment and finance ministries in a number of European countries (Norway, UK, Netherlands, Netherlands,

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63 The issue-attention cycle describes the gradual decline in attention as a ‘reigning in by power brokers’. This description should not simply be understood as a forced closure of debate by policy actors, but instead occurs following a variety of criticisms. Power brokers may therefore include influential citizens’ groups, regulators, industry interests, policymakers, etc.
Germany, Denmark, etc.). During this period, CCS began to be included in several major economies’ energy and climate change policies (e.g. the 2002 Norwegian Ministry of Petroleum and Energy Report on Gas Technology, and the 2003 UK Energy White Paper). At the Gleneagles G8 summit in 2005, UK Prime Minister Tony Blair placed CCS policy on the international stage and the UK government announced its competition for a CCS demonstration facility in November 2007. During this period the EU also established two dedicated funding mechanisms, the EEPR and the NER300, to support construction of demonstration projects.

De Coninck and Bäckstrand have documented the rise of CCS on the agendas of international organisations and a summary of their findings is shown in Figure 8.2 (de Coninck and Bäckstrand 2011). In only a few years, several international organisations involved in climate change and fossil fuel policies carved out a central position for CCS in their energy technology strategies. As an example, in 2009 the IEA developed its CCS Technology Roadmap with a global ambition of 100 projects by 2020 and over 3,000 projects by 2050 – requiring funding of $4 billion per year for early projects up to 2020, and a total investment of $5.8 trillion by 2050 (IEA 2009).
A review of public sector R&D funding over this period also suggests that CCS was quickly thought of as a serious policy tool for clean energy technologies. IEA data for the US and Norway show the rapid rise in funding from 2004 to 2009 (see figures 8.3, 8.4 and 8.5 below). In the US, much of this funding was channelled into support for new projects under the Federal Government’s FutureGen programme, as well as private-led operations with supplementary federal financing.

Figure 8.4: Share of expenditure on energy R&D in the US as a percentage of total, 2000 to 2009 (IEA 2011b).
Figure 8.5: Expenditure on energy R&D in Norway 2000-2009, at constant 2009 US$ and exchange rates (IEA 2011b).

The rise of CCS advocacy and increased funding over this period cannot be divorced from the equally meteoric rise in media attention devoted to climate change and carbon reduction between the mid-1990s and the COP-15 in 2009. The Princeton University Pacala/Socolow climate stabilisation wedges (2004), the Stern Review on the economics of climate change (Stern 2006), the Garnaut Review on the impacts of climate change in Australia (Garnaut 2008), and the IPCC's Fourth Assessment Report (2007) all brought perspective to the decarbonisation debate by focusing on the required scale of technology deployment. In the 1990s, it was widely believed that the scale of GHG reduction required from 1990 to 2050 to avoid dangerous climate change was on the order of 60% (IPCC 1990; RCEP 2000). Advances in climate modelling, and a more rapid than anticipated growth in global emissions, led to that number increasing in about 2005 to an 80% reduction (in industrialised countries) by 2050. Government policies from the 1990s promoting renewable energy had mixed but, in most countries, limited success and globally there was a realisation that complete reliance on renewable energy technologies for electricity supply would be challenging due to intermittency in flows, energy storage complications, grid limitations and competition for land (IPCC 2007; Giampietro and Mayumi 2009), as well as public opposition e.g. to on-shore wind farms and bioenergy crops (Devine-Wright 2011).
The promise (and criticism) of CCS is that it allows the *status quo* to remain largely untouched – the reliance on large, centralised fossil fuel power plants embedded within an electricity grid. End-of-pipe modifications to existing infrastructure was an attractive alternative to large energy companies as well as many governments, compared with overhauls in transportation networks and the introduction of large-scale renewables and micro-grids, which appeared to require extensive socio-technical and behavioural changes (Unruh and Carrillo-Hermosilla 2006). Faced with an inexorable demand for electricity, a need to replace old power plants and a mixed reaction to renewables and nuclear, CCS began to appear as an attractive alternative, especially to policymakers and politicians who had been accumulating political capital from promulgating carbon reduction targets (Winskel 2012).

**CCS under greater scrutiny**

Focus on CCS has now largely moved beyond early enthusiasm towards cost assessments and towards increased stakeholder scepticism (see Figure 8.6 below). The greater attention to costs has come about as large-scale demonstration projects have moved closer to being realised and regulations have been inscribed into law, raising a plethora of difficult-to-answer questions about design and engineering, planning and logistics, storage reservoirs, legal and insurance issues, regulatory affairs and public acceptability. A greater number of industrial, political and advisory actors have also begun to review CCS in the light of technical viability and budgets. Focus has generally shifted from expensive large demonstration projects towards smaller pilot projects and more accurate information has become available to evaluate technical, economic and socio-political aspects. This new wave of evaluations by a wider set of stakeholders has arguably led to a more sceptical stance towards CCS.
There is also an important difference between decarbonisation options that are themselves electricity generation technologies, such as renewables and nuclear power, and CCS, which is an add-on to existing energy generation technologies. Unlike renewables, nuclear and energy efficiency technologies, CCS stands or falls entirely on its carbon mitigation potential because it adds to the cost of electricity generation with no other benefit than carbon abatement. The main, and increasingly important, exception to this is where CO₂ is used in EOR. In the US, there are 35 years of successful experience with CO₂-EOR using natural sources of CO₂ and approximately 50 million tonnes are used for this purpose annually (Parsons Brinckerhoff 2011). The US industry is prepared to pay up to $38 per tCO₂ (Pollak et al. 2011), well above its price in the EU ETS. While some have therefore considered using CO₂ captured from CCS systems for EOR to incentivise CCS development, it remains unclear whether this has sufficiently
large benefits to cover the cost of capture and transportation from large emission sources (Dooley et al. 2010). Current industrial needs are far less than the amounts envisioned stored under CCS and there are no other commercial markets for large amounts of CO₂ (Parsons Brinckerhoff 2011).

As an investment opportunity, CCS is therefore risky compared to energy generation technologies because it ceases to deliver a financial return when carbon reduction targets are relaxed, and when cheaper or more publicly acceptable decarbonisation options arise. If carbon mitigation does not gain momentum as a more pressing policy objective, renewable energy and nuclear power plants will still be generating power and returning revenue, while energy efficiency will save end-users money. CO₂ capture plants could theoretically be switched off on occasion to reduce costs associated with the energy penalty, but the high capital costs of construction remain. CO₂-EOR, with its additional revenue stream from oil recovery, is therefore a more attractive proposition to investors and it is not surprising that this appears to be the dominant theme behind CCS support in the US rather than climate change mitigation (Pollak et al. 2011). And increasingly, the term ‘carbon capture, utilisation and storage’ (CCUS) has gained a foothold as the preferred economic context for new CCS activities.

This in part is a response to rising cost estimates that are now in the upper ranges, or even above, earlier estimates. In the period 2000-2006, costs of $20-$80 per tCO₂ avoided were widely quoted, e.g. by the IPCC in its 2005 Special Report. Cost estimates at that time were more or less the exclusive preserve of specialists, whether in companies or academia. By 2008 some of these analysts were indicating that early project costs could rise to $90-$100 per tCO₂ avoided, with cautiously worded expressions of uncertainty by stakeholders from industry and consultancies beyond the core CCS arena (Economist 2009a, 2009b). The consultancy firm McKinsey suggested in a November 2008 report that the carbon abatement cost of early demonstration projects would be around $90-$135 per tCO₂ avoided and that these might be reduced to $45–$60
per tCO₂ by 2030 following learning effects⁶⁴ (McKinsey 2008). Some of the more recent cost estimates listed in chapter 2 are now even above this range.

Rising cost estimates have come at the same time that early project failures have begun to occur. The first demonstration project was cancelled in 2007 when the UK government decided not to fund the DF1 Peterhead project and was followed by budget overruns in the US FutureGen 1.0 project in 2008. Questions began to be raised regarding the funding of future demonstrations (GCCSI 2010) and the possibility of developing economically feasible projects within a foreseeable timeframe (Balagopal et al. 2010). The new infrastructure required for large-scale capture also began to be discussed more widely in stakeholder circles, alongside issues of regulation and public acceptability. Continued hostility to the inclusion of CCS projects in the UN’s CDM by Brazil and other nations, and the failure of COP-15 to agree upon a clear pathway for future carbon reduction objectives, hindered the flow of funding to developing nations and the establishment of potential export markets for capture technologies. On the other hand, following COP-16 and COP-17 CCS was formally accepted as a possible mitigation strategy in the CDM.

From 2008 onwards, local public hostility to CO₂ storage projects began to appear in the Netherlands, Germany, Denmark and the US, and caught the industry by surprise. Advocates have largely presented CCS as a boon to the public at large, because it squares high-carbon energy generation with mitigation, and because it introduces new jobs. Public protests, websites and some NGOs however portrayed industry actors as regressive representatives of distrusted multi-nationalists acting in their own commercial self-interest. A main criticism levelled against CCS was that vested interests in the fossil fuel and power generation sectors would point to the prospect of future development to justify the construction of new coal-fired power plants, and never mitigate the associated carbon emissions in the long run. NGOs found themselves divided

⁶⁴ See comments on uncertainties attached to learning effects in chapter 2.
over such concerns, with Greenpeace International taking an overtly critical stance (Greenpeace 2008), WWF UK reluctantly accepting the role of CCS alongside other low-carbon technologies (Allott and Kaszewski 2008), and Bellona continuing its strong support for CCS (Stangeland 2008). The vocal and media-savvy ‘climate camps’ in the UK captured the limelight in 2008 with their protests over new proposals for coal-fired powered plants, such as Kingsnorth (Corry and Riesch 2012). In 2011, Friends of the Earth Scotland issued a statement that was critical of CCS, reversing an earlier more positive stance towards the technology:

> *CCS is still largely theoretical and unproven at any commercial scale, and to spend so much time and effort working towards something that may never actually happen risks creating a significant and dangerous diversion in the short-term from the important aim of harnessing and developing Scotland’s renewable, clean and truly sustainable energy sources.*

(Davidson 2011)

In Germany, local protests led state-level politicians to oppose the federal Government’s plans for demonstration projects in 2010, especially after Greenpeace obtained and published a list of potential storage sites. In summer 2011, the German ‘climate and energy camp’ in Brandenburg devoted itself to a critique of CCS, and in a press release stated that, “the energy companies will be liable for the extremely risky underground CO₂ dumps only for their first 30 years of operation. Afterwards it will be the public that will have to bear the risk of possible damages from these dumps for many centuries” (InfoshopNews 2011).

Rather than seeing CCS as a panacea to the high-carbon techno-industrial complex, a variety of stakeholders thus increasingly portrayed it as an unfavourable public policy objective. The bleak prospects for CCS likewise led industry stakeholders, such RWE Npower’s policy director John McElroy, to pronounce that serious policy debates were dead in the water: ”Certain [EU] member states are no-go areas [for CCS projects]. It’s disappointing we haven’t got to grips with that even though we’ve been discussing it for 10 years” (Carr
The tone of communiqués from the CCSA also conveyed the increasing frustration felt by industry stakeholders as exemplified in this statement from a letter to the UK’s Minister for Energy and Climate Change in June 2011:

[T]he CCSA Board is concerned that industry does not yet see firm Government signals about the long-term viability of a CCS market, particularly when compared to the signals given for nuclear and renewables ....Without clear recognition from Government that CCS will be part of the generation mix, we are concerned that industry’s appetite for continued commitment will shrink. We would urge you to say that roll out (of CCS projects beyond the 4 demonstration projects planned for the UK) should be planned on the basis that Projects 1-4 will be successful and will yield valuable learning through planning, design, build and operation.

(CCSA 2011)

The tone of supportive stakeholders has not changed much since this letter and the absence of enabling policies and funds for demonstration projects has led to voiced frustration in conferences, both in the UK and among EU stakeholders more widely. There has likewise been a growing acknowledgment among supporters that the public at large has not been convinced that the technology system provides wider social benefits:

Discussion of CCS has been seen as an excuse for continued investment in unabated fossil fuel infrastructure. The CCS sector has struggled to set out a positive case that truly aligns with efforts to reduce CO\textsubscript{2} emissions. As a consequence, it has undermined its own credibility. This is not a good starting point for a more positive dialogue.

(Littlecott et al. 2013: 4)

Since 2006 there has been a slow-down in planned projects and many have been cancelled. The following is a list of some noticeable setbacks for CCS in recent years:

• Barendrecht, the Netherlands: a storage project operated by Shell was abandoned in 2010 following public opposition. The Dutch government
thereafter decided that all future storage projects would be conducted off-shore (at greater expense to developers).

- **Germany:** in 2011 Vattenfall faced well-organised local opposition towards a proposed pilot storage project near Jänschwalde in Brandenburg and decided to cancel development. Public protests had previously erupted in Schleswig-Holstein in 2010 in response to the power company RWE’s proposed CO₂ pipeline. Following these concerns, the proposed CCS law took an unprecedented turn for German regulatory culture by permitting individual States to veto implementation of local projects. Political leaders from two States with the greatest estimated storage capacities – Schleswig-Holstein and Lower Saxony – indicated their intention to veto projects.

- **The US:** objections to CCS have emerged from local communities, e.g. in Greenfield, Ohio and Long Beach, California (Bradbury 2012). Projects have also been cancelled including the 2011 Mountaineer project in West Virginia (Wells and Elgin 2011) and the PurGen One project in New Jersey. The Kemper County project in Mississippi, previously thought to be approaching a final investment decision (GCCSI 2013), faces a regulatory review following cost overruns and the departure of top management (Hallerman 2013a).

- **Vedsted, Denmark:** Vattenfall was refused permission to carry out a storage research project in October 2011 following public outcry.

- **Norway:** as described in the previous chapter, the budget attached to the Mongstad project has soared since its announcement in 2006 and an investment decision for full-scale capture has been pushed from 2012 to 2018.

- **The UK:** in 2007, BP and partners cancelled the DF1 Peterhead project in Scotland, citing a lack of government commitment to funding, and moved it to Abu Dhabi, where it was announced in 2011 that the project would be suspended pending a more favourable economic context. The UK government instead decided to award funds through a competition and invited applications for demonstration projects. By 2011 only Scottish
Power’s Longannet project remained, but the government refused to fund it, citing higher than anticipated transportation costs. The competition was thereafter restarted (see chapter 3 for more details).

- EU: by April 2012, 200 million ETS allowances had been sold to raise funds for the NER300 mechanism to support 12 demonstration projects. With the lower than expected price of allowances in the market, allowances were sold for an average price of 8.53 euros per ton, far below the expected 30 euros per ton that had figured prominently in projections when the scheme was introduced in 2009. Both the NER300 and the EEPR funding mechanisms have failed to secure the financial viability of proposed recipient projects (Littlecott et al. 2013).

Downs’ framework suggests that the delays, rising cost estimates, and inadequate funding witnessed in the CCS arena should be expected when great hopes are pinned on an emerging technological solution to solve an environmental problem. Rather than see increasing degrees of roll-out of demonstration projects in the near-term as some have proposed (Gibbins and Chalmers 2008; CCSA 2011), the issue-attention cycle suggests that costs, scrutiny of claims, and technical setbacks are more likely to increase and to delay or cancel actual application. In an interview in late 2012, an industry stakeholder who had spent several years promoting CCS to the UK government summed up the country’s policy landscape and implications for development as follows:

*We saw the Labour Government, and it was Miliband actually who was Secretary of State, adopt a target of four demonstration projects. I mean there was a complete policy around having four demonstration projects in the UK, and a follow-up programme to that, to create a levy on electricity to pay for four demonstration projects. And what we saw from the Conservative opposition at that time, [Charles] Hendry, was, this is all too slow, they are not moving, they are doing a lot of talking, but they are not moving. We’re going to come in and we’re going to move. And when they came in the first thing was, we are sticking to four demonstrations, it’s in the Coalition Agreement. And the initial signs we’re that they were going to move. I think they have been hobbled by the Treasury, this levy*
controlled funding of first renewables and [then] CCS. The Treasury realised, my goodness, we are committing ourselves to all this different sort of spending, which is equivalent to adding cost and therefore similar to taxes. And I think they’ve been holding things back ever since. And they actually have a good argument for not proceeding faster than the rest of the world. It is pointless, us building CCS plants very rapidly and nobody also doing it or, you know, us decarbonising and no one else doing it. So there is a tension between what we were told were the targets, and those targets have driven some very serious policies, but then they haven’t followed through.

(R36)

The news for CCS development is not all cancellations and delays and some plans have moved ahead as scheduled including the Canadian Boundary Dam project in Saskatchewan, which aims to finish construction of a demonstration unit in 2014 (MIT 2013). The UK government also continued its new competition and in March 2013 selected two projects to prepare detailed engineering studies (Hallerman 2013b). However, the projects that have been planned and developed in recent years are arguably smaller and certainly fewer than previously imagined. Rather than assertively being deployed worldwide as a carbon mitigation system, CCS is still in the early stages of technological innovation and demonstration. This delay in meting the aims set out in the early-mid 2000s is not merely explained by inflated hopes and hyped expectations on the part of scientists and technology developers (Bazerman 2006; Borup et al. 2006; Hansson 2012), but also unfulfilled promises of funding and financing shortfalls, inadequate policy frameworks, regulatory uncertainty, poor understanding and engagement with public stakeholders, and problems with strategic project management. These setbacks and the challenges they have posed to expectations of several large-scale CCS plants under official EU policy aims, were acknowledged by the general manager of the IEAGHG in August 2013, who pointed to the incremental learning strategy pursued in the US, as relatively successfully in securing public acceptance (Hallerman 2013c).
Analysis of UK CCS articles in *ENDS*, 2008 to 2011

In keeping with Downs’ hypothesis that scrutiny increases and prospects are revised as evidence on feasibility and impacts accumulates, the perception of CCS to a wider stakeholder community was assessed by reviewing articles from the UK’s leading environmental professionals’ journal, *ENDS*. This journal was selected because of its broad professional readership, and its emphasis on cost assessments, technical discussions, and detailed policy developments. The search covered three years, from February 2008 to February 2011. The titles of each article were scored from between 1 and 5, where 1 represents a very negative perspective on CCS, and 5 a very positive perspective. The contents of each article were also scored on the same 5-point scale. There is inevitably a degree of subjectivity associated with such scoring and to increase consistency scores were therefore assigned individually by two researchers with a selection of articles compared to ensure consistency (intercoder reliability in these cases was close to 100%). In total, 191 articles were scored, increasing from 43 articles in 2008, to 53 in 2009, and 88 in 2010. Results for title and content scores are shown in Figures 8.7 and 8.8, respectively. Overall, these scores show an increased negativity in the tone of reporting over the three-year period, both in the headlines and, more strikingly, in the contents of the articles. While a negative trend emerges there is some degree of variability, which can be put down to the UK’s General Election of May 2010 and to the pronouncement of the Energy Market Reform in 2011.
Figure 8.7: Analysis of titles of articles published in ENDS referring to CCS, February 2008 to February 2011 (5 = very positive view on CCS, and 1 = very negative view on CCS).

Figure 8.8: Analysis of content of articles published in ENDS referring to CCS, February 2008 to February 2011 (5 = very positive view on CCS, and 1 = very negative view on CCS).
Discussion

Downs explained the turn from high expectations and optimism to critical scrutiny and Reigning in of high hopes, in terms of an increasing awareness of costs relative to benefits. This framing can be enriched by considering Collingridge and Reeve’s (C&R) model of under- and over-critical science for policy-making (Collingridge and Reeve 1986), as well as Wynne’s points about risk assessment and social evaluation (1992, 2002). C&R suggest that political consensus on an issue will result in under-critical acceptance of knowledge claims while political dissensus will result in over-critical scrutiny and scepticism. Such scrutiny and scepticism in turn increases as the prospects of higher costs become clearer to operators, funders and wider stakeholders, which leads to probing questions about fundamental knowledge and technical feasibility. During the 1990s and until the mid-2000s, CCS was primarily presented in technical terms and a discussion of its social role and impacts was largely absent from most analysis. This changed as studies and projects emerged that placed analyses more overtly into socially contingent contexts. A variety of non-committed stakeholders brought different perspectives on risk acceptance into the debate (Bradbury 2012; Hammond and Shackley 2010), and indeterminacies arose as technical assessments met with social realities.

The increasing realisation of downstream effects by a wider set of stakeholders brought contending values and technical uncertainties to the fore, challenging claims, opening new ground for questions and fuelling dissensus. Chapter 6 illustrated this dynamic following the prospect of inadequate funding for technology development, which led to a reappraisal of the implications of coupling the CCS Directive with ETS allowances. Chapter 7 showed how early and deterministic decisions on technology development strategies at the Mongstad demonstration project, led to cost increases and reassessments of suitability in the face of poorly understood health implications. Together with the waning support for CCS in more recent years this shows how narrow
assessments and presentations of uncertainties as primarily technical risks, have obscured socially mediated indeterminacies that have dramatically changed the course of developments.

While political consensus in earlier years on the urgent need for decarbonisation supported an under-critical perspective of CCS as a vital ingredient in the mitigation mix, simplistic assessments arguably also stoked the fire of dissensus. In recent years, criticisms of the wider social benefits and costs of CCS have increased and policy mechanisms have fallen short of previous commitments. The costs of mitigation compared with business as usual have also been reassessed in light of a global economic recession, which has restricted the reach of public finances. Such developments call into question the premises that underlie linear conceptions of environmental concern and technology acceptance. Increasing levels of information do not necessarily change public attitudes (Wynne 2006) when culturally distinct understandings are left unaddressed (Douglas and Wildavsky 1982; Hulme 2009). The limited success of the environmental movement (O’Riordan 2009) likewise points to the politically fuelled context of debate about fundamental science in democratic institutions (Hajer 1995), where movements may selectively use and discard claims presented by scientists as they fit with specific moral agendas (Yearley 1992). When official assessments also construct risks as narrowly technical concerns and, implicitly or explicitly, marginalise other voices, disputes over socially contingent interpretations of uncertainties are likely to arise and lead to reassessments of fundamental premises, rather than quiet consensus (Wynne 1992; Stirling 2008b). This points to the instrumental value of wider stakeholder involvement in debates concerned with ambiguous definitions of pollution and sustainable resource utilisation (Yearley 2005), as well as the relevance of participatory processes in influencing environmental technology governance (Stirling 2008a), and the development of scientific assessment tools such as models (Yearley 2006). The CCS debate would have done well and still may benefit from inviting a more nuanced discussion of governance options at earlier stages by revisiting understandings of flexibility
and precaution in scientific regulation, by embracing complexity in technology appraisal, and by inviting more critical stakeholders to influence official appraisals of downstream impacts, rather than being left to protest methodologies and policies as an afterthought.
9. Conclusions

At the start of this research process I was advised that my intentions and questions would be best served by following an iterative methodology. I have since realised that many of the insights I have presented throughout these chapters would not have been possible otherwise. Had I pursued a narrower data collection method – e.g. using ethnmethodology to focus on geophysicists – I would no doubt have had more in-depth and theoretically dense insights to share and bring to bear on SSK scholarship. Conversely, had I opted to carry out an extensive survey of a multitude of stakeholders’ perceptions of risks and uncertainties, I would have been able to statistically analyse beliefs and correlate them with social groups. As it stands, my thesis has been oriented towards a critical engagement with questions about dominant beliefs in expert communities, knowledge production in the science-policy interface, its relevance and implications for technology governance, and the sustainability and suitability of specific policies and regulations. I have been able to address this range of questions and their connection to one another, specifically because I have followed an iterative methodology focused on case studies.

Following this methodology has been challenging and enriching. I have had to immerse myself in a broad range of topics including theories of technology innovation, regulatory mechanisms, science-policy, geoscience research, SSK literature, etc. I began this thesis by looking at what a wide range and a more specific group of experts (geoscientists) believe about risks and uncertainties that bear on CCS, and how the epistemic community of geoscientists researching the geological storage of CO₂ have reached their conclusions. I have considered how this epistemic community has produced and presented their work. I have charted how this body of work has been drawn on in policy circles to address specific questions about CO₂ storage security and safety, and how other research has been used to assess the public health risks of amine emissions from CO₂ capture plant. I have shown how regulatory frameworks are informed by, and in turn condition, research activities to address particular questions that
become pertinent within regulatory science. And I have asked how scientific knowledge and the institutions that produce it, have informed technology policy and shaped the governance of CCS in the face of uncertainty and indeterminacy. A conceptual model was formulated at the end of the previous chapter to account for the role of such factors in the development of emerging environmental technologies.

Those with a strong realist bent may refute the presentation of technical risks in this thesis as socially constructed phenomena that can be deconstructed or revised when social circumstances change. It is however hard to disagree with the statement that waning support for CCS in more recent years has resulted from a variety of setbacks, rather than simply a dearth of financing. Organised public opposition has resisted projects for a variety of reasons; costs have been seriously underestimated; prospective operators have not found a business case that would make storage liabilities feasible; technology development strategies have been poorly designed; and public financing programmes have been inadequate. I do not claim that my thesis presents the definitive account of CCS development and governance over the past few years. But considering the material that I have discussed, one clear lesson that arises is that attempts to account for risks through narrowly focused scientific assessments, have been challenged and undermined when encountered by external social commitments. To demonstrate this, I have deconstructed definitions of risk and the implications of uncertainties and indeterminacies to risk assessments and risk management strategies. The interview findings in chapter 3 identified an epistemic tension on the part of a range of expert stakeholders, between a deductive certitude that large-scale CCS would be developed within the coming decade and function as planned, and inductive uncertainty around several scientific, technology and policy issues. When interviews for that chapter were carried out in 2009, there was arguably considerably more optimism amongst CCS promoters than there is today. However, even at that time worries were voiced over the potential for cost reductions and the absence of a strategic financial framework in the UK and across Europe. Such uncertainties have not
been alleviated in recent years, but instead increased, despite more technical and financial analyses being produced. Such studies have had to discount or suspend a host of variables about public support, regulatory mechanisms and financing, to continue to present CCS as a viable public policy for climate change mitigation.

Where the rhetoric of technical risk has dominated in debates, acknowledgement of uncertainty and indeterminacy is therefore desirable to promote a more honest assessment of prospects for development and deployment. Assessments based largely on expert judgments of complexities often presume that management options can be harnessed without assessing their wider social implications. Scientific analyses that are used to underpin particular policy options thereby implicitly support and construct a political sentiment towards environmental pollutants and technologies. As I discuss in chapter 4, the research conducted by the epistemic community of geoscientists working with CCS, is not only conditioned by the tools at their disposal, but also their background training and what they consider to be analogous data. When results have been limited by data accessibility and largely constrained to a few highly uncertain types of measurements, rather than present scientific assessments as 'objective facts', the role of subjectivity should be emphasised in official assessments. Acknowledging that geoscientists are also driven by common emotional commitments to climate change mitigation and that ethical positions are often referred to as reasons for being involved with CCS research and championing its development, should be seen as an opportunity to ask more probing questions about whether this compromises their analyses and leads to appropriate recommendations for policy.

While geoscientists have noted a number of technical uncertainties in publications that quantify CO₂ storage and leakage, the epistemic community has also been noticeably successful in presenting the risks as known, comparatively low and manageable. The community's intellectual and ethical common ground thus poses a challenge to alternative configurations and beliefs
by presenting CO$_2$ storage as generally safe and desirable to pursue in many contexts. This particular case therefore invites a revision to the theory of epistemic communities, namely by pointing out how common normative and principled beliefs, causal beliefs, validity standards, and a common policy enterprise may implicitly sanitise multivalency from debate. This brings to mind the substantive thesis presented in chapter 3, 'conditional inevitability'. Geoscientists clearly reflect deeply on the ways that analytical conventions and data might yield alternative conclusions, but the community has ultimately evinced a high degree of conformity in beliefs about the future state of injected CO$_2$. Just as beliefs about the necessity of climate change mitigation, renewable technologies and the availability of components from the oil and gas sector shore up beliefs about the future of CCS, argumentative patterns among geoscientists reveal an oxymoronic form of reasoning. Scientists interviewed for this research acknowledge the many assumptions that underlie modelling efforts and the scant availability of direct empirical measurements at key sites such as Sleipner. However, theoretically informed notions of dynamics in the deep geology hold sway when it comes to understanding long-term stability – analytical matters for which indeterminacy and ignorance inevitably creep in. Despite major uncertainty in the emerging science and lack of clarity in the directions of policies worldwide, beliefs about the prospects and merits of CCS are well established. This commonality in findings informed by interpretive flexibility and Haas’ notion of epistemic communities is noteworthy, because it underscores a willingness among stakeholders to have confirmatory conclusions triumph in the face of major uncertainties and evidence to the contrary.

This is somewhat surprising considering the variety of disciplinary backgrounds, even within the geosciences, that have converged on CO$_2$ storage research. It is also surprising considering the scant availability of direct measurements that are relied on to support model results. Compared with climate modelling – now arguably also primarily conducted in the interest of assessing risk management options for climate change mitigation policy – the
field of CO₂ storage relies on comparatively small datasets and simple models. Geoscientists have presented this work in relation to a range of knowledge analogues and reference points in order to construct quantitative risk assessments that have been informed by deductions from theory to quantify a few key parameters, including the permeability of different rock strata, fluid migration rates and long-term stability. While the process of bootstrapping and the conditional nature of knowledge production may be well understood by insiders who are prompted to reflect on their reasoning, it is not an aspect of scientific research that is voiced to the public at large.

Nor has this feature of the knowledge production process been adequately reflected on when research about CCS, whether CO₂ capture or storage, has been presented in policy and regulatory fora. Here the rhetoric of risk has even more clearly reflected a concern to simplify messages about the desirability of pursuing a policy agenda with the potential to support widespread implementation in the coming decades. Techno-economic analyses by international policy groups such as the IEA have taken the technical viability of CCS systems for granted and oftentimes presented a highly optimistic picture of cost reduction trajectories. Similar to geoscience research about CO₂ storage, this begs the question of the suitability of assumptions that have supported proposals for development trajectories and so-called ‘roadmaps’. Such prospective analyses are contingent on existing policies, regulatory mechanisms, the availability and pricing of fossil fuels, as well as the overall economic climate, and should therefore be viewed as narratives in support of particular social configurations. While it may be obvious that heroic assumptions often are made to support assessments of policies and regulations, the technical viability of CCS is regularly simply presented as a fact that arises from solid science and similar enough experiences in the hydrocarbon industry. The stamp of preordained yet highly conditional reasoning is thus a common argumentative thread to the many stakeholder agendas and milieus that come together to buoy CCS as a scientific, technical, policy, economic and social potentiality.
To a degree this reasoning has coevolved and framings of technical evidence in regulations and policies have impacted the activities of scientists by conditioning their work as responses to specific concerns. While geoscientists were clearly involved in framing basic questions of the risk and research debate in the early days of CO₂ storage, in later years – in an EU context particularly since the publication of the CCS Directive in 2009 – much of their work has been in response to demands for leakage quantification and the development of methodologies that combine several monitoring technologies. Research aimed at assessing the safety and security of leakage will hopefully become increasingly robust as more data from a range of empirical activities is assembled and modelled. Variables that are based on very long-term dynamics may however remain unquantifiable or too complex to model accurately. The question of how models of CO₂ storage present risk in nature will therefore continue to be intimately tied to data collection methods and analytical conventions.

Epistemological boundaries in these models are often drafted for the practical purpose of limiting variable inputs and results to those that are thought to most dramatically affect storage dynamics and leakage potentialities. Instrumentation has likewise supported the creation of specific and limited kinds of information and defined how data is adapted to a modelling environment. Such analytical choices emanate from specific ways of investigating and knowing the natural world, but they also condition the knowledge available to regulators and a wider audience interested in assessing the risks of CO₂ storage. One example is seismic data. When geoscientists have pointed to the virtues of seismic imaging as a monitoring tool based on sites such as Sleipner, it is rarely explicit that the data normally are compared with pressure and temperature measurements from wells used for very different purposes, namely hydrocarbon extraction activities. In spite of this, the resulting seismic visual data have been pointed to as evidence that storage can
be undertaken in a manner that allows a detailed account of injected volumes and migration patterns.

Time-lapse seismic imaging has largely been seen as a successful means of communicating the safety of CO$_2$ storage to a wider non-specialist public. Where theoretically driven reasoning may be convincing to specialists who can draw on a research experience with geological formations and fluid flow, visual demonstrations are particularly visceral proof to bring to a general public. However, the pedagogical success of such images follows the explanations that geoscientists tell about their distinctiveness and value. Behind their apparent simplicity lies a much more complex narrative about instrumentation, data acquisition, analysis, and interpretation. Geoscientists might argue that it is too difficult or complex to explain such processes to a non-specialist public and that in any case what matters are experts’ judgments. The same can be argued for diagrams that depict the long-term fate of injected CO$_2$ becoming increasingly stable and securely stored over time. The counterpoint is that scientists should be wary of how far they step into communication, if that activity leads them to provide simplified accounts that gloss over uncertainty and ignorance in favour of simplistic narratives of risk.

This is perhaps even more important to recognise when technical accounts involve implicit assumptions about indeterminacies, including failure management options and the desirability of climate change mitigation to a wider section of society. Wynne’s account of Cumbrian sheep farmers’ scepticism towards official risk assessment methodologies suggests that a reflexive account of analytical frameworks is not only an ethical responsibility. It may likewise serve an instrumental purpose in ensuring that assessments are accurate and that better data is gathered. In the Cumbria case, assumptions about radioactive contamination and exposure routes involved economic consequences for the local farming population as well as potential consequences to the health of consumers who ate the slaughtered sheep. Acknowledging that different populations may hold contrasting values about
the appropriateness of precautionary measures to risk management – and indeed contrasting notions of what constitutes precaution in a given case – should likewise lead scientists who produce research about CCS to think critically about political and policy values embedded in and emanating from their work, what these imply about risk acceptability, and what the resulting research may therefore imply about social welfare and management options.

Social processes can thus be seen as constitutive elements of technical reasoning across a number of points of evaluation. Geoscientists’ reasoning about evidence and uncertainty is to a considerable extent determined by agreement on acceptable methodological practices including seismic data collection and what may reasonably be observed from such data, as well as less evident consensus processes about the social and ethical value of being engaged with their research field. The work of defining science and technology policies may on the other hand be more widely accepted as an inherently political process. As is evident from recent years’ fluctuating levels of support for CCS, political processes variously emphasise technical knowledge and research in support of dominant interests and aims without charting consistent courses for technology development. Along the way, emerging regulations have been thrown into the mix and reshaped understandings of relevant information by introducing acceptance thresholds on leakage (the CCS Directive) and emissions of amines (Norwegian regulators). Studies that take science and risk as socially constructed phenomena thereby acknowledge that a variety of social processes lead actors to form and use data; data that in turn reconfigures perspectives on relevant information for official purposes. While it may be possible to provide quantitative information using models, such outputs often involve tacit judgments about relevant parameters, adequate precision, possible extrapolations and measures of uncertainty. When regulations move the incidence of an evidence burden further upstream by demanding technical performance measures, scientists are therefore (implicitly) asked to provide socially mediated thresholds for knowledge production to meet external standards. Regulatory strategies thereby give shape to the production of
scientific evidence by redefining relevant elements for data collection, how to bound methodologies, and how to use results to inform further research activities.

Regulatory science can thereby lead to expectations that are incommensurate with the ways that scientists conceptualise uncertainties, as well as governance strategies that close down questions about wider stakeholder perceptions of risk, rather than explore their implications for deployment and management strategies. To a large extent such tensions may be alleviated with a greater allowance for complexity in analyses and error in project planning and regulation. Embracing complexity in scientific analyses, by drawing the social dimensions of risk assessment more distinctly into discussions about technical parameters, would encourage debate about the potential for indeterminacies to shift conclusions outside the bounds of orderly and easily modelled dynamics, and into spaces that involve undefined uncertainties. Increasing public involvement in scientific knowledge production may lead to a renewed questioning of fundamental premises, but it may likewise invite debate about predetermined ethical and social commitments before they escalate into controversies. An alternative is to encourage targeted involvement of stakeholders who have expressed a specific interest or curiosity around a scientific or technical issue to become more closely involved in critiquing assessments.

This would include questioning the fundamental drivers behind technology development. Recent years have shown that the dominant aim attached to CCS – the mitigation of anthropogenic climate change – has met with criticism and redefinitions. And it has become difficult to argue for CCS without explicitly mentioning revenue generation in the same sentence through enhanced hydrocarbon recovery. At the same time, countries such as the UK have decided to continue to construct gas-fired power plants to support base-load energy generation, rather than coal and nuclear plants. Flexibility has thus become championed over energy security concerns with imported gas; climate change
mitigation no longer carries the political cache that it did a handful of years ago; costly government-led innovation has become difficult to justify in a harsh economic climate; and widespread scepticism and opposition has coalesced against large-scale energy sector technologies, in some cases specifically against CCS. All of this has meant that CCS in most parts of the world has been put on the backburner until further notice and may very well face a period of dormancy similar to that of the 1990s and early 2000s. Policymakers should be mindful of these challenges, examine how they have changed the landscape for development and deployment, and ask how they should be addressed over the long term if CCS is to become a widespread public policy option.

Providing fertile soil for heightened expectations may be politically desirable when running on a platform of sustainable development that emphasises continued economic growth. There was certainly a pronounced pro-environmental quality to the rhetoric of political victors in the US, the UK and Norway in the mid- to late-2000s when climate change appeared to be a top policy priority. Such hype can however easily foster disconnects between policy platforms and research findings by politicising environmental policy and regulatory measures. A stark example of this was witnessed in 2009 when heads of state met at the COP-15 negotiations in Copenhagen to set new terms for climate change mitigation in the post-Kyoto Protocol era. The disappointing resolutions that came out of Copenhagen were subsequently blamed on the marginalisation of regular negotiators in favour of a highly politicised debate. CCS supporters have similarly framed the technology system in political rhetoric by suggesting a number of attractive associated welfare aims that might accrue to adopters. Scientists have found themselves in a position as technology advocates arguing for the worth of CCS beyond its role as a research question, and the few available demonstration projects have become key elements in proselytising sceptics. This has meant that more technical arguments in favour of promoting development have been up for review from a wider public and necessitated a rhetorical defence by advocates that addresses
not only the quality of scientific and technical assessments, but also the social value of mitigation.

Discourse about CCS has moreover been considerably connected with concerns about the suitability of different energy generation mixes and ownership structures. Various stakeholders have partaken in such discussions by presenting technical assessments to support their own value-based positions about local renewable energy projects, centralised nuclear reactors, alternative fossil fuels (such as hydraulic fracturing for shale gas), energy efficiency measures and policies for behavioural change. Politics and values have thereby taken centre stage in discussions about CCS not only in relation to the desirability of climate change mitigation, but also in reference to energy futures. Advocates, including members of the geoscience epistemic community, have likewise argued for CCS on the basis that it is a crucial part of sustainable energy generation. This normative basis should be more apparent when research is used in policy fora to promote mechanisms that have widespread and long-term implications for energy planning. From a social constructivist perspective, the concentration of research scientists who believe in the inherent good of their involvement within the field is problematic, because value-laden assumptions influence how the significance of uncertainties is interpreted and how information is communicated to a non-specialist public. As interviews and documents highlighted in this thesis have shown, CCS is often portrayed in a deterministic light as if it were a given that cost reductions will occur, technical uncertainties will be resolved, and the public at large will support widespread development. The emphasis in communications of seismic images to a wider public is thereby only one area where a more critical examination of how specialists communicate their work is warranted for ethical reasons. This includes the assumption that oil and gas fields will be best suited for near-future CO₂ injection; the support for post-combustion technology as the primary capture option in developed countries; the question of whether coal and gas should both be prioritised for capture installations; how analytical insights from very long-term (>1,000 years) physicochemical reactions should be weighed
when deciding the suitability of storage sites; and crucially ‘how safe’ injection sites should be, whatever this may mean to different stakeholders. Engaging specialists in all of these research areas with members of the wider public, to foster discussion about implied values within assessments, may lead to more open acknowledgement of uncertainties and indeterminacies, as well as less simplistic presentations of research and more reflection on social commitments. Reflecting on the thesis of conditional inevitability presented in chapter 3, scrutiny of such beliefs by questioning the assumptions that support them may help develop more socially sustainable and robust decisions.

Much of the criticism of CCS by NGOs and publics has mostly focused on assumptions within political circles about desirable energy futures, such as carbon lock-in. Following Cartwright’s challenge to the evidence we bring to bear on observations of regularly occurring phenomena in science, can we similarly draw some implications about the regulation of emerging scientific practices and imagine a regulatory language that is sensitive towards the difficulty of providing evidence for repeatable law-like regularities? I think that we can. Rhetoric along these lines is present in legal principles that allow a degree of flexibility in interpretations of critical thresholds and safety limits of regulatory science. As Jasanoff and other STS scholars have pointed out in multiple case studies, such questions are necessarily focused on negotiated quantities and value-laden assessments of how to weigh unknown variables. Geoscientists cannot say what should constitute a reasonable course of action in the event of detectable CO₂ leakage from a geological store, without invoking opinions about the value of uncertainty to society as a whole. Similarly, prescriptive formulations about migration risks evidenced by seismic imaging will be subject to expert judgments about likely fluid flow patterns, the presence of conduits such as fractures, and critical levels of pressurisation. Components of the Guidance Documents to the CCS Directive allow for context specific interpretations of uncertainty, precaution and acceptable evidence. Leakage thresholds and liability provisions are however treated as absolute categories in the Directive. Perhaps the dominant interpretation of the precautionary
principle under EU law, where risk assessment is separated from risk management practices, presents a more fundamental challenge to regulatory science aimed at environmental risk mitigation. There is widespread agreement among geoscientists that proper storage conditions may still involve some leakage, albeit low levels, and policymakers would do well to ask how this point of consensus should be weighed not only in risk assessments, but also in management practices. Geoscientists have of course been part of these negotiations and I am not suggesting that their voices did not influence regulatory formulations. Indeed, many explained in interviews that the Directive largely provided an acceptable basis from which to work and emphasised the allowances for flexibility to use a range of detection technologies. The separation of risk assessment and risk management strategies implied by the wording in the Directive, does however raise cause for concern about the admission of uncertainty and indeterminacy in technical assessments. If regulations demand precise conclusions from geoscientists about leakage risk, what hope is there for a more nuanced discussion of how values and commitments influence technical assessments and communication to a non-specialist public?

Similarly, as I discuss in chapter 7, technology development that is based on scientific assessments must allow for a degree of uncertainty to be successful. Here I pick up on a point presented in the conclusions to a recent publication (Markusson, Shackley and Evar 2012), which introduced the idea of ‘lumpy incrementalism’. This term arises from three assumptions: that development trajectories for CCS should not be too narrowly defined – which may lead to the undue pre-selection of technologies at an early stage; that the scale of capital investments likely will remain relatively large or lumpy; and that the scale of efforts devoted to development should follow what has been learnt in technology R&D rather than the whims of political trends. There is a possible tension between the call for flexibility on the one hand and the admission that relatively large capital investments are required. A workable technology policy would therefore seek to draw support from a wider stakeholder network,
openly discuss how the many goals attached to CCS could or should be reached, and aim to coordinate development activities along with other countries. This would also require simultaneous exploration of several different configurations for technology components.

A process of lumpy incrementalism as defined here, would take into account policymakers’ priorities for evidence, but would aim to foster a debate about the aims and values involved in risk assessments for regulatory science. This could involve presenting a range of contrasting conclusions based on different interpretations of uncertainty attached to variables. It could also involve scientists communicating this multiplicity of risk assessment scenarios to policymakers and wider stakeholders rather than seeking to present few and bounded studies all pointing to the same conclusions. Finally, it could foster regulatory wording that did not advocate evidence of zero-risk scenarios, but instead acknowledged that scientists’ training more likely will condition them to believe that leakage likely is going to take place, whether this is detected or not.

A process of lumpy incrementalism would also include some suggestions for the geoscience research agenda where the choice of geological storage options should be revisited in the light of aims to generate a more robust understanding of a range of potential scenarios. Evidence from the Utsira formation has been said to show that injected CO₂ appears to be relatively safely stored under ideal settings. However, there is much more detailed data available on geological features and fluid flow patterns from oil and gas fields. Expert stakeholders from research and industry therefore agree that such sites are better suited for research activities because they can supply richer data. While storage in saline aquifers will be necessary to meet long-term projections for CCS, such as those presented by the IEA, research activities aimed at gathering more detailed knowledge from sites with geological characteristics that are theoretically less well suited to permanent containment than Utsira, may help to better address questions about failure scenarios and remediation. Such work would in turn guide understandings of best available conditions and help to frame research
activities as less concerned with delivering positive results and more focused on uncovering surprising findings.

As for technology policy, lumpy incrementalism would emphasise that some forms of failure are endemic to all R&D efforts and that differentiated trial and error testing therefore should be emphasised. Rather than pre-selecting technologies that have not been tested in the relevant technical and social environments, policymakers should instead direct capital towards a multiplicity of efforts. Crucially, such efforts should seek to assess the suitability of build-up procedures in light of socially mediated indeterminacies. Involving a wider segment of the general public in discussions about risk acceptance during early stages of research and development practices would be helpful in this regard.

We should question what R&D efforts in CCS to date have aimed to deliver, from a scientific perspective arguably interested in falsification, and a public policy perspective that should arguably be focused on increasing the robustness and transparency of information used to guide assessments for regulatory science and technology policy. A policy of lumpy incrementalism would generally emphasise a broad set of research activities rather than narrow and predetermined ones. Having decided that CCS is a public policy option that may have to be pursued does not mean that all evidence should indicate that activities would be risk free. Rather, risks and uncertainties associated with CCS should be compared with a range of other policy options and invite the views of several stakeholders to help decide whether widespread deployment is a worthwhile aim. While European CCS insiders may lament the slow progress in recent years, the increasing emphasis on smaller pilot plants has the potential to focus research on more configurations for technology components and geological settings and advance learning for a variety of options. We need only look to past years’ emphasis on CO₂ capture as an add-on primarily to coal-fired power plants to see why more variety in research efforts would be desirable: following finds of unconventional gas in the US and elsewhere and the continuing importance of flexible gas plants in European power generation, CCS
is now increasingly being framed as CO₂ mitigation option for gas-fired power plants. The continued critical review of supposedly fundamental assumptions implied by this innovation strategy acknowledges that energy policy ought to be reflexively positioned towards market-driven changes and notions of inevitability.

As an example, and as I argue in chapter 6, the idea for a carbon market was originally proposed in the EU to allow emitters to invest in least-cost mitigation. It is now however widely recognised that the price of allowances has been too low to incentivise the desired level of mitigation and downgraded previously rosy prospects for CCS deployment. The tie between the CCS and ETS Directives has furthermore resulted in an unaccountable financial risk for prospective operators rather than a cash flow to support projects. Two fundamental premises for financing CCS thereby changed dramatically over a short period of time, redrawing the technology innovation landscape. If governance strategies for CCS in part aim to promote development of multiple research activities, and to test component configurations, then costs from a surrender of allowances could be reduced by allowing probability calculations of liabilities, letting operators find mitigation projects at lower prices outside of the EU ETS, or by settling the future price of any surrendered allowances as a weighted average of prices over the time of injection. The latter option would allow a precise calculation of storage liabilities in balance sheets.

There exist no predetermined means of assuring that emerging technologies will succeed as accepted policy options and lead the way towards functional dominant designs. Reflexive innovation processes will however invite a review of aims and shine a light on assumptions in analytical work. This is markedly different from a call towards more detailed analysis. Rather, lumpy incrementalism implies a review process of fundamental biases, socially constructed facts and their implications for development. This redefines rigour in scientific and policy work, placing the emphasis on process over analysis in order to ask whether hidden or unspoken assumptions underlie conclusions. As
an innovation strategy, lumpy incrementalism would thereby help define the conditional underlying the inevitable in technology development. This also implies a more inclusive decision making process in which potential designs undergo early-stage review by more stakeholders in order to address potential sources of dissent and socio-technical shortcomings.

The specific suggestions outlined here that emerge from the process of lumpy incrementalism would aim to counteract a tendency to pigeonhole the consequences of emerging technologies within public policy debates as technically determined outcomes, by opening debate up to a variety of stakeholder voices, emphasising flexibility in regulatory science and technology policy and inviting a reflexive account of articulated and assumed innovation trajectories. It remains for future work to assess whether this could be implemented as a workable strategy within technology innovation.
Annex 1. Interview participants

I wish to thank the following 60 individuals for their participation in anonymous interviews throughout 2009-2013. Listing is alphabetical by surname. Numbers in front do not correspond to those assigned to quotations throughout the thesis.

1. Dr. Maxine Akhurst, British Geological Survey
2. Jason Anderson, WWF International
3. Dr. Dan Arnold, Heriot Watt University
4. Prof. Dr. Ir. Rob Arts, Nederlandse Organisatie voor Toegepast Natuurwetenschappelijk Onderzoek (TNO)
5. John Barry, Shell
6. Antony Benham, British Geological Survey
7. Dr. Andy Chadwick, British Geological Survey
8. Dr. Hannah Chalmers, Carbon Capture and Storage Association
9. Prof. Jeff Chapman, Carbon Capture and Storage Association
10. Ananth Chikkatur, PhD, ICF International
11. Dr. Chris Clarke, University College London
12. Kirsty Clough, WWF UK
13. Jose Condor, PhD, IPAC-CO2
14. Prof. Andrew Curtis, University of Edinburgh
15. Prof. Richard Davies, Durham University
16. Dr. Vasily Demyanov, Heriot Watt University
17. Jean Desroches, PhD, Schlumberger Carbon Services
18. Tim Dixon, IEAGHG
19. Dr. Mike Farley, Industrial and Power Association
20. Prof. Quentin Fisher, University of Leeds
21. Prof. Jon Gluyas, Durham University
23. Dr. Heleen Groenenberg, Ecofys
24. Prof. Stuart Haszeldine, Scottish Carbon Capture and Storage
25. Ben Hedley, Durham University
27. Prof. Richard Hobbs, Durham University
28. Dr. Sam Holloway, British Geological Survey
29. Richard Hotchkiss, RWE nPower
30. Dr. Dave Jones, British Geological Survey
31. Matteo Loizzo, Schlumberger Carbon Services
32. Dr. Jim Lorsong, 2Co Energy Limited
33. Prof. Colin Macbeth, Herriot Watt University
34. Dr. Chris Mansfield, Shell
35. Dr. Ondrej Masek, University of Edinburgh
36. Guy Mason, BP Alternative Energy
37. Duncan McLaren, Friends of the Earth Scotland
38. Dr. Dimitri Mignard, University of Edinburgh
40. Dr. Grant Nicoll, Scottish Carbon Capture and Storage
41. Curt Oldenburg, PhD, Lawrence Berkeley National Laboratory
42. Dr. Doug Parr, Greenpeace UK
43. Dr. Jonathan Pearce, British Geological Survey
44. Ian Phillips, CO2DeepStore
45. Dan Pike, UK Department of Energy and Climate Change
46. Dr. Debbie Polson, University of Edinburgh
47. Dr. Nick Riley, British Geological Survey
48. Dr. Graham Russell, University of Edinburgh
49. David Rutland, UK Department of Energy and Climate Change
50. Prof. Stefaan Simons, University College London
51. Dr. Aage Stangeland, Bellona Foundation
52. Dr. Karl Stephen, Heriot Watt University
53. Fran Watson, Durham University
54. Rosemary Whitbread, UK Health and Safety Executive
55. Ton Wildenborg, Nederlandse Organisatie voor Toegepast Natuurwetenschappelijk Onderzoek (TNO)
56. Dr. Mark Wilkinson, Scottish Carbon Capture and Storage
57. Iain Wright, BP
58. Prof. Ian Wright, National Oceanography Centre
59. Dr. Heleen de Coninck, Radboud University Nijmegen
60. Chris Davies, European Parliament
Annex 2. Chapter 3 survey questions and statistical results

Below is the survey used to gauge 19 respondents’ perceptions of the uncertainty surrounding CCS in the UK. The parenthetical legend before each issue denotes its use in gauging technology (t) or policy (p) uncertainty.

Please rate the uncertainty for each issue on an ordinal scale from 1 (lowest) to 4 (highest):
(t) Application of capture technologies to large-scale CCS projects
(t) Reliable transportation options for CO₂
(t) CO₂ behaviour in the subsurface
(t) Efficiency penalties for CCS demonstration projects
(t) Permanent storage of CO₂ and risk of leakage
(t/p) Scale-up potential of CCS demonstrations to large-scale projects
(p) Form of CCS regulations in the UK
(p) Ownership structure of the CCS chain
(p) Means of procuring adequate financing for large-scale projects in the UK
(p) Risk of lock-in to inferior CCS technologies and policies
(p) Public acceptance of CCS projects in the UK

Survey summary

Sum uncertainty: The sum of points for all individuals in each of the three stakeholders groups.

Uncertainty by experts: The mean value in each of the three stakeholder groups.

% points of maximum: Ratio of the mean values to theoretical total for each of the three stakeholder groups.

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<th>Uncertainty by Experts</th>
<th>% Points of Maximum</th>
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<tr>
<td>Sum Uncertainty</td>
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<td>64</td>
<td>135</td>
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</table>
### Statistical results

#### Eta squared correlation test

**Experts / Technology Uncertainty Crosstabulation**

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<thead>
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<th></th>
<th>Technology Uncertainty</th>
<th>Total</th>
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</tr>
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<td>Users</td>
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<tr>
<td>Outsiders</td>
<td>0 1 1 0 1 1 1 0 1 1 1 1 8</td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td>1 2 2 3 3 1 2 1 1 1 1 1 18</td>
<td></td>
</tr>
</tbody>
</table>

#### Directional Measures

<table>
<thead>
<tr>
<th>Nominal by Interval</th>
<th>Eta</th>
<th>Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Expert Dependent</td>
<td></td>
<td>.798</td>
</tr>
<tr>
<td>Technology Uncertainty Dependent</td>
<td></td>
<td>.409</td>
</tr>
</tbody>
</table>

#### Kruskal-Wallis test

<table>
<thead>
<tr>
<th>Ranks</th>
<th>Stakeholder</th>
<th>N</th>
<th>K-W Mean Rank</th>
</tr>
</thead>
<tbody>
<tr>
<td>Technology Uncertainty</td>
<td>Developer</td>
<td>6</td>
<td>8.58</td>
</tr>
<tr>
<td></td>
<td>User</td>
<td>4</td>
<td>6.75</td>
</tr>
<tr>
<td></td>
<td>Outsider</td>
<td>8</td>
<td>11.56</td>
</tr>
<tr>
<td></td>
<td>Total</td>
<td>18</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Test Statistics (a,b)</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Technology Uncertainty</td>
<td></td>
</tr>
<tr>
<td>Chi-Square</td>
<td>2.460</td>
</tr>
<tr>
<td>df</td>
<td>2</td>
</tr>
<tr>
<td>Asymp. Sig.</td>
<td>.292</td>
</tr>
</tbody>
</table>

a Kruskal Wallis Test

b Grouping Variable: Stk
Annex 3. Sleipner core-set search terms in Web of Knowledge

Extracted from Web of Knowledge on September 17, 2012

Search terms
Topic=("CCS" OR "CO2 sequestration" OR "CO2 storage" OR "carbon sequestration"
OR "carbon storage" OR "geological sequestration" OR "geosequestration" OR
"carbon capture and storage")


Databases=ALL

Lemmatization=On

Results: 14,065

Limitation of results
Refined by: Research Areas= ( GEOCHEMISTRY GEOPHYSICS OR GEOLOGY OR
PHYSICAL GEOGRAPHY OR GEOGRAPHY )

Results: 1,363

Search within results for research related to the Sleipner project
Topic=("sleipner" OR "utsira")

Results: 24
Annex 4. Social network analysis measures applied to Sleipner co-authorship network

Centrality measures

_Degree centrality_ is as defined:

\[ C_D(n_i) = \frac{d(n_i)}{g - 1} \]

where \( C_D(n_i) \) is the ratio of the degree \( d(n_i) \) of a node \( n_i \) and the number of nodes \( g \) in a network.

_Betweenness centrality_ is defined as:

\[ g(v) = \sum_{s \neq v \neq t} \frac{\sigma_{st}(v)}{\sigma_{st}} \]

where \( \sigma_{st} \) is the total number of shortest paths from node \( s \) to node \( t \) and \( \sigma_{st}(v) \) is the number of those paths that pass through \( v \).

Centrality calculations

Input dataset: Centrality measures for the main component of the Extended Manual Search Sleipner Co-Authorship Network.

Similarity matrix: Measures the correlation between multiple centrality measures within the network’s main component. Peripheral actors are not included in this measure, as their marginal positions will distort the results and give a poor impression of knowledge flow between primary actors.

<table>
<thead>
<tr>
<th></th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td>Degree</td>
<td>1.000</td>
<td>0.848</td>
<td>0.660</td>
<td>0.702</td>
<td>0.845</td>
<td>0.757</td>
</tr>
<tr>
<td>BonPw</td>
<td>0.848</td>
<td>1.000</td>
<td>0.900</td>
<td>0.880</td>
<td>1.000</td>
<td>0.558</td>
</tr>
<tr>
<td>2Step</td>
<td>0.660</td>
<td>0.900</td>
<td>1.000</td>
<td>0.954</td>
<td>0.901</td>
<td>0.413</td>
</tr>
<tr>
<td>ARD</td>
<td>0.702</td>
<td>0.880</td>
<td>0.954</td>
<td>1.000</td>
<td>0.880</td>
<td>0.489</td>
</tr>
<tr>
<td>Eigen</td>
<td>0.845</td>
<td>1.000</td>
<td>0.901</td>
<td>0.880</td>
<td>1.000</td>
<td>0.555</td>
</tr>
<tr>
<td>Betweenness</td>
<td>0.757</td>
<td>0.558</td>
<td>0.413</td>
<td>0.489</td>
<td>0.555</td>
<td>1.000</td>
</tr>
</tbody>
</table>

_Cronbach’s Alpha = 0.95_  

Cronbach’s alpha assesses internal consistency (intercorrelation) among test items. It is formally defined as:

\[ \alpha = \frac{K}{K-1} \left( 1 - \frac{\sum_{i=1}^{K} \sigma_i^2}{\sigma_X^2} \right) \]
where \( K \) is the number of components, \( \sigma^2 \) the variances of the total test scores, and \( \sigma^2_i \) the variances of component \( i \) for the actors in the network. A score of 0 indicates that none of the centrality measures are related to one another, whereas a score of 1 would indicate that they are all the same measure. A score of 0.95 indicates a high degree of consistency among the centrality measures (Bland and Altman 1997).

**Statistical tests**

Standard formulas for computing standard errors and inferential tests on attributes assume independent observations. Applying them in social network analysis when observations are not independent can therefore be misleading. An alternative approach is to rely on methods that estimate standard errors for the specific network component. These ‘boot-strapping’ approaches calculate sampling distributions of statistics directly from the networks by using random assignment across thousands of trials (Hanneman and Riddle 2005: chapter 18).

The Sleipner network measures the actual numbers of publications between actors. It is therefore valued by instances of co-authorship, which becomes the relevant component in measuring the *density* of the network or the *central tendency* defined as the average strength of the tie across all relations. In order to assess whether the density of a network is relatively small or large a meaningful null hypothesis has to be chosen as the comparative basis. In this case, it might be relevant to convert ties to binary values thereby disregarding the strength of a tie as a function of instances of co-authorship with any other individual, and instead test whether there is a general tendency for most actors to function as more than mere bridges between others. In other words, we want to test whether the mean number of ties connecting any single actor to the network is significantly larger than \( 2/295 = 0.0068 \).

**Input dataset:** Binary conversion of the Extended Manual Search Sleipner Co-Authorship Network

<table>
<thead>
<tr>
<th>Results</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Density parameter</td>
<td>0.0068</td>
</tr>
<tr>
<td>Density of network</td>
<td>0.0206</td>
</tr>
<tr>
<td>Difference</td>
<td>0.0138</td>
</tr>
<tr>
<td>Variance of ties for network</td>
<td>0.0202</td>
</tr>
<tr>
<td>Classical estimate of standard error</td>
<td>0.0005</td>
</tr>
<tr>
<td>Number of bootstrap samples</td>
<td>5,000</td>
</tr>
<tr>
<td>Estimated standard error for density</td>
<td>0.0026</td>
</tr>
<tr>
<td>Average bootstrap density</td>
<td>0.0205</td>
</tr>
<tr>
<td>z-score</td>
<td>5.3185</td>
</tr>
<tr>
<td>Proportion of absolute differences as large as observed</td>
<td>0.0002</td>
</tr>
</tbody>
</table>
The comparison generates an actual network density of 0.0206, 0.0138 greater than the 0.0068 tested for. In other words, the mean number of ties from a node is 0.0206 * 295 = 6. Compared to our null hypothesis – that actors on average would function as bridges with just 2 ties – this is a relatively large number of ties and indicates that the network is fairly dense.

Next we can ask how often the calculated difference between the null hypothesis and our observation would occur by random sampling variation, if the null hypothesis were true. Using the classical estimate of standard error (0.0005) could be misleading, because relations in the network are not generated from random sampling. Instead, we can refer to the much higher estimated standard error of 0.0026 generated by the 5,000 randomly drawn sub-samples from the network, which in turn constructs a sampling distribution of density measures. The z-score (test statistic) of 5.3185 is also significant in this regard. Note that this value is based on the higher estimated standard error (0.0026) rather than the classical estimate of standard error (0.0005), which would generate a far higher test statistic. Even under this conservative measure the result is clearly significant (p < .0001).
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