Opening Up Spaces for Reflexivity? Scientists' Discourses about Stem Cell Research and Public Engagement

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I declare that the work in this thesis is my own, unless otherwise specified.
It has not been presented for any other degree or qualification.

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Table of Contents

List of tables

List of figures

List of abbreviations

AIDS	Acquired Immunodeficiency Syndrome
AS cells	Adult stem cells
ASCR	Adult stem cell research
cPUS	Critical or constructive public understanding of science
Engt.	Engagement
ES cells	Embryonic stem cells
ESCR	Embryonic stem cell research
ESRC	Economic and Social Research Council
FACS	Fluorescent Activated Cell Sorting
GCM	General Circulation Model
HFE Act	Human Fertilisation and Embryology Act
HFEA	Human fertilisation and Embryology Authority
HGU	Human Genetics Unit
IP	Intellectual property
ISSCR	International Society for Stem Cell Research
IVF	In vitro fertilisation
MRC	Medical Research Council
NHS	National Health Service (UK)
NHMRC	National Health and Medical Research Council (Australia)
NIH	National Institute of Health (USA)
PR	Public relations
QASA	Qualitative Analysis Software Aid
SCNT	Somatic cell nuclear transfer
SCR	Stem cell research
SSK	Sociology of scientific knowledge
STS	Science and Technology Studies
TWOD	Truth will out device

Abstract

This thesis starts with what the House of Lords Third Report (2000) has identified as a "crisis of trust" between science and society. It explores ways of addressing this crisis by examining stem cell researchers' discourses about their work and public engagement, and suggests ways of improving scientists' engagement with publics.

My journey from natural to social sciences started with an in-depth critical analysis of constructive (or critical) perspectives on public understanding of science (e.g. Irwin and Wynne). This highlighted the importance of investigating scientific institutions and scientists, and their embedded assumptions about publics, engagement and science. My research expands upon the limited empirical research on this topic and draws upon data from interviews and discussions with 54 stem cell researchers (of different levels of seniority and field of research, in Australia and the UK). Using informants' discourse as a "topic" and a "resource" (Gilbert and Mulkay), the thesis explores in detail the strategic and socially contingent definitions and boundaries (Gieryn) in stem cell research (SCR).

Analysis of the empirical material develops four main themes. Firstly, the language and conceptual fluidity of SCR is emphasised and shown to enable scientists to conduct "boundary-work" in a variety of ways. Secondly, discourses and performances of (un)certainty are examined to highlight a diversity of socially contingent identities SCR professionals can draw upon. This examination draws on MacKenzie's "certainty trough" but also improves it by problematising the concept of "*distance* from knowledge production". Thirdly, scientists' expressions of trust and ambivalence are analysed as interactions with particular "expert systems" such as processes of informed consent, commercialisation or legislation in conditions of increased globalisation. By highlighting hermeneutic aspects of trust, this analysis is sharpened and shows that there are elements of "counter-modernity" as well as "reflexive modernisation" in SCR. It is argued that, to further explore the reflexive potential of stem cell professionals' critiques of their work, these need to be further discussed in public. The forth and final theme focuses more specifically on engagement. Stem cell researchers' accounts are shown to construct and perform publics, scientists and engagement – and thus "scientific citizenship" – in a variety of ways. This variety can be made sense of by reflecting on conceptions of expertise, democracy, and power. This enables the development of six "ideal-types" of engagement that can be used heuristically to study performances of citizenship.

The thesis concludes by discussing its main contributions to knowledge. It highlights how social scientists can encourage greater "interpretative reflexivity" (Lynch) on the part of scientists; this can, in turn, lead to improved science-public relations.

Chapter 1

Introduction

The traditional 'good shepherd' self image of the governors [of science] is maintained, though with the modification that we now need to talk to the sheep and not merely push them around.

(Ravetz, 2005: 51)

In December 2006, it emerged that the Human Fertilisation and Embryology Authority (HFEA) had licensed a research group in Newcastle (UK) to ask women, not undergoing medical treatment, to donate their eggs to stem cell research (Dreaper and Buchanan, 2006). This decision caused a storm of criticisms, particularly as it was made before the end of the HFEA's public consultation on this complex issue. In this context, the title of this consultation, *Donating eggs for research; safeguarding donors* (HFEA, 2007), seems rather insincere; since it is difficult to safeguard donors if one does not listen to their concerns. This example of supposed public engagement appears to be mere lip-service and, referring back to the epigraph: it is unlikely the HFEA can maintain its image of good shepherd when it talks to, but ignores and possibly pushes about, the public. This example goes to the heart of widespread concerns about trust in science, its institutions and its governance; these are the focus of this thesis.

1.1 A "crisis of trust"?

The House of Lords Select Committee on Science and Technology Third Report (2000) opens by identifying a "crisis of trust" between science and society:

> Society's relationship with science is in a critical phase. […] On the one hand, there has never been a time when issues involving science were more exciting, the

> public more interested, or the opportunities more apparent. On the other hand, public confidence in scientific advice to Government had been rocked by a series of events, culminating in the BSE fiasco; and many people are deeply uneasy about the huge opportunities presented by areas of science including biotechnology and information technology, which seem to be advancing far ahead of their awareness and assent. (House of Lords, 2000: 1.1)

The report laments that science, despite being part of everyday life, does not automatically command confidence, particularly if it is associated with industry or government. Other examples, in the UK, of the lack of automatic trust in science and its institutions include the rejection of genetically modified foods and the unwillingness of some parents to give their child the MMR (measles, mumps and rubella) vaccine.

The House of Lords report warns that this situation must be dealt with since it has implications for national prosperity (House of Lords, 2000: 1.12-14). It suggests that part of the solution to this "crisis" lies in establishing a dialogue between science and members of the public, and encouraging public views and values to be included in decision-making around science. However, as the opening HFEA example suggests, this is not always straightforward. The present doctoral research starts with this institutional concern with trust in science and the possibility of improving it through public engagement activities. It critically examines science-public[1] relations through the lens of stem cell research (henceforth SCR). Specifically, it investigates scientists' discourses about their work and public engagement, and how these fit into broader contexts of power and trust relations in modern society.

1.2 The public understanding of science

The current preoccupation with science-society relations is not new (see Irwin, 1995). Traditionally, scientific institutions have assumed that greater public education in science – seen as value-free – will improve public approval and enable participation in social and political life (Irwin, 1995: 14). This way of thinking has been called the "deficit model of public understanding of science" (Wynne, 1991).

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[1] I choose here to use a dash (-) rather than a forward-slash (/) to indicate that science and publics are not in opposition to each other. Rather, both are part of society, and scientists, science and publics play different roles (as lay people and as experts) at different times.

Institutional concepts of the deficit model crystallised in the UK in 1985, with the publication of the Royal Society Report on the "Public Understanding of Science" (also called the Bodmer report). In a now famous quote, the panel urged scientists to get more involved in communicating science:

> our most direct and urgent message must be to the scientists themselves: Learn to communicate with the public, be willing to do so and consider it your duty to do so (Royal Society, 1986: 60)

This approach to improving science-public relations has not been successful, and has been criticised.

In particular, research that comes under the umbrella of critical or constructivist public understanding of science (cPUS) has shown that attempts at increasing public knowledge have *not* led to increased levels of support for science. For instance, Sturgis and Allum (2004) highlight the rising unease surrounding new scientific discoveries, despite efforts at science popularisation. Further, Collins and Evans note that people who are more informed about science often display less support for it (2002: 274); and, Evans and Durant (1995) find that people who are more knowledgeable about science are less supportive of contentious areas of research than people who are less knowledgeable about science. In a study specifically examining public perceptions of cloning, the Wellcome Trust found that, as people learnt more about the use of cloned embryos in research, they expressed more reservations towards this technology (Wellcome Trust, 1998: 6.4). Collectively, these sorts of studies demonstrate that science-public relations are not readily improved through public education.

One potential limitation to the deficit model to address science-public problems stems from the particular image of science that is projected in these education campaigns. Often, attempts at gaining public trust involve portraying science as unproblematic, certain and unified. However, people experience the uncertainties and contingencies of science in their daily lives and therefore, when they are confronted with this unfamiliar portrayal of science, this can result in a decrease in trust (e.g. Collins, 1987; Wynne, 1988; Collins and Pinch, 1993; Yearley, 2000). Many studies show that members of the public have multi-layered understandings of science and that it is the continued dismissal of this knowledge, rather than public misunderstanding, which can be the source of mistrust. This is shown vividly in the book *Misunderstanding Science? The Public Reconstruction of Science and Technology* edited by Irwin and Wynne (1996).

This book and other cPUS research suggest the need to "problematise" notions of "public", "understanding" and "science". That is, "understanding" should be seen as a more interactional process than simply being able to repeat the "correct" definition of DNA. Rather, people have complex and sophisticated understandings of science, which come from using their embodied experiences and including social factors (such as the relevance of the information or the context in which it is given) to make sense of science. Thus, the "public" is not a homogeneous group that passively awaits information; "publics" are active participants in their interactions with science. In addition, "scientific facts" are not revealed from nature through rational thought, but are embedded in social and cultural processes. In this regard, there are always theories and assumptions that shape "facts". For example, the validity of experiments is negotiated by social groups, not given by nature (e.g. Collins and Pinch, 1993). Thus, there are no simple "facts" that can simply be taught to lay people in order to make them support science; and lay people should be included in discussions around science as they may have interesting, valuable and legitimate knowledge and viewpoints to contribute.

Scientific institutions are starting to take note of this cPUS research. In particular, the House of Lords Third Report (2000) drew on the above book, and one of its authors was a specialist advisor to the Select Committee on Science and Technology. Notwithstanding the use of language associated with the deficit model, the report highlights that people have an understanding of risk and that their mistrust of science may not be rooted in ignorance. Rather than simply proposing education as a solution, it highlights the need for openness and the inclusion of multiple views in decision-making around science.

Additionally, in 2002, Copus, the Committee for the Public Understanding of Science created in the wake of the Bodmer report, stated that:

> We have reached the conclusion that the top-down approach which Copus currently exemplifies is no longer appropriate to the wider agenda that the science communication community is now addressing. (Copus News, 2002)

Thus, we are now beginning to observe some evidence of the influence of cPUS research on some UK scientific institutions and there appears to be a shift from the deficit model towards a more dialogic model of science-society relations. However, whether this change is visible in the practice of public engagement, or whether it is purely rhetorical, needs further examination.

1.3 The co-existence of "two voices"

Closer inspection of science policy suggests that the above shift is far from revolutionary. Hagendijk notes that a variety of policy documents try to include concepts drawn from public *engagement* rather than public *education*. However, these documents often read:

> as if two voices are struggling to be heard. The dominant voice, assuring the reader that citizens' concerns should be taken seriously, and ought not to be treated in a condescending way. In contrast with this, however, a second, more 'scientistic' voice argues that the public can only contribute properly if it is adequately educated and instructed. (Hagendijk, 2004: 46)

Irwin (2006) draws similar conclusions when studying a variety of "engagement" exercises and official reports. He notes an "uneasy blend of 'old' and 'new'" forms of governance (2006: 299). This more recent cPUS work suggests the need to be hopeful but also sceptical of the potential of public engagement.

Wynne (2006) notes that scientific authorities, rather than problematising their assumptions – such as the universality of science, the separation of science from politics, or the technocratic and expert-led framing of risk – continually re-invent various deficit models. He finds that the "deficit of understanding" is replaced by a "deficit of trust", which can supposedly be managed with improved "transparency". He argues that scientific institutional bodies do not turn a critical eye on themselves and that most changes towards "engagement" are mere illusions:

> virtually all of the mushrooming commitment to public citizen engagement in 'science policy' or 'scientific-technical issues', or to 'democratizing science', is something of a mirage, at least thus far. It imposes severe and unspoken framing limits around these new processes, such that the continuing failure to *democratically sensitize* science, and its persistent non-accountability to publics even in the new (if still limited) 'participatory' ethos, is omitted from critical attention. (Wynne, 2005b: 68, original emphasis)

This indicates that sociologists need to explore the assumptions made by scientists and policy-makers, as well as the framing of engagement exercises, and how and why these remain unexamined.

In the field of SCR, there are similar calls for engagement and/or education. For example, the Australian Stem Cell Centre (ASCC) seems firmly committed to "education":

> Education is a fundamental component of the work we do at the Australian Stem Cell Centre. The ASCC is committed to providing a range of community programs designed to help the public understand emerging issues in biotechnology and, in particular, stem cell technology. (Australian Stem Cell Centre Website)

In contrast, the UK's Institute for Stem Cell Research adopts the language of "outreach". This could reflect different levels of uptake of cPUS suggestions by different institutions.

Therefore, it seems that any shifts in institutional body language around engagement have not been very radical, even if some insights from cPUS have been taken on board. There is still much need for a contemporary examination of scientists and their practices and assumptions, particularly in a controversial area like SCR. This is what this thesis seeks to do.

1.4 Stem cell research

There is a broad agreement within the scientific community that stem cells are fascinating research tools with which to study fundamental biology, and that this could lead to findings from which therapeutic applications may derive. Stem cells are cells from which originate all other cells, and therefore tissues and organisms. They enable lizards' tails to grow anew after being chopped off; they enable bone marrow transplants to repopulate someone's blood and rebuild their immune system; they enable gut and skin epithelium to renew itself throughout adult life; they enable tissue repair in multiple organs. Some stem cells exist in an organism and generate new cells until the organism's death (for example, bone marrow stem cells). Some stem cells only exist early during development, but have the potential to give rise to many different cell types (for example, embryonic stem cells). And some stem cells can be obtained by putting DNA from a terminally differentiated cell (that is, not a stem cell) into an egg, then causing this newly created cell to start dividing and, thus, give rise to a cloned embryo. Stem cells can occur in living organisms, or can be grown in culture. These cells also map onto a number of issues of public concern.

In order to study the fluid science-public interface, it makes sense to choose an area of research where scientists and members of the public are likely to interact. This often occurs if research is "controversial", which for Adele Clarke can come about:

> if the research has direct implications, if it challenges or threatens the 'natural order', if it is relevant to some politicized issue, if sentiment has mobilised a related social movement, or if the research is in competition for scarce resources (cited in Epstein, 1996: 5)

SCR fulfils many of these criteria and is therefore a promising location for a thesis focussing on science-public relations. For instance, SCR is hailed by some as the new miracle cure for diabetes, Alzheimer's, Parkinson's and various other devastating diseases. However, it raises fundamental questions about human identity with its association with human reproductive cloning. The need for women to donate their eggs raises issues of power, coercion and ownership of the body. The use of human embryos builds on earlier controversies about abortion and the sanctity of life (Parry, 2003a). Additionally, the funding of SCR, particularly in the United States but also within Europe, is fuelling many political disagreements. SCR fits into wider contexts of scarce funding for research and worries around commercial applications and patenting. SCR has also been very present in the public eye, in part due to its high profile proponents (such as the late Christopher Reeves, Nancy Reagan and Michael J. Fox) and opponents (Georges W. Bush or Mel Gibson). Yet, in spite of all these ethical, social and political problems, SCR attracts many researchers (Lewis, 2007).

For SCR to prosper beyond what it has already achieved (such as in basic developmental studies using mouse embryonic stem cells) and perhaps lead to further therapeutic applications in humans, it requires public support: from politicians, to release government funding for research or change laws in order to make embryo research legal (which is based on assumptions of broader public support); from patients, to participate in clinical trials or donate material (be it embryos, eggs, skin, bone marrow etc.); or from investors, to fund aspects of research and development. Thus, it is interesting to explore scientists' engagement with publics in the context of SCR as this research is controversial as well as exciting.

1.5 Studying scientists in the UK and Australia[2]

This thesis studies scientists, stem cell researchers in particular, with a view to contributing to improving scientists' engagement with public groups. There have been many cPUS studies of publics and their sophisticated understandings of science. Here, I want to address a

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[2] The research design is developed in more detail in chapter 3.

gap in the literature and focus specifically on scientists and their discourses. I examine how they talk about their work, but also about publics and engagement. This is important as scientists are influential participants in public engagement and the framing of engagement exercises.

Public discussions around SCR have often taken the form of binary oppositions, both in the media and in parliament: adult versus embryonic stem cell research, science versus religion, curing patients versus respecting unborn lives, emotions versus rationality, or reproductive versus therapeutic cloning (e.g. Parry, 2003a; Williams *et al.*, 2003; Harvey, 2005). This thesis is interested in the complexities and diversities in these seemingly straightforward positions. In particular, it does not aim to homogenize scientists, but rather to explore and highlight the diversity in their voices.

A variety of stem cell scientists, in a variety of settings, are investigated: early and late-career, female and male researchers, with academic and commercial backgrounds, during one-to-one interviews, group interviews and larger group discussions. This research also draws on some publicly available discourses (from legislative debates for example). Thus, it accesses dominant and marginal discourses, in public and private settings. By studying what informants say and how they say it, discourse is used as a “topic” and a “resource” (Gilbert and Mulkay, 1984).

Another interesting aspect of this study is its international nature: it examines stem cell researchers in two different countries. Various people have highlighted the importance of doing international comparisons (e.g. Jasanoff, 2005a) and there can be national differences in levels of public engagement[3]. The UK and Australia were elected as study locations because both these countries are English speaking, have similar styles of governance and are committed to scientific research (and particularly to SCR). However, at the time of my research, they had different legislations governing SCR: the creation of embryos for research was illegal in Australia, but legal in the UK.

Overall, the conceptual framework of this thesis is sympathetic to cPUS. It examines scientists and how they negotiate their authority in different settings (in particular what sorts of boundaries they draw, cf. Gieryn, 1983; 1995); how they portray their work, and its

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[3] For example, Denmark displays more openness and allows for more public criticism and therefore perhaps engenders more trust than in other countries (Rose, 2000; Irwin, 2006).

uncertainties and promises; how they construct their status as “expert” or “lay” actors in the context of increased globalisation (Giddens, 1990; Beck, 1992); and how they construct “scientific citizenship” (Irwin, 1995; 2001).

1.6 Broader relevance and outline of thesis

The audience for this research is multi-disciplinary because its findings are relevant to social and natural scientists, including, but not restricted to, those with a specific interest in SCR. Nevertheless, the present thesis is more particularly targeted at sociologists. Thus, although the introduction is meant to be accessible to the interested “lay” audience, the following chapters have a stronger sociological focus.

Chapter 2 critically discusses the literatures which form the backdrop to my research. It introduces the key theoretical concepts that underpin my thesis. Some particular concepts are subsequently discussed in more detail in relevant chapters, whilst some specific literatures are only introduced in appropriate chapters.

Chapter 2 starts by exploring engagement and its limitations by drawing on the sociology of scientific knowledge and cPUS, alongside development studies. It highlights that all forms of science-public interactions perform and impose particular identities on participants, which may lead to public alienation from science. The chapter then explores ways of conceptualising and making sense of trust, bringing together Beck and Giddens’ models, with the more hermeneutic approaches of Wynne and Szerszynski. Having argued that trust cannot be engineered, the chapter goes on to discuss how “reflexivity” may be a way of improving science-public relations by challenging the traditional, expert-led framings of public engagement. Routes towards emancipation are then theorised by drawing on Bourdieu and Foucault’s work on relational power and on social identity theory. Furthermore, the chapter discusses the two main sociological tools which enable the investigation of scientists’ discourses: Gieryn’s “boundary work”; and Gilbert and Mulkay’s approach to discourse analysis. Overall, by problematising the contrast between “lay” and “expert” knowledge, this chapter enables the present study of stem cell researchers to employ a variety of conceptual frameworks to view these scientists as both as “experts” and “publics”, and to explore routes towards reflexivity.

Chapter 3 discusses my research design and how the above conceptual tools were used in practice. It sets out my research questions in detail and explains how they came about. It discusses some aspects of my theoretical and epistemological frameworks. It then details my data collection and analysis and concludes by reflecting upon the limitations of the study.

Chapter 4 marks the beginning of my empirical chapters. It has two main aims: one is to introduce some of the language of SCR and how stem cells are classified; the other is to show the constructed and contested nature of some of these categories. This chapter starts rather descriptively; this is needed to preface the rest of the thesis, as some of my analytical discussions revolve around the constructions and use of labels in SCR. It discusses different types of stem cells such as those found in adult tissues and those found in embryos derived from IVF or cloning. It highlights that there is no obvious or naturally given way of classifying or defining these cells. The chapter then turns to data obtained from interviews and discussions. After highlighting that what counts as central to SCR varies between my informants, it examines different criteria used for patrolling the boundaries around SCR. How these are mobilised is dependent on informants' social locations. This chapter shows that "stem cell" can be used as a boundary-object (Star and Griesemer, 1989) and its conceptual fluidity enables the alignment of multiple agendas in a field fraught with uncertainty.

Chapter 5 examines this uncertainty in more detail. Using MacKenzie's (1990) "certainty trough" to make sense of informants' discourses of (un)certainty and, then, building on this model using Lahsen's (2005a) problematisation of the "distance" from knowledge production, it highlights how social locations and the use of salient social identities can influence these discourses. This shifts the focus towards the performativity of language. Drawing on the sociology of expectations (e.g. Brown and Michael, 2003), it also analyses the hype and hope surrounding SCR, particularly in the context of competition for research funding or cognitive authority. This chapter offers the opportunity to start thinking normatively about (un)certainty discourses in the context of public engagement.

Chapter 6 centres on stem cell researchers' trust and ambivalence by exploring people they describe using labels such as "cowboy" or "maverick". By analysing informants as "lay", to the extent that they have to enter "expert systems" (Giddens, 1990), it reveals a number of ambivalences expressed by professionals with regards to particular areas of their work. This trust (or lack of) is not only analysed as the expression of an "actual" feeling based on rational calculations, but also as a rhetorical but “virtual” trust (Wynne, 1996b) or a performance of trust (Szerszynski, 1999). Professionals’ concerns voiced about clinical trials, informed consent, commercialisation, and the internationalisation and globalisation of research, are shown to open up areas for “reflexive scientization” (Beck, 1992) and “interpretative reflexivity” (Lynch, 2000). This suggests that professional discourses may have a more revolutionary potential than previously thought (cf. Kerr and Cunningham-Burley, 2000).

Chapters 7 and 8 focus more specifically on engagement. They are concerned with how my informants’ discourses around publics, SCR and engagement construct and perform “scientific citizenship” in particular ways. Chapter 7 focuses on the variety of constructions of citizenship present in stem cell researchers’ accounts, as suggested by Irwin (2001). It reveals some of the assumptions embedded in these accounts by examining three aspects of SCR. Firstly, it examines an episode of boundary-work aimed at differentiating and merging stem cells derived from “embryos” and from “foetuses”. Secondly, it looks at the label “therapeutic cloning” and how it can be used to open up or close down discussions around SCR. Thirdly, it investigates how informants talk about reproductive cloning and use science fiction imagery. The analysis of these three issues around SCR reveals that these scientists draw on a number of hierarchical dualisms (cf. Faulkner, 2000): technical versus cultural, social or political; rational versus irrational, emotional or values-based; and reductionist versus holistic worldviews. They construct the roles of publics, scientists and engagement in multiple and socially contingent ways.

Chapter 8 makes sense of these multiple concepts of scientific citizenship and develops six “ideal types” (Weber, 1949) of public engagement in science. These range from engagement as a public relations exercise, to engagement as educating potential consumers or citizens, and to engagement as an opportunity to draw on a variety of situated knowledges and reflect upon science, its assumptions and its consequences. Each ideal type assigns different roles and levels of importance to publics, scientists, “rational facts”, education, dialogue, and so on. Each one also reflects a different conception of “democracy”. This chapter argues that, contrary to Ezrahi’s (1990) assumptions, scientistic worldviews and “utopian rationalism” still dominate public engagement. It suggests the need to acknowledge and challenge power imbalances (Mouffe, 1996) and look for types of engagement that do not simply reinforce scientists’ cognitive authority.

Chapter 9 concludes the thesis by highlighting its main conclusions. Using “reflexivity” as a narrative thread, it explores my contributions to social sciences and to public engagement. In particular, it highlights the interpretative flexibility and performativity of stem cell researchers’ discourses. It also discusses the extent to which scientists are publics. It then explores opportunities for increased reflexivity on the part of scientists and their institutions. This can be facilitated by social scientists: they have a role in bringing to light and critically analysing the embedded assumptions which frame scientists’ knowledge claims, as well as science-public interactions. Eschewing a normative position that would see social scientists replace scientists as the main “experts”, the thesis concludes by making more specific suggestions for improved public engagement and science-public relations.

Chapter 2

Literature review

2.1 Introduction

The starting point of this thesis is the concern voiced by various groups about a "crisis of trust" between science and publics. As raised in the introductory chapter, researchers interested in critical or constructivist approaches to "public understanding of science" (cPUS) have criticised the sole use of education as a means of restoring trust in science. Instead, they advocate the recognition and incorporation of other forms of knowledge, through public *engagement* in science. However, despite cPUS views gaining public support and informing some policy-making, old voices of education and knowledge deficit remain alongside these new voices of engagement. The extent and meaningfulness of these slight shifts towards engagement need to be further examined if we want to better understand and improve science-public relations.

As this chapter further discusses, work in the Sociology of Scientific Knowledge (SSK) tradition has demonstrated that scientific knowledge and "expertise" are socially constructed. In addition, cPUS research has provided a wealth of empirical and conceptual material on the interactions between science and publics from the so-called "lay" perspective – in particular, highlighting the abundance of sophisticated and relevant "lay knowledge" and "lay expertise". However, the voices of "experts" are often dominant in science-public interactions and science and its institutions play important roles in public engagement activities, often *framing* them. Thus, it is also important to consider in more detail how scientists react to calls to engage with these knowledges, how they shape science-public interactions, as well as the sorts of voices and identities that are privileged in public engagement.

This thesis aims to build on the limited amount of work on science-public interactions, seen from the so-called "expert" perspective. It uses a constructivist epistemology informed by studies of power and language to examine how scientists (stem cell researchers in particular) talk about and do public engagement in science. Following in particular Irwin and Wynne's recent work, it offers a contemporary investigation of how stem cell researchers' discourses and worldviews shape science-public interactions. It also specifically searches for potential points of resistance to the traditional "lay" versus "expert" framing of science-public interactions.

This chapter discusses the literature forming the backdrop to my PhD[4]. In particular, it problematises the categories "lay" and "expert" and combines conceptual tools and frameworks traditionally used to study "lay" publics with ones used to study "experts"[5]. This enables analyses of stem cell researchers as both constructing and drawing on multiple and contingent identities and discourses. It firstly examines cPUS in more detail, as well as some limitations of engagement. After highlighting that "knowledge" and "expertise" are socially constructed, this section details two empirical studies of "lay-expert" interactions and the sometimes uneasy combination of different forms of knowledge. Drawing on the Development Studies' critique of participation, it then explores how the framing of engagement can impose identities and worldviews on participants. Secondly, the chapter turns to trust, a central concern of many interested in public engagement. It highlights the limitations of instrumentalising it and suggests the need for greater "reflexivity". Thirdly, the chapter explores opportunities for reflexivity and emancipation from the traditional framings of engagement. Since I believe that scientific institutional reflexivity is not automatic (unlike some authors discussed here), I focus on ways of conceptualising power relations which enable an analysis of how reflexivity can challenge the status quo. This section looks at the role of social sciences in revealing the contingent nature of framings (of engagement for example) that we take for granted and suggests emancipation away from these. It also explores how social identity theory can help make sense of oppression and emancipation. Finally, the chapter focuses on scientists' discourses, which are often central in shaping

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[4] My epistemological and ontological frameworks, as well as my research questions, are further discussed in chapter 3.

[5] Although I wish to problematise concepts such as "public", "understanding" and "expertise", there is a tension between using the language of social construction, and over-problematising to the point where language loses all meaning. Wynne (1992) notes that Sheila Jasanoff is successful in maintaining a good balance between the two in her book *The Fifth Branch*. I hope to achieve the same here.

engagement, and examines the two major frameworks used throughout the thesis to analyse these. One is “boundary-work” and the other “discourse analysis”.

The chapter aims to show that the study of scientists, and their discourses and rhetorical strategies, can provide insights to help “improve” science-public relations. My normative position as to what constitutes “improvement” will become apparent as the chapter, and the thesis as a whole, proceeds.

2.2 CPUS and the social construction of expertise and knowledge; The need for better public engagement

A number of studies, such as those by SSK scholars, have demonstrated that scientific knowledge is socially constructed and that the validity of scientific claims is socially negotiated. This, in combination with cPUS findings that “lay” people hold alternative forms of sophisticated knowledge, challenges traditional conceptions of “expertise” and raises questions as to why publics are expected, under the deficit model of PUS, to simply accept scientific “facts” as given, rather than having a more active role in shaping science.

2.2.1 The social construction of scientific knowledge

As outlined in the introductory chapter, science is embedded in social and cultural processes. A variety of studies from the various Science and Technology Studies (STS) schools, especially from the SSK tradition, have shown the socially constructed nature of science and technology[6] (see for instance Bloor, 1976; Collins and Pinch, 1979; Latour and Woolgar, 1979; Barnes, 1983; Collins, 1983; Shapin and Schaffer, 1985; MacKenzie, 1990). They argue that knowledge claims and definitions are socially negotiated rather than mirror images of nature. These are socially interpreted and open to discussion, controversy and dismissal. Historically embedded rhetorical strategies, tactical alliances, interests and social locations all play a role in the negotiation of scientific claims. Scientific theories are “underdetermined” by reality and observations are “theory-laden” (Bloor, 1976). Thus,

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[6] I use the term science throughout this thesis to imply science and technology, unless I am specifically distinguishing between the two as I believe that many findings about the social construction of science apply to technology and vice versa (Pinch and Bijker, 1984).

scientific knowledge can be considered “situated” (Haraway, 1988) or “contextual” (Irwin, 1995).

Collins and Pinch’s (1993) discussion of the “experimenter’s regress” is particularly salient here, and highlights the role of social factors in the constitution of knowledge claims. They show that controversies around science are not resolved by further experimentation: if a scientist obtains a result from a particular experiment, the knowledge claims that s/he makes can be contradicted *ad infinitum* by another scientist who believes the experiment is not being done “correctly”. If this second scientist replicates the experiment but gets a different result, there is no way of settling, simply by undertaking more experiments, whether the hypothesis being tested is wrong, or whether the experiments are being done slightly differently. Collins and Pinch show that it is through social negotiations that the validity of the knowledge claim is set. These negotiations also determine which persons constitute appropriate witnesses to these experiments (see Shapin and Schaffer, 1985).

This social constructivist framework is equally relevant (and applicable) to technological knowledge. Mackenzie’s (1990; 1998) thorough investigation of intercontinental ballistic missiles demonstrates that there is no given or pre-determined trajectory that technological developments follow. It also shows how the accuracy of these missiles, which could not be easily empirically tested, was negotiated by a number of key figures in the area, and how this “accuracy” was related to a variety of political factors and interests such as personal, institutional or national commitments to particular technologies or objectives. By developing the concept of “heterogeneous engineering”, Mackenzie highlights that social actors “engineer” technical artefacts, as well as other social actors, in order to fit their interests (rather than artefacts existing independently of people). An important finding, made by Mackenzie (to which I shall return in chapter 5) is that the certainty displayed towards particular aspects of the technology is dependent on individuals’ proximity to knowledge production.

Thus, a particular scientific claim about the natural world is temporary and contingent; its acceptance the result of social negotiation. This poses the question of how publics, or “lay” people, make sense of science and whether they have any knowledge to bring to these negotiations and broader discussions around science – I turn to this in the next section. The social construction of scientific knowledge also problematises the role of scientists in public debates about science. Indeed, if scientists do not have a more direct access to truth, why should their view take precedence in public debates about science?

2.2.2 The importance of "lay" knowledge

A variety of studies have highlighted that lay people can have sophisticated understandings of science (for instance Wynne, 1991; Irwin, 1995; Wynne, 1995; Irwin and Wynne, 1996).

In particular, Kerr *et al.* (1998a; 1998b) have also shown that publics can display very intricate, socially located, understandings of science. In the context of the new human genetics, they show that lay people hold four different types of knowledge: "technical" knowledge, around genetic inheritance for example; "methodological" knowledge, such as knowledge of the limitations of genetic testing; "institutional" knowledge, such as the links between research and funding or commercialisation; and "cultural" knowledge, about the social and cultural contexts in which knowledge is produced. Although these knowledges, particularly the first two, may not always be "accurate", they do not prevent participants from having sophisticated conversations about science and its limits (Kerr *et al.*, 1998b).

Parry (2003b) also shows that publics have complex and sophisticated understandings. Her informants make sense of SCR and cloning within broader contexts, such as their trust in scientific institutions, their knowledge of the way science is "done", their lived experiences of disease, their awareness of commercialisation, the international nature of scientific research and the attending difficulties in legislating research (see also Parry, 2006).

However, science may seem to "disappear" in everyday language (Irwin *et al.*, 1996). For example, discourses about science are often made at two distinct levels: "science-in-general" and "science-in-particular" (Michael, 1992). Here, "science-in-general" corresponds to an intangible entity that is evoked in terms of the motives of research, the institutional context and the figure of the scientist, and "science in particular" is located in day-to-day life and has more practical applications. The disappearance of "science" can also be due to the way that lay people re-appropriate it for themselves, and translate it into language and concepts they are more familiar with, and upon which they can base decisions. For example, patients make sense of genetic risk, not in terms of statistics, but in absolute terms – should I/should I not have a child, go for screening, etc. (see Parsons and Atkinson, 1992; Lambert and Rose, 1996). They make sense of science in a particular context (see also Barns *et al.*, 2000).

The above studies demonstrate that people can intelligently and critically discuss research and its implications, without needing professional scientific training. Input from publics can, in fact, open up debates around scientific developments. The above discussions were in part facilitated by the context in which they took place – focus groups in many cases, which enables people to develop their own views and understandings in a respectful and non-adversary environment (Kitzinger and Barbour, 1999). They are good examples of ways in which public "engagement" in science can be conducted, and highlight the richness that lay people can bring to these conversations when they are not marginalised or excluded. Here, an important feature of engagement is the upstream involvement of lay people in discussions about future directions of research. The objectives are not to teach lay people about abstract objective science, but to explore and learn from their understandings of science in context.

A variety of methods for doing engagement have been suggested in addition to focus groups, such as public hearings, consensus conferences (Davison *et al.*, 1997; Mayer and Geurts, 1998) or citizens juries (Dunkerley and Glasner, 1998); and a variety of calls have been made for more lay involvement in healthcare, both in the UK (e.g. Kerr *et al.*, 1998b) and Australia (Wiseman *et al.*, 2003). These methods often are successful at eliciting a variety of views but, even in contexts where lay knowledge is particularly encouraged, it tends to be treated by scientists and lay people alike as inferior and less relevant than "expert" knowledge (Kerr *et al.*, 2007). Additionally, local level successes encounter difficulties when they need to be translated to national contexts, where policy decisions are made (see Dunkerley and Glasner, 1998). In particular the complexities and uncertainties revealed during these small-scale discussions have difficulties finding their place in the dominant discourses around science, which retain their focus on universality, prediction and control, as I discuss further in the next section.

2.2.3 *Farmers, activists and expertise: combining different sorts of knowledges and worldviews?*

Despite the presence of sophisticated lay knowledges, the views of so-called "experts" tend to take precedence over these, often leading to troubled science-public relations.

As Nelkin argues, experts are “both indispensable and suspect” (1975: 36) in public discussions. Their authority, resting on beliefs about their objectivity, neutrality and rationality, is used to shore up different positions.; it becomes a “crucial political resource” (Nelkin, 1984 [1979]: 16). However, the attribution of this label is strategic and context-dependent (see also Kerr *et al.*, 2007) and being able to call oneself an “expert” has important implications in terms of the authority this bestows to one’s views. Thus, “expertise” should be viewed as a social construction, rather than a something that exists *out there* (Turner, 2001, see also chapter 8). Expertise is thus context-dependent and establishing the ways in which it is an outcome of negotiations between different actors is central to studying public engagement.

Here, I discuss two encounters between people considered “lay” and “expert”. In the first one, “lay” views were ignored, creating discord between farmers and government scientists. In the second one, patient activists, after a process of” expertification”, succeeded in having their knowledge and views respected by medical professionals, creating a more harmonious working relationship around the design of clinical trials. These two empirical studies question the sole reliance on “expert” knowledge and show the value of including alternative forms of knowledge in decision-making around science. They support calls for more public engagement in science and I return to them later in the chapter to show how the different conceptual tools I use can help make sense of them.

The first study is that of Wynne’s now famous study of the Cumbrian sheep farmers near Sellafield (e.g. 1996b; 1996c). Following the Chernobyl disaster, high levels of radiation were measured in this region, leading to a ban on lamb sales. Despite initial reassurances by government scientists sent to investigate the problem that the ban would be short, radiation levels remained high. This led to an indefinite ban and to potential ruin for the farmers.

Wynne shows that the government scientists applied their particular laboratory-based knowledge to the conditions in the Cumbrian hills. They assumed their knowledge to be universal and therefore directly translatable from the laboratory to the hills. Wynne argues that the scientists were committed to a version of nature as predictable and controllable. In contrast, the local farmers thought the variety in the local conditions – such as the ecological environment and the sheep’s feeding and penning habits – should be taken into account by the scientists. The farmers knew and understood this environment, but saw it as unpredictable and uncontrollable. These two worldviews co-existed in the Cumbrian hills, but did not come together. The scientists remained faithful to their supposedly universal laboratory knowledge and dismissed the farmers' knowledge as irrelevant. This led to a series of contradictory and un-implementable measures being set.

Government scientists ignored important knowledge claims made by the farmers; this was in part responsible for the contamination of the sheep remaining high and for a lack of timely and appropriate solutions for the local economy being found. For example, the scientists undertook some experiments to measure the efficiency of a particular mineral – bentonite – in absorbing radiation and preventing sheep from becoming contaminated. They did this by spreading different concentrations of bentonite over particular areas, then penning sheep in these areas with the aim of measuring the contaminations levels in these sheep. The farmers immediately pointed out that the sheep, regardless of their contamination levels, would loose condition if they were prevented from roaming free. They argued that this would confound the experiment, but the scientists ignored their comments. The latter eventually abandoned the experiments, but never acknowledged why (see Wynne, 1996b: 67; 1996c: 26). The government scientists also proposed solutions that were inappropriate and they did not draw on the farmers' expertise to adjust their recommendations. They told the farmers that, in order to reduce the sheep's contamination levels, they should take the animals from high level contamination areas – the fell tops – to low level contamination areas – the valley. The scientists ignored the local knowledge that there is not enough grass in the valley to feed the sheep for long periods of time (see Wynne, 1996b: 66; 1996c: 36).

In this case study, government scientists, with the authority to recommend ending the ban on lamb sales, did not recognise farmers as having any relevant knowledge or "expertise" to bring to the way radiation measures and research in Cumbria were undertaken. In addition, they did not recognise the uncertainty or the contingency of their own knowledge. This was in part responsible for ruining the scientists' credibility in the eyes of the farmers, who had seen their own knowledge dismissed, but had also witnessed the "messiness" of the scientists' measurements and knowledge construction. Here, scientists' and farmers' knowledges were not fruitfully combined, but I think this study suggests that better incorporation of public knowledge could have lead to better decision-making and would have facilitated cooperation rather than mistrust between these two groups.

In the second case study, holders of avowedly non-universal knowledge were able to affect the conduct of scientific research. Epstein (1995; 1996) investigates the role of AIDS activists in shaping clinical trials in California. To have an impact, this heterogeneous group of activists had to learn the customs of the scientific community, and find ways of entering it. They had to appear legitimate to the formal "experts", which they did by a "process of 'expertification'" (1996: 13): attending conferences, reading peer-reviewed papers and getting familiar with the research and its lingo.

Interestingly, the activists were also able to create new ways of establishing their credibility in the eyes of scientists. They introduced credibility currencies beyond those normally associated with science. For instance, they argued that it was *morally* important that patients should be able to participate in clinical trials, which were re-constructed as a social good, with inherent risks that individuals could choose to bear. Activists also challenged the idea that clinical trials could yield "clean" data by only including "pure" subjects in trials (that is those who had never been in trials and were not currently under any medication). Instead, they argued that trials should be used directly as a way of treating patients, and should, as one of the activists argued: "incorporate the heterogeneity, occasional or frequent ambiguity, and other 'messy' aspects of ordinary clinical practice" (Epstein, 1995: 422). Here, activists went through a process of expertification, but also were able to change the conduct of science. People with different kinds of knowledge were able to find common ground and cooperate. This example shows that public engagement can lead to decisions around science that are satisfactory to patients as well as professionals.

These two examples, in conjunction with the cPUS and SSK work discussed earlier, challenge the idea that people traditionally labelled "experts" are the only relevant people to make decisions around science and begs the question of what "expertise" means and how public engagement activities are framed.

The above two empirical studies were re-analysed by Collins and Evans (2002). Of relevance here, is that their analysis does not deal appropriately with the negotiations that take place in order to be conferred with the status of "expert" (see Jasanoff, 2003b). Collins and Evans argue that the failure in cooperation in Wynne's example was due to the farmers lacking "interactional" expertise; that is, lacking the ability to show the scientists they had relevant knowledge. However, as Wynne (2003) argues in his response to Collins and Evans, the lack of cooperation was related to the government scientists imposing frameworks of meanings onto the farmers, such as reducing the concept of “risk” to that of predictable and calculable risk.

In terms of public engagement, Collins and Evans pose the question in terms of the “problem of extension”:

> Should the political legitimacy of technical decisions in the public domain be maximized by referring them to the widest democratic process, or should such decisions be based on the best expert advice? (2002: 235)

Collins and Evans thus take a realist view of expertise. In doing this, they fail to analyse the political manoeuvrings that come into play when labelling people as experts or not, and as having a role in engagement or not. By contrast, in following Nelkin, Wynne, Jasanoff and others, I see this as central. Wynne argues that, by not attending to the imposition of frameworks onto public discussions by powerful “expert” institutions, Collins and Evans:

> risk reinforcing in practice just this authoritarian social idiom, in which public meanings (and identities) are not problematised, but presumed and imposed. (Wynne, 2003: 404)

Following Wynne, I believe it is crucial to problematises “science”, and question how and why it becomes constructed as the “natural” framework in which to discuss questions about risk, rather than accepting science as the self-evident frame.

Further, Jasanoff contends that Collins and Evans hold a very reductive way of conceiving of engagement. Particularly, in the context of modern democracies, she argues that publics should have a role in critically assessing the frameworks used by scientific and policy institutions, and checking whether they meet acceptable standards according to a particular culture’s “distinctive civic epistemology” (2003b: 398). This thesis seeks to pay particular attention to how engagement is framed by scientists and what sorts of citizenship this enables. In the next section, I explore how work in Development Studies has started examining how worldviews and identities are imposed during science-public interactions

2.2.4 Engagement and participation; “performing” identities

CPUS and Development Studies shed light on how the framing of engagement activities, or “participatory development” as it is called in the latter field, imposes particular identities on participants. The present thesis draws on this literature to show how particular ways of framing engagement, even with the best intentions of openness and respect for lay knowledge, can render dominant or marginal particular kinds of voices and impose particular identities and worldviews on participants. I now examine particular science-public encounters and think about how identities can be imposed and challenged when science and publics meet.

Both Wynne and Epstein argue that encounters between science and publics can challenge people's identities. For example, when the Cumbrian farmers saw their knowledge (which they took as highly relevant to the radiation experiments) being dismissed by government scientists, they "experienced their identities to be threatened" (Wynne, 1996c: 20). The hierarchy of knowledge implicit in this dismissal (with farmers' knowledge constructed as less relevant and "expert" than scientists') reinforces a type of social relation between scientists and farmers. Scientists were imposing their unacknowledged cultural assumptions onto the farmers, who felt their status as experts in hill farming went unrecognised. By contrast, the AIDS activists in Epstein's example were aware of research communities and other authorities attempting to impose identities upon them. They looked for ways to challenge this and create their own identities and "social meanings" (Epstein, 1995: 413).

Development Studies also examine how identities can be imposed. In particular, participatory development is concerned with involving local people, from developing countries, in decisions about their livelihoods. It seems to answer many calls for improved public engagement with science, and for the recognition of the importance of local perspectives and understandings. However, this literature also highlights the limitations of participation – see, for example, the articles collected in *Participation: The New Tyranny?* (Cooke and Kothari, 2001a). Henkel and Stirrat's view is particularly salient here. They argue that participation only empowers people to fit the expectations of developed world donors:

> They are being empowered to be elements in the great projects of 'the modern': as citizens of the institutions of the modern state; as consumers in the global market; as responsible patients in the health system; as rational farmers increasing GNP; as participants in the labour market, and so on. (Henkel and Stirrat, 2001: 182)

Thus, participation projects try to remodel participants to fit a particular desirable image and, therefore, they impose particular identities. In this sense, Henkel and Stirrat argue that empowerment, through participatory development, is equivalent to what Foucault calls "subjection" (see below).

These impositions relate to the "performativity" of language. Speech act theory makes sense of how language can change the social world, how it can *perform* certain things. Drawing on Austin (1975) and Searle (1969), who are the principle founders of this theory, Szerszynski argues that:

> Through language and gesture we are performing certain acts which can, in the right circumstances, bring about social effects hermeneutically, through changing the definition of a situation, and of its constituent social relations. (1999: 245)

In speech act theory, discourse can serve to reify certain social relations and identities. That this reification does not always occur does not, in my view, remove the performative aspect of language: the acts are *performed*, whether or not the social world changes. In the context of engagement, discourses and practices of engagement can therefore create situations where only certain types of engagement are possible. We must therefore pay attention to the sorts of identities *performed* by particular engagement practices.

For example, Irwin (2001) examines how a public consultation in the UK constructed particular versions of the "scientific citizen" (such as ignorant and in need of education). He also highlights how the GM Nation discussions, by looking for the "open-minded" and "innocent" citizen, could exclude people with more radical and polarised views (Irwin, 2006: 315). This imposes particular identities on participants. Although Irwin does not use the word "perform", I would argue that these constructions of citizenship (attempt to) reify certain identities and, rather than *describing* a version of reality, they help it into being. These constructions are thus, in my view, "performances of citizenship". Irwin suggests social scientists examine citizenship and engagement exercises in more detail, which the present research does.

One interesting example is Genus and Rogers-Hayden's (2005) study of the New Zealand Royal Commission on Genetic Modification which reveals hidden institutional cultural commitments. For instance, despite a commitment to broad public participation (including religious, Maori, industrial and ecological groups) and to examining a diversity of issues, many voices were not heard in this engagement exercise. Genus and Rogers-Hayden link this to the framing of the exercise. In particular, the template for submissions to the public consultation required participants to treat ecological, cultural, ethical, economic and other issues separately. This "reductionist approach" advantaged informants who viewed science as distinct from the context in which it is done, but disadvantaged informants who had a more “holistic” view of the world and saw science, the environment and humans as interconnected. It made submissions from the latter seem clumsy and disjointed, as they would have needed to explain their ontological framework at the beginning of each question (Rogers-Hayden, 2003: 530). Thus, their views were “trivialised” (Hindmarsh and Rogers-Hayden, 2004: 11).

Goven (2006) argues that by framing the public inquiry in this manner, the Royal Commission imposed a modernist view of science (seen as having predictable consequences on the environment, which are separate from economic impacts and cultural factors). However, this worldview was not acknowledged as such, and was presented as *the way things are*, rather than a cultural commitment. By contrast, arguments such as those made by environmentalists were labelled as “worldviews” and framed as “values-based”. Consequently, the Commission could dismiss voices which it specifically associated with particular worldviews, whilst listening to, and accepting, voices for which it acknowledged no worldview. Goven argues that the Commission justified the dismissal of these other voices labelled as “values” by arguing that, in a “pluralistic society”, the only way for these “values-based views” to be legitimate is to be consensual (2006: 576). As the voices belonging to the modernistic worldviews had no acknowledged values, they did not need to be universally held and could, thus, remain cognitively superior. Here under the guise of being open, this engagement exercise could impose identities: scientific citizenship could only be enacted by people who adhered to this unacknowledged modernist worldview.

The release of the Commission’s conclusions was followed by widespread public protests. highlighting, as does Wynne’s example above, that the imposition of identities can strain science-public relations. This suggests that engagement, if it does not take into account people’s worldviews and fails to acknowledge its own framings, can in fact further alienate publics from science and potentially deepen the “crisis of trust”. The present thesis pays particular attention to how scientists’ discourses can esteem or trivialise the voices of particular publics in engagement contexts and explores related trust relations.

2.3 Conceptualizing, engineering and enabling trust

2.3.1 Institutions and "instrumentalising" trust

As argued in the introductory chapter, scientific institutions are concerned with public mistrust in science. The House of Lords Science and Society Report (2000) seems to want to address the problem of trust through two-way dialogue and transparency. It suggests that:

> Openness is crucial, though it is not a panacea and is not without cost (1.21).
>
> Suppressing uncertainty is bound to lead to frequent changes of dogmatic position, and to the loss of public trust and respect (4.8).

Quotes such as these demonstrate awareness by the Select Committee on Science and Society that trust cannot be obtained by projecting an image of certainty and that transparency and openness are necessary.

However, this tactic of openness and transparency has been criticised by Brown and Michael (2003) and Irwin and Michael (2003), who argue that transparency as a means to obtain trust is unsuitable. Indeed, full transparency is never attainable and agreeing on acceptable criteria for transparency requires different actors to already be in relations of trust[7]. Nevertheless, Irwin and Michael argue that transparency can be used as a rhetorical tool to project an image of suffering and authenticity: actors can show that they are trying to be transparent, and that they are suffering in the process, due to various obstacles. This leads to an authentification of what they are doing:

> [w]hat these spokes-persons seem to be doing is tacitly claiming to make transparent their reasoning […]. It is just that it is their suffering which is made transparent (performed) and which, so to speak, in the final instance, 'authenticates' their reasoning. (Irwin and Michael, 2003: 126)

Here, transparency takes on a *performative* dimension. This highlights the need to ask how widespread and meaningful these calls for openness and dialogue are.

In addition, Wynne notes that these strategies to improve trust cannot ever work:

> it is a contradiction in terms to instrumentalise a relationship which is supposed to be based on trust. It is simply not possible to expect the other in a relationship to trust oneself, if one's assumed objective is to manage and control the other's

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[7] This is reminiscent of Collins and Pinch' "experimenter's regress".

> response. The only thing which one can expect to control, and to take responsibility for, is *one's own trustworthiness* (2006: 218-9, original emphasis)

Wynne suggests that the way towards this, is for science and scientists to become aware of, and openly acknowledge, their social and cultural embeddedness; to become self aware and "reflexive". He suggests that:

> The most fundamental objective for public engagement with 'science' should be to encourage the institutional culture of 'science' to acknowledge and question its own cultural assumptions and commitments. (Wynne, 2005a: 19)

This is something this research seeks to follow up on. I turn to reflexivity in section 2.4. However, before that, it is necessary to explore analytical tools that can help make sense of trust (since expression of trust or mistrust may indicate opportunities or a need for greater reflexivity, as discussed in chapter 6) .

2.3.2 Rational-calculative and hermeneutic trust

Section 2.2 has problematised the dichotomy between "expert" and "lay" knowledge. Nevertheless, we are often in situations where we have to depend upon – trust in – other people's knowledge. In addition, the framing of engagement and the threat to particular identities can lead to mistrust, and (as discussed in 2.3.1) scientific institutions are concerned about public trust in them. Trust is therefore a central concept in studying science-public relations. In the following section, I draw on social theory to show how important trust is in current times. Although some aspects of social theory are problematic (I discuss some critiques of theories of modernity here and in the next section), other aspects – such as the concern with macro-level circumstances and the analysis of "expert"-"lay" relations – can fruitfully be combined with cPUS (my main theoretical framework) – as has been done by others (e.g. Irwin and Michael, 2003).

Trust and dependence between different groups are particularly important in the case of SCR, which can be considered an area of knowledge creation typical of "late modernity" (Giddens, 1990) or "risk society" (Beck, 1992; 1996): it involves large amounts of capital, public-private alliances, collaborations between various international "experts" (in cell biology, law, policy, ethics, medicine), local and global[8] regulations, international trading of

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[8] Global regulations include the United Nations attempts at banning cloning.

human material, as well as the creation of invisible pervasive risks that institutions have trouble coping with.

For both Beck and Giddens, individuals today can no longer rely on their local knowledge and kinship ties to make sense of the world in which they evolve. They become dependent on other forms of knowledge. For Beck, this gives rise to a pessimistic vision:

> [...] the degree, the extent and symptoms of people's endangerment are fundamentally *dependent on external knowledge*. [...] the affected parties are becoming *incompetent* in matters of their own affliction. They lose an essential part of their cognitive sovereignty. The harmful, threatening, inimical lies in wait everywhere, but whether it is inimical or friendly is beyond one's own power of judgement (Beck, 1992: 53-4, original emphasis)

Giddens also argues that individuals can no longer judge risk or understand how everything around them functions. His rather more optimistic analysis sees individuals as relying on "expert systems" which, in a process of "disembedding", offer guarantees of proper functioning: they restructure social relations across "indefinite spans of time-space" (Giddens, 1990: 21). These systems enable people to go about their lives without having to *know* how everything functions (how bridges stay up, for example). For expert systems to serve this function, people must *trust* in them (to prevent bridges from collapsing by overseeing engineering standards). This trust is bestowed without any direct contact between expert system and individuals. Giddens calls these "faceless commitments" "in which faith is sustained in the working of knowledge of which the lay person is largely ignorant" (Giddens, 1990: 88). Thus, trust here depends on faith in the systems to work as they are supposed to. Trust is often renewed, maintained and/or challenged at "access points" where individuals or groups come into contact with representatives of the expert system.

Both Beck and Giddens see that it is not always possible, given the globalised nature of late modern society, to opt-out of modernity and its institutions. People become dependent on these systems and have to trust them which can give rise to "ambivalence". For Giddens:

> Respect for technical knowledge usually exists in conjunction with a pragmatic attitude towards abstract systems, based upon attitudes of scepticism or reserve. Many people, as it were, make a 'bargain with modernity' in terms of the trust they vest in […] expert systems. (1990: 90)

For Beck:

> the risk consciousness of the afflicted, which is frequently expressed in the environmental movement, and in criticism of industry, experts and culture, is

> usually both *critical* and *credulous* of science. A solid background of faith in science is part of the paradoxical basic equipment of the critique of modernization. (Beck, 1992: 72, original emphasis)

Here, Beck notes the presence of ambivalent trust, where conditional trust in institutions is mixed with feelings of mistrust, disenchantment and dependence

Both Beck and Giddens see bestowals of trust as results of rational calculations and evaluations of risk and trustworthiness. They argue that, previously, people trusted experts. But with the increased visibility of expert disputes, lay people have to evaluate the trust-worthiness of different experts and choose which ones to trust. Beck and Giddens seem to think that lay people automatically trust experts in the absence of public disputes and that it is only in the presence of public disputes between experts that the black box of expert certainty is opened, and that lay people have to decide which experts to trust.

This view has been criticised, in particular by Wynne who disagrees with this "notion of public mistrust [which] is instrumental-calculative" (Wynne, 1996b: 47). He argues that Beck and Giddens have fallen for "a widespread confusion between unreflexive trust, and reflexive dependency and private ambivalence" (1996: 48). He develops the concept of "as-if" trust (Wynne, 1995: 381; 1996b: 47-55). Here, people feel they depend on experts/authorities. They do not trust these, but lack the agency to do away, or vocally disagree, with them. Therefore people have to *act* "as-if" they trust them, and hope for the best:

> [People] are aware of their dependency, and of their lack of agency even if the boundaries of this are uncertain; and awareness of these conditions occasions anxiety, a sense of risk, and an active interest in evidence, for example about the basis of their unavoidable as-if 'trust' in those experts. These lay public processes are deeply imbued with reflexivity even though no public dissent or contestation is apparent. (Wynne, 1996b: 50)

Thus for Wynne, the absence of vocal contestation on the part of lay people does not imply actual trust, and the trust that seems bestowed on institutions is only a "virtual" (1996b: 50) trust. For example, the Cumbrian farmers in Wynne's research tended not to tell scientists how much they mistrusted them.

Wynne is therefore criticising Beck and Giddens for equating *expressions* of trust with *actual* trust and advocates a more *hermeneutic* concept of trust. However, Wynne has also been taken to task for equating private expressions of mistrust for actual mistrust. Rather, Szerszynski (1999) uses speech act theory to study the language of trust and suggests that trust can be a "directive declaration". In stating that they trust an institution, people are in fact attempting to project an image on the institution which will encourage it to act as described, and be trustworthy. This is a fruitful way of making sense of trust declarations which are also *performative*.

I am persuaded by Wynne's criticism of Beck and Giddens, and Szerszynski's criticism of Wynne. These raise the problems of analysing *expressions* of trust, mistrust or ambivalence: as a researcher, it is difficult to access what people *really* think. Thus, one should think about whether discourse is a description or a construction of a reality – in other words, if it should be analysed as a "topic" or a "resource" (Gilbert and Mulkay, 1984, see below). Although I disagree with Beck and Giddens' rational-calculative model of trust and their often realist conception of expertise, I find that their notion of dependence on expert systems in a global world (which is difficult to opt-out of) can be usefully combined with hermeneutic conceptions of (mis)trust in order to explore public engagement in stem cell research. In particular, it is interesting to analyse scientists' expressions of trust, since, as Giddens (1990: 146) also argues, they too are "lay" in many aspects of their lives and work, and have to rely on experts systems. In particular, expressions of concern can point to opportunities for greater "reflexivity", which I discuss below.

This chapter so far has suggested that trust and mistrust are central dimensions of science-public relations and need to be explored. However, it has also underlined the difficulty in instrumentally engineering trust through openness or through forms of public engagement that impose problematic identities on participants. Therefore, I now want to examine how people involved in engagement can become more aware of their own framings – how they can become more "reflexive" – and how this can improve public engagement and enable alternative forms of knowledge to be recognised, thus enabling emancipation from traditional power relations.

2.4 "Reflexivity" and emancipation

There have been many calls to empower publics to effect change in the governance of science. However, as discussed above (see 2.2.4), these are often framed around very constrained sets of identities and do not question the central role of science in shaping these science-public encounters. I want to suggest that social scientists can play a role in helping members of scientific institutions to become more aware of their founding assumptions and how these influence interactions with publics; that is how they can become more “reflexive”. This can offer alternative ways of conceptualising and using science in public. For changes to take place, it is crucial to consider how power relations can be transformed to build on this reflexivity. The relational notions of power as developed by Foucault and Bourdieu are most useful in this context. In particular, similar to Epstein, I build on some of Bourdieu’s concepts to analyse possibilities for challenging the dominant framings of public engagement.

2.4.1 “Substantive” and “interpretative” reflexivity

As shown above (see 2.3.1), Wynne argues that scientists and their institutions need to become “reflexive”, rather than try to engineer trust. There are various conceptualisations of “reflexivity” (e.g. Lynch, 2000). For the purpose of clarity, it is important to explain what versions of reflexivity are drawn upon in this thesis[9]. Here, I start with Beck and Giddens as they are central in conceptualising this notion. I then explore how their “substantive reflexivity” might be further developed in order to more profoundly challenge some of the scientistic framings of public engagement. I suggest this may improve science-public relations.

According to Beck, modernity needs to become “reflexive” in order to address the issues it throws up. For him, this implies modernity’s “self-confrontation” (1996: 28) and concern “with its unintended consequences, risks and their implications on its foundations” (2000: 226). This reflexivity is made possible by society’s increasing awareness of the social construction of risks, or “man-made hybrids” (Beck, 2000: 221).

Sociology and other social sciences, for Beck, have a role in promoting reflexive modernization, in the “rupture” from enlightenment or “primary scientization”, to “reflexive scientization”:

> The *agents of rupture* are the disciplines of the critical application of science to itself – the theory of science and the history of science, cognitive sociology and the sociology of science, psychology and empirical ethnology of science and so on – which have been gnawing at the foundations of the self-dogmatizing of

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[9] There are a number of other conceptualisations of “reflexivity”, notably “methodological reflexivity” which I discuss in chapter 3.

> scientific rationality with varying success since the beginning of the century. (Beck, 1992: 165, original emphasis)

Beck contrasts this reflexive scientization with a less desirable "counter modernity", a re-invention of "modernity", where "fascination with the ersatz god of progress" and institutionalised non-responsibility take over (Beck, 1992: 214). Here, there is an opportunity for radical but constructive critiques of science to promote "primary scientization" over "counter modernity", and to open up a role for public engagement in and appraisal of science. This is where some of the aims of this thesis are located, although this work seeks to go beyond Beck, as discussed below.

Beck also sees a central role in reflexive modernization for a variety of "sub-politics", such as ecological movements, citizen or consumer groups, counter-experts and industrial competitors (e.g. Beck, 1996: 32-3; see also Leach and Scoones, 2005: 32). There are many such groups relevant to SCR, with voices, expertise and criticisms coming from a variety of local or trans-national organisations including: feminist movements, religious groups, anti-abortion activists, research institutes and funders, individual scientists or the international "Hands off my ovaries" association. All these groups can form a variety of alliances facilitated by international communication networks. They can have a role in re-framing the discussions around SCR and help scientists confront the assumptions embedded in their work and discourses.

Giddens' version of reflexivity is similar to Beck's in that it suggests that the ever-increasing amount of knowledge available in modern enhances our awareness of the risks created by science. A crucial difference between these two scholars is that, according to Giddens, reflexivity does not need to be a response to challenges from outside science. Rather science, in reflexive modernity, is *inherently* reflexive:

> reflexivity of modern social life consists in the fact that social practices are constantly examined and reformed in light of incoming information about those very practices, thus constitutively altering their character (Giddens, 1990: 38)

Thus, Giddens' reflexivity is supposed to describe what is occurring in late modernity, whereas Beck's reflexive modernity is more of a normative statement of what society should aim for.

There are many critiques of these theories of modernisation (see Wynne, 1996b; Rose, 2000; Scott, 2000). In particular, it has been argued that the differences between early and late modernity are not as striking as Beck and Giddens suggest. In addition, their conceptions of reflexivity – which can be labelled "substantive reflexivity"– privilege scientistic and technocratic worldviews (Lynch, 2000: 28) and is mainly pre-occupied with planning for and addressing unintended consequences. Thus, although Beck's reflexive modernisation seems a promising aim in the context of improving science-public relations (in contrast to counter-modernity), it is insufficient: if self-confrontation is simply concerned with unintended consequences and scientistically framed *risks*, it will not profoundly challenge the current ways in which science-public interactions take place. Since it is precisely this scientistic and technocratic framing that imposes alienating identities on "lay" participants (Wynne, 1993; 1996b), a form of reflexivity that puts into question this framing is needed. Science must acknowledge and explore its frames and worldviews, and how these shape science-public interactions.

Consequently, "interpretative" notions of reflexivity become useful – where reflexivity is concerned with "a style of interpretation that imagines and identifies non-obvious alternatives to habitual ways of thinking and acting" (Lynch, 2000: 32). If science is unreflexive in this sense, this can cause problems and, as Jasanoff argues:

> The danger here is not simply public alienation or apathy in the face of technological change, accompanied by a hollowing out of meaning, intimacy, connection and emotion. Real as these threats may be to the ideal of democratic engagement, more important still is the loss of reflexivity within the scientific enterprise itself, a phenomenon that disables modern science from recognizing, and admitting, how profoundly normative are its visions of progress (Jasanoff, 2005b: 196-7)

Following on from this, the present thesis seeks ways to develop the potential for scientific institutional reflexivity in the sense advocated by Jasanoff and Wynne where reflexivity is:

> the process of identifying, and critically examining (*and thus rendering open to change*), the basic, pre-analytic assumptions that frame knowledge-commitments (Wynne, 1993: 324, emphasis added)

Thus, building on but moving away from Beck, this thesis promotes a more "interpretative" form of reflexivity. "Counter modernity" and "reflexive modernization" are used as idealised categories with which to compare data. Sub-politics and social scientific analyses are seen as playing an important role in critiquing the institutions of science, but even more so, in providing alternative means of representation and interpretation. In keeping with my commitment to constructivism, these alternatives are not put forward as *more truthful* than scientistic ones[10]: they are suggestions of alternatives which may help science improve its relations with publics.

Kerr and Cunningham-Burley (2000) have found evidence of limited reflexivity in scientists' discourses[11]. This thesis further explores the kinds of reflexivity already in evidence in scientists' discourses and seeks ways of further developing these. For example, by highlighting scientists' "pre-analytic assumptions", it may suggest new paths towards (interpretative forms of) reflexivity, so that framings of engagement can be negotiated, rather than blindly imposed on participants. However, in order for the reflexive potential of stem cell researchers' discourses to challenge the framing of engagement, we need to take into account (and also challenge) the existing power relations as they shape science-public interactions in problematic ways. To do this, we need sophisticated tools to analyse power.

2.4.2 Conceptualizing power and emancipation

Development studies scholars draw our attention to power relations and highlight the need to understand these in the context of public engagement. For instance, Hildyard *et al.*, in their case study about forest management in India, found that most consultations reproduce ready-existing local power relations (see also Stirling, 2005) and tend to be dominated by the more powerful groups, usually men from politically strong villages. Other research shows that participation may often do nothing more than reflect the agenda of aid organisations or other external groups (e.g. Francis, 2001), or may reproduce and reify pre-existing power imbalances (Kothari, 2001). Thus it is important to think about how these existing power relations can be conceptualised and challenged if we want alternative frames of meaning to take hold and improve public engagement.

Foucault's theorisation of power is central to many development studies' analyses. For him, power is not a monolithic entity which some possess and others do not (1976: 123). Rather, it corresponds to a "multiplicity of power relations" and strategies whose "institutional crystallisation takes shape in state apparatuses, the formulation of laws and in social hegemonies" (1976: 121-2, my translation). Pervasive and meticulous methods of surveillance and control enable the "subjection" or "assujettissement" (1975:139) of people.

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[10] More details of my epistemological and ontological commitments are given in chapter 3.

[11] See chapter 6.

It is through these unacknowledged – and hard to acknowledge – control and "normalisation" mechanisms that power relations are played out in engagement. For instance, norms such as "women are less dominant than men" can be reinforced through participatory development, rather than questioned.

I find Foucault's concepts of "normalisation" and "subjection" particularly salient to the study of public engagement and how it can, in an unseen manner, impose identities and framings on participants[12]. However, some critiques of his work are of relevance here: it has been argued that Foucault does not suggest any ways to challenge existing power relations and strive for emancipation. Although he notes that "wherever there is power, there is resistance" (1976: 125), he finds that these nodes of resistance rarely create great changes in power relations (127) and he is more concerned with studying small transformations. Feminists in particular have been critical and Harstock (1993), for example, argues that power relations need to be critiqued and transformed by creating alternatives to the dominant cultures. The possibility of resistance or emancipation from dominant cultures is especially salient in public engagement with SCR, as many publics may not feel they have anything to contribute to discussions and may consistently defer authority to "experts" (see Kerr *et al.*, 2007).

Bourdieu's analysis of power offers ways of challenging power relations and Epstein uses this to make sense of how the AIDS activists he studied succeeded in changing the way clinical trial were undertaken. Bourdieu develops the concepts of "habitus" and "field" which makes sense of individual agency[13] within objective structures. "Fields" are objective complexes of socially and historically contingent relations. "Habituses" are:

> systems of durable and transposable dispositions, structured structures predisposed to function as structuring structures, that is as principles generating and organising practices and representations that can be objectively adapted to

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[12] Foucault is also a central figure in analyses of power in medical sociology. Nikolas Rose has developed an interesting re-interpretation of Foucault's work in this context, which centres on doctor-patient relations (e.g. Rose, 2001; Rabinow and Rose, 2006; Rose, 2007). However, he focuses on how Foucault's work is useful to make sense of normalization forces *on the body*. This interest in biomedicine, in the "somatic individual" and its "fleshly corporeal existence" (Rose, 2007: 26) and in "neurochemical selves" (Rose, 2007: 187) is less useful when thinking about public engagement in SCR; particularly when one wants to challenge framings of engagement that tend to privilege scientists enrolling patients as desperate for treatments (e.g. Parry, 2003b).

[13] Although Bourdieu has been criticised for not allowing for agency (see Knorr-Cetina, 1982; Williams, 1995), I find that his framework does enable it particularly when it is combined with insights from social identity theory (see section 2.4.3). However I do take on board Knorr-Cetina's view that macro-level claims such as those made by Bourdieu need to be empirically investigated at the "micro-level" (1982: 116), which this thesis seeks to do.

> their goal without the necessity of the conscious aiming towards these ends or of the express mastery of the operations necessary to attain them [...] (Bourdieu, 1980: 88, my translation)

These dispositions – norms and ways of thinking for example – are infused into people and, thus, become *embodied* dispositions. The field structures the habitus, and the habitus gives meaning to the field (Bourdieu and Wacquant, 1992: 127). People act according to the habitus and the field. In particular, if the structure of the field maps onto internalised norms (habitus), the social order seems given by nature (1980: 229-30; see also Cronin, 1996); this can impose identities on people, unbeknownst to them. According to Bourdieu, emancipation from these embodied dispositions is possible, in particular through the scientific "field" – which includes social sciences for Bourdieu.

Fields are the products of historical processes, but due to constant struggles are also constantly changing (Bourdieu and Wacquant, 1992: 104). Although the most dominant agents in a given field have an interest in keeping particular areas "outside the *limits* of the struggle" (Bourdieu, 1975: 109, original emphasis), other agents can question and change what is at stake in the struggle:

> The primary locus of resistance to power on Bourdieu's analysis is not the political field but the scientific field, since scientific representations of social practices can dispel the mystifications underlying symbolic domination by revealing the arbitrariness of the social divisions it serves to legitimate. (Cronin, 1996: 76)

For Bourdieu, the scientific field can reveal the arbitrariness of the structure of the field, and therefore of power relations. This enables particular power relations to be challenged and transformed.

However, it is necessary for these revelations to be credible. This depends on the "symbolic power" one can mobilise, where symbolic power:

> is defined in and by a definite relation that creates belief in the legitimacy of the words and of the person who utters them, and it operates only inasmuch as those who undergo it recognise those who wield it (Bourdieu and Wacquant, 1992: 148)[14]

This power comes from having different levels of "cultural capital" which, in the scientific field for example comes from publications, peer recognition and the struggle for the

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[14] Bourdieu's symbolic power is similar to Gieryn's notion "epistemic authority" which can be achieved through boundary-work, as discussed below.

authority to speak on scientific matters and impose the definition of what counts as science. This capital can be converted into other kinds of capital, such as when scientists with authority in the scientific field reconvert – successfully or not – this authority in the "political field"[15] or in policy, and try to impose their definitions there too.

In terms of engagement, Bourdieu's work is very salient. Indeed, many viewpoints, particularly "lay" objections, can be excluded from debates for not fitting the frameworks of debates (see 2.2.4). The importance or relevance of lay knowledge is not commonly part of the habitus. Thus science, including social science, by revealing the embodied dispositions and the structure of the field, can show that the accepted social order (for example, that natural scientific voices have more authority and power than others) is not given by nature and therefore can be challenged. This directly relates back to Wynne when he argues for the role of social scientists in examining (therefore opening up to change) the assumptions that frame knowledge (see 2.4.1).

Therefore, I believe that a good way of "empowering" lay people in public engagement is to convert their capital into capital recognised in the scientific field. Epstein finds that this is what his AIDS activists could do (1995: 415): they used their high levels of cultural capital (their access to good education for example) and converted it into capital that made them credible in the eyes of scientists (they learnt how to understand scientific papers and talk using scientific jargon). Even better, one can resist the technicist framing of public debates by redefining the rules governing the structure of the field and making a new kind of capital gain credibility in the field of interest. In Epstein's study, AIDS activists made the access to a social good (the clinical trial), which can be constructed a "moral" (rather than "scientific") credibility, become important in the conduct of clinical trials (1995: 420-1).

It is important to note here that, following Epstein, I diverge from Bourdieu in the way I see the "objectivity" and "truth" of the alternatives that social science can reveal[16]. Indeed, for Bourdieu, "scientific truth" exists and is knowable. Wacquant argues that Bourdieu, even though he thinks that knowledge needs to be deconstructed and that science is "an eminently political activity", is also "passionately modernist": he sees the progress of reason as leading to further autonomy in the scientific field and better access to truth. (Bourdieu and

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[15] Bourdieu makes a difference between the "political field" and political aspects which are present in all fields.

[16] This is mentioned in 2.4.1 and will be further discussed in chapter 3.

Wacquant, 1992: 67)[17]. Thus, in Bourdieu's conceptualisation of emancipation, there is an idea that the alternative frameworks offered by social scientists, for example, will be *more truthful* and more objective than the unexamined and unacknowledged ones. Here, his reflexivity can be labelled "reflexive objectification" (Lynch, 2000: 30). In contrast, I do not think that social scientists have a better access to truth or are more objective than other people interested in public engagement. I think they can offer an alternative lens through which to view the framing of engagement, reveal assumptions made by scientists and help reveal the arbitrariness of the status quo, thus open up ways of changing it. To put it in Bourdieu's terms, their role is to offer ways of challenging the "rules of the game" (Bourdieu and Wacquant, 1992: 98-99) in the "field" of public engagement.

Thus, using the same framework as Epstein, I draw particularly on Foucault's notion of power as relational and exercised through small normalising forces, and on Bourdieu's ideas that the rules of the game in any field can be challenged through the revelation of the arbitrariness of the structure of the field, and that there are different kinds of capital which can be mobilised and converted into others.

2.4.3 Social identities

Previous sections have highlighted how identities can be imposed by, and challenged during, encounters between science and publics. However, participants in these exercises (be they scientists or publics) can still decide, to an extent, which identities to enact. I now discuss "social identity theory", which provides an analytical framework to understand the contingency of action and discourse.

Social identity theory derives from social psychology and is a useful conceptual tool for this research[18]. It is "intended to be a social psychological theory of intergroup relations, group processes, and the social self" (Hogg *et al.*, 1995: 259). It studies the psycho-social links between structure and agency:

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[17] In this, I agree with Dreyfus and Rabinow's sympathetic critique of Bourdieu where they argue that: "the meaning of human action is not accessible to a scientific theory" (1993: 35) and that "one must abandon the claim to have a scientific sociology" (1993: 43).

[18] Psychological approaches have been criticised for being reductive (e.g. Wynne, 1995). However, despite its roots in social psychology, social identity theory expressly distances itself from other approaches in psychology which focus solely on cognitive processes (as I discuss in the body of the text).

> Social identity is intended as a concept that mediates between social context and the action of human subjects. It is not seen as a psychological reality that determines social reality. (Reicher, 2004: 933)

Social identity theory provides mechanisms for understanding identity at an individual level, whilst taking into account broader socio-cultural factors (including belonging to a profession, nationality, gender etc.). It specifically attempts to avoid cognitively reductionist accounts of people's identities (Hogg *et al.*, 1995). For example, Reicher argues that "one of the basic tenets of the social identity tradition is that identities are cultural constructs" (2004: 937) and "it is necessary to avoid reductionist misreadings of the theories that would explain human social action simply by reference to psychological processes" (2004: 921). Thus, although some of the methods used (with "dependant variables" and "experiments" in "laboratories") are quite different to what I to do in my thesis, the broader concepts are relevant to my work and can be combined with my constructivist framework.

Social identity theory develops the concept of "salience"[19]: people can draw on a number of identities, and the one they enact depends on the actual situation, the audience, the identities at their disposal (Stets and Burke, 2000: 231). In particular, identity salience depends on who is perceived to be the group one wishes to belong to – the "in-group" – and the group one wishes to be differentiated from – the "out-group" (Hogg *et al.*, 1995: 260; see also Stets and Burke, 2000: 225). Social identity theory has been extended into "self-categorization theory":

> according to self-categorization theory, people cognitively represent social groups in terms of *prototypes*. A prototype is a subjective representation of the defining attributes (example, beliefs, attitudes, behaviours) of a social category, which is *actively constructed* from relevant social information in the immediate or more enduring interactive context. (Hogg *et al.*, 1995: 261, emphases added)

This notion of "prototype" explains why certain categorisations give a stereotypical view of others and self, as can be the case during episodes of "boundary-work" (discussed below). Importantly, the prototype and identity depend on which out-group the particular identity construction is addressed at.

Social identity theory also highlights the notion of identity as "reflexive"[20]:

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[19] Which is different to the more static notion of salience developed by "identity theorists", from the symbolic interactionist tradition (see Hogg *et al.*, 1995 for a comparison of these two approaches).

[20] This notion of reflexivity is more restricted than the interpretative reflexivity that I favour. However, awareness of and confrontation with one's identities and self-categorisations contribute

> In social identity theory and identity theory, the self is *reflexive* in that it can take itself as an object and can categorize, classify, or name itself in particular ways in relation to other social categories or classifications. (Stets and Burke, 2000: 224, emphasis added)

Thus, although identity options are to an extent constrained by the physical world, people can draw on different identities in different circumstances, without this being simply an unconscious reflex predetermined by their social locations[21]. They have a certain degree of choice in which identities they enact and identities can create social situations and effect change. However, similar to the Cumbrian farmers, identity negotiations are not always straightforward, and trust and power also are a central aspect of the encounter between science and publics.

Social identity theory enables accounts of power relations and can make sense of unacknowledged normalizing forces – such as those inculcated through the habitus – and of more noticeable, coercive forces. "Subjection" will be more noticeable and felt as more conflictual if the two groups or individuals do not share common salient identities and do not see themselves as belonging to the same in-groups (thus explaining why people feel their identities are being threatened). Although some groups may, for example, be recruited to help others, this does not mean they are incapable of challenging this. People can use their identities to change the social world, in particular if they have access to identities where they have a higher status:

> If dominated Bs have access to systems of identities that are external to the system of domination and associated identities, they can draw on alternative interpretative horizons [...]. As a consequence, the identity as Bs may eventually be re-anchored and re-constructed in ways that transcend and challenge the prevailing social order. In addition to this cognitive resource and its contribution to cognitive liberation [...], alternative identities also provide access to valuable social and material resources (for example new allies and their funds), which should help to further loosen the grip of the original domination. (Simon and Oakes, 2006: 129)

This is similar to the situation where people can effect change when they see the arbitrariness of the habitus but, here, a particular mechanism for change is revealed.

Thus, a possible explanation for Epstein's case study, discussed above, is that the AIDS activists had a low status identity in clinical research. However, they had access to high

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ones awareness of embedded assumptions and worldviews, thereby contributing to further interpretative reflexivity.

[21] Although this does not imply that all identities are conscious choices, or rational calculations.

status identities (in their role as businessmen, for example) and found the situation where they were in a weak position unnatural. They could see that this social order was amenable to change and they effected this by the process of “expertification”, but also changed the structure of the “field” of clinical trials by reconstructing it such that they had a higher status identity and their understandings were relevant. They converted some of their capital into capital relevant in clinical trials. They also altered what was considered to be relevant capital by mobilising resources and alliances. Thus, as Reicher highlights:

> These definitions of identity are projects, not descriptions. They are intended to mobilize people to create social reality. (2004: 938)

Therefore by revealing the arbitrariness of power relations and of the structure of the field, social scientists can help empower people, particularly by showing them that they have access to other identities than the ones imposed on them. This can not only encourage “lay” people to engage more actively with science, but also encourage scientists to change their discourses and the identities they enact (they do not *have* to evince certainty in public, for example).

2.5 Studying scientists and their discourses and rhetorical strategies

Much of the literature I have discussed criticises projections of particular identities, epistemologies and frameworks onto publics. However, similar projections are made onto scientists and institutions (such as their obsession with prediction and control). This thesis investigates whether this is accurate for the scientists I interview, and how this relates to the maintenance of cognitive authority. For example, it explores whether their epistemological commitments are different to publics’. It also explores how scientists, in particular through their discourses, shape encounters with publics. In this section, I discuss the main analytical tools I use for studying scientists and their discourses.

2.5.1 Boundary-work

As Bourdieu notes, part of the symbolic power of a scientist comes from his/her ability to define what counts as “science”:

> The definition of what is at stake in the scientific struggle is thus one of the issues at stake in the scientific struggle, and the dominant are those who manage to

> impose a definition of science which says that the most accomplished realization of science consists in having, being, and doing what they have, are, or do. (Bourdieu, 1975: 96)

No author has done more on this struggle about the definition or boundaries of science than Gieryn and his cultural cartography of science[22].

Previous sections have shown that scientific claims are socially constructed and that the notion of expertise is highly problematic. Despite this, science holds a lot of cognitive authority and often frames engagement exercises. As this special location of science in society is not given by the nature of scientific knowledge, it is important to think about the construction of legitimacy, credibility or authority. One way to do this, suggested by Gieryn, is to look at how science sets itself apart from other disciplines or social groups.

Gieryn moves beyond traditional essentialist criteria for the demarcation of science from non-science (e.g. Merton, 1972). He chooses to investigate how scientists erect boundaries between what they see as "science" and as "non-science" and how the construction of these boundaries can contribute to scientists' authority. He does this by introducing the concept of "cultural cartography": "a mapping out of epistemic authority, credible methods, reliable facts" (Gieryn, 1999: 4). Gieryn finds that people of opposing views draw out different representations or "maps" of science. Each map justifies why science is special. Gieryn calls this process "boundary-work', which is:

> the discursive attribution of selected qualities to scientists, methods, and scientific claims for the purpose of drawing a rhetorical boundary between science and some less authoritative residual non-science (1999: 4-5)

It is important to note that Gieryn is not interested in who is "right" or "wrong", or which science becomes vindicated by history (this idea of symmetry is shared by SSK scholars in general). Rather, boundary-work corresponds to the *rhetorical* construction, de-construction and re-construction of boundaries. This is where interesting notions about science can be revealed and the creation of epistemic authority can be seen. Gieryn suggests that "the public better learn about 'science' through contrasts with 'non-science'"(1983: 791). He follows

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[22] Bourdieu's use of fields is much narrower than Gieryn's notion of scientific disciplines. Bourdieu is also talking about "idealised agents" rather than real people, thus his meanings for forces and struggles are different to those of Gieryn. Nevertheless, these two approaches can be used in conjunction since Gieryn's cultural cartography can be one way of examining how actors rhetorically attempt to set what is within and out with the limits of a field. Also, Gieryn, like Bourdieu, sees a constraining role for the structure of the field, or what Gieryn calls the "peculiarities of each of these arenas" (1999: 24).

this idea through his various episodes of boundary-work; by looking at the discourses of key figures in scientific arguments, he identifies four types of reasons for which boundary-work is conducted .

Firstly, boundary-work can be done to “expand” the authority of science. Science is opposed to other disciplines in a flattering manner; this enables the authority of science to extend to spaces formerly occupied by other intellectual or practical activities. Gieryn gives the example of the boundaries erected between science and religion and mechanics, by Tyndall in Victorian England (1983: 783-7).

Secondly, Gieryn describes boundary-work conducted for “monopolization” of resources or authority away from other activities. In this case, people who want to be regarded as scientists exclude competitors from within, in order to keep hold of the authority science already has. Gieryn explains that do this, rivals draw alternative maps of science, including themselves and their practices, but excluding their opponents as non-scientific or *pseudo-scientific*, amateurish or fake. Gieryn shows that this type of boundary-work was done by supporters and opponents of phrenology in Edinburgh (1983: 788-9). He notes that different definitions for particular terms were given by each group. These mapped onto broader political struggles and social locations in which the cultural cartographers were situated.

Gieryn describes a third reason for boundary-work: “expulsion” of individuals from a field. This type of boundary-work is similar to monopolization except that particular members are rejected from science, rather than a whole field being excluded[23]. In both cases, pejorative labels are one of the discursive tools used by the insiders. The main objective of this boundary-work is to protect authority by not being associated with individuals or practices that are seen as non-scientific. Expulsion has also been done to stem cell researchers claiming to work towards human reproductive cloning (Parry, under review).

The final type of boundary-work that Gieryn studied is aimed at the “protection of autonomy”. Here, scientists draw boundaries to prevent outsiders from invading science and using its authority either to make science responsible for things scientists feel are not within their remit, or to enable outsiders to become responsible for things that scientists see as their preserve. The protection of autonomy can be done to separate science from politics, industry,

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[23] In his book, Gieryn in fact uses the term “expulsion” to describe the battle between two intellectual activities .

or from its consequences (1983: 789-91). When science protects its autonomy from infringement by other areas like politics, it becomes important for scientists, and indeed for politicians, to keep science close to, but not within, the realm of politics, so that science can maintain its role as neutral adviser. This relates back to Nelkin's discussion of expertise as a strategic label (see 2.2.3). It can also be useful to define arenas which are between, but not part of, science and politics (or industry etc.). Jasanoff, for example, examines the role of expert advisory committees:

> such bodies keep alive the politically useful fiction that science is apolitical and that scientific advisers are capable of 'speaking truth to power'" (Jasanoff quoted in Jasanoff, 2002: 374).

These arenas can be appealed to by politicians and scientists alike to maintain the authority of science.

When science protects its autonomy by separating the generation of "facts" from their application, scientists can set up a duality within science – as pure and applied – and draw on it. For example, by asking scientists about the social environment in which they work, Kerr *et al.* show how scientists distance themselves from the implications of their findings (such as the use of genetic data by insurance companies) and at the same time accept some form of responsibility towards society (by advising governments and educating the public in a "neutral" fashion). By doing effective boundary-work and using the fluid "micro/macro split" (see the following section), these professionals can maintain their authority. This contrasts with Kerr *et al.*'s (1998b) discussion with lay groups, which challenge these discourses and their given authority.

Cultural cartography is a useful way of studying scientists in the context of public engagement and fits well within my overall theoretical framework, despite some of Gieryn's language that at times seems to indicate realist claims[24]. This concept has been used, for example, to study stem cell researchers (Wainwright *et al.*, 2006a) and clinicians (Ehrich *et al.*, 2006). In both the papers, the authors describe how professionals, when discussing their work (for example when they explain why they are happy to work on some sources of stem cells but not others), draw on considerations that the authors label "ethical" considerations, and how professionals use this to try to position themselves as "ethical". The authors of these

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[24] For example, Gieryn states that: "science *is* both pure and applied, theoretical and empirical" (1983: 792, original emphasis). Rather, I would say that science *can be experienced and constructed* by cultural cartographers as pure and applied, theoretical and empirical.

two papers call this practice “ethical boundary-work” and argue that, contrary to usual boundary-work which increases the authority if science, this boundary-work “enhances the authority of ‘non-science’[…] and de-privileges science” (Wainwright *et al.*, 2006a: 735). This use of boundary-work is slightly different to mine. Indeed, I want to avoid imposing analysts’ categories of what counts as “science” and “non-science” and I would not want to argue that science is essentially different to ethics or legislation, for example. I would interpret this “ethical boundary-work”, not as de-privileging science, but as privileging a construction of science as an ethical pursuit.

Gieryn’s draws several general themes from his studies of boundary work. Firstly, he notes that episodes of boundary work only occur in times of struggle, when there is competition for funding or when the authority of science is at stake. Indeed, when someone makes claims that go against a particular scientific field’s ideas, s/he is not always “expelled” but usually ignored if resources (pecuniary or cognitive) are not at risk.

Secondly, Gieryn highlights that boundaries are very fluid and permeable; they include different characteristics depending on who is drawing them and when, for which purpose and for what audience[25]. Thus, different repertoires are used in each instance. The maps of science are created *de novo* each time. Although the cartographer may be inspired by previous maps, s/he includes only the features s/he wants at that particular moment. Thus, this thesis will highlight the social locations of individuals drawing different maps of SCR. These first two themes reveal that boundaries are constructed in response to external pressures as well as internal interests.

Thirdly, Gieryn draws attention to the fact that these maps give us an insight into how science can be understood in our society. Like its boundaries, science can be seen as variable and ambiguous. The different discursive repertoires highlight the (socially constructed) dual nature of science: basic science as opposed to applied science. By looking at the boundaries that my informants draw around SCR in general, and around particular areas within SCR, the constructed nature of this field is revealed. This highlights the ways in which stem cell researchers seek to promote their authority.

Lastly, Gieryn highlights the importance of winning these episodes of boundary-work.

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[25] This is similar to arguing that particular identities become “salient” in particular social circumstances.

> The *stakes are very large* for cultural cartographers themselves, for their particular version of nature hangs in the balance. [...] Losers see their claims moved out from fact to illusion, lie, ulterior motive, or *faith* while they (and their methods, practices, organizations, and institutions) get marginalized or excluded fully from the domain of epistemic authority reserved for science and its genuinely licensed practitioners. To the victors, go the spoils of successful cultural cartography: not only do their claims become real enough for others to act on them, not only is their *authority to make truth provisionally sustained*, but they enjoy (for a while anyway) the soaring esteem, cascading influence and possibly abundant material resources (cash, equipment, bodies-and-minds) needed to make *still more truthful tales*. (1999: 13-4, my emphases)

Similarly, in SCR, there have been calls to stop work using embryos, and replace it with "adult" SCR, which some consider more ethically sound. Thus, if a map is stabilised, where embryo research is outside the boundaries of "good science", the whole future of embryonic SCR would be at stake. This ability to have one's utterances of "truth" recognised, at least temporally, as such is reminiscent of Bourdieu's analysis of symbolic capital above. Like Bourdieu, Gieryn notes that this "epistemic authority" is not something that exists objectively, rather, is constructed and acted out socially. This thesis examines how my informants locate and act upon authority, to which knowledge claims they assign the label "truth", and how this impacts on engagement. This must be done without imposing an analyst' view of what science is or should be, as suggested by Gilbert and Mulkay' (1984) version of discourse analysis.

2.5.2 Discourse analysis

Both Bourdieu and Foucault direct our attention towards discourses[26] as reflections and constitutions of power relations. Discourse is also the focus of a variety of social studies of science, including the rhetorical strategies used in cultural cartography, as we have examined above (see also Mulkay, 1993; Wright, 1993; Michael, 1996; Irwin, 2006). Therefore, I am interested in scientists' "discourse", which for me is concerned not only with "linguistic utterances" (Bourdieu and Wacquant, 1992: 146) – which can be written as well as spoken –

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[26] Foucault does not use the term "discourse" in English in the works I discuss above, and his use of that label in his earlier works – on which I do not draw – is too restrictive and linguistic for my use of this concept (see Sawyer, 2002, for a detailed critique of Foucault's use of the term "discourse"). Nevertheless, my use of the label "discourse" corresponds well to the sorts of power relations and normalising influences that Foucault discusses in the works on which I draw. In addition, he uses the term "discours" in French for example in "Histoire de la Sexualité 1" (1976). Therefore Foucault has made me think about "discourse", not in the sense he uses in English, but in a broader sense, which is relevant to this thesis.

but also with the social context in which they take place – that is the relation to other utterances[27], who they are aimed at, the power relations that are being played out, whether the speaker is seen or not to have the authority to make these statements, the identities taken on by the speaker of attributed to him/her etc. My ontological, epistemological and methodological commitments, especially in relation to discourse, are set out in more detail in the following chapter on my research design. Before turning to that, I want to end the present chapter by discussing the main analytical tools used in this thesis.

Gilbert and Mulkay's book "Opening Pandora's Box" (1984) has been very influential in social studies of scientists' discourses and is a central inspiration for my work on stem cell researchers. For Gilbert and Mulkay:

> Discourse analysis […] is the attempt to identify and describe regularities in the methods used by participants as they construct the discourse through which they establish the character of their actions and beliefs in the course of interaction.

Gilbert and Mulkay break with previous sociological tradition in that they suggest the importance of studying how scientists talk, before using what they say as resources to build a picture of the social world (see also Mulkay and Gilbert, 1982; Mulkay *et al.*, 1983). They advocate that the study of discourse as a "topic" is "methodologically prior" to that of discourse as a "resource" (1984: 8). I follow Gilbert and Mulkay's approach in that I use discourse as a "topic" [28]. However, following others such as Kerr *et al.* (1997), I also use discourse as a "resource"[29] and I am also interested in dimensions beyond discourse, such as power and institutional settings[30].

To investigate scientists' discourse, Gilbert and Mulkay look for regularities and interpretative flexibilities in their talk. They uncover that scientists use two main modes of accounting for the social world, using the "contingent repertoire" and the "empiricist repertoire":

> We call this repertoire the 'empiricist repertoire' because it portrays scientists' actions and beliefs as following unproblematically and inescapably from the empirical characteristics of an impersonal natural world. (1984: 56)

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[27] See, for example, the concept of intertextuality, coined by Kristeva when relaying Bakhtin's work on the relation between utterances (discussed in Fairclough, 1992)

[28] By studying discourse as a "topic", I can also examine the *performative* aspects of language.

[29] This is true in particular at the beginning of chapter 5, although I come to the realisation that using discourse as a resource is limited.

[30] My ontology and epistemology, including a focus on discourse analysis, are set out more explicitly in chapter 3.

> When this [contingent] repertoire is employed, scientists' actions are no longer depicted as generic responses to the realities of the natural world, but as the activities and judgements of specific individuals acting on the basis of their personal inclinations and particular social positions. (1984: 57)

Gilbert and Mulkay find that the empiricist repertoire is very common in scientific publications, whereas the contingent repertoire is more common in informal accounts. Similarly, stem cell researchers' use of various repertoires and interpretative devices will be explored. In particular, examples where scientists accept contingent factors as intrinsically linked to the production of scientific knowledge must be explored, since, like Wynne, I am advocating more (interpretative) reflexivity on the part of scientific institutions and looking for ways to incorporate lay knowledges into decision making around science.

One role of the contingent repertoire described by Gilbert and Mulkay is that it enables scientists to "account for error". They use the empiricist repertoire to describe their own actions, and the contingent repertoire to explain the error in others' beliefs which are contrary to their own (1984: 72) Gilbert and Mulkay find that this "asymmetrical counterposition of empiricist and contingent versions of action and belief" (1984: 82) reinforces the idea that the speakers beliefs are "true", that is uncontaminated by contingent factors.

The "truth will out device" or TWOD (1984: 90-111) is another key rhetorical strategy found by Gilbert and Mulkay, whereby "temporality is used as an interpretive device". TWODs are examples of "reconciliation devices" as they enable present uncertainties, which are often attributed to contingent factors, to be treated as unproblematic as they will inevitably be resolved in the future, when "truth" will emerge and researchers' empiricist conceptions of the world can be reinforced. This strategy reinforces a notion of the "truth" as insulated from contingent factors that can cause errors; thus, even if a speaker is in error, s/he can have faith that eventually "truth" will shed any contingent contaminations and be revealed.

An additional finding by Gilbert and Mulkay is that "consensus" is "best conceived as a contextually variable aspect of scientists' discourse" (1984: 112). It enables participants to augment the authority of their own view by suggesting it is simply a consensus view, and that anyone who thinks about it properly would come to the same conclusion.

Using a slightly adapted version of Gilbert and Mulkay's discourse analysis, Kerr *et al.* (1997) reveal another interpretative device: the "micro/macro split", used by new human genetics professionals. This device enables professionals to maintain the claim that "good science" is neutral and objective – at the micro, empirical level – even if contingent factors can be detected at the macro level of its generation and application. This division is permeable: there may be contingent influences on research itself – like the areas funded at particular times or the selection of what experiments should be done first – and scientists may enter the social realm – but only to give objective advice. Thus, the flexibility of the "micro/macro split" enables scientists to maintain their authority and acknowledge the existence of contingent factors, but also highlight that these do not affect the final outcome of scientific research: "objective truth".

All these interpretative devices serve to heighten the speaker's authority, or in Bourdieu's words, it increases their "symbolic power" by "creating belief in the legitimacy of the words and of the person who utters them" (Bourdieu and Wacquant, 1992: 148). As noted by Kerr *et al.* (1997), accounts of power relations are missing in Gilbert and Mulkay's analysis of scientists' discourse. This thesis, in contrast, pays particular attention to how informants maintain or generate authority in their utterances and enable particular power relations.

Gilbert and Mulkay's version of discourse analysis has also been criticised by Gieryn (1982: 291-2), who argues that social science would become impoverished were it to focus solely on discourse as a "topic". He argues for the need to look at science within its institutional context, including power relations and social norms. Acknowledging this critique, but accepting the analytical significance of discourse as a topic, the present research relates discourses to broader contexts such as parliamentary debates. It examines scientists' accounts of different aspects of SCR, how they discuss engagement and how they dynamically construct categories such "good science", "expertise", "lay knowledge" or "embryo", and what implications these constructions and categorisations have for public engagement in science.

2.6 Conclusion

These diverse literatures inform the present thesis. They help problematise the "lay"/"expert" dichotomy and open up roads towards (interpretative) reflexivity in science and improved science-public relations. However, two provisos must be made before turning to the research design. Firstly, as Michael and Brown (2000) highlight, it is very easy and appealing to impute "deficits" to others (deficits of scientific, sociological, or political knowledge, of trust, etc.). This thesis does more than point out scientists' deficits in reflexivity (for example), it also investigates other dimensions of their identities and shows that they are, like other social beings, full of complexities and ambiguities. Secondly, engagement is not put forward as a panacea. This follows suggestions from development studies research, for example:

> Ironically, though, authentic reflexivity requires a level of open-mindedness that accepts that participatory development may inevitably be tyrannical, and a preparedness to abandon it if this is the case. Thus any meaningful attempt to save participatory development requires a sincere acceptance of the possibility that it should not be saved. (Cooke and Kothari, 2001b:15)

Thus, although the present research is conducted with a commitment to engagement, this is not to suggest that engagement is either necessary or sufficient for the resolution of all science-public relation problems.

Chapter 3

Research design:
Studying scientists and their discourses

3.1 Introduction

This thesis is concerned with public engagement in SCR and stem cell researchers' engagement with publics. It examines stem cells researchers' discourses around these themes, as a way of exploring constructions of SCR, expertise and scientific citizenship.

This chapter discusses my research design. It starts with the research questions and how they came about. It then focuses on the epistemological framework and theoretical commitments guiding the research. It then discusses data collection and data analysis, before concluding with a reflecting upon this study and its limitations. This research was undertaken in an iterative fashion, where phases of data analysis informed subsequent phases of data collection. Thus, the separations between the sections in this chapter, done for the sake of clarity, are somewhat artificial.

3.2 Research questions

3.2.1 Background and initial decisions

The research design developed gradually throughout my thesis, shaped by previous education, guidance from supervisors, interests and findings from earlier phases of data collection and data analysis.

I was registered to do a traditional laboratory-based PhD after my research Masters in Human Genetics. When my science supervisor, Veronica, gave me the opportunity of doing a more ethico-sociological PhD, I seized upon it. Given the chance to focus of a variety of subjects, I chose SCR. One reason for this was a particularly novel stem cell paper I had come across during my Masters year. It was considered ground-breaking at the time (by myself and various science journals) because it suggested that stem cells from the bone marrow could give rise not only to various types of blood cells but also to a range of other cells types including skin, nerve and liver cells (see Jiang *et al.*, 2002). This suggested that cells found in adults were much more versatile than previously thought. Another reason for choosing SCR was that it can be seen to include recent work on cloning (such as the creation of Dolly the sheep) and the derivation of stem cells from human embryos. Therefore it seemed like a very exciting area of scientific research, which also raised issues of public concern.

Early decisions about my thesis occurred at a time when many research institutions, including my funding body (the MRC), were trying to improve their public communication strategies. I remember my first conversation with Sarah, who was to become my social science supervisor, when I talked about researching ways to improve public "understanding" of the promise of SCR (needless to say that conversation would be different today). We discussed her research on publics and their sophisticated understandings of SCR. This was the start of my rethinking of science-public relations and led to my focus on two way engagement, rather than one-way communication. At this point, I realised I was in a privileged position, having access to scientists through my contacts from my earlier training and "speaking their language". I settled upon studying relations between science and publics by looking at scientists' accounts, especially since scientists have a central role in public engagement.

3.2.2 Questions and analytical framework

Whilst thinking about the main themes for my thesis, I started doing more reading around SCR and sociology, and following some STS courses. This helped develop my research questions. Starting with the "crisis of trust" identified by the House of Lords (2000), this research examines "public engagement" and its multiple meanings. Although there is a need for social scientists to highlight that lay people have relevant and legitimate contributions to make to science, it is also important to do more that that, including examining scientists' discourses about engagement. Using a variety of sociological tools, this thesis studies the fluid science-public interface, since this is a social location where interesting power, authority and trust negotiations take place. My thesis addresses two main research questions:

1. *How do scientists talk about their views, understandings, and experiences of publics, engagement, and SCR and the issues surrounding this field?*

This research question raises four inter-related themes:

i) The construction of the stem cell field

This theme examines what counts as "SCR" for some researchers and what does not; what is raised as the greatest promise of SCR or the most concerning issues; how definitions of different types of stem cells are given; how the history of the stem cell field is rendered or how it's futures are imagined; and whether some scientists acknowledge that their field may be constructed.

ii) The construction of publics

This theme looks at the roles envisaged by scientists for different publics; how publics are defined and contrasted, or assimilated, to scientists and experts; how science-public relations are played out and discussed; and the sorts of knowledges, expertises, emotions or fears projected onto publics.

iii) The purpose of engagement

This studies the meanings that scientists give to engagement; what aims they describe for it (such as promotion of research, education, raising problems etc.); how they would "improve" it; who they see as having a role in it; whether there is a move away from the deficit model; and how engagement, legislation and decision-making are, or should be, related.

iv) Reflexivity

This thesis specifically looks for opportunities for "reflexivity" and "emancipation" from existing power relations (as discussed in 2.4). It investigates how scientists view themselves; whether they confront the consequences of their work and the mutual shaping of science and society; and whether there is a move towards "reflexive scientization" (Beck, 1992: 165) and more "interpretative" (Lynch, 2000) forms of reflexivity (see section 2.4.1).

2. *What implications do these discourses have for public engagement in science and scientists' engagement with publics?*

This theme arose during data analysis and is the focus of the last two substantive chapters. The way in which scientists conceive of, define, label and project images of, publics, engagement and SCR, shapes how science-public relations can take place. If publics are seen as uncritical vessels for information, the routes to more inclusive engagement will be different to those possible if publics are seen as active agents in the construction of science. Similarly, scientists' views of engagement, and of the sorts of "capital" (for example technological expertise) acceptable in public discussions, will shape science-public relations. We should also think about how opportunities for reflexivity can be used to change how public engagement is done.

There are *three main analytical foci* to explore these research questions:

1. *Factors shaping these discourses and views*

As Kerr *et al.* (1998c: 179) suggest, social locations influence professional concerns. These locations include the extent to which people's identities and situatedness (such as research field, seniority, gender, experiences of engagement, or contact with publics such as patients and patent lawyers) play a role in scientists' discourses of public engagement in SCR. The impact of national settings (different legislations, history of science-public relations, political and ethical environments) is particularly relevant here, since science is at once local and international. This can highlight some factors that could help improve dialogues between scientists and publics.

2. *Strategies to maintain authority*

These comprise the language and labels used by scientists in different settings (for example in private or in public, with peers or with other types of "experts"); the discursive tools they deploy to maintain authority; the boundaries they erect; and how authority, legitimacy, trust and power are negotiated. A better understanding of rhetorical strategies can provide an insight into scientists' concerns. It can also suggest, for example, how certain features of "private" discourses can be brought out into "public".

3. Expertise, trust and contextual knowledge

The different epistemological frameworks used by scientists to make sense of the world around them are explored. The analysis teases out ways in which particular forms of knowledge are legitimised by scientists, who is given the status of "expert" and how relations between experts and lay people are negotiated. Occasions when scientists portray themselves as "lay", and need to rely on other "experts", are also explored. This provides insights into relations of trust between various actors and can suggest ways in which alternative types of knowledge can be respected and used in public discussions and policy-making.

This thesis seeks to speak to social and natural scientists since both have an interest in science-public relations. The initial responses from talking to ex-colleagues, as well as the support the MRC has displayed for this project, support this assertion. In addition, the fact that I was supervised by a natural scientist as well as a social scientist helps ensure this research stays relevant to both groups.

3.3 Epistemological and theoretical frameworks

3.3.1 Ontology, epistemology and methodology

As a scientist by training, I came to this project with a positivist approach and was expecting to uncover part of the unique reality that exists outside of human activity. However, this viewpoint ignores the importance of investigating how particular forms of knowledge become accepted as "fact" and others not; and how power and authority are managed. Informed by my various readings and classes, I now take the position that all forms of knowledge are socially constructed and consider myself an epistemological relativist. However, I reject ontological relativism and the concept that there is no unique external reality. I believe that there is a common physical world, which exists independently of our knowledge of it, and that "[t]he real world is culturally filtered as meanings are constructed" (Jones, 2002: 248). I think the investigation of what shapes the construction of meaning can help solve problems in the physical world.

Although we cannot directly *know* the world (by observing it or by using reason), we make sense of it: we classify it, we impose meaning on it, we make knowledge claims about it and we shape others' claims about it. These activities are achieved through *discourse*, in the sense adopted by Wetherell from Mouffe and Laclau, where discourse is: "the unceasing human activity of making meaning" (Wetherell, 1998: 403). Thus, discourse plays a central role in constituting us as social beings. Following Potter and Wetherell, whose work builds on and extends Gilbert and Mulkay's, this research takes an approach to discourse analysis which:

> is concerned with talk and texts as social practices [...] has a triple concern with action, construction and variability [...] [and has a] concern with the rhetorical or argumentative organization of talks and texts (Potter and Wetherell, 1994: 48).

This definition of discourse is broader than that used by conversation analysts and structuralists, for example (critiqued in Wetherell, 1998), or even by Gilbert and Mulkay, whose focus on "accounts" is quite restrictive[31].

Discourse is seen as a *creative*, rather than a *descriptive*, activity. This conceptualisation enables me to explore what my participants say and how this can construct different versions of reality or impose particular worldviews. However, I do not claim to access what any of them *really* think; like many of the researchers discussed in the literature review, this PhD is not concerned with the truth content of particular utterances, but in how utterances are made and what implications they may have.

One of my main sources of data was interviews. Interviewing is currently a very prominent method of qualitative data collection in the social sciences and some argue that we live in an "interview society" where the interview is assumed to be central in accessing and making sense of our *real* identities (Atkinson and Silverman, 1997; Silverman, 2001: 22). However, in keeping with my epistemology, I think that an interview cannot exactly reflect private thoughts (see Michael, 1996: 97) and one should not ignore the "creative activities" and "biographical work" undertaken by interviewer and interviewee (Atkinson and Silverman, 1997). To reiterate, interviews are not used to access who my informants *really* are and what they *really* think. Rather, they are used to access informants' *accounts* of events and the implications of these. Interviews are similar to naturally occurring conversations in that they often consist of retrospective accounts in which informants make sense of, and re-order, the past in light of the present (Silverman, 1973). Thus, they are a good means of investigating scientists' interpretative practices (Gilbert and Mulkay, 1984; Potter and Mulkay, 1985). In

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[31] I further discuss how I draw on Gilbert and Mulkay's frameworks but expand them in section 3.5.1 below.

addition, interviews are not “culturally unique” (Kerr *et al.*, 1997; see also Wetherell, 2003: 13) and many of the researchers interviewed had previously been interviewed by journalists or had given evidence in parliament. Nevertheless, one-to-one interviews provide access to data not otherwise obtainable because focussed discussions give rise to more data on a specific topic than naturally occurring talk (Rapley, 2004: 29). Interviews are also useful in that they reveal practices and repertoires, used in these more private settings, which can be reproduced in public to improve science-public relations.

It is also important to look beyond particular instances of discourse. Indeed, as suggested by Wetherell, I take into account the situatedness of the various discursive processes:

> productive and constructive discursive processes [are seen] as extending way beyond the activities in the immediate conversation. The constructive process emerges historically. Past and current collective negotiations organize the spaces (physical, institutional, and symbolic) in which conversations take place, for example, as well as the ways in which people and events can be represented within them. (Wetherell, 2003: 24)

Therefore, I complement accounts from interviews with discourses found in other arenas such as scientific journals, the news media and parliamentary debates, and consider each instance of discourse in their broader institutional, historical and cultural contexts.

3.3.2 Symmetry, “methodological” reflexivity and infinite regress

The epistemological relativism I have discussed above has specific methodological counterparts. In particular, the present research aims to be “symmetrical in method” (Bloor, 1976). This is the third tenet of the Sociology of Scientific Knowledge. It means that all beliefs are analysed in the same sociological fashion, whether they correspond to what is currently held to be “true” or “false”. The notion of symmetry is particularly pertinent for a study of science where there are disagreements amongst stem cell researchers about the promises of various types of stem cells. My analysis is “impartial” (Bloor, 1976) to the truth or falsity of knowledge claims and, rather, investigates why and how particular constructions are mobilised in particular circumstances, and what implications this has for engagement.

Another central tenet of SSK is that it should be “reflexive” – what Lynch would call “methodological reflexivity” (2000: 29-30): that is, its explanations of how scientific knowledge claims come about should also be applicable to social scientific knowledge claims (Bloor, 1976). Bloor responds to those who argue that this form of reflexivity causes SSK to become “self-refuting” by stating that this criticism is only convincing if one believes that social construction equates with error, which for him is not the case. (Bloor, 1976 [1991]: 16). Likewise, I see the knowledge claims that I make as socially contingent. Nevertheless, they can have implications for the practice of engagement, since they offer alternative interpretations and frames of meaning (as discussed in 2.4). Thus for me, relativist arguments can be applied to social scientific findings without undermining them.

A similar *tu quoque* argument has been made against discourse analysis: how can discourse analysts deconstruct and analyse accounts, when their findings will, themselves, be conveyed in particular accounts which can also be deconstructed (discussed in Fuhrman and Oehler, 1986; Potter, 1987)? Many discourse analysts respond to this by focussing on reflexivity and thereby reflecting upon how analysts’ own texts are created. Some even try out new forms of academic writing that eschew the standard, single-voice authoritative account, via so-called “new literary forms” (discussed in Potter and Wetherell, 1987; e.g. Woolgar, 1993). Although I agree with Woolgar and others that this focus on reflexivity and the creation of the analysts’ accounts is interesting and a “means of signalling an opportunity for theoretical sensitizing” (Woolgar, 1992: 338), I do concur with some critics of this work who find that this radical form of reflexivity can lead to a “relativist regress” causing us “to have nothing to say” (Collins and Yearley, 1992: 302), or at least nothing *interesting* to say. As Barber argues:

> No amount of reflexive writing about the way sociologists write about science can pretend to replace writing about science itself and will only generate endless volumes of initially interesting but eventually sterile prose. (1992: 114)

Following this, whilst remaining aware of the constructedness of the knowledge claims – and of the accounts of the knowledge claims – made in this thesis, my research is more focussed on how *scientists* construct their claims and what these claims *do*.

3.3.3 Values and political stances

The above discussion on reflexivity has highlighted the importance of reflecting on one’s knowledge claims, methodological and epistemological choices, and on the constructedness of one’s accounts. Part of this involves thinking about one’s values and political aims.

No research can be separated from the researcher's values, experience, background and so on and be "value-free" (Gouldner, 1962). Therefore, although the present research takes a methodologically symmetrical and impartial approach, it steps away from some SSK scholars who argue that we should try as far as possible to be value-free (e.g. Collins, 1996): my views have inevitably been challenged, changed and shaped by this study and I cannot pretend that I am neutral with regards to what is said about SCR in public. This does not mean that one must "side with the underdog" as suggested by other SSK scholars (e.g. Scott *et al.*, 1990) and feminists (e.g. Wilkinson, 1999), since the underdog is not always that easy to identify (see Ashmore, 1996; Collins, 1996; Wynne, 1996a). Nevertheless, I am aware that my research may be "captured" (Scott *et al.*, 1990) by some stem cell researchers or publics and I also briefly reflect on this in the concluding chapter.

So, rather than attempting to be value *free*, I try to be value *aware*. One strategy for this is to make one's assumptions explicit. This position has been advocated by many feminists (e.g. Stanley and Wise, 1993; Maynard and Purvis, 1994). I do this at three levels. Firstly, I try to make my (ever-changing) views and feelings about SCR and engagement explicit to myself. This involved writing a research diary and thinking about my views during interviews and during the analysis process. Secondly, I shared some of my views and feelings with my informants before interviews by providing them with information concerning my background and thoughts about engagement. Thirdly, I include biographical details and my theoretical commitments in the present thesis and in other contexts where I present my research findings.

In addition to this, while I am committed to an impartial analysis, I cannot shy away from political implications of my research. Following Wetherell, we cannot claim that analysts have a better access to truth than participants[32]. Nevertheless, we do have important contributions to make since: "In the past these forms of knowing derived from scholarship have been *useful* and *powerful*. (Wetherell, 2003: 25, emphases added). I believe that sociological analyses of scientists' discourses can have important and normative implications for public engagement in science.

In a similar vein, Winner (1993) has highlighted the crucial political role scholarly research can play. He criticises the idea that one can *describe* different interpretations – of artefacts in

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[32] This differs from Bourdieu, as discussed in 2.4.2.

his example – and not be concerned with power and the implications of some interpretations becoming fact and not others. He argues that:

> It is necessary for social theorists to go beyond what positivists call *value neutrality* and what constructivists resurrect as *interpretative flexibility*. One must move on to offer coherent arguments about which ends, principles and conditions deserve no only our attention but also our commitment. (Winner, 1993: 374)

Following Winner, the present work is considered to be inherently and unavoidably political: by pointing out alternative framings for public engagement, for example, I am making a political statement that these statements are worth pointing out, and might be worth adopting. My work specifically seeks to offer alternatives to the way public engagement is done and, in the conclusion, will even make specific normative suggestions as to how engagement could be improved.

3.3.4 A grounded theory-inspired approach

Overall, this research project is data driven. It draws on certain methodological principles of grounded theory, with data collection and analysis feeding into each other. I do not however adopt grounded theory's realist epistemology, instead following constructivist adaptations of this approach.

Grounded theory is an approach to qualitative research developed by Glaser and Strauss (1967) that involves an iterative process of constantly going between empirical data collection and analysis in order to generate theories *grounded* in data. As Pidgeon (1996) argues, grounded theory is a methodology, but also an epistemological stance. Epistemologically, some grounded theorists argue that there are *true* interpretation of the world which can be revealed; that is, they believe in the "*discovery* of theory from data" (Glaser and Strauss, 1967: 1, emphasis added). This original formulation of grounded theory has evolved, with Glaser becoming more interested in theory "construction", and Strauss (publishing with Corbin) in theory "discovery" (discussed in Heath and Cowley, 2004) and hypothesis testing (discussed in Pidgeon, 1996). Strauss' focus in particular does not fit well with my epistemological commitments.

In keeping with my epistemology, I prefer to follow constructionist revisions of grounded theory and focus on the strengths of its methodology, rather than on its more problematic realist epistemology. Constructionist versions of grounded theory highlight the “creative dynamic character of the research process” and the importance of the researchers’ “perspective from which they seek to build their analyses” (Pidgeon, 1996: 83, see also Potter and Wetherell, 1994: 51), which fit in well with my own epistemology. In addition, some of these revision suggest focussing more on discourse analytic methods (Pidgeon, 1996: 83) such as the approaches I favour (e.g. Gilbert and Mulkay, 1984; Potter and Mulkay, 1985; Potter and Wetherell, 1987; Potter, 1996).

From grounded theory, I adopt an interest in “sensitizing concept”. This notion was introduced by Blumer to describe a concept that “gives the user a general sense of reference and guidance in approaching empirical instances” (1954: 7). It highlights the need for researchers to have theoretical insights to help in their data analysis. Here, sensitizing concepts include the construction of expertise and knowledge, “boundary-work”, power, authority and “trust”. These concepts guide the coding and analysis of my data but do not dictate the themes that emerge. From grounded theory I also adopt a commitment to “constant comparison” between data and analysis (Glaser and Strauss, 1967; see also Pidgeon, 1996: 77-80; Pidgeon and Henwood, 1996; Heath and Cowley, 2004) and “theoretical sampling” (see below). My research is thus very iterative and data collection and analysis happen concurrently, with initial phases of data collection informing and shaping subsequent ones.

3.4 Data collection

My overall strategy is to study accounts of stem cell researchers in and from different social locations and to explore the implications of these. Some of my choices are outlined above but the methodological decisions that came later are discussed here. The main sources of data are interviews with 48 stem cell researchers (31 in Australia and 17 in the UK) and two multi-disciplinary discussions (in the UK), bringing the total number of stem cell researchers investigated to 54.

3.4.1 Multiple sites of data collection

1. *Who and what?*

Stem cell researchers from different social locations are studied. Researchers of varying levels of seniority are spoken to: research assistants, PhD students, post-docs and group leaders. By accessing dominant as well as marginal discourses, this research reveals differences between, for example, "media-savvy" senior researchers and their less experienced colleagues. Researchers involved in human as well as animal SCR are included, partly because few scientists have licences for human SCR, and partly because both these types of SCR are closely related, with findings from one feeding into the other and vice versa. This research also looks at scientists involved in adult and embryonic human (and mouse) SCR, in order to explore whether the dichotomies between these two areas, set up in the media, are reproduced in other social locations. I also speak to a couple of researchers involved in immunology, which is directly related to the clinical application of SCR. In addition, the study sample includes purely academic researchers, as well as ones involved in commercialisation, because of public concerns about this (Parry, 2003b).

The scientific research environment is often considered male dominated and I was planning on specifically trying to speak with women as well as men researchers. However, the selection of people for interviews or participation in the multi-disciplinary discussions tended to be based on people's topic of study, level of seniority and geographical location (and of course on their availability). Nevertheless, approximately one third of my interviewees are women and there are more women than men in the multi-disciplinary discussions.

2. *Studying two countries*

The beginning of my research design followed the decision in the USA not to allow the federal funding of research that destroys embryos, including embryonic stem cell research. This led to warnings in the scientific press of an exodus of scientists from the USA to the UK where embryonic stem cell research is permitted (see Science, 2001) and highlighted the impact of legislation on research. I therefore opted to study scientists in countries with different legislative frameworks. There is a limit to what can be achieved in the timeframe of a PhD and two countries is a good number for doctoral research.

The first country I chose was the UK, Scotland in particular, because it is an internationally recognised centre of excellence in SCR, and more pragmatically, Edinburgh was where I was studying and knew scientists. The second country was Australia, chosen for a variety of reasons. One is that, like the UK, it is a leader in reproductive technologies[33], is at the forefront of SCR (e.g. BBC News, 2001) and is one of the few countries to have derived embryonic cell lines approved for use in the USA (NIH, 2001). In both the UK and Australia, there is also an institutional commitment to including public voices in some decisions around science: for example, in the UK, the Human Fertilisation and Embryology Authority regularly consults publics; and, in Australia, the National Health and Medical Research Council welcomes submissions from the Australian community on a variety of issues.

An important reason to focus on the UK and Australia is that, although they have similar systems of governance, they had, at the time of my data collection, different legislation with regards to SCR. In the UK, scientists could create embryos and then cell lines using cloning techniques, whereas those in Australia could not[34]. This situation changed when the Australian legislation was amended to allow the cloning of embryos for research purposes. My Australian interviews took place in the lead up to the reviews of the cloning legislation. This means that in the UK, the battle to allow the creation embryo for research had essentially been won, whereas in Australia, it was about to be fought again.

Although data were gathered in two countries, this thesis does not systematically compare scientists in Australia and the UK. It is not aiming to interview “equivalent” people (same gender, level of seniority, media experience and research topic) in the two countries, in order to uncover the precise factor that creates a difference in opinion. It is, instead, looking at a variety of researchers, in a variety of settings, to explore some differences and similarities. Therefore this research is not either a case-study, with each country being one “unit of analysis” (Yin, 1984: 29). I find that in order to address my research questions, it is more useful to think in terms of multiple instances of science-public interactions.

From these studies, I am not hoping to draw “grande generalizations”, but “petite generalizations” (Stake, 1995: 7); that is, small refinements to my findings and further insights. Even a two-country study focussing on scientists’ discourses can shed light on notions as complex science, publics and engagement, and conclusions relating to the UK and Australia can be generalised to most developed countries. Similarly, findings derived by

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[33] For instance, Melbourne witnessed the world’s first IVF pregnancy under Carl Wood’s supervision (De Kretzer *et. al*, 1973), donor insemination program (Leeton and Backwell, 1976) and frozen embryo baby (Trounson and Mohr, 1983) (for other examples, see http://www.monashivf.edu.au/about/milestones.html).

[34] See chapter 4 for more details on these legislations and on how stem cells are derived.

focussing on SCR can be expanded to other sciences that are particularly relevant to public engagement.

3. Access, sampling and timing

Potential informants were accessed by: directly contacting researchers I knew from working in science; attending scientific conferences or public talks where I would introduce myself, provide a four page summary of my background and PhD; sending out letters with details of my research to people I had not met and following up with an e-mail; contacting people recommended by earlier interviewees; and asking group leaders if I could interview some other members of their team.

Thus, a combination of "theoretical", or "purposive" (Silverman, 2001: 250-4; Gobo, 2004: 448-9), and "snowball" (Gobo, 2004: 449) sampling was adopted. That is, informants were selected based on their relevance to the research questions and on suggestions by other informants. After a few interviews and initial rounds of data analysis, thus after "constant comparison", new participants were identified. This was done until reaching "theoretical saturation" (Glaser and Strauss, 1967: 61-2).

Data collection started in March 2004, with the first multi-disciplinary discussion. This was followed by a few pilot interviews. I then went to Australia from September 2004 to March 2005 and conducted my Australian interviews. Upon returning to the UK, I conducted some more interviews. Data collection ended in September 2005 with the second multi-disciplinary discussion.

The timing of my research corresponds to a unique set of circumstances. In addition to the interesting circumstances with regards to the Australian legislative changes, the data collection was also marked by high profile publications and retractions in the field of SCR. The first set of events and publications relate to the ability (or not) of some adult cells to turn into other types of cells (for more details, see chapter 4). The second set relates to the promise of "therapeutic cloning" (or the ability to create patient-specific stem cells lines by cloning cells from an adult person, see chapter 4). In February 2004, a paper announcing the proof-of-principle of human therapeutic cloning was published by a team in South Korea (Hwang *et al.*, 2004, retracted). In May 2005, the same team published a paper showing they had managed to drastically improve their cloning efficiencies, suggesting that therapeutic cloning could quite easily be used routinely in the clinic. However, these exciting publications were followed by a string of events[35] culminating in the retraction of the papers in January 2006 (Kennedy, 2006), due to all the findings being fraudulent[36]. All my Australian interviews and the first half of my UK ones took place after the publication of the first Korean paper. The other half of my UK data collection took place after the second publication and before the retractions. The promises and uncertainties raised by my informants would probably be different were my data collection to take place today.

4. Scientists in multiple social locations

Stem cell researchers were spoken to alone (in one-to-one interviews), in peer-groups (in group interviews) and in more "public" settings, with people from other specialisms (in multi-disciplinary discussions). This is complemented by reading some of the transcripts of the Australian parliamentary discussions about SCR as well as the submissions sent in during various phases of the legislation amendments. I also keep up with the scientific literature, attend scientific conferences and public discussions, watch televised debates about SCR and follow some of the international changes of legislation and discussions about SCR. Qualitative methods are used because they are the best suited to answering my research questions. Indeed, I am not interested in the percentage of stem cell researchers that are "very supportive of", "supportive of", "opposed to" or "strongly opposed to" public engagement. Rather, I want to explore scientists' discourses in detail and think about them in terms of knowledge construction, maintenance of authority and public engagement.

3.4.2 Multi-disciplinary discussions

Two multi-disciplinary discussions were organised in the UK. These are a sort of cross between focus groups and science-public interactions.

The first multi-disciplinary discussion was my first point of data collection. It was entitled "Stem cells, Scientists and Publics: Generating a Dialogue" and took place over an afternoon. Twelve people participated (including the chair) and I observed. Many of the participants knew each other. There were:

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[35] For a 2005 summary of this, see Nature News (see also Check and Cyranoski, 2005; Bionews, 2006; Normile, 2006; Park *et al.*, 2006; Snyder and Loring, 2006 for more details and more recent updates).

[36] There have also been many problems with unethical conduct and accusations of embezzlement (for more details, see SNU Investigation Committee, 2006)

- three stem cell researchers,
- three people working as researchers or administrators in a scientific institute with an interest in SCR,
- five social scientists whose work focuses on reproductive technologies, science and technology studies and/or public engagement,
- one lawyer[37].

There were eight women and four men, and there was one person in an early stage of their career and eleven in later stages (group leaders, senior lecturers and professors). Participants were allocated particular seats in order to encourage them to mix. This discussion took place at INNOGEN and was co-sponsored by INNOGEN and the MRC Human Genetics Unit. It was audio and video recorded.

One stem cell researcher and one social scientist began the afternoon with two talks. The former opened the discussion with an introduction to SCR, in particular its technical aspects and promises for clinical and pharmaceutical applications. The latter followed by a reflection on the constructions of publics, ignorance and risks, and on earlier embryos debates. Each talk took 25 minutes, which left 5 minutes at the end of each for questions of clarification. We then broke out for tea. This enabled participants to digest some of the information they had just received and chat amongst themselves. We then reconvened for a more in depth discussion. This focussed on the "strands for discussion" (see appendix 2) given on a hand-out distributed at the beginning of the afternoon and on some of the issues raised in the question time before the break. The discussion was very lively and raised many issues. Shortly before the end of the afternoon, the chair brought the discussion to a close and asked the participants to share their thoughts about the event.

The second multi-disciplinary discussion was my last point of data collection. It was entitled "Stem cells, Scientists and Publics: Continuing the Dialogue" and took place over a morning. In the first instance, I sent an e-mail invitation to people who had participated in the first multi-disciplinary discussion, asking them their availabilities. After finding the most suitable date, I sent letter invitations to new participants. There were fifteen participants:

- six social scientists,

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[37] These separations are to an extent artificial as many of the social scientists are former natural scientists and many of the scientists had an interest in public engagement, often participating in various engagement exercises themselves.

- four stem cell researchers,
- two scientists,
- two lawyers,
- one ethicist.

There were twelve women and three men; and three people early in their career and twelve at later stages. Again, participants sat round a large table, but I did not give them set placements. The event took at the MRC Human Genetics Unit and was co-sponsored by INNOGEN and the MRC. It was audio recorded.

The format was slightly different. I chaired and, rather than opening with two talks, discussions were encouraged using prompts, such as a speech made by Lord Winston about the hype of SCR, headlines from various scientific publications and discussions in Nature about whether or not publics should have a say in which scientific research projects get funding. Tea, coffee, fruit and biscuits were available throughout to encourage a relaxed atmosphere. As in the first event, I closed the discussion by asking everyone to give their thoughts about the morning. All the participants seemed to enjoy both discussions. Many came to see me afterwards and told me so. These events are interesting to analyse as a process of public engagement, albeit participants were all elites.

In both cases, although there were particular topics that I wanted to focus on, the participants were also able to raise their own issues and concerns. In this sense, these multi-disciplinary discussions were similar to focus groups where participants have some control over the conversation topics (Wilkinson, 1999: 70) and which:

> are ideal for exploring people's experiences, opinions, wishes and concerns. The method is particularly useful for allowing participants to generate their own questions, frames and concepts and to pursue their own priorities on their own terms, in their own vocabulary (Kitzinger and Barbour, 1999: 5)

However, my participants inhabited different social worlds, which at times meant that there were communication problems due to people using specialist vocabularies.

These group settings can often be difficult to use with elite professionals. Indeed, Kerr conducted a focus group with senior health care professionals and claimed it was the "the most difficult research interaction she ha[d] ever encountered" (Cunningham-Burley *et al.*, 1999: 196). However this sort of approach is necessary to explore power relations and science public interactions. In addition the same authors note that "focus group research can be harnessed to challenge the power of experts and contribute towards greater public engagement with policy decisions" (2000: 186). This certainly was the case here, where there was no sense of "non-scientists" deferring to stem cell researchers, and where many scientists commented upon how much they had learnt.

3.4.3 One-to-one interviews

I conducted 37 one-to-one interviews: 24 in Australia and 13 in the UK; 14 with women, 34 with men; 10 with PhD students, 4 with post-docs and 23 with group leaders (or senior managers). The academic versus commercial divide is more difficult to establish as many researchers hold patents, are on advisory boards of companies and work in laboratories that are part government and part industry funded. The interviews ranged from 40 to 105 minutes, with most lasting approximately 60 minutes. They took place in the scientists' place of work (either private offices or common rooms), except for one that was over the phone. Out of all the people I approached, the overwhelming majority accepted to be interviewed. This is unusual and may indicate participants were interested in the topic of this research.

Each interview began by me highlighting that, although quotes would not be attributed to people, complete anonymity could not guaranteed, due to the small size of the stem cell community. I would then ask informants if they accepted to be interviewed and recorded. For the first four interviews, consent was obtained orally, but for subsequent interviews, informants signed an informed consent form which also asked to state whether or not they wanted a copy of the transcript or a summary of the findings (see appendix 4). Approximately one third of informants wanted a copy of the transcript and the findings, one third wanted a copy of the findings only and one third wanted neither. Informants also received a summary of my research. This covered my education, the topic, objectives and methodology of my PhD, some points made by the critical Public Understanding of Science literature and the name of my supervisors. I added their contact details after the suggestion from one participant (see below).

The topic guide, which evolved throughout my research, started with the same question to all participants, concerning their work and its relation to SCR. The rest of the interview was organised in themes (legislation, engagement, commercialisation etc.) and used prompts (quotes from other interviews, quotes from scientists in the press, legislative guidelines) to help the discussion. The topic guide was rarely followed in order, or the questions asked with the exact same wording. Indeed, my questions often followed directly from what my informants had been talking about. When they used particularly interesting expressions I would make note of these and return to them if/when an appropriate moment came up.

3.4.4 Group interviews

Three group interviews were conducted: one with senior researchers (one woman and two men) and one with junior researchers (three women and one man) in Australia; and one with junior researchers (one woman and three men) in the UK. All three interviews lasted about one hour.

The senior researchers were a well established pre-existing group in that these three people worked together and spent a lot of their spare time together too. They chose to be interviewed in a group rather than individually. The second group interview in Australia was with some of their students. I specifically requested a group setting as the day was drawing to a close and I felt I would obtain more information from four students in a group, than one or two spoken to individually. These researchers told me that, although they worked in the same laboratory, they were not used to talking to each other about the issues surrounding their work. Nevertheless, the interview went well and again, participants were able to challenge each other's views and refine their positions.

These first two group interviews were sort of ad-hoc and I had not planned a different set of questions for them. However, as they were a good data collection method, especially with younger informants, I decided to organise another group interview in the UK. I arranged for a room, brought cakes and juice, and developed a slightly different topic guide with specific prompts. I was planning for this group interview to be a focus group with participants talking to each other, rather than to me. However, the room we were in was long and thin; this meant that the four participants were sat in a line opposite me, making conversations amongst themselves difficult. Thus, it was difficult to encourage them to create their own questions and raise issues of importance to them. Nevertheless, the prompts did generate interesting discussions between the participants.

3.4.5 Background activities

The above data were analysed in the context of some more public discourses. I observed stem cell researchers in scientific conferences and public engagement exercises. Although I was not always able to interview scientists I had seen talking in public, it was very useful to bear in mind the sort of discourse made in public when trying to make sense of those made in a more private setting. I also followed submissions made to the Australian Parliament by some of the scientists I had interviewed. This data was not included in the thesis, but helped me make better sense of the interviews. I also presented some of my findings to scientists and made notes of the comments raised in response to this. I also read any media articles that discussed stem cell related issues; I gained knowledge of many of these through a weekly e-mail from Bionews.

In addition, I kept up with the scientific literature by closely following the contents of: Nature, Nature Cell Biology, Nature Medicine, Nature Genetics, Nature Biotechnology and Science. I also set up automatic alerts to with key words such as “embryonic stem cell” or “human stem cell” which kept me up to date with relevant articles in journals such as Cell, Stem Cells and Journal of Cell Science. This selection of journals means that I am not problematising the peer-review system, or the hierarchies set up by scientists; I am not specifically looking at publications in non-peer reviewed journals or low impact journals.

3.4.6 Reflections on data collection

1. Consequences of working with a small community: anonymity and access

One of my main concerns with this project was the difficulty of maintaining anonymity in such a small community as that of SCR. Indeed I had read about a study done in a small community where participants had subsequently identified the quotes from every other participant (Robson and Robson, 2002). Thus, I knew that I could not guarantee anonymity and I highlighted this in the summary and orally at the beginning of each interview. Most participants responded by saying that they either did not care what people thought since their views were well known, or that they did not think any one would have problems with what they would say. If I did ask them something they did not want to talk about they either went “off the record” (see below) or removed it from the transcript that I sent for checking (this only happened once).

Another problem that comes from working with a small community is precisely the small number of potential participants. Although I was aware of this when I started my research, I did not realise how much competition there would be for stem cell researchers in the UK. It seems journalists, and various social scientists flock towards them. I had to extend my UK research from Scotland to England and Wales. However, thanks to good response rates from Australian interviewees, I was easily able to conduct sufficient numbers of interviews. Thus, in retrospect, I was very lucky to do some research outside the UK. In addition, the multi-disciplinary discussions were quite popular and I thankfully did not have to fight for participants.

2. *Creating a rapport with "elites"*

Accessing and studying elites is notoriously difficult (Hertz and Imber, 1995; Puwar, 1997; Cunningham-Burley *et al.*, 1999; Undheim, 2003). The scientists studied, particularly the senior ones, as well as the people invited to the multi-disciplinary discussions, can all be considered elites in that they all occupy "a key place in power networks" (Undheim, 2003: online). I have already discussed some of the means by which I gained access to stem cell researchers for interviews and multi-disciplinary discussions. I think it was easier for me to organise the multi-disciplinary discussions in the UK than a group discussion with senior researchers in Australia because I had support from institutions that participants were familiar with and many of them knew (of) my supervisors.

Once I had access to these elites, I aimed to create a rapport in which participants not only felt they could open up to me, but also respected me. One aspect of this was to highlight that participants could stop the interview at anytime and should not feel obliged to answer any question they were uncomfortable with. In particular, I insisted on showing participants where the "stop" button was on my recording device. Most laughed, telling me that they would not be revealing anything that secret. However, two interviewees did ask me to stop the recording to answer some questions "off the record" and others did comment during or after the interviews that they had ended up talking about topics they had not really thought about before and revealed more of their opinions than they had expected. The fact that no-one contacted me to be removed from the study, or removed large sections of transcripts sent back to them, indicates that they did not subsequently change their mind about their revelations.

In order to gain respect and not be patronised, I was also keen to demonstrate that I could "do" science and didn't need to be lectured. Thus, my four page summary contained a few lines from my CV detailing my education and work experience in natural sciences (genetics in particular, see appendix 3). I also made sure I was wearing closed shoes, as open-toed ones are not allowed in laboratories for health and safety reasons. In addition, if someone started explaining something to me in very simplistic terms and was getting away from what I was interested in talking about, I would quickly make a comment or ask a question which "showed off" my understanding. I tried to renegotiate the power relations between researchers and myself by highlighting my knowledge. I also tended to use "technical" terms such as blastocyst or SCNT (somatic cell nuclear transfer), rather than embryo or therapeutic cloning, especially at the beginning of the interviews. This can be problematic as I am reinforcing a language that black-boxes many of the issues I want to examine. To deal with this, as the interview progressed, I would ask specific question relating to the language of SCR.

Highlighting my identity as a trained scientist could be problematic as some informants may have assumed that, as a scientist, I was necessarily on their "side" and wanted to find ways of educating publics for example. However, I also highlighted my identity as a social scientist. I made clear in my summary, my oral introduction and during my questioning, that I was trying to be more critical of the participation agendas. As Ostrander (1995: 149) suggests, I made my objectives clear to "establish some control over the research situations from the outset". There was one interview however where I found it quite difficult to steer the conversation. It was at the end of the day, one of my last interviews in a week averaging three to four interviews a day. The informant was very chatty and kept going off topic. As I was suffering from interview fatigue, I let this interview turn into a conversation. It was fascinating but there was not much data I could directly use. Following that, I no longer undertook such concentrated episodes of data collection.

3. *A difficult phone interview*

One of my interviews was rather unpleasant. It was the only one that took place over the phone. I think the fact that we were not face to face contributed to my difficulty in establishing a rapport. For most of the interview, the informant treated me like a journalist and tried to second guess why I was asking questions and how I would distort his words. When I asked him about informed consent, he refused to answer and instead dissected my consent form, telling me it was inappropriate because it did not contain contact information of my supervisors for people to complain if need be. To try and regain control of the interview, I started explaining some of my questions, highlighting that I was not a journalist but a social scientist. For example, I explained that I was interested in who he wanted to include in public consultations not to criticise him if he forgot someone, but to try and get an idea about who scientists think are relevant and legitimate voices in debates around SCR. By the end of our interview, he was much nicer and stopped answering my questions with pre-prepared sentences full of provisos. After this experience, I decided to concentrate on face-to-face interviews, even if that meant more travelling.

4. Interviewing students and young researchers

When I interviewed junior participants, the power relations were different to when I interviewed senior researchers. In particular, I accessed many of them by interviewing their supervisors and asking if they would mind me speaking to their students, assistants or post-docs (there were some exceptions when I targeted particular students I was interested in). This raised an issue of consent and coercion as participants may feel they had to be interviewed because their supervisors had agreed. Therefore I made it clear that they were under no obligation to participate. We could either just have a chat or they could go back to the lab. In addition, I spent more time explaining my project because they would not have received my summary in advance. All the students I spoke with however seemed quite happy to participate.

3.4.7 Data presentation

1. Transcriptions

I transcribed the data for all my interviews and discussions (except: two interviews where I forgot to turn the microphone on; one interview and part of the second multi-disciplinary discussion that were transcribed by another person to help me save time; one interview where the informant chatted a lot and rarely addressed my questions, and one interview during which a fire alarm went off making the transcribing an extremely unpleasant – I only transcribed relevant sections in these cases).

The interviews and discussions were recorded on mini-disks. During the transcription process, I created track marks approximately every five minutes of discussion. That is, I divided the initial unique track corresponding to the whole interview into multiple tracks, each about five minutes long. I then noted the track number alongside the transcripts. This enabled me to easily return to particular sections of the interview on the minidisk, without having to re-listen to the whole recording. This was particularly useful when, after multiple readings of an interview, the tone of a particular passage, which may have been obvious during transcription, became lost to me.

I sent back transcripts to participants that requested it. A couple of the more senior ones edited them and returned them to me. They mainly removed hesitations and made certain the passages about the "science" were clear. This made these interviews more polished. However the content, rhetorical and interpretative practices were not much affected. I only included the edited transcripts in my analysis

2. *Presentation of transcripts and anonymisation*

I gave each participant a pseudonym. However, some participants are quite famous, and can easily be recognised, particularly when I asked them about their participation in parliamentary debates or comments made during media interviews. I gave these people a second pseudonym[38] which I used in instances where they would be recognisable.

In some instances, I felt it was important to give a feeling for the whole sample I interviewed, usually in the form of a table, highlighting a particular aspect of each of my informants. Here, I used numbers or letters that nowhere else refer back to the individuals. In this way, there can be no attribution of quotes to particular people, but particular quotes can be put in the context of the whole sample of data collection.

The symbols used in the transcripts reproduced in this thesis are as follows:

- "[" marks an interruption

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[38] This may raise the hackles of some people in Australia who are familiar with the scandal that broke out around the publication of a book by Helen Garner about a case of sexual harassment ("The first Stone: some questions about sex and power", Pan Macmillan Australia, 1996). In it, the author uses several pseudonyms for quotes from one particular feminist who criticised her. This gave the impression that there was an army of feminists out to get the author. This was widely controversial and leaves many uneasy about the use of multiple pseudonyms for one person. However, I feel it is appropriate in my case as I only ever use a maximum of two pseudonyms and I really feel not to do so would be detrimental to the trust relationship I built with my informants. I thank Leigh Dale for bringing this controversy to my attention.

- "Interviewee: talk [Interviewee 2: talk] talk" indicates interviewee 2 is talking at the same time as interviewee 1
- "Interviewee 1: talk [word] talk": indicates a word that I have added to make the meaning of Interviewee 1's sentence clearer
- "..." indicates a hesitation
- "[...]" indicates that part of the quote has been removed

3.5 Data analysis

3.5.1 Discourse analytic approach

My approach to discourse analysis draws heavily on Gilbert and Mulkay's work – and uses a number of analytical tools they develop (discussed in 2.5.2) – but also on Potter and Wetherell's development of this work.

The principal data collected are discourses from interviews and multi-disciplinary discussions, analysed within broader contexts in which they are used and constructed. I am interested in multiple layers of discourse, including: what stem cell researchers say and how they say it; rhetorical strategies and the performativity of language; power relations, identities and worldviews as created and played out through discourse, interpretative flexibility and intertextuality; and social, legislative and institutional contexts. My analysis takes into account these various interests and goes beyond conversation analysis which focuses on turn-taking and the organization of talk (discussed in Potter, 1996; Wetherell, 1998).

Following Wetherell, discourse analysis in this thesis involves two "related movements"[39]: "One is the identification and analysis of patterns (cultural resources), while the other is theorizing and explaining this pattern" (Wetherell, 2003: 13-4). In the first movement, data are clustered into "units of analysis" – which correspond to "patterns" across interviews, for example, rather than individual interviews (Wetherell, 2003: 14) – and examined for the ways in which people talk and construct their accounts, in particular for which "interpretative regularities" such as the "truth will out device" (Gilbert and Mulkay, 1984) are used. Particular "argumentative threads" are examined as well as participants "methods

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[39] Given the iterative nature of my research, these movements are not completely separated in time.

for sense-making" (Wetherell, 1998: 404). In the second movement, more emphasis is given to the implications of participants' discourses which are made sense of in the context of social scientific concepts.

This type of analysis enables me, for example, to explore an interest in:

> how the effect of truth is created in discourse and in how certain discursive mobilizations become powerful – so powerful that they are the orthodoxy, almost entirely persuasive, beyond which we can barely think. (Wetherell, 2003: 14)

Thus my broad conception of discourse gives rise to an analysis that pays attention to the fine grained details of linguistic utterances, but also explores rhetorical strategies and examines and takes into account the broader contexts (including power relations) in which discourses are constructed.

3.5.2 An iterative process

After each instance of data collection I wrote down my initial impressions. This would include brief comments on how my informant and I had interacted, whether I felt it was a good interview or discussion, if there were any points of particular significance etc. I also made a note of questions of prompts that had (or not) worked particularly well.

I undertook two main phases of intense social scientific literature reading. The first was during the first year of my thesis, when I was discovering the field. It helped me develop many of my sensitizing concepts. The second was at the end of my data collection, when I started building chapters. It helped me deepen my knowledge of the field, explore some notions I was uncovering and hone and give strength to my analysis.

Transcription and correction of transcripts was an essential moment of preliminary analysis. During these times of very close engagement with my informants talk, many thoughts would spring to my mind. I made note of these immediately in the body of the transcript. These would relate to comments on questions being leading or to particular analytical points of interest, and were guided by sensitizing concepts such as "the deficit model", the concept of "nature", "expertise" etc. Some of these initial thoughts became central themes in final chapters; others fell to the way-side. At each phase there was a mix of "substantive" (with codes relating for example to "issues raised", "experience of engagement", "views on patients") and "theoretical" (for example "boundary-work", "example of reflexivity", "discourse of suffering") coding (Glaser cited in Heath and Cowley, 2004). The former was more common in the earlier stages of analysis, and the latter in the later stages.

At various points during the year of data collection and then during the subsequent year, I presented my findings at meetings. This was a time to return to my transcripts and recordings and re-engage with them in detail. I would then examine many interviews or events in a short time, and specifically look for discursive regularities, recurrent themes, quotes of interest and striking differences within and between the interviews and discussions. This would correspond to another significant phase of coding.

As I got more familiar with my data and found more recurring themes, I started developing a coding tree. It evolved with each new episode of data collection and analysis. The codes were assigned to old interviews as I re-read them or to new interviews as I transcribed/edited them. The coding was done manually in Word documents at the beginning of the analysis, but then was done using a custom built Microsoft Access database called QASA (discussed in the following section).

One particularly challenging aspects of the data analysis was taking into account some of the many contingent factors shaping my informants' discourses. Two were especially relevant. Firstly, as my topic guide consisted mainly of themes, and as I rarely used the same wording to ask my questions, I had to constantly return to the whole interview, checking how I had asked the question and making a note if it prompted a particular type of response. Secondly, I reflected upon where my informants were locating me (see Gilbert, 1980): as a social scientists, as a natural scientist, as a peer, as a journalist etc. This changed throughout some interviews, and affected participants' discourses.

3.5.3 "QASA"

At the beginning of my data analysis, Word was used to search for particular expressions or codes, and to highlight sections of text. However, as the volume of data grew, this became unmanageable.

I did not have (free) access to any of the usual software. But a very good friend of mine offered to develop a data analysis programme. The programme, called QASA (Qualitative Analysis Software Aid), evolved throughout my PhD. By the end, it was quite sophisticated. It enabled me to:

- import whole transcripts from Word;
- create background files linked to each interview where I could keep information on the interviewee, their field of work, their gender etc;
- create codes with associated files containing the name of the code, its label, its definition and perhaps a more detailed explanation;
- assign codes to particular sections of text;
- add notes or comments to particular sections of text;
- search and retrieve all sections of text containing a particular word, assigned a particular code, and/or/not spoken by a particular person;
- merge codes;
- rank codes (for example "boundary-work" and "discourse of suffering" were originally separate codes, but subsequently became daughter codes under the new "strategies and language" parent code);
- visualise the coding tree;
- perform complex searches and retrieve relevant sections of data.

Most importantly, QASA enables the re-contextualisation of each section of data. After doing a search for *coded as* "boundary-work adult versus embryonic" AND *spoken by* "X" NOT *containing text* "foetus", I could click on each hit and be taken back to the interview transcript in its totality. Thus, I could check what my exact question was or what the previous discussion was about. This was particularly significant for the group interviews and the multi-disciplinary discussions where simple "snapshots" of the data can miss essential aspects of the discussion processes (Catterall and Maclaran, 1997).

3.6 Reflections and limitations

1. Negotiating multi-disciplinarity

What started out as a thesis seeking ways of improving science communication, quickly became concerned with two-way public engagement and critically examining the construction of knowledge. Therefore, it may seem to some readers that I have become "anti-science". This is not the case. Indeed, I am still very excited about many of the promises and achievements of science. This thesis is not about pointing the finger at scientists and blaming them for all the problems of trust in society. However, it is not either about going back to a deficit model of public understanding. Although I do highlight many problems and issues about some of the ways my informants talk, I also want to underscore the possibilities that these critical appraisals offer not only to the improvement in the communication but also to the appreciation of science.

This thesis still aims, as it always did, to be of relevance to scientists as well as social scientists. Thus, I endeavour to contribute to social scientific theory as well as making suggestions about engagement and science discourses that are relevant to natural scientists.

2. *Limitations and further studies*

There is one particular piece of research I would do differently, if I were to start again: I would begin my research by doing an in-depth analysis of the parliamentary debates in Australia that led to the legislation on human embryos. This was done in the UK (Parry, 2003a) and creates a very good backdrop for the analysis of more private conversations. One of the reasons I did not do this, was because I hoped that other researchers, who were involved in work around SCR before me, would do it. Unfortunately, although various papers on the Australian stem cell debates were published, none were from the sociological and discursive analytical frameworks I would have liked to see.

Although I reached theoretical saturation, there are aspects of this project that could have benefited from further study. For instance, it would have been interesting to combine group and individual settings with the same young researchers, rather than organising group interviews for some, and one-to-one interviews for others. This way, I would have been able to raise some of the issues and contradictions in group settings, and explore them in detail in individual interviews. I would also have been able to contrast topics they felt they could/not raise in front of their peers. In addition, it would have liked to gain access to all the stem cell researchers I saw talking in publics debates. I was able to do that for some, and this gave me an insight into some of the contrasts between public and private discourses. However, many of the researchers who talk in public are in much demand and cannot always spare the time for interviews.

Another limitation is that I did not fully analyse the multi-disciplinary discussions. In particular it would be interesting to explore how these various “experts” position themselves with regards to other “publics” who are also “experts”. Unfortunately, there was no room in this thesis to discuss this in detail. However, due to my iterative approach, these multi-disciplinary discussions shaped the rest of my research and analysis. Indeed the first multi-disciplinary discussion was instrumental in raising some of the issues I explored in interviews, and it helped me elaborate some of my early analytical categories. I also presented its preliminary analysis at an international conference and some of the questions I was asked there shaped my later research. The form taken by the second multi-disciplinary discussion was strongly influenced by my previous findings, and the comments that people made during that morning were very influential in the later stages of my analysis and write-up. Further analysis of these discussions is, therefore, something to do in the next stage of my career.

Chapter 4

Constructions and definitions of stem cell research

Public debates are processes of signification, in which meanings, definitions and concepts are discussed and wagered. Many voices partake in this debate, arguing from different interested positions, each trying to put their marks upon the debate.

The trophy of a public debate is to turn a particular interpretation into an accepted fact which seems beyond the stage of negotiation.

(Van Dyck, 1995: 13)

4.1 Introduction

Many discussions around SCR tend to suggest that there exist precisely defined entities such as "adult" stem cells (AS cells) or "embryonic" stem cells (ES cells); this includes public discussions and social scientific analyses (e.g. Parry, 2003b). However, this chapter will demonstrate that there are rarely such clear definitions to be found in SCR. By investigating the discourses of a variety of stem cell researchers, I show that, even if these simple definitions are necessary in public, they are fiercely contested in more private situations such as interviews.

This chapter has a dual purpose. Firstly, it aims to make readers familiar with the language of SCR: it gives an introduction to terms such as "somatic cell nuclear transfer", "cloning", "pluripotency" and "adult" versus "embryonic" SCR. It highlights the difficulty in defining "SCR". Secondly, it aims to show that these concepts are socially defined and categorised; they are mobilised in order to promote particular aspects of SCR and draw boundaries around "good" or "promising" science. As Barnes (1983) points out, such categorisations and classifications are shaped by contingent goals and interests (which may or may not be recognised as such) and are an interesting location for sociological investigation.

Before exploring and deconstructing some of the taken-for-granted meanings in SCR and how professionals categorise their work, the first section of the chapter locates these discourses within broader contexts. The subsequent section focuses on terminology and highlights the difficulties in pinning down exactly what counts as a "stem cell" and where to find one. It examines labels in the natural scientific literature and then turns to what my informants consider stem cells to be. The next section of the chapter draws on Gieryn's work to examine the cultural cartography conducted by the stem cell researchers I interview. Five different (but often related) criteria that can be used for boundary-work are identified: the age of the stem cell field, naturalness, differentiation potential, safety and potential applications.

This chapter demonstrates that what counts as SCR is different for different researchers and highlights the contingency of "stemness". It suggests that public "ignorance" of the "facts" can be a reflection of the uncertainty and confusion around these "facts", as they are discussed by stem cell professionals. However, I do not want to imply that language is purely strategic; rather, it is a reflection of "interdependence between societal structures, cultural background, stem cell science and its terminology" (Hauskeller, 2005: 41). Extensive footnotes are provided in this chapter to enable interested readers to look up some of the natural scientific publications. However, the main text should serve as ample introduction to some of the complexities and conceptual ambiguities of SCR.

4.2 Contexts of SCR discourses

In order to make sense of stem cell researchers' discourses, some salient features of the context in which they take place needs to be highlighted. In the UK, the Human Fertilisation and Embryology Act (1990) allows the creation, storage or use of human embryos outside the body, for specific purposes including *in vitro* fertilisation (IVF) treatment and research into infertility. It was amended in 2001 to extend the legal use and creation of embryos to research for the purposes of: "(a) increasing knowledge about the development of embryos; (b) increasing knowledge about serious disease, or (c) enabling any such knowledge to be applied in developing treatments for serious disease" (Human Fertilisation and Embryology (Research Purposes) Regulations, 2001). The definition of embryo in this document includes embryos created by fusing egg and sperm and by “somatic cell nuclear transfer” – or SCNT, also called “cloning” (see HFEA, 2003). Subsequent to the HFE Act amendment, emergency legislation was also put into place to ban “reproductive cloning”, or the implantation into a woman of an embryo created by SCNT (for more details, see Parry, 2003a; Hauskeller, 2004).

In Australia, the situation during my data collection was slightly different as there was no established federal legislation governing the use of embryos created outside the body, but a patchwork of State legislations (for more details, see Magri, 2003; Morley and Hall, 2003; Harvey, 2005; Dodds and Ankeny, 2006). In 2001, the Council of Australian Governments called for nationally consistent regulations. By June 2002, the Research Involving Embryos and Prohibition of Human Cloning Bill 2002 was introduced into the House of Representatives (federal parliament). It was debated and split into two after a free vote, resulting in two bills: the Prohibition of Human Cloning Bill and the Research Involving Embryos Bill. Both bills were passed by the House of Representatives without amendment, then by the Senate in December 2002 (after free votes). They became the Prohibition of Human Cloning Act (2002) and the Research Involving Embryos Act (2002). They made the use of “surplus” IVF embryos in research legal, but banned the creation of embryos specifically for research, including by fertilisation. They also banned all forms of human “therapeutic” and “reproductive” cloning. The acts were immediately timetabled for review by 19th December 2005. Following this review (by the Lockhart Committee), the moratorium on therapeutic cloning was lifted (Prohibition of Human Cloning for Reproduction and the Regulation of Human Embryo Research Amendment Act, 2006; discussed in Harvey, 2007). At the time of my interviews, research on cloned embryos was not allowed in Australia.

Both the amendment of the HFE Act and the introduction of two pieces of legislation in Australia in 2002 followed heated discussions in parliament and the media, with a selection of stakeholders and interested parties. Analyses of these discussions (e.g. Herrmann, 2003; Parry, 2003a; Williams *et al.*, 2003; Goggin and Newell, 2004; Harvey, 2005; Kitzinger and Williams, 2005; Newell, 2006) reveal a number of points relevant to this chapter. One important point is that proponents[40] of embryonic SCR (ESCR) presented themselves as the voices of “rationality”, in opposition to “emotional” (Newell, 2006: 280) or “irrational”

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[40] As suggested by Parry (2003a: 198), many people held more nuanced positions than simply for or against ESCR. Nevertheless, the debates, both in the UK and Australia, took place in the lead up to votes for or against research on embryos, or for or against cloning. Thus many public discussions were organised in this dichotomous fashion.

others (Parry, 2003a: 185), such as religious leaders, lay publics, or persons identifying with having a disability. Supporters of ESCR often portrayed their views as based on “the facts” (Kitzinger and Williams, 2005: 736). This chapter will show that these “facts”, including the definition of “stem cells”, are far from clear, even amongst professional stem cell researchers.

In addition, these studies show that a central part of the pro-ESCR case, was to show that this work was the best – quickest, most likely to be successful – way of obtaining *therapies*. This involved recruiting patients and “constructing a demand” (Parry, 2003a: 187; see also Harvey, 2005; and Parry, under review), without leaving space for alternative interpretations of disease and disability (Newell, 2006).

To reinforce this promise of ESCR-derived therapies, proponents in the UK also needed to “deny the potential of adult cells, as otherwise the research on embryos might be seen as unnecessary” (Kitzinger and Williams, 2005: 738). This was done in part by highlighting that AS cells have less “plasticity” than ES cells (Parry, 2003a: 195); that is, they cannot give rise to as many different kinds of cell types as ES cells can, they are not as “pluripotent”.

Demoting AS cells by highlighting their lack of plasticity became more difficult to do by the time the Australian debates took place. In 2002 (as these debates were in full swing), a paper was published by a group in Minnesota lead by Verfaillie, suggesting that AS cells may have as much plasticity as ES cells (I examine this in more detail in the body of this chapter). This paper (Jiang *et al.*, 2002) was explicitly drawn upon by the news media and opponents of ESCR to promote ASCR (adult SCR) at the expense of ESCR. The central argument, here, was: if AS cells are as versatile as ES cells, but do not raise any ethical problems, why not simply focus on AS cells? Proponents of ESCR responded to this threat by patrolling the boundaries of SCR and drawing maps of this research to increase the cognitive authority (cf. Gieryn, 1983; 1995) of ESCR and reduce that of ASCR. However, as I show in the body of this chapter, ASCR versus ESCR is not the only place where boundary-work takes place. The uncertainty and fluidity of the field of SCR enables professionals to do boundary-work in a variety of ways, promoting the authority of SCR as a whole, or of particular areas within.

4.3 Defining and finding stem cells

Stem cells are defined by the International Society for SCR (ISSCR) as:

> Cells that have both the capacity to self-renew (make more stem cells by cell division) as well as to differentiate into mature, specialized cells. (www.isscr.org/glossary)

There is however a spectrum of definitions for "stem cells" and the defining characteristics of these cells are contested and constructed. Before I attempt to give definitions of stem cells, I detail a few types of stem cells, to provide a flavour for what SCR can be about.

4.3.1 Types of stem cells

SCR is often separated into two categories: ASCR and ESCR. The term "AS cells" usually describes particular cells found in adults (or children) that can give rise to cells of a specific

Table 1: Some pros and cons put forward about tissue-specific stem cells

Pros	Cons
- established use in therapy (in bone marrow transplants for example) - no immune rejection if stem cell donor and recipient are the same person - have been identified in many tissues[41] - few problems of uncontrolled growth or differentiation	- exist in very small numbers - difficult to identify in certain tissues[42] - cell lines would have the same genetic problems as the donor[43].

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[41] For example, researchers have found stem cells for the external surfaces of the body such as skin, hair and cornea (Miller *et al.*, 1997; Blanpain *et al.*, 2004; Stingl *et al.*, 2006; Yu *et al.*, 2006), for the small intestine (Potten and Loeffler, 1990), for the liver (Herrera *et al.*, 2006), and for cardiac (Leinwand, 2003; Anversa *et al.*, 2006) and skeletal (Collins *et al.*, 2005; Rando, 2005) muscle. The human brain has unexpectedly been shown to go through regeneration (Eriksson *et al.*, 1998), and putative neural stem cells have been identified in rats and mice (Reynolds and Weiss, 1992; Gritti *et al.*, 1996; Johansson *et al.*, 1999), and humans (reviewed in Factor and William J. Weiner, 2002: chap. 50).

[42] Some tissue based stem cells remain elusive, such as those of the lung (Rawlins and Hogan, 2006).

[43] Therefore, it is difficult to use tissue-based stem cells from a donor to repair tissue damage in that same person, if the damage is caused by a genetic condition. This problem can be solved by genetically altering the cells before they are re-implanted into the patient, as was done in humans by Mavilio (2007).

tissue or physiological system. This includes for example stem cells in the bone marrow that can give rise to all the blood cells types (white blood cells, red blood cells, platelets etc.). These stem cells can also be called "tissue-specific" stem cells. Some pros and cons of these are summarised in Table 1. These stem cells are present in the body but can also be isolated and maintained *in vitro*.

The term "ES cells" describes cells that have been derived from an embryo, can remain in an undifferentiated state, and can also give rise to all kinds of cell types (such as red blood cells, skin cells, nerve cells etc). There are several ways of obtaining embryos. The most common one is to ask gamete donors if they agree to their embryo – originally created for IVF – being

Table 2: Some pros and cons put forward about IVF ES cell lines

Pros	Cons
- can theoretically give rise to all cell types of the body - derived from embryos that are in theory "surplus" to IVF and would otherwise be destroyed - human work well grounded in mouse ESCR - grow in large numbers	- destruction of embryos[44] - potential immune rejection of cell line derivatives - difficulty in controlling ES cell differentiation and proliferation[45] = risk of cancer and tumour formation[46] - difficulty in creating pathogen-free culture conditions[47] - the "surplus" embryos may be of poor quality (frozen-thawed, low grade embryos to start with)

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[44] A variety of alternatives to the destructive embryos are being investigated and critiqued (for example: Melton *et al.*, 2004; Chung *et al.*, 2005; Cowan *et al.*, 2005; Meissner and Jaenisch, 2005; Holden, 2006; Marchant, 2006; Snyder *et al.*, 2006; Weissman, 2006).

[45] It seems that certain cell lines have a propensity to go down particular lineages more easily and many factors will need to be understood to easily direct any cell line down a particular lineage (see Pera and Trounson, 2004). Some researchers have been successful in this directed differentiation and have managed to obtain neural, pancreatic, muscle, epithelial, lung and other cells (e.g. Lowell *et al.*, 2005; Rippon *et al.*, 2006; Yao *et al.*, 2006).

[46] Stem cells are very similar to carcinoma cells and many cancers are caused by cells reverting to their undifferentiated and proliferating state. For a review of the difficulties of directed differentiation and the risks of cancer and tumour formation, see Carson *et al.* (2006).

[47] However, growth media free of human and animal components – therefore less likely to carry pathogens – are being investigated and developed (e.g. Richards *et al.*, 2002; Hovatta *et al.*, 2003; Amit *et al.*, 2004; Klimanskaya *et al.*, 2005; Rajala *et al.*, 2007).

used in research. To derive ES cells from such embryos, a particular group of cells (the inner cell mass) is removed from the embryo to establish a cell line. This process destroys the embryo as it can no longer develop into an adult organism by being implanted into a woman. The ES cells are cultured under specific conditions in order to be maintained as "pluripotent" cell lines *in vitro*. Some advantages and disadvantages of stem cells derived from IVF embryos is shown in Table 2 .

The first group to derive human ES cells was led by Thomson (see Thomson *et al.*, 1998). Only a few laboratories have successfully generated cell lines from IVF embryos (for recent reviews see Hoffman and Carpenter, 2005; Trounson, 2006). ES cells have been shown to give rise, *in vivo* or *in vitro*, to derivatives of the three embryonic "germ layers" (that is the three main groups of cells needed to make up a whole grown organism: "ectoderm", "endoderm" and "mesoderm") as well as extra-embryonic lineages[48].

One of the main advantages put forward by supporters of ESCR is that it can be done using "surplus" IVF embryos. However, these can be of poor quality (see Table 2) and research suggests that the embryos that are sought are fresh, high quality embryos, not ones left-over from IVF (see Parry, 2006).

Another way of obtaining embryos is to create them by somatic cell nuclear transfer, or SCNT (also known as cell nuclear replacement or CNR). This involves taking the nucleus (which contains most of the genetic material) out of a somatic cell (a cell that is not a gamete) donated by a particular person, and putting it into an enucleated oocyte (egg). This cell starts dividing, and gives rise to an embryo, which can then be cultured to produce a blastocyst (early embryo) that may in turn transform into an ES cell line (also called nuclear transfer ES cell line). This cell line would have the same nuclear genetic material as the person who donated the adult cell; that is the cells would be genetic clones of the adult's cells[49]. Therefore, if specific cell types (say skin cells) were developed from SCNT ES cell line, they could be re-injected into the original cell donor without immune rejection being a problem (for example it could be grafted onto the site of a burn to help skin regeneration). This idea forms the basis of "therapeutic cloning" (discussed in Hochedlinger and Jaenisch,

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[48] ES cells can differentiate into cell types such as gut epithelium, cartilage, bone, muscle, neural epithelium, blood, neural cells, cardiomyocytes, yolk sacs and trophectoderm (Thomson *et al.*, 1998; Xu *et al.*, 2002; Pera and Trounson, 2004; Hoffman and Carpenter, 2005; Trounson, 2006).

[49] However, the egg cytoplasm also contributes small amounts of genetic material and factors that regulate gene expression.

2003; 2006; Yang *et al.*, 2007). Some pros and cons of SCR with SCNT derived cell lines are outlined in Table 3.

Table 3: Some pros and cons put forward about SCNT ES cell lines

Pros	Cons
- patient-specific therapies, no immune rejection - can potentially derive cell lines from diseased patients to study that disease - platform for drug testing	- the development of a cell line following SCNT has not been achieved in humans[50] - human oocytes are considered a rare and precious resource[51] and high numbers are likely to be needed to derive each cell line - potential risks of coercion of women to donate eggs[52] - risk of ovarian hyper stimulation syndrome associated with oocyte procurement[53] - SCNT is an inefficient process and potential patient-specific treatments are likely to be expensive - cell lines would have the same genetic defects as the donor - cells lines would have additional epigenetic abnormalities[54]

In theory, if the embryo created by SCNT was implanted into a woman's uterus, it could develop into a foetus and then a baby. This procedure is called reproductive cloning (see Table 4) and is the way in which Dolly the sheep was created (Wilmut *et al.*, 1997).

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[50] The creation of cell lines by SCNT has been achieved in mice (Munsie *et al.*, 2000; Wakayama *et al.*, 2001). Only the creation of a nuclear transfer embryo has been achieved in humans (Stojkovic *et al.*, 2005), not that of a cell line, despite claims of the contrary (Hwang *et al.*, 2004, retracted).

[51] Alternatives to the use of human oocytes such as rabbit oocytes (Chen *et al.*, 2003; Edwards, 2004) or ES cell-derived oocytes (Hubner, 2003; Clark, 2004; Brown, 2006) have been discussed. Some researchers are also attempting to reprogram somatic cells in large numbers by fusing them with ES cells (Cowan *et al.*, 2005; Strelchenko *et al.*, 2006).

[52] For example junior members of staff donated their eggs to their group leader's project in Korea (Holden, 2005; Hawes and Oakley, 2006).

[53] This has been reviewed by Avecillas *et al.* (2004), but is rarely discussed in the medical community (Ferber, personal communication).

[54] Many limitations are discussed in Yang *et al* (2007).

Table 4: Some pros and cons put forward about reproductive cloning

Pros	Cons
- could potentially help certain infertile couples have genetically related children	- if ever achieved, would create a person with the same nuclear DNA as another, raising issues of identity - there are many developmental abnormalities in cloned animals and live births are very rare[55]

Reproductive cloning has been widely rejected, but it has also been put forward as a potential way of helping infertile couples, for example by the Royal Society of Edinburgh in a submission to the House of Commons Science and Technology Committee:

> There may be acceptable reasons for human cloning, which might, for example, include the creation of a child following the death of another – especially if the intending parents are now infertile [...]. The use of cloning for some reasons might legitimately be banned but there may be merit in exploring whether or not cloning is always wrong. (Royal Society of Edinburgh, 2004: paragraph 8)

The cloning experiments described above suggest that certain cells found in adults (any cells, not just stem cells like those in bone marrow) retain the information to become any other cell in the body. That is, as they differentiate and become more specialised, they do not *forget* how to become other sorts of cells. This ability to "de-differentiate" into less specialised cells goes against previous fundamental assumptions in biology.

A variety of adult cells committed to a particular cell lineage (such as blood cells) seem to have the ability to become cells from different lineages (such as neural cells)[56]. This is sometimes called "trans-differentiation". These AS cells are said to show "developmental plasticity". The research by Verfaillie's group (introduced in 4.2) suggests that a particular subset of adult bone marrow stem cells from mice and rats (called mesenchymal stem cells) can become cell types of all three germ layers *in vivo* and *in vitro* (Jiang *et al.*, 2002). It has been suggested these cells have a developmental potential similar to ES cells and that they

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[55] Many cloned animals are abnormal (discussed by Solter, 2000), and Dolly died prematurely (Whitfield, 2003).

[56] For example neural stem cells (Bjornson *et al.*, 1999) and muscle stem cells (Jackson *et al.*, 1999) can take on haematopoietic "fates". Bone marrow cells can become epithelial (Krause *et al.*, 2001), skeletal muscle (Ferrari *et al.*, 1998; Labarge and Blau, 2002) and cardiac muscle (Orlic *et al.*, 2001) cells. Some of these fate changes are in response to injury but also seem to happen under biological conditions (for a review see Clarke and Frisen, 2001).

may be “pluri-” or “multipotent”. A variety of studies have been published since, supporting the plasticity of adult cells (some are reviewed in: Lakshmipathy and Verfaillie, 2005). An example in humans is the ability of cells taken from inside the nose to give rise to a variety of cells from all three germ layers (Murrell *et al.*, 2005); or the possibility of reprogramming fibroblast[57] cells into cells with ES-like properties (Wernig *et al.*, 2007).

However, not all scientists are convinced of the plasticity of these adult cells; in particular, many of these results have not been reproduced in other laboratories. Some researcher suggest that the apparent trans-differentiation events are very rare if they occur at all; others suggest that they are in fact due to cell *fusion* and not *trans-differentiation* (differentiated cells are postulated to fuse with other types of differentiated cells and take on this new developmental fate). The plasticity, or not, of adult stem cells has been the source of lively discussions within the scientific community[58]. If it occurs, it could enable the study of biological processes and perhaps pave the way towards autologous[59] therapies (see Table 5).

Table 5: Some Pros and cons put forward about “plastic” AS cells

Pros	Cons
- could potentially provide autologous cell therapies and avoid immune rejection - enable the study of stem cell “niches”[60] in vivo	- difficult to identify and isolate - rare - may not really be plastic - cell lines would have the same genetic defects as the donor

Stem cells from foetuses[61] can differentiate into many cell types. They are often classified as “AS cells”[62] even though this can be counterintuitive, and some people prefer to keep them in a separate category. In some instances, however, it becomes useful to classify cells

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[57] Fibroblasts are present in connective tissue and gives rise to other cell types present in this tissue.

[58] For example: Dewitt and Knight (2002), Joshi and Enver (2002), Wagers *et al.* (2002), Ying *et al.* (2002), Alison *et al.* (2003), Wagers and Weissman (2004) and Check (2007).

[59] Autologous therapies use material from the patient himself of herself.

[60] A niche is the dynamic environment in which stem cells exist and function in the body. It is the result of the interplay between the anatomic locations of the cells, signals from neighbouring cells and physiological conditions such as pH.

[61] Germline cells (Shamblott *et al.*, 1998), and neural (Cai *et al.*, 2002) and haematopoietic (Jordan *et al.*, 1990) stem cells with a broad differentiation potential have been discovered in foetuses.

[62] For example, “Adult stem cells have been found in sources including bone marrow, blood, the brain, skeletal muscle, the pancreas, fetal tissue and tissue from the umbilical cord” (Parliament of Australia, 2002a: 2.14)

derived from foetuses as ES cells (as I will discuss in chapter 7). Using foetal material raises different kinds of issues to the use of embryos, such as the ethical difficulties in getting informed consent from people going through terminations. Cord blood stem cells also can give rise to many cells types[63] and can be classified as adult stem cells or kept in a separate category.

4.3.2 Defining stem cells?

In 1978, a group of scientists, composed mainly of haematologists, decided upon the following definition for stem cells:

> Cells with extensive self-maintaining (self-renewal capacity), extending throughout the whole (or most) of the life-span of the organism. *Differentiation potential* is a property of some types of stem cells but it is *not an essential feature* of stem-ness (Lajtha quoted in Lajtha, 1983, emphasis added)

This definition differs from the first one, by the ISSCR, given at the beginning of section 4.3. In this second definition, "differentiation potential" is not defining of "stemness"; rather, it is the ability of a cell population to maintain itself over long periods of time that is defining. The latter is a property of haematopoietic stem cells. A third definition comes from a paper published in 2005: "The Stem State: *Plasticity Is Essential*, Whereas Self-Renewal and Hierarchy Are Optional" (Zipori, 2005, emphasis added). This definition focuses on the opposite characteristic to the previous definition. It suggests that "plasticity" defines "stemness".

These last two definitions arose in scientific publications. They represent extremes in a continuum of emphasis in the meaning of stem cell. It could be that the divergence is due to different temporal contexts: whereas the second definition was given in the late 1970, when haematopoietic stem cells were the best-known stem cell type, the third one was given more recently, when embryonic stem cells have perhaps become more dominant (although, it is certainly not the case that everyone now thinks that stem cells are defined by their differentiation potential as I will show below). It could also be that this divergence is due to different research focuses: researchers in haematopoietic stem cells, still tend to focus on self-renewal (e.g. Adams and Scadden, 2006: 333), whilst ES cell workers tend to focus on differentiation potential (e.g. Trounson, 2006: 208) . A range of definitions is currently used

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[63] See especially the cells derived from cord blood by Kogler *et al.* (2004).

by stem cell researchers and others. In addition, "differentiation potential", often used to classify stem cells, is a very fluid concept, as I now show.

4.3.3 Classifying stem cells according to their differentiation potential

The classification of cells according to their differentiation potential can become an important site of social negotiations. In particular, much of the 2002 Australian parliamentary debates came to focus on the differentiation potential of various stem cells[64].

Stem cells can be described as "totipotent", "pluripotent", "multipotent" and "unipotent". The last term describes cells that can only give rise to one cell type, and did not often come up in my research. The other three terms came up more frequently; although each one was not necessarily used to describe the same stem cell types (particularly "pluripotent" and "multipotent" had contested meanings). These terms were also present in news media, public discussions and scientific publications. The ISSCR website defines them as follows:

> Multipotent stem cells
> Stem cells whose progeny are of multiple differentiated cell types, but all within a particular tissue, organ, or physiological system. For example, blood-forming (haematopoietic) stem cells are single multipotent cells that can produce all cell types that are normal components of the blood.
>
> Pluripotent stem cells
> Stem cells that can become all the cell types that are found in an implanted embryo, fetus, or developed organism, but not embryonic components of the trophoblast and placenta (these are usually called extra-embryonic).
>
> Totipotent stem cells
> Stem cells that can give rise to all cell types that are found in an embryo, fetus, or developed organism, including the embryonic components of the trophoblast and placenta required to support development and birth. The zygote and the cells at the very early stages following fertilization (i.e., the 2-cell stage) are considered totipotent. (www.isscr.org/glossary)

As I raised in the introduction, it was, at times, necessary for supporters of ESCR to downplay the promise of ASCR. This was done, in part, by classifying ES cells, and not AS cells, as "pluripotent". It is beyond the remit of this chapter to carry out an in depth investigation of the mobilisation of the terms "pluripotent" and "multipotent", but I shall

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[64] I return to the scientism prevalent in debates around SCR in chapter 7.

make a brief illustration, in order to introduce the reader to the fluidity of these concepts central to SCR. I do this by discussing three papers and their use of these labels.

The Thomson publication I introduced above describes the derivation of "pluripotent" embryonic stem cell lines. These could differentiate *in vivo* into cells of the three germ layers, including:

> gut epithelium (endoderm); cartilage, bone, smooth muscle, and striated muscle (mesoderm); and neural epithelium, embryonic ganglia, and stratified squamous epithelium (ectoderm) (Thomson *et al.*, 1998: 1146)

When ES cell lines were made to differentiate *in vitro*, they also gave rise to endoderm and trophoblast derivatives. That ES cells are pluripotent is generally accepted.

The Verfaillie paper describes the derivation of stem cells labelled as having "pluripotency" in the title, but as being "multipotent" in the abstract. These cells were given the name: "multipotent adult progenitor cells" (MAPCs). They:

> differentiate, at the single cell level, not only into mesenchymal cells, but also cells with visceral mesoderm, neuroectoderm and endoderm characteristics *in vitro*. When injected into an early blastocyst, single MAPCs contribute to most, if not all, somatic cell types. On transplantation into a non-irradiated host, MAPCs engraft and differentiate to the haematopoietic lineage, in addition to the epithelium of liver, lung and gut. (Jiang *et al.*, 2002: 41)

Even though these cells seem to give rise to a similar variety of cells as Thomson's ES cells above, they are not always called "pluripotent". For example:

> Recent research on adult stem cells indicates that they have the capacity to generate not only the tissue in which they are found, but to generate the specialised cell type of another tissue. It is thought, however, that adult stem cells can differentiate into a more restricted range of tissues or organs than embryonic stem cells. They are thus described as 'multipotent' rather than 'pluripotent' (Parliament of Australia, 2002a: 2.33)

Here being able to differentiate into cells beyond one particular lineage (beyond the haematopoietic lineage for example) is not enough to earn cells the label "pluripotent" as the ISSCR definition above would suggest. Opponents of ESCR however did describe these cells as pluripotent (e.g. Good, 2002).

A third paper I wish to mention briefly here is by Murell *et al.* (2005). They derived cells from adult olfactory mucosa (made up of olfactory epithelium and mucus secreting glands) which could give rise to a variety of cells including: neurons (497), cardiac muscle, skeletal muscle and liver cells i*n vitro* (499); and blood vessel, brain, spinal cord, liver, skeletal muscle, gut (499) and haematopoietic cells (503) *in vivo*. Here again, differentiation beyond the lineage of origin (neuronal lineage) was not sufficient to gain the label "pluripotent". The authors however suggest in their discussion that the cells they study could be labelled "pluripotent" but they do not use this label in their title. Whether this is the journal editors' or the authors' choice, I do not know. However, it seems likely that at the time of the Verfaillie publication, the term "pluripotent" was not as contested as it became by the time of the Murrell publication.

Thus, we see that cells that can differentiate beyond their lineage of origin can be labelled both "pluri-" and "multipotent". The differentiation potential measured for any cell type does not pre-determine the label that will be used to describe it. Rather labels are used more strategically and contingently. Part of this conceptual fluidity comes from the difficulty of *pointing* to a cell and saying: this is a stem cell, and it can differentiate into these five other cell types. Some of the difficulties that come from this can be seen as illustrations of the Collins and Pinch's experimenter's regress (see 2.2.1).

ES cells are often defined as pluripotent:

> Embryonic stem cells by definition, can give rise to all tissue types (pluripotent). (Parliament of Australia, 2002b: 3.20)

They are *assumed* to have this property in humans because they are derived from the inner cell mass, which has that ability in mice. However, ES cells are not equivalent to inner cell mass cells[65] (they have no *in vivo* counterpart) and the mouse inner cell mass cells are not the same as the human inner cell mass cells. It is difficult to directly examine what happens to a particular group of cells *in vivo* and they usually have to be taken out of their usual environment to be studied[66]. Thus, the observed properties of stem cells could simply be artefacts of being cultured *in vitro*.

In humans, an indicator of a group of cells' differentiation potential is given by removing them from their biological location, creating a cell line, and injecting some of these cells into

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[65] It has also been suggested that ES cells may be more similar to early germ cells than inner cell mass cells (Zwaka and Thomson, 2005)

[66] This was already pointed out in reference to haematopoietic stem cells, in the early 1980s (Potten, 1983).

immune deficient mice[67]. A sort of tumour usually develops and the different cells types that arise therein can be examined. Again, however, it is difficult to simply observe what the different cell types are by looking at cell morphology. Cell surface markers are used, but this is an indirect measure and to draw any conclusions, researchers need to believe that the link between cell surface markers and cell types has been "accurately" established. This highlights that these claims are "theory-laden" and "under-determined" by reality (see 2.2.1). The difficulty in establishing what cells differentiate into, and what they would differentiate into were they not taken out of their usual context, highlights the socially constructed nature of the labels used to describe them and their differentiation potential.

This section has shown that the field of SCR is wrought with uncertainties and many of the labels used within it are contested. Nevertheless, I hope that I have given the reader an introduction to some of the meanings of these labels, as well as an indication of some of the ways in which they are mobilised.

4.3.4 Professionals' accounts: multiple foci for SCR

So far, the impossibility of pointing to a stem cell has been shown. Indirect methods are necessary to assess the characteristics of these cells and the labels used to describe their characteristics are conceptually fluid. As a result, many types of research are labelled SCR. In this section, I give an overview of what my informants see as central to SCR.

The emphasis my informants place on various areas within SCR is summarised in Table 6. Overall, as expected, they tend to see the area they work in as central to the field. There are no *systematic* differences based on researchers' gender or level of seniority concerning what sorts of stem cells they portray as important. Researchers in Australia more frequently defend one aspect of SCR at the expense of another; whereas researchers in the UK appear less polarised. There are also differences between these two countries in how researchers talk about SCNT (which I discuss in more detail in chapter 5).

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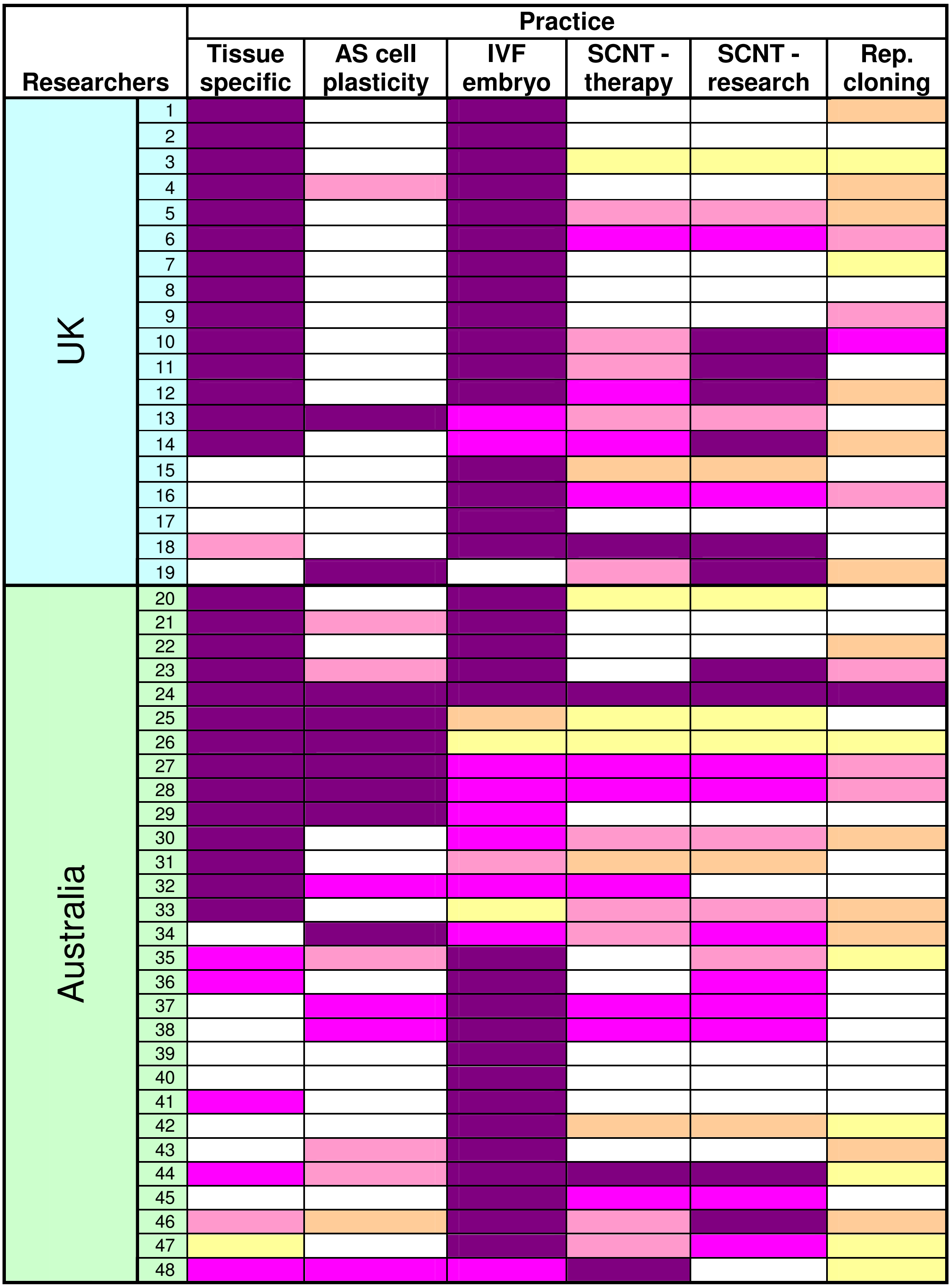
[67] It may be possible one day to identify stem cells by their chromatin conformation, which enables access to a variety of genes and suggests an epigenetic (rather than genetic) control of stemness (e.g. Wiblin *et al.*, 2005; Azuara *et al.*, 2006; Buszczak and Spradling, 2006).



Researchers		Practice					
		Tissue specific	AS cell plasticity	IVF embryo	SCNT - therapy	SCNT - research	Rep. cloning
UK	1	🟪		🟪			🟧
	2	🟪		🟪			
	3	🟪		🟪	🟨	🟨	🟨
	4	🟪	▨	🟪			🟧
	5	🟪		🟪	▨	▨	🟧
	6	🟪		🟪	🟥	🟥	▨
	7	🟪		🟪			🟨
	8	🟪		🟪			
	9	🟪		🟪			▨
	10	🟪		🟪	▨	🟪	🟥
	11	🟪		🟪	▨	🟪	
	12	🟪		🟪	🟥	🟪	🟧
	13	🟪	🟪	🟥	▨	▨	
	14	🟪		🟥	🟥	🟪	🟧
	15			🟪	🟧	🟧	
	16			🟪	🟥	🟥	▨
	17			🟪			
	18	▨		🟪	🟪	🟪	
	19		🟪		▨	🟪	🟧
Australia	20	🟪		🟪	🟨	🟨	
	21	🟪	▨	🟪			
	22	🟪		🟪			🟧
	23	🟪	▨	🟪		🟪	▨
	24	🟪	🟪	🟪	🟪	🟪	🟪
	25	🟪	🟪	🟧	🟨	🟨	
	26	🟪	🟪	🟨	🟨	🟨	🟨
	27	🟪	🟪	🟥	🟥	🟥	▨
	28	🟪	🟪	🟥	🟥	🟥	▨
	29	🟪	🟪	🟥			
	30	🟪		🟥	▨	▨	🟧
	31	🟪		▨	🟧	🟧	
	32	🟪	🟥	🟥	🟥		
	33	🟪		🟨	▨	▨	🟧
	34		🟪	🟥	▨	🟥	🟧
	35	🟥	▨	🟪		▨	🟨
	36	🟥		🟪		🟥	
	37		🟥	🟪	🟥	🟥	
	38		🟥	🟪	🟥	🟥	
	39			🟪			
	40			🟪			
	41	🟥		🟪			
	42			🟪	🟧	🟧	🟨
	43		▨	🟪			🟧
	44	🟥	▨	🟪	🟪	🟪	🟨
	45			🟪	🟥	🟥	
	46	▨	🟧	🟪	▨	🟪	🟧
	47	🟨		🟪	▨	🟥	🟨
	48	🟥	🟥	🟥	🟪		🟨

Table 6: What is central to SCR?

Table 6: What is central to SCR?

My informants portray different areas of research as central to SCR. The importance accorded to each area is colour coded, with yellow indicating a practice that the interviewees specifically excluded from SCR, and purple indicating a practice portrayed as central to SCR. "Tissue-specific" SCR, here, specifically corresponds to the study of AS cells such as those found in the bone marrow or the brain. The study of "AS cell plasticity" corresponds to attempts at finding or examining cells found in adult tissues (including cord blood and foetal matter) that can differentiate into cell types outside their original lineage. Research using "IVF embryos" is what is commonly referred to as ESCR, and only includes work with embryos from IVF clinics, so-called "surplus" embryos. "SCNT – therapy" is the study of cloning processes with the specific intention of developing patient-specific therapies. "SCNT – research" encompasses studies to develop SCNT embryos to further understand basic biological processes or to develop new drugs on SCNT ES cell lines. Researchers are grouped according to the country in which they work and then broadly classified thematically (with those portraying tissue-specific SCR as central grouped together, for example). The data come from one-to-one interview settings and group settings. The blanks correspond to aspects of SCR that were not covered during our discussions. I have not given any more biographical details in the interest of anonymity. The purpose of this table is to highlight the diversity of professional views on what counts as SCR.

Table legend:

Specifically excluded from SCR	**Small part of SCR**	**Part of SCR**	**Important part of SCR**	**Central part of SCR**
🟨	🟧	🩷	🟣	🟪

For some researchers, like nº 47, the main part of SCR is doing research on embryos from IVF clinics. This area is the most exciting and likely to give rise to therapeutic applications. For others, like nº 31, SCR is an old field, based in the study of haematopoietic or epithelial stem cells. Even though ESCR may contribute some new information, adult cells are the most likely to lead to therapies as they have been doing for years.

For researchers like n^{o} 32, although SCR started with the study of adult cells, it has seen a revival with the discovery of AS cell plasticity. The main point of SCR now is to study this exciting phenomenon in order to better understand stem cell niches and develop autologous therapies.

Researchers, such as n^{o} 18, see the possibility of using SCNT for patient specific therapies as one of the main aims of SCR and as the "perfect solution" to the problem of transplant rejection. Many other researchers, though, see the role of SCNT in SCR as providing better research tools, rather than therapies. Some researchers, like n^{o} 20, specifically do not classify SCNT as central to SCR as they are uncomfortable with the idea of creating an embryo to destroy it:

> I think [ASCR and ESCR] have to go hand in hand. I don't think embryos should be created specifically for research purposes. I think that IVF surplus stocks can certainly be used to derive enough ES cell lines, for whatever people want to do. Yeah, I guess I do have a bit of a problem with that [the creation of embryos for research].

Finally, for some researchers, like n^{o} 24, SCR is a mix and match of all aspects, including research on reproductive cloning for potential alleviation of infertility.

Some aspects of my informants' representations of SCR have just been sketched. The data given here are inevitably disembedded from their social location and, in the interest of anonymity, informants are not examined in turn, with details of their background given to explain their accounts. Nevertheless, following this indication of the variety in stem cell researchers' portrayals of SCR, the ways in which social locations shape these discourses need to be highlighted and examined. This is done in a variety of ways in the remainder of the thesis. I start this task in the subsequent section by examining the boundary-work conducted by stem cell researchers when talking about their field, and how this serves to maintain the authority of particular aspects of SCR.

4.4 Patrolling the stem cell boundaries

Being able to position oneself as a stem cell researcher, adult stem cell researcher and/or embryonic stem cell researcher has important implications. There are financial rewards to doing SCR. These are accessible for example by joining prestigious research institutes, endowed with large financial resources, which specifically study SCR (the Australian Stem

Cell Centre in Melbourne, the Institute of Stem Cell Research in Edinburgh or the Wellcome Trust Centre for Stem Cell Research in Cambridge) or by applying for funding specifically earmarked for SCR (via the UK Stem Cell Initiative). However, not all stem cell funding is available for all types of SCR, and some will only go to ASCR (the National Adult Stem Cell Research Centre in Australia, see Abbott, 2006) and some specifically exclude research that destroys embryos (the European Seventh Framework Programme, see Council of European Unions, 2006). So there are implications to labelling one's work as SCR, or embryonic versus adult SCR. My informants included and excluded research on different types of stem cells in their definition of good or promising SCR by highlighting various characteristics of these cells. I am not trying to imply that all scientists deliberately mis-lead funders and give definitions of SCR they do not believe in, simply in order to obtain funding; rather, the conceptual fluidity and the uncertainty besetting this field enables boundary-work to be conducted in many ways.

4.4.1 Age as a selection criterion

One of my informants was dismayed at public ignorance around SCR, particularly the supposed misunderstanding that SCR is new, when it is, according to him, old. This section shows how the age of SCR is one of the contingent characteristics mobilised by stem cell professionals in order to promote particular aspects of this field.

Some publications start the history of SCR at different time points: at the first mammalian *in vitro* fertilisations, in 1878 – also highlighting the importance of the first live human IVF birth, in 1978 (Brown University); at the derivations of the first ES cell lines from mice, in 1981 (ISSCR, 2005); at the derivation of the first human embryonic stem cell lines, in 1998 (often in introductions to sociological papers such as: Magri, 2003; or scientific papers such as: Hoffman and Carpenter, 2005; Keller, 2005; Zipori, 2005); or at the first bone marrow transplantations, in 1956 (Olinger, 2006). My informants draw on this diversity of starting points and conduct boundary-work by constructing SCR as "emerging" and/or "established".

In some cases, my informants describe SCR as emerging and full of promise, in opposition to areas that have been around for a long time, have not cured all diseases and are therefore passé. For example, Brian, a young researcher working on ES cells dismisses ASCR as it has failed to realise its promises:

> Brian: You can easily derive adult haematopoietic stem cells which reconstitute bone marrow, but my supervisor is an oncologist, and although we've been able to derive those cells for 20 years, leukaemia still kills people after bone marrow transplants. So it's not a cure, it doesn't actually work that well, unfortunately.

Brian does not discuss two of the main problems with bone marrow transplants for leukaemia treatment: the rejection of the grafts and the difficulties in destroying the cancer cells. These problems will not automatically be solved using cell lines derived from IVF embryos. Rather Brian sets up the failure of tissue stem cell-based therapies as the reason for focussing on new therapies that have not yet failed. He draws the map of promising research to include ESCR, which he describes as new and therefore able to offer new possibilities for therapy; and to exclude ASCR, which he describes as old, and already proven incapable of curing leukaemia. This cultural cartography takes age as a criterion. It "expands" the cognitive authority of ESCR to an area previously covered by ASCR, but where Brian claims ASCR has already failed: leukaemia treatment, and by extension, therapies in general. Importantly here, *therapeutic* applications are where success and failure of different cell types are judged.

For some researchers, SCR is young and promising, even without considering its therapeutic potential:

> Danielle: So when I decided I wanted to come back and do a PhD, I wanted to pick an area that I thought was very promising and that I believed in, but I still felt was very young and had the potential for a lot of basic research. Because I obviously want a career in basic research. So I saw the field of stem cells as being really fascinating, I thought it had a lot of promise for applications but also for basic research, a lot of information to be gathered. So I chose that area.

Danielle is using the label "the field of stem cells" and not for example the field of *embryonic* stem cells. One could imagine, if simply given this quote, that she was referring to a variety of stem cells sources. However, given the story that she tells in this interview about not being able to do her research in the United States, and her less enthusiastic responses when I ask her about other types of stem cells, I am quite certain that she is talking about doing research on cell lines derived from IVF embryos. Thus for her, the label SCR is used to describe what may, more narrowly, be given the label ESCR. For Danielle, this field is exciting because its youth implies that a lot of discoveries can still be made. This is in contrast to tissue-specific SCR, for example, that has been under investigation for longer, and therefore is perceived to have less potential for basic science findings. Danielle does not mention the potential findings that could come from studying AS cell plasticity.

Contrary to the above example, other scientists conduct boundary-work to "monopolise" the authority that ASCR already has, and exclude what is considered to be the new, hyped, ESCR. SCR is portrayed as an established field in order to suggest it is full of promise and a good area to focus research on. For Sylvester, an adult stem cell researcher, SCR is clearly rooted in older work such studies of haematopoiesis. This does not mean that newer research, such as the discovery of brain stem cells, has not revitalised the field. He includes his work in the map of promising research by highlighting that it is an established field and has already proven itself – by having therapeutic potential:

> Sylvester: So in some ways the catch-cry of stem cell revolution is sort of wrong anyway. There was a revolution, and it happened 30 years ago, and it was about using bone marrow; they saved an enormous amount of people. [...] So, there's a confusion there as well as if somehow this is a new phenomenon, so called tissue or adult stem cell versus embryonic stem cells which, that's, that's not the debate at all.

Concerning ES cells, Sylvester argues that: "there's nothing special about them, they might sort of fill in gaps in places". He dismisses the idea that ES cells are bringing about a "stem cell revolution". For him, AS cells have already proven themselves to be very exciting and therapeutically applicable cells.

Approximately half my informants work in ESCR and are, unsurprisingly, very supportive of that work. Some are quite exclusive, such as Danielle. Others argue that ES cells are to be studied alongside other types of stem cells; even if they describe ES cells as more promising. Similarly, most of the researchers focussing on ASCR describe that aspect as central to SCR; with Sylvester being the most ardent supporter of ASCR.

Many researchers also draw on the age of the field to promote both ESCR and ASCR. For example, Stanley, who works on AS cells, views SCR as old and based on tissue-specific stem cell work. He also sees it as an evolving and exciting field, and is happy to include ESCR. He starts the history of the term SCR with studies of endogenous stem cells such as those in the skin:

> Stanley: I've been working in this field for quite a number of years, it is kind of ironic in the sense that the term stem cell could have, could be seen, I'm not saying I see it, it could be seen to have been hijacked by those working on embryonic stem cells. Because of course, the very concept of a stem cell really emerged from the study of adult tissues, it is not something that arose from embryonic stem cells; [...]. Again it was really trying to invoke the concept of a stem cell in those constitutively renewing tissues that led to concepts of stem cells

> and ultimately to the search for, you know, the Holy Grail. Of course Holy Grail, it's like a moving series of goal posts, the goal has changed.

Although Stanley describes SCR as coming out of adult work, he does not, here or elsewhere in the interview, argue for putting ES to the side. Rather he argues that science constantly changes, "like a moving series of goal posts", and that we should study all the different areas that are promising. This is similar to how scientists argued, in the UK parliament, for the continued study of all different avenues of research (Parry, 2003a).

Similar to many other scientists, Stanley identifies no direct competitors for resources; the discussion is set up as if funding was infinite. This suggests boundary-work analysis is inadequate: this researcher is not trying to "expand" the cognitive authority of SCR to areas previously occupied by competitors; he is simply aiming to expand the authority of SCR. This sort of discursive strategy masks that resources are limited. It does not enable questions around what funding gets cut if this new area of biotechnology gets funded. For example, Newell (2006) argues that this focus on biotechnological solutions often masks the lack of resources for less glamorous areas, such as wheelchair provision or carer salaries.

Thus, SCR can be portrayed as beginning with: bone marrow studies, work on endogenous stem cells, the derivation of ES cells or the discovery of adult stem cell plasticity. The age of stem cell research is up for negotiation and labelling the field as old or new is used in cultural cartography. Both the youth and established aspects of SCR can be used as criteria to suggest this research holds promise. The ways in which researchers portray the field does not necessarily reflect what area they work in.

4.4.2 Nature as a selection criterion

The concept of "nature" is a powerful resource that can be mobilised in various ways. For example, Parry (2003b) shows how publics draw on their lived experiences to construct the category of "nature" which they can drawn upon in contingent ways in their discourses on SCR and cloning. Brown and Michael (2003) discuss professionals' uses of "nature": xeno-transplantation's difficulties are explained by its "un-naturalness", and is contrasted to SCR's predicted success, and its naturalness. In their cultural cartography, my informants portrayed as "natural" particular areas of research that they supported and were comfortable with, and excluded others as "unnatural".

Nature can be used to “monopolise” resources for SCR by excluding reproductive cloning as “unnatural”, in contrast to a more “natural” reproductive method: IVF. One researcher tells me he does not find reproductive cloning “acceptable”. When I ask him why, one of his reasons is: “it just doesn’t feel right to me”. When I ask him if it would be acceptable in the context of treating infertility, he responds:

> Peter: Yeah, you’re asking a question which I haven’t thought about. I have a problem with it. I do have a problem with it. So why, I’m not actually, other than those reasons I gave you. I’m not, I’m worried about, I’m partly worried about a mistake happening that you don’t expect, um [N: Being passed on?] being passed on forever. Partly, I think that’s partly it. Also it just seems like there are alternatives, I mean, we’ve always got adoption.
>
> Nicola: But then that would be the same for IVF, why have IVF? So would you have some[
>
> Peter:]IVF to me is a bit more natural,[…] I don’t have any problem with that because there, there may be some physical reason or some sperm motility reason, or some issue like that that’s kind of you’re not, yeah, I don’t have a problem quite so much with that.

Peter seems to suggest that infertility should only be treated by “natural” means such as IVF and adoption. He does not reflect on processes of “naturalisation” that technologies such as IVF have gone through (cf. Parry, 2003b), but rather posits “nature” as a stable category. He uses nature to exclude reproductive cloning from acceptable science. This reinforces the message that SCR is natural, therefore acceptable.

Using nature is also a powerful way of doing adult/embryonic boundary-work. In the following quote, an ES cell scientist draws the map of promising research to include nature, and thus ESCR, and to “expel” ASCR. Although he does not explicitly appeal to the label “nature”, he does so implicitly by using “meant to”:

> Vincent: In the case of the embryonic stem cell, it can generate all the cell types that it would do if it was still in the embryo and if that embryo had been implanted. […] And these cells because they come from the early embryo they’re meant to do this. So there’s lots of discussions and hype about adult stem cells. There is no evidence that an adult stem cell …that there can be any physiological function for an adult stem cell to generate these types of tissues. So … Whereas these cells, this is what they are meant to do.

Vincent is promoting research on ES cells by arguing that these are “meant” to differentiate into many different cell types, as this is what the cells in an embryo do in order to form a grown adult. He expels AS cell plasticity by highlighting that adult cells do not “naturally” – that is *in vivo* – differentiate into many different cell types.

Interestingly, Vincent, later on in this discussion, highlights the absence of any *in vivo* equivalent to ES cells, suggesting these to be artefacts of culture:

> Vincent: So the critical thing here is that we do not know what the real relationship is between an embryonic stem cell in culture and cells in the embryo. So, so, it's short hand to say that the inner cell mass becomes ES cells, but the inner cell mass is not a group of ES cells. There's no stem cell, real stem cell function in the early embryo. Stem cells [...], they may be cells that are purely artefacts, being generated in culture.

Vincent makes this claim following a question about why more than one stem cell line is needed for research, if they can indeed differentiate into all different cell types. It is part of his argument that more research is needed to better understand exactly what ES cells are and how they work. In this way, Vincent, in the first quote, contrasts the “naturalness” of ES cell differentiation potential to the “unnaturalness” of trying to make adult cells differentiate into many cell types. There naturalness implies something that occurs *in vivo*. However, Vincent also needs to show that ES cells are not simple to understand and are not the same thing as the inner cell mass: they need to be further investigated. So in the second quote, he draws on the difference between the “naturally occurring” inner cell mass and the “artefactual” ES cells to justify more research. Here, whether or not this inner cell mass is “meant” to be put *in vitro* and transformed onto ES cells no longer seems relevant. The map of good research is drawn alternately to include the use of “naturally” occurring ES cells, which are better than “unnatural” AS cells; and alternately to include the study of ES cells, which are not “naturally” occurring, and therefore need to be investigated. This flexible use of “nature” highlights the strategic character of this particular account.

“Nature” can also be used to promote tissue-specific SCR at the expense of ESCR:

> Zach: The sort of idea that the embryonic are much more… pluripotent, and have [a] much greater range of differentiation, but, I don’t know how that relates... I think, I like the idea of, with my work, that we’re not trying to push them too far anyway. Like we’re trying to encourage them to go down their natural pathway, just more efficiently I suppose. So I get a little sort of concerned that just because you can change a cell into something else, it doesn’t really need to.

Zach is a PhD student working on a particular type of tissue-specific stem cells, trying to understand their differentiation in more detail. He is attempting to monopolise resources to maintain the cognitive authority of his work by suggesting that it is more “natural” than trying to forcefully "push" ES cells down particular differentiation pathways that they may not be meant to follow. He excludes ESCR by painting it as a fashionable whim, trying to make cells do something they would not "naturally" do.

In summary, "nature" is a useful interpretive resource that can flexibly promote different areas of research (IVF-derived ESCR, tissue-specific SCR, AS cell plasticity) and exclude others (reproductive cloning, ASCR, ESCR). The constructedness of "nature" is visible here; however, none of my informants reflected upon it, unlike lay groups examined by Parry (2003b).

4.4.3 Differentiation potential as a selection criterion

As I argue in the introduction, the potential plasticity of adult cells, raised by the publication of the Verfaillie paper, posed a direct challenge to ESCR. In the UK, this paper was mentioned in the news media (Highfield, 2002; Whitehouse, 2002). However the parliamentary debates were over by the time of its publication and ESCR was already legal. In Australia, the concept of AS cell plasticity were raised in submissions and briefs given to Parliament (e.g. Do No Harm, 2002; Information and Research Services, 2002), in Parliamentary discussions (e.g. Official Committee Hansard, 2002, September 19th; 2002, September 24th), in reviews of the stem cell debates (e.g. Herbert, 2002) and in the news media (ABC News Online, 2002a; Wroe, 2002). It was put forward by opponents of research using embryos as a reason to focus on ASCR, rather than ESCR:

> Work at the University of Minnesota points to the fact the destruction of human embryos is unnecessary to obtain stem cells for 'miracle cures'. (Catholic Women's League Australia-NSW Inc, 2002).

To respond to this, supporters of ESCR needed to organise their defence. They challenged the logic that the plasticity of some AS cells was enough to render ESCR obsolete; they dismissed the findings by highlighting that they had not been reproduced and that these cells if they exist, are very rare and difficult to isolate; and they backed the parallel investigation of AS and ES cells (e.g. Dr. Elefanty, Official Committee Hansard, 2002, September 24th: CA194). These are the same arguments as the ones made in the British Parliamentary debates (Parry, 2003a). They reinforced this point by drawing on Verfaillie's statements that her work should not be used as an excuse to stop ESCR.

The potential plasticity of AS cells is also a common theme during my interviews. Some researchers express the feeling that ASCR became a much more potent challenge to the legalisation of ESCR when it could compete on the "scientific" side:

> Peter: The use of adult stem cells has certainly become one of the strongest arguments from the lobby groups, the Catholic lobby groups in particular, for not doing embryonic stem cell work. So, in the beginning, I guess the science for the adult stem cells has moved along even more quickly in recent years, at the beginning, there wasn't that potential so the debate was more, is it ethically acceptable or not. Without there being an alternative to, I guess, help the Catholic debate further. So I think they're in a stronger position, have a scientific kind of arm in their argument that they didn't have before.

By talking about this "scientific arm", Stanley indicates that "technical" challenges (ones that result directly from laboratory-based findings), rather than "ethical" ones, hold more sway in debates about the future of science (I return to the often scientistic nature of public debates in chapter 7). This quote suggests that for this informant, plasticity is an important criterion by which to judge the promise of a stem cell.

Many of my Australian informants dismiss ASCR plasticity during interviews, using similar strategies to the ones in parliament (some of my informants gave evidence in parliament too). For example, Philip saw the plasticity of AS cells as exaggerated during parliamentary debates:

> Philip: I think it was a time when there were some apparently startling observations in the stem cell field about adult cells, I think those observations gained a lot of exposure. They were politicised to quite a substantial degree because of this thing as seeing this as an alternative to working on embryonic stem cells. So it was partly the media, perhaps even partly the scientific journals, perhaps made somewhat more out of these claims than was merited.

Phillip is suggesting that AS cell plasticity was hyped. By focussing on differentiation potential and describing ES cells as more plastic (therefore more promising) than AS cells, he attempts to "monopolise" cognitive authority for ESCR and away from ASCR. ASCR is excluded from within the map of promising research.

Another researcher also dismisses suggestions that AS cells are as plastic as ES cells, but highlights a variety of other criteria which suggest that all different kinds of stem cells should be investigated:

> Victor: You know it's the same argument about adult versus embryonic stem cells, 'hey adult stem cells do this, why should we use embryonic'. I agree, but the problem is you can't proliferate them that fast, you won't get the numbers you

> want, they don't have the same differential potential, you need to treat them differently. So therefore, let's work on both. Let's actually find out the best way to do it, rather than the safest way to do it initially, see if we can actually get this whole system to work. And then try to see if you can transfer information from one system to another one that might be more safe or more practical.

Victor suggests that there are various criteria which distinguish between cell types, beyond differentiation potential, such as cell proliferation or culture conditions. All these criteria are "technical", and the ethics of using different sources does not, here, come into his judgment. He is "protecting the autonomy" of SCR from infringement by ethics. He is also promoting SCR in general and not reflecting on what may loose funding or authority if we work on all these different types of stem cells.

Other researchers equate plasticity with promising research, and suggest that cells with a high differentiation potential, but less ethical problems than ES, cells should take precedence over these:

> Simon: We were all absolutely gob-smacked to read this paper last year, absolutely amazing paper…. [all these different cell types] they're all present in cord blood. So the whole stem cell/ cloning debate is finished, well there's no debate, [there] is no ethical, no possible ethical objection to collecting cord blood.

Simon is discussing findings suggesting that cord blood stem cells can give rise to cell types from all three germ layers. He portrays these cells to be as promising as ES cells, but without ethical problems. For him this does not lead to a loss of autonomy of science, which has to defer to ethical criteria; rather, he constructs the map of "good" SCR to include "ethical" SCR. He describes the discovery of these more "ethical" cells as sufficient to resolve the stem cell debates.

For Simon, ethics and plasticity are the main criteria by which to decide which type of SCR should be pursued, whereas for Peter and Philip, suggestions of adult cell plasticity are not considered enough to stop ESCR. This difference mirrors these participants' professional interests, with Philip and Peter working on ES cells, and Simon focussing more broadly on therapeutic applications (and therefore being less threatened by the potential of AS cells, taken as cord blood stem cells here). However, Victor (above), who like Peter and Philip dismisses claims that AS cells are as plastic as ES and supports research down all avenues, works on understanding cell differentiation. Therefore his defence of ES cells does not advance his narrow professional interests (I return to the idea of defending science as a whole, rather than any one particular area in the following chapter).

Another researcher does not consider adult cell plasticity to be mere hype, unlike Philip and Peter:

> Nathan: We've had a lot of trouble getting our paper published particularly because you have embryonic stem cell groups competing for money. And now that adult stem cells are coming out and being shown to be able to do a lot of things that embryonic could do, without all of the ethical dilemmas and, like immune rejection issues that the embryonic ones have, that they're I guess being subjective in their analysis of a paper that's submitted for publication. They say: well these guys now are competitors, adult stem cell guys are going to be taking a lot of our money away because we're being restricted by legislations and also because they've been showing great things so...

Nathan is a PhD student. He works on adult cell plasticity. For him, differentiation potential is an important criterion to judge stem cells by, and he places ASCR on the side of promising research. However, as for Simon, plasticity is not the only criterion. Nathan highlights ES cell ethical and immune rejection issues in order to promote ASCR and exclude ESCR from good research.

In addition, the above quote is particularly interesting as Nathan is unusual in that he highlights the "political" nature of science: even when research on IVF embryo-derived stem cells is legal (as it was at the time of this interview), he argues that too many achievements from AS cells risk directing funds away from ESCR. Nathan suggests that researchers (peer-reviewers here) are still concerned by the challenge posed by ASCR, and therefore less willing to promote the publication of findings in support of AS cell plasticity (which is what the un-published paper Nathan is referring to was about). He "accounts for the error" of the peer-reviewers by pointing to contingent professional interests (see 2.5.2). This difficulty in publishing could also reflect the global nature of science: the peer-reviewers may be in a different country such as the USA, where ESCR is still not legal. Nathan, here, reflects on the limited and competitive nature of funding.

4.4.4 Safety and control as criteria

Some people, rather than focussing on differentiation potential as the hallmark of a good stem cell, focus on the need for safety and controlling this differentiation:

> The stem cell is a bit like the griffin of mythology – half lion, half eagle; grand and powerful, but potentially monstrous in effect. These essentially unspecialized

> cells can renew their own population while supplying cells that mature (differentiate) into the specialized cells necessary for all tissues. Although this ability to reproduce and self-renew is sublime when functioning properly, its disorder creates masses of dysfunctional replicating cells. (Janzen and Scadden, 2006)

If the differentiation of stem cells is not controlled, their use in the clinic could lead to dangerous effects such as cancers.

The differentiation potential of ES cells was used as a negative point by opponents of ESCR during Australian parliamentary debates:

> Why would you want to put cells into a person which have the potential to change into other cell types that are not required? Those particular cells, due to their totipotential, can give rise to teratomas; that is, tumours formed by cells which can give rise to multiple tissues. *The greater plasticity of embryonic cells is a disadvantage, not an advantage*. (Prof. Good, Official Committee Hansard, 2002, September 19th: CA 127-8)

Similar arguments are present in my data.

Some of my informants, both in the UK and Australia, are concerned about clinical trials going on around the world where different types of stem cells are injected into people, without a good understanding of how differentiation operates, and therefore without being able to control it. For John, in the UK, this sort of work is pseudo-science, which he opposes to "real science":

> John: Well, real science, I mean, what I mean by real science in that instance is not just sort of stuffing stem cells into my favourite injury, but actually doing some developmental biology and finding out how you're going to control them.

Here, understanding control is more important than using the cells with the most differentiation potential. John does not exclude a particular cell type, but rather certain practices from his map of good research.

Nathan (I introduced above), excludes ESCR from good science by trying to monopolise resources for ASCR, which he describes as safer:

> Nathan: I think adult cells are miles ahead, miles, miles, miles ahead. I mean people have had [...] bone marrow grafts for years, all of the animal work shows that they don't make tumours, there's no problem, it's a benign thing in a way. Whereas the embryonic one, just about every animal experiment you see, and the collaborators with us have seen, o my god. I've seen a rat, they put [ES cells] in the brain or something, and it's growing right out of his head, walking around

> with this big growth sitting out of its skull. That's an extreme case, but that's sort of how it is now.

Nathan draws the map of good SCR to include the use of cells that are well studied and understood, and therefore proven to be safe; he excludes the use of newer ES cells, which so far seem to cause cancer.

Ted, from the UK, contrasts ES cells (high differentiation potential and high risks) with AS cells (lower differentiation potential and lower cancer risks):

> Ted: [...] we know [ES cells have] got a potentiality, in some ways they've got too much potentiality. The problem with adult stem cells is, we know that they're not carcinogenic but do they [have] a potentiality? So the kind of mirror image problem if you see what I mean. And I suspect at the end of the day, it's not going to be either adult stem cells or human ES cells, it's going to be some kind of mix of the two or one informed by the other.

Ted suggests that finding pluripotent cells is not the quintessential goal of SCR. In contrast to Nathan, however, he does not use the risk of cancer as a reason to monopolise resources just for ASCR and exclude ESCR. Rather he suggests he wants to promote SCR in general: all these problems can be overcome by more research (cf. Parry, 2003a; under review)

For other informants, although more research is needed to reduce the risk of cancer, the focus is on improving ES cell work, rather than studying different types of stem cells:

> Peter: So there's a big risk of embryonal carcinomas and that's something that has to be overcome, and there are ways, potential ways of overcoming that by sorting by FACS[68] or by some sort of techniques, sorting the good cells away from the bad cells.

Peter's rhetorical strategy aims at "monopolizing" resources for further research on ES cells, and away from ASCR. The risk of cancer is a particular challenge in the context where one of the main ways in which to judge good research is whether it holds therapeutic promise. I now turn to this final criterion.

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[68] FACS, or Fluorescent Activated Cell Sorting, is a technique for sorting labelled cells, based on the light they emit.

4.4.5 Research tool or therapy?

As Parry suggests, it is often necessary for proponents of a new and controversial area of research to create a demand for it (2003a: 155) and:

> Crucial to the construction of potential user groups as desperate was the construction of SCR as the only hope of producing therapies that would cure people (2003a: 158).

Although this discourse was commonly used by my informants, they also drew upon a duality often used in cultural cartography: the idea that science can be "pure" as well as "applied" (e.g. Gieryn, 1983: 791). Informants not only highlighted SCR's direct therapeutic potential via transplantation therapies, but also its role as a research tool.

Sylvester, whom I introduced above as a firm supporter of ASCR, conducts much of his boundary-work to "monopolise" resources for ASCR by focussing on its therapeutic applications. He dismisses the clinical potential of ESCR as hype:

> Sylvester: the debate was being driven by the promises of using [ESCR] for therapy, which seemed to be a little premature at that stage, I mean the promise is always there, but the promise is there for anything you can think about, and the idea that you want to cure disease, doesn't translate into the fact that you can do it.

He is in essence critiquing the use of "promise" in the same way that Kitzinger and Williams critique the use of "hope" in discourses promoting embryos research: proponents of controversial research can use words such as "hope" to conjure images of "limitless and imminent potential", without having to provide any hard evidence. Hence, if nothing materialises, they have an "escape clause" as they were only ever talking about possibilities, not making forecasts (Kitzinger and Williams, 2005: 738). However, as Sylvester argues, "the promise is there for anything you can think about". Instead, Sylvester prefers focussing on evidence of therapies that do work (as the ones from ASCR discussed above). For him, whether ES or AS cells have a good differentiation potential is not the crux of the matter; it is whether they have a good therapeutic potential.

Rachel, another Australian researcher, highlights the importance of focussing on therapeutic, rather than differentiation, potential:

> Rachel: There's been a lot of controversy in the stem cell field. Some people just do not believe that blood, bone marrow stem cells, can actually form other tissues. Other people believe it's true. We certainly find evidence that at a very low

> percentage, cells from [an adult tissue] are coming in and forming [cells in another adult tissue]. So I think it happens, but not very often. So whether this is trans-differentiation, [...] whether it's fusion for example, [...] if it's actually just an inflammatory cell coming in and playing a role in the repair process. So whether that inflammatory cell is actually forming a new cell, we don't actually know. Like it might just be part of the repair and remodelling process that tissue goes through during inflammation. But that's fine [...]. So we try to think of it as a balance of injury and repair, and it's really important to understand those processes, more in order to try and control that.

For Rachel, what matters is not whether AS cells are pluripotent and can "trans-differentiate"; rather, it is that they can contribute to injury "repair" and thus have clinical applications. The aim of good research is to understand how cells can contribute to organ repair, not to find a pluripotent cell. Therapeutic potential is not diminished if this repair occurs through "fusion" rather than "transdifferentiation". Lack of plasticity is not a reason to exclude a cell, rather, lack of therapeutic potential is. However, the potential does not have to be directly via transplantation, it can come from understanding basic cell.

Gary, an Australian PhD student working on AS cells argues that these hold the most promise as they are "easier" to use in therapy. This promise also comes from a good understanding of how these cells develop i*n vivo*:

> Gary: I think [autologous cell therapy] will be one of the more, easier, on a relative scale. It'll be some of the easier things to do rather than differentiate ES cells into particular lineages and then re-introduce them back into a patient, um. I think yeah the idea of isolating a patient's own stem cells, expanding them in vitro and then putting them back in, is a very tempting possibility, um. But even more so I think understanding the niches that stem cells are living in the body and how they function normally. And then trying to stimulate them, sort of give them a kick start rather than taking them out, and growing them up and putting them back in.

Gary does not dismiss ES cells for being less applicable in therapy; indeed, all different kinds of cells types can provide information about developmental processes.

Although transplantation therapies are often touted in public fora, many of my informants do not see them as central to ESCR. For example, Ted from the UK argues that while transplantation may be what comes to our mind first, the promise of SCR may lie elsewhere:

> Ted: I think there's a potentiality that the field will allow us to understand or develop mechanisms for regenerating tissue or supporting tissue. [...] and that's not necessarily though transplanting cells. It might [be] that with embryonal or adult stem cells allow us to grow tissues [...]; or maybe grow subsidiary stem cells that could be re-implanted [...]; or it might be that they will allow us to understand

> the mechanisms by which such tissues can be regenerated and then develop standard pharmaceutical drugs with which to intervene; or it might be that it will allow us to develop tissues which we can use to test drugs on for example and develop new drugs. Because at the moment, pharmaceutical companies find it very difficult to test both the effects of drugs, and drug toxicity in animal models and animals are different to humans in important ways.

SCR is described as very interesting primarily because it is new research tool, and secondarily because it may have applications in drug development or directly in therapies. Here, stem cells do not need to be transplantable and pluripotent in order to be promising and worth studying.

The dual construction of science as pure and applied is a particularly useful resource for talking about the promises of SCNT. Although public discourses about SCNT often focus on patient-specific therapies, I find that only four of my informants considered this a likely possibility (one of these was a scientist giving a presentation during a multi-disciplinary discussion, and was therefore talking in a much more public context than my interviewees). Many informants, like Terence in the UK, describe SCNT as a research tool, rather than a therapy:

> Terence: I mean, I think the consideration of really at this moment in time, getting somatic cells and developing using oocytes and developing cloned ES derivatives for cell therapy, I mean it is an attractive technical... possibility and I think the jury is still out as to whether it is a probability. There are too many technical issues and unknown factors associated with the major aspects of the technology. [...] At the moment I would say for me, it [patient-specific therapies] is still fiction rather than fact.

Although Terence paints SCNT as unlikely to provide therapies directly (it is a “possibility”, “fiction rather than fact”), he argues it is a promising research tool and therefore he does not exclude it from his definition of SCR. Thus, the duality of science enables this informant to “monopolise” resources for SCR as a whole, by highlighting either its use as a research tool, or as a therapy.

4.5 Conclusion

This chapter shows that there are no unique definitions for “stem cell” and other labels related to this area of research. It highlights that different people centre SCR on different areas, including ESCR, ASCR, “therapeutic cloning” and adult stem cell plasticity. For some, only one area is central; for others, a combination of areas constitutes SCR. Depending on which stories people tell, they raise different promises and issues, and imagine different trajectories for the research. These different emphases of stem cell definitions may indicate that “stem cell” is a boundary object (Star and Griesemer, 1989): the same label is used by different people who give it different meanings and this may facilitate institutions such as stem cell centres to be set up, or stem cell consortia to receive funding.

The fluidity of the term “stem cell” can be paralleled to that of the term “biotechnology”. As Bud (1989) argues, the latter also had historically changing definitions. It was used to describe the study of fermentation processes decades before it was associated with recombinant DNA technologies. This *hijacking* of an old term to describe a newer area can also be seen in the application of the label “stem cell”, initially used to describe gut epithelial stem cells, to embryo derived stem cells. Similar to “biotechnology”, people who draw on the label “stem cell” do not always think about how it has been used in the past.

The conceptual fluidity in SCR enables my informants to conduct boundary-work in various ways, using diverse criteria. This chapter also explores attempts at “monopolising” resources and cognitive authority for different aspects of SCR. Some of my informants paint SCR as an emerging field, with much therapeutic potential, that uses cells with a high level of differentiation potential. Others paint it as an established field, with therapeutic potential, that uses cells that are “pluripotent” and or easy to control. The promise of SCR is also made by appealing to the importance of basic research or of doing research that does not go against “nature”. The demarcations between different types of SCR are not just made by measuring cells’ differential potential, or by contrasting “adult” to “embryonic” SCR.

The UK and Australian parliamentary debates often centred around the “question of life” (e.g. Parry, 2003a; Harvey, 2005); even though many participants tried to focus the debates on “technical” issues (as I discuss in 4.1 and 4.3.3 above). In contrast, by looking at stem cell researchers discourses, during these interviews, it becomes apparent that the adult/embryo binary opposition is often constructed as revolving around “technical” factors (I look at this in more detail in chapter 7). This enables my informants to promote their own area as well as research more generally. In particular, by talking in terms of therapies, scientists can enrol patients and emphasise the importance of science in helping society. They can exclude particular cell types as not promising, either because they are not “plastic:” enough” or because they are likely to cause tumours.

In summary, there are many co-existing understandings for the concepts used in SCR, and these are given different levels of importance by different stem cell researchers. There is no unique way of “understanding” what SCR is about. I would argue that for most public “misunderstandings” the same “misunderstandings” exist among scientists. However, public debates are often set up in such a way that participants (both publics and scientists) feel they can only contribute if they know the “facts” (as I discuss in 2.2.4). This framing of debates and people’s assumptions that there are unique definitions needs to be unpacked and examined if we want to open up public debates to broader publics (I return to this in chapter 7). Therefore, I suggest that the ability to give exact definitions of “stem cell” cannot be a requirement for participating in discussions about the future of SCR, and the constructedness of these multiple definitions needs to be recognised. Social scientists can highlight this diversity to scientists policy-makers and diverse other potential engagement participants.

In addition, the focus of debates on certain contested meanings and not others excludes many relevant aspects from discussions. To paraphrase Star (1985): the more stem cell researchers argue with one another about how to use stem cells (such as adult or embryonic), the less salient the question of whether to use them becomes[69]. By focussing on whether we should use adult or embryonic stem cells, or by focussing on the virtue of pluripotent stem cells, we are not asking if money could be better spent on recruiting carers, improving access for wheelchair users or promoting healthy eating, for example. This emphasis on local debates hides broader uncertainties about the role of science in society. As Star puts it, this reduces uncertainty by translating local uncertainty to global certainty. The next chapter focus on how scientists manage uncertainty.

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[69] The original is: “The more localizationists argued with one another about *how* to do, for example, ablation experiments, the less salient the question of *whether* to do them became” Star (1985: 412).

Chapter 5

(Un)certainty discourses and SCR futures

5.1 Introduction

Chapter 4 traced some of the diversity in definitions of SCR and accounts of what constitutes "good" or "promising" research. This chapter looks at my informants' discourses in more detail and examines how social locations can shape these. It focuses on futures imagined for SCR and the uses of discourses of (un)certainty.

During public discussions about SCR, a number of futures for this technology are presented. These can be full of hope or fear (e.g. Kitzinger and Williams, 2005). Regardless of the pessimistic or optimist contents of these futures, they are usually presented as if certain or inevitable; for example, if ESCR is legalised, cures *will* be found (Kitzinger and Williams, 2005: 735-6). Thus, modernist notions of prediction and control, and the equation of knowledge with certainty are powerful tropes that dominate many discourses around SCR and its applications for therapies. However, data from more private interactions, such as interviews, can reveal more subtlety and uncertainty in stem cell researchers' discourses.

STS has long been concerned with "certainty" in science and it is an interesting site for sociological investigation. As Campbell puts it:

> since the authority of expertise is predicated on superior knowledge, the question of uncertainty touches on the credibility and importance of scientists as experts, with the result that the amount of knowledge becomes a major theme in the debates among scientists when they appear as experts. (Campbell, 1985: 429)

Pinch finds that researchers most intimately involved with a particular area of knowledge often express high levels of confidence in it (1981: 151). However, they are also aware of many uncertainties within their area, which they may or may not choose to voice (1981: 153-4). Collins (1987) also finds that researchers want to project images of certainty in public fora, particularly on television. (Un)certainty discourse can be used strategically (Campbell, 1985). Star (1985) argues that scientists have many ways in which to transform what she calls "local uncertainties" into "global certainty", or "facts". For example, she finds that scientists may delete uncertainties besetting the classification of pathologies, which arise from the divergence of symptoms between individual patients, by choosing to focus on "exemplar" cases.

Uncertainty can also be reduced by mobilising possible futures as interpretive resources. Brown and Michael discuss how these futures can be studied not as a "neutral temporal space" but as an "analytical object" (2003: 4) and how this can be used to build on MacKenzie's certainty trough. From this emerging "sociology of expectations" (see also Brown, 2003; Borup *et al.*, 2006; Horst, 2007) arises another interesting feature of (un)certainty discourses: performativity. For example, expectations of future "biovalue" for particular types of stem cells (Waldby, 2002) can result in parents banking their babies' cord blood in the present (Brown and Kraft, 2006). In particular, Michael finds that the performativity of imagined futures can include a variety of aspects:

> the presentation of self, the production of subject positions for readers/viewers, the enrolments and alignment of various others, the bringing into being of a particular state of affairs. (Michael, 2000: 22)

Consequently, the performativity of language also needs to be investigated

This chapter explores how stem cell researchers talk about imagined futures for SCR and examines their mobilisation of (un)certainty discourses. It starts by discussing a particularly useful tool for exploring (un)certainty: MacKenzie's certainty trough (1990; 1998). It shows how this model helps to make sense of stem cell researchers' discourses with relation to their distance from knowledge production. The chapter then highlights some data that does not fit the certainty trough and builds on Lahsen's critique of this model to develop it by taking into account certain features of knowledge production and the psychology of knowledge producers. The subsequent part of the chapter examines some of the "recurrent interpretative resources" (Gilbert and Mulkay, 1984) my informants mobilise, and explores the performativity and strategic uses of (un)certainty discourses, including: the necessity of displaying certainty in public, the mobilisation of pasts and futures as well as the change in discourses as technologies become more established. The chapter then shows how social identity theory (e.g. Hogg *et al.*, 1995; Stets and Burke, 2000) can help build on the certainty trough by focussing on the sorts of identities scientists mobilise in particular social circumstances. I conclude by discussing some of the implications of these (un)certainty discourses for public engagement in science.

5.2 Using the certainty trough to look at discourses around SCR

During each of my interviews, I ask my informants: “What, for *you*, are the *best* promises that could come out of SCR?” This is to elicit answers beyond the usual public promises of finding therapies for any, and every, condition. I am provided with a range of responses, which I start analysing with MacKenzie’s “certainty trough”. In this section, I introduce this concept and discuss how it can be used in the case of SCR.

5.2.1 MacKenzie’s certainty trough

MacKenzie (1990; 1998) discusses the construction of certainty for the accuracy of intercontinental ballistic missiles. Building on Collins’ work on certainty in science, he introduces the concept of “certainty trough” depicted in figure 1. He identifies two groups of people who are uncertain about the accuracy of intercontinental ballistic missiles. One group includes people who are closely involved with producing knowledge about these missiles. These people are aware of the difficulties associated with measuring, assessing and predicting missile accuracy and express uncertainties concerning the accuracy estimates and their usefulness. (I indicate them by **A** in figure 1). Another group includes people who are “alienated” or “committed to an alternative weapon system” (1990: 371). They also expressed uncertainties with regards to missile accuracies (location **C**). In between these two, MacKenzie suggests the presence of third group of people: those loyal to the work and those who “believe what the brochures tell them” (1990: 371). These people lie in the “certainty trough” (location **B**). Thus, MacKenzie sees the “distance” from knowledge production as an important factor modulating people’s certainty.

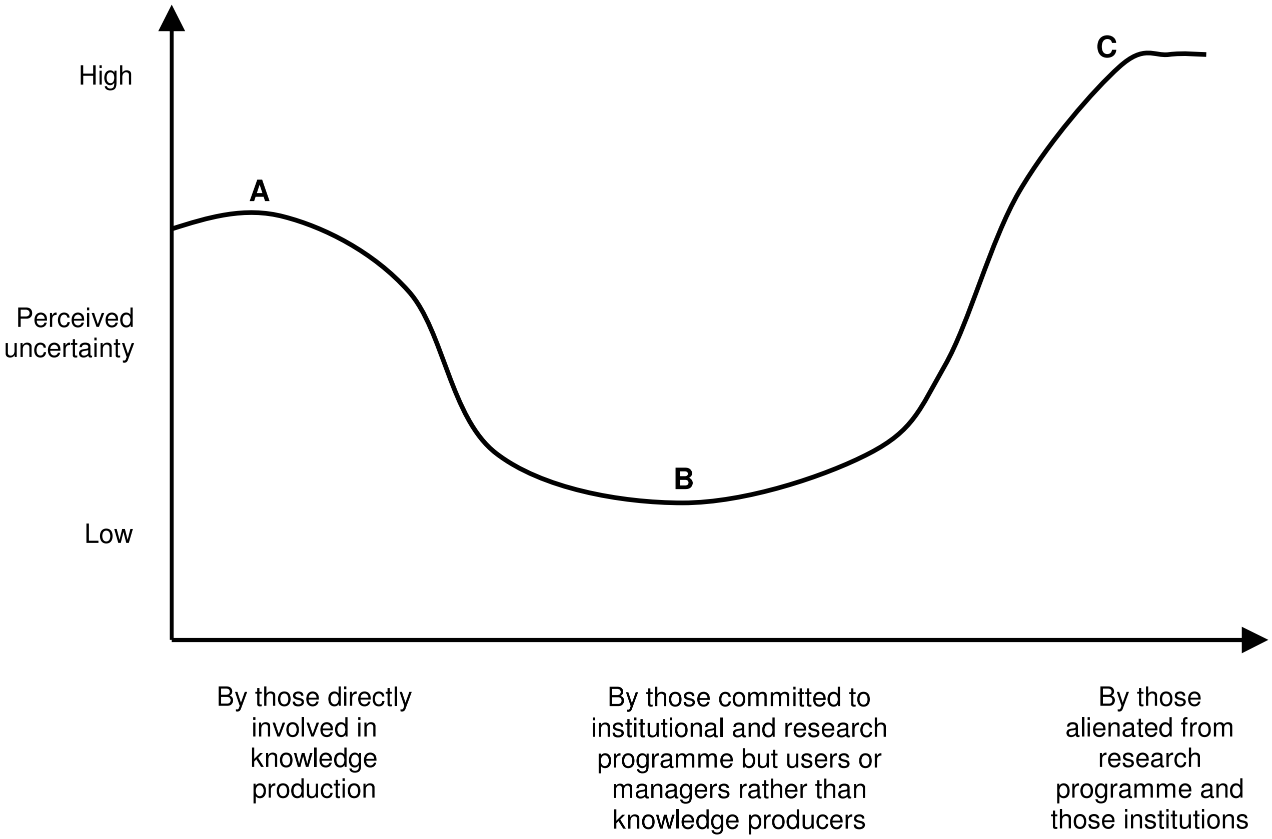

Figure 1: The certainty trough.

Adapted from MacKenzie (1990: 372)

There are three key points regarding the certainty trough that I wish to highlight here. The first is that MacKenzie couches the description of his concept in very tentative terms:

> It is possible, indeed, that this schematic and impressionistic diagram *might* describe (I am merely speculating) the distribution of certainty about any established technology.” (1990: 371, original emphasis)

From this, I assume he is suggesting the certainty trough should be taken as a model or a heuristic tool, not as a necessary description of how people *are*, and that it may not be universally applicable. The second point relates to the notion of “*established* technologies”. MacKenzie suggests that this model applies to “established”, rather than “emerging”, technologies. However, as I showed in the previous chapter (see 4.4.1), the ascription of this label to some technologies and not to others is itself a construction and a rhetorical strategy. As a result, this notion is not helpful when deciding whether to apply the certainty trough (although, see 5.4.3, below). The third point concerns the attribution of certainty: for most of his book, MacKenzie uses people’s *accounts* of certainty to establish their location on the "certainty" axis, and what he knows of their research to establish their location on the "distance from knowledge production" axis. So, I would argue that he treats these accounts as "resources" (Gilbert and Mulkay, 1984). However, I believe that strategic use of language should not be ignored and as outlined in chapter 2, I use discourse as a "topic" as well as a "resource"[70].

For the start of this chapter, like MacKenzie, I use discourse as a resource (Gilbert and Mulkay, 1984): I place my informants on the certainty trough graph according to what they told me in interviews, as well as what I know of their published works, research programs and what they said in public debates. Their placement also depends on how essential I take certain factors to be, such as their belonging to stem cell research groups. For instance, when researchers work directly on AS cells but belong to a consortium with a strong focus on embryonic stem cells, they can feel alienated from, or included in, the promises of ESCR. To decide where to put them on the graph, I mainly take my cues from side comments made during interviews, but I feel it is important to keep in mind and analyse these tensions in people's social locations. However, as I will reveal during this chapter, discourses, particularly discourses of (un)certainty can have strategic roles; therefore I will, later in this chapter, move away from MacKenzie's use of discourses as "resources", and instead use them as a "topics".

I interview a range of researchers, including junior PhD students spending most of their time at the bench, senior group leaders spending theirs lobbying politicians, people working on immunology, epigenetics, embryos, blood stem cells. As I hint in the previous chapter, they express differing amounts of certainty about the future of different aspects of SCR. To make sense of these data, I placed my informants on certainty trough graphs. However, it quickly became obvious that I could not have a graph for certainty expressed about SCR in general and I required a number of graphs to break down the different aspects of SCR. I developed four graphs: for ESCR using IVF embryos, ASCR (including work on adult cell plasticity), SCNT as a research tool and SCNT for therapies. These graphs (not reproduced here) were very useful in making sense of part of my data.

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[70] MacKenzie is not unaware of the strategic dimension of (un)certainty discourses as discussed in section 5.4.

5.2.2 Confirming the certainty trough

My topic guide did not specifically address the difficulties my informants were encountering in the laboratory. Nevertheless, many people mention these and how they related to uncertainties about the future of SCR. For instance, when answering a question on whether both adult and embryonic SCR should be pursued, the following female postdoctoral fellow says:

> Heidi: there'll be other disorders that maybe derivatives of embryonic stem cells will produce cells that would be important for research […] [M]y work is […] trying to derive beta cells from human embryonic stem cells, which is a way off, as you probably gathered if you were sitting in those sessions, I tried to avoid even the word beta cells!

Heidi is aware of a number of "technical" difficulties and uncertainties relating to her particular work (she is also referring to talks where many people were exposing their difficulties in similar areas). She thinks that embryonic stem cell work has a future. However she uses very tentative language – "maybe", "trying" and "a way off" – that highlights her uncertainties. She is an illustration of location **A**: she is aware of many difficulties that may beset the future use of ES cells in deriving beta cells, which is the area of SCR that she works in.

Some of the people I interview express much more certainty about the future and potential of SCR; they do not work directly in the particular area of SCR under discussion and do not raise the technical difficulties this area may encounter. They can be located in **B**. The following quote is about the promise of somatic cell nuclear transfer (SCNT):

> Peter: one of the best arguments for doing [SCNT] is that you would take a [somatic] cell from the patient, this is a sort of scenario that's I guess slightly science fiction but not too far away from a possibility anyway, […] get the nucleus, transfer it into some host, perhaps a host embryonic stem cell, […], grow up embryonic stem cells from that, that have the same genetic material as the potential recipient, then if we understood this process I mentioned before of how to properly direct the embryonic stem cells into the right type of cells, you could certainly then get a population of stem cells to transplant back into the same patient that was the donor of the somatic cell in the first place.

Peter is not involved in SCNT as, at the time of this interview, it was illegal in Australia (where he works). However, he belongs to a large research group that had publicly expressed its wishes to see SCNT legalised. Peter is aware of potential difficulties as evidenced by the use of "science fiction" or "if", but does not detail a list of specific limitations. He is much more certain about the future of SCNT than most other people I spoke to. He expresses strong confidence that the difficulties are surmountable. This is particularly noticeable when I ask him about whether access to these tailor-made therapies may be difficult:

> Peter: I don't know… even though it's expensive, […] it would get absorbed, a lot of the costs wouldn't get passed back on I think in the immediate term I think to the patient.

This scientist does not know much about technical difficulties of creating cell lines from nuclear transfer embryos and using these in therapies. Here, his distance from knowledge production about SCNT can explain why he expresses certainty about the ease of future application of this technology. He is a good example of location **B**.

Some of my informants fall into category **C**: they express uncertainty about the promises of technologies to which they are not committed. One PhD student, Danielle, after thinking long and hard about what to do for her thesis, decided on ESCR. She could think of many promises for this work, but was very uncertain as to how ASCR could be useful. I also interview several researchers working on AS cells, who do not belong to a joint adult/embryonic SCR group, and who are having trouble publishing their results. I consider them to be alienated from ESCR. These researchers, contrary to Danielle above, are very certain about the promises of ASCR, but not of ESCR:

> Connor: I expect that adult stem cells will be the way that you have therapies. I don't expect embryonic stem cells really ever to be therapies.

They could think of many obstacles to the use of ES cells and were very uncertain about their future. The commitment of these researchers to a particular technology (either ASCR or ESCR) can explain why they express high levels of uncertainty towards competing technologies.

The above data confirms the certainty trough. Nevertheless, there are also many other examples that directly contradicted it. To investigate this, I draw on Lahsen's critique of the certainty trough, which I now discuss.

5.3 Building on the certainty trough: problematising "distance"

5.3.1 Critiquing the certainty trough

Myanna Lahsen makes useful developments to MacKenzie's certainty trough. She discusses its use for certainty distributions around General Circulation Models (GCMs) in climate predictions. Like me, she points out that MacKenzie may have only meant his concept as a "heuristic" device (2005a: 897) . However, she notes that the wide use of the certainty trough requires its careful appraisal. She discusses four features of knowledge production and producers that should be included in the certainty trough to make it more powerful, particularly to improve the concept of "distance".

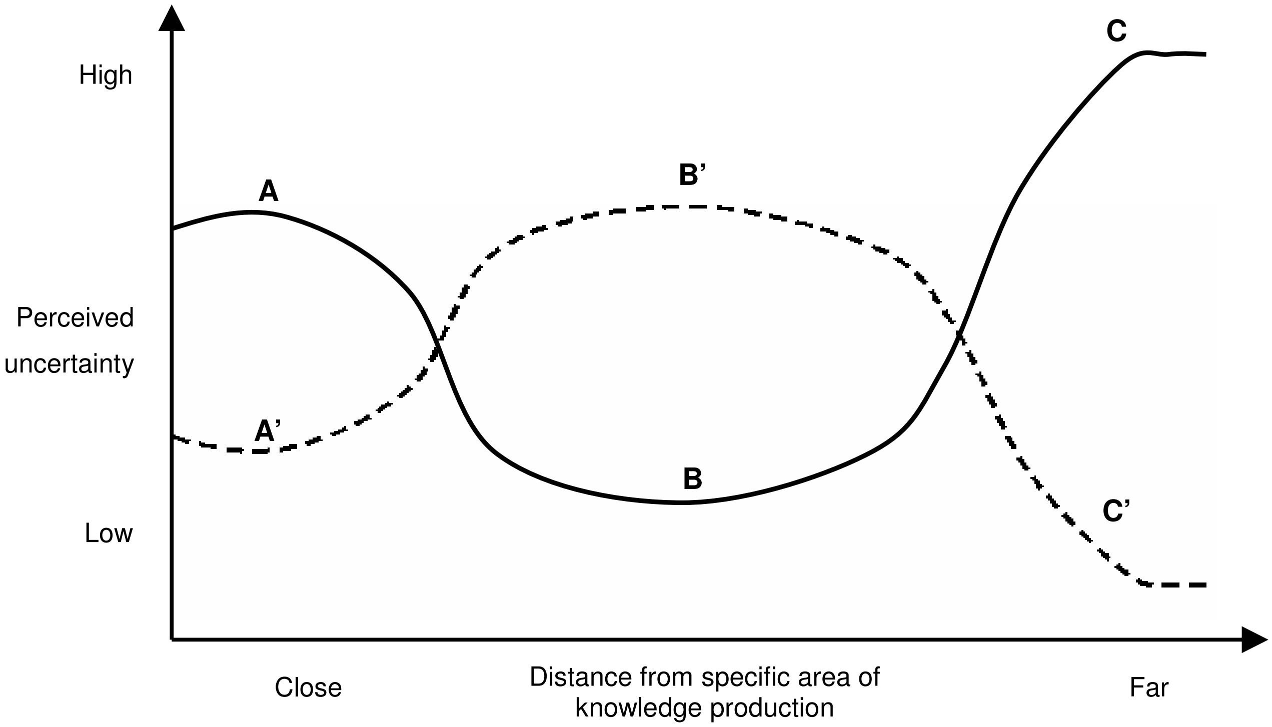

Figure 2: Building on the certainty trough

The dotted line is a mirror image of MacKenzie's certainty trough model (full line). People located in **A'** (low uncertainty, close to knowledge production) are in the mirror image location to **A** (high uncertainty, close to knowledge creation). The same goes for locations **B** and **B'**, and **C** and **C'**. People can be located anywhere between the full and dotted lines. The divergence from the full line can be accounted for by factors such as psychological factors. These are further discussed in the text.

Lahsen firstly shows that knowledge can be produced in various sites; secondly that users and producers of knowledge may be intersecting groups; and thirdly that some types of inaccuracies can be better identified by people with a different expertise to those producing the knowledge. From these first three points, Lahsen argues that:

> there is no single vantage point from which to best evaluate the performance of a single complex GCM [in predicting climate change] (2005a: 897).

The fourth set of features that Lahsen suggests modulates certainty is "social and psychological factors" (2005a: 897) from which she derives the trope of "critical distance" (2005: 903). She finds that scientists may be *unable* to criticise their work, and thus their degree of "certainty" may be difficult to establish. Critical distance has two components. Firstly scientists can become "seduced" by their work and therefore unable to *discern* uncertainties in it; they are too *close* to their work. Secondly, they may be unable to *voice* uncertainties about their work. This highlights the *performativity* of discourse (see Szerszynski, 1999), where informants may *use* certainty to project particular images, increasing the authority of their work and enabling particular futures while disabling others (e.g. Michael, 2000; Brown and Michael, 2003; Horst, 2007); the act of describing a particular future as certain can create the social conditions for that future to materialise. This notion of critical distance is helpful in understanding how and why researchers may *display* more or less certainty in different contexts and I return to it below. It also highlights that different identities may be "salient" in these different contexts (as discussed below in 5.5).

Like Lahsen for general circulation models, I want to argue that it is very difficult, in SCR, to locate a particular point from which to measure certainty and distance from knowledge production. Indeed, as noted above (and in chapter 4), there are multiple types of SCR. In addition, there exist multi-site and multi-national research groups in collaborations as well as in competition. Like Lahsen, I firstly find that knowledge about stem cells and their application comes from diverse sites: laboratories work on different aspects of this field (such as immune rejection or finding appropriate media to grow cells in) and the application of SCR requires knowledge from clinics, commercial ventures, pharmaceutical companies, policy circles etc. Secondly, users and producers can be one and the same people: someone organising a clinical trial can use knowledge claims from the laboratory, but produce another set of knowledge claims. Similarly in the context of public engagement in science, patients, some of the targeted end-users, also have knowledge relevant to the application of SCR, and some big research funders encompass or represent patient groups (like the Juvenile Diabetes

Research Foundation). Thirdly, and following from this, people who do not work at the bench can be better placed to identify uncertainties that could affect the future of SCR (for example, working with IVF patients can highlight that embryos are a precious resource , indicating it might be difficult to obtain enough for research). Fourthly, certain informants appear to find it difficult to *see* particular uncertainties.

The above four points problematise the notion "distance" and explain why some people appear close to the site of knowledge production, but express a lot of certainty (location **A'**) and others who appear one step removed from knowledge production, but express uncertainty (location **B'**). I examine these in the following section (I focus on the difficulty in *voicing* uncertainties, leading to people being located in **C'**, in section 5.5).

5.3.2 Data for locations A' and B'

One of the PhD students in my sample is a particularly good illustration of location **A'**. He works on developing cellular therapies for a particular disease using AS cells from the affected patient (autologous therapies) and expresses certainty regarding the future applications of his work:

> Gary: I really think that the possibility of autologous cell therapy is really the most interesting, and probably will be the most beneficial at least in the shorter term.

His confidence in the future of his work is suggested by his use of "most beneficial" and "shorter term". In addition, when I asked him about whether access to these treatments would be a problem, he replied:

> Gary: Once a high-throughput system has been developed, I think it would be [viable] […] I think yeah, once the system's established, hopefully, the technology that's required to do that would become more affordable, more accessible to everyone.

Gary expresses certainty with regard to the future application of his work in the clinic he seems certain that it will be technically and financially straightforward to obtain these therapies. I can see three possible reasons for this apparent contradiction to the certainty trough. Firstly, Gary is only close to one aspect of knowledge production, thus he may not be aware of all the potential difficulties in bringing cellular therapies to patients (such as the difficulties in obtaining approval for clinical trials or the high financial cost of setting up these trials). So, although he is close to knowledge creation about how to derive stem cell lines from adult cells, he is far from knowledge creation, for example, about the application of cellular therapies. Secondly, he is possibly certain that there will be no difficulties in applying his work because he has been successful in his research so far, and therefore may not have encountered as many difficulties as other more senior researchers, or less successful PhD students (he has a good publication record and his supervisor describes him as the best student she has ever had). Finally, it is possible that he is simply repeating his supervisor's view; I did not interview her, but from her public talks, she too seems committed to developing cellular therapies. Therefore, both Gary's successes and his social location in a group that sees the promise of autologous therapies can explain his difficulty in seeing many factors that may limit the clinical application of his work. A combination of factors can therefore explain his location in **A'**.

During my analysis, I also place people in **B'**. In terms of distance, these researchers appear to be one step removed from a particular area of knowledge production to which they are committed and yet, they display many uncertainties towards it. Some people in **B'** are removed from a particular area of knowledge creation, such as studying ES cells for use in therapies, but work in a related area, such as immunology. They may display uncertainty towards ESCR because they know of the difficulty of solving immune rejection of ES cells from their own work. Other people in **B'** are removed from a particular area of knowledge creation because they are program directors and spend more time in the public eye, talking about the research, than at the bench. They may display uncertainty towards the particular area of SCR their group is working on because they have learnt from others about a diversity of limitations to SCR. For example, they may learn about ethical difficulties of oocyte procurement from participating in public debates, or about the problems that hyping can cause from talking to disappointed patients. These two explanations are related, as public spheres can be considered alternative sites of knowledge production.

As an illustration of **B'**, John works on spinal cord repair. Part of his research is committed to *using* AS cells, but he is also very interested in the potential of ESCR. However he expressed uncertainty with regard to any of these cells sources to be miracle cures. When I asked him how people with spinal cord injuries saw him and whether they thought he was coming to "save" them, he answers:

> John: Well, spinal injury patients are a bit more canny than that, they've been around, they've had their injury many of them for decades, and they've seen scientists come and go, they're aware that nobody has actually produced a cure

> for them yet, so they look at you with a fairly cynical eye. But, no I think it's clear to all of them that there are treatments coming to clinical trials soon for them, which probably are going to offer some benefit, so they're very interested and they're interested in talking to you.

Although John is committed to SCR, he is uncertain of its future, partly because interactions with patients have highlighted that cures do not always come along as promised.

In summary, it is difficult to determine a single site of knowledge production in SCR (and in science in general) from which researchers may or may not be distant. One could imagine solving some of these difficulties by drawing up different certainty trough graphs for different aspects of SCR (as I did when starting to sort through my data). However, this is not very satisfactory, as there are probably infinite sub-fields and knowledge production sites which are relevant to SCR. In addition, "critical distance", particularly the strategic use of language can further hinder the use of the certainty trough. I now turn to some of the regularities in my informants' discourses around SCR futures. In particular I show how performances of (un)certainty can give strategic meaning to locations B and C on the certainty trough.

5.4 Performing (un)certainty and SCR futures

Up until now, I have been doing what Gilbert and Mulkay (1984) would call "classic sociology": I have been using interview data and other sources (such as publications) to find out about my informants' views on the promises of SCR – and therefore using discourse as a resource. However, many social scientists argue that language can be performative (e.g. Szerszynski, 1999; Brown and Michael, 2003, see also 2.2.4) and it is necessary to also use discourse as a topic. Looking for interpretive resources and regularities, and turning to analytically prior questions about how my informants talk and in which contexts, this section shows how stem cell researchers resolve some of the inconsistencies in their accounts. It highlights factors that influence informants' discourses and pays particular attention to the "repertoires"[71] that they use. In particular, I look at how they use (un)certainty discourses as interpretive resources since these are performative and can serve to "enable some technoscientific worlds and disable others" (Brown and Michael, 2003: 14).

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[71] For an introduction to Gilbert and Mulkay's use of these repertoires, see chapter 2.

5.4.1 Evincing certainty in public

The notion of "critical distance", especially its second component, highlights the importance of the context in which discussions take place. Donald MacKenzie was not unaware of this. He discusses the case of Charles Stark Draper who, as well as being close to research on intercontinental ballistic missiles, was a key actor in their promotion. MacKenzie suggests that when technologies are "emerging" and when many people are criticising them, "there are often to be found charismatic technical figures, close to its heart, who *evince* great certainty" (1990: 371, emphasis added). He calls these people "heterogeneous engineers", as they must recruit and organise people and artefacts in a variety of worlds. By using the term "evince", he highlights the strategic aspect of language and suggests that particular discourses can be useful resources for promoting the authority of a research area. As I have already stated, the difference between "established" and "emerging" technologies is partly rhetorical. Thus, evincing certainty and heterogeneous engineering are necessary at any moment when the struggle for cognitive authority is heightened (cf. Gieryn, 1995); this can occur when technologies are seen and/or constructed as "emerging".

One of my informants is very similar to Charles Stark Draper and I would consider him a heterogeneous engineer. He is very active in the field of ES cells: although he spends less time in the laboratory now, he is one of the vocal proponents of ESCR. This locates him in **B** on the certainty trough.

> Barry: I think in the application side, when you have diseases like respiratory diseases growing so rapidly, throughout the whole wide world, and the only thing you can do for that is a whole lung transplant, I mean, we need embryonic stem cells for cell therapy, we've just got to get there; because I can't see any alternative. The harder we work, and the [more] careful the work we do, to establish the efficacy and the safety of these methods, then, I think we'll have a new medicine that'll be extraordinarily powerful.

In this extract, he evinces certainty about the potential application of ES cell therapies for respiratory diseases: he is using therapeutic potential to promote ESCR (see chapter 4). In particular, when he says "we've just got to get there", his tone of voice implies that it is just a matter of getting the work done, and therapies will inevitable come forward.

However, by looking at other sources than this interview, I find that this informant could be uncertain about the future of ESCR, particularly given the competition from other forms of stem cells, as suggested by a comment from one of his colleagues:

> [He] is fully aware of [the publication of a paper showing the promises of ASCR], he's a little bit threatened by it because the empire might come crumbling down.[72]

Rather than decide which account I believe and where Barry should *really* be on the certainty trough, I am asking why he expresses so much certainty in the quote above, and particularly, why he does not mention the possibilities that could come from ASCR at any point during our interview. I would argue that Barry is making a *strategic omission* to promote the authority of ESCR. He does not engage in boundary-work. He is not creating a cultural space from which he can exclude ASCR, for example; he simply does not acknowledge the possibilities that could come from research other than ESCR. This goes further than the cases discussed in Irwin and Wynne (1996) where lay knowledge is excluded because unrecognised; here, ASCR is strategically ignored by a researcher who is well aware of it. By not addressing the possibility of ASCR, he is hoping to evince certainty and promote the authority of ESCR. Here, silence is very powerful and Barry's certainty can be performative: by talking about ES cell based therapies with such certainty, he can create a situation where these therapies can exist.

Other informants also used certainty discourses as interpretive resources to promote their field. Some did this by displacing the responsibility for reducing uncertainty into other spheres or social worlds (cf. Shackley and Wynne, 1996: 290-2). For example, they move indeterminacy to another field (such as immunology), another area (such as policy), or another social group (often "wider society"). This is highlighted in the following example where a group leader comments on his responsibilities for the potential applications of his work:

> Peter: I mean there's so many steps that, […]a lot of safety steps, you know animal models, phase I clinical trials which are safety trials basically, all those things are really under the control of the drug company and it's up to them to work out really whether the lead that I've given them is dodgy or not, I feel.

By doing this, he places the risks and their resolution under the remit of drug companies. In this way, he is free to continue his work without worrying about the uncertainties it creates. Uncertainty is placed outside his area, certainty inside.

Other researchers also highlight certainties, rather than uncertainties, in other fields; this can serve to create or maintain certainty in their own field, or in an area of research more

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[72] I have chosen not to attribute this quote to protect the speaker's anonymity, but also because I do not want to focus on the social location of this speaker.

generally (cf. Star, 1985). For example, researchers in cell therapies use the apparent certainty and feasibility of other therapies, like organ transplantation, to justify their work, even though the difficulties faced by these technologies may be very different from each other. They are bringing (what Star would call) "outside" certainty "inside".

Another way of displacing uncertainties about one's work is by projecting them, and their resolution, into the future. The following quote is from a scientist who is very committed to ESCR. He makes a list of future achievements of ESCR, placing any difficulties in the future:

> Ben: In the short term, there'll be discovery of a whole new range of drugs that will help diseases that are based on driving of embryonic stem cells into various lineages or the prevention of disease phenotypes. I clearly think that these are the short term opportunities, I think the medium term, is that we'll be able to use some of these cells to treat, by cell therapy, a number of different diseases, but also that you'll be able to use these cells for gene therapy in patients who have genetic disease and if you can do that for a young person, I think it's marvellous. I think we'll be able to reprogram the immune-system, […] that is probably more medium to long term aspirations, but it's all there […]. I whish I was 20 years old and starting because I think it's very exciting times.

This list projects certainty about the potential accomplishments of ESCR. By "scheduling into the future" and "identifying when and how key uncertainties will be reduced" (Shackley and Wynne, 1996: 287-90), Ben can *perform* certainty. These strategies, which Shackley and Wynne call "boundary ordering devices", are similar to "truth will out devices" or TWODs (introduced 2.5.2, see also below in 5.4.2) in that they too prevent present day uncertainty from challenging present day authority of science[73].

Nevertheless, not all my informants evince certainty in the context of the interview. Participants' views of my identity (as a fellow scientist, as a young female, as a member of the public, as a conduit towards broader publics, as a journalist) is revealed in the forms of argumentation they adopt. For instance, when they use the same discourse in our interview as in public fora, this suggests they see me as a member of the public or a conduit towards them. This is similar to what Pinch finds:

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[73] TWODs give more of a sense that, somehow, contingent factors will be eliminated, and objective truth can be revealed; their aim is to legitimate empiricist conceptions of the world in the face of evidence of contingencies. Whereas boundary ordering devices show more specifically how uncertainties will, actively, be reduced; their aim is to "secure the relevance and authority of a particular kind of knowledge [...] and of a corresponding kind of policy making" (Shackley and Wynne, 1996: 293).

> Many informants seemed to regard me as a representative of the public and hence as a vehicle by which to transmit the public face of the debate" (1981: 154)
>
> [...] when scientists perceive a possible public audience they tend to act defensively and stress the certainty of their own areas – while at the same time, doubting the certainty of others' (1981: 155).

However, in contrast to what Pinch argues, some researchers treat me like a fellow scientist taking an interest in promoting science to "lay" people; they confide in me and share their uncertainties. Thus, I observe a variety of (un)certainty discourses in these semi-private interactions.

Many of my informants draw on discourses of certainty and of uncertainty about a variety of topics. This can cause inconsistencies because opposing discourses are used either by different people about the same topic, or by the same person in different contexts. These inconsistencies can be resolved by using "reconciliation devices" (Gilbert and Mulkay, 1984: 111), which I explore in the next sections.

5.4.2 Mobilising pasts and futures

Like Brown and Michael (2003), I want to look at how my informants talk about the future of SCR within the context of past accounts of other biotechnologies. Brown and Michael distinguish two ways in which futures are used: firstly, by *retrospecting prospects*, or "recollection of past futures", where informants talk about expectations that were once held about other technologies; secondly, by *prospecting retrospects*, where informants use past futures in the present to construct futures.

Some researchers describe mistaken previous high hopes in one field and subsequently display high hopes for SCR (their own area, or SCR in general):

> Peter: There's still this big gap in the lack of enough donors [for organ transplantation], lack of the right kind of donor, the tissue type matching is not right. So for me, we've had adult stem cells for 20 years, and they haven't, there's still a need to find an alternative. So I think embryonic stem cells still have a potential to fill that gap.

In this quote, Peter is confident about the use of ES cells for therapies, although he notes that the certainty about the future of organ transplantation did not fulfil its promises. He is making an assumption that the problems faced by organ transplants in the past – such as a lack of donors or immune rejection – will not be faced by ES cell therapies in the present. Why ES cell therapies and transplantations have a different fate will just be resolved in the future

In contrast to Peter, the following researcher uses the common retrospect of the previously imagined promising futures of gene therapy[74] which went unfulfilled, to argue that the future of ESCR may not be as promising as some people make out:

> Bernard: Look it's like gene therapy. I don't know if you've ever looked at that, but you should look at that as a very similar profile, that is gene therapy now goes back, but the hype was exactly the same, enormous hype, and then it was discovered of course, gene therapy was an oxymoron because you could never keep gene expression on, never any evidence it was actually doing anything, so it's back to ground zero.

And shortly after:

> Bernard: So I think the similarities to genetic engineering are amazing, amazing. Same, big, huge promise to cure all, which you know, you should know, a priori that's not going to be right […]. So you know it's going to be wrong, you have to work out what part might be right, […]. It may just die a little death.

Bernard uses the retrospect of gene therapy and its temporal progression from promise to disappointment to prospect that ESCR has an uncertain future. Bernard works in ASCR and made a strong case against ESCR during our interview.

Similarly, Heidi uses mistaken previous high hopes to explain her present uncertainty. She starts by reflecting on the previous promises associated with cancer:

> Heidi: When I left Uni, I wanted to work curing cancer, […] I mean there was a big emphasis then that we could cure cancer […]. And we all know that, what are the cures, how is cancer treated? You cut it out, breast cancers, you cut out your breast, and you have severe radio-therapy and that's 20 years on from that promise of curing cancer.

She is retrospecting the prospect of curing cancer and dismissing it as hype. She then prospects this retrospect and envisages a future for ESCR, with similar problems to those encountered by cancer treatments. She contrasts her views with that of colleagues who are making the same mistakes and not learning from the past:

> Heidi: [...] but I think here there's an incredible amount of hype, I mean listening to scientists get up and say 'we can cure diseases in 5 years', maybe, I mean I'm

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[74] This is also used to talk about the prospects of xeno-transplantation (see Brown and Michael, 2003).

> not saying they can't, I have no idea what they're doing, maybe they can, and I know there are clinical trials going on, but I think one has to be very careful, don't you think?

Although she expresses excitement about the future of ESCR, she raises potential difficulties (Heidi was quoted discussing the difficulties of working with beta cells in 5.2.2. above,). She uses the past to prevent herself from hyping again in the present about the future of SCR. She resolves the contradiction between her support and investment in this work (performing certainty) and her discourse of uncertainty, as well as the contradiction between her uncertainty and colleagues' certainty, by appealing to past biotechnologies where the promises were over hyped. She can insulate herself from any difficulties, should stem cell therapies not see the light. Therefore, uncertainty can become a strategic resource. This can happen in particular in policy discussions (see Campbell, 1985; Shackley and Wynne, 1996).

Heidi also uses uncertainty discourses to promote research in the following quote. She discusses the promises that could come from SCR and, although she places the resolution of uncertainties into the future, she uses these uncertainties to highlight the broad nature of SCR and thus the need to explore many different avenues of research:

> Heidi: Whether that's all the disorders that people are throwing out there, or whether it's a few, it could very well be that through the understanding of regeneration, that there are some disorders you don't need to put a cell back, or maybe you can understand how to trigger the adult stem cells, or regenerative cells to actually do the job. And it's all part of this work.

This is what Gilbert and Mulkay call a TWOD (1984: 90-111) where the resolution of contradictory interpretations exist in the present is enabled by a projection into the future, when the “truth” unadulterated by contingent factors, will inevitably be uncovered. This scientist also uses the fluidity between research tool and therapy (also introduced in chapter 4) to which I return later in this chapter.

The following senior researcher in Australia also appeals to time in order to reconcile contradictions:

> Albert: I'd say the embryonic stem cell […] researchers are currently very defensive, and very pro-active and aggressive and getting money, but defensive about the ethical side of what they're doing. And adult stem cell researchers are less well funded and having to kind of be defensive about what is the power, the potency of adult stem cells because the kind of zeitgeist is that they don't have the same […] potency of developing a different cell type. So you know, maybe there, I'd say there's definitely polarisation out there. But I didn't sense it particularly in

> the Australian scientific community, except people defending what they were doing.

For him, there is an inconsistency between his certainty discourse around the promises of ASCR, and others' discourses of uncertainty for this research. By using the term "zeitgeist" (spirit of the time), he implies that the lack of recognition of ASCR is due to the current fashion and that, eventually, truth will prevail and ASCR will be recognised. So here, I find another use of the TWOD. For this researcher, people who do not realise the potential of ASCR are mistaken. Albert constructs the error in these people's views as due to "contingent" factors, such as current fashions or the fact that they are simply defending their own work, which may be in competition with ASCR. This is what Gilbert and Mulkay call the "asymmetrical accounting of error" (1984: 63-89) where contingent factors explain why people have mistaken beliefs, whereas empiricists accounts explain "true" beliefs.

My informants also appeal (implicitly) to the concept of "cultural lag" (Ogburn, 1950, especially 200-13) where people opposing an area of research will eventually come to accept it in time. This enables them to reduce uncertainty in SCR and suggest trajectories where their work will become more and more acceptable, without questioning it, or the reasons for the uncertainties they are thus reducing. This assumes a particular trajectory for innovation where it is acceptable to hype in order to get over initial public rejection. I now turn to this.

5.4.3 Uncertainty and the dynamics of expectations

I have suggested that past futures can be used in the present to construct imagined futures, drawing on various discourses of (un)certainty. Brown (2003) highlights the importance of studying the situatedness of these future oriented discourses. He argues that expectations are "embedded spatially" (2005:5); they vary according to the social location of people voicing these expectations. I have illustrated this in the above sections. He also argues that expectations are embedded in "temporal conditions" and "Innovation concepts in biotechnology [...] will vary according to whether they are presented as new or old" (2003: 10, emphasis added). Brown notes two central characteristics to the "dynamics of expectations": firstly, when a biotechnology is seen/constructed as emerging, particular images and visions of the future are described; secondly, as some of the initial promises, which are often exaggerated, fail to materialise, this leads to disillusionment and disappointment. I want to explore this, in my view problematic, discourse of hype to disillusionment, or what Brown and Michael call the "early promise and subsequent disappointment" narrative structure (2003: 10). Building on the sociology of expectations, I examine how (un)certainty discourses change temporarily. Temporality includes the *construction* of areas of research as emerging or established.

The rhetoric of Lord Winston displayed a striking change over time. In 2000, before the UK legislation allowing the creation of embryos for research was put in place, the discourse he used was very certain about the future promises of ESCR for therapies.

> 1. Speculation is growing about the official response to the chief medical officer's forthcoming report on research using cells from embryos. A wise government will understand that although such work might upset a few members of the public, *many lives might almost certainly be saved.*
>
> 2. In short, an injured or sick patient could have one of his [sic] own nuclei transferred into a spare embryonic cell from which the nucleus had been removed.
>
> 3. There are a vast number of possible applications using stem cell treatments. Given further research, they could help repair various damaged organs. Heart attack victims *should* benefit from them; stem cell injection into the heart muscle is an exciting prospect. Stem cells *are likely* to be valuable in treating strokes, or possibly to repair the spinal cord in paraplegics. Thousands of people suffer from diabetes – stem cells could probably be used to regrow their insulin-producing pancreatic cells. And *horrendous* burns or severe arthritis might be treated by replacement cells generated in the laboratory.
>
> 4. People find it ironic that we led the world in reproductive research because of this liberal and humane legislation, but now there are chances to actually *save lives*, other countries are overtaking us. We need to recognise that it would be *unethical* not to use this new information for the *alleviation of fatal diseases*. (Winston, 2000, emphases and numbers added)

In these extracts, Lord Winston evinces certainty for the use of SCR (especially ESCR). In the first quote, he talks of lives being saved; in the second one, he uses very certain terms to describe the use of SCNT. In the third quote, he makes a list of many possible applications of stem cell "treatments", again using a language of certainty. He also enrols patients and uses very emotive expressions to "construct a demand" as was done during the stem cell parliamentary debates in the UK (Parry, 2003a). In the final quote, he talks about saving lives and even suggests that it would be "unethical" to not do this research, which some people may themselves consider unethical, as he briefly mentions in the first quote (although he does suggest that these are few in number). He also brings in the notion of international competition, adding another dimension to the need to invest in the future of these therapies.

This sort of discourse used to secure the future of a technology is similar to what MacKenzie points out when he talks about the heterogeneous engineering role of Charles Stark Draper.

In 2005, Lord Winston says the following in his presidential address to the British Association Festival of Science:

> More recently, both in Britain and America, huge publicity has been given to stem cells, particularly embryonic stem cells, and the potential they offer. Of course, the study of stem cells is one of the most exciting areas in biology but I think it is unlikely that embryonic stem cells are likely to be useful in healthcare for a long time. There are many basic problems – their low cell cycle time leading to slow replication in culture and the fact there may be selective pressure for the faster growing, but possibly abnormal cells, to dominate a culture system; the instability of embryonic cells in general and their remarkable propensity to produce abnormal numbers of chromosomes; the difficulty in weeding out all rogue cells that might proliferate; the risk that stem cells after forced differentiation in culture may undergo de-differentiation, or abnormalities of gene expression, after transfer to the patient with potential for huge harm. All these difficulties, and many others, may be overcome in time. But during the political campaign to encourage the UK Parliament to accept liberal legislation, *some parliamentarians were clearly led to believe that a major clinical application was just around the corner*. Of course, much of the lobbying came from patient interest groups, such as the excellent Parkinson's Disease Society, but it was stimulated by the scientist's observations. (Winston, 2005, emphases added)

This lengthy quote shows the list of difficulties that Winston argues could prevent stem cells from being useful in clinics. This unreflexive change of rhetoric between the two sets of quotes is very noteworthy: none of my informants were very critical and very supportive of any one area of research (though they may be very critical of one and very supportive of another). Both sets of quotes are drawn from talks to similar public audiences. Thus, the best explanation for this contrast in discourses relates to a temporal change. I would argue that the certainty and "rhetoric of hope" (highlighting the "alleviation of fatal diseases") in the first quotes was seen as necessary by Winston in a time of struggle, when an emerging technology, ESCR, needed to be legalised. In the second quote, the uncertain future of ESCR and its attending "rhetoric of fear" (highlighting "rogue cells" and "huge harm") become necessary, since some promises of ESCR have failed to materialise. So, once research has become established (or at least legalised), and as initial expectations are shown to have been over-optimistic, scientists can start acknowledging the original promises as hype (even if blaming others for it). In this way, the authority of science as a good source of knowledge and predictions about the natural world is safe-guarded. Scientists can then go on to make new promises about other areas of research which are emerging, knowing that the future of ESCR (here) is not under threat.

I only interviewed my informants once, and therefore did not access how their discourses change over time. However, my Australian and UK informants were, to an extent, in different temporal locations with regards to the dynamic of expectation. Indeed, in the UK, SCNT was legalised in 2000; whereas in Australia, my interviews occur in the lead up to the legislative reviews (which caused SCNT to be legalised). Thus, SCNT was, at the time of my interviews, more established in the UK than Australia. I now explore how temporal situations can shape scientists discourses around SCNT.

In the context of my interviews, scientists from the UK tend to accept SCNT as a given, they do not usually voice particularly strong feelings about it and just took it as being part and parcel of SCR. They do, however, suggest some limitations:

> Tim: So, yeah, it's an example of balance isn't it? It seems to me therapeutic cloning, what they call therapeutic cloning is one of several approaches that may lead to significant advances, whether it's *the* best or not, is not something worry about, but if as an approach it seems to offer some reasonable chance of advance, alongside other approaches, and if the risks associated with it are not very significant, then I think it's very reasonable. (informant emphasis)

Scientists working in Australia do not automatically accept SCNT as part of SCR and talk about it more *actively*: some think it should remain banned and others are waiting for a different political climate in which to promote its legalisation. In the following quote, a researcher in Australia explains why he thinks UK scientists were so keen to legalise SCNT:

> Andreas: I think it was superb marketing opportunism, because the UK was well behind in stem cells, in human stem cells, none of the line isolations came from there and I think it was a wonderful piece of marketing and taking an opportunity which the rest of the world have not taken up.

He is using the contingent repertoire to account for the differences between his view on SCNT and that held in UK legislation. For Andreas, SCNT is not an integral part of SCR.

What is particularly interesting, however, is that, although during my interviews Australia was at a similar stage to the UK in 2000, when Winston made the first set of claims discussed above, I do not come across any assertions that were very certain about the future of SCNT for therapies. I do not come across a single statement similar to Winston's first set of quotes above. Although several informants include the use of SCNT in therapies as part of SCR, it is often discussed in the context of potential limitations, or by highlighting its use as a research tool that may lead to therapies. I can see two main possible reasons for this. One is that my interviews are not parliamentary debates or any other contexts that my informants might see as potentially leading to a change in scientific autonomy. Another is that, at the time of my interviews, groups around the world had been trying to develop cell lines from nuclear transfer embryos, but without success. Thus, the belief in the potential of SCNT for therapies might have diminished (in public and scientific communities), making hype around SCNT a less useful and less appropriate rhetoric strategy. The latter explanation is supported by the fact that many submissions to the committee reviewing the legislation on SCNT in Australia, in 2005, made the case for the legalisation of this technique by focussing on its potential as a research tool, not a therapy. Thus, even though SCNT was not legal and some of my informants wanted it legalised, the potential of SCNT as a therapy was not hyped by my informants in the same way Winston did. This indicates that local conditions (here, of SCNT as illegal) are inevitably embedded in global contexts (of SCNT) (cf. Giddens, 1990; Beck, 1992): even though SCNT is illegal in Australia, research is taking place elsewhere, suggesting limitations to its application as a therapy, and shaping the discourses about SCNT in Australia.

This section suggests that the stem cell researchers I interviewed in Australia, unlike Winston at a similar legislative juncture, did not feel the need to hype the promise of SCNT in these private conversations.

5.4.4 Mobilising the dynamics of expectation

Another possible reason for the lack of hype about SCNT in my Australian informants' discourses could be their awareness of the potential problems of hype for the future of science:

> Arthur: I think that there is, at least among some members of the public, there is this feeling that scientists tend to over-play and be maybe over-optimistic and say they're going to cure lots of things and then nothing happens. I think it's not a good thing, if they are going to engage with the public, I think it's good to tell them, give them accurate information, rather than making it sound like you're really at the leading edge and that you're going to ... provide cures. I think a little reflection tells us that these things can be very complicated and going to take a lot of time.

Arthur seems to think that hype, even when a technology is emerging, can lead to public disillusionment. He seems more aware than Brown (2003) suggests for his informants of the cost of hype. He wants to avoid disillusionment even if hyping could facilitate the enactment of particular desired futures. He draws on the promise to disappointment narrative structure to explain why he does not hype.

However, this is not to say that most of my informants do not see the need for hype. In some instances, they are reflexive about the promise to disappointment narrative structure in order to resolve contradictions between certainty and uncertainty discourses: they suggest it is acceptable and/or normal to hype when a field is emerging. In the following quote, a researcher is using the retrospect of gene therapy and its high hopes followed by disappointment, and comparing it to SCR:

> John: I mean I think gene therapy is ongoing you know and the 1st trials, well, there were one or 2 disasters, where people jumped the gun before it was safe, but nevertheless gene therapy will happen, […]so it's not like it's died, it's like it's got on a rather more sensible curve. Stem cell biology is similar, in that it's been tremendously puffed up, way beyond what it can deliver, and so there aren't stem cell therapies, except in one or 2 rather specific instances like blood, and there won't be stem cell therapies for quite a while that actually mean anything in most fields. Because an awful lot of real science has to get done first.

The "comparatibility" of a newer technology and an older one can lie in "their common social dynamic, such as the need to hype innovation in order to curry investment" (Brown and Michael, 2003: 12). This type of discourse normalises narratives of hype when there is competition for resources, followed by disillusionment, rather than questioning if it is appropriate to hype in the first place, particularly if the expectations are that the promises will not be fulfilled.

The mechanisms for research funding can also be drawn on my scientists to justify their need to express high levels of certainty in times of struggle:

> David: I think it's a bit of a conflict because to get in the funding, you've got to sell things […] that can be a double-edged sword because you try to say you should put money into this because we can develop these sorts of therapies and this will help lots of people […]; there is a bit of a conflict there between selling stuff and then creating false expectations.

This researcher reflects on the role of the grant system in shaping discourses around science. He reconciles his private uncertainty discourse with the discourse of certainty that he uses when applying for funding by appealing to the nature of the system: one must make grand promises in order to get a chance to work at realising those promises. In these circumstances, David must perform an identity of certainty in order to enable a future of funding to happen.

All these strategic and temporally dynamic discourses complicate the use of the certainty trough. In conjunction, the enactment of different identities in different contexts also adds complexity to the analysis of (un)certainty discourses, as I now discuss.

5.5 Protecting the whole: identity salience and location C'

Chapter 2 (see 2.4.3) shows that people may draw on particular identities in particular circumstances: by choosing to enact an identity that is "salient" (Stets and Burke, 2000) to the situation, they can enable particular outcomes to occur, such as the enrolment of patients. I draw on social identity theory to make sense of the mobilisation of these different identities.

As I suggest in chapter 4, scientists can reinforce the authority of a variety of research areas beyond their narrow professional interests. This is particularly noticeable when researchers committed to a particular technology do *not* voice their uncertainties concerning competing technologies – this would correspond to location **C'** on the certainty trough graph. This again highlights the importance of context and the performativity of discourse. In the following example, Pierce, whose work competes for funding with ESCR and who dismisses ESCR as hype explains why he does not voice his uncertainties in public (both quotes are from the first ten minutes of the interview):

> Pierce: But what has worried me a little, [...] is that on the other side, the proponents of embryonic stem cells have almost made the raison-d'être for doing embryonic stem cells, the idea that they had this staunch right wing Christian movement against them.
>
> Pierce: I mean it's almost to the degree of saying well, as a scientist, you had to be on the embryonic stem cell side, because it was almost like, well, this is the Visigoths at the door, that you know, were going to take away all the scientific principles.

Here, Pierce is arguing that it can be difficult in public to highlight uncertainties about one particular area of research because many colleagues may find this threatens science as a whole. Thus, in public contexts, Pierce's identity as a scientist, rather than specifically as a researcher working in a field competing with ESCR, is more salient. He reconciles the contradiction between his private uncertainty and his public certainty by pointing out the differences in audiences and the importance of defending the authority of science as a whole. He is reflexive about the boundary-work that he is conducting in order to protect the autonomy of science from publics (here dominated by the right wing Christians), which are considered anti-science.

Similarly, Victor has designed his whole research strategy in order to avoid SCNT on the grounds that, aside from being illegal in Australia, he can see many limitations with regards to its clinical potential. However, when I ask him if he would voice these uncertainties, raised in our interview, in public, in the context of the lobbying for the legalisation of SCNT, he replies:

> Victor: I will still support [SCNT] in principle, you know, as a research tool if nothing else. Because it still has a benefit, if […] you can take a cell and you can cure a patient with it, even if it's in a controlled situation, at least it's telling you something about the science. And then, if you can use that system to work out which gene is being switched on or switched off, in your somatic cell, that again is going to tell you something.

This researcher is faced with two sets of contradictions. On the one hand, he is uncertain about SCNT and expresses this in a strong way by premising his research on the unworkability of this technology, yet he does not voice these uncertainties in public. On the other hand, his uncertainty discourse contrasts with the certainty evinced by other fellow researchers. He reconciles all these apparent contradictions by strategically appealing to research tools and to therapies: he describes himself as a practical man (data not shown), explaining why he prefers working in areas that have good therapeutic potential; nevertheless he also portrays himself as a scientist who supports research in general, even if it has no immediate applications. In this way, he can reconcile his identities as a scientist and as a stem cell researcher avoiding SCNT, both of which are salient in the context of this interview. In addition, by talking in terms of research tool, this scientist creates a situation where SCR – and research in general – cannot fail as it will produce knowledge; itself synonym, for him, with "progress"[75].

Similarly, the following young researcher, although he is more supportive of applied research, feels that there is a pressure to also display support towards basic research:

> Gary: I personally think that all scientists, um I guess not all... I'm a big fan of doing scientific research with a direct purpose in mind, I probably shouldn't say [N: You can take it out if you don't want] It's just my personal opinion, but I don't really, a lot of the research that you see, we worked in this protein and it does this. A lot of the time I'll sit there and, you know, great, but how's that

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[75] Brown and Michael discuss Van Lente's idea of "Western progress" as something which is "culturally cherished as other idiographic formations like justice, freedom and democracy" (2003: 6).

> relevant to anything that could possibly be of any use? [laugh] [N: huhuh, OK] So, it probably is fantastic, in most cases, it is fantastic work, um, I personally um would always be doing something that I think would have a direct impact on public health.

This informant is reflexive about the need to enact a particular identity: he feels there is a normative force, exercised by the scientific community, towards supporting basic research. Thus, he does not talk as if he belongs to the same "in-group" (Hogg *et al.*, 1995) as these other scientists and he argues that he must enact a particular identity in order satisfy this more powerful group (where power is the ability to recruit other people to attain one's goals).

5.6 Conclusion: identities, certainty and public engagement

These stem cell researchers raise a variety of promises and uncertainties concerning SCR. MacKenzie's (1990) certainty trough is a very useful heuristic tool to explore these accounts, especially in early stages of analysis. Some of the discourses could easily be located within the three types of (un)certainty MacKenzie identified. Nevertheless, there are many dimensions missing from this model. Firstly, the notion of "distance" needs to be problematised, not least because there are multiple sites of knowledge production in SCR. Secondly, the performativity of language is much more central than MacKenzie suggests in his discussion of the certainty trough[76], and this aspect is difficult to account for using his model. I would argue that the strategic use of language is not only relevant to "emerging" technologies, but for any area that is competing with others for resources (cf. Gieryn, 1995). Thirdly, my informants draw on many different discourses and identities in different contexts and this is not reflected in the certainty trough.

I identify a variety of factors that shape stem cell researchers' discourses about the future of SCR. These include their experience in the field, their area of research, the status of scientific findings, current legislation, superiors' opinions and the perceived audience for their accounts. There are no true or false imagined futures, but analysing these futures enables us to think about how discourses are shaped. This analysis confirms the value of international comparisons. As the comparison of the UK and Australia shows, the shape of

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[76] Mackenzie is, however, fully aware of the performativity of language (e.g. MacKenzie, 2003; MacKenzie and Millo, 2003)

discussions around SCR is clearly different in different countries. For all these reasons, it is important to locate these promissory discourses within their broader contexts.

Brown and Michael argue that "there are few limiting constraints on the hyperbolic expectations seeded by entrepreneurial innovators" (2003: 13) and they suggest that entrepreneurs can relatively easily move towards other innovations, this is perhaps what Winston does. However, as Brown (2003) and some of my informants point out , there is a cost to hype. In particular, repeated high hope followed by disillusionment leads to mistrust, not only from publics, but also from investors. Collins and Pinch show that by pushing an image of certainty, science suggests it can provide all the answers. When this proves to be impossible, people may become wary, and seem to be constantly changing their mind about science:

> Instable equilibrium – flip-flop thinking – is the inevitable consequence of a model of science and technology which is supposed to deliver complete certainty (Collins and Pinch, 1993: 142).

Therefore, I suggest that scientists must move away from this standard narrative of "optimism to obstruction" (Brown and Michael, 2003: 12). Uncertainties need to be brought out into broader public spheres, but not only once the future of a technology seems secure.

To help towards this, social scientists have a role in showing the situatedness of these (un)certainty discourses to scientists. By showing the arbitrariness of particular norms (see 2.4) such as "certainty and hype improve support for science", we can help scientists, and others, challenge the dominant discourses heard in public fora. The next chapter discusses where these challenges could come from, by examining some of the concerns my informants raise and the ambivalence they display towards SCR.

Chapter 6

Cowboys and mavericks:

Professional trust and ambivalence

> ***Cowboy**: 1) A hired man, especially in the western United States, who tends cattle and performs many of his duties on horseback*
>
> *2) An adventurous hero.*
>
> *3) Slang. A reckless person, such as a driver, pilot, or manager, who ignores potential risk*
>
> *(www.answers.com/topic/cowboy).*

> ***Mavericks:** propose innovations the ... World refuses to accept as within the limits of what it ordinarily produces [...]. Not surprisingly, mavericks get a hostile reception when they represent their innovations to other ... World members. Because it violates some of the [...] World's conventions in a blatant way, [...].*
>
> *(Clarke, 2000: 40)*

6.1 Introduction

Early in my data analysis, I noted that stem cell researchers are critical of practices and contexts of SCR which enable some people to act like, as some informants put it, "cowboys" or "mavericks". Upon examining these accounts in more detail, I find that not all informants are critical of the same practices, and that these criticisms are bound up in complex relations of trust and dependence between scientists, their colleagues, institutions of science and broader national and international legislation. Thus, there seem to be many expressions of "ambivalence" which need to be examined in more detail. In this chapter, I critically draw on theories of "reflexive" modernity (Giddens, 1990; Beck, 1992, see discussion in chapter 2) and studies of trust and ambivalence (especially Wynne, 1996b; 1996c; Kerr and Cunningham-Burley, 2000) to explore the reflexive potential of professionals' discourses of trust and ambivalence.

As discussed in the literature review, trust, mistrust and reflexivity are tightly linked in today's society. Trust is central to Giddens' work. For him, in late modernity, we are no longer able to judge risk but instead need to rely on "expert systems" (see 2.3.2) which he defines as:

> systems of technical accomplishment or professional expertise that organise large areas of the material and social environments in which we live today (Giddens, 1990: 27).

For these to serve their function and create ontological safety, individuals need to trust in them. However, both Beck and Giddens argue that the visibility of public disputes has increased public mistrust of the institutions of modernity, such as science. For Beck, this leads to an intensified critique of science, illustrated by the actions of "sub-politics", such as environmental movements (e.g. Beck, 1996), which, in turn, encourages science to confront its consequences and become "reflexive". For Giddens, institutions automatically become reflexive through their increased awareness of their role in risk creation.

Wynne diverges from Beck and Giddens' "rational-calculative" version of trust, and suggests a more hermeneutic and relational notion where, for example, publics have to act "as-if" they trust the institutions on which they depend (1996b, see also 2.3.2). Wynne is quite pessimistic with regards to scientists' reflexivity. He argues that, although they may think about some of the limitations and contingencies of their work, this is due to external criticisms and "a related sense of insecurity, rather than by any intrinsic qualities of self-criticism" (Wynne, 1996c: 43). In later work, he also finds no evidence of reflexivity, in the sense of questioning one's assumptions, in the institutions of science (e.g. Wynne, 2005b: 79).

Beck and Wynne both see a central role for "sub-politics" in questioning the framing of science, in contrast to Giddens' more automatic reflexivity. However, Beck is much more optimistic than Wynne regarding the consequences of this questioning. For Beck, the resulting reflexivity of institutional science can enable a shift from early modernity and "primary scientization" (and its attending faith in progress) to "reflexive scientization", where the dogmatism of science is radically examined and challenged. This can help society avoid “counter-modernity” (Beck, 1992), which he sees as a return to Enlightenment commitments.

The work of Kerr *et al.* examines in detail the ambivalence and reflexive potential of both lay and professional discourses in relation to the new human genetics. One key finding is that lay people’s accounts are very sophisticated and reveal a number of ambiguities and uncertainties regarding research in this area and its application (1998a; 1998b). If these were further explored, they could enhance public engagement and critique genetics practices. However, Kerr and Cunningham-Burley argue that the power of these accounts to question dominant framings has not been used to its full potential:

> Lay ambivalence about the new human genetics is clearly apparent in the clinic and beyond; and it is unlikely to diminish as research and services expand. However, we suggest that it is currently more sedative than revolutionary, remaining largely privatised and inert. (Kerr and Cunningham-Burley, 2000: 294)

Another key finding is that professionals’ accounts of their work rarely problematise their practices and, instead, shore up their authority (Kerr *et al.*, 1997; 1998c; Kerr and Cunningham-Burley, 2000). In this sense, it seems that science rarely meets the conditions of “reflexive modernisation”: even when professionals do reflect upon problematic issues relating to their work (for example the use of genetic information by insurance companies), they do so in such a way that it does not question the assumptions entrenched in their practices. Rather, it shifts the blame towards others, or reinforces their authority in particular areas, such as education. In addition, sub-politics, here coming out of lay ambivalences, seem unable to challenge science. This work has been built on by other sociologists who find that scientists express complex feelings, including ambivalence, towards their work (e.g. Wainwright *et al.*, 2006a; Ehrich *et al.*, 2007).

As argued above (see 2.4), a central aim of this thesis is to explore the potential for greater reflexivity by scientists as a way of improving science-public relations. This chapter contrasts my findings to Wynne’s pessimism and builds on some of the avenues for reflexivity suggested by Kerr and Cunningham-Burley. For me, reflexivity consists in scientists acknowledging and taking responsibility for the “unintended consequences” of science (Giddens, 1990; Beck, 1992; see also Beck, 2000), but even more so in an “interpretative reflexivity”. The latter involves acknowledging the contingencies, situatedness and embedded assumptions of science (e.g. Wynne, 2005a). An exploration of scientists’ trust and ambivalence is a vital step towards potential reflexivity. In particular, expressions of mistrust or ambivalence can reveal concerns and, therefore, critiques of science. They can also reveal instances where scientists become aware of “pre-analytic assumptions that frame knowledge commitments” (Wynne, 1993: 324) which they disagree with and may want to challenge.

Because the distinction between lay people and experts is problematised in this research (see 2.2), stem researchers can be examined as individuals who, like publics, must rely upon, and trust in, various systems of expertise and knowledge production. As Giddens states:

> But the lay person – and *all* of us are lay persons in respect of the vast majority of expert systems – must ride the juggernaut [of science]. The lack of control which many of us feel about some of the circumstances of our lives is real. It is against this backdrop that we should understand patterns of privatism and engagement (Giddens, 1990: 146, original emphasis)

So this chapter examines stem cell scientists’ discourses of trust in, and ambivalence towards, expert systems. From this, it reveals whether these professional discourses, unlike the professional and lay discourses discussed above, have a potential for scientific institutional reflexivity and “revolutionary” change; or whether instead, they remain largely private and inert (cf. Kerr and Cunningham-Burley, 2000).

The ambivalence in my informants’ accounts echoes the dual meanings of “cowboy” and “maverick” suggested in the opening epigraphs. When integrated into a “rhetoric of hope” (Mulkay, 1993), “cowboy” and “maverick” can have a positive connotation; that of an adventurer or trailblazer who, against the odds, prevails and makes great discoveries: the “adventurous hero” from the epigraph. When integrated into a “rhetoric of fear”, they can have a negative connotation; that of someone wild, thoughtless and out of control, who can be dangerous to the rest of the community: the “reckless person”. Although my informants use these two particular labels in ways that only imply the negative connotation, their stories also draw upon “narratives” (Shapin, 1995) implying the trailblazing aspect of science. Thus, by exploring how different professionals talk about what conditions enable “cowboys” or “mavericks” to take over, I investigate expressions and relations of trust, mistrust and dependence and draw out opportunities for reflexivity.

I examine four areas related to SCR towards which my informants displayed mixtures of trust, ambivalence and/or dependence. These are: going to clinical trial, informed consent, the commercialisation of research, and the internationalisation of research[77]. Analytically, I examine ambivalence by distinguishing between cowboys seen as “adventurous heroes” or trailblazers, and cowboys seen as “reckless” and dangerous. I draw on Beck and Giddens’ rational calculative bestowals of trust (taking into account Wynne’s critique of it), Wynne’s (1996b) concept of “as-if trust” and Szerszynski’s (1999) notion of performative trust. In the conclusion, I draw out aspects of my informants’ accounts that reflect elements of “reflexive modernization”, but also of “counter modernity”, which might limit the potential for this reflexivity.

6.2 Going to clinical trial

As demonstrated in earlier chapters, therapeutic applications are often portrayed as central aims for SCR. A central part of the process of developing these therapeutic applications is the conduct of successful clinical trials. This can be considered an “expert system” as clinical trials organise different “experts” and areas of knowledge production such as local, national or international regulators, clinical staff, patients, healthy volunteers, informed consent processes, and ethics review boards or results from animal studies. Stem cell researchers need to enter these systems if they want their research to be taken to the clinic, and they need to rely on the knowledge and expertise of these others. If clinical trials go well, they can promote scientific research. However, if clinical trials go wrong (and particularly if patients suffer, as was recently the case in trials in London, see BBC News, 2006), they become visible examples of the failure of science to deal with the risks that it creates.

It is unsurprising that clinical trials often feature during my interviews. My informants use the terms “cowboys” and “mavericks” to describe potentially dangerous people going to clinical trial too early and putting the reputation of the whole field at risk. Alongside this discourse is another describing researchers who take risks but bring treatments and therapies closer. In my analysis, I categorise the latter as being labelled “trailblazing” types, whilst the former are “reckless” types.

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[77] The title of this chapter may have suggested that I will be focussing on scientists interested in reproductive cloning. However, there was little discussion about these during interviews. Nevertheless, I do examine aspects of reproductive cloning in chapter 7.

One of my informants, a group leader in Australia, talks at length about clinical trials. In the following four quotes, he criticises some researchers whom he considers to be going to trial far too early.

> 1. Stanley: You see situations these days which really disturb me in the adult field about people trying to stick bone marrow in various tissues and I find that absolutely horrific, because [...], there's not even a scientific basis in an animal model really to underpin those studies, [...]; I strongly suspect it's a lot of very ambitious clinicians who have power over their institutional ethics committee, bludgeon their way into these studies. I think that's terrible because all of us I'm sure would agree that one of the worst things we could do is to go prematurely into clinical studies.
>
> 2. Stanley: [...] And the reason that I mention the adult cell work is because that really does concern me that whoever it is that's bludgeoning those therapies through these committees is succeeding based on what I think is a bunch of snake oil to be quite honest. If the same sorts of individuals are going to be involved in ES therapies, we could unwittingly see ourselves in the midst of a real problem. Because if a few cowboys get hold of ES cells and we have worse case scenario, yes tumours formed in the recipients of those ES derived progeny, my God that will set the ES field back, well, it could kill it. So I'm very worried about that.
>
> 3. Stanley: There would be a sort of ripple effect that would go all the way back to researchers that are trying to do these studies in a rigorous, scientific and solid clinical based manner, going through all the right hoops and everything.
>
> 4. Stanley: It almost argues for another level of regulation for these kinds of therapies. See the problem is, these kinds of cell therapies are not, there's no official mechanism for regulating them.

The first two quotes happen in quick succession in the second half of the interview, and the last two follow several minutes later, after we discussed another topic. Stanley is criticising particular individuals involved in ASCR (the area he works in) who want to start clinical trials. He is doing an "asymmetric accounting of error" (Gilbert and Mulkay, 1984) where these researchers are managing to go to trial too early due to contingent reasons: they hold too much power over their ethics committees and they do not have proper "scientific" data to support their case and they "bludgeon" their way through based on "snake oil". They are thus acting *recklessly*. The danger lies in the downstream effects that could impact upon *good* scientists, who are more "rigorous" and "scientific". Stanley is conducting boundary-work to "expel" (cf. Gieryn) these individuals from the *bona fide* community of scientists. Here, it seems that "good" science includes good evidence before proceeding to trial, such as animal models. Stanley sees the difficulty in predicting and controlling what happens in clinical trials. Drawing on his experience of sitting on regulatory committees (data not shown), Stanley suggests introducing further regulations to deal with the potential problems of inadequate trials.

By contrast, earlier in the interview, Stanley gives an account of science which highlights the *trailblazing* characteristics of researchers and downplays the importance of having all the evidence, and argues for less regulation of science:

> Stanley: Having sort of grown up in the adult stem cell field and done some of my training in institutes where bone marrow transplantation for example started, there was an awful lot of empiricism in the implementation of those, of what has now become a very standard from of therapy.
>
> Nicola: Sorry, what do you mean by empiricism?
>
> Stanley: It's really, empiricism meaning you're asking very simplistic questions and you're going into a situation unaware of a lot of seemingly very important questions. But that ignorance doesn't prevent you from going forward. Um, that said I mean nowadays, people are regulated up the wazoo, much more difficult environment in which to work, and they, regulatory environments in particular are much more stringent and… I see that actually as a problem in truth. [...] as I say, I do believe in empiricism, as the way forward, you've got, if we,... if [particular research group] had taken that view, [...] or if that view had been imposed upon them shall we say, we wouldn't be doing bone marrow transplantation today. So I think you know, it's got to be a real balancing act to make sure that, to me, there's appropriate… grounds to allow exploration of potentially interesting fields with as much safety data as one can reasonably expect, but not more. Because otherwise you're imposing you know imaginary problems which are really an unfair burden to put on any clinical researcher.

Here empiricism means going forward and developing SCR or science but without knowing all the risks and unknowns. This unusual use of the term "empiricism" reminds me of Beck's use of "counter-modern":

> Here the *counter-modernity* of faith in progress becomes clear. It is a type of *secular religion of modernity*. All the features of a religious faith apply to it, such as trust in the unknown and the intangible or trust against one's own better judgement, without knowing the way or the 'how'. Faith in progress is the self-confidence of modernity in its own technology that has become creativity. The productive forces, along with those who develop and administer them, science and business, have taken the place of God and the Church. (Beck, 1992: 214)

What is particularly noteworthy is that Stanley does not reflect upon the tension between this exchange (which comes in the first half of the interview) and the quotes above: on the one hand, he argues that it is appropriate to go to clinical trials before understanding everything as this enables breakthroughs (which would be prevented with excessive, "up the wazoo", regulation); on the other hand, he argues that there are risks in going to trial early and doing this should be controlled more tightly. This points to the interpretative flexibility of *absence* of data: the absence of data can be used to encourage or discourage moving towards trials. Stanley resolves this tension by separating "us" and "disreputable others" (Michael and Birke, 1994). "Us" corresponds to good scientists who go to trial at appropriate times even though they do not know the answer to all the questions, and who have the "unfair burden" of excessive regulation; "disreputable others" corresponds to those putting the whole field in jeopardy by going to trial without enough evidence and should be regulated more tightly.

The difficulty in finding the appropriate time to go to clinical trials is explored in my focus group with young researchers in the UK:

> Xavier: [...] But I guess if you don't even really understand the basics of the biology of it, then how can we move on the therapeutics of it? I mean people see the end of it, but they don't always see the process, the basic science.
>
> Yuri: I actually disagree with that. I think that you don't actually necessarily have a full understanding of the basic research before you can look for therapies,[
>
> Zoë:]I think you have to have reasonable understanding.
>
> Yuri: I don't know, I think most of the therapies that have been discovered in the history of science have been by people who were woefully uneducated about the actual, or even just uninformed about really what was going on, they just happened to try to mix a couple of things together and it worked, I think in general, biology is so complicated that often the best way to find therapy use is to try things, and not necessarily to over-engineer them before you...[
>
> Zoë:]I don't know, I find that very difficult. If you're in a hospital, and you are dealing with patients and you see the person, I don't know, it probably depends on the character, but I wouldn't try, I wouldn't just go and say well let's try and see if the person survives or not, it's easy to say, but it's very difficult to do it, you know facing the actual patient.
>
> Yuri: I mean, that's definitely true, I think that the development of any therapy is very difficult, my point I guess is really that even if you understand things often, or even if you think you understand things, often, even when you do try them in the clinic, the results are never, almost never as you would expect them to be.

This exchange highlights a number of recurrent themes. The narrative of science as serendipitous advances is present in Yuri's talk, as well as the risk of "unintended consequences" (Giddens, 1990) and the difficulty in predicting and controlling risks from new scientific knowledge (Beck). Zoë's talk highlights the importance of "cultural knowledge" in knowledge production. This term has been used by Kerr *et al.* in the context of the new human genetics to describe knowledge:

> not specifically about genetic science or its institutions, but concerning the wider social and cultural context in which these are located (1998b: 49)

Zoe acknowledges that her interactions with patients, or "facework commitments", at one of the "access points" to the expert systems of clinical trials, would impact on her decision to go to clinical trial. These informants, unlike Stanley, explore the tension between *trailblazing* and *recklessness*; between developing great therapies and harming patients by using mis-understood drugs or processes.

Many other researchers also voice trust in the expert systems enabling clinical trials to take place. The following PhD student for example professes little knowledge of the process of getting ethical approval to go to trial, but says that he trusts the system:

> Brian: Presumably there's a lot of ethical considerations for clinical trials, and if they're at that stage, they must have pretty good evidence from animal models that it works, so, it's always justified. And the process to get to a human trial is unbelievably complex, so they must be, [inaudible] they know what they're doing.

For Brian, it seems that the difficulty of getting approval for clinical trials is good evidence that if researchers do proceed to trials, they are doing so appropriately. He places trust in the mechanisms evaluating the ethics proposal as well as the appropriateness of animal models. But he then goes on to say:

> Brian: Whenever we write ethics proposal, we always have to justify why we're doing the work. And in most cases we'll write that this is our foreseeable outcome, and our foreseeable outcome is not something we'll do in the next 2 years, which is what we write the ethics for, so you know this therapy has a potential for human whatever, and we're working in mice. And there're so many differences between mice and humans ES cells it's not funny.

In this quote, Brian points out that animals and humans are very different. However, this is the sort of data used to convince ethics review boards that it is time to go to trial. He is drawing on "technical knowledge" (Kerr *et al.*, 1998b: 45) about animal and human research, as well as on "institutional knowledge" (Kerr *et al.*, 1998b: 48) about the processes of obtaining ethical clearance. The difficulty in extrapolating from animals to humans raises problems about the process of proceeding towards clinical trial. The difficulty in inferring risks to humans from animal data was highlighted many times in my interviews. There are not always alternatives and researchers cannot always opt-out of using animal models; they just have to act "as-if" they trust them to give data relevant to humans. Thus, I think there is a strong element of "as-if" trust (Wynne, 1996b) in animal models.

This section highlights that my informants, like other lay people (cf. Irwin and Wynne, 1996; Wynne, 1996b; Kerr *et al.*, 1998b; Kerr and Cunningham-Burley, 2000) draw upon different types of inter-related knowledges to make sense of the expert systems of clinical trials. Acknowledging the difficulties of translating animal into human data and the unpredictability of the outcomes of clinical trials, as well as demanding, and participating in, further regulation, are all elements of what Beck calls “reflexive scientization”. However, Stanley’s version of “empiricism” is more akin to “counter modernity”. I explore this in the concluding part to this chapter. Informants have different views about what uncertainties can and cannot be ignored before going to clinical trials; this leads to variations in the use of labels such as cow-boy and maverick.

6.3 Informed consent

Informed consent is central to SCR. It is an expert system in that it requires special expertise (in law, counselling etc.); it is necessary for systems such as clinical trials and egg or embryo donation to take place; few stem cell researchers have any direct involvement in the process, but they have to trust that it is done correctly in order to continue with their everyday lives (even working on cell lines that have already been derived can become problematic if it is found out that these were obtained “unethically”). However, it has been argued that although informed consent is a laudable aim, it has many limitations (e.g. Pfeffer and Kent, 2006) and is not achievable in practice (Corrigan, 2003). It can, in some instances, be a process of legitimation. Here, I explore my informants’ accounts of informed consent. For some, the limitations of consent enable *reckless* research to go ahead.

Many of my informants express trust in this expert system. For example, the following is an answer from a British researcher to a question about two UK-based researchers wanting to ask healthy women to donate oocytes. I ask Ted whether the risks of this procedure are sufficiently discussed. He answers:

> Ted: I'm sure they will be. It won't be [the researchers] going out to discuss it with those women I would have thought, it will be the IVF clinics themselves, this is speculation on my part, I don't know exactly, I would have thought that it would be clinicians not involved in the research personally will be asking for consent, that would be fairly standard, I would have thought that the consent forms and so on will all have gone through an ethics review process and I think people have a right to give informed consent, provided they understand the risks, I would imagine that the risks have been set out for them in the information, certainly

> those are the standards I would expect, I don't see reason to believe [these researchers] would not be supporting those standards, and if they are, I have no problem with that.

Ted expresses confidence in the consenting procedure, and in systems, such as ethics reviews, that control it. He is voicing trust in a system he is not very familiar with. He knows one of the researchers I am talking about and this could help him *have* trust in the appropriateness of this particular act of obtaining informed consent; that is this may be an example of Giddens' or Beck's rational-calculative trust. What is also interesting is that he does not question the framing of informed consent: for him the obvious people to define or identify risks are clinicians, not women or patients.

In some instances, I specifically highlight some of the limitations of informed consent but that does not elicit confrontation with potential problems this could cause. For example, I discuss Parry's (2003b) findings that women or couples going through IVF are often stressed during the process. This results in them signing a number of forms and not necessarily realising/remembering they have consented to donate their potential embryos to SCR. One PhD student responds:

> Paul: Yeah. I mean we don't do any derivations here[78], so it's not something I'm too involved in, and in the UK, I'm that concerned, my mind is put at rest a bit; I was in [UK town], [...] and we were shown round the IVF clinic there by the head of it, a very nice guy and he took a small group of us round and told us exactly what goes on and showed us these forms and who talks to the patients and actually he came across extremely well. He was clearly very caring, very sort of sensible man and, even though that was just one IVF clinic, I thought well, if they're all as good as this, then I feel pretty reassured that in the UK it's pretty good. The private ones in the UK I don't know much about, and you know, it's probably the ones in other countries that I would be more concerned about. I mean, there was, I don't know...certain ethical issues with the Korean work, which were shown to be completely fine in the end[79], but, you're always going to get things like this and you don't know often what goes on I think in those countries.

Several things are evident in this extract. First, Paul has no direct experience of getting informed consent. However, his experience and knowledge of a particular person involved in this process makes him trust the system as a whole. The fact that this person is "very caring", for example, puts Paul's concerns at rest; thus a positive encounter at an expert system

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[78] This means that his laboratory does not derive ES cell lines from embryos, and therefore he does not feel directly concerned with the issue of informed consent; he works on human ES cell lines that have been derived elsewhere.

[79] Several months after this interview, the ethical concerns for the Korean work were in fact shown to be well-founded (see 3.4.1).

"access point" (Giddens, 1990) seems to result in trust. Nevertheless, this trust is restricted to public clinics in the UK. I will return to trust relations with other countries and with commercial and private aspects of research later in this chapter, as many scientists express unease towards these two aspects of SCR. Second, Paul does not seem to take on board the limits of informed consent that I put to him. He does not, for example, ask if the consent procedures I refer to took place in the UK and seems unwilling to question this system in the UK. Perhaps to question it would put his work into question and destabilise his identity as an ethically legitimate stem cell researcher. Indeed, if Paul puts into question the fundamental legitimacy of the informed consent system on which his work (on ES cells) relies, this would force him to confront his dependence on an untrustworthy system. This parallels a case described by Wynne (1996b) where indigenous populations depended on careless companies exploiting their land. When they had to confront their dependence on these companies (following a serious incident), this heightened their sense of powerlessness and threatened their identity and sense of worth. A serious breach of ethics in the UK may force Paul to confront his powerlessness.

Another researcher, Theo, particularly raises my concerns that scientists may not be willing to question informed consent and confront the consequences of its limits. He seems to have a problem with the consent system and knows of specific flaws relating to it; but he refuses, upon several occasions, to talk about it with me. I ask Theo if he sees any issues that could come from SCR. Rather than answering, he fields the question straight back to me. When I suggest that the unpredictable nature of research may render *informed* consent difficult, he either ignores the question, or highlights the "hell which we all go through to get informed consent". The latter suggest the use of a "discourse of suffering" to indicate authenticity (Brown and Michael, 2002; Irwin and Michael, 2003). He also diverts the question by criticising my own consent form for not fulfilling particular criteria (such as the length of time I was planning to keep data for). He black boxes certain aspects of informed consent when I ask him if he is involved in creating the consent form:

> Theo: Let's not get carried away, there are two things involved in consent forms, one is it's best to have a template, I mean otherwise you've got to become a lawyer to be able to create something properly, or someone who's got experience, you take the template and you modify it, but there's a logic, you've got to be logical, use common sense; what's a consent form for? You say precisely what you want, what you mean, which is a) I'd like your embryonic, your cell lines, your spare fertilised eggs, for the purpose of medical research; you state it you know, plain English; Number 2, the part that people agree that effectively they no longer wish to utilise them for reproductive purposes; [...] 3, they have the opportunity for appropriate consultation with relevant people over a period of

> time [...] 4, that in fact they agree that they will not gain any financial benefit out of the passing on of the spare fertilised eggs and 5th thing, there would be the question of the commercialisation [...] they agree that they will have no role associated with that. They're the key points I could see in a consent form, you just have to put them in plain simple English. [...] but no, I'm not involved in that [...].

By focussing the difficulties of consent of the use of "plain English" and the lack of remuneration for embryos donors, Theo can paint informed consent as unproblematic. By discussing these particular details of the form, he buries issues around the framing of the consent form and the process of obtaining consent (the stressful context in which consent is sought, unbalanced power relations, the assumption that "medical research" is a synonymous with progress etc., cf. Parry, 2006). This sort of discussion around informed consent serves to legitimate this kind of research, down-grade any concerns publics may have (of course it is appropriate to donate, it is for "medical research") and closes down conversations that try to question the assumptions of science and its institutions.

By contrast, some researchers do reflect upon the limits of consent. For example, a PhD student (Brian) draws on his cultural and institutional knowledge to make sense of informed consent. He suggests potential discrepancies between what people think is going to happen to their material – the use of which they have consented to – and what may actually happen to it. However, he ends this quote by again *voicing* trust in the system. This extract is an answer to my asking him if he knows how potential donors feel about donating to SCR:

> Brian: um … I'd like to think that I do; only because I know, I would say that most people that donate eggs would be ill-informed about what's actually going to happen to them. Although I'm sure it's explained to them. Um, obviously those who do donate don't have a problem with it, because they think they're contributing to science and so on. It's like people who donate their bodies to science, don't know what actually happens. If they went to any anatomy department, any university, I'm sure they wouldn't donate their body. Because I've been there, so I and um, yeah, so hopefully they are, surely you have to consent to it and surely you're supposed to be informed about anything you do that affects something like that. But I, I don't know.

Brian acknowledges that he does not really know how people feel about donating and he reflects on the gap between informed consent as an ideal, and as a practice. Based on his understanding of donating one's body to science, Brian infers that people may be donating eggs without really knowing what for; but that this fits in with a broader ideal of "contributing to science". He then "hopes" that the people donating are informed, as "surely" they must be. There are two ways of analysing this expression of trust. Firstly, by seeing it as empty trust or "as-if" trust. Brian knows that there are problems with informed consent and that people donate without really understanding what for. However, as a lot of SCR relies on donations, and therefore appropriate consent, he does not want to question this system. Indeed, if the consent system can be undermined, many aspects of SCR would be undermined too. Brian, here, may *have* to trust in the expert system of informed consent, even if he expresses private mis-trust in it. He does not really have the choice of leaving this expert system, unless he leaves most of science and medicine. It would also be difficult for him to perform an "adaptive reaction" such as "radical engagement" (Giddens, 1990: 134-7) by, for example, forming a group to overhaul the current consenting system, as this might jeopardise his location in research. Secondly, it could be seen as a performance of trust, where Brian, through me (as the interviewer and potential conduit to other groups), is exhorting the relevant people to explain the issues as far as possible and obtain the best consent possible. It is also noteworthy that Brian compares donating eggs to donating other body parts. This can be seen as a legitimating device where a practice such as egg donation, which may be considered ethically troublesome, is associated with a potentially less ethically troublesome practice in order to make it seem more acceptable (of course, it is also possible that Brian sees no difference between donating eggs and other body parts).

Another researcher also was concerned with the limits of informed consent:

> Heidi: But I find it really interesting people who donate embryos for ES cells because I used to do consenting[80] in the 1990s [in an IVF clinic] ands no-one ever donated when I consented, I used to say you do realise these cells will just keep going, I mean the theory is that they will be around, and you don't really know what's going to happen to them, you know, [...].

Heidi, here, relates how she entered in conversation with potential donors about the unpredictability of scientific research. This resulted in people not donating their spare IVF embryos for research. By highlighting the uncertainty of science and accepting donors' choice not to donate, Heidi is confronting some of the problems thrown up by scientific research. She may conclude from her own experience that these people are not being given the same information that she would give. However, she now works on human ES cell lines, and *has* to trust that consent has been obtained in the best possible way. In summary, this section shows that the limits of informed consent as an expert system are at times confronted and at times black-boxed by researchers.

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[80] This is as part of her job working in an IVF clinic, not as a stem cell researcher.

6.4 Trust in Commercial Companies?

A central part of "risk society" is globalisation through the creation of new alliances, in particular the intensification of links between governments, academia and industry (Adam and Van Loon, 2000: 12). Ravetz (2003: 811) points out that these new associations can cause conflicting situations for governments, particularly when technological innovations are controversial. He notes that governments in most countries support industries, but have duties towards publics (see also Irwin and Michael, 2003: 57). Problems can arise when publics do not want discoveries made in public-funded university laboratories to be exploited by profit-making companies. SCR is an example of a science of Risk Society and late modernity: it has strong academic and industrial ties. For example, the Australian Stem Cell Centre was set up by the government with the express aim that the centre should be self sufficient and able to attract commercial funding to sustain itself within a few years. The possible problems deriving from these hybrid enterprises are particularly salient for SCR since potential embryo donors may not want the cells lines derived from their embryos to be exploited commercially (Parry, 2003b) and there may be no easy way for governments to satisfy donors and companies. There are also strong pressures on the stem cell community to obtain commercially relevant findings and therefore patents. This has led to one of the most all-encompassing patents being granted. The patent is held by the group that derived the first human ES cells and has been heavily criticised for being "'onerous, restrictive, and uncooperative' barriers to cures" (Gallagher, 2006; see also Kintisch, 2006; Loring and Campbell, 2006; McGee, 2006). Some commercial undertakings were labelled "cowboy outfits" by one of my informants, and many expressed ambivalence towards pressures of increased commercialisation.

Commercialisation, like informed consent and clinical trials, can be considered an expert system, which many of my informants have to enter. Some researchers, who want to focus on basic science, find they have to also think about commercial applications. In an environment with limited funding, this is, as one informant put it, a "survival thing for scientists". My informants have varied levels of interactions with commercial companies: from very broad with researchers working for companies, all the way to very limited, with researchers who have never thought about applying for a patent. All the group leaders I interview in Australia have some experience with commercial outfits and patenting, and only a couple of group leaders in the UK have no experience of it. Most of my interviews have therefore interacted with expert systems related to commercialisation at some point: by applying for patents, seeking patent lawyers' advice, selling a discovery to a company, or advising companies. Everyone I interview that has had some involvement with commercial aspects of science is quite positive about it. However, they also display a certain amount of ambivalence, seeing commercialisation as both necessary and dangerous, with some companies considered "cowboy outfits". Many scientists see working alongside industrial research and development as inevitable.

During an interview with a group leader, I discuss surveys that find people to be less supportive of commercial scientists than academic researchers. I ask him if the increasing links between academia and industry could be problematic in the future. His reply is as follows:

> Ted: Good question. I think it has to be managed carefully, and transparently obviously, [...] It's a source of funding, and that's, funding is funding, it's resource, if you want to do anything in the world, you need to have resource. And a resource is not going to come from the universities or the NHS, not in the kind of level of resources that's going to be required to drive this field forward, and particularly if we're going to compete with California and Korea and China and so on and so forth, we can't do that with £1000 from the MRC once every 3 years, it's just not going to happen, we need the investment in infrastructure in order to compete, and that investment has to come through government or private funding. On the other hand I think that does hold risk I agree with you in terms of public perception, we have to, that concerns me.

Ted seems to be assuming that sources of funding will not affect what sort of research gets undertaken and how it will be applied. Here, the sole concern with commercialisation seems to be "public perception" of commercialisation. Ted sees research as dependent on commercial funding and maybe he *has* to believe that commercial partners are trustworthy. The only problem that needs to be managed is the public response to commercialisation. This is similar to the lip-service that Beck identifies as arising from new alliances in Risk Society:

> Thus business and technoscientific action acquire a *new political and moral dimension* that had previously seemed alien to techno-economic activity. If one wished, one might say that the devil of the economy must sprinkle himself with the holy water of public morality and put on a halo of concern for society and nature. (Beck, 1992: 186, original emphasis)

Thus, too often, the bases of public mistrust are not questioned and the main attention seems to be focussed on dealing with public fear.

However these *public* fears (for example about the misuse by commercial companies of material donated to public organisations) are echoed in my informants discourses. Riana, a group leader from the UK is particularly concerned about the secrecy that pervades commercial research:

> Riana: The main problems I have with the commercialisation are that because of commercial pressures it tends to lead to publishing bias and particularly firms that are doing their own research [...] and where they are on the Stock Market, have a great reluctance to honest, to what I would say was honest publishing, in other words publishing results that come out of the laboratory that actually would have a negative impact on what you're doing, but are really important in terms of the whole field. So, for example I'm working with neural stem cells, we've done quite a lot of work recently which rather indicates that if you proliferate for a long time in culture, they're very difficult to direct towards particular phenotypes. We're just about to publish that but if we were a company it would do our share price no good at all to publish that information, it probably wouldn't see the light of day. It actually, in terms of the bigger picture I think it is very important. So I do have real worries. And, also in terms of hyping-up positive results which again unbalances the field. The stem cell field is particularly rife with commercialisation and I think there're some real inherent dangers in there.

Many other researchers have concerns about this "publishing bias"; they do not trust companies to publish all their results.

In parallel, certain researchers, like the following PhD student, contrast the goals of academic and commercial science:

> Martin: I'd be less inclined to believe science from companies than from research groups where their motivation is more for patient care or producing therapies or for the greater good or things like that, as opposed to companies that are trying to make money.

So this young Australian researcher argues that commercial research is less trustworthy due to its focus on profit. He is in a laboratory with little commercial funding, and has no experience with patents. The narrative of scientists as doing work "for the greater good" is noticeable.

Martin's view seems to contrast to that of another young Australian researcher, Brian, who is very supportive of commercial scientific research, to the point where he is hoping for patents nearly as much as publications. Nevertheless, a form of "as-if trust" becomes apparent when I ask Brian if he is concerned that commercial research may not lead to therapies:

> Brian: Yeah, in most cases hopefully they go hand in hand, so people, they want to make money because they can sell, hopefully at a decent price, some therapy that people are going to be able to use, [...]. Um, so hopefully they go hand in hand, sometimes maybe they don't, because companies try to make more money than they should, but as scientists we don't really have any control over that. [...]

Whilst behaving in a manner indicating trust, Brian expresses a sense of powerlessness about controlling how companies take research towards therapies. The quote highlights the dependence of modern science on these companies.

The potentially problematic reliance upon commercial outfits is also visible here:

> Victor: I'd worked, done some voluntary work in a part of India where nutrition was a major problem, these kids basically didn't get enough protein, [...] and I realised that you couldn't get money for research that was just going to go out there, you know there were always going to be companies that wanted to control the funding, they had a patent position, they wanted to guard that patent, but for me it was OK, if it takes 5 years for that protein to get to those kids, better get there in 5 years than not to be there at all. So I've been very, fairly practical and pragmatic about the way the whole system works, the whole [way the] commercial world goes and in Australia especially you, there's a lot more commercial money, there's a much higher commercial focus on research, [...] I know that a lot of scientists get very upset about the fact that, well what is all this project planning whatever you know, milestones, we should just be allowed to do our science, do our creative stuff, sure, I mean I'd live to be able to do that, but I don't think we have the luxury anymore, because there isn't that much money out there.

Victor sees the importance of commercial funding, even if that leads to longer delays for treatments to reach patients, a change to the sort of research that can be undertaken, and an increase in micro-management. He seems to acknowledge these pitfalls and accept them as the way science now has to be conducted. His view that you can't simply bemoan the lack of blue sky funding and that it is better to just accept commercial funding as part of the system is echoed by others. This dependence on commercialisation indicates to me that much of the proffered "trust" is in fact "as-if trust".

The lack of institutional acknowledgment reflection of the widespread concerns that stem from commercialisation and the focus on managing public "irrational" fears mean that various issues that could arise from the high involvement of commercial outfit in SCR are not given enough space for discussion. In particular, the following researcher highlights one of the risks of commercialisation –the cost of potential therapies- during a multi-disciplinary discussion. I rarely saw this risk discussed elsewhere by stem cell researchers:

> Sylvie: As a stem cell researcher it actually worries me a lot that even if the hype turns out to be true and we do develop a successful therapy, who's going to be able to afford that therapy because it's probably going to be developed partly with private money and commercial money. And that's what worries me more actually than the hype is that we're almost fooling people that even if we do develop medicines that are going to be able to cure people's diseases; well it's going to be very expensive.

This researcher suggests that the heavy involvement of private funds in SCR risks making therapies unaffordable, and she argues this needs to be addressed. In my view, the cost of therapies is an significant issues that needs further exploration in public.

6.5 Internationalisation and global legislation

In 2005, it was revealed that a team of researchers in Korea had falsified their data and potentially coerced female researchers to donate their eggs. This had international ramifications and this became the subject of discussions and criticisms around the world, contributing to the breakdown of the Koreans' collaboration with a laboratory in the USA and prompting calls for international regulations.

The above is a part of the globalisation of risk and highlights the difficulty in, for example, using cell lines derived in other countries where ethical standards may be different. Beck and Giddens argue that globalisation is a key factor in this intensification of material risks and their perception. Giddens underlines the role of globalisation which he defines as:

> the intensification of worldwide social relations which link distant localities in such a way that local happenings are shaped by events occurring many miles away and vice versa. (1990: 64)

This puts increasing demands on regulation that must take on a more global character (Irwin and Michael, 2003: 46). Many of my informants were concerned about "maverick practices" being possible in other countries.

The use of boundary-work by the people I interviewed often serves to expel researchers in foreign countries. They identify countries which they describe as inappropriately legislated and, by highlighting this, they can portray the research in these countries as unethical and therefore exclude their researcher from the legitimate scientific community:

> Fiona: Well, that's my concern too I suppose, as a scientist, you know, the technology is good, it can potentially cure these diseases, but what else could be done with these embryos or these stem cells, you know by people who maybe aren't as stringently controlled as we are here, for example.

Others express concern about "some unregulated guys somewhere" and the impacts that their *recklessness* could have on SCR as a whole.

Alongside this concern run expressions of powerlessness: scientists depend on these researchers in other countries to be trustworthy and properly regulated, but they cannot be sure this is the case. All they can do is *hope* so. However private mistrust is accompanied with public statements of trust, as these researchers many not want to be seen to constantly undermine other researchers in the same field. For example:

> Paul: I mean there are guys in the States using embryonic carcinoma cells, which are kind of like a malignant counterpart of embryonic stem cells, in patients, which, how on earth that got through approval I don't know, but they're using it, and fingers crossed you don't wake up one day and the Daily Mail has it on the front page, but there's nothing you can do.

Paul does not seem to trust the American legislation and regulation as it is allowing experiments which, to him, seem unethical. Earlier in this interview, he also criticises the lack of legislation in the USA, which enables privately funded laboratories to do pretty much as they wish. However, he feels he cannot do anything but *hope* that nothing goes wrong and that the public or press do not find out. Paul goes about his daily life hoping that this legislative recklessness will not lead to any troubles for SCR. He does not feel he can vocally criticise the USA, particularly as he is planning to go there for post-doctoral work; since, despite his misgivings, that country offers many opportunities for scientists. Thus, I would argue that Paul is displaying “as-if trust” as he feels he cannot eschew the USA.

The USA is often seen as a dominant player in SCR. For example, one PhD student argues:

> Gary: The International Society for Stem cell Research is really driven by USA based researchers. So a lot of what they say sort of goes.

In Australia particularly, many researchers feel they have to prove themselves abroad, usually the UK or the USA, before they are accepted back in Australia. For them it is difficult to opt-out of research in the USA, even if they want to have a career in Australia. The USA is seen as enabling *reckless* research due to limited regulation; but also as enabling *trailblazing* research due to high levels of funding. Hence, many of my informants articulate ambivalent feelings towards this country. In particular, many as Paul above, want to work there, but express concerned about the legislative framework:

> Wendy: It concerns me that places as big as America, they don’t have the legislation that we do. I think Britain has got the legislation pretty much right, for where we are, I think it allows us to do just enough, with keeping a decent safety boundary before we go too far. I don’t think, there's countries there that have absolutely no regulation whatsoever in stem cell research and there’s countries say, some states in America that still allow reproductive cloning and people think America has a lot tighter legislation, because they can’t work on certain things,

> but there's so many loop-hole in laws... I don't know what it's like in Korea or India, but I'm assuming that there's loopholes, it kind of concerns me that the legal people can't keep up with science occasionally. [...]
>
> Nicola: You're thinking about going to the States for your post-doc, and you mentioned that you're concerned about the lack of legislation over there; I mean is that something that makes you think twice about applying there, or...?
>
> Wendy: Yeah, it makes me think, but then again, I know the lab that I'm going to so I know that I'd never be asked to do anything that I didn't think was ethical, and I know that the whole building that I'm going to has a very good reputation, so there's no way they would put their reputation on the line to do something, if they didn't believe it. But it does concern me that a country as big as America hasn't got its legislation sorted [...].

Wendy, here, is making a "facework commitment" (Giddens, 1990): based on the person she has met from the laboratory she is applying to – so, based on the person at an "access point" for the "expert system" of SCR in the USA – she "trusts" him, and the laboratory, and has decided to go to the USA for her post-doc. This trust could be rational-calculative –that is based on Wendy's evaluation of the trustworthiness this person and by extension of this laboratory and its workers. It could also be "as-if trust", in the context where doing a post-doc in a well regarded laboratory in the USA is good for one's career and Wendy may not have the option of leaving this particular expert system (refusing to work in the USA could be damaging for her career as a research scientist). Wendy also uses a narrative of scientists as responsible people that do need boundaries but are happy to abide by them. However, there is a sense of powerlessness: if people want to do something that isn't allowed, there are always "loopholes". This reflects similar concerns identified by Parry (2003b) in patients and potential donors.

Wendy also indicates that legislators and policy-makers should be trying harder to "keep-up" with research, thus the responsibility for inappropriate use of stem cell technologies can be deflected towards these people, who are not scientists. Nevertheless, my informants do not always deflect all the responsibility for regulation onto others, and many are very supportive of the move by scientists in the USA towards setting up voluntary guidelines for ESCR (see Holden and Vogel, 2005).

Many informants are also troubled with what happens or may happen in other countries. There are many concerns about China and India; and a few comments on Korea. However the difficulties, and for some the impossibility, in regulating such a globalised science are often highlighted.

6.6 Conclusion: Opportunities for reflexivity?

The dual meaning of the labels "cowboy" and "maverick" reflects the ambivalence expressed by stem cell researchers towards a variety of expert systems related to their work. The inherent uncertainties of science enable different people and practices to be painted as *reckless* and/or *trailblazing*. On the one hand, my informants voiced support for *trailblazing* aspects of science (for example Stanley's "empiricism"); but on the other hand, they often seemed unavoidably tied in with, and dependent upon, expert systems rife with *recklessness*, as displayed by "disreputable others" (Michael and Birke, 1994).

I want to examine whether these ambivalences enable "reflexive scientization". For Beck:

> the consequences and risks of modernization can only be brought into view by passing through the *critique* (and counter-critique) of the scientific service systems from different sciences. The opportunities for reflexive scientization consequently seem to grow in direct proportion to the risks and the list of shortcomings of modernization, and in inverse proportion to the unbroken faith in progress of techno-scientific civilization (1992: 160, original emphasis).
>
> public discussion of modernization risks *is* the route for transformation of mistakes into opportunities for expansion under the conditions of reflexive scientization (1992: 161, original emphasis).

These elements of "reflexive scientization" can be found in my informants' discourses. For instance, they list limitations to science and some of its expert systems: the difficulty in evaluating risks and "unforeseen consequences"; the dual importance of trailblazing and risk minimising; the non-correspondence between human and animal data; the limits of informed consent especially when addressing the unpredictability of science; the importance and risks of commercialisation; and the difficulties in adequate global legislation. At times, they confront these: by suggesting, or supporting, the implementation of further legislation; electing not to participate in certain types of research (commercial for example); denouncing *reckless* behaviour; choosing to go to trial only when they are satisfied with the amount of pre-clinical data they have; or by highlighting the uncertainties of science, even in the context of informed consent and risking that people will not participate. Their performances of trust may also be conceived as attempts by scientists to compel others to *act* in a trustworthy manner. Thus, the elements of reflexive scientization highlighted here give reason to be more optimistic than Wynne about the reflexive potential of stem cell researchers' discourses. In addition, not all the limitations raised by scientists are the result of criticisms from outside the scientific community.

Nevertheless, like Kerr and Cunningham-Burley (2000), I find many elements of "counter modernity" or "unbroken faith in progress of techno-scientific civilization" in SCR professionals' discourses: Stanley's "empiricism"; the black-boxing of aspects of informed consent or its use as a legitimatory device (they signed the consent form, therefore I cannot be blamed for any problems); choices to work in countries deemed to have unsatisfactory regulations; the focus on "managing" public mistrust rather than examining its causes; and the deflection of responsibility towards, potentially "disreputable", others (companies, legislators, publics). My informants rarely, if ever, question the assumptions embedded in SCR and its practice. These assumptions contribute to conditions of ambivalence. Thus, these expert systems seem to enable some professionals to pass the "hot potato" around as "one can do something and continue doing it without having to take personnel responsibility for it." (Beck, 1992: 32-33).

In contrast to what Kerr and Cunningham-Burley (2000) suggest for new genetics professionals' discourses, I find that these stem cell professionals' accounts are varied and some informants are prepared to raise fundamental questions about the assumptions embedded in SCR. Thus, I suggest that these discourses do open up ways of challenging dominant framings in SCR and science in general and they are not always "a powerful brake on the radical potential of reflexive modernisation" (Kerr and Cunningham-Burley, 2000: 297)

My informants, nonetheless, often have to rely on expert systems, despite knowing that these are problematic: for example, by working with commercial companies despite concerns that potential therapies may be inaccessible to most people; or by working with human ES cells despite misgivings about informed consent. Notwithstanding the cognitive authority of science, I think it is arguable that these scientists may find themselves in positions where they see no choice but to work within these systems they do not trust. Similar to Wynne's farmers, who were socially dependent on Sellafield through professional, kinship and friendship networks (Wynne, 1996c: 40), stem cell researchers are socially dependent on a variety of expert systems through professional, kinship and friendship networks. To deal with this "social dependency" which threatens their social identity (cf. Wynne, 1996b), both farmers and stem cell researchers express "as-if" trust through their discourses and actions;

this is less threatening than dwelling upon their feelings of powerlessness. These stem cell researchers (like publics) have to participate in expert systems not only because they lack the knowledge (as Giddens argues, 1990: 33), but also the *power*, to do otherwise (or at least this is how they portray themselves).

However, I find it unsatisfying to conclude that these professionals are indeed as powerless as some of the communities described by Wynne. Rather, I would argue that they need to mobilise the authority they do have – as powerful players in a variety of networks and "fields" (Bourdieu, 1975) – and use it to create and protect spaces for these private ambivalences to become more public and "revolutionary" (cf. Kerr and Cunningham-Burley, 2000). I suggest that one element of this is to explore ambivalence with others (such as stem cell researchers from other areas, but also different kinds of publics, including patients, lawyers and ethicists) during processes of public engagement in science. I turn to various ways in which stem cell researchers' discourses can frame and construct public engagement in the following chapter.

Chapter 7

Talking of publics and engagement;

Performing scientific citizenship

7.1 Introduction: constructions of publics, science and engagement

Chapter 6 highlighted elements of reflexive modernity in stem cell researchers' discourses. It suggested my informants' concerns should be raised and discussed in public contexts; this is part of public engagement. However, drawing on the development studies and cPUS literatures, chapter 2 discussed how different types of engagement exercises constitute participants in particular ways and so impose identities upon them (see 2.2.4). Therefore, the reflexive potential of both publics' and scientists' discourses can be limited by the framing of engagement; this needs to be explored.

Detailed analysis of public discussions reveals that their framing can, on the one hand, cast some voices as value-based, and therefore make them easy to dismiss; and on the other hand, cast some voices as objective and factual, and therefore grant them cognitive superiority. As Irwin suggests, social scientists must go beyond simply advocating public engagement and:

> [...] instead explore the social processes, underlying assumptions and operational principles through which scientific citizenship is constructed in particular settings. (Irwin, 2001: 15)

As scientists are often essential participants in these engagement exercises and their framing, it is imperative to look at how their accounts and acts of engagement perform and constitute publics, scientists and engagement itself in particular ways. This is rarely examined:

> [...] participation or engagement literature tends to ignore how scientific knowledge unwittingly *performs* its imagined publics in normative ways and

> reflects its imagined publics, its commercial reference groups and meanings in its own culture. (Wynne, 2006: 219, original emphasis)

This chapter aims to address this gap, especially since the lack of acknowledgement of scientists' assumptions, including by social and natural scientists, may lead to further public disaffection (see also Wynne, 2005a; 2006).

Here, scientists' constructions of who has a role in engagement, what kind of role and in what kind of engagement are investigated. To do this, I examine three issues relating to SCR and how my informants talk about them. The first issue relates to cell lines derived from "primordial germ cells", and how these cells were classified as "embryonic" stem cells during a public debate in the Australian Parliament. By focussing on this particular instance of public engagement and how my informants made sense of it, I draw out the scientistic framing of the discussion, the authority of "technical" over "social" or "ethical" discourses and the variety in what my informants think are appropriate topics for public discussions. The second issue is the proposed rhetorical shift from "therapeutic cloning" to "somatic cell nuclear transfer" or "cell nuclear replacement". My informants' accounts of this show that some of them want to use labels that black-box this technology, whilst others want to open discussions around it and explore people's feelings towards it. These accounts also draw on the research tool/therapy dichotomy introduced earlier (see 4.4.5) and highlight the sort of discourses scientists feel are (in)appropriate in public fora such as parliamentary debates. The third issue concerns "reproductive cloning" and the use of fictional imagery. When my informants talk about reproductive cloning, some impute science-fiction based fears to publics thus devaluing their concerns, whilst others draw on science fiction imagery themselves to make sense of their views of reproductive cloning. These accounts reveal the tension between expressing one's "ethical" or "values-based" concerns and maintaining one's cognitive authority by drawing on "rational" discourses.

My informants' accounts of these three aspects of SCR draw upon several dualisms: *technical* versus *cultural*, *social*, *ethical* or *political*; *rational* versus *irrational*, *emotional* or *values-based*; and *reductionist* versus *holistic*. Like many dualisms, these are hierarchical (cf. Faulkner, 2000), with *technical*, *rational* and *reductionist* often portrayed as cognitively superior to their counterparts. These discursive dualisms co-exist, often in contradictory ways, in my data; my informants consequently have multiple repertoires on which to draw. Some acknowledge this and explore it reflexively. These different repertoires are used as analytical categories to explore how my informants normatively construct public engagement.

These dualisms are often a reflection of the contradictory roles of experts in public discussions about science: they need to be kept close to but not within politics (as shown in the literature review by my discussion on expertise, see 2.2, and on the "protection of autonomy", 2.5.1). Similarly, Haraway shows the importance of casting oneself as *rational* rather than *irrational* or *emotional*, especially in public disputes:

> The struggle is over who gets to count as a rational actor, as well as an author of knowledge, in the dramas and courts of technoscience. In the United States, it is very hard to ask directly if new technologies and ways of doing science are instruments for increasing social equality and democratically distributed well-being. Those questions are readily made to seem merely ideological, while the issues of safety and labelling can be cast as themselves technical, and so open to rational (objective, negotiated, adjudicated, liberal) resolution. The power to define what counts as technical or as political is very much at the heart of technoscience. (Haraway, 1997: 89)

However, as I explore in this chapter, it is no longer possible (if it ever was) to keep politics and ethics out of science. I conclude by arguing that scientists do not systematically seek to distance themselves from these aspects of their work. They draw upon a variety of repertoires and worldviews, and seem to have taken up criticisms of the deficit model to varying degrees.

Scientists' accounts examined in this chapter all reveal a variety of constructions of publics (uninformed, in need of education, knowledgeable, emotive, concerned, and irrational), scientists (rational, emotional, neutral providers of information, (un)trustworthy) and engagement (education, conversation, opportunity to manage one's image, opportunity to explore public concerns). I make sense of this variety in chapter 8 where I draw parallels between such constructions of publics, engagement and conceptions of democracy.

7.2 A rat in parliament; engagement and scientism

In 2002, during the lead-up to the new legislation on SCR and cloning in Australia, an eminent SCR professional, Oscar, gave evidence to Parliament. He discussed an experiment undertaken in the USA as an example of the therapeutic potential of human ESCR. In this experiment, a virus was injected into rats, partly destroying their motor-neurons and so reducing their ability to walk. Their mobility was partially restored after they were injected with particular cells. Oscar, in a brief he circulated to Parliamentarians, described this research as:

> Human ES cells directed into neural stem cells and motor neurone cells – when injected into the spinal column of rats with a motor neurone lesion (viral induced) – no muscle control at all below C6 (lower body) – were completely reversed (animals walked again and had control of bowel and bladder function) – potential application for human Motor Neurone Disease" (Oscar quoted in Parliament of Australia, 2002a: section 2.20).

Oscar also showed a video of the rats before and after their injections with the "Human ES cell" derivatives, showing how their walking improved with the treatment[81].

This episode was discussed in the media (ABC News Online, 2002b) and in Parliament (Parliament of Australia, 2002a: especially sections 2.21-2). Opponents of ESCR made several criticisms about this experiment and the way it was described. One particularly significant one was related to the nomenclature: it was said that the cells used in the experiment should not have been described as "embryonic" stem cells, as they were derived from "differentiated germ cells from the early sex gland of a two month old aborted human *fetus*" (Do No Harm, 2002, emphasis added). In contrast, ES cells are usually derived from a "blastocyst" which is only a few days old (see Thomson *et al.*, 1998). The researchers who undertook this work say they used "human pluripotent embryonic germ (EG) culture" which was "generated from primordial germ cells as described" (Douglas Kerr *et al.*, 2003: 5132); that is from:

> Gonadal ridges and mesenteries of 5- to 9-week postfertilization human embryos (obtained as a result of therapeutic termination of pregnancy by using a protocol approved by the Joint Committee on Clinical Investigation of the Johns Hopkins University School of Medicine) (Shamblott et al., 1998)

Deciding whether to use the term embryo or foetus is particularly problematic as the label "embryo" is often used for the first 8 weeks of development post-fertilisation, before the organism "becomes" a foetus[82]. Therefore, five to nine week old beings can be assigned either label.

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[81] See a press release from John Hopkins University where this research took place: https://hopkinsnet.jhu.edu/servlet/page?_pageid=584&_dad=portal30p&_schema=PORTAL30P.

[82] The HFEA, for example, defines foetus as: "The term used for an embryo after the eighth week of development until birth" (http://www.hfea.gov.uk.)

Other criticisms included that the rats were not "cured", that Oscar owned shares in a stem cell related company (therefore creating a conflict of interest) and that the research was presented as published when this was not the case. Oscar was given the opportunity to respond in the Senate. He explained that he had already rid himself of his shares and that his assumption about the research's publication's status was a mistake. He also argued, using a variety of examples, that the "terms 'embryonic stem cells' and 'embryonic germ cells' are often used interchangeably" (Official Committee Hansard, 2002, September 24th: CA 191). He highlighted that: "Both represent human pluripotential stem cells derived from embryos and are quite distinct from adult tissue stem cells".

Taking this case as one example of public engagement, I examine what versions of publics, scientists and engagement my informants contrast when talking about it. I draw out two themes: scientism and boundary-work conducted for "expulsion".

7.2.1 The prominence of scientism

"Scientism" suggests that science alone can provide all the answers. It is an "institutionalized idolatry of science" (Wynne, 2006: 214), and:

> It reinforces, without question or comment, the widespread sentiment that science should be the only authority for belief and the only criterion for action. (Ziman, 1991: 33)

As I demonstrated earlier (see 2.2.4), public consultations that focus on *technical* aspects of a particular scientific development can marginalise other aspects embedded within it. Parry (under review) looks at stem cell researchers' accounts of embryonic stem cells (ES cells). She argues that by rhetorically and visually focussing on *technical* aspects of these cells – emphasising their small size, their physical resemblance with other cells in the body, their "absence of identifiable human features or structures, such as limbs", and by naming them "blastocysts" – scientists remove other more contentious issues from the discussion. These issues include the circumstances under which embryos are procured and the potential related health risks to donors.

Focussing solely on *technical* issues also pre-empts discussions about choosing between adult and embryonic stem cells on "ethical grounds" (where cells from the skin are seen as less *ethically* problematic than cells from embryos). Scientists can then focus on cells' "pluripotentiality" – that is their ability to differentiate into various cell types – and paint ES cells as more promising. This "dehumanisation" and "scientisation" isolates science from other values and focuses discussions on aspects in which scientists are themselves experts. These sorts of understandings of embryos, focussing on their size and cell-like appearance, were also very prominent in the media (e.g. Williams *et al.*, 2003).

In this context, where proponents of ESCR are keen to highlight the cell-like (non-foetus-like) visual properties of ES cells, the merging of ES cells with germ cells is particularly noteworthy. The primordial gonadal ridge from which primordial germ cells and then "embryonic germ cells" are derived is a primordial *organ*. Organogenesis has thus begun in the entity from which these cells are derived, and in this sense, this entity is becoming more human-like, moving towards having discernable physical human features. As feminist writers have previously shown (e.g. Petchesky, 1987), foetal images are powerful in highlighting the kinship between these entities and fully-developed humans. It is therefore interesting to see how my informants view this episode and how they define ES cells and germ cells.

When I interview Oscar, I ask him whether his experience of public speaking has helped him for the stem cell parliamentary debates:

> Oscar: I was always of the view that you should be direct and as simple as possible, the problem is you actually get very small grabs or very small bites for transmitting your information, you know 5 minutes, 1 minute, 10 minutes is about what it is often, so you can't be too complicated or too complex, because you spend all your time addressing the complexity of the issue. So in getting that information across, you need to be careful that you're portraying it in an accurate way and so, there were certainly claims, because I'd taken a much shorter bite at some of these things that the information that was transmitted wasn't accurate enough for some people.

Oscar is referring to the rat episode. He argues that in order to get a message across, in a limited time, one has to be succinct, but also accurate. He is indicating, as he did in Parliament, that it is appropriate to use "ES cells" as a short-hand for germ cells, in order to make his message clear. For him, not distinguishing between so-called embryonic germ cells and ES cells is not a problem as he judges these on their *pluripotentiality*. For him the valid characteristics on which to differentiate stem cells area particular set of *technical* ones. He constructs his audience (parliamentarians and other public groups) as wanting technical information stripped of its contingency and complexity. Public discussions are occasions for simplifying the *facts*, not raising new questions.

By drawing on "scientistic" arguments, Oscar is not leaving a space for discussions about the origin of the stem cells. It could be argued that deriving cells from an "embryo" that is a few days old raises different issues to deriving cells from an aborted "foetus" that already has started developing primordial organs. Not everyone with an interest in SCR would be happy with the rhetorical separation of cells from their source, as is evidenced by some anti-abortion submissions to Parliament. There is no evidence here to suggest that Oscar is using this terminology because he does "not feel properly qualified to take an explicit position on the moral status of the object of concern" (Hauskeller, 2005). Indeed, he stated in parliament that he felt "uncomfortable" working on aborted foetuses (Official Committee Hansard, 2002, September 24th: CA 207). Rather I would argue that this choice is more strategic. If one vital aspect of the pro-ESCR case was to distance ES cells from human, likening these cells to cells from an aborted foetus (and its associated powerful imagery) risks closing the distance between ES cells and humans and therefore the destruction of life. Therefore it is a strategic necessity in this instance not to highlight that these cells come from an entity that could be labelled "foetus".

In addition, by using experiments done with germ cells derivatives and labelling them ES cells, Oscar extends the definition of ES cells to accommodate the successes of these germ cells. This in turn "expands" the authority of ESCR. Although Oscar did not reflect upon this rhetorical strategy, another researcher did:

> Georges: [Oscar] was relying on data to prove you know that embryonic stem cells could regenerate I forget what it was, spinal cords I guess, of these rats, and the only data he could find, was not from embryonic stem cells. It wasn't that he was trying to sort of, you know sort of… tell a white lie, he was telling a real lie. Because I mean, the point is that he didn't have data, that's what I think he, you might say well, what are those cells called, embryonal stem cells[
>
> Nicola :]Foetal weren't they?
>
> Georges: Foetal… foetal germinal stem cells, something like that, I'm not an embryologist, but they were several steps downstream from embryonic stem cells, so he was using those because they were, that experiment obviously worked.

Georges works in a field related to SCR (immunology) and is opposed to ESCR. He is criticising Oscar's tactics of labelling ES cells and germ cells as the same. Georges, like Oscar, justifies his position on *technical* grounds: he argues that germ cells are "several steps downstream from embryonic stem cells". This implies that they are more differentiated and therefore are likely to have different properties.

What is remarkable is that Georges does not agree with ESCR for several reasons, one of which is that he opposes the destruction of embryos. His views are informed by his Catholic religion. I would, therefore, have expected him to highlight the problems with using tissue from aborted foetuses. However, both here and in Parliament where he also gave expert evidence, he chooses to make his case on what I classify as *technical* grounds. He mentions during our interview that he used to highlight his religious view, but found that this "weakens your argument". Georges' reflection highlights the prominence of scientism in public debates. Both Georges and Oscar have broad experience of public talking and have probably learnt through experience that *technical* claims hold more sway than arguments based on personal beliefs. Both these researchers are drawing on a "modernistic worldview" (Goven, 2006) where *technical facts* are the gold standard by which to judge cells and science. In this way, people who cannot articulate their views scientistically are excluded as a result of the frameworks in which debates take place.

This scientism was embedded in the parliamentary context. Although many parliamentarians strongly opposed to research on embryos succeeded in focussing the Senate hearings on Oscar's character and the ethical and religious aspects of using ES cells, the report following the hearings focussed on *technical* aspects of ESCR and separated them from other aspects of the research. As I highlighted in chapter 4, the report also focussed on pluripotentiality:

> For the purposes of this inquiry, the properties of cells that are of interest are those related to their capacity to 'differentiate' or 'specialise' into particular kinds of tissue (Parliament of Australia, 2002a: section 2.27).

Thus, the public debate was effectively ring-fenced to only address the *technical* properties of stem cells. This makes it difficult for people who take a *holistic* (rather than *reductionist*) approach to these issues world – who see the nature and value of ES cells and germ cells as inseparable from the contexts of their derivation and application – to express their views about ESCR.

7.2.2 Boundary-work for protection or expulsion; defending science

This rat episode also illustrates another type of boundary-work, "expulsion", which is conducted to banish "pseudo-scientists" from the realm of science and thus protect its good name. Gieryn (1995: 432-4) discusses the case of Sir Cyril Burt, a psychologist publicly accused of fraud. Burt was a high profile researcher whose work confirmed "hereditarian" theories (who see psychological factors as determined by genetics) but was criticised by "environmentalists" (who see them as determined by the environment) for being spurious. Initially, the hereditarians defended Burt, describing his work as merely "sloppy", and accused those who attacked his character of being politically motivated (accordingly "unscientific" and unreasonable). The environmentalists continued to argue that Burt was the one being unduly influenced by political factors and giving science a bad name. However, when it became clear that Burt's conduct was indeed fraudulent, hereditarians aligned their position with that of the environmentalists. They expelled Burt from their ranks, blaming "personal and idiosyncratic troubles", and highlighting the role of psychologists in policing their own field. They could secure the authority of psychology and its credibility as an appropriate scientific endeavour (whether focussing on environmental or genetic factors). Similar attempts at expulsion' can be seen in my informants' views on Oscar and the role of public debates, raising questions about what my informants see as relevant in public judgements about SCR and stem cell researchers.

Although I never specifically raise this rat incident, it comes up in several interviews. Many researchers feel that this episode had had a deleterious effect on relations between the stem cell community and public groups. The two quotes below are example of this:

> Connor: He kind of clouded the issue because he was a bit free and easy with his definition of stem cells. And I don't think that did the cause too well because it says that, you know, it comes down to what you were talking about, public perception of scientists, the public expects scientists to be precise in their use of terms and definitions and so on. So I think the reaction to him just referring to those as stem cells, the people then, particularly the opponents would say, look you lied on that when he was probably, I'm not necessarily trying to defend him, I can imagine that he was just trying to make it easy as it were, generalising, yeah. Because the debate was extremely heated, it wasn't the right time to generalise.
>
> Nathan: To tell you, a lot of Australians in the scientific community particularly in the adult stem cell community were pretty pissed off at [Oscar], because he's got a lot of money, he made a lot of money and made a lot of money sneakily. And also that way that he went about, yeah, I guess trying to deceive the public through the media with that rat walking again. That's not science, that's wrong, and it's nearly embarrassing in a way to be associated with that area of science when you see things like that going on. I've had to defend myself over that, say I'm not involved in embryonic, what he did is not what other embryonic people do either, it's what he did and it was wrong and, yeah.

Both these researchers work in ASCR and disagree with the way Oscar presented this experiment. They feel that this rat episode had a direct effect on public trust, not only for researchers in ESCR, but also in ASCR. Here, public discussions are seen as important moments for scientists to manage their image of trustworthiness amongst wider publics.

Nathan attempts to "expel" Oscar from science because he sees him as unduly influenced by social factors such as money. ASCR competes with ESCR for funding; hence Nathan's attitude parallels that of the Gieryn's environmentalists. For Nathan, the character of a researcher is fundamental to the credibility of his/her work and untrustworthy scientists need to be expelled to protect SCR as a whole. Connor is more generous and argues that the context of public debates may have made it difficult for Oscar to explain what he saw as the differences between ES cells and germ cells. However, Connor does accept that these two cell types are different; thus he is not simply judging them on their pluripotentiality. Connor seems to be acting like Gieryn's hereditarians early in the debate: he would characterise Oscar's talk as "sloppy" rather than deliberately misleading. Both these researchers understand the importance of factors, such as trustworthiness, in public assessments of science and they construct publics as having means of judging science and scientists. They impose a less scientistic framing on public discussions than the informants discussed above, in 7.2.1: they argue that the choice between different types of stem cells will be made by taking into account factors beyond the cells' pluripotentiality, and including the personal commercial links and ethics of advocates of particular cell types. Both Connor and Nathan are happy to be working in ASCR which they portray as raising fewer ethical problems than ESCR, therefore it is perhaps unsurprising that they are not desperate to defend Oscar.

Steven, on the contrary, criticised the parliamentary debates and following media stories for enabling these sorts of personal, rather than technical, issues to be raised:

> Steven: [...] I've heard [Oscar] for example you know, and seen debates there and in a really, he's generally been on the back foot, he's been faced with a fairly staunch set of opposition [inaudible], I have to say I sort of take pity on him because a lot of the, a lot of the questions I thought were really left field and really not, they were for shock value only, not really to, to really ask probing questions, they were simply there to rattle his cage.

Steven works in adult stem cell research (ASCR), but has collaborations with ESCR. He is contrasting ES cell researchers, whom he described, earlier in the interview, as presenting their "data" in a balanced fashion, with their opponents (anti-ESCR parliamentarians here) who ask inappropriate questions. Although Steven does not mention specifically what questions he is alluding to, the only ones that were asked were those about Oscar's alleged misrepresentations of the data and its publication status, and those about his share ownership.

Thus, it is clear that Steven feels that public debates should not focus on the trustworthiness of particular scientists, or issues such as conflicts of interests, but should focus on “the data”. Therefore, public debates are seen as an arena for presenting scientific “facts” and other questions are not seen as appropriate. He is defending Oscar, as Gieryn’s hereditarians did in another context, by accusing opponents of having ulterior motives (political ones) rather than simply being interested in the “facts”. Steven dismisses as irrelevant the contexts in which research takes place, such as the increased commercialisation of science (cf. Goven, 2006).

Bernard is a supporter of SCR in general, but was quite critical during our interview of the way the rat experiment was discussed in Parliament and in the press:

> Bernard: I said look, the real problem with the media is not whether, this [Oscar] controversy, I don’t know if you know about this, where he shoved this film of this rat. The story of [these] rats was that they had a viral motor-neuron disease, they didn’t have spinal cord injury, nothing to do with that. And all his studies, [...] showed that there was no neuronal replacement in those animals[83], there was some glial replacement. Anyway, unfortunately [Oscar] showed this in the context of the Senate, in which there were people with kids in wheelchairs, and everything else, and purported to say that it was a spinal cord recovery and they were embryonic stem cells. What the press jumped up about, was that they weren’t embryonic stem cells, but they were these gonadal stem cells which, you know...
>
> Bernard: But the point was that they missed the major discrepancy that this had nothing to do with spinal cord injury and that the data was unpublished and it wasn’t verified and you know, and it really upset me greatly, […]. But they kept wanting to ask me “but these weren’t really embryonic stem cells; well they were gonadal, virtually the same properties, it doesn’t concern me, it’s silly for him to say they were embryonic, he probably thought they were at the time, I have no idea. But the argument got lost in this embryonic or not embryonic, rather than in whether they actually did anything, which seems to me to be the bottom line about this whole debate. I mean if they don’t do anything, there is no debate.

Bernard, like Oscar, does not distinguish between ES and germ cells; he paints them as having similar “properties”. However, he does see a difference between reducing the rats’ mobility by using a virus that prevents their neurons from working properly, and by creating a spinal cord injury by, for example, severing their spinal chord. He indicates that the symptoms of the latter would not have been reverted by this experiment. Bernard’s focus on this is likely to be due to his clinical experience of spinal cord injuries. Here, we see a scientist criticising the public discussion processes for not allowing certain questions to be raised. Although the focus remains scientistic, this example also shows that, for Bernard, the

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[83] These are the sort of problems certain commentators highlighted to the Australian senators in the wake of Oscar presenting this data. They argued that the rats were thus not “cured” (see 7.2).

value of particular cell types is embedded in their potential application. This last quote also shows Bernard's view of public discussions as special fora where, due to the presence of publics, like the "people with kids in wheelchairs", scientists have a special duty to watch what they say, be accurate and not raise false hopes. These particular publics are seen as highly emotive and desperate for cures. So for this scientist, SCR cannot be disembedded from its application and its political and social contexts.

7.2.3 Multiple constructions

This rat episode has enabled me to look at a variety of sociologically interesting aspects of my informants' discourses. In particular, it highlights how informants are scientistic in their accounts and do not address other questions that may be raised by their research. It shows their wish to provide "accurate" "technical" information to guide policy (also discussed for new genetics professionals in Kerr *et al.*, 1997) and educate publics into appropriately distinguishing various types of stem cells (that is by looking at their pluripotentiality). By focussing on *technical* aspects, scientists can avoid a loss of authority which often accompanies the entry of scientists into political and policy arenas (Jasanoff, 1990; Gieryn, 1995; Parry, under review).

Even when focussing on the *technical*, however, there are many possible positions as to what SCR really is, or should be, about. These different positions have different outcomes in terms of which cell types are portrayed as most promising. Again, there is no unique story about SCR (see also chapter 4).

Some researchers, who are critical of Oscar's manoeuvrings, think that questioning scientists' personal trustworthiness is an important part of engagement. They draw on a more holistic worldview and highlight that ES cells are judged on a variety of factors. It is relevant here that these researchers (quoted above) all work in ASCR, and therefore may have less to lose professionally by criticising ESCR[84]. Nevertheless, they share the view that publics need simple educational messages.

Publics are seen at times as emotive patients and relatives, politicians, or the general public. They are constructed as receptacles for factual information, as easily misled and unable to

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[84] Although see 5.5, on the importance of professional solidarity.

judge science on their own without scientists pointing out the relevant aspects. Yet, the public are also portrayed as able to judge science and scientists using indicators such as trustworthiness and involvement in commercialisation. Public discussions were seen as important arenas for scientists to explain their work, and convince others of its usefulness, but also manage their image of trustworthiness.

7.3 “Therapeutic cloning”; labels to black-box technologies?

At the time of my interviews, “therapeutic cloning” was legal in the UK, and illegal in Australia. The permissive legislation in the UK was partly attributed to the successful severing of “therapeutic” from (more problematic) “reproductive” cloning (Parry, 2003a; 2003b). Part of this success was due to scientists highlighting the “therapeutic” potential of ESCR (see 4.2). Another key strategy was to rhetorically separate the two by avoiding the term “cloning” and replacing “therapeutic cloning” with “somatic cell nuclear transfer” (SCNT) or “cell nuclear replacement” (CNR).

In the UK, the Human Fertilisation and Embryology Authority (HFEA) and the Human Genetics Advisory Commission released a joint report in 1998 in response to growing public concerns about embryo research and cloning. In it, they encouraged a change of nomenclature from “therapeutic *cloning*” to “therapeutic uses of *cell nuclear replacement*” to “avoid the stigma of the word ‘cloning’” (discussed in Parry, 2003a: 179, original emphasis). Kitzinger and Williams have noted that, when scientists talk in public, they tend to use “therapeutic cloning” as, unlike CNR, the label directly implies therapies (2005: 735). Opponents of this work, however, tended to use the label “human cloning” thus “implicitly conjuring up visions of full reproductive cloning” (Kitzinger and Williams, 2005: 734-5). In 2004, the International Society for Stem Cell Research (ISSCR) decided at its annual conference that “nuclear transfer” should be used in preference to “cloning”. The term “therapeutic cloning” was seen as a “commercial” term with “negative connotations, “cloning” as not “accurate” and “therapeutic” as “misleading” (see ISSCR, 2004). Other scientific bodies have since made similar calls (see Vogelstein *et al.*, 2002; Hauskeller, 2005)

The choice of labels such as “therapeutic cloning” in public engagement can therefore promote and/or hinder particular SCR futures. For this reason, the label is a fruitful site for sociological analysis, both in the UK, where the case for therapeutic cloning has been won, and in Australia, where my interviews took place in the build-up to the discussions for the 2005 legislative reviews when SCNT became legal. My informants' accounts of this label, as well as on the techniques associated with it, provide insights into their construction of publics and engagement, as well as SCR.

7.3.1 "Cloning": opening-up discussions or black-boxing

Mulkay's (1994) analysis of the 1980s embryo debates in the UK offer a useful precursor to today's SCR debates. In particular, he has shown how labels serve as powerful political devices. He argues that two opposing images of the embryo came into play, resting on different labels. The people opposed to research on embryos, by using the label "embryo", drew on readily available cultural resources to render experiments on defenceless "un-born children" (1994: 621) repugnant. People supportive of embryo research, however, had no available cultural trope to establish the appropriateness of this work. They therefore had to invent a new image and label that were more acceptable. They did this by creating the term "pre-embryo", which designates the first 14 days of development after fertilisation, before the appearance of the "primitive streak". Mulkay argues that the label "pre-embryo" was an intrinsic part of the eventual success of the pro-embryonic research groups in Parliament.

Similarly, proponents of therapeutic cloning can have difficulties in promoting their work and separating it from reproductive cloning. Opponents, by playing on the association between reproductive and therapeutic cloning, can undermine this type of research. Proponents have to create a new image of this technology as separate from cloning full humans, but that promises applications in research and therapies. The label "SCNT" offers one such image.

During my interviews in Australia, the label "therapeutic cloning" is repeatedly commented upon. This is probably related to the upcoming legislative reviews for which some of my informants were preparing submissions. Some think SCNT is more appropriate and accurate, others disagree. One researcher feels that it is necessary to change the label and no longer use the word "cloning" due to its connotations:

> Balthazar: I'm sure you'll get to it, when you talk about somatic cell nuclear transfer. The issue basically is people call it cloning, and it is therapeutic cloning, but my God, all you've got to do is look at it from the science point of view and see what you're doing is taking a nucleus and putting it from one cell into

> another. It’s much better to utilise the term nuclear transfer, because it is, it describes exactly what you’re talking about and it’s devoid of all the hype, and devoid of the association with the concept of cloning another human being, which is what basically what a lot of people get upset about the possibilities of. It’s not, yes technically it can be done, or theoretically it can be done by a similar process, but you know, checks and balances can be put into place to prevent such a thing, so let’s call it what it is, which is nuclear transfer. That’s all it is. The issue is …the word cloning means lots of things to different people, and I think it’s [better] to utilise words that describe what you’re talking about.

Here Balthazar is implying that “nuclear transfer”, a more *technical* description, is more accurate. He specifically highlights that the label nuclear transfer can be distanced from reproductive cloning. Thus, he can see the power of labels in carrying particular connotations; but for him, in this case, the more accurate description is one that is devoid of these connotations. He does not reflect upon the power of labels such as SCNT in deflecting criticism or in black-boxing issues related to the technology: they can foreclose any public discussion about the potential “slippery slope” towards human reproductive cloning or about issues of kinship and identity. These labels also help to mask the origin of the cells: the word “cloning” reminds us that the cells in question come from a particular donor, whose genome is to be reproduced, whereas the word “nuclear transfer” more easily isolates the technique to the laboratory, with no connections to the outside world, to the nucleus donor and his/her cells, or to the oocyte donor, her body and her potential hyper-stimulation syndrome. The implication, here, is that public debates should focus on *technical* aspects and avoid raising all the concerns mentioned above.

Another informant from Australia, Gerald, raises the ISSCR’s wish to change the label. He agrees with Balthazar that we should avoid the word cloning – which, for him, has the “wrong implications”; that is, human reproductive cloning. However, he does not find “somatic cell nuclear transfer” a much more helpful label, as: “the general public, they have no idea what somatic cell nuclear transfer meant”. He also argues that this label does not directly imply the promise of therapies, as I shall discuss in the next section. Therefore this new label makes it difficult to get a simple message across.

One Australian informant takes a different approach. Although she agrees that “cloning” is not a very accurate label for the process of nuclear transfer, she criticises the move to simply change its name:

> Heidi: [The ISSCR] brought out an edict to all members that they shouldn't use the word cloning, they should use nuclear transfer, well, in actual fact, nuclear transfer is a technology that doesn't always result in cloning, [...], so it's actually a

> misnomer from what they're doing, but also I mean, to me, I don't understand why they think if you call it something else, people are not going to, people are going to think it's OK. I don't, I don't really understand why they think people are that stupid. I don't know. Maybe they do think people are that stupid, but I don't think they are! Most people who are stupid probably don't care.
>
> Heidi: [People] are not against the word, they're against the technique to make children, so just because you call it nuclear transfer, they still should be against it, you're just saying they don't understand what you're talking about because nuclear transfer is meaningless. To me, that seems really stupid, why not just discuss the issue, like say "OK, well why are you against cloning?", and you know, be very clear about what cloning is, and what is the problem with it, and if people are against it, you regulate it, but…

This informant argues that dissociating reproductive and therapeutic cloning by name will not help resolve any difficulties ("I don't understand why they think if you call it something else, people are not going to, people are going to think it's OK"), and portrays these discursive changes as tactics to avoid difficult conversations. She agrees with Gerald above that people may not know the meaning of "nuclear transfer" but suggests, therefore, that it is appropriate to talk about "therapeutic cloning" and discuss the issues that this label raises. For her, public engagement is not just about getting a simple message across to members of the public, but also involves discussing any concerns that people may have. Like many of the informants who want to black-box SCR and ignore questions that cannot be answered by science, Heidi had broad experience of engaging with publics. However, this has taught her to listen to (and respect) other people's knowledges. So experience of engagement can for some scientists lead to an increased ring-fencing of issues and strategies to avoid raising difficult questions, but for others to a willingness to listen to and learn from other people. Of particular interest here, is that Heidi works on human ESCR. Therefore, further public discussions of SCR could lead to her work being directly under threat.

The above informants all work in Australia. The situation in the UK is slightly different and the change of labelling suggested by the ISSCR is not raised during my interviews. Informants do not correct my use of the term "therapeutic cloning" and are comfortable with the terms SCNT, nuclear transfer and cloning. One informant does comment on the difficulty of banning reproductive cloning and allowing therapeutic cloning, when they are both "from the same technique". Although fears of reproductive cloning are often projected onto "the public" (see later in this chapter), the need to disassociate reproductive and therapeutic cloning in public discussions was not raised. This could be an indication that the boundary between reproductive and therapeutic cloning is seen as successfully constructed and stable enough for the moment.

7.3.2 "Therapeutic" cloning? What can and can't be said in public

During the UK debates leading to the 2001 HFEA amendments, proponents of therapeutic cloning were keen to emphasise its potential "therapeutic" applications. However, as I noted in 5.4.3, many of my informants, in both countries, suggest (in the semi-private context of the interview) that the technology for therapeutic cloning may better be applied for research than directly for therapies. The only scientist to specifically promote the potential of SCNT in tailor-made therapies does so in the semi-public context of giving a presentation in a multi-disciplinary discussion:

> Vincent: I thought I should touch on [...] cloning because this is the other component of the controversy. So from the point of view of a scientist, this is really just something beautiful because it's the perfect solution to rejection. So rejection is the problem you would have in transplanting any of these cells. And it's a major major problem, one that actually really limits the value transplants. Hmm, you know, there are various potential solutions, but cloning, would offer what might be an ideal solution. [...] So this has been demonstrated now several times, in the mouse, [...] and now recently also mentioned in human. So we, we know that in principle it could work. Of course, we don't know [...] that there may not be some risk to this procedure, we don't know that these cells are fully programmed accurately, and we don't know, [inaudible] be able to say anything about the efficiency of it, whether it would be economically viable. But we're only going to be able to answer those questions by doing research.

This is typical of the public talks I heard promoting the use of SCNT in therapies, but contrasts heavily with the sort of accounts I heard during interviews. In this section then, I examine the therapeutic aspect of "therapeutic cloning" and SCR more generally and address questions about what scientists feel they can and cannot say in public.

Philip, working in Australia, raises problems with both words in the label "therapeutic cloning". When answering my question on whether avoiding the label "therapeutic cloning" is useful in public debates, he says:

> Philip: I think, these things, there's kind of an interesting history in this whole area that you may know about. In the old days of the IVF debate in the UK there was an attempt to substitute the term pre-embryo, to substitute for embryo, in what's currently done in IVF. I think the trouble with therapeutic cloning is that it's a terminology that's not accurate. It's not in and of itself therapeutic, the only goal isn't therapy, it's also research. And it's not cloning in the sense that cloning means to make a new individual, it's really, if you look at it in a scientific light, as cellular reprogramming. So it's an experiment with cultured cells. I think if we can get that idea across, we'll have done a good job, but all we can do is put the

> concepts out there as clearly as possible for the public, and then it's not down to us to make the decision.

This quote is worthy of note on three accounts. First, although Philip draws a parallel between the rhetorical manoeuvrings during the current stem cell debates and those during the 1980s, he does not seem to acknowledge these as *rhetorical manoeuvrings per se*, and does not give these changes in labels any political weight. Rather, and this is the second point of note, he places himself and other stem cell researchers as neutral and objective advice providers (as do genetics professionals, cf. Kerr *et al.*, 1997). Having said this, Philip is one of the very few scientists I interviewed who had no problem supporting the idea of scientists "lobbying" governments. Third, Philip, like Oscar and Balthazar above, constructs public discussions as fora to get simple, clear, accurate and objective messages across to the public. Again there is no reflection on the social factors that shape the choice of the message to be delivered (and, as I discuss later in this section, Philip is clearly aware that there are several ways of talking about therapeutic cloning).

Many other researchers in the UK and Australia share Philip's view that therapeutic cloning is unlikely to directly provide therapies:

> Ted: [...] the current paradigm using cloning, cell cloning, from a therapeutic perspective is a no goer.

Interestingly, when Ted talks about the "paradigm", he is referring to what *public* discourses centre around, not discourses within the scientific community. Indeed, after this quote, Ted gives examples of eminent stem cell researchers who, in his view, no longer believe that SCNT will be used directly in therapy. The dominant discourse in the scientific community, according to Ted, is that SCNT is a promising research tool. This highlights the divergence between what stem cell researchers may think and what has filtered out – or what scientists have tried to transmit – to broader communities.

One explanation as to why this divergence exists can be found in the following account by Philip. He makes a most revealing statement about his view of publics and engagement, which shows that he is aware of different ways of presenting scientific "facts":

> Philip: I think in that role, our job is to explain as clearly as possible the science behind everything to clear up any misconceptions [...].
>
> Nicola: Is that difficult?

> Philip: It is difficult. To give you an example, I think that actually in some ways one of the most important contributions of embryonic stem cells in particular will perhaps not come from their direct use in the clinic, but rather from their use as research tools: for understanding human gene function, for understanding disease, for developing new medicines. Because for the first time with this system we have a source of virtually any type of normal human cell to study in the laboratory, to manipulate. And if you think about it, that's a very very powerful tool; but it's quite difficult to explain to the public. It's much easier to explain to the public well this might one day make this person walk, and so I think in trying to put across the benefits, potential benefits, of this research, which the scientific community overwhelmingly felt they were there, one tended to focus on those sorts of outcomes rather than these equally important things that are perhaps more difficult for the public to understand.

Philip is commenting on the public debates in the lead up to the 2002 legislation on SCR in Australia. From this quote, I propose that, for Phillip, public discussions are seen as an occasion to promote a particular area of science (here SCR, particularly using embryos), by highlighting its potential role in therapy, rather than highlighting that it may not work in that fashion, and therapies may be arrived at by other routes. Again Philip casts himself as a purveyor of neutral information. However, by painting this picture of SCR (that is by focussing on the easy-to-explain therapeutic advantages), he is not simply purveying neutral information: he is *selling* it. This quote highlights his conception of the public as ignorant, and unlikely to possess sophisticated understandings of how science operates, and in need of receiving simple images such as “SCR equals therapy”. Philip does acknowledge this to an extent[85] but it is, in my view, still troubling that he constructs engagement in this way.

There are other examples in the UK and Australia of the above conception of engagement. Often, as soon as I asked my informants how they would present SCNT in public, they highlighted the need of many patients for a cure and suggested SCNT as a way towards that, even if they had just told me they could not see any therapies coming directly out of SCNT[86]. For example, Ted opened several public talks I attended by highlighting the therapeutic potential of SCNT and contrasting it with the immune rejection problems faced by conventional organ transplantation.

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[85] For example later in the interview, he does highlight that the case for SCNT should be made by focussing on its potential as a tool, rather than a therapy.

[86] Nevertheless, I do want to point out that in a submission, made by one of the laboratories for which some of my informants work and sent several months after these interviews to the committee reviewing the SCR legislation, the case for SCNT was made in terms of research tools, and did not overemphasise the tailor-made therapy approach.

I found that researchers, particularly those with experience of interacting with patients, are keen not to offer false promises, either to avoid backlashes or simply patients' disappointment. Some researchers do not fear public backlashes, and portray publics as capable of seeing the provisional nature of science, and therefore of forgiving scientists for making promises that do not bear fruit (data not shown). In addition, a variety of public talks, both in the UK and Australia, tend to focus on the direct therapeutic promise of SCR. This indicates a disparity between talks to broad publics, and talks to me or to patients. It may indicate that some scientists see patients as different kinds of publics, and may acknowledge they can play particular roles in science governance (see chapter 8)

7.3.3 Multiple constructions

Some of my informants do not want to leave space in public discussions for understandings of cloning and nuclear transfer that associate reproductive and therapeutic cloning. According to them, the only proper or "true" understanding of these two technologies is one that emphasises their differences, not one that highlights the fact that they derive from the same technology (which others were keen to highlight). They want to avoid the label "therapeutic cloning" and dissociate therapeutic from reproductive cloning techniques. These stem cell researchers seek to achieve the same with "SCNT" as was achieved with "pre-embryo": create a cultural trope which does not have ethically troublesome implications.
They focus on other questions about cloning and how it might help patients, for example. Here, publics are constructed as lacking a stock of knowledge and experience with which to judge scientists and their pronouncements, implicitly promoting the need for education rather than participation.

Most of the researchers in this section construct publics as potential patients, wanting quick fixes and cures, and uninterested in research. They also portray them as unable to see any link between advances in research tool and in therapies. Thus, here, public engagement is aimed at "educating" publics into being supportive of therapeutic cloning by "informing" them about the therapeutic promise rather than the uncertainty surrounding this technology.

For one researcher (Heidi) however, hiding behind new labels is not an answer, and will not allay people's fears of cloning. For her, it is essential to engage with people's fears rather than dismiss them as unfounded, regardless of the labels used. Publics are constructed as holding relevant knowledge and holding legitimate fears (not based on mis-understandings for example) that need to be examined. Engagement is an opportunity to enter into conversations with diverse publics, rather than foreclose debates by black-boxing technology. Because Heidi works on human ESCR, and because respecting people's concerns about cloning human embryos may directly impact on other forms of ES cell derivations, it seems that she is prepared to put her job at risk in order to have these fascinating, but challenging conversations.

I have also highlighted that there is a disjunction between what many of my informants say about SCNT (that it would be a useful research tool) and what public talks often focus on (the direct use of cells derived from SCNT in patient-specific therapies). Thus, some informants view engagement as a means of promoting science like SCNT, by focussing on what they think publics will find more acceptable; whereas other informants are more prepared to question what they are doing and use engagement to air concerns and issues about their work.

7.4 Reproductive cloning, science fiction and rationality

Mulkay (1996) has examined the use of science fiction images in the 1980s embryo debates in the UK. IVF, at the time, was in its very early infancy and to make sense of it, many people looked to science fiction. As Mulkay argues:

> What could be more natural than to fill the missing parts of the test-tube story along Frankenstein lines? (Mulkay, 1996: 158)

He then explains how members of the press and Parliament drew on science fiction to discredit those objecting to embryo research and IVF. By steeping their opponents' views in fictional imagery, proponents of IVF were "removing [their] opponents' objections from the sphere of fact to the cognitively inferior domain of fiction" (1996: 162). As a result:

> When Frankenstein appeared within the context of pro-research discourse, he was made to speak, not of the dangers of science, but of the credulity, ignorance, and dogmatism of those who were unwilling to endorse the advance of science knowledge. (Mulkay, 1996: 169)

Consequently, science fiction can be used to discredit critics of science.

Gamson and Modigliani introduce the concept of "interpretive packages" which are clusters of elements such as "metaphors, catchphrases, visual images, moral appeals and other symbolic devices" that characterise a temporarily dynamic discourse which provides people with "interpretation and meaning for relevant events" (1989: 2). There are a variety of competing packages available for people to draw upon in order to make sense of the world, but these packages are also themselves shaped by shared cultural meanings. These packages are signalled by "condensing symbols" (1989: 3) which often correspond to strong images from particular works of fiction. These can include "one hundred dictators" from Levine's *Boys from Brazil* or "alpha males" from Huxley's *Brave New World.* As these symbols draw on well known images, they are short-cut ways of expressing particular thoughts or feelings. However, there are often manifold interpretations to these works of fiction. Thus, in the above IVF story, the condensing symbol was "Frankenstein" for example which has "cultural resonance" (1989: 5) with shared popular culture and can convey a dystopian image about science. However, by being projected onto opponents of embryo research, it can serve as an interpretative package indicating these people are *irrational* to believe in science fiction.

Mulkay highlights that proponents of IVF and embryo research also drew on fictional narratives. Indeed they needed to project themselves into the future and imagine IVF (see also Mulkay, 1993). However, the projections they made were always utopian, and as there were no well-known and readily available utopian fictions with which to associate these imaginings, these were never labelled as fantasies. Therefore, the people articulating them could maintain their cognitive authority by not being associated with science fiction.

Reproductive cloning, like IVF in the 1980s, is currently in its early stages of development. It has been successful in certain animals and has never been achieved (to my knowledge), in humans. Not surprisingly then, science fiction is a common interpretative device to make sense of it. A variety of science fiction stories and characters have been drawn upon by publics, scientists and the media during discussions about cloning. These include *Boys from Brazi*l, *Frankenstein* and *Brave New World* (see Nerlich *et al.*, 2001).

Most scientists have come out in opposition to reproductive cloning (as did some scientists with regards to IVF). They were keen to distance it from the more acceptable therapeutic cloning and there were no widespread mainstream utopian discourses in defence of reproductive cloning (for an exception see Nerlich and Clarke, 2003). Kitzinger and

Williams show how supporters of SCR and therapeutic cloning, similar to supporters of IVF, used science fiction "as a rhetorical weapon to discredit the opposition" (2005: 737). Parry found during her interviews that stem cell researchers "expelled" reproductive cloners as "pseudo-scientists", and openly aligned their views with what they perceived public views to be (Parry, under review).This also occurs in more public settings such as in the media or parliamentary debates (Nerlich and Clarke, 2003; Parry, 2003a). Of particular interest here, is that, by agreeing with public views, scientists move from the discursive domain of *fact* and *rational* thought, towards that of *values* and *fears*, which they usually construct as epistemologically inferior. Ethical reasoning is embraced here, and the issues are no longer scientised: publics are "enrolled" when it comes to criticising reproductive cloning (Parry, under review).

My interviews with stem cell researchers reveal two important differences from the above findings. Firstly, my informants do not mention any of the people who claimed to want to achieve human reproductive cloning. I believe this is due to the timing of my research: when Parry did her interviews and when the above media samples were analysed, reproductive cloners had the attention of the media and were promoting people's fears about cloning and embryo research in general. However, by the time I was interviewing, all these people's claims had been dismissed and SCR had retained its aura of "good science". Secondly – and this could be due to the larger sample size compared to that of Parry and to the private setting compared to that of the media and parliament – I find more variety in my informants' approaches to reproductive cloning. Thus, although several researchers criticise publics for drawing on science fiction to judge SCR and cloning, many others also themselves draw on explicit dystopian science fiction references to make sense of their own fears. However, rather than accepting that their views could also be shaped by science fiction and "*cultural* knowledge"(that is knowledge of the social and cultural contexts of knowledge production, Kerr *et al.*, 1998b, see 2.2.2), some of these researchers try to *rationalise* their science fiction-based critique of reproductive cloning. Looking at the use of science fiction imagery and rationalisation techniques has implications for the role of *values*, and *cultural* knowledge in decision-making abound SCR and science.

7.4.1 Science fiction in scientists' discourses

Some of my informants criticise members of the public for drawing on popular science fiction to make sense of cloning. In the following quote, for example, a researcher condemns both the media and the press for associating scientists with Dr Frankenstein:

> Clara: There's lot of … bad press you know, Dr Frankensteins, so that's the other thing, I find that really annoying, this whole, like with the cloning debate, and the public issue was because you know a scientist is going to go away and clone something just because they can, and… We all have better things to do!

Here, Clara is implying that scientists are responsible people who would not waste their time on something as unsavoury as human reproductive cloning. She does not seem to think that these images of Dr. Frankenstein could be ways of expressing concerns over the potential of science to run away and lead to perhaps unintended consequences. She is steeping criticisms of SCR in science fiction, and can thus dismiss them as *irrational*.

Another of my informants also criticises members of the public for basing their views on science fiction, after I mention the frequently voiced concern that clones would not have their own identity:

> Ted: I think [clones] would have their own identity, because, I come back to identical twins, in practice, I think we're being kind of skewed by [...] The Boys from Brazil kind of scenario, of creating mini-Hitlers, you're probably too young to remember a film by Woody Alan, called 'Sleeper', have you seen that? It's probably worth watching that, because [the] grandmaster's nose [is] to be cloned to form a new one... It's ridiculous obviously but in practice, when we have identical twins, we might say, ouh, they look very similar, but we don't somehow think they are the same person, or they have the same identity, we treat them as separate people, so we are completely capable of distinguishing between genetic identity and psycho-social identity.

Ted is highlighting the idea that a person's genetic makeup does not determine their "psycho-social" identity. In other words, he condemns genetic determinism and highlights the role of nurture as well as nature in identity formation. Ted is partly blaming science fiction such as *The Boys from Brazil* for this focus on genetic determinism. Scientists project science fiction imagery onto publics, whose fears then no longer have to be taken seriously and can be dismissed. This is similar to what Mulkay (1993) describes in the 1980's IVF debates.

In contrast to the above informants and to Mulkay's findings, some of my informants expressed their concerns using science fiction imagery. In the following quote, I ask a UK adult stem cell researcher what he thought about his country being one of the only ones to have legalised SCNT. He answers by criticising other countries' decisions for being based on emotions rather than facts. He then describes his own concerns about reproductive cloning, and draws on imagined futures derived from science fiction:

> John: Yes, I think, well you have to ask why the other countries have made it illegal, you know, and it's mostly these sorts of semi-religious or emotional things about, about them being human embryos or human cells. I don't really see, I mean, I think you can now make the case that any cell of your body is potentially a stem cell, and so giving blood is no different actually from giving stem cells, so I don't really see that you know we should take a different view about these cells to any other cells, unless people are proposing to grow them up into, into live human beings, where there's a whole set of different issues that come up, but, so no, I think our legislation is sensible.
>
> Nicola: So what issues concern you about reproductive cloning then? Is it the safety issues…?
>
> John: Well at the moment it's unsafe for a start so you shouldn't do it. [...] I mean I think the first thing is safety, and unless it's absolutely safe, you shouldn't do it, there are issues of more sort of widespread issues of public health like [...], sexual reproduction actually has a function of mixing up the gene pool and if reproductive cloning became common place you could end up with a really rather restricted gene pool [...], …um, I don't terribly like the idea of having one hundred copies of some of the world's dictators, [laugh][
>
> N:]Is this Boys from Brazil?[
>
> John:]Which could easily happen[
>
> Nicola:]Do you think that? That that could happen? I mean in terms of environmental factors being important, for example, I mean I don't necessarily, I mean Dolly was different to her mum for example, and I don't know…[
>
> John:]Well we don't know actually. Um, but I think it's quite possible that a dear leader would make one hundred copies of himself [laugh].

By highlighting his approval of UK legislation, John indicates that for him scientific facts and not "semi-religious or emotional things" are what should drive decisions around science. He is satisfied that reproductive cloning is banned and gives three reasons. The first is related to safety and is often used as grounds not to reproductively clone human beings. The second was given to me by several informants and highlights the importance of having a varied gene pool for adaptation in case of environmental changes. The third is the most remarkable here. John suggests that allowing reproductive cloning could lead to the creation of hundreds of identical dictators. This scenario is, in my view, a cultural trope clearly inspired by “Boys from Brazil”. John does not see this as a fantasy scenario, in fact he seems to think that it is quite possible and “could easily happen”. When I quiz him about the role of nurture in shaping identity, he indicates that there is no clear evidence that environment would play a large role: “Well we don’t know actually”. Although John may acknowledge the cultural reference to science fiction, he does not portray his concerns about reproductive cloning as based on *irrational* fears; rather the idea that a leader would want to use cloning to make one hundred copies of himself seems to him quite reasonable. Thus, John contrasts unsatisfactory legislation or decision making around science, based on emotions and religion, with sound decisions, based on evidence and risk assessments – even if these risks are re-interpretations of science fiction scenarios. He avoids sounding contradictory by suggesting that the (potentially non-fictional) scientific storyline is realistic, based on the evaluation of available data (such as the status of current scientific knowledge or the character of current leaders) and by implication, then, is not *emotional*. So being able to explain one’s views *rationally* is important. I will come back to this in the next section.

John’s is a very genetically deterministic view, which contrasts with Ted’s (on page 191 above). Other informants such as Barry, from Australia, also draw on deterministic discourses:

> Barry: In terms of reproduction I see no real merit in having a cloned individual because it’s really only a facsimile of, genetically, of someone who’s already there.

By using the label “facsimile”, Barry is using “metaphors” of clones as “photocopies” (Nerlich and Clarke, 2003) to dismiss reproductive cloning. Thus, similarly to publics and media (Nerlich *et al.*, 2001; Nerlich and Clarke, 2003), he is drawing on cultural imagery.

The above quotes by John and Barry illustrate Gamson and Modigliani’s (1989) concept of “interpretive packages”. One of these packages could be “reproductive cloning is unacceptable”. In John’s case, “one hundred dictators” is a “condensing symbol” which has a particular cultural resonance, through its association with the storyline of *Boys from Brazil* and is thus likely to suggest similar meanings to various people who might hear it. Such condensing symbols may also play the role of “boundary objects” (Star and Griesemer, 1989). For instance, using the imagery of *Boys from Brazil* can be a short-cut to saying reproductive cloning is bad. However, people may have different interpretations of the movie itself. For example some might take from it the message that all the boys cloned from

Hitler’s cells will become like Hitler, and therefore that clones lack their own identity and are not full members of the human race. Others may assume that the book is not about cloning *per se* but is a critique of genetic determinism, a social and philosophical commentary on the responsibilities, of scientists, of society and of politicians, or a multi-faceted oeuvre highlighting the complexities of issues such as cloning. It is particularly noteworthy here that Ira Levine’s novel is perhaps unusual in that it highlights the role of nurture in identity formation. Van Dijck (1999) discusses how the complex meanings of science fiction like *Boys from Brazil* and their critique of society are often lost in media and scientists’ recasting of the stories. This certainly seems to be the case above, when Ted blames this book for genetic deterministic ideas.

Whatever the interpretations of “one hundred dictators” or *Boys from Brazil* are, having these common symbols helps these informants mobilise a strong voice against reproductive cloning. This contrasts to those like Clara, at the beginning of the section, who dismiss concerns about reproductive cloning.

7.4.2 Rationalising emotions

I have shown how some of my informants draw on culturally available images and symbols to express their concerns about reproductive cloning. In this section, I explore how they make sense of their fears, concerns and emotions, particularly with regard to decision making and legislation around SCR. Some researchers are happy to paint their views as grounded in values, fears and other cultural knowledges. Others work hard to *rationalize* their views and render them *objective* and *factual*. This follows on from Irwin and Michael’s (2003: chapter 4) discussion showing that commitments to rationalization are visible in modern society but take place in increasingly complex global relations.

In the following quotes, Peter and I are discussing how to regulate SCR. He starts by saying that it is unethical to ban a particular area of SCR (ESCR here) because of a “strong feeling” felt by certain people who are opposed to it. Then we turn to reproductive cloning, a practice against which Peter expresses strong feelings. I ask him if it should therefore be banned. He eventually says yes, but seems to have a problem with suggesting that feelings are sufficient to ban something and tries to explain his view in terms of safety. We start by talking about ESCR:

> Peter: [...]But to ban it, because a certain sub-population of the community has a strong feeling that no-one should have access to this because they don't believe in it themselves, I think that's wrong, I think that's ethically wrong.
>
> [...]
>
> Nicola: If say embryonic stem cell research went forward and somatic cell nuclear transfer was, became more safe and reproductive cloning became safe, what would you say to that? What, in the legislative point of view, in the ethical point of view, how should that be handled? [clarifications]
>
> Peter: I don't think that's acceptable.
>
> Nicola: OK. Why do you not think that's acceptable?
>
> Peter: Because you're making, well because, even though we think it's safe, if something, there's a couple of reasons. First of all, I can't see any point in making a new human for the sake of you know making a new person that's going to grow up as a person, you know, I don't see any medical, necessarily any medical benefit that couldn't be achieved in an other way. There's something, maybe I have some religion in there somewhere deep down, there's something wrong about that, it just doesn't feel right to me, I don't see the need for it and it doesn't feel right.
>
> Nicola: So do you then think it should be banned?
>
> Peter: But I don't... so it comes back to where, where do you think human life begins. I don't have a problem with, dealing with cells in the dish all the time, I don't think a few cells in the dish are, have a soul basically. I guess if they're used for a therapeutic reason, that's fine, but to make a whole new living organism that can also then pass it's genetic material on, even, mistake or no mistake, to the next generation, and then throughout the rest of mankind, and there's a potential for a tiny mistake to be passed on forever. And you know, possibilities are horrendous ethically, you know, I can think of disaster scenarios where you have a cloned person that has some, that you think is fine but has some terrible disease, some new disease, some early aging disease like Dolly or something that gets to twenty and wants, like everyone else, to have their family, all of a sudden, I know I'm talking about, maybe talking eugenics here but, you don't really want, there's no need to have that situation happen, [...] we shouldn't allow you know reproductive cloning.

For Peter, concerns based on religious or emotive grounds are not good reasons to put breaks on science. After finishing the interview, he told me that I had made him think about things he had not really considered before. I believe Peter is, here, struggling to find reasons to ban reproductive cloning. His initial "gut-reaction" (in his words) against reproductive cloning, which presumably he had not explored before, is not seen as a strong enough justification for approving of the ban on this technology. Peter finally says that reproductive cloning should not be allowed, but only after he has found and voiced concerns which are more *technical* – such as the creation of people with unknowable and undetected mutations. Here, we see two opposing voices: one arguing that decisions should be based on *rational* facts, the other explaining concerns around reproductive cloning in more *emotional* or *value-laden* language. This extract highlights how difficult it is for some scientists to accept what they see as *emotional* or *irrational* reasons for shaping technological trajectories. However, it is also visible that these emotions shape their concerns, even if they go unrecognised or unacknowledged. It seems that Peter does not have an ethics-based repertoire at his disposal.

Other researchers are more comfortable recognising their views as shaped by emotions and cultural tropes. For example, Zach says the following:

> Zach: [...] I can't see any productive reason for [reproductive cloning]… and I, I suppose I'm sort of contradicting myself in saying that if there's no obvious benefit, why should you do it when I've said there's other things that have been banned that shouldn't have been banned because of the possibility of stuff[87]… for that one, I just can't, I can't understand why you would need that sort of an aspect, it seems, and in that case, it's not actually for research, it's for people to have children along other pathways so, I don't know if it needs to be banned I suppose. I'm not really big on banning stuff, so [laugh] I just can't see the necessity for it. If that's like the final option and you've gone through absolutely every other possible way to have a child, you've really been unlucky but…it seems a very extreme way, you know, I'm just not really comfortable with the idea of people basically raising themselves, and I know it's not the same and nurture comes into it but I probably just see too much science-fi to [laugh] to separate from my way of thinking.

Zach is commenting on the contradiction between his criticism of stopping research due to *ethical* reasons and his fear, based in science fiction, of reproductive cloning. He is willing to admit that his views are shaped by his cultural environment. As a PhD student with little experience of speaking with publics, Zach appears to be more prepared than other informants to acknowledge his views as being contradictory and influenced by science fiction. He seems to be acknowledging and accepting a role for *value-based* decisions making in certain instances and does not need to explain his view *rationally*. Hence, Zach flexibly draws on two repertoires here: a *technico-rational* one, and a more socio-cultural one; he is taking a more holistic approach to making sense of his views than many other informants.

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[87] He is here referring to his opinion that SCNT and ESCR should not be banned because people have *ethnical* or religious views against them.

7.4.3 Multiple Constructions

Stem cell researchers' discussions of reproductive cloning construct engagement, publics, and scientists in particular ways. Engagement is seen by most of these informants as an opportunity for rational decisions. Some scientists criticise particular instances of decision-making around science for being skewed by science-fiction scenarios, or religious and emotional thinking. They feel the need to rectify this by highlighting the "facts" of SCR and cloning. The voice of scientism remains dominant, even if there is general support for public participation; the latter is often only seen as appropriate once emotions and values have been stripped away.

Fears are projected onto publics but are often delegitimised; this is done by arguing that these concerns are based on improbable science-fiction scenarios, religion, emotions or values. However, this section also highlights that scientists' views are shaped by a number of factors, including science fiction imagery. This is rarely acknowledged. If any momentary departures from rationality are identified during our conversations, they are usually subsequently rationalised by drawing on *technical* reasoning. This desire for rationality in discussions around science seems to stem not only from scientists' (typical) commitment to an image of science as an objective provider of truth, but also from their experiences of public engagement: the one stem cell researcher in this section who is willing to acknowledge a role, if limited, for more holistic approaches to science has little experience of public engagement.

7.5 Conclusion: multiple performances

The examination of these three issues has revealed that stem cell researchers have a variety of socially located ways of constructing science, publics and engagement. For some, public engagement requires the use of labels and "condensing symbols" to black-box particular aspects of SCR and focus on its more *technical* aspects. This scientization supposedly enables simple messages to be put across; it aims at educating publics and, so, fostering their support. It also dismisses scientists' personal attributes or interests as irrelevant. For others, engagement is an opportunity to unpack labels and look into black boxes; to delve into the various concerns people have; to examine scientists and the factors that shape their work (and maybe "expel" said scientists); and to think about the contexts in which, for example, stem cells are derived and might be applied. As a result, for some scientists there is, and for others there is not, an acknowledged place for explicit interests and values in discussions around science. Different scientists have different concerns about the role of promoting/hyping SCR.

Publics are at times painted as misunderstanding science, lacking in judgement and relevant knowledge or experience, easily duped, irrational and in need of education. At other times, and by other informants, they are described as sources of relevant knowledge, who can voice legitimate concerns and bring alternative points of view. They can be broad homogeneous groups or include a variety of people with different expertises, knowledges or perspectives, such as patients, politicians or journalists. As for scientists, they can be seen as neutral, rational, independent and objective information providers, holders of the only relevant expertise, or they can be one of many voices with their own social, cultural, ethical and political locations and related interests.

Accordingly, there are also multiple worldviews discernible, more or less reinforced by the setting in which different engagement activities take place. There is a strong modernistic worldview, with its attendant universal and predictive science which inevitably leads to progress and can be easily disconnected from the contexts of its creation and application. There are also more holistic worldviews which see science as uncertain, socially and culturally embedded, and with inseparable ethical, technical, cultural, environmental, economic (and so on) issues.

These various conceptions are tightly linked to the contexts of the discussion: the timing in which interviews took place, the locations of my informants in the field of SCR, their country of work and origin, their experience of talking in public or working with particular patients, the level of temporary stabilisation of various boundaries around SCR and the importance of the issues (such as future funding) at stake.

The existence of these worldviews has implications for engagement. It seems that the deficit model is still present, if not dominant, as is the desire to make decisions by separating values and emotions from the "facts". However, many of my informants also acknowledged the existence of theirs and others' values and assumptions, thus using more participatory or dialogic voices, and suggesting opportunities for engagement. Experience of talking with publics has had different effects on stem cell researchers. For some, it highlights the power of scientism and has taught them to focus on *technical* aspects of SCR, at the expense of taking a more holistic approach. For others, it has reinforced a desire to reveal and discuss problematic issues with publics.

These different constructions perform engagement in different fashions. Describing publics as uninformed and scientists as neutral information providers can create a reality where engagement becomes education; describing both publics and scientists as located in particular social contexts and having relevant concerns can create a reality where engagement becomes respectful conversations between a diversity of people who can challenge the usual scientistic framing of engagement. These constructions open up engagement to different sorts of participants. In the next chapter, I look more closely at these constructions and link them with broader notions of “scientific citizenship” and democracy.

Chapter 8

Ideal types of engagement and models of democracy

While many commentators portray a lack of public understanding of science as an obstacle to democratic vitality, it may be that the reverse is also true; that impoverished democracy and intensifying hegemony around science is a major obstacle to the enhanced public understanding of science.

(Wynne, 1991: 121)

8.1 Introduction: moving away from "utopian rationalism"?

Ezrahi argues that modern Western democracies have used science and its authority to legitimise their power. He finds that this ideological and political role of science decreased at the end of the 20th century due to a decrease in science's authority in legitimating public action and "a decline of beliefs in the existence of objective external reality, in the possibility of universally valid knowledge" (1990: 14). Similar to Beck and Giddens, Ezrahi puts forward science-related disasters such as Chernobyl as contributors to mistrust in science and its ability to inform political choice. He argues that many in policy, politics and social science now consider the idea of policy decisions being taken by an informed public making rational decisions, as utopian:

> the formerly normative conception of policy-making as a process of rational or informed problem solving became widely discredited on the grounds that it represented a version of *utopian rationalism* that ignored both the complex symbolic and normative aspects of collective choices, and the fundamentally political logic of public policy-making involving negotiations, compromises, and control (Ezrahi, 1990: 244, emphasis added)

In this context, science for Ezrahi is now used as a "privatized resource for the advancement of diverse particular views" (1990: 248). That is, science is drawn upon to make personal choices (about whether or not to smoke, for example). Ezrahi suggests that the dissociation between politics and supposedly "objective" knowledge is good and "perhaps the ultimate move towards the autonomy of politics in our culture" (1990: 282).

However, as we saw in the previous chapter, rationality and scientistic understandings are prominent in many stem cell researchers' accounts. Chapter 5 also indicated that discourses around science are often made by projecting images of certainty. This chapter examines how my informants envisage scientific knowledge and its role in policy and decision-making and how their views on engagement construct different types of "scientific citizenship" (see chapters 2 and 7). It asks if, as Ezrahi suggests for policy makers and others, some scientists are moving away from "utopian rationalism".

The previous chapters drew out the richness and diversity of scientists' views and understandings of science, SCR, knowledge, publics and engagement. This chapter specifically addresses my second research question, about the implications of scientists' discourses on public engagement. It starts by an over-view of some literature linking expertise, theories of democracy and performances of scientific citizenship. Then, drawing directly from interview data, it develops six "ideal types" of engagement, before concluding with a discussion on power.

8.2 Investigating democratic citizenship and expertise

The literature review (particularly section 2.2.4) argues that particular forms of public engagement construct particular types of publics. Going one step further, Michael and Brown (2000) show how accounts of engagement constitute citizens and fit into various versions of democracy. They show that some scientists portray themselves as separate from publics and recommend dialogue with some "publics-in-particular" (such as *moderate* animal activists). These scientists are deploying an "external" model of dialogue; that is, they engage in conversation with people outside the scientific community. Other scientists, or the same scientists at other times, deploy an "internal" model of dialogue where discussions around science are conducted within the scientific community; here, scientists see themselves as part of "publics-in-general" and, therefore, as holding the same fears, concerns and "commonsensical stories" as publics.

Similarly, Elam and Bertilsson (2003) explore scientific citizenship by examining the shift from science conducted in ivory towers, to calls for public understanding of science (as in the Bodmer Report) and then for public engagement with science. They draw out the models of scientific citizenship and democracy constructed by each: respectively, the Enlightenment model, where scientists, who discover "truth", are the only scientific citizens; advanced consumer democracies, where citizens need to be educated about science in order to appropriately consume the products of scientific innovation; and deliberative democracy, where a variety of citizens can deliberate and reach a rational consensus about science. They then critique the conditions of deliberative democracy for being too idealistic and argue that:

> By valuing rationality, reserve, selflessness and powers of argumentation, deliberative democracy as a democratic politics played out on scientists' home turf (Elam and Bertilsson, 2003: 242).

Instead, they argue for a more radical democracy, dismissing the idea that "power can be bracketed out of politics" (2003: 244).

Studies of democracy must therefore explore the problematic concept of expertise and the authority particular kinds of experts may exert (as suggested by Irwin, 2006: 304). The notion of expertise has been problematic in the context of modern democracies. For example, Turner argues that for some:

> We are left with a picture of modern liberal democracies as shams, with a public whose culture and life-world are controlled or 'steered' by experts whose doings are beyond public comprehension [...], but whose 'expert' knowledge is nothing but ideology, ideology made more powerful by virtue of the fact that its character is concealed. (Turner, 2001: 127)

However, for Turner, this is only a problem for liberal democracies if we assume that there is a higher "reason" or God that should direct our assessment of experts. Instead, he believes that publics can judge experts, and decide or not to give their claims the temporary status of "neutral fact". These decisions are "political"; that is "a matter of decision, not truth" (2001: 143). This granting of cognitive authority is not the same as an acceptance of extensive power over one's life, but enables non-experts to delegate, for a limited time, certain decisions to experts (for a further discussion of expertise, see 2.2).

Nevertheless, given the framing of science-public interactions (as discussed, for example, in 2.2.4), it may be difficult for publics to have the agency *not* to grant cognitive authority to experts, since existing structures often reinforce the latter's power. Wynne, drawing on SSK and development studies, argues that:

> Dominant hegemonic discourses of risk embody tacit power and cultural relations, and transmit these through their global 'scientific' status and [...] through consequent international networks of training, recruitment and accreditation. This throws into sharp relief the ways in which discourses of risk embody and project assumptions about social and cultural context, which in turn embody particular models of the salient 'public' or 'citizen'. (Wynne, 2005b: 75-6)

Wynne's argument is in sharp contrast to Turner's claim that certain forms of expertise can unproblematically be given authority, either because they have successfully been accredited within their own discipline, or because publics give them authority. As an empirical example of how problematic the granting of authority can be, Lahsen's (2005b) study of climate debates in the USA shows how certain groups have disproportionate financial and political power, and therefore influence on policy. Thus, the various calls for the "democratisation of expertise" (see the special issue of Science and Public Policy, 2003) will be difficult to realise unless this process comes in parallel with wider changes in social order and power relations (as suggested by Epstein, 1996: 352).

In addition, Jasanoff (2003a) highlights the problems not only with elitist/technocratic conceptions of expertise, but also with relativist ones. In some instances politicians, assuming that all expertise is contingent, find it appropriate to simply appoint experts who have matching political convictions to their own. Instead, Jasanoff argues that standards of transparency and accountability should be sought and this is where publics should play a role in democratic societies (see also Jasanoff, 2003b). Therefore it is fundamental to see how potential participants in democratic decision-making conceive, perform and drawn upon notions of expertise. Scientists' views are particularly important since, despite Ezrahi's claims, scientific knowledge is a crucial currency in modern democracies (as discussed in Lahsen, 2005b).

Drawing in particular on Michael and Brown (2000), and Elam and Bertilsson (2003), this chapter looks at how my informants' accounts of engagement perform specific types of citizenship, expertise and democracy. Rather than trying to make an exhaustive list of all the different ways in which engagement can be conceptualised and trying to trace this back to the complex cultural factors that come into play, it is more useful here to set out models that can be used as heuristic tools. This is done by drawing on Max Weber's concept of "ideal type" which can be:

> a mental construct for the scrutiny and systematic characterisation of individual concrete patterns which are significant in their uniqueness, such as Christianity, Capitalism etc. (Weber, 1949: 100)

As Weber highlights, an ideal type does not have to correspond to what the researcher sees as an "ideal" situation. Using this tool, my analysis reveals emerging patterns from the rich and diverse data collected on stem cell researchers. Even if one particular scientist may not exactly belong to one of my ideal types, using this tool as a heuristic device can identify ways in which citizenship, publics and democracy are co-constructed. Once some ideal types of engagement and citizenship have been identified, empirical data can be compared to them, and differences and similarities drawn out. The selection of ideal types discussed is of course not exhaustive, but serves as a starting point for further studies. Six ideal types of engagement are developed here (see Table 7).

Table 7: Six ideal types of engagement

Table legend:

→	Technical capital
+	Technical capital that leads to support and appreciation of a research endeavour
-	Technical capital that leads to the questioning of a research endeavour
+/-	Technical capital that can lead to support and/or questioning of a research endeavour
→	Political (or consumer) capital
+	Political capital that leads to support and appreciation of a research endeavour
-	Political capital that leads to the questioning of a research endeavour
+/-	Political capital that can lead to support and/or questioning of a research endeavour
→	Various types of capital: cultural, political, technical etc., without strict boundaries between each
+&-	Both + and - capital: knowledges, expertises, views etc. that support and question various research endeavours and understandings of the world

Engt. type	Engagement as:	Scientists	Engt. flow	Publics	
		Attributes		Who?	Attributes
I	Internal dialogue with scientists as publics – **Republic of science**	· Rational or try · Capital: all technical + all political	↩ +/- ↩ +/- → +/-	· Undifferentiated	· Emotional · Capital: none
II	Educating scientific consumers/citizens – **Advanced consumer democracy (Lib. dem.)**	· Rational · Capital: all technical	→ +/- +/- ←	· Undifferentiated · Citizens · Consumers	· Emotional, but can try to be rational · Capital: political (vote, consume)
III	Recruiting publics/patients to support science or lobby – **Liberal democracy**	· Rational · Capital: all technical	→ + + ←	· Undifferentiated · Patients · Politicians	· Emotional, but can try to be rational, excludes extremists · Capital: political (lobby)
IV	P.R. to keep public at bay; scientists decide alone – **New republic of Science**	· Rational · Capital: all technical + partial Political	↩ +/- → + - ←	· Undifferentiated · Funders	· Emotional · Capital: political (can stop science)
V	Constructive conversations between rational experts to make decisions; these can be informed by public/consumer opinions and views – **Elitist deliberative democracy**	· Rational or try · Capital: partial technical + partial political	→ +/- → +/- +/- ← +/- ←	· Publics-in-particular: e.g. IP lawyers, legislators, patient reps	· Rational, or try · Capital: partial technical + partial political
			→ +/- → +/- +/- ←	· Undifferentiated · Patients · Citizens · Consumers	· Emotional, but can try to be rational · Capital: very partial political (convey emotions)
VI	Constructive conversations between people with multiple identities and socially contingent knowledges to shape possible futures – **Radical/plural democracy**	· Emotional and rational · Capital: multiple and partial	→ +&- +&- ←	· Multiple, include scientists	· Emotional and rational · Capital: multiple and partial

Table 7: Six ideal types of engagement

These ideal types are not direct reflections of reality, but correspond to how my informants construct and perform different versions of engagement. Each one considers the sorts of "capital" (Bourdieu, 1975) described as relevant by my informants. Several types of capital are distinguished. *Technical capital* includes technical knowledges; that is those painted as rational and objective, usually universal, where contingencies lead to error. It also includes forms of authority typically recognised by scientists, such as publications or the ability to impose (what I would call) scientistic definitions onto discussions. This is contrasted to *political* and *consumer capital* which include the power to shape the direction of science by voting, consuming, funding, criticising etc. Engagement ideal type VI fits into a slightly different framework, where the "technical" and the "political" are no longer separated. Rather, in a manner more consistent with SSK and my other theoretical commitments, capital here encompasses a range of cultural, situated or socially constructed knowledges, as well as the ability to shape decisions in a variety of ways.

For each ideal type, I point out potential participants for engagement, the roles envisaged for scientists and publics, the understandings of "knowledge" and "expertise", the conceptions of scientific citizenship, the models of democracy and the recurrence of the model. In particular, I examine whether my informants draw on liberal democratic traditions, where publics and scientists are portrayed as individuals with rights and where citizenship is just "one identity amongst others" (Mouffe, 1992b: 237); on republican traditions where identity is subsumed under the membership to a community and where citizenship is "the dominant identity that overrides all others" (Mouffe, 1992b: 237); or on other traditions. My informants draw on different conceptions of scientific citizenship throughout one particular interview (as shown in Table 8). The recurrence of each ideal type and the social factors that shape it are discussed. There are no differences due to the country of the informants. I now turn to a detailed discussion of these ideal types.

	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	41
I										■					■										■	■				■					■			■			■	8
II	■					■					■		■	■		■			■	■				■							■								■	■		12
III	■	■					■								■								■		■		■			■			■			■	■	■				12
IV	■	■										■		■				■	■												■					■						8
V	■			■								■	■				■				■		■				■		■	■		■	■	■								13
VI			■		■		■		■				■									■						■														7
																																										60

Table 8: Distribution of ideal types of engagement amongst informants

Most informants drew on several ideal types of engagement during one interview. The blue squares mark which models were drawn on by each interviewee. Each letter corresponds to one informant, with black letters indicating interviewees from Australia, and purple letters interviewees from the UK. Further details have not been included to protect informants' anonymity. The Roman numerals correspond to the ideal type numbers. The final column indicates the total number of informants considered here (end of the top row) or the number of informants drawing on each particular ideal type. Only data from interviews is included (excluding two group interviews where the there was not enough data from all participants to classify them).

8.3 Type I – Engagement as internal dialogue with scientists as publics

	Engagement as	Scientists	Engt. flow	Publics	
		Attributes		Who?	Attributes
I	Internal dialogue with scientists as publics – **Republic of science**	· Rational or try · Capital: all technical + all political	↩ +/- ↩ +/- → +/-	· Un-differentiated	· Emotional · Capital: none

In this ideal type of engagement, scientists self-regulate because they have the necessary technical and political capital to do so. They locate themselves as part of the public and therefore any decisions about science can be made internally, by scientists, whose fears reflect those of the public. This model of engagement is expressed by five PhD students and three group leaders (from both the UK and Australia).

During an interview, I ask an informant whether he thinks that embryo donors and patients can "bring anything to the way science is done", whether scientists can learn anything from them. His answer is as follows:

> Ian: In terms of fertility clinics, I think the thing they can bring is obviously the embryo, and if they're more aware and more well educated, maybe we would sway more people to donate, or maybe it might have the opposite effect. I don't know. And people suffering from Alzheimer's, what can they bring?

When I ask this question, I am hoping for examples of experience-based expertise, such as different meanings for "embryo". But when I ask Ian about what people with infertility can "bring", he only suggests the *physical* embryo. This portrays scientists as the only ones holding relevant expertise. I then use this quote as a prompt in a subsequent interview:

> Paul: I mean, it's quite a harsh statement that they make but I think it's reasonably true. I mean, honestly what can someone with Alzheimer's or a family of someone with Alzheimer's really bring. *As scientists we know* there's a chronic need for better therapy, and we're all extremely motivated, so I don't see what they could particularly tell us that would make us do anything differently.

Scientists, such as the two PhD students above, who deploy this model consider they have all the necessary capital to make decisions around science. They have the *technical* capital to judge the promises and risks of research and its application and to know what concerns publics may raise. This is because these scientists portray themselves as part of the "public-in-general" (Michael and Brown, 2000). They also have all the necessary *political* capital to vote or make decisions in their field. They discuss no role for publics as “scientific citizens”: the provision of biological material is not seen as a *choice* related to practices of citizenship, but as a gesture made automatically, if and when the material is needed. These scientists locate themselves apart from outside influences (from publics, commercial groups, politicians etc.) and, if they have any concerns about their work, they turn to their peers. They portray the majority of scientists as more “virtuous” than others (Shapin, 1995: 404). Here, technical capital is automatically converted into political capital.

Publics are depicted as emotional. However, some are seen to share “commonsensical stories” (Michael and Brown, 2000) with scientists such as: if you accept abortion, you cannot reject ESCR. These stories can shape research through scientists’ recasting of them in rational terms. Other publics are too irrational to share these stories (for example extreme anti-abortion groups) and their views must not shape research. Public stories that are not shared by scientists can be rejected (made irrelevant to science) by being cast as not based on sound science:

> Danielle: I think that the public should be engaged, if they’re interested, in the ethical debate. I think that the public, if they have strong feelings, should be allowed to express those feelings. And I think that scientists should respect that some people may, may be very well informed and may not like what they’re doing. You can’t just say ‘Well they don’t understand the science so of course they’re not going to agree with it; if they only understood, then they’d think it’s wonderful’, because they may understand quite well, and they may still not agree with it. I think that, though, that you also need to realise that the general public probably does not have 8 years of, you know, focussed study in that arena of science [...]. I do think that the public’s opinion is important, but I think it should also, that the public should respect scientific community’s opinion is also very important, because they are the experts in this area. And so I think it’s a mix between public approval and the scientific approval. There’re a lot of people in the scientific community that don’t agree with embryonic research, I think the majority of scientists do. So I guess my opinion is that I’m, as a scientist, I’m very open to the public’s opinions and thoughts, if they’re well based in, you know, well founded, well understood [...]. If they want to have an opinion, should at least become well informed of the technology.

A central limitation of the deficit model is described here: knowledge does not necessarily lead to acceptance. However, these criticisms of science can be relegated as inferior to “scientific” views, as they are not based on “eight years of study”. Thus, some elements of the critique of deficit model – particularly the need to respect other forms of knowledge – are recognised, but only partially embraced. In particular, the last sentence indicates that the need to be well informed before being able to legitimately criticise science.

The knowledge used to make decisions around science must be rational and objective. Some a*ppropriate* views can also be included, like the commonsensical stories above, but they have to be rationalised *by scientists*. For Michael and Brown (2000), the rationalisation of concerns about animal welfare can be brought about through the experience of working with animals for a long time. In SCR, the rationalisation of fears about cloning can be through logical analysis of biological risks (see 7.4.2), or the rationalisation of the need for therapies can be through experience with patient groups. People are allowed to hold *opinions*, but not shape science.

Engagement takes the form of rational discussions between scientists, with public engagement being the incorporation of commonsensical stories. It also includes a small measure of talking to the public and "letting them know what we're doing". Although some aspects of the critique of the deficit model are raised, the importance of having an "informed" view remains. This normative position is operationalised by excluding anyone who does not have recognised scientific training, or who can be portrayed as "emotional" or "irrational". Informants draw on this model either to describe how science works in most instances, or how it should work. Informants with little or no experience of engagement seem to think that science easily can be isolated from the rest of society in this way. Informants that have experience of public engagement at the parliamentary level see a distant and exceptional role for politicians in setting legal frameworks (such as banning reproductive cloning or research on embryos) but do not accept a role for the public shaping of research any more directly.

Here, scientific citizenship is only held by scientists and, as Michael and Brown (2000: 9-10) find, democracy is only vested in particular people. I would describe this form of democracy as similar to Polanyi's (1962) concept of Republicanism, where the "Republic of Science" is a "Society of Explorers" who "strive towards a hidden reality" (1962: 67) and are independent of external pressures[88]. This Republic of Science is also identified by Rothman et al. (1996) as one of the models of science that British scientists draw upon[89]. It could also be described as a very limited type of "civic republicanism" which, according to Leach and Scoones (2005), sees individuals as part of groups that can become politically involved. One central aspect of republicanism is that it involves a community with shared values, whose

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[88] Fuller (2000) has criticised this view, especially for the idea that these "explorers" can be independent.

[89] This model is arguably institutionalised within British and Australian contexts.

members handle common concerns and interests through discussions within the community (e.g. Cunningham, 2002: 55-6). Here, the community is that of scientists.

8.4 Type II – Engagement as educating scientific consumers/citizens

	Engagement as	**Scientists**	**Engt. flow**	**Publics**	
		Attributes		**Who?**	**Attributes**
II	Educating scientific consumers/citizens – **Advanced consumer democracy (Lib. dem.)**	· Rational · Capital: all technical	→ +/- ← +/-	· Un-differentiated · Citizens · Consumers	· Emotional, but can try to be rational · Capital: political (vote, consume)

In this ideal type, engagement is aimed at providing a variety of publics with neutral information and offering them a number of options. Individuals can then choose between these, either by exercising their rights as consumers (by choosing or not to buy a product) or as citizens (by voting). This ideal type is used by informants from the UK and Australia: five PhD students, one post-doc and six group leaders.

Scientists describe themselves as objective providers of neutral information, as discussed by Kerr *et al.* (1997, see also chapter 7 above). They hold all the relevant technical capital, which is understood as “certified” expertise (Collins and Evans, 2002). Scientists do not have the right to make all the decisions about science however; they lack political capital. Their role is to give impartial advice about technical issues:

> Victor: So scientists, I don’t know if we are the best people to talk through all those issues. We’re only there for one aspect and that is can you do something, you know, what is the chance of this working. What is the chance of this going wrong, producing something that's dangerous. [...] What working in stem cells does for me, it tells me what is probably possible, what probably is going to be very hard, or it’s going to be impossible, and that’s all.

The purpose of engagement is to *inform* publics about options made possible by scientists, such as donating or not, participating in a trial or not. That is, scientists share their technical capital with publics, through education. Publics are seen as temporarily ignorant but some can be empowered to make appropriate, rational and informed decisions, once they have been educated:

> Philip: I think one of the things about this debate, is that for many of the people who are opposed to this work, these are matters of religious belief, and religious

> belief is by its definition not something that's up for lot of discussion, you know either you believe [laugh] or you don't believe. And that's kind of the end of the story. I'm not really competent to get into religious/philosophical discussion with these individuals about their beliefs, not my job, not my role. So I think we just have to accept that this, abortion is another example in which there will be a significant minority of the community that is never in consensus. And that's why we have to make a democratic decision and either move forward or not.

This ideal type is similar to the conception of engagement-as-education developed by the Association of Scientific Workers where citizens need to be taught about science in order to become democratically competent (Irwin, 1995: 10-13). Legitimate political capital comes with scientific education. However, some publics, even with the best education, may come out against science, due to their irrational religious beliefs, for example. Importantly, this does not deny them citizenship (see below).

Publics in this ideal type are not only potential voters, they are also potential consumers:

> Nicola: OK. So would you for example um, if you went to speak to patients for example, um, would you, if they had particular ideas about how treatment would be more effective, or how stem cells could be used, would you listen to that? [
>
> Gary:]Oh, I'd absolutely take that on board, because they're the, they'll be the number one consumers. [laugh] [N: OK] and they probably have a lot more idea about kidney disease than I ever would because they have to live with it every day. [N: OK] So I absolutely would respect their opinions. (1, 82)

Citizens are "consumers". This relates back to Ezrahi's argument that science is used for "private" decision-making. Citizens voting via their wallets can, of course, also influence state policy; nevertheless, science is not always assumed to speak directly to governments and justify their actions. Michael and Brown note a "blurring of the boundaries between 'citizen' and consumer'" (2000: 16). This is also discussed by Michael who notes that with the rise of the "New Right":

> It seems to be becoming increasingly problematic to separate out – to keep distinct – the practices of citizenship from those of consumption (Michael, 1998: 320).

People are seen to consume science not only because it fulfils a function and is practical, but also because knowing about science performs certain types of identities and this consumption of science has aesthetic reasons (Michael, 1998). In the above quote, the "opinions" of patients only become legitimate because they are potential consumers, making decisions about what product to consume. They have citizen rights through their consumption.

Science, here, is not an independent republic as above. Rather, scientific citizens are all sorts of *educated* publics and scientists who vote and consume. I see them as belonging to what Elam (using the Bodmer report as an example) calls an “advanced consumer society”:

> Scientific citizens are imagined as participating in a different type of consumer democracy. Unlike conventional consumers of established products with clearly defined characteristics, consumer-citizens confronting the novelties of science-based innovation are unavoidably individuals with incomplete information, who are being asked to pass judgement on things that literally no one can claim to fully know or understand. The challenge of scientific citizenship is, therefore, one of political decision-making under conditions of exceptional uncertainty. (Elam and Bertilsson, 2003: 239-40)

Unlike Elam and Bertilsson’s conception of this advanced consumer society, I find no acknowledgement these uncertainties in my informant’s accounts. Rather, the education process is seen as a simple process of giving objective, clear, universal knowledge. This is a crucial point in the context of public engagement to which I return in the concluding chapter.

In this ideal type, there is a recognition that scientists cannot make all decisions about science alone. This is in sharp contrast to model I, where scientists have all the necessary capital for decision-making. There is also an idea that citizens have rights in decisions around science because they pay taxes, and therefore partly fund science. Neutral information is, nevertheless, necessary here before any decisions can be made. The State is seen as benevolent and citizens are able to act rationally – by supporting science once they are educated about it. People “hold faith in the modern States expertise” and science is the main vehicle of authority and expertise (Leach and Scoones, 2005: 22). Further, society is a “market structured network of interactions among private persons” who are trying to advance their “private interests against a government apparatus specializing in the administrative employment of political power for collective goals” (Habermas, 1996: 21). This engagement ideal type accordingly reflects a notion of “advanced consumer society”, which is, in my view, based on liberal democratic notions that highlight the importance of individual freedoms (Cunningham, 2002: 30).

8.5 Type III – Engagement as recruiting supporters

	Engagement as	Scientists	Engt. flow	Publics	
		Attributes		Who?	Attributes
III	Recruiting publics/patients to support science or lobby – **Liberal democracy**	· Rational · Capital: all technical	→ + + ←	· Un-differentiated · Patients · Politicians	· Emotional, but can try to be rational, excludes extremists · Capital: political (lobby)

In this ideal type, engagement involves educating publics about science in order to recruit them into supporting it and perhaps lobbying politicians and other decision makers on behalf of scientists. The notion that education leads to support was drawn upon by many of my informants in both countries (explicitly by twelve) and the strategic use of patients or public groups to convince politicians or other decision makers was explicitly made by four of these informants.

Scientists who draw on this model portray themselves as objective and with the necessary technical capital to predict the best future for science and society. However, they view themselves as lacking the necessary political capital to make decisions around science; these are made by funders and politicians. In addition, scientists cannot easily convert their technical capital into political capital:

> John: [...] it's quite hard for scientist to lobby on their own behalf, because it's seen as self-serving and it's much better and much more effective in many ways to the patients to lobby for themselves, so you're generally lobbying you know for a group of patients that you're trying to cure, um and you think that you're doing a procedure that would help them, it's actually better to put the patients up there than to put yourself up there generally.

Here, John argues that for scientists to be seen as promoting their interests can challenge their neutrality; in turn, this may diminish their technical capital as well as their political capital. Indeed, to maintain their status as experts and their position in the scientific field – that is their technical capital – but also their status in the political field as objective fact producers, these scientists have to be seen as *neutral* (see also Jasanoff, 2002).

As in the first two ideal types, publics are seen as ignorant and emotional, but some have the political capital to convince politicians and others about the promises of science. For example, some scientists feel that having community support can be politically very useful:

> Terence: So politicians, you know people who make the law, listen to these people so there is a very strong persuasive sort of argument or case, therefore a strong level of support that can be provided in the right direction by these individual groups. So in a way it would be too narrow minded to go forward thinking that you don't necessarily have to involve these people because they are not informed; [that] they will just rock up to the clinic and accept what therapy is developed at that moment in time. That is not going to be the case... You know because, no matter how much they are going to be long term dissatisfied with their current treatment, if they don't really understand what is happening, they won't take their clinicians' word for granted. And they won't you know accept or sign up to have any kind of experimental therapy which would involve stem cells.

Often, publics-as-patients are seen to carry more weight, have more political capital, than other publics. Once they have been appropriately educated with technical information, they will inevitably, if they are rational, support science.

One informant argues that "the more education that people receive, the more liberal[90] they're prepared to be, because they understand the issues better". I then raise some cPUS studies going against this assumption. He replies by drawing a parallel with debates around genetically modified organisms (GMOs):

> Barry: I mean the GMO debates are a classic, because I think generally the claims that GMOs are so dangerous is nonsense, yet it is one of the most contentious issues, and the one that the community is probably least supportive of, and so here's a reality grab that it [education] doesn't work, and the more you educate some of these people, the more critical they become. It doesn't mean to say that the process ought to be thrown out but, I think it's helpful to know that the scientists and other people are willing to engage in discussions and debates.

This informant does not go on to suggest that engagement should be abandoned or modified (as others do, see model IV below). However, Barry draws a clear distinction between, on the one hand, scientists and other rational and acceptable citizens and, on the other hand, "some of these people", who even when educated remain critical, and are by inference, irrational. This paints scientists and some publics as having educated intelligent discussions and trying to progress science, whilst, unfortunately, some irrational people try to prevent this. The limits of deficit model-style education only seems to apply to extremists here. This implies that for most people, scientific education leads to support of science in general.

Knowledge is depicted as objective, and it is assumed that more of it will increase support for science, echoing the traditional deficit model of public understanding. However, two

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[90] This informant uses liberal in the sense of giving people (scientists) freedom to do as they wish.

types of publics are pictured. On the one hand, there is a majority public which can be educated and shown the promise of science, and who holds a worldview compatible with science as a vehicle of progress. On the other hand, there is a minority public, such as religious groups, that will not be convinced by education and whose worldviews contrast with those of scientists[91]. The latter do not have the minimum requirements to count as appropriate publics as they cannot enter rational debate and are not amenable to being enlightened.

There are two main distinctions between engagement ideal types II and III. Firstly, in type II, especially in Philip's quote, even if people have irrational views, they can legitimately participate in democratic decision-making around science. By contrast, in type III, legitimate citizenship is more restricted and anyone who is irrational, and for whom education does not lead to support, is excluded. Secondly, and consequently, type III draws on the assumption that more public knowledge implies more public support. Thus, these legitimate citizens will use their political capital to *support* science. In a circular manner, if they do not support science, they are *not* legitimate citizens.

One particularly relevant sub-group of publics is patients. The notion that they have more political capital than scientists was specifically expressed by group leaders with engagement experience. Thus, experience of engagement can lead to scientists becoming more aware of strategies to improve their position – here, by using patients who support their cause.

Similar to ideal type II though, ideal type III draws on liberal perspectives of citizenship. The State is seen as benevolent but can only make the correct decisions after having been educated by science, via messengers such as patients.

8.6 Type IV – Engagement as a public relations exercise

	Engagement as	Scientists	Engt. flow	Publics	
		Attributes		Who?	Attributes
IV	P.R. to keep public at bay; scientists decide alone – **New republic of Science**	· Rational · Capital: all technical + partial political	↩+/- → + - ←	· Un-differentiated · Funders	· Emotional · Capital: political (can stop science)

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[91] These are similar to Michael and Brown's (2000) extreme animal activists.

In this ideal type, scientists should ideally be left alone to self-regulate as they have all the necessary technical capital to do so. However, irrational public fears can go against scientific progress and need to be *managed* through engagement, which involves projecting a good image of research. This model was not central to most interviews, but it was suggested by eight researchers, who worked in both countries, at various levels of seniority.

Scientists who draw on this ideal type portray themselves and their colleagues as objective and endowed with all the necessary technical capital to set agendas around science. This includes knowing what is right for society. Similar to engagement type I, scientists consider themselves responsible enough to self-regulate, with the internal workings of science ensuring that fraudsters and pseudo-scientists are not given free rein. This view of scientists is exemplified in the quotes in section 7.4.1, where scientists are assumed to have better things to do than clone human beings.

Publics are portrayed as irrelevant to science but nevertheless able to counter progress if not effectively managed. Different informants focus on a variety of specific publics, depending on who they have had interactions with. Particular publics that my informants want specific attention paid to include: funders, who hold the purse strings and therefore need to be shown promising results if they are to continue investing their money in a particular project; members of ethics boards, who need to be won over to permit research (see for example the discussion on clinical trials in chapter 6); or the media, who have a strong influence on public opinion and, therefore, need to be given the “right” stories to prevent widespread unpopularity. Thus, all these publics have political capital that can go against science: they can mobilise existing modes of communication and power structures to slow science down.

Engagement involves projecting a particular image and should therefore not be done by inexperienced researchers:

> Nicola: I just want to talk about public engagement. In terms of your students, do you encourage them to talk to lay groups and things like that, or do you think that it’s something that you should do later in your career?
>
> Rachel: No, when you know what you’re talking and you’re good at communicating. I think the students are still developing their communications skills, it’s like you take them to conferences and things like that, but actually speaking to, because they do represent your group as well, you have to remember that [laugh] and I think you do have to portray a certain message to the general

> public that isn't too complicated, so I would do that instead of my students, they should be in the lab writing their thesis [laugh]!

I ask the same question to various other group leaders. They all either say that they encourage their students to talk to publics, or that they didn't encourage them because spending time away from the laboratory could be detrimental to their career (in terms of writing publications). Rachel is the only researcher who states that engagement is about "portraying a certain message" and "representing the group". During the course of our interview, she is also very critical of the effect that animal rights activists have had on research, claiming that the additional paperwork now required inhibits research. This indicates that experience of public influence on research can lead certain people to be more strategic about engagement and reduce their willingness for dialogue.

Another researcher, a post-doc with no experience of engagement, is initially very supportive of engagement, in the sense of educating publics. However, after I suggest that education does not necessarily lead to support, he seems to change his mind:

> David: I think it's still important to go and talk to people. It does sort of surprise me, I thought the more, if you didn't have any religious objections, then the more you knew about an area, the more supportive you would tend to be about it. So from that point of view it is surprising. But I don't think that would be a reason to not talk to people about science. Probably you have to be a bit sensitive about what sort of things you talk about, and the way you talk about them, so maybe that's a reason to think about the way the story is delivered, rather than trying to keep things quiet.

Here, the critique of the deficit model is re-appropriated to turn engagement into public relations. The quote by Philip about the necessity of selling SCR based on its (potentially unrealistic) promise as a therapy rather than a tool (see 7.3.2) is another good example of how strategic engagement can become.

Not all scientists who draw on this model are necessarily comfortable with it. They tend to blame the need for hype on the contexts of research and engagement which might dictate the need to strategically select the sorts of information made available to particular publics. As I discussed in chapter 5, the exclusion of uncertainty from public representations of science is often seen as necessary.

This ideal type is similar to ideal type I, but it draws on lessons learnt from experience of public talks and from cPUS work. I call it the "*New* Republic of Science". Scientists who draw on this model seem to long for the independence of science from politics and publics, but have learnt that they need money and to be strategic about their engagement. In this model, there is no pretence that scientific knowledge is neutral and should be provided to people who can then make democratic decisions (as in model II). Instead, people need to be given the “right” kind of information so they can make the “right” kind of choices and not impeded science.

Increased public knowledge, here, is seen as potentially dangerous for science. Both positive and negative technical aspects of research – promises and risks – can be discussed within the Republic of science (including the risks of cancer and teratomas, the numbers of eggs and embryos potentially needed, and various other uncertainties). However, only *positive* knowledge should leave the Republic. That is, public engagement involves telling people about the promises of science, not raising potential problems or uncertainties. Specific knowledge is selected for dissemination in order to create public support, as opposed to model III, where any increase in legitimate citizens’ knowledge is assumed to command support for science. Thus, rather than excluding irrational people from *bona fide* citizenship as in model III, in model IV, citizens from outside the Republic are *managed* through lip-service and public relations exercises. This renders any citizenship outwith the Republic of Science rather meaningless.

8.7 Type V – Engagement as mixing elite expertises

	Engagement as	**Scientists**	**Engt. flow**	**Publics**	
		Attributes		**Who?**	**Attributes**
V	Constructive conversations between rational experts to make decisions; these can be informed by public/consumer opinions and views – **Elite deliberative democracy**	· Rational or try · Capital: partial technical + partial political	→ +/- → +/- +/- ← +/- ←	· Publics-in-particular: e.g. IP lawyers, legislators, patient reps.	· Rational, or try · Capital: partial technical + partial political
			→ +/- → +/- +/- ←	· Un-differentiated · Patients · Citizens · Consumers	· Emotional, but can try to be rational · Capital: very partial political (convey emotions)

Scientists and other experts or "publics-in-particular" (Michael and Brown, 2000) decide together on the course of action in a rational way. They can be aware of, and interested in, broader public views. Thirteen scientists – from both countries, of all areas of seniority and working in all areas of SCR – appealed to this ideal type.

Scientists who draw on this model describe themselves as holding partial objective knowledge, but as needing help from others to make decisions about science. They belong to a particular community with a set of expertises and power resources, but see others as relevant to science. Technical capital, albeit encompassing various types of knowledge and symbolic power, is shared amongst different groups.

This other technical capital is held by "publics-in-particular" (Michael and Brown, 2000): intellectual property (IP) lawyers, investors, clinicians and the like, who have their own specialist knowledge. My informants suggested a variety of relevant "publics-in-particular" or stakeholders when my questions prompted them to do so. For example, in Australia, I used as a probe a quote from the NHMRC (National Health and Medical Research Council) about their review of the SCR legislation:

> The reviews will involve consultation with the Australian, State and Territory governments, relevant agencies and a broad range of *persons with expertise* in or *experience* of relevant disciplines and will also involve an extensive public consultation phase. (NHMRC, 2003, emphasis added)

I ask my informants which experts and relevant knowledges they would call upon. In the UK I use several probes. In all cases, I ask if there is a role for people other than scientists in discussions around science. Answers differ depending on my informants' social location. For instance, informants who have experience of setting up companies acknowledge the expertise of business people and patent lawyers, or scientists involved in clinical trials acknowledge the expertise of clinicians and biotech companies. The following clinician/research scientist talks about the mutual learning that can occur between clinicians, researchers and people from the commercial world, through, what he calls, a "constructive dialogue":

> Ted: For scientists, driving forwards the field is what counts, for a medic it's treating patients which counts, for somebody who's trying to run a private sector organisation, it's the profits which count at the end of the day. [...] But of course none of those is mutually exclusive: if you're a doctor, you still have to think about money from time to time; if you're running a private organisation, you do have to think about the science and its application, you can't just make money if

> you see what I mean. They're overlapping worlds; the emphasis tends to be slightly different [...]. So it's important to understand that if you're going to have a constructive dialogue.

Another informant, when talking about clinical trials, suggests it might be relevant to have the input from experts in community views:

> Riana: Take for example, say the use of foetal tissue, whether that was appropriate or not. From my perspective I see the patients, the disease, so I have a close understanding of the nature of that disease, its incurable nature and the lack of any other therapy, so that's one perspective. I also have a perspective from the scientific point of view, in that I know the literature and I've seen, I've been involved in studies which show that that particular approach in a laboratory context can be very useful, and I have a detailed understanding of that. I have a detailed understanding of the problem but what I don't have, probably the right perspective, I still have a perspective but it, I haven't done any work which samples what's acceptable in the community and how it impacts socially. Obviously I understand from my local interactions but I don't have a special knowledge, I have a layman's knowledge from that point of view.

This informant is locating herself as an expert in certain aspects relevant to clinical trials and research, but lay in others (cf. Kerr *et al.*, 2007). This suggests she sees a role for social scientists' "expertise" in discussions around science.

Not all publics, however, become licensed for deliberations about science. Emotional publics, such as "individual patients" must not be included in rational decision-making:

> Nicola: Talking about your discussions with the [pharmaceutical licensing body] and finding a solution that's acceptable to everyone, do you think there is room for talking to other people than politicians and scientists? Patients, ethicists, different other groups...? Do you think they have a role in those discussions?
>
> Stanley: Ummm... good question. I think ethicists for sure. I think that's very important. We don't, certainly on the body I sit on, there's no real consultation with ethicists as part of that, [...] but I think ethicists have a major role to play. Patient groups, individual patients, no; but I think patient groups where, and the reason I say that is obviously because it's very difficult when you get impassioned pleas from you know an ill patient, or a relative, you know it's very difficult. But when you get coordinated approaches from a knowledgeable body, and I think like the Juvenile Diabetes Research Foundation in the States is a classic case in point, very effective lobbying, very influential, that's excellent. So I think more of that would be very good.

In consequence, there are two groups of publics: those with relevant expertise and those without. To qualify as a potential "public-in-particular", people must convert their capital into, or have, capital recognised by scientists (this can be through a process of "expertification", see 2.2.3). One of the essential criteria to qualify as a public-in-particular in this model is rationality: one's contribution needs to be presented as rational in order to be acceptable.

The tacit model of dialogue drawn on here is "external" (Michael and Brown, 2000), where decisions are not made internally, by scientists within their own community but acting as publics; rather they are made externally to the scientific community, in discussion with other experts. Thus, engagement involves discussions with *elite* groups. For Michael and Brown, the purpose of these "external" discussions is to *educate* these publics-in-particular (see especially 2000: 5-6). By contrast, in my version of "external" discussions, the purpose is to *share* expert knowledges. Accounts such as these echo what Collins and Evans (2002) advocate when arguing that people with relevant technical expertise can shape research. This means that each expert group contributes its particular technical capital – such as how to derive a cell line, what sorts of inventions are patentable, or how to get funding – and have the political capital to push that decision through. Scientists alone do not consider that they have the capital to decide what research is appropriate for society.

However, following an enumeration of various "experts" relevant to science, a PhD student added:

> Gary: Getting everyone's feedback I think is always useful, um, whether or not you want to take that on board is another [laugh], another issue.

Thus, although the notion of engagement as talking with other elites was drawn upon by a variety of informants, it is unclear how much these other expertises are accepted in practice, particularly if they deeply affect the way science is done.

This ideal type of engagement corresponds to an example of deliberative democracy, but is much more elitist than many advocates of this democracy would like to see it. In particular, the processes of this engagement model are similar to processes found in deliberative democracy, but criteria for participation are quite restricted. Deliberative democracy is an extension of civic republicanism (Cunningham, 2002: 54). It can be used to describe a relatively broad range of citizen interactions, but the type of deliberative democracy that my informants perform most closely resembles Habermas' conception of it, also called "discourse theory of democracy" (Habermas, 1996). In this type of democracy, decisions are reached through processes of education from experts, and through deliberation with peers (Goodin and Niemeyer, 2003). However, for my informants, not everyone can be educated;

here, deliberation and education occurs between peers who are all “experts” of a kind acceptable in scientistic societies.

Although never implicitly invoked in my data, this model of citizenship draws on Habermas’ “ideal speech situations”. This notion has been criticised for its “naivety about the politics and power relations of such encounters” (Leach and Scoones, 2005: 25). Benhabib, a strong supporter of deliberative democracy suggests that

> within discourse theory each individual has the same symmetrical rights to various speech acts, to initiate new topics, to ask for reflection about the presuppositions of the conversations, and so on. (Benhabib, 1996: 78)

She argues that, although this can be seen as utopian, it does explain some democratic practices which are already taking place (1996: 84). Relating these theories back to my data, stem cell researchers describe interactions (such as encounters between researchers and institutional ethics boards) where ideal speech situations may indeed be attainable. However, to be an individual who qualifies for these interactions, one already has to be in a particular location in the political and power relations and have recognised expertise. This is where the *negotiation* of the legitimacy of expertise becomes very relevant. It is done through examining people’s credentials and by excluding certain views by portraying them as “emotional” (see 7.4.2).

Like engagement ideal type III, citizenship in type V is limited to people who fulfil particular criteria and hold a worldview consistent with scientific “progress”. Unlike models I and IV, where no-one outside the Republic of Science can bring anything good to scientific research, and unlike models II and III where publics only hold political capital, in model V, there are those outside the scientific community who hold relevant technical capital. Scientists are willing, at least to an extent, to build on these alternative expertises. In this way, they can depict themselves as reflexive and willing to include others’ in decision-making around science, whilst at the same time, excluding views they judge inappropriate by refusing to assign them “expert” status.

8.8 Type VI – Engagement as upstream mixing of situated knowledges

	Engagement as	Scientists	Engt. flow	Publics	
		Attributes		Who?	Attributes
VI	Constructive conversations between people with multiple identities and socially produced knowledges to shape possible futures – **Radical/plural democracy**	· Emotional and rational · Capital: multiple and partial	+&- → +&- ←	· Multiple, include scientists	· Emotional and rational · Capital: multiple and partial

Scientists and a variety of publics hold diverse socially contingent knowledges that can be used to shape the future direction of research. This model was only used by seven informants, including PhD students, post-docs and group leaders, from the UK and Australia.

One of the most striking differences between this ideal type of engagement and the five others discussed, is that scientists, here, do not portray themselves as totally objective and rational, with any departure from this used to account for errors (Gilbert and Mulkay, 1984). Rather they describe themselves as having interests:

> Simon: We like to think of the Holy Grail of Science today being evidence based medicine, but, [laughs] it's the evidence that you're prepared to accept that influences your medicine.

These scientists argue that they cannot dissociate their science from their other views and ethics. For them, "science is inherently political" and it is done by people with their own interests, biases and commitments. Some argue that these interests need to be acknowledged in order not to interfere in decisions where they are not appropriate. These scientists draw on, and recognize, a diversity of fragmented identities; such as researcher, mother, relative of a sick person and so on.

Publics are seen as multiple and include scientists. Examples are: highly informed patients, patients who want no say in their treatments, people who have no problem donating tissue or embryos, people who only want to donate certain tissues, scientists who have never entered a fertility clinic, people in wheelchairs who worry about their carer not coming on time or people in wheelchairs who have heard too many unrealised promises. Knowledge is depicted as non-universal and based on life experience. For me, this implies that these informants accept it as “situated”, “contingent” or “socially constructed” (although these exact expressions were not used).

These scientists do not believe they can, or should, self-regulate. Engagement involves the upstream shaping of science (including future directions of research and the set-up of clinical trials) by diverse people sharing their situated knowledges during constructive conversations. For example:

> Nicola: So do you learn anything from meeting the patients?
>
> Albert: Yeah I think you, I suppose yes, apart from a sense of urgency, which you kind of have, and you might say “yeah I think it’s a good idea” but when you meet people, it gives you a greater sense of urgency. But spinal cord injury for example is a good example, most neuro-scientists say [...] the goal in spinal cord injury research would be to have somebody to get out of their chair and walk. [...] On the other hand, people in wheelchairs will say, let’s say paraplegic first, if people with paraplegia say what they want, first is bowel and bladder control, then they want sexual function, then they’d be looking for sensation of movement. [...] But that’s not on the horizon of neuro-scientists normally. And if you look at all the research in the literature, very little of it is looking at those aspects, and I think that's something that I’ve learned as a neuro-scientist coming in and we’re doing some more work in that area, in autonomic nervous system. So you can learn stuff.

This clinical researcher is giving a specific example of how he changed his research priorities after meeting particular patients.

Stemming from scientists’ acknowledgement that their knowledge is situated, the criteria for citizenship and participation in decision-making around science are different here from all the ones discussed above: one does not have to present one’s contribution as based on rational and objective facts, and on expertise certified through degrees in science for example (this is a main difference between engagement ideal types V and VI):

> Nicola: In terms of expertise, I mean obviously you, people who were involved in stem cell research are experts in that, but do you think you know, for example, patient groups and infertility groups, so they have a form of knowledge or something you could label as expertise, a special relation or something, important opinion that could be important in the debates?
>
> Heidi: O, absolutely, infertility groups clearly because they understand what it is to make that decision of donating embryos, people, patient groups, people with the disorders that everyone's talking about trying to cure, because they understand what it is to maybe be promised therapies and to go through those sorts of treatments, or the need for them.
>
> [...]

> Nicola: Some scientists I've spoken to have this idea that we have the facts and the truth, and [
>
> Heidi:]Nobody else knows anything? It's incredibly patronising isn't it? Don't you think? [...] I mean I hear that a lot and. There's a Wellcome Trust study that came out, and I must look it up because whenever I'm at these public sociologist debate, partly I don't go because they make me really annoyed, but I want to get up and say this, there's a Wellcome Trust study, and OK, this is the UK, but they went around asking people what they felt about different sciences and they questioned them to what they knew, and it turned out that the people who knew the most, and understood the science, were the more against it. So whenever I hear scientists say if they could understand the science, we need to teach them science, or it's because they're ignorant and don't understand what a stem cell is, and that's why they're against it... It's an incredibly patronising viewpoint. It's like, and even if they don't understand the science and they still have an opinion, it's still, it's everyone's, that's everyone's right to, I mean I probably don't understand the nuances of the Iraqi war, but I have a hugely strong opinion about that, and I would campaign against it. I just don't understand why people have that view that they're any different. Don't you think? I don't know.

Here, it is clear that Heidi is aware of a study contradicting the deficit model. Rather than using this to justify more cautious engagement (as some informants do in section 8.6), she uses it to criticise arrogant and “patronising” people who draw on this model. She advocates action, such as campaigning, based on strong opinions rather than specific technical knowledge. Another informant also highlights that certain decision making bodies, such as ethics committees, can be too elitist.

The conception of citizenship drawn upon here is similar to Turner’s conception of expertise: it is not given by a higher order – such as Reason – but negotiated politically. One does not need to appeal to rational thought in order to become a legitimate participant in discussions around science. I would argue that this engagement ideal type draws upon a notion of citizenship as “practiced engagement”:

> A perspective on citizenship as practised engagement of social solidarities [...] allows for the possibility of global citizen action but in ways that are often contingent, fragmented and diffuse, emerging through the expression of aspects of people’s global and local identities. Rather than recourse to the establishment of global institutions to guarantee global citizenship rights, these are claimed and might be institutionally supported through more diverse actions linked across different sites. (Leach and Scoones, 2005: 34)

This is a useful way of thinking about scientific citizenship, particularly in the context of globalisation, which “renders theories of citizenship situated solely within the context of nation-states (whether liberal or civic republican persuasion) as highly limited” (Leach and

Scoones, 2005: 34). Engagement model VI is the only one where multiple people can legitimately challenge the framing of science and public engagement in science.

This ideal type draws on features reminiscent of "radical and plural democracy", which involves going beyond the individualism of liberal theories of democracy and beyond the homogenising concepts of civic republicanism (Mouffe, 1992a: 5). Radical/plural democrats recognise the "precariousness of identities" (Mouffe, 1992a: 10) and argue for the need to think about identities in a non-essentialist way, with the "social agent" as:

> the articulation of an ensemble of subject positions, constructed within specific discourses and always precariously and temporarily sutured at the intersection of these subject positions (Mouffe, 1992b: 237).

They also support "a collective identification with a radical democratic interpretation of the principles of the liberal-democratic regime: liberty and equality." (Mouffe, 1992b: 236). There are multiple ways of interpreting these principles:

> To the idea that the exercise of citizenship consists in adopting a universal point of view, made equivalent to Reason and reserved to men, I am opposing the idea that it consists in identifying with the ethico-political principles of modern democracy and that there can be as many forms of citizenship as there are interpretations of those principles. (Mouffe, 1992b: 237)

This also chimes well with Turner's conception of "expertise".

Few of my informants, I am sure, would recognise themselves as radical/plural democrats. However, I did find that their portrayals of scientific knowledge as contingent and their description of scientists' and publics' multiple and fragmented identities was consistent with the radical/plural vision of democracy. Here, scientific citizenship is performed through participation in the creation of agendas for science. However, many of the scientists who drew on this ideal type also highlighted the current need to, at least rhetorically, appeal to reason and rationality[92]. Thus, many of my informants were of the opinion that situated knowledges, if acknowledged as such, are not a powerful currency in decision-making. Consequently there is a need to reflect upon the power relations at play here. This is a central feature of "radical and plural democracy", to which I return in the conclusion of this chapter.

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[92] Witness, for example, the "weakening" of ones argument once religious opinion is imputed, as discussed in 7.2.1.

8.9 Conclusion: power and emancipation; inroads towards broader "engagement"?

This chapter has explored the types of engagement performed by some stem cell researchers. It focuses on notions of expertise, capital and theories of democracy, and develops six ideal types of engagement implied by my interview data.

Michael and Brown's (2000) two models of dialogue are supported by some of these data. Ideal type I corresponds to their idea of an internal dialogue, and ideal type V (mixing elite expertises) corresponds to an external dialogue. But there are various other ways of portraying the relation between science and publics, as developed in my other models of engagement.

Ezrahi (1990) notes that objective knowledge is no longer directly used to guide policy. Scientists who portray engagement as an internal dialogue between scientists as publics (ideal type I) would disagree. Rather, they argue that decision- and policy-making roles should belong to scientists themselves. In addition, if in ideal type II (educating consumer/citizens), "public" is taken to refer to politicians, we also see the direct influence of science on policy. However, if the "public" for this second model of engagement refers to other consumers/citizens, then the influence of science on policy is no longer direct; rather, it is made through the private choices of these citizens. This is more akin to the role Ezrahi sees for science today.

In all but model I, scientists acknowledge that the "best" science is not always enough to justify decisions. In engagement models III (recruiting publics) and IV (PR), the political capital of people such as patients and animal activists become crucial in influencing decisions around what sorts of research can go forward. In models V and VI, other expertises and knowledges are seen as equally legitimate and relevant as scientists' in decision about the future and framing of science. In these four models, science alone is not seen as enough to shore up policy and political decisions, confirming this aspect of Ezrahi's argument.

My informants draw on various conceptions of expertise. In ideal type I and IV (PR), scientists argue that decisions about science *should* be made internally, by scientists who have all the necessary knowledge. Expertise is not problematic for this vision of democracy as limited Republicanism because those who are experts are those who belong to the

Republic of Science and should be making decisions. Here, technical capital *should* automatically convert into political capital. Expertise is “certified” (Collins and Evans, 2002) through years of study and the acquisition of diplomas or the publication of papers. It is reified and technical knowledge of science implies the ability to make decisions about science and society.

In ideal types II (educating consumer/citizens) and III (recruiting publics), expertise is not considered a problem either. Although it is held by an elite minority who do not have the power to make decisions alone, it is possible to “educate” decision-makers with the appropriate knowledge. As a result, certain publics can be taught to make the “right” – rational – decisions by learning the relevant knowledge from neutral experts. Relevant knowledge in these two models is reduced to scientific/technical expertise. For ideal type V (mixing elite expertises), however, the pool of relevant knowledge is widened and expertise would be considered limited if it only included scientific/technical knowledge. Here, other sorts of technical/rational expertises are included such as in patent law, commercialisation or professional social sciences. All these expertises have to be “certified” by belonging to a recognised (usually professional) body.

In ideal type VI (engagement as mixing situated knowledges), expertise is only problematic to democracy if it is reduced to technical expertise. Rather, it can be incorporated into democracy if it is seen as political, that is negotiated, situated and temporary (Turner, 2001). This is the only ideal type of engagement that indicates a move away from “utopian rationalism” (Ezrahi, 1990). However, scientists that draw on this model do not portray it as widespread in practice.

Thus, this ideal type brings us back to the problem of power, since this version of expertise does not hold much sway in most decision-making fora. If even scientists, generally recognised as having more epistemic authority than most (Gieryn, 1995; Shapin, 1995), find it difficult to win arguments by appealing to versions of expertise and citizenship as politically negotiable, what chance does that leave to other publics? Maybe inroads to more inclusive engagement could come through working with the radical/plural notion of democracy, which recognises power differences as central to political interactions:

> In coming to terms with pluralism, what is really at stake, is power and antagonism and their ineradicable character. (Mouffe, 1996: 247)

> The main question of democratic politics, then, becomes not how to eliminate power but how to constitute forms of power that are compatible with democratic values. (Mouffe, 1996: 248)

As part of this recognition, there is a need to acknowledge that anyone's claims are situated, and not objective and universal (1996: 247-8), as some of my informants, drawing on ideal type of engagement VI, do.

The project of "radical and plural democracy" also calls for the abandonment of appeals to higher orders such as Reason or rationality:

> Such a project recognizes that the specificity of modern pluralist democracy – even a well ordered one – resides not in the absence of domination and of violence but in the establishment of a set of institutions through which they can be limited and contested. To negate the ineradicable character of antagonism and aim at a universal rational consensus – this is the real threat to democracy. Indeed this can lead to violence being unrecognized and hidden behind appeals to "rationality", as is often the case in liberal thinking, which disguises the necessary frontiers and forms of exclusion behind pretences of "neutrality". (Mouffe, 1996: 248)

Thus, for truly inclusive engagement, there is a need to bring to light power differences. The other ideal types of engagement developed here hopefully furnish us with tools to start doing just this. We must problematise people's calls for more "engagement" and examine what they mean by this. In particular, it is necessary to problematise calls for deliberative democracy, which reinforces scientists' cognitive authority (Elam and Bertilsson, 2003) and permits the exclusion of any voices painted as "irrational". This may be one of the reasons for the limited successes of some engagement exercises which seem to reproduce existing power differences (Kerr *et al.*, 2007). This chapter suggests a role for social scientists in harnessing and creating spaces in public engagement for accounts, such as those found in engagement type VI. There are also other ways of finding and promoting scientists' reflexivity, as I discuss in the concluding chapter

Chapter 9

Conclusions:
Scientists, social science and opportunities for reflexivity

Stimulated by what the House of Lords termed a "crisis of trust" between science and society and by calls for better or more public engagement in science, this thesis has focussed on one particular area of science – stem cell research – and its practitioners, institutions and interactions with publics. It develops a micro-level empirical exploration and an in-depth analysis of stem cell researchers, and their discourses about their work and public engagement. Drawing mainly on interviews with over fifty researchers, it expands the limited body of work which combines a cPUS framework with an SSK-informed study of scientific institutions, scientists, and their embedded assumptions about public engagement. It achieves not only academic aims, by contributing to a number of inter-disciplinary literatures, but also political aims, by offering suggestions for improved science-public relations.

Reflexivity is an important analytical thread that runs throughout this work. This concept has been the focus of a number of debates in academia, with the quest for greater reflexivity at times resembling a game of "epistemological chicken" (Collins and Yearley, 1992). Following Lynch (2000), the present work does not position reflexivity as either the "academic virtue" *par excellence,* or a "source of privileged knowledge". It does nevertheless argue for reflexivity as an important means of improving science-public relations through rigorous social scientific analysis.

There are two main and inter-related forms of reflexivity which are important to this thesis. Firstly, "interpretative" reflexivity (Lynch, 2000), which involves revealing to institutions (here, scientific institutions) some of their embedded assumptions and helping them to consider alternative ways of imagining themselves, their knowledge claims and their interactions with others. Secondly, a more "internal" reflexivity (Wynne, 1996d) that can be termed "methodological" reflexivity (Lynch, 2000) and that focuses on the social science researcher's own embedded assumptions which shape her knowledge claims. By paying attention to these two forms of reflexivity, this work can put forward alternative framings for public engagement in science whilst remaining open to the fact that these alternatives also rest on assumptions, have limits and should be open to re-examination and change.

This last chapter highlights the main conclusions from my thesis. Firstly, it reminds us why interpretative forms of reflexivity are important and how the present work contributes to the literature on this. Secondly, it turns its eye towards the construction of the knowledge claims made here and how they are socially situated. The chapter then pulls together some central findings concerning stem cell researchers and their discourses about engagement. It highlights not only how these contribute to diverse areas of scholarly pursuit, but also how they can be used to create opportunities for institutional interpretative reflexivity. So thirdly, this chapter focuses on the interpretative flexibility of stem cell researchers' discourses about their work and public engagement. Fourthly, it examines findings suggesting scientists are publics too and the implications of this. Fifthly, it looks more specifically at evidence suggesting there are already opportunities for scientists to reflect on their institutional and social embeddedness. Sixthly, it draws attention to the potential roles of social science and social scientists in a move towards scientific institutional reflexivity. Finally, the chapter ends by discussing a vision of improved science-public relations facilitated by public engagement forms that leave space for reflexivity.

9.1 Interpretative reflexivity

Critical public understanding of science and development studies have alerted us to the existence of diverse and sophisticated "lay" knowledges. However, the dismissal of these in many engagement or participation fora can lead to people's identities being threatened and to ever more troubled science-public relations. This dismissal is, more often than not, due to the framings of these interactions which, in an unacknowledged way, privilege particular forms of knowledge and particular identities over others – usually favouring understandings which are scientistic and couched in certainty, and which focus on a knowable, predictable and controllable nature (see chapter 2). Given these findings, some social scientists have argued that it is important to explore the minutiae of science-public interactions and their framing (e.g. Irwin, 2001; 2006) and to help change scientific institutional contexts by encouraging members of these institutions to reflect on their own cultural and social embeddedness (e.g. Wynne, 2005a; 2006). This is precisely where the present thesis locates itself: by exploring scientists and their discourses, it reveals some of the framings that shape science-public interactions and, by highlighting them to scientists, it can encourage scientific institutional reflexivity, which in turn may foster better science-public relations.

The present thesis has been a study of scientists in a promising but controversial area: stem cell research. These scientists are frequently in the public eye, and are interested and concerned about public support for their work. Their voices are often dominant in public engagement – for example, when they are called in front of parliament to give evidence to inform decisions about the legalisation of certain practices. Therefore, their discourses can be very important in shaping science-public interactions. By studying these discourses and how they frame engagement, this thesis sought to uncover the often invisible socio-cultural assumptions that shape scientists' epistemological commitments and discursive practices, and therefore that shape the possible forms science-public relations can take. By holding up a mirror to stem cell researchers and showing them these assumptions, social scientists may be able to encourage reflexivity in these researchers and encourage a "change of institutional cultures" (Wynne, 2005a: 19) away from those that seem to foster mistrust in science (Wynne, 1996b; 2006).

This thesis contributes to debates about modernity, trust and reflexivity by putting forward, and searching for opportunities for, a type of reflexivity that enables profoundly different ways of thinking about the world. In this, it builds on, but goes further than, Giddens' and Beck's "substantive" forms of reflexivity (Lynch, 2000) to advocate an "interpretative" form of reflexivity (Lynch, 2000). From Giddens (1990), it takes the importance of modifying current practices in the light of new knowledge. Unlike Giddens, it does not see these changes as automatic given the conditions of modernity and, instead, the version of reflexivity advocated here highlights a role for social scientists and scientists to *actively* participate in the transformations of current practices (of engagement for example). From Beck (1992), it takes the useful analytical categories of "counter modernity" and "reflexive modernization", where the conditions of the latter (which is seen as preferable to the former) should be identified and promoted. It also builds on the role seen for a variety of "sub-politics" to critique the institutions of science. However, the focus of interpretative reflexivity goes beyond that of Beck on the “confrontation with unintended consequences”. Thus, in contrast to both these scholars, the aim of reflexivity here is to examine epistemological commitments framing knowledge claims, rather than to provide further scientistic “facts” which will – supposedly – improve prediction and control, and therefore trust in science. This type of reflexivity relies on a conception of trust that goes beyond Beck and Giddens’ rational-calculative model (see below).

The present work also contributes to and further develops cPUS. Most empirical research in this multidisciplinary field has focussed on the existence of sophisticated lay understandings and how identities and epistemologies are imposed on lay people (e.g. Irwin and Wynne, 1996; Kerr *et al.*, 1998b; Parry, 2003b). Here, I pay heed to the work of Development Studies scholars with their focus on power and the framing of participation exercises (e.g. Cooke and Kothari, 2001a), and to recent calls and emerging findings by cPUS researchers, (some working with people from Development Studies, e.g. Leach *et al.*, 2005), to explore the intricacies of public engagement and its framing (e.g. Rogers-Hayden, 2003; Irwin, 2006). This enables me to offer a study of *scientists*, rather than publics, as a contribution to cPUS, and to focus on how *power relations* are played out (how scientists maintain their authority for example), and how they can be transformed. An understanding of these dynamics is essential in order to open the door to different framings of public engagement.

In particular, by focussing on how stem cell researchers talk about not only their work, but how they should interact with publics, I am able to develop a typology of the types of “scientific citizenship” (used in Irwin's sense, e.g. 2001) which they *perform*. I see a role for social scientists in helping stem cell researchers reflect on this in order to create alternative forms of science-public interactions (as I discuss towards the end of this conclusion). I also examine, similarly to the way lay people have been studied, whether identities and epistemologies are imposed on scientists too when they participate in science-public encounters.

9.2 Elements of methodological reflexivity

Wynne has argued that “SSK is badly prepared for dealing with the public arena by a lack of reflexivity over its own founding preoccupations” (Wynne, 1996d: 362). Given my aims to use sociological findings to contribute to improving public engagement in science, it is important to lay out some of the factors that shape this research. Thus, before holding up a mirror to scientists and highlighting some of their embedded assumptions, I need to reflect on my own epistemological commitments and on the constructedness of my knowledge claims; this requires a commitment to methodological reflexivity. It must also be noted here that my “founding preoccupations” are not rigid and have evolved as a result of conducting this research.

I have a dual membership as a natural and social scientist. Both of these identities have influenced this research in a number of ways. Firstly, I am committed to science being a valuable part of society; I have never considered options for improving science-public relations that would require dramatically cutting down scientific research and development. Having said this, my training in social science has revealed to me that the central role of science in setting and shaping our ways of understanding the world, its problems and their solutions should be challenged. I seek solutions where science is still a respected creator of knowledge claims about the world, but where it takes on a more modest role as only one source of authority and truths amongst others. This overall political aim did change throughout this research and my normative view of the balance between science and other forms of expertise and knowledge in society did shift. In particular, the initial focus on finding better ways of communicating science became a focus on encouraging scientists to become more reflexive. I currently am very interested in highlighting to scientists the constructedness of their knowledge, without implying that this falsifies it or suggesting that it becomes *mere* social construction. However I am not afraid to acknowledge that this does entail questioning the scientific knowledge involved (Wynne, 1996d: 379)

Secondly, on a more practical level, this dual membership has shaped most of the particular instances of data collection that my work is based on. Many of my interviews were set up through contacts I had as a scientist. Also, the CV I sent informants highlighted my funding by a research council that would usually fund natural science research, and my training as a geneticist, but also as a sociologist. Thus, the conversations I had with these scientists was influenced by their projection of one or several identities onto me, and there were probably many instances when informants opened up to me in a way they would have felt less comfortable doing with another social scientist or a journalist.

Thirdly, as well as being informed by my natural scientific knowledge, my data analysis is embedded within frameworks and epistemological commitments derived from my training as a sociologist in Edinburgh. The teachings of SSK's Strong Programme have heavily influenced me, as have other constructivist perspectives, including discourse analysis. I started out my research keen to use discourse as a "topic" *and* a "resource", in the same way Kerr *et al.* (1997) do for example. However, as my data collection and analysis proceeded, it became clear to me that we can never access what people *really* think, and therefore we cannot use informants' accounts as a resource to create definitive accounts of events. Therefore I became more interested in discourse as a resource and in its *performativity* (e.g. Gilbert and Mulkay, 1984; Potter, 1996; Szerszynski, 1999). This enables me to contribute to cPUS and STS (SSK in particular): building on studies of lay people and their knowledge, and on studies of scientists and the interests shaping their knowledge claims, I explore how scientists' discourses can privilege or marginalise various voices and forms of knowledge and expertise in public fora. I develop a typology of discourses about engagement and the sorts of citizenship these perform (further discussed below). Thus, although my study focuses on discourse as a "topic", it is not reductive (as argued by Gieryn, 1982): because discourse is not separate from but constitutive of institutional cultures, power relations and so on, the form of discourse analysis advocated here offers a way of studying all these sociologically interesting things.

Finally, my social constructivist background brings out the importance of thinking about the constructed nature of my own knowledge claims. Although I am committed to helping scientists be more reflexive by giving them a sociologically informed understanding of their knowledge, I do not argue that my knowledge claims are more *truthful* than theirs. They are instead offered as ways to open-up for examination and change the already-existing, often crystallised, and sometimes problematic forms of public engagement. Thus, the interpretative reflexivity I am advocating is different to Bourdieu's reflexivity thesis, and social scientific understandings are not seen as *truer* than natural scientific or commonsensical ones.

The rest of this conclusion will draw out the themes which cut across my work and will link my empirical findings with the literature. It will highlight contributions to knowledge and normative and analytical implications of my conclusions.

9.3 Flexible interpretations and performativity in stem cell research

My work confirms a number of findings from STS, particularly in relation to the interpretative flexibility of scientists' discourses, and the socially negotiated nature of scientific claims and authority. It also develops previous work on the performativity of scientists' discourses.

The body of empirical data in this thesis, collected in a number of different locations from a number of different informants, shows that scientists describe and define stem cells and stem cell research in diverse and sociologically interesting ways. Knowledge claims about stem cell research are "under-determined by reality" and "theory-laden" (Bloor, 1976), since one cannot point to a stem cell in nature or directly measure concepts such as "pluripotency", and since definitions and classifications of what counts as a *good* (or *real*) stem cell will depend on the social location (including goals and interests) of the people articulating these definitions. Thus the conceptual fluidity surrounding SCR provides anyone talking about this field with many opportunities for interpretative flexibility. This interpretative flexibility enables stem cell researchers' discourse to *do* a number of things, which I make sense of in sociological terms

Firstly, stem cell researchers' discourses often take the rhetorical form of "boundary-work" (Gieryn, 1983; 1995). By analysing the boundaries they erect and the diverse criteria they use, I show that stem cell researchers attempt to give greater authority to certain areas of SCR, or to SCR or science as a whole. As an illustration, boundary-work work is conducted around the promise of adult stem cells versus embryonic stem cells, pluripotent versus lineage restricted stem cells, or "natural" versus "unnatural" reproductive technologies.

Often, boundaries around science and around particular aspects of SCR mutually shape each other. If science is constructed as aiming for therapies, a boundary between pluripotent – yet potentially cancerous – stem cells and lineage-restricted – yet safer – stem cells may be erected. If science is constructed as focussing on basic biological processes, a boundary between exciting, pluripotent stem cells as research tools, and relatively unexciting, well-understood stem cells may be erected. However, boundary-work in this study does not always reflect my informants' narrow institutional interests, and in some instances, the defence of SCR can become a microcosm for the defence of science as a whole. These multiple boundaries highlight that SCR can be analysed as a boundary object, with a conceptual fluidity enabling multiple competing agendas to be satisfied.

As Wainwright *et al.* (2006a) also find, the boundaries erected in relation to SCR in this research draw on the ambiguities and uncertainties that my informants experience in this field. These boundaries are more than just rhetoric: for example, SCR can *really* be experienced as a brand new field or as an established one. However, unlike Wainwright *et al.*, I try not to impose an analyst's definition of SCR or science onto this study (for example, where realist assumptions about what counts as "scientific" in opposition to what counts as "ethical" are imposed onto informants' discourses). Instead, I focus on the implications of the boundaries erected. Thus, when a researcher draws a boundary between "ethical" adult stem cells and "less ethical" embryonic stem cells, I do not see this as a reduction in the authority of science through an inclusion of "ethical" (as opposed to "scientific") criteria Rather, I see this as a temporary normative construction, made by my informant, of science as an "ethical" pursuit; in opposition to "non-science", constructed as less ethical. This can shore up the authority of science against claims that scientists are not ethical and need further regulation to be kept in check.

Secondly, stem cell researchers' discourses can *perform* certainty. MacKenzie's (1990) certainty trough was a useful heuristic to make some sense of stem cell researchers' (un)certainty discourses about the future of SCR and how these relate to the "distance" from knowledge production. However, this concept needs to be developed as it cannot take into account the situatedness of discourses. Building on Lahsen's (2005a) critique of the certainty trough, I analysed discourses of (un)certainty as "topics" and focussed on their performative dimension: stem cell researchers may try to create belief in the promises of SCR by evincing certainty.

By analysing these sorts of future-oriented discourses, the present thesis thus contributes to the "sociology of expectations" (cf. Brown and Michael, 2003; Brown and Kraft, 2006; Wainwright *et al.*, 2006b; Horst, 2007). I found that stem cell researchers often highlight the need to hype promises in early stages of research in order to secure political, legislative, financial and/or public support (for example, by over-selling the potential for cures or by not mentioning particular limitations such as teratoma formation). This is what Brown and Michael (2003) call the narrative of "optimism to obstruction". Therefore, discourses of certainty may, under certain circumstances, lead to a belief in the future promise of SCR and change the funding environment. Like Wynne, I do not want to suggest that any contrast between private expressions of uncertainty and public expressions of certainty are some form of "conspiracy"; these contrasts can be a way of reconciling a number of different external pressures (1996d: 378). However, as I discuss below, it is important for social scientists to reveal and analyse these, and to open-up ways for public discourses about science to confront these contrasts.

Thirdly, particular ways of classifying stem cells, or the focus on certain definitions rather than others, can shape the avenues open for discussions about SCR. Since stem cells are not given in nature, there are a number of ways to define and classify them. Wainwright *et al.* (2006a) describe variations in the ways in which SCR professionals describe embryos. Similarly, I find variety in how they classify, for example, embryonic stem cells. I then go on to analyse what these definitions *do*. I locate these definitions, for example, in the context of parliamentary debates and show that, by classifying stem cells according to scientistic criteria such as pluripotency, discussions about the origin of stem cells (aborted foetuses or embryos "left-over" from IVF) are fore-closed. This also reinforces the dominance of technical (rather than lived or embodied) forms of expertise in public fora.

Thus, scientists' discourses about their work, but also about what forms public engagement should take, enables certain types of citizens to have a valid place in discussions around science: these discourses *perform* certain types of "scientific citizenship" (see Irwin, 2001). By linking theses discourses with theories of democracy, I show that stem cell researchers can perform six different types of citizenship, including: engagement modelled on versions of liberal democracy, where citizenship involves consuming science, or being enrolled by scientists and lobbying for them; engagement modelled on deliberative democracy, where citizenship involves sharing and discussing "expert" knowledges; and engagement modelled on radical/plural democracy, where citizenship involves sharing multiple contextual knowledges and challenging "normalising" (Foucault, 1976) power structures. This typology could potentially be used to encourage interpretative reflexivity (see below).

In summary, scientists' discourses can do a number of things: they establish, promote or demote the epistemic authority of certain areas of SCR or science; they set up particular definitions as beyond negotiation (Van Dyck, 1995), thereby framing discussions about science (sometimes encouraging scientistic understandings, and also fore-closing or opening up discussions); and they can change the material world (for example, by affecting funding or the forms that engagement take). It is also important to note that an emphasis on what should count as a stem cell diverts attention from discussions about whether stem cells are what we want to be using in the first place. In other words, a focus on internal, small-scale debates puts out of view larger scale uncertainties about the role of science in society (see Star, 1985). Thus, boundary-work and other rhetorical strategies discussed here have implications beyond the narrow confines of technical claims and their authority; they shape engagement and the meaning and role of science itself.

9.4 Scientists as publics

This work has elaborated more fully than others the extent to which and ways in which scientists are publics. Taking seriously the deconstruction of the lay-expert dichotomy, it has used analytical tools from STS usually used to study scientists, in combination with tools from social theory and cPUS usually used to study lay people.

Like Ehrich and her colleagues (2006; 2007), I look at the diversity in professionals' views and understandings of their work (see also 9.3). In particular, similarly to publics, professionals express a number of *concerns* about SCR. Interestingly, many of these are articulated by drawing on similar motifs to the ones used by lay publics (such as the Frankenstein script or images from *Boys from Brazil*). This suggests that available cultural tropes are an essential way of making sense of complex science and the issues which are embedded within it. They are useful not only for lay publics (e.g. Mulkay, 1996) but also for professionals. Scientists, like publics, simplify existing science-fiction storylines to order the world and express themselves. The fact that they rarely use these cultural tropes to refer to their understandings in public fora (the examples drawn on here all come from interview data) should be explored (see below).

Using the analytical lens of "expert systems" (Giddens, 1990), I make sense of the ambivalence expressed by professionals towards their work and their own role. In this way, I am specifically examining how professionals are also "lay", as suggested by Giddens. Even though I have critiqued theories of modernisation for reifying expertise, I find this tool particularly useful in exploring the discourses of stem cell researchers. I undertake an in-depth analysis of some of the many expert systems that stem cell researchers *have* to rely upon in order to go about their daily life and work. I develop the literature on trust and modernity by combining this analysis of expert systems with a hermeneutic, rather than rational-calculative, conceptualisation of expressions of *trust* (unlike Giddens, 1990; Beck, 1992; see a critique in Wynne, 1996b) and of *mistrust* (Wynne, 1996b; see a critique in Szerszynski, 1999). Thus, I analyse scientists' discourses in the same way I would lay discourses, and I explore which concerns about expert systems are articulated under which circumstance. For instance, I find that in private settings, some stem cell researchers may reflect on past disappointments due to over-hyped promises, and may also raise a number of specific concerns they have about SCR and the way it is conducted and applied (in particular in relation to clinical trials, commercialisation and the globalisation of research).

Concerns that are raised in private settings by stem cell researchers about particular aspects of their work and its location in society need to be explored; they can have an impact on future ways of doing public engagement (see below). Showing that these concerns can be discussed under certain circumstances suggests a role for social scientists to create spaces in other fora for such concerns to be raised. These concerns are also evidence of, and opportunities for further reflexivity, including interpretative reflexivity. This is an important aspect of the present research to which I now turn to.

9.5 Spaces for reflexivity?

This research provides a micro-level empirical investigation of broad claims about trust and reflexivity in modern society (e.g. Beck, 1992). It highlights the presence of professional ambivalence and examines the conditions which enable this ambivalence to be expressed. It also demonstrates that scientists can be reflexive about their practices, knowledge and cultural location. In many instances, stem cell researchers' discourses do serve to shore up their professional authority (as Kerr and Cunningham-Burley find for new genetics professionals, 2000). Nevertheless, I find more evidence of, and opportunities for, "reflexive modernization" and even interpretative reflexivity than other scholars, thus building on the limited empirical data in this area (e.g. Ehrich *et al.*, 2006). Although this reflexivity is not a dominant feature of scientists' discourses, its presence has important implications for science-public relations.

This thesis's analysis of trust in expert systems reveals opportunities for and examples of scientists' self confrontation with the potential consequences of their work; that is, "substantive reflexivity". For example, some express concerns that research on humans might be approved based on results from animal experiments, when there can be a lack of correspondence between the two. This in turn can cause clinical trials to be dangerous for participants.

Some expressions of mistrust or ambivalence are also accompanied by explicit reflections on the context of knowledge production and on the embedded (and potentially problematic) cultural-institutional body language. For instance, clinical trials can be led by people with high levels of capital (such as influence on ethics committees); this may lead to unsafe protocols being tested on humans; or informed consent may not sufficiently highlight the uncertainty of knowledge claims. Raising these sorts of concerns creates opportunities for change.

Some scientists are also aware of the culturally and socially situated nature of their knowledge. For instance, stem cell researchers sometimes specifically highlight how social factors shape scientific claims (not only false claims), which contrasts with the more common discursive construct that "asymmetrically" accounts for "error" by only using the "contingent repertoire" for *false* claims, and the "empiricist repertoire" for *true* ones (see Gilbert and Mulkay, 1984).

These expressions of concern are examples of reflexivity, as well as opportunities for further reflexivity. This reflexivity takes on a number of forms in these stem cell researchers' discourses. It can be methodological reflexivity, where scientists reflect on the contexts of production of their own knowledge claims. It can also be substantive reflexivity, where further knowledge leads scientists to confront the unintended consequences of their work. Finally, it can be substantive reflexivity. In this case, an awareness of the arbitrariness of particular science-public interactions – such as a focus on certainty in informed consent settings – can be revealed and opened up to change. Following this, new ways of imagining and framing these interactions are put forward: for example, a scientist may choose to highlight the unpredictability of science to potential donors, even at the risk that they will then not consent to donate their material. The latter form of reflexivity is invoked, even at the risk of stopping some areas of science from going forward and of letting "non-technical" criteria be used to shape the future of research.

This reflexivity occurs not only in response to external critiques (cf. Wynne, 1996c) or to "sub-politics" (cf. Beck, 1992) – even though some examples of reflexivity come from talking to patients or becoming acquainted with social science critiques of the deficit model of public understanding of science. Some stem cell professionals attribute their reflexivity to their work putting them in contact with forms of knowledge that challenge their own understandings (for example, when people who work on embryonic stem cells visit fertility clinics and talk to colleagues who care for infertile people, and therefore have different ideas about the "value" of an embryo). This resembles Giddens' version of "reflexivity", which becomes intrinsic to the processes of science. However, this reflexivity is, in no way, automatic or inevitable. Indeed, my data also suggest that contact with other forms of knowledge can lead scientists to become less reflexive, less open to criticisms, and more protective of their authority (for example, when refusing to discuss the limitations of informed consent).

Thus, there is evidence in stem cell researchers' discourses of a reflexivity that breaks with a scientistic understanding of the world. However, this reflexivity can be very uncommon during interviews with some researchers, and is very rare in public fora. The challenge for social scientists and others is to extend the opportunities for this reflexivity.

Many of my informants say they cannot use certain forms of argument in certain fora. For example, they discuss the difficulty of using religious, ethical or *emotionally*-framed reasons for and against particular practices. They also rarely want to highlight the social contingency of their knowledge claims, even in private. Although these could be considered to be a discourses of "suffering" and performances of "authenticity" (see Brown and Michael, 2002), it is interesting that they exist, and to explore in which circumstances they are deployed. If reflections on the usually unacknowledged framings of scientific knowledge are made by scientists in the private context of the research interview, it might be useful for social scientists to highlight this discrepancy and make scientists reflect upon it. It would also be useful to think about what sorts of spaces could be created to enable scientists to be this reflexive (as discussed below).

9.6 Reflexivity and the role of social scientists

I see a practical and explicitly political role for social scientists in improving science-public relations: by facilitating interpretative reflexivity. This does not, however, entail setting social scientists up as experts with authority over (natural) scientists. Rather, this entails transforming the framing of engagement and encouraging scientists to explore their assumptions. This, in turn, may lead to scientists taking on a different, perhaps more modest, role in public engagement.

A number of roles for social scientists have been advocated by different people. Even within the field of SSK, various positions have been put forward (see the special issue in Social Studies of Science, 1996). For me, given the multiplicity and contingency of views, identities and knowledge claims, it is impossible to choose "sides", let alone side with the underdog. It is also impossible to remain "neutral", since any contribution to debates about science will influence and shape these (even if this is only at the micro-level, for example, influencing one person). In this, I follow Wynne (1996d) and believe that social scientists, in particular informed by SSK, can play an interesting role by problematising scientific knowledge and its role in society, and by encouraging scientists to reflect on this.

I want to make the claim (which has political implications) that public engagement does not have to be the way that it is. To use Bourdieu's language, but eschewing his realist commitments, I want to reveal the arbitrariness of the "structure of the field" of public engagement and create opportunities for changing the "rules of the game" in science-public relations. This can be done at two levels: at the level of individual scientists, by helping them acknowledge their embedded cultural assumptions (thus opening these up to change); and at the level of wider society, by building on scientists' interpretative reflexivity and making it more widespread and visible. The main (socially located, open to criticism and change) assumption underlying this goal is that public engagement, if it is very scientistic and framed by problematic but un-acknowledged cultural assumptions, can be *improved*. This can be done by reflecting upon the uncertainty and contingency of knowledge claims and by changing the normalising forces (Foucault, 1976) that shape current engagement practices, in order to enable different voices to gain epistemic authority.

More specifically, social scientists can show scientists that they too are publics, for example, that they too use science fiction imagery to make sense of SCR. This may encourage them to share their fears and concerns in terms that make sense to other members of the public. It may also encourage them to stop dismissing concerns expressed by publics using science fiction or other available cultural references. Social scientists can also highlight to scientists that, at times, they openly accept "contingent" rather than "empiricist" reasons for making decisions or constructing knowledge claims. This highlights that *contingent* or *socially constructed* does not equate with *falsehood*, and may encourage scientists to be more modest about their knowledge claims and accept that (equally contingent) claims made by publics should not be dismissed out of hand. By showing them that even technical claims are social, and by suggesting alternative frameworks for engagement (ones that welcome the language of feelings and values, for example), social scientists may create situations where scientists find they can draw on other discourses and identities than the ones they have previously felt restricted to.

Social scientists should also highlight the performativity of scientists' discourses to them. In particular, we could show them, for example using the typology developed in chapter 8, that these discourses enable and disable different kinds of scientific citizenship. This can open up opportunities for yet other forms of engagement, or favour existing ones (which already tend towards respecting lay views, for example). For instance, if we demonstrate to scientists that during one interview, they switch between a model of engagement as an "internal dialogue with scientists as publics" and another as "constructive conversations", we might encourage them to reflect upon why they sometimes dismiss and sometimes include forms of expertise other than their own. This has the potential to lead to more inclusion in public engagement.

We should also challenge the idea that scientistic classifications are the "natural" way of thinking about the world, and we should challenge the idea that science is the "natural" frame for public engagement in science. As Mouffe (1992b; 1996) suggests, we need to fundamentally question the existing power relations that play out in public engagement, and for example, by bringing definitions back to the stage of negotiations (see Van Dyck, 1995), we can change the power hierarchies of who has a say in what. In addition, if we highlight that scientists are not the voices of rationality (without implying that they are *merely* emotional and political and therefore wrong), we can seek ways of accommodating avowedly socially and culturally embedded views in public engagement. If, on the contrary, we maintain the fantasy that scientific knowledge is free of influences and founding assumptions, we will end up ignoring particular forms of bias which may lead to worsened science-public relations, as was the case in New Zealand after the inquiry on Genetic Modification (e.g. Genus and Rogers-Hayden, 2005).

9.7 Improving science-public relations?

There are a number of specific ways in which I think science-public relations could be improved through changes in the "body language of science" (Wynne, 2005a) and changes in public engagement with science and scientists' engagement with publics.

In particular, I think it would be useful to more frequently highlight the uncertainty of science in public fora and discourage the common narrative of "optimism to obstruction" (Brown and Michael, 2003). I also believe that discussions around science need to specifically address concerns that scientists raise in private, such as: the potential for women to be coerced into donating their eggs, the ownership of biological material, the difficulties (and perhaps impossibility) of obtaining true informed consent, the domination certain clinicians can exert over local ethics committees, the risks of tumour formation from stem cells and the likely prohibitive cost of any therapies derived from cloning technologies. This can be done by inviting scientists, talking at public events, to raise these issues themselves, or by presenting findings such as those from this thesis in public fora.

I have argued above that the scientistic framing of public engagement needs to be challenged. One way social scientists can do this is by organising engagement events which cover topics that publics request (this can be determined during earlier focus groups discussions, for example). Also, it might be useful to organise events to discuss the advantages and disadvantages of stem cells, but rather than classifying these cells according to criteria such as pluripotency, they could be classified according to their origin: this would highlight rather than down-play the ethical and social issues raised by the use of stem cells from different sources (foetuses, embryos, cord blood etc.). These events could also be fora to re-examine and question labels such as "therapeutic cloning" versus "somatic cell nuclear transfer", and to explore the issues raised by choosing one definition over another.

This research has also shown that some of the so-called "irrational" fears projected onto publics (by scientists and others) are also to be found in scientists' accounts (for example, concerns about country leaders creating clones of themselves). These fears should not be dismissed as irrelevant, or responded to with supposedly unproblematic "facts". Rather, they should be explored, since they can often be the expression of deep-seated and very relevant anxieties. We must acknowledge that concerns, decisions or arguments do not have to be cast as “rational” in order to be legitimate, and publics and scientists have a shared status as rightful citizens.

Similarly, many of the public “misunderstandings” are present in scientists’ accounts (such as ambiguities about how old the field of SCR “really” is). Rather than dismissing all these as wrong, and looking for the definitive account of SCR, we should acknowledge that these different stories can co-exist. It is also interesting to reflect upon why people draw on particular definitions, what concerns or promises they want to highlight by using these definitions, or what boundaries they want to erect.

Importantly, in order to encourage scientists to express their private concerns in public, or to move away from their language of rationality and certainty, there need to be changes to public engagement environments. In particular, it might be advantageous to set up discussions in less adversarial and polarised ways. This can be achieved by encouraging participants to explore their commonalities, rather than focussing on their disagreements (for example, by imagining futures they would all agree with). It is also important, in any science-public interaction, to highlight to participants that all views should be respected. This can be facilitated by holding discussions in small settings (as was done here in the multi-disciplinary discussions), but is also possible in larger events (which might begin with small round-table discussions, and then might increase in scale). Another way of encouraging scientists to change their public discourses is to build relationships with them: participate in their meetings and offer social scientific knowledge there, and invite them to other events where they are welcomed.

In response to the Royal Society’s concern about a “crisis of trust”, I would suggest, in keeping with Wynne (2006), that there is no way to instrumentally *make* people trust science. Nevertheless, an interpretative form of reflexivity on the part of scientists and their institutions is an important means by which they can seek to *be more trustworthy*. If scientists accept the limits of their knowledge and stop making illegitimate claims that alienate publics who disagree with the founding assumptions of these claims, science can be given a more legitimate and modest role in society. This may in turn reduce further public alienation. However, I do not want to suggest yet another easy fix to yet another deficit (cf. Wynne, 2006): this is not a case of solving the deficit of public trust caused by a deficit in scientists’ reflexivity simply by telling scientists their knowledge is socially constructed. Rather, working to improve science-public relations will take a long-term commitment by social scientists, scientists and others to come together and explore, question, reflect upon and share their embedded assumptions about knowledge, the place of science in society and the imagined futures that we want to aim for.

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Appendix 1: Stem cell definitions

Here are a few definitions to help navigate my thesis. In keeping with my theoretical commitments, these are to be taken as a guide only, all the terms used tend to be constructed differently by different researchers in different circumstances.

Adult stem cells: Stem cells that can be found in adults, for example haematopoietic (blood) stem cells. This label can also describe cells found in cord blood and in foetuses. These cells usually differentiate into a restricted range of cell types (such as the different skin cells), but some adult cells seem to give rise to many different cell types (such as neural stem cells giving rise to haematopoietic cell types, or apparently multipotent haematopoietic cells).

Autologous therapy: Therapy that helps a particular patient by using their own tissue or cells.

Blastocyst: Early stage embryo.

Cloning: (in biology) To create a genetically identical copy. There are different types of cloning: cloning plants from cuttings, cloning individual genes or particular DNA segments, cloning cells or cloning whole organisms.

Differentiation: Process by which cells divide and specialise in to evermore specialised cell types.

Embryo: Fertilised egg that has started dividing but has not yet given rise to fully developed organs. The earliest stages can also be called pre-embryonic. In humans, the label can be used to describe the first 8 weeks, 10 weeks or 3 months of development. Some sources also use it to describe all the stages until implantation in the uterus or birth.

Embryonic germ cells: Cells that have been derived from primordial germ cells (for more details, see chapter 7).

Embryonic stem cells (ES cells): Cells derived from an embryo that is a few days old. The embryos can be derived from in vitro fertilization or somatic cell nuclear transfer.

Foetus: Usually describes the stages of development following embryonic stages.

Germ cells: Gametes; sperm in human males, oocytes in human females. Contain one set of chromosomes.

Germ layers: Tissues found in the three germ layers (endoderm, mesoderm and ectoderm) are the precursors of all the tissues of the adult organism.

Gonadal ridge: Precursor to gonads (testes and ovaries in humans).

Haematopoietic stem cells: Stem cells of the blood system.

Inner cell mass: Group of cells inside the embryo (at blastocyst stage) from which ES cells are derived.

Multipotent stem cells: Stem cells that can give rise to many cell types.

Nucleus (of a cell): Contains most of the genetic material necessary to build a whole organism. It is packaged up into chromosomes (the rest of the genetic material is present in small organelles: mitochondria).

Oocyte: Female gamete; unfertilised egg cell. Contains one set of chromosomes.

Plasticity (developmental -): characteristic of a specialised cell that can give rise to a specialised cell of a different tissue.

Pluripotent stem cells: Stem cells that can give rise to all (or most) cell types in the body.

Primordial germ cells: Cells that can mature into germ cells. Contain two sets of chromosomes.

Reproductive cloning: somatic cell nuclear transfer followed by the implantation of the resulting embryo into a female. Can result in the birth of offspring with the same nuclear DNA as the donor.

Somatic cell: Cell that is not a gamete. Contains two sets of chromosomes

Somatic cell nuclear transfer, SCNT (or cell nuclear transfer, CNR): to take a nucleus from a somatic cell and place it in an enucleated oocyte. In some cases, the resulting cell can be made to start dividing and give rise to an embryo with the same nuclear DNA as that of the somatic cell donor.

Stem cells: cells that can self-renew and give rise, by differentiation, to various cell-types.

Teratoma: Tumour that usually contains a variety of cell types (adj. teratogenic: ability to give rise to teratomas)

Therapeutic cloning: often used interchangeably with SCNT. Involves taking the nucleus of a somatic cell from a patient, placing it in an enucleated oocyte, prompting cell division to start and continue till blastocyst stage, removing the inner cell mass and culturing it. The aim in humans would be to be able to obtain cell lines (ES cells) that could be used to treat the patient that initially gave the somatic cell. One blastocyst has been obtained in this way by a group in Newcastle, but no human ES cell lines have yet been obtained in this way (although this is what a Korean group claimed to have done).

Trans-differentiation: Ability of cells which are already committed to becoming cells from a particular tissue to, instead, become cells from a different tissue (e.g. haematopoietic cells becoming neural cells)

Appendix 2: Multi-disciplinary discussion one, strands for discussion

Stem cells, Scientists and Publics: Generating a Dialogue

A multidisciplinary discussion

Strands for the discussion:

Substantive discussion:

- How and when the aims and details of current and future work should/could be discussed publicly
- How and to what extent legislators should enter into dialogue
- How and at what stage of scientific development legislation should be considered
- Whether there are any topics where some or all groups should not be involved
- Under what circumstances areas of laboratory-based scientific enquiry might be vetoed by society and what happens in this case
- The impact legislation has on scientific practices
- How, if at all, the translation from lab to clinic should take place

Establishing Dialogue

- The reasons for open discussion of aims, plans and progress in science and medicine
- How scientists can better learn from/listen to publics
- Which engagement practices could best be used in the stem cell debate
- What avenues of communication are most effective or problematic
- How lay-expertise can impact on research practices
- How, when and where a broad spectrum of publics can be engaged
- Whether the outcome of discussions should end in consensus or whether more pluralistic solutions can be achieved in reality

Appendix 3: Background information provided to participants

Scientists' Understandings of Publics and Engagement Practices; Stem Cell Research in the UK and Australia

Nicola Marks, PhD research

My background:

I came to this PhD through training as a geneticist to post-graduate level, work experience in Australia, as well as an interest in stem cell research and the interactions between scientists and members of the public.

2003- MRC Human Genetics Unit and INNOGEN, the ESRC Centre for Social and Economic Research on Innovation in Genomics, University of Edinburgh, UK
PhD
Supervisors: Prof. Veronica van Heyningen (MRC HGU) and Dr. Sarah Parry (Sociology/RCSS/INNOGEN)

2002-2003: MRC Human Genetics Unit, University of Edinburgh, UK
Masters by Research in Life Sciences Awarded with Distinction

2001-2002: Royal Children's Foundation, Royal Brisbane Hospital and Queensland Health Pathology and Scientific Services, Brisbane, Australia
Research assistant and scientist

1998-2001: St Catharine's College, University of Cambridge, UK
B.A. (Hons.) in Natural Sciences, Pt. II Genetics 2.1

The research project and its background:

Brief literature review:

Social scientists have shown that changes in modern society -such as globalisation, commercialisation or the visibility of scientific disputes- can have strong impacts on trust between members of the public and the scientific community. Various institutions have tried to address this 'crisis of trust' by providing more information about science.

However, studies of the 'public understanding of science' have shown that 'understanding' science is much more multifaceted than the assimilation of facts and that it depends widely on other social and cultural factors. Thus simply 'educating' lay-people is not the way to improving science-public relations.

In addition, the notion that only knowledge derived from controlled laboratory experiments can be used for science policy has been put into question. Indeed, various studies show that alternative forms of knowledge, such as that derived from living with an illness, are important too. It has thus been suggested that more inclusive dialogues between scientists, policy-makers, stake-holders and others members of the public could lead to better decision-making around science.

Although the ways in which publics interact with science and scientists has been widely investigated, the ways in which scientists view and interact with various members of the public has not been the object of as much scholarship. I hope to redress this gap in the literature.

Aims and objectives:

Overall objective:
Provide insights into engagement practices and science-public relations in order to help improve dialogues between science and publics, particularly in the case of stem cell research (SCR).

Main research question:
What are scientists' views, understandings and experiences of publics, engagement, as well as SCR and the issues surrounding it?

Some points of interest:
What are the issues in SCR?
How could these better be discussed in public?
How could different forms of knowledge play a role in shaping the future directions of SCR?
How do scientists maintain their authority despite increased public challenges?

Methodology:

My overall strategy is to investigate SCR scientists' accounts in different settings.

Comparing the UK and Australia:

I am talking to various researchers in the UK and Australia. The idea is not to perform a systematic comparison between the two countries. In this sense, I am not speaking to 'equivalent' people (same gender, level of seniority, media experience and research topic) in the two countries, in order to uncover the precise factor that creates a difference in opinion. I am instead looking at a variety of researchers, in a variety of settings to see what differences and similarities arise.

I am doing an international comparison as I think it is interesting to look at the role different legislations play as well as the impact of different histories in science-public relations. I chose Australia because it has a strong research agenda in SCR and, although it is similar to the UK culturally, the regulations concerning the use of embryos in research are different. I thought this would provide interesting contrasts. In addition, I have access and contacts in Australia which should facilitate the research process.

Different people in different settings:

Australia:

Interviews:
I am interviewing junior and senior SCR scientists, working on adult and embryonic stem cells, in Brisbane and Melbourne. I am also interested in speaking to scientists from other fields who have been active in the promotion or criticism of SCR. I am aiming to speak to 15-25 scientists. Some of the topics I would like to discuss are the issues they see in SCR, their experience of interacting with members of the public and their views on some social science findings.

Focussed group discussions:
I plan to conduct 2 group discussions in Australia. The groups will be composed of peers (I hope to do one focus group with researchers at the beginning of their career and one with more senior researchers). These interviews aim either to investigate particular experiences of engagement or generate thoughts around SCR controversial issues or social science findings.
Follow-up:
Subsequent to the focus groups, I hope to briefly speak with the participants in order to obtain their reflections on the process.

Observational research:
If the opportunity arises, I would like to observe SCR scientists in the process of interacting with public groups. This could include presenting evidence to policy-makers, talking to venture capitalists or funding agencies, speaking in schools, giving press conferences or public lectures etc.

UK:

I have already organised a group discussion between senior natural and social scientists to talk about particular issues pertaining to SCR and also about how people with different types of expertise can interact.

On my return, I plan to conduct another similar group discussion, this time involving more people.

I also plan to do interviews (I have already conducted a few), focus groups and observational research. The balance of each will depend on what I managed to achieve and what I found in Australia.

Confidentiality and anonymity

With the permission of the participants, I audio-record the interviews and group discussions. If this is acceptable, I transcribe them in full and code the names to indicate information such as country of residence, level of seniority, type of research and gender. I will be the only person to have access to the full transcript which will be stored on a computer, accessible only by password, but other researchers involved in the project will be shown extracts.

If the participants do not wish to be audio-recorded, I ask their permission to take notes, which I write up after the interview, and code in the same way I would a transcript.

Upon request from the participants, I can send a copy of the transcript from which they may remove anything they do not wish to appear.

I may use quotes from individual interviews and group discussions in my thesis. The name of the research participants will nowhere appear in my thesis and the quotes will only be attributed by code (see above).

I must however highlight the fact that the stem cell community is not very large, and there is a small risk that someone may recognise a quote as originating from a particular person. The precautions I am taking will reduce this risk, but I cannot guarantee full anonymity.

If the participants decide at any time during the interview or in the 6 months subsequent to it that they no longer wish to take part in the research, they are free to end their participation without stating a reason. The transcripts and recordings would then be erased and not included in the study.

If the participants indicate an interest, I would be happy to send them a summary of my findings when I reach the end of my PhD.

Contact details:

Nicola Marks:
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Appendix 4: Research consent form

Scientists' Understandings of Publics and Engagement Practices; Stem Cell Research in the UK and Australia

Investigator:	Nicola Marks
Contact:	nicola.marks@hgu.mrc.ac.uk

MRC Human Genetics Unit
Western General Hospital
Edinburgh EH4 2XU, UK

INNOGEN, ESRC Centre for Social and Economic Research on Innovation in Genomics
The University of Edinburgh, High School Yards
Edinburgh EH1 1LZ,UK

I have been given information about the project and I agree to participate in it. I understand that:

- I will be asked to take part in an interview
- My permission will be sought to audio-record the interview and that the recording will be transcribed in full and my identity will be coded in a manner that renders the transcripts anonymous
- I can refuse to be audio-recorded and that in this case the interviewer will take notes that will be anonymised in the same manner as the transcripts
- The information I provide for the interview will be treated confidentially
- The interviewer will be the only person to have access to the full transcripts and that information may only be shared with research staff directly involved in the project, such as the interviewer's supervisors
- The data will be stored securely
- My participation is voluntary and I can stop the recording and/or the interview at any time without penalty and without giving a reason
- I can ask for my data to be removed from the project at any time within 6 months of the interview taking place
- I can request a copy of the transcript and I can ask for it to be amended
- I can ask for a summary of the findings

Please tick as appropriate:

☐ I give my permission for the information I give to be used for research purposes (including research publications and reports) with preservation of anonymity through coding of my name

☐ I give my permission for the interview to be audio-recorded

☐ I request a copy of the transcript

☐ I request a summary of the findings

PLEASE PRINT YOUR NAME: ....
SIGNED: .... DATE: ....
RESEARCHER'S NAME: ....
SIGNED: .... DATE: ....