Translational Science- The Very Idea: Transformations in Contemporary Academic Health Research?

by

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Abstract

In recent years translational research has been one of the central goals of science and health research policymakers in England and Wales (Cooksey, 2006; Department of Health, 2007). For these agents it carries the promise of a solution to longstanding problems regarding the role played by public sciences in relation to innovations in medicine and public health. The research in this thesis explores how this abstracted, ‘flattened’ framework (cf. Latour, 1990) is transformed within a territory its champions are seeking to conquer: local academic research practices. Translational research is given the Science and Technology Studies (STS) ‘treatment’, which subverts often lauded, seemingly self-evident claims presented by scientists, engineers, policymakers and so on, helping instead to illuminate science and technology as real, situated artefacts built by people in local and specific situations (Woolgar et al., 2009).

With such sensibilities in mind, the aim of the research is to describe how the idea of translational research transports into local academic knowledge production sites of the type policy authors are seeking to enrol, reporting on how aspects of the local production practices are adapted to accommodate this goal and where resistances occur. In short this thesis reports on how translational research is performed and ‘worked out’ in real-world knowledge producing settings within universities. The empirical chapters are structured around three separate case studies of research groups hosted by medical schools in English universities. These are institutions which alongside training of the medical curriculum, practise research, and increasingly it would seem, engage in near-market activities, reflecting wider shifts in the mission of the university in society (Etzkowitz, 2008). The primary method used to generate data was qualitative interviewing, supported by documentary analysis. One case study also granted access for observation of meetings over an extensive period. In writing each case study the objective was to capture how translational research has transformed (or not) mundane knowledge producing practices of researchers in these settings.

The empirical contribution of the thesis is towards a small body of work Wainwright et al. (2009) labelled ‘social studies of translational research’. So far studies have been mainly historical in scope, focussing for instance on failures to translate highly promising basic science discoveries
like stem cells into routine clinical practice. Other studies have used alternative approaches like discourse analysis. Yet studies have said little about the situatedness and performativity of this term itself in the working lives of scientists in university settings. By using a range of STS concepts and methods – including ANT derived notions like ‘immutable mobile’ (Latour, 1987) and ‘objects’ (Law and Singleton, 2005), and symbolic interactionist work on ‘boundary objects’ (Star and Greismer, 1989) - this research has attempted to make a substantive addition to this literature. The theoretical contribution comes from adding empirical flesh to recent calls to synthesise theories on mobility/fluidity with theories about the function of place in contemporary technoscientific production (Henke & Gieryn, 2008). As Henke and Gieryn argue, this is important in order to provide a better account of how artefacts travel than is currently provided by STS theory. As a seemingly geographically dispersed form of production, translational research provides a useful lens through which to consider issues of mobility and place. I will also critically reflect on the frameworks used in the course of empirical analysis which draw from earlier laboratory studies. By treating them as topics and not just resources of STS inquiry, the thesis considers some of the strengths and limitations of these earlier works in light of emergent problems raised by studying translational research over the course of the inquiry.
Declaration

This thesis and the work to which it refers are the results of my own efforts. Any ideas, data, images or text resulting from the work of others (whether published or unpublished) are fully identified as such within the work and attributed to their originator in the text, bibliography or in footnotes. This thesis has not been submitted in whole or in part for any other academic degree or professional qualification. I agree that the University has the right to submit my work to the plagiarism detection service TurnitinUK for originality checks. Whether or not drafts have been so-assessed, the University reserves the right to require an electronic version of the final document (as submitted) for assessment as above.

Signed:

26.9.12
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1. The Research in this Thesis: An Introductory Overview

This thesis examines how an idea - translational research - is transformed into practice by groups of researchers currently working in academic medical science settings in English universities. Drawing on concepts and theories of science and technology studies (STS) - particularly the approach of actor-network theory (ANT) - the study seeks to enhance sociological understanding of this term by observing how it is extended, accommodated and resisted by researchers in real-world academic settings. This study provides a timely intervention, given slow but steady awareness emerging of a need for improved sociological accounts of this phenomenon (see Chapter 3), not to mention the great importance being attached to this concept in communications between various scientific, medical, public health, policy, industrial and civic communities over recent times (see Chapter 2). On this basis, the research questions to be pursued over the course of this thesis are as follows:

How does the idea of translational research transport into local academic knowledge production sites of academic medicine which policy authors are seeking to enrol?

What aspects of the local production practices are adapted to accommodate this goal and where do resistances occur?

As will become clear throughout this thesis, arriving at a working definition of this concept is not a straightforward task. There is however several familiar narrative threads which typically cluster around the term. The metaphor ‘translation’ is a means of inscribing movement across boundaries. In medical and science policy discourse the boundaries usually being referred to are between the social worlds of the laboratory and the clinic. Indeed the catchphrase ‘bench-to-
‘bedside’ has often been used as a short-hand summarising movements across these boundaries. What warrants attention towards such boundary movement is the need of institutions in ‘post-industrial’ (Bell, 1973), ‘late-modern’ (Beck et al., 1994) capitalist societies to innovate much more successfully than they do at present. To survive into the future institutions will rely on emergence and acceptance of innovations, and capacity for innovation is reliant upon a strong science base (Nowotny, 2008). One of the anxieties which the notion of translational research taps into—especially in the UK—is the failure to ‘translate’ from a strong science base into successful market and clinical innovations. ‘Falling behind’ fears frequently draw on United States and Japan as frames of reference, as these nations have demonstrated significantly greater success in exploiting scientific discoveries for commercial purposes (Freeman and Soete, 1997). In the private sector, in order to remain competitive, pharmaceutical and biotechnology industries will need to successfully exploit novel insights which scientific research— including university-based research—is capable of producing. Governments are drawn towards articulating alignment between university science and this industry because it promises to enhance the health and wealth of populations in the form of breakthrough treatments/diagnostics and economic growth respectively (Nowotny, 2008). Likewise for public-sector healthcare systems to keep-up with technological developments and health challenges of changing demographics, requires efficient means for ensuring rapid translation of safe and cost-effective health innovations (radical and incremental) into routine clinical practice (McAnanay et al., 2010). The category of translational research both points to these priorities and offers itself as a solution. This phenomenon is therefore no simple artefact of scientific curiosity, but carries performative definitions of what publicly-funded medical science can and therefore ought to achieve, and how it should do so. It is proposed that in order for scientific research to attend to this goal, it must develop and demonstrate ways of negotiating and collapsing the boundaries which inhibit ‘movement’ of innovation ‘from bench-to-bedside’ (Wainwright et al., 2009). Translational
research therefore implies the redrawing of boundaries, away from autonomous social worlds towards formation of new interactions and styles of practice between bench and bedside.

One of the most striking features one notices of translational research is its sheer plasticity: it is a term that appears highly transportable across communities with particularly diverse interests and institutions. Part of the challenge of studying this phenomenon therefore is deploying and developing tools which can adequately map-out and make sense of the mobility and durability of this idea as it moves across diverse network spaces. Deploying appropriate tools is one way in which this thesis advances sociological knowledge of translational research. Another is by providing an account of the real, mundane struggles that are experienced within a key population expected to shift this category ‘from rhetoric to reality’: academic researchers. Rather than being a unified whole, today contemporary science is usually thought of as being characterised by quasi-autonomous fields with distinct ‘epistemic cultures’ (Knorr-Cetina, 1999).

The comparative scope of the design in this thesis makes it especially well equipped to capture how a dis-embedded framework like translational research relates to quite diverse epistemic and institutional arrangements across the social worlds of medical academic research. With such concerns in mind, the main aim of the thesis is to explore how translational research is accommodated and made ‘workable’ in different research settings and report on the kinds of problems that have so far emerged in respect to pursuing this goal.

As this phenomenon has at the core of its concerns problems regarding innovation, it is logical to bring into the study sociological theories of innovation. From here there are a number of different models for studying innovation, many of which have been attacked by writers like Latour (1987). One such approach, associated with earlier trends in history of science and technology, is to link successful innovations back to minds of great individuals, usually with particularly special sets of entrepreneurial and/or scientific-engineering skills. A second- not always mutually exclusive- explanation credits the ‘diffusion’ of an innovation with inner
qualities of the innovation itself. This account has been commonplace in explaining for instance, the emergence and stabilisation of scientific facts and machines at the expense of rival products. As sociologists and historians of science over the last three decades have convincingly argued, neither of these provides good versions of innovation processes. One problem is that they are ‘Whig History’ forms of storytelling which are provided by the winners (Latour, 1987). Versions of ‘discovery’ and ‘invention’ are also very bad ways of reporting what scientists and engineers do because they leave invisible much of the effort, skill, and cost which characterise the work practices of these cultures (Woolgar, 1988). A third alternative provided by science and technology studies (STS) introduces constructionist sensibilities in order to account for the success of innovation. These versions typically emphasise the practices which go into making innovations workable, rather than relying on explanations which privilege either properties of the things themselves or hero-worshipping accounts of successful individuals. To generate these insights the field of STS emphasises the use of close empirical research being carried-out amongst the very sites in which production, development and usage (innovation) occurs (Knorr-Cetina, 1995). Following this third approach, the research in this thesis draws insight from original empirical materials generated through qualitative forms of analysis oriented towards scientists at work.

The fieldwork for this thesis was conducted over the course of 2011 (Chapter 5) and the writing-up of these materials into empirical chapters during 2012. Prior to this in 2010 I had conducted a small pilot study amongst clinical researchers in another university (Chapter 5). Drawing on these theoretical and primary empirical resources has enabled me to produce a version of the phenomenon which errs away from heroic or hyperbolic accounts, towards real, specific, situated accounts of what it is like for academic scientists to work with translational research.
Overview of Thesis Chapters

Having introduced the topic of this inquiry, the next task is to set-out the context in which the idea of translational research has emerged to prominence over recent times. The second chapter provides an account of why this term is seen as important and by whom. This chapter also considers the various meanings that have been attached to the term. This then provides the basis for treatment of the concept throughout the rest of the thesis.

Before embarking into a research journey, one should consult existing knowledge about the terrain they wish to explore. The third chapter begins with a review of the sociological literature which has been used to inform this research and to which the research in turn is intended to contribute. The second part of this chapter then elaborates further the primary theoretical approach - Actor-Network Theory (ANT) - that has been brought to bear on the phenomenon. Once a general overview of this approach is provided, I will outline a set of sensitising concepts that were central to performing data analysis and presentation. These were drawn in the main from Latour and Woolgar’s (1986) influential monograph *Laboratory Life*, with support from other relevant STS literature studying scientists at work.

Having delineated the existing knowledge that has been brought into this study, I will start describing how the research has been carried-out and why certain tools and resources were thought useful and appropriate in doing so. This begins in Chapter 4 with an overview of the philosophical perspectives underpinning theories and methods of this thesis. The overview is followed by explication of the research design (Chapter 5), first in terms of the research methods and standards that were borrowed from text-books to inform the empirical analysis, then in terms of the actual practicalities of designing and performing the research.
The main empirical material is organised around three case studies, each concerned with a discreet research group based in medical schools hosted by English universities. Each case study is given its own separate chapter (6-8).

Finally the concluding chapter (9) briefly summarises what has preceded, before embarking on a discussion of the main empirical findings, and indeed, what it is these contribute to theoretical and policy understanding of translational research. Included also in this chapter is consideration of the relative strengths of the theoretical approach adopted in this thesis, both for this particular project and even for future social science studies of translational research.
2. Putting Translational Research in its Place

Introduction

This chapter aims to provide an overview of the context in which translational research has emerged in English science and health research policy over recent times. It deals both with issues of importance that are attached to the concept, that is why it matters and to whom, before setting-out how the terms has variously been defined by a number of interested parties. This provides a basis for sketching-out how the term will be used in the context of this study.

1. Background: Locating Translational Research in the UK- Old Wine and New Bottles

One of the main reasons that the institutions of science hold such a grip on the modern world is that they are seen as indispensable to the growth and sustainability of modern societies, as the knowledge of societies is seen as dependent upon having a strong scientific base (Nowotny, 2006a). Since the post-war period, the central condition for public subsidy of science- that it can and indeed should help to tackle the ‘grand challenges’ of society- has remained largely intact. However, what has changed are ideas about how this can be achieved (Rip, 2011). There have been a number of efforts to re-negotiate the science-society contract over this time-frame. These have evolved between the laissez-faire ‘linear model’ of the immediate post-war period with its efforts to separate applied from basic research, and later attempts to open-up spaces between basic and applied science (strategic science). Whichever strategy has been tried, policymakers have long struggled to tackle the troubled trajectories between promising scientific
knowledge and its application. An ‘innovation paradox’ – where a strong science base is perennially undermined by inability to capitalise on knowledge transfer – has long been acknowledged as a perennial problem amongst European policymakers (Freeman and Soete, 1997, Willetts, 2010). Typically this narrative is repeated in juxtaposition to successes regularly achieved by rivals in the U.S. and Japan. Translational research is arguably the latest in a long-line of innovation concepts proposed for tackling this deficit in the arenas of medical research – the most funded and arguably most prioritised area of the sciences today.

As will be made apparent throughout this thesis, translational research does not hold a fixed set of definitions in place with regards its meaning. Yet I will try now to set-out for the reader some of the problems and narratives which are regularly associated with this term. This in part introduces translational research as a social problem rather than merely a narrow and esoteric scientific one.

**Funding Science: The British Context**

In the UK, successive governments have sought to position themselves as venture capitalists backing research opportunities and initiatives with an expectation of them leading to social and economic pay-offs (Etzkowitz, 2008). Typically, like most Western governments, they have been more likely to support high-tech innovation, as this is thought to hold the promise of bringing radical rather than incremental change in response to convergent problems faced by late capitalist states (Nowotny, 2006a). Since World War II there has been a patchwork of different approaches utilised by British policymakers, similar to those followed in other countries. The interdependence between science and the state has been an enduring feature of their relationship, however, so has uncertainty in each camp about how best to coordinate and mutually control the other’s activities (Louis and Anderson, 1998, 86). Two broad caricatures have emerged in science policy literature to account for the changing relations between how
science funding has been managed, both of which variously describe the British experience (Freeman and Soete, 1997).

In the first of these governments provide funding for scientists whilst granting them autonomy to pursue their substantive interests. This is based on normative assumptions about scientific knowledge and institutions as a public good. It is often thought to characterise the organisation of Post-War public science systems, particularly in the United States. This model was expounded most notably in Vannevar Bush’s oft-cited address to President Roosevelt (Bush, 1945) and found normative vindication in early sociology of science writings (e.g. Hagstrom, 1965, Merton, 1973). Within this schema, authoritative communications take on largely horizontal forms and scientists can exit from one research interest to another with relative ease (Whitley, 2011). This ‘linear model of innovation’ typically separates basic from applied research, with the former referring to research questions not primarily oriented towards application, and the latter being driven by specific practical-oriented questions (Stokes, 1997, this separation habitually reappears, see for instance Cooksey, 2006, 13-14). Linear model reasoning sees these domains as involving different competencies and thus requiring separate institutions. For instance, in UK and France for long periods applied research was manifested in public R&D institutions through the setting-up of large government laboratories, with universities, based on 19th Century Humboldt models, positioned as an enclave for autonomous basic research (Laredo and Mustar, 2004, 21). As a standalone working model, the ‘frontier’ approach has been out-of-favour for some time. There are a number of powerful criticisms that have undermined its framework, for instance regarding the lack of accountability it can lead scientists to exhibit (Dasgupta and David, 1994, 488), and its reliance on overly-simplistic understandings of technological innovation flowing seamlessly from basic research (Balconi et al., 2010, Godin, 2006).

The second approach takes a somewhat different tact. Given the costly nature of much basic research and lack of predictability about how and when it transfers into technological
innovation, governments seek to create and foster institutions which prioritise material needs and maximise efficiency and efficacy of scientific research deriving from public funds (Dasgupta & David 1994, 488). In biomedical research in Britain—often taken as a key exemplar of science-society trends (Nowotny et al., 2001)—this has involved various policy efforts seeking to predict and control where investments in basic science should best be made (Comroe and Dripps, 1976).

Put simply:

“The cornucopia of science contains many fruits, and the challenge is to pick the best of them.” (Van der Meulen, 1998, 398)

The notion of strategic research suggests scientists should provide a strong platform of excellent knowledge from which others in the innovation system are able to innovate (Irvine and Martin, 1984, Rip, 2002). An effect of strategic research initiatives is the blurring of the line between basic and applied research. This interventionist model is characterised by vertical authoritative communications, strategic interests of governments (not the substantive interests of scientists), and relative difficulty of exit for scientists (irreversibility) (Whitley, 2011). An early articulation of this discourse was evident in Lord Rothschild’s 1972 report to the UK Government, which sought to establish a customer-contractor relationship between government and science (Rothschild, 1971). Similarly, the notion of principle-agent games, whereby governments play the role of principle (buyer) whilst researchers play the role of agents (seller) captures varying strategies employed by governments and research institutes in pursuit of their respective interests (Morris, 2003).

Efforts to stimulate the relationship between the science base and industry had been occurring since the 1980s, notably with the Alvey Programme, which introduced foresight as an instrument into technology policy, which recognised implicitly that “the UK science base could no longer pretend to support the full range of opportunities, and hence that prioritisation would be
necessary’ (Sheen, 1998, 250). The use of foresight co-evolved with the rise to prominence of a new research category of strategic science (Irvine & Martin, 1984). This ‘prioritisation’ was reinforced over the next decade, most notably in the 1993 White Paper on Science, Engineering and Technology, in which research councils were given mission-statements to support research and post-graduate training linked explicitly to national goals (wealth creation and quality of life) (Sheen, 1998, 260). During the 1990s the Department of Trade and Industry (DTI) saw an increasing university-industry nexus as key to repairing the technology gap in British industry, particularly its manufacturing base (Sheen, 1998). Tellingly, over this period, the DTI took over from the Cabinet Office the task of coordinating the Office of Science and Technology (OST) (Georghiou, 2001, 259), signalling the continued efforts to align public science with national economic and social needs. Despite almost two decades of efforts to make public science conducive to industrial innovation, the 1996-1997 Dearing Committee raised again perennial concerns about the need to sustain funding of civil R&D and university funding in order to remain nationally competitive (Georghiou, 2001, 260-261). The positioning of the science base as the engine of economic growth and competitiveness at the national level was a theme continued by the New Labour government who took power in 1997. Their 1998 Comprehensive Spending Review increased funding for science and technology, infrastructure, and outlined the costs of missing out on waves of new genomic discoveries as it had done in computing technology (ibid. 261; Nelson and Rosenberg, 1993). The 1998 White Paper Our Competitive Future: Building the Knowledge Driven Economy, also signalled a shift towards the biosciences, the area of science linked in to UK’s most successful industry (pharmaceuticals), that promised significant high-tech advances and that was lobbied for by the Wellcome Trust, an influential medical research charity (Georghiou 2001, 293).

Despite a robust theoretical distinction between laissez-faire and interventionist models, both these logics come simultaneously to shape how successive British governments have invested in
science. The approach of ‘muddling through’ (Lindblom, 1959) has been used to tackle the ‘basic
double-edged problem of how to get policy interested in the conduct of science, and how to get
science interested in the problems of policy’ (Van Der Meulen, 198, 398). Although there are
strongly interventionist components to the funding of scientific research in many parts of the UK
research system, governments have rarely sought to micro-manage the minutiae of governance
or research. Institutions of science are granted autonomy, with the caveat that they produce
‘excellence’, value-for-money\(^2\) and their outputs promises to lead to (high-tech) innovation.
Institutions such as citation numbers, university rankings (e.g. Shanghai Rankings), and Nobel
Prizes, have been appropriated/developed by government agencies and public organisations as
tangible measures of countries innovative capacities (Rip, 2011). These may be read as new
public management-type governance initiatives aimed at steering ‘grass-roots’ research from a
distance (Ferlie, 1996). In this context, research councils have been enrolled as particularly
important ‘intermediaries’ in relaying governments’ various messages to researchers (van der
Meulen and Rip, 1998). Together research councils’ institutes and universities constitute the
science base- i.e. the ‘research performing sector’ of the British economy (Georghiou, 2001,
256). Located at the interface of government and research, research councils’ role is seen by
governments as ‘steering’ the public sciences towards innovative and enterprising research.
Research councils have typically set-up strategic portfolios around important promissory areas of
science which they wish to foster (Dasgupta & David 1994, 505). These initiatives are often
captured under ‘umbrella labels’ uniting a diverse range of intellectual, cultural and instrumental
interests around formalised agendas (van Lente and Rip, 1998b). Within these spaces research
proposals compatible with strategic agendas of funders have an explicit advantage. As such,

\(^1\) From this one might expect that the more interventionist notion of translational research will not be the
only discourse at work in the current landscape of British science policy.

\(^2\) An example being RCUK’s introduction of new public management-type initiatives like full-economic
costing of research.
portfolios provide protected spaces in which promising labels become more-or-less self-fulfilling (ibid.).

It is argued that there are no ‘natural’ boundaries between categories like ‘basic’, ‘applied’ ‘strategic’, ‘translational’ research: they are ideological constructs that carry certain interests, assumptions and expectations about how science works and what it should contribute. This follows recent revisionist arguments in science policy studies, for instance questioning the extent to which the post-war ‘linear’ model was in fact ever an empirical reality. Following Callon (1994), Croissant and Smith-Doerr argue instead this Cold War-era configuration was a ‘boundary-making claim, a linear narrative to try to separate university research from the market, rather than reality’ (Croissant and Smith-Doerr, 2008, 702). Further support for this constructionist thread can be found in Calvert’s study of basic research against in the context an evolving funding backdrop, in which she found ‘boundaries between basic research and other activities are being actively contested because of increasing pressures for applicability in scientific research’ (Calvert, 2006, 200). But to say that these boundaries are constructed does not mean to say they are entirely fictional or ethereal3: these ideologies have had performative effects on the organisation of science, some of which are still present in today’s research system (Croissant and Smith-Doerr, 2008). As such, one might typically expect to find in ‘grass-roots’ academic settings a pragmatic co-existence between different ‘institutional logics’, not the entire displacement of one logic by another (Gibbons et al., 1994, Swan et al., 2010).

Translational Research in English Research Policy

Historically funding bodies like Medical Research Council (MRC)- and the medical science profession more generally- enjoyed some autonomy from government (Salter, 2004). However this arrangement, sometimes referred to as the Haldane Principle, has arguably given way in

3 “Robustness is not an absolute concept, nor is it a relative concept either. It is a relational concept.” (Nowotny, 2006a, 5). In other words, it must be achieved (Latour, 2005, 138).
recent times to further government intervention (Georghiou, 2001). The erosion of this autonomy arguably reflects how state dependent institutions of ‘big medicine’ and biomedical science (and their promises for radical innovation) and public health more broadly have become increasingly indispensable to policymakers anticipating the impact of ageing populations and prevalence of chronic illnesses on public health care systems (Lowy, 1996, 52).

A number of recent policy and funding initiatives introduced by government agencies are clearly sensitive to the perception of an innovation paradox. Establishing funding and institutional innovation to foster translational research has become a major priority of science/health research councils and charities in the UK. This more interventionist push has meant that other areas of the innovation system have been forced into considering this pathway, such as the UK’s NHS and university sector (Kaye et al., 2007, 739). In the context of national science/health research, explicit pronouncements about translational research as a key priority in future years for UK research institutes, including the NHS, can be traced back to at least 2003. Here the publication of the Department of Health’s *Genetics White Paper*, prepared the ground for what was to follow, as did the government’s 2004 *10 Year Science and Innovation Framework*, as both sought to position the NHS as a key research actor with whom academics and industrialists in the UK could utilise, particularly in translating frontier research into clinical trials.

This was followed-up in 2006 with an Independent Report to the government authored by Sir David Cooksey, an eminent businessman, followed shortly the governments’ very own *Best Research for Best Health* published in 2007. The Cooksey Report sought to reorganise the whole structure of health research funding in England, in order to consolidate ‘excellence’ of basic and applied research, but also to build capacity for translational research (2006, 2-3). This has triggered a number of institutional innovations, including the establishment of NHS Innovation Hubs to support licensing and patenting of promising new therapeutics and diagnostics (Kaye et al., 2007, 741). The MRC’s strategy to meet these demands is premised almost exclusively on
translational research (Cairncross and Dusic, 2008, 17). MRC has in turn modelled its translational heuristic on the T1 and T2 models of the American NIH. T1 and T2 refer to identified ‘roadblocks’ in the translational research pathway (Table 1). Having being formulated at the Institute of Medicine’s Clinical Research Roundtable meeting in 2003, this agenda has been taken-up subsequently in NIH’s Clinical and Translational Science Award (CTSA) program (Woolf, 2008, 111). It is notable here how narratives shaping strategy in one national context (United States) travel and have the propensity to influence others⁴. There appears strong anecdotal support here for the argument that late-capitalist states engage in strategic ‘racing’ games (van den Belt and Rip, 1987) along similar technoscientific trajectories in response to ‘convergent’ sets of problems, such as healthcare and employment (Bell, 1973, Drucker, 1993). Evidence of mimesis in the UK is further reinforced by shifting organisational routines of major research charities⁵ like the Wellcome Trust, which has assembled a Technology Transfer portfolio premised upon developing ‘promising translational initiatives’. Operating from an annual budget of approximately £60m, the main aim is to bridge the gap between promising fundamental research and commercially successful medical innovations which benefit society (see Figure 1)⁶. Funding is allocated amongst academic groups, companies, and partnerships initiatives between these two institutional spheres. The full range of initiatives to have been funded by the Technology Transfer to date is listed publicly on the Wellcome Trust’s website, under a page revealingly named ‘Technology Transfer Showcase’ (Wellcome Trust, n.d.a). This portfolio of unfolding and/or completed initiatives sends a clear signal to researcher audiences that research proposals must chime with their translational agenda. By definition, this sort of outcome is

⁴ National innovation systems can be said to exhibit communication patterns of relatively ‘open-systems’ (DiMaggio & Powell 1983).

⁵ The UK is atypical in that charities account for an unusually high proportion of R&D funding for the science base (£392.7m = 12% in 1997-1998). The Wellcome Trust is by far the most prominent of these (Georghiou 2001, 257).

⁶ The focus is, for the most part, on pre-clinical initiatives oriented towards unmet medical needs, with some funds also going to research at a clinical stage.
expected to require levels of expertise and knowledge reaching beyond the capacities of single disciplinary confines. The requirement therefore is that applicants are trans-disciplinary in structure.

The move towards translational research was also impressed as a key priority at the National Institute of Health Research’s (NIHR) inception in 2006, which was set-up in order to unite and promote government funded research in the NHS (DH, 2007). Whereas much of the translational emphasis for MRC was on basic biomedical research, for NIHR the emphasis is more on translating applied forms of knowledge into clinical practice (Cooksey, 2006, 7). The joint translational priority across MRC and NIHR is evidenced by the setting-up of a single health research budget shared between these two public bodies. This is overseen by Office for Strategic Coordination of Health Research (OSCHR), an independent office set-up at interface of the Departments of Health and Trade and Industry in order to manage the government’s health research strategy. Given the related interest in translational research, some consternation was expressed as to whether these two bodies (which together encompass the entire health research spectrum) should remain separate (Cooksey, 2006, 7).

Since 2008, a series of collaborative projects have been set-up in order to support rapid translation of basic and clinical research, named Biomedical Research Units and Academic Health Science Centres respectively. Such high-profile investments signal to the academic research community that translational collaboration, particularly involving clinical trials with the health service, is seen as a priority in the current research landscape. In 2010, despite a new government and global economic crisis over the intervening period, Britain’s coalition government continued much of the rhetoric and priorities of New Labour’s support for conspicuous investment in translational research as a motor for long-term economic growth. The Comprehensive Spending Review of 2010 ring-fenced the science budget in real-terms over a five-year period and increased spending on health research in the NHS, for example through
allocation of £775m worth of funding across the 11 Biomedical Research Centres (Gibney, 2012). Likewise in 2011 Sir David Nicholson’s Innovation, Health, and Wealth report set out plans to support existing Academic Health Science Centres with new Academic Health Science Networks, which together it is claimed ‘will identify high impact innovations and spread their use at pace and scale throughout their networks’ (DH, 2011, 19), thereby prioritising once again the need to stop breakdowns of innovations into routine clinical practice.

This brief overview captures how over the recent times translational research has emerged as an increasingly prominent institutional pressure surrounding university-based researchers working within a very broad spectrum of health research in England. Indeed shortly after these research reforms prioritising translational research were introduced, an article was published expressing concern that narrowing focus around high-tech biomedical research in the NHS presented grave opportunity costs for other areas of health-related research such as family practice (Shaw and Greenhalgh, 2008, 2518). This serves as a reminder that such labels are not neutral, but carry ideologies and assumptions which have potential to court controversy as they interact amongst different epistemic communities.
Figure 1: Wellcome Trust’s Depiction of the Translational Research Process
(Translational Awards)

Source: Wellcome Trust, n.d.b
<table>
<thead>
<tr>
<th>Aim</th>
<th>T1</th>
<th>T2</th>
</tr>
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<tr>
<td>Develop new intervention methods from knowledge based on laboratory findings; Ensure safe first testing of interventions in humans</td>
<td>Bring T1 results to the clinic</td>
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<th>Setting</th>
<th>Laboratories, clinical research facilities</th>
<th>Community and Ambulatory Care Settings</th>
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| Investigators | Appropriately trained clinical scientists adept at using cutting-edge technologies | Health care practitioners; Administrators; Academics |

| Required Skills | Molecular biology, genetics, basic science skills | Implementing and evaluating clinical science in ‘real-world’ settings; Understanding multiple disciplines informing intervention’s design |

| Main Challenges | Biological and technological problems; Trial recruitment; Regulation | Human behaviour; Organisational inertia; Infrastructure and resource constraints; Proving effectiveness amongst ‘moving targets’ |

2. Locating and Conceptualising Translational Research in the Thesis

The immediately obvious point to make having studied this topic for two years is that I have struggled to find a common, agreed-upon definition of this term. Perhaps this is not surprising given what one commentator has observed:

“Translational research means different things to different people, but it seems important to almost everyone.” (Woolf, 2008, 211)

This is arguably due to translational research being a socially constructed discourse, rather than simply a ‘fact or normative attribute of contemporary medical knowledge-production’ (Wainwright et al. 2006b, 2053). One of the first observations to make of this concept is that it appears ‘indexical’ to certain networks. In the ANT lexicon this means that it is a term that has been stabilised, has presence and purchase within some networks and not others. The types of networks I refer to here can be associated with those networks of practices associated with what sometimes gets referred to as the ‘medical-industrial complexes’ of late modern societies. This positioning points towards several goals around which the label translational research is mobilised in contemporary innovation systems. In addition to offering a clinical utility, it is claimed translational research can also refer to civic and commercial benefits as aims from funding public health sciences (Summarised in Figure 2) (Lander & Atkinson-Grosjean 2011).
One of the more confusing aspects of studying the recent turn towards translational research is that it appears to have existed for some time, notably within the epistemic practices of biomedicine\(^7\) in the 20\(^{th}\) Century. Biomedicine is a term (loosely) denoting research that lies at the interface of strictly biological and strictly clinical research (Cambrosio et al., 2006a, 3141).

The latest emphasis on translational research can be read as an attempt to proactively foster stronger linkages between biomedicine’s biological and clinical worlds, premised on the earlier successes this field has produced. This historical bifurcation has been described succinctly by Cambrosio et al:

> “Alternative representations describe translational research as a bridge connecting two different worlds [biology and medicine] that only occasionally meet in an uneasy partnership... or as an emerging interface between the laboratory and the clinic that

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\(^7\) For the record the earliest known usage of the ‘bench-to-bedside’ notion was traced back to an Editorial in the *New England Journal of Medicine* in 1968 (Feldman 2008, 2).
should become a distinctive sphere of activity in its own right.” (Cambrosio et al., 2006a, 3140)

Such changing dynamics of translational research point towards an emerging shift in the epistemic cultures of 21st Century biomedicine: from one where laboratory and the clinic were bonded through coincidence and affinity, to one in which they are increasingly becoming bonded through necessity (see Knorr-Cetina, 1999, 1). What appears certain is that the label appears to have gained a new lease of life in medical and science policy discourse. Despite being sceptical of the notion that there is a single or historically constant definition of translational research to give the reader it is nonetheless helpful to identify some point-of-reference in which to enter into exploration of this topic. Arguably the most prominent definitions and discussions of translational research available in English policy context is one captured in the Cooksey Report, and refers to the process of moving discoveries out of basic and clinical research towards clinical application (Cooksey, 2006). This definition separates translational research from other categories of research such as basic and applied. In this discourse translational research plugs or bridges ‘remaining gaps’ between bench and bedside, thus building on basic and applied capabilities (Burgess and Tracy, 2011, 30). Images of interrupted flows are powerfully deployed in such propositions.

Within the new regime of translational research (which this study is primarily interested in), there are two main representations of translational research. The first refers to the process of moving basic scientific discoveries made in laboratories towards interventions in humans. This is referred to by NIH and MRC as T1 translation (see Table 1). This definition is similar to that routinely found in industrial contexts of drug development. In this sector translational research (or ‘translational medicine’) is said to have great commercial promise in reducing delays between discovery/proof of principle and beginning clinical trials (Hurko and Rutkowski, 2005, 31). Hitherto within the health research landscape, most of the focus and investment is said to
have been on T1 translation. This is perhaps due to the anticipated pay-off from large sums of investment put into developing basic science capabilities in fields such as genomics, proteomics, gene transfer, stem cell biology, structural biology and imaging into clinical investigation (Feldman, 2008, 1). Likewise it could also signify the relative power that certain (hi-tech) research fields have in courting funding, partly derived from their commercial linkages and partly from the promising allure of these prospective interventions hold amongst investors in science (Shaw and Greenhalgh, 2008, Brown and Michael, 2003). Either way, to date it is largely accepted in policy discourse that too much emphasis has been placed on T1 translation of discoveries into clinical experimentation and too little emphasis on T2: adoption into clinical practice (Contopoulos-Ioannidis et al., 2003, 433).

The second model to delineate then is one which encompasses the whole bench-to-bedside continuum, incorporating both T1 and T2 processes. It can be read as a strategic response to the propensity for T1 developments to be ‘lost in translation’ (Lenfant, 2003). The focus of the NIH’s and MRC’s strategy around these two prongs is an explicit response to this perception (Cooksey, 2006). A number of prescriptive models have emerged which seek to prioritise the main development ‘phases’ needed in order for the translational promise to be realised in promising fields like genomics (Collins et al., 2003). A typical response has been to focus on different areas of priority in the translational continuum. These are typically demarcated into series of (largely stepwise) phases, which include:

1) Movement of biomedical research into diagnostics/therapies
2) Development into evidence-based protocols
3) Development into clinical practice
4) Evaluation of real-world impact on health (Khoury et al., 2007)
Although these displays for demarcating the social actors and processes involved in translation appear somewhat linear and overlook the presence of feedback, they are nonetheless helpful in terms of pointing towards prominent efforts by the medical and policy communities to map-out boundaries of the assemblages partaking on this general task. This broader ‘bench-to-bedside’ definition of translational research is what ‘macro-actors’ in the UK research and innovation system are currently attempting to project upon the world of academic health research. These models now appear to reflect standard ways of framing the translational challenge\(^8\), meaning it is this second definition with which my study seeks to engage. Social studies of translational research (reviewed in next section) describe this literature as concerned with the analysis of ‘bench-to-bedside dynamics’ (Wainwright et al., 2009), however most of the writings associated with this problem focus upon T1 forms of translation. In following the scope of the phenomenon set-out by Cooksey, Khoury and others, I intend to incorporate T2 versions of translation into the study as well, thereby following the updated definitions of bench-to-bedside which social studies of translation should begin to take seriously (McAneney et al., 2010).

**Conclusion**

This chapter has provided an account of translational research within the context of English science and medical research policy and sought to articulate the kinds of meanings which have been attached to it through various sources. Even from this short description, it will have become clear that this is no simple task. Posed rhetorically, if there is no clear consensus about what translational research means within the populations I study, then why should I, the researcher, seek to impose one? Following a pragmatist strategy, I instead propose that a more productive approach is trying to look for what the term is being made to do, for what purposes and in what circumstances. The pragmatist tools introduced in this thesis to engage with this

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\(^8\) For this reason Khoury’s model is taken as inclusion/exclusion criteria in sampling the main cases in this study (see Chapter 5).
task- labels/metaphor, boundary objects, and immutable mobiles- are set-out in the literature review. It is important to qualify that definitions from within the world of policy and research are treated as useful entrance points to help design the empirical studies in this thesis, rather than fixed statements about what translational research ‘really means’. It is suggested that following this approach offers an attractive way-out of what would otherwise be a rather frustrating and fruitless intellectual cul-de-sac.
3. Translational Research and Social Science Literature: A Review

Introduction

The aim of this chapter is to review the literature used to inform the study of translational research in this thesis. Clearly one such objective of such a review should be to report on what has been said in previous sociological literature on the topic, pointing out which aspects have set-out an agenda for the research in this thesis to follow and which areas have been left open for potential improvements. One such general improvement I suggest is to pull-in knowledge emanating from a much larger body of research pre-dating and paralleling studies of translational research: what is dubbed here ‘re-contextualisation’ literature (following a term coined by Nowotny et al., 2001). The first part of this chapter therefore is dedicated to reviewing literature on ‘re-contextualisation’ and sociological studies of translational research. The second part turns to the areas of STS literature brought-in to help answer the research questions and build on existing stocks of knowledge with regards this topic. This includes an overview of the relevant parts of the actor-network approach, research on boundary-forming activities of science, and a set of specific sensitising concepts drawn from STS laboratory ethnographies, in particular Latour and Woolgar’s (1986) *Laboratory Life*, in many ways a founding text in the ANT tradition.
1. Substantive and Theoretical Contributions towards Translational Research

To date there has not been a wealth of literature produced about translational research. In one of the few studies to have directly engaged with the topic, Wainwright et al. (2009), speculate that one reason for the relative absence of such studies could be that it is a relatively recent phenomenon. On a provocative note, I would speculate that to date the term has proved somewhat unwieldy and off-putting for social scientists because it has not been particularly well conceptualised. This is something I hope to put right through the thesis. In this review I identify with two types of ways that sociological literature engages with translational research. A first is included because it has been helpful in making me think about translational research and points to ways in which the problems addressed in this thesis can be of interest to audiences interested in studying the interactions between science and its context. However, the term translational research is not given explicit mention in these works. The second constitutes the most important body of literature as it is the one to which I aim to make a direct contribution: sociological studies which have addressed this topic explicitly. I shall now provide an overview of each of these.

Scientists, Innovation and ‘Re-contextualised’ Science

The first type of literature drawn on during research on this thesis addresses implicitly debates and concerns which are typically associated with the term ‘translational research’ without actually using the expression as a marker for their work. This category encompasses vast arrays of literature from across a number of social science fields. Themes emanating from this literature have fed-into my thinking about translational research and how to go about studying it, which is therefore why I provide a brief overview of particularly key contributions.
One such area of work provides theoretical and (some) empirical attention to the changing institutions of academic science (reviewed in Hessels and van Lente, 2008). STS Laboratory studies have rarely accentuated the influence of the university as an institution in its studies of scientists. By contrast ‘re-contextualisation’ literature has placed particular emphasis on the role articulated by the university in society and how this has transformed over particular historical periods. In short, this has meant the Humboldtian model of the university as an institution supporting pure inquiry and training has been replaced by a more near-market institution acting as, amongst other things, an economic agent in society. This has shifted certain changes in knowledge production practices, most famously the claim that academic science is moving in a direction away from Humboldtian ‘Mode 1’ towards new forms of ‘Mode 2’ production (Gibbons et al., 1994; Nowotny et al., 2001). Work on ‘re-contextualisation’ of science has run in parallel to studies of translational research, but rarely have they intersected. This is perhaps beginning to change, with one recent article drawing on innovation systems theory in order to open-up understanding of the phenomenon of translational research (Lander and Atkinson-Grosjean, 2011), although so far this is an exception. Works in these various traditions – Mode 2, triple helix, innovation systems and so forth- have focussed in part upon how new and old institutions interact in the ‘ivory towers’ of academic research in universities, for example, in strategic responses of academics towards meeting pressures like commercialisation and relevance. Of the former, studies have observed the strategies and boundary work tactics exercised by scientists reconciling new institutional pressures of commercialisation with older Mertonian-type norms associated with academic science (Webster, 1994, Etzkowitz, 1998, Cooper, 2009). Likewise, studies have begun to focus upon how academic scientists incorporate struggles for relevance into their practices (Rip, 1994, Hessels et al., 2009). These issues have become pronounced as the Humboldt model of European and North American universities as an enclave for pure research and teaching has slowly begun to lose ground to more entrepreneurial aspirations and cuts in public funding (Etzkowitz et al., 2000, Etzkowitz, 2002), which one might equate with a
general rise in neo-liberal politics (Slaughter and Rhoades, 2004). I would cite such sociological investigations into institutional changes in academic sciences as influencing the way my research questions have taken shape. However, some of the most influential theoretical contributions on this topic, especially writings reporting ‘Mode 2’ changes, have been criticised as overly-reified claims based on little primary empirical evidence (Weingart, 1997, Shinn, 2002). To compensate, using a concept like translational research, which is member-generated, helps one to address some of the general themes captured by these ‘re-contextualisation’ arguments without falling into their particular representational traps. Indeed evidence for this neglect can be read from a chapter and edited work one of these Mode 2 authors (Nowotny, 2006a), which mentions in passing the need for scientists to demonstrate translation, without providing any further substantive or theoretical elaboration on this cursory observation. These studies would benefit from taking seriously the performative role of metaphor and language in how scientists describe their activities, particularly as institutions of science such as the university appear to undergo significant transformations.

Another set of studies useful for my understanding have been ones which focus upon networks of practice involving academic scientists, clinicians, policymakers, and other interested parties. Some of this work is historical in scope, focussing for instance on clusters of institutions which emerged post-war at the interstices of science and medicine to form new practices of ‘biomedicine’ (Löwy, 1996). Such spaces in which biology and medicine meet and interact were labelled ‘biomedical platforms’ (Keating and Cambrosio, 2003) in which ‘bioclinical collectives’ come together (Rabeharisoa and Bourret, 2009). These studies point to the importance of various amalgamated institutions – theories, experiments, artefacts, practices, infrastructures - for facilitating translation between bench and bedside. But these historical studies take focus away from academic settings and focus more on hospitals as sites of research.
Also focussing on networks of practice have been real-time studies of (expensive) efforts to forge strategic research partnerships between biomedical scientists, industry and health services. These institutions appear less immediately identifiable with the norms and discourse of academic science, but more with institutional logics of triple-helix type initiatives. Studies in this vein typically reveal the struggles that were faced in efforts to translate research within these hybrid networks. One set of articles on science-policy collaborations in the UK Genetics Knowledge Parks scheme showed that the stability of agreed-upon definitions of scientific objects are often fragile and can change over time, as events and shifting expectations lead to the ontological re-specification of research problems attributed by certain actors (Robertson, 2007, McGivern and Dopson, 2010, Swan et al., 2010). In Canada, Atkinson-Grosjean (2006) focused upon difficulties faced in forging short-term and lasting relations within strategic science policy networks called Centres of Excellence. The focus on ‘merchant’ or ‘translation’ science was made explicit by policymakers and industrialists at the outset of these projects, and, ultimately, failure to live-up to these heightened promises, which she claims led to their termination.

Focussing on the same initiative, Lehoux et al. (Lehoux et al., 2008, 2010) described difficulties in conducting ‘epistemic conversations’ within such hybrid spaces, which they attribute to different political interests and epistemic cultures of those involved. Theoretically these epistemic conversations resemble the types of pidgins and creoles which must typically emerge in order for exchanges to occur in the processes of creating and articulating ‘trading zones’ (Galison, 1999). Although somewhat intellectually derivative, where these real-time studies on networks of practice make a useful contribution is in pointing out that forced interaction is not necessarily a recipe for successful translation, suggesting more reflexive thought and effort is needed. In studying these novel contexts these studies reaffirmed the utility of existing sociological theorising on innovation which states how capacity to negotiate local coordinations whilst attaching different global meanings to objects is key to communication (and indeed success) within hybrid spaces (e.g. Star & Greismer, 1989; Galison, 1999). Unlike theorists like Galison
however, they do not focus upon the importance of actual physical sites in which production and interaction between the two worlds meet. The studies also argue that ‘academic culture’ is a cause of much inertia in these spaces without articulating in any great depth what practices constitute and extend this form of culture and how exactly these inhibit translation. Notably absent from these discussions is the institution of the university and the agendas it impresses upon research.

Although not stating translational research as an explicit topic of interest, I find much of these diverse forms of literature informative because through studying science-innovation in action, it serves as a reminder that science and technology are cultural artefacts built through processes of social struggle. This then reinforces the appropriateness of introducing STS resources to study translational research in mundane practices. For my part it is hoped that scholars interested in institutional changes within academic sciences will find this study useful and informative.

**Social Studies of Translational Research**

The Second type of literature actually takes translational research as an explicit topic of concern. These types of studies are considered to be those which aim to ‘foreground the concept’ (Lander and Atkinson-Grosjean, 2011, 537) and/or to explore ‘bench-to-bedside dynamics’ (Wainwright et al., 2009, 41). Reflecting shifts in scope of policy definitions recent studies have adapted the meaning of ‘bench-to-bedside to incorporate T2 forms of translation, as well as T1, into problem area of ‘social studies of translational research’ (McAneney et al., 2010, Lander and Atkinson-Grosjean, 2011). My own study also reflects this shift.

A large proportion of studies which have engaged directly with translational research have been located in the frontier field of stem cell science, which has seen a great deal of social science interest more generally. One feature which was especially informative about this work on stem cells is its engagement with an emerging sub-field of STS sometimes called the ‘sociology of
expectations’ (Brown and Michael, 2003, Borup et al., 2006). The sociology of expectations focuses upon promises around prospective and emerging technoscience as a basis for collective action in the present. The explicit interest in translational research enters studies on stem cells for instance, because the promise of translation is the goal which brings all sorts of social actors together to form ‘communities of promise’, particularly its clinical actors (Wainwright et al., 2006b; Martin et al., 2008). From this it can be inferred that the metaphor translation also holds anticipatory promises within networks converging around many other areas of biomedicine (Martin et al., 2008). It is argued that translational research is neither a fact nor norm of contemporary biomedicine, but an aspect of the discourse (Wainwright et al., 2006b). Although I do not disagree with this position, I find the notion of discourse rather dissatisfying in regards to conceptualising translational research.

In searching for a useful alternative to the rather vague notion of discourse, I found case studies in the sociology of expectations on the emergence of a promising new field of technoscience to be informative (Van Lente and Rip, 1998a, 1998b). These authors point towards labels as devices for constructing and ordering expectations by emerging macro-actors like research councils, who organised new agendas and portfolios around a promissory label called ‘membrane technology’. What this showed was that although labels are often saturated in sexy rhetoric, they are not necessarily inconsequential, as they can mobilise action to change priorities and organisational routines. Research reporting on an emerging ‘triple helix’ of university-industry-government relations (Etzkowitz and Leydesdorff, 2000) have shown how actors associated with each institutional sphere have different interests and means of moderating their interdependencies (Louis and Anderson, 1998, 86). As such, the meanings and expectations which get attached to terms like translational research will become stabilised in different ways over time and as one moves across observing various network points. This makes the prospect of trying to set-out at the beginning one’s study with a concrete definition of translational research a rather daunting
task for the sociologist. Here I propose instead to treat terms like ‘basic’, ‘strategic’ and ‘translational’ research as labels and metaphors which are instrumental: they are seen as helpful for imposing order on a heterogeneous set of practices, whilst, simultaneously promising to bring about change to an existing state-of-affairs which are regarded as unsatisfactory (Czaniawska, 1990). Labels and metaphors are enabling tools, that do not necessarily by themselves cause the advent of a new type of research activity called translational research, but invite a host of other interested actors – university researchers, medical charities, industry, NHS-to interact and collaborate in newly created, largely empty spaces (Rip, 1997). How these labels are ‘filled-in’ is a much more interesting, rewarding and feasible problem to address than ‘what they are’. But Van Lente and Rip’s theorising does not provide a clear toolkit for describing how the term transforms as it is mobilised into action. Therefore in addition to their theorising on labels in technoscience, I suggest introducing terms like boundary object and immutable mobile as means of capturing how the term stabilises, travels and endures (or not) as it is extended through networks into the hands of different people (for overview of these two concepts see the second part of this review chapter). This combined approach enables one to explore performativity of this concept as it relates to real-world practices.

A second contribution in this literature has been the focus on difficulties of forging innovation through networks of practice and institutional barriers towards doing so. These studies have retrieved some of the practical implications from translational research by shining a spotlight on how increasing proximities and interactions between bench and bedside might bring about new tensions, risks, liabilities and boundaries (not just convergences and opportunities). Critically considering these issues is one means in which existing and future social science studies (of which I am counting this thesis) can offer a novel and insightful contribution to both empirical and policy matters of concern. One of the most common observations from social studies of translational research has been that the direction of this process is not simply one-way (bench-
to-bedside), but will also follow a ‘bedside-to-bench’ trajectory (Wainwright et al., 2006b, Martin et al., 2008). For example, in stem cell research, it is thought necessary for clinicians and stem cell scientists to discuss from an early stage what questions are likely to be answerable and lead to useful applications (Wainwright et al. 2006b). Such feedback arguments re-enforce the idea that translational research is an intensely collaborative process (Wainwright et al., 2009). This appears to be a reasonable and convincing point to make. But the story they describe in context of stem cells where policymakers subscribe to overly-linear models, naively assuming there to be a steady-state in the dynamics between basic scientific research, the clinic and the commercial sectors (e.g. Martin et al. 2008, 30) is not one I fully recognise in UK research policy discourse. From my own readings of policy and medical literature, these stakeholders appear to think more reflexively than this. For example, Cooksey Report acknowledged the ‘bedside to bench’ trajectory as being equally valid dimension of the translational research enterprise, as do a number of commentators in medical journals (Hammerschmidt, 2004, 5, Hallenback, 2010). Likewise although there is a tendency to underplay feedback in constructing phase models, there is at least some acknowledgement in these sources of there being potential for overlaps and feedback between them (e.g. Khoury et al., 2007).

Where the contribution of work on stem cells has been generally useful for a study of translational research like this is in articulating how closer collaborations between bench and bedside induce unforeseen complexities in the relations between the worlds of science and medicine than are imagined by linear imageries. Recent case studies have exposed a number of examples of issues and boundaries which have already emerged and could do so further at these intersections. Martin et al. (2008) show how tensions emerged between different constituents in networks coalescing around the promissory object of Haemotopic stem cells, for instance over difficulties of recruitment into Phase II clinical trials. Drawing on Bourdieu’s notion of habitus, Wainwright et al. (2006b) point to institutional differences between the worlds of biologists and
clinicians as a source of tension and reluctance to pursue further collaboration. These case study findings suggest policymakers and other stakeholders should exercise caution towards deterministic rhetoric and grand narratives that translational research will diffuse seamlessly into everyday research practices. Together these micro-level studies suggest social scientists should continue to pursue how boundaries are constructed, experienced and negotiated by agents on-the-ground. Part of the problems which can emerge in relation to the metaphors of translation in areas like stem cell science is excessive hyperbole (Brown, 2003). Indeed in this field speed - moving from lab to clinic ‘as soon as possible’- has been an important dimension of resource mobilisation and collective action (Wainwright et al., 2009, 43). Sociologists can contribute versions of innovation which are not accepting of the hype surrounding a field like stem cells, or indeed emerging technoscience more generally (Michael, 2000).

One of the consequences of there being very few studies on translational research, many of which have focussed on stem cell research, is that the findings naturally lean towards events and difficulties in that particular domain. Embryonic stem cell research promises breakthrough technologies which pose qualitatively new problems of risk for existing regulatory and ethical institutions (Wainwright et al., 2006a). Whether such hurdles to clinical translation exist in similar ways or to the same degree in cases of biomedical science where incremental (rather than breakthrough) interventions are under development seems less certain. Therefore studies of translational research should broaden to include cases dealing with less radical (and hyped) forms of translation. Additionally as the promise of stem cells has, thus far, failed to live-up to earlier promises, there is a risk of translational research being associated with an incommensurability thesis. During my own studies, I became aware that the notion of incommensurability between social worlds involved in stem cell research might not fit so well to the situations of other types of research. Rather than follow a Kuhnian-type thesis of incommensurability (1970), I drew influence from STS studies which look at how collaborations
between social worlds and their realities get forged, focussing especially on struggles to conduct what writers like Galison (1999) and Mol (2002) call ‘coordination work’.

A key article in support of this tact in the sociological literature was written by Lander and Atkinson-Grosjean (2011). Their argument is that boundaries are disrupted by translational research- although disturbances can be a productive as well as inhibiting feature of translation work. These authors are particularly taken by the notion that boundary objects facilitate translational work between epistemic communities (an argument also finding implicit support in Swan et al., 2007). The most prominent examples of boundary objects are the research/application problems around which these communities converge on a given project (ibid, 538). Clinician scientists were identified as mediators (or ‘boundary spanners’) performing important ‘articulation work’ between laboratory and clinic (Lander and Atkinson-Grosjean, 2011, 542). They state:

“Throughout this process, ideas, artefacts and individuals crossed boundaries, moving from the clinic to the research lab and back to the clinic again.” (ibid, 543)

This has influenced my thesis, which also utilises boundary concepts such as these (for elaboration see sensitising concepts review below). I find this depiction of translational work as one of ‘traffic’ between social worlds of laboratory and clinic/industry an immensely appealing one (see also Wainwright et al., 2009). Like them I also wish to foreground the concept of translational research and follow it into practice (Lander and Atkinson-Grosjean, 2011, 537, a vitally important analytic goal for social studies of translational research. But I have also sought to bring-in certain methods and concepts with a view to adding and improving upon their effort to pursue this aim.

One aspect is that Lander and Atkinson-Grosjean rather overlook the importance of place in favour of mobilities (a more widespread problem in STS articulated by Henke and Gieryn, 2008).
Studying the places and institutions which get associated with translational research, reporting what happens there and how informants see this as helping translation are questions I tease-out through empirical chapters and consolidate in the conclusion.

I find Lander and Atkinson-Grosjean’s (2011) aim of ‘foregrounding the concept of translational science [...] to illustrate how research, artefacts, ideas, diagnosis and treatment transport themselves between the clinical and academic environments in the biomedical innovation system’ (p.537) a generally attractive one. Although their study has helped to open-up this project, it has by no means has fully dealt with issues of transportation of the idea. Indeed although articulating this problem, their article provides little by way of empirical follow-through. For my part, ANT terms like immutable mobile and inscription devices, as well as boundary objects, will be used to explore the concept’s mobilities further. This constitutes a useful and novel way of retrieving the practice of translational research. Furthermore, Lander and Atkinson-Grosjean largely ‘black-box’ how epistemic cultures variously construct and accommodate translational research. STS teaches that sciences are not unified and therefore how translational research is defined will vary across localised practices of different communities. Perhaps one of the reasons that the translational literature has failed to pick-up on differences in how sciences accommodate and define this concept comes from the propensity to focus on single case studies. My comparative study then foregrounds how institutions of different epistemic cultures are used to construct local definitions and practical responses to translational research. Overall then I address the transportability and travel of translational research in a way that other studies which claim to ‘foreground the concept’ have not.

A general weakness of literature on translational research is that ‘academic culture’ is used as a major theme to explain the difficulties of ‘institutionalising’ translational research, but this theme has been under-elaborated. Bringing in laboratory study sensibilities enables a more wide-scale exploration of ‘academic culture’, which has been left untouched in two ways by
translational research literature. First few studies sample from the population of academic scientists. Many barriers academic scientists face in making the target concept work are not addressed by authors like Lander and Atkinson-Grosjean, because their study samples ‘boundary spanners’ (with infrastructures already in place to be mobilised). In entering into ‘ivory tower’ institutions of universities synonymous with ‘old-order’ academic science, there are perhaps more opportunities to observe resistance and controversy, given assumptions about the relative autonomy of this population and difficulties in transforming their practices. In sampling exclusively from ‘boundary spanner’ academic scientists, I suspect a number of controversies will have been missed. Second those studies that do sample from this population, like Wainwright et al. (2006b) have not drawn much from sociology of science literature: many of practices which characterise mundane activities of scientists are left untouched.

**Conclusion on Translational Research Literature**

The first part of the literature review has addressed existing sociological knowledge brought into this study about translational research. It was suggested that research broadly associated with a ‘re-contextualisation’ thesis has been useful in terms of relating a phenomenon like translational research to broader transformations in contexts of academic knowledge production in recent times. An empirical arm of this literature has also provided some inspiration in beginning to think about how academic scientists reconcile the various pressures posed by such changes in their mundane work activities. Surprisingly sociological research on translational research has been slow to draw on this wider body of literature, and conversely, ‘re-contextualisation’ literature has neglected member-generated categories like translation and translational research as a way in for exploring some of these issues in empirical settings. In this sense, then, my research can be seen as a bridge between these two types of literature.

Substantive sociological literature with an explicit interest in this topic has so far been thin on the ground. It has focussed for the much part on empirically-led criticisms of scientists’ and
policymakers’ discourse with regards translational research, posing that the prospective hype surrounding the term as a magic bullet for modern medicine is ill-founded, given short-term difficulties and resistances experienced by scientists and clinicians in fields like stem cell research. Although surely correct in their caution towards grand narratives, the sparseness of these studies has meant much knowledge gathered to-date on translation originates from the context of stem cell research, a frontier field which hitherto has enjoyed only moderate success with regards translational research, but where political pressures and expectations are high. Another substantive argument deriving from this literature has been to dismiss accounts of translational research as following a linear model of innovation. As well as imagining bench-to-bedside dynamics analysts then must also realise the importance of bedside-to-bench feedback. Despite agreeing with this emphasis on mobilities, I would also like to take the opportunity in this thesis to defend the importance of place in the social study of translational research dynamics. Overall then, the focus of sociological literature on single case studies, largely on stem cell translational research, have not provided the final word on translational research, and their program should be extended to include comparisons between the travel of this concept to different cultures of medical sciences and their local production sites. The fledgling body of sociological literature provides important but not exhaustive resources to tackle this aim. Therefore, I have seen fit to bring-in additional intellectual tools and concepts from STS, in particular the program often referred to as ANT. My use of Latour and Woolgar’s (1986) frameworks to help capture some more of the mundane aspects of scientists’ work makes a helpful contribution to studies interested in pointing to difficulties and possibilities of translation in contemporary technoscience. The sensitising concepts deployed to open-up further avenues of empirical findings will now be the focus of the next section of this literature review.
2. Theories and Concepts for Locating the Mundane in Translational Research

This section introduces an overview of key sensitising concepts and themes imported into this study from STS research on scientists. One of the most notable contributions to come out of the field of STS is to provide a provocative re-evaluation of previously received views about how the sciences produce knowledge about the world. In the grammar of Pickering (1995), scientists should not be understood as working objectively with ideas and objects as in old order depictions, but as engaging in struggles within a dynamic world of performances. According to ANT, the main approach followed in this thesis, scientific activity is about organising humans and non-humans into stabilised networks of association which are capable of producing certain ordered effects. In reality this process often fails as elements do not always hold together. Indeed reality is defined in ANT as that which resists (Latour, 1987), and scientists must devise ways of accommodating and working with known and emerging resistances. Such ‘dances of agency’ between humans and non-humans in science was labelled elsewhere as ‘the mangle of practice’ (Pickering, 1995). As an explanatory paradigm, retrieving the practice from science points towards the processes and struggles by which connections are made. In short, then, scientific activity should be approached as being concerned with the production of order from disorder.

STS has provided a series of case studies and conceptual resources for analysing how scientists go about reducing ‘background noise’ and producing ‘clear signals’ out of this dynamic state of affairs. Latour and Woolgar’s (1986) influential text Laboratory Life articulates four such frameworks and details how these get constructed and imposed in the mundane work practices of scientists (p.37). Mundane is often taken to mean dull, grey, and uninteresting. Indeed the idea of being interested in ‘practices’ of scientists at work was thought uninteresting and irrelevant by earlier philosophy of science. STS’s concern for the mundane has therefore been a
very deliberate strategy used in order to produce stories about science and technology which retrieve and take seriously practices. ANT provides an alternative account of the materiality of science and technology in terms of local, specific, situated practices and provides tools which help to retrieve such practices. In doing so, this provides a very different story of the materiality of facts, truth, reality, theory, ideas and so on than is provided in prominent earlier versions of science.

Setting out on my fieldwork, the aim was to see what a ‘sexy’ new concept- translational research- looked like amongst the mundane work practices of scientists ‘at the coalface’. As I set-off on this journey Latour and Woolgar’s frameworks and associated STS concepts helped to navigate the mundane world of scientists. Each sensitising ‘framework’ is summarised below in brief, with an account of aims and approaches of the original versions and additional information as to which aspects of the frameworks were thought particularly relevant in opening-up questions about translational research in this study and why. Before addressing sensitising concepts from Laboratory Life, I will set-out other important concepts from ANT and related STS toolkits which are drawn-on regularly throughout the thesis, such as ‘translation’ (or ‘transformation’), ‘boundary objects’, and ‘immutable mobiles’.

Transformation and Actor-Network Theory

The ANT notion of transformation⁹ is a useful conceptual and methodological tool in exploring the networks of associations which are at the centre of scientists’ practical efforts to re-shape their societies. It is through these ‘actor-networks’ that science and technology are able to speak-for and intervene in the world and reconstruct the social contexts of which they form a

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⁹ Most ANT works in fact use the word translation, not transformation. But given the prevalence of this word and its adjective ‘translational’ throughout this thesis I decided not to keep the word translation in referring to theoretical issues provoked by ANT. Otherwise things would be altogether too confusing. Where I have kept the word translation is in locating its indexical uses and meanings in the networks of researchers who form my cases.
part (Callon, 1986, 20). Emerging from laboratory studies, ANT has sought to retrieve practices from the study of science and technology which had for so long focused only on the outcomes of science. ANT has led to accounts of how phenomena achieve durability and mobility (materiality) which are often very different from those routinely found in modernist scientific discourse (Law, 1994). In the ANT scheme, materiality is a relational effect that is always embedded in ‘networks of the social’ (Law, 1994, 102). ANT questions how science and technology are able to exert any kind of force in a context where numerous actors ‘develop complicated strategies and many possible innovations with social and technical implications’ (Callon, 1991, 133). The answer seems to be ‘with some difficulty!’ Doing materiality, it turns out, takes skill and effort. It involves building networks out of heterogeneous elements; ordering actors and intermediaries to occupy positions and play allotted roles (assembling ‘actor-networks’). But objectifying elements into actor-networks is all the more difficult given that ‘the components of the networks, have, as it were, no natural tendency to play the roles to which they have been allocated’ (Law, 1994, 103). Furthermore once assembled, actor-networks must overcome efforts to break its ties and inhibit its expanse by other actor-networks (withstanding ‘trials of strength’) (Latour, 1987). Materiality then is the exception rather than the rule, as durability and mobility of (often unruly) objects is only built and maintained at great effort and cost. In ANT transformation is the work done to achieve such durability (Callon, 1986, 28).

Successful transformation involves not only recruiting allies, but also establishing an equivalence between them (Callon, 1998, 52). This equivalence centres on an intermediary-like a machine,

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10 Actor-networks are taken to include both human and non-human elements. This is important for studying science because instruments form important parts of the networks that carry facts and machines back and forth. For instance, most scientific theories today cannot be performed or demonstrated without instruments (Latour 1987, 250-251).

11 On the flipside, an actor-network is an arrangement of constituent elements that have been transformed (Callon, 1986, 32-33).
product or scientific argument- that defines two objects (Callon, 1991, 144-145). Establishing this equivalence can take on a number of forms (for a detailed typology of transformation strategies and tactics see Latour, 1987). In the modern world, scientific knowledge is a prominent example of an intermediary, the equivalence of which typically gets established as follows:

“A team of biochemists can define other actors and suggest the following translation: We want what you want, so ally yourself with us by endorsing our research and you will have a greater chance of obtaining what you want.” (Callon, 1995, 52)

Successfully mobilised facts and machines, in Latour’s writings, eventually take the form of ‘black-boxes’ amongst new users. Polonium for instance, went from being an obscure artefact in the hands of Marie Curie to a stabilised fact amongst ‘many more, but much less informed, hands’ (Latour, 1987, 138). Many of the new users therefore did not need to be as qualified or pre-occupied as Curie had been towards Polonium. On the contrary the success of the fact can be explained in terms of a great number of people being much less concerned than Curie, being content merely to treat Polonium as a ‘black-box’ around which to develop their local practices.

To convince new audiences of the facticity of this supposed element at the time required the transformation of various interests and actions to the point that Polonium became a stabilised scientific fact. In order to extend their networks, scientists and engineers must develop implicit or explicit sociological theories, for instance about the users of a prospective intermediary (Callon, 1987, Akrich, 1992). Stabilised objects thus have ‘scripts’ about the prospective users written into them, which the users themselves must make sense of and negotiate (Akrich, 1992).

A curious feature of stabilised objects is that often scripts and the transformation efforts that went into making them stable features of networks appear invisible at the end of successful innovation processes (Callon, 1986, 24). For actor network theorists, this is the reason why analysts and practitioners we study have both had great difficulty in being (relational) materialists. Given this contention, ANT states that successful transformations and materiality
should only be considered by sociologists after the fact and as more-or-less reversible (Callon, 1991).

Following ANT logic, one can postulate that translational research is one particularly prominent intermediary circulating the contemporary research system, but there may also be other actor-networks complementing, challenging, and/or being silenced by it. Here ANT is used to explore the practical, real-world-making effects that efforts to transform translational research are having on-the-ground. In retrieving the practice from translational research, ANT offers a useful methodological tool for flattening macro-micro divisions which sociological and organisational theories often articulate (Callon, 1991). Part of the appeal of this flattening is that scale and agency are viewed as outcomes of action which are to be determined empirically, rather than as external properties of the world which analysts can know and decide about in advance. Actors are outcomes of actions (and organisations outcomes of organising), rather than inputs (Czarniawska, 2004). These features are imputed to analysts who have ‘followed actants around’. In advance here the semiotic term actant is used to refer to those entities whose agency has not yet been imputed to analysts. To take an example from this research, in advance it might appear common-sense to view formal bodies like UK government, MRC, or NIHR as macro-actors in stories told about translational research. Yet in the approach taken here, prior to analysis they are treated as actants, whose degrees of actor-hood are not yet determined. This leaves open the possibility that these cases are not being acted upon to any significant degree: whether indeed they are and where is an empirical question.

**Negotiating the Boundaries of Science**

The metaphor ‘translation’ implies movement across boundaries. STS literature on boundaries in science form a useful set of sensitising concepts with which to consider questions regarding boundaries researchers encounter in (dis)engaging with translational research. In particular, this
literature has been instructive in providing concepts about two forms of activity working to shape boundaries: closing (‘boundary work’) and opening (‘boundary object’). The metaphor of boundaries derives here from cartography.

The concept of boundary work takes science as a system of knowledge dominated by a particular group of professionals (‘scientists’) in modern society. According to this scheme, professionalism is a mode of control, rather than simply an occupation. As a constructionist theory of science, boundary work sees the authority of scientific knowledge and scientists as being continuously contested and negotiated (Gieryn, 1983). As a professionalised group, scientists’ power has been accomplished through disciplining and creating new knowledge about aspects of the world to which other groups in society attach value. Boundary work thus follows the STS style of questioning received rhetoric of science, with scientists seen as a professional group interested in selling their product. Boundary work theorists have elucidated this by focussing on rhetorical strategies and tactics used\(^\text{12}\) to defend science from non-science, scientists from non-scientists, and the validity of scientific from non-scientific claims (Gieryn, 1995, 393). As demarcation is part of a ‘cultural and rhetorical game’ rather than essential characteristics of science, theorists are concerned with how scientists and others contest and negotiate these ‘social conventions’ (ibid. 398). For instance, the norms scientists use to distinguish scientific knowledge, methods, and institutions, are in fact resources mobilised as and when they are deemed useful to the situation at hand (‘surface norms’) (Gieryn, 1995, 400, see also Mulkay, 1976). In addition to the actual performance of boundary work, an equally important strategic aspect in the management of science is keeping this work hidden (Gieryn, 1995, 412-413). Analytically, essentialists do boundary work and constructionists watch it get done by others in society\(^\text{13}\) (Gieryn, 1995, 394).

\(^{12}\) It is not just scientists who practise boundary work: they are also able to get others to defend their interests and further their rhetoric.

\(^{13}\) Boundary-work can be found in both the work sites of science and other sites such as courts, mass media, and public speeches (Gieryn, 1995, 412).
This work can be found routinely in the dramaturgical communications of scientists, which make interviews and public statements particularly useful data sources. Boundaries can also be seen in more naturalistic settings, through observing interactions or reading through relevant documents. In the context of this study, the notion helps one to consider the varying extent to which respondents describe translational research as a threat to existing boundaries (e.g. of professional autonomy or credibility), or whether they are simply able to handle it as ‘business as usual’.

The notion of boundary work is often taken as synonymous with closure, but this association of scientists with closure strategies must be qualified. Boundary work does not simply close down, as by definition closure of one boundary causes shifts to others (as when redrawing a map). Boundary work is therefore an interactive process. In addition, as well as seeking to close-off and defend the boundaries of science, scientists must also routinely communicate with others. The notion of ‘boundary objects’ captures not only activities and practices which separate science from non-science (e.g. standardised procedures used by professional scientists for handling specimens), but also how social worlds are brought together in the process of making knowledge (Star and Griesemer, 1989). Boundaries between social worlds can be inhibiting in processes of knowledge production and exchange, but boundary objects help to open-up communication (Carlile, 2002, Fox, 2011). Transformation across these divides requires simplification, and certain objects can help to facilitate this. In innovation contexts, for instance, boundary objects help to open-up conceptual and instrumental spaces allowing for different social actors to work together (Swan et al., 2007). Within this theoretical grammar, determining the absoluteness of definitions and standards is of little interest. Instead definitions and standards are interesting for

Latour’s inscription devices and immutable mobiles have similar effects as boundary objects (Latour 1987).
what they do, namely helping people to talk to one another and coordinate exchanges (Star and Griesemer, 1989).

A neglected feature in writings on boundary objects is their propensity to ‘speak to’ people. This becomes instructive if one begins to consider the appeal of a metaphor like ‘translation’. Translational research was frequently acknowledged for its aesthetic qualities by respondents in my empirical studies, who would attach to it the statuses like ‘buzzword’ or ‘catchphrase’, which was ‘of the moment’, ‘in fashion’, ‘sexy’, and ‘seems to be everywhere’. The analogy of ideas as examples of fashions and commodities (that can be ‘packaged’) has been documented by those interested in how metaphor comes to shape actions (Lakoff and Johnson, 2005, 105). Hence, although it can be criticised as a linear reification, translational research may also serve an instrumental purpose when acting as a boundary object bringing different groups together and helping them to converse. As such, the performative effects of translational research should not be dismissed, even if there is some scepticism towards rhetoric accompanying it. Hence this concept can be enrolled into this study as a means for considering the performative and practical dynamics of translational research as a device in the governance of research.

There are certain similarities between boundary objects and actor-network theory notions like ‘immutable mobiles’. Immutable mobiles are inscription devices which, as they are mobilised from one point of a network (a ‘centre-of-calculation’) to another, are stable enough to affect some sort of action in remote regions. To extend a device requires enough of its original coordinates to hold together as it moves between parts of a network (Latour, 1987, 227-228). By themselves inscription devices cannot achieve these ends, but rely upon agency of various allies who accept them as indispensable to their purposes and thus extend the materiality of the devices into other sites, thus extending the network. Transforming these remote interests and practices is far from a foregone conclusion. Translational research might be thought of as such a device, insofar as it is mobilised by interested parties like policymakers with a view to steering
distant events in research laboratories. The label gets attached to various formal devices like agendas and portfolios of research councils, which, through conversations, demonstrations, and media travel from centres-of-calculation (e.g. MRC Headquarters) to other peripheral sites (e.g. laboratories, universities) in the research system, and back again, without losing their form. In everyday practices mediation is facilitated by very mundane items, which provide flat surfaces on which to coordinate and mobilise definitions (Latour, 1990). Things like websites, emails, PDF documents, PowerPoint slides, application letters appear everyday means by which transportation of translational research definitions are made to last over time and endure through space (i.e. achieve action at a distance). But even if these items hold, in order to coordinate exchanges effectively, certain definitions attached to translational research by either party have to hold enough to transform the interests and enrol the support of allies. Part of the problem of extending inscriptions and definitions is that when put into practice elsewhere in the networks, they encounter trials of strength and resistances.

Theoretically what likens the concepts of boundary object and immutable mobile is in pointing one towards the associations which must be forged in contemporary technoscience in order for stabilised effects and outcomes to be achieved. The capacity to open-up communication and frame equivalence in networks is a feature they share in common. They would suggest that the work done in negotiating local meanings and practices are of central importance in order for translational research to work in practice. It is precisely the ambiguity and plasticity which renders the term translational research so mobile and durable across different networks of academic research. It is not the presence of global standards, but the coordination work that goes into stabilising local practices and global definitions which is helpful in exploring the practices of translational research. A consequence of this theorising is that seeking globally stabilised a priori definition(s) of translational research appears an act of folly on the part of the analyst. Whether translational research is a successful boundary object or immutable mobile is
the important question which can be approached through descriptive empirical case studies. As such these concepts are conducive to the task of exploring the (relational) substance of the target concept translational research.

*History of the laboratory over a Historical Period*

The framework set-out in the *Laboratory Life* chapter ‘History of the laboratory...’ explicitly sought to redress the issue of objectivity in science, particularly those arguments put forward by scientists and philosophers regarding the genesis and solidity of facts. Through ethnographic observation, the authors traced the production, circulation, emerging controversies and eventual stabilisation of a new scientific fact emanating from the laboratory. They found the word ‘discovery’ provided a particularly weak term for explaining how scientists go about creating knowledge of the world. Instead what they help to advance through this and subsequent chapters is an argument and set of methods which elaborate facts as things which are ‘constructed’ and ‘constituted’ by real people in mundane, highly specific, carefully organised workplaces and extended amongst other people with analogous practices working in similarly unusual places. The process of establishing truth is fraught with social struggle, thus refuting the storybook image of science as value-free. Yet the word constructed should not be read to mean that scientists can do as they wish with ‘reality’, as the non-human entities they bring into their networks frequently exhibit resistances. This chapter then was an attempt to claim that stories about the solidity of scientific facts should be told in terms of associations between human and non-human elements in the networks of scientists. In retrieving the practice from scientific work, one can produce a very different account of the materiality of science and technology than is provided in accounts of ‘discovery’ by scientists and their champions in the public domain.

The approach of taking facts as constructed is certainly one which I have found useful in understanding how science works. Yet I will not replicate the initial project of Latour and
Woolgar’s chapter, as such constructionist arguments are now more-or-less taken-for-granted within STS. I wish instead to take forward a particular position expounded here which has been developed further by later ANT, stating that objects become stable inside networks of relations (1986, see also Law and Singleton, 2005). Once the relations holding an object together separate so does the solidified status of that object. Objectivity may be an outcome of scientific work, but this does not mean that science proceeds ‘objectively’. Similarly although stabilised artefacts like ideas, theory, patterns may be effects brought about by the extension of networks, they provide very weak accounts of that extension (see Latour, 1990).

The ANT definition of objects is a helpful reference point in retracing the production practices of each case. In particular, the device developed in Latour and Woolgar’s original framework can be used to map-out which aspects of laboratory production practices the ‘idea’ of translational research is associated with, and how it gets transformed and stabilised within the work of each case study. Interviews, observations and documents facilitate the re-tracing of objects produced (or ‘things’ in production) by laboratories and their associates (Latour, 2005). With this aid I sought to trace in each case the products that were deemed ‘translate-able’ and networks in which these were made visible and stable (if at all). The aim then was not only to map out the connections which are associated through such objects (the net-works), but the work that is done in order to produce and mobilise such objects (the net-works, or work-nets) (Czarniawska, 2004, Latour, 2005). Interview questions like ‘what does translational research mean in the context of your work…’ were prompts for generating data useful for retracing such connections.

**Inscriptions**

Following the organisation of material elements within the workspace of the Salk Institute Laboratory, Latour and Woolgar (1986) came to argue that diverse elements located within different part of the lab- benches, offices, libraries, meeting rooms- were connected to one another primarily through processes of writing. Practices of producing inscriptions are therefore
what connects very different looking actions ‘inside’ the laboratory (e.g. experiments) to those performed with the laboratory’s ‘outside’ (e.g. publishing). The inside and outside are therefore not autonomous realms but connected via practices centred on ‘performing’ inscriptions (producing, writing about, reading, circulating them etc).

In Latour’s ANT, inscriptions constitute a particularly important technology in modern science, as they have the capacity to render ‘phenomena’ visible, provide them with longevity, and with greater mobility. In short inscriptions are important in science because they enable action at a distance. The production and dissemination of facts is described as a process of ‘black-boxing’, whereby inscriptions gain the status of facts because they have been extended across networks without being ‘opened-up’ and disintegrating (Latour and Woolgar, 1986). Latour and Woolgar’s account of black-boxing explained this effect not simply as a result of the content of inscriptions, but also because of the devices and associated actions which render them mobile (‘inscription devices’). The claims Salk scientists made about the enzyme TRF could be accepted as a fact by other laboratories if the latter could successfully assemble an inscription device (‘assay’) similar enough to replicate and confirm the Salk’s results. Alternatively, as is often the case in large-scale modern science, smaller labs are often unable to afford repeating experiments made elsewhere and therefore are reliant on testimony of others. Kuhn’s (1970) notion of paradigm offers a powerful account of how some facts can acquire the status of being stable and trusted within scientific communities based on the testimony of others. Black-boxes can be stabilised for periods of time, before being opened again: they are only as strong as the networks holding them in place and so are always reversible in principle.

Far from being straightforward accounts of nature by the modest witness of the scientist, the scientific paper can be read as a persuasive text composed of interlocking statements which have been designed to resist de-construction by prospective audiences. Part of the interest of laboratory studies and other early STS studies was with the styles of literary reasoning which
were mobilised in writings of scientists. What literary genre and devices characterised the construction of such seemingly persuasive accounts of the world was a question which required empirical scrutiny. Latour and Woolgar (1986) and Knorr-Cetina (1981) both dedicated sections of chapters to the real-time process of writing and re-writing a journal for publication in a scientific journal by their respective laboratories. Latour and Woolgar (1986) noted how over time comments members of the laboratory make about the facticity of a given inscription undergo transformations in the level of stability attached to it. They provide a typology to capture the variation in ‘facticity’ attributed to statements by scientists. These categories range from Type I, II, III, IV, or V statements. Representing a sliding scale in terms of strength of claims towards facticity, ranging from the strongest- Type V- which remove all traces of a human voice from the statement, to the weakest- Type I- which are highly speculative and feature grammatical modalities like *maybe*, *unlikely*, *not confirmed*, *potentially* (p.84). Type V statements make claims which rest on assumptions of definiteness, for instance the statement ‘the protein x was added to the assay’ takes as uncontroversial the assumption that x is a protein. The statement ‘in my opinion x is possibly a protein’ carries significantly less certainty owing to the presence of modalities like ‘my opinion’ and ‘possibly’. These weaker Type I claims, typically found in talk between scientists at the lab bench and scribblings on earlier draft documents, are seldom found in final published statements of scientific texts, as the presence of modalities and human agency would undermine the persuasiveness of the claim to facticity. The fact would become an artefact. But according to Latour and Woolgar, at earlier points in their histories all stabilised facts are subject to controversies over whether attributions of certainty are to be credited.

One of the most original contributions of Latour and Woolgar’s work was to put forward a radical claim about scientific facts: that what scientists referred to as facts were no more than the composition and reworking of numerous inscriptions which have withstood deconstruction.
This argument can be seen as the product of a larger project in the sociology of science whose aim was to retrieve the practice from science so as not to settle for accounts of reality that were based on the ‘picture-book’ versions which were available both in the public domain and philosophy of science. Facts are thus not quasi-religious revelations from nature which scientists modestly witness and report. The word ‘construction’ was therefore introduced as a better means of understanding how reality was ‘made’ rather than ‘discovered’ by scientists in the laboratory. Whilst still a controversial thesis in some quarters, in the context of STS the importance of inscriptions in the scientific production process has provided the basis for much important work in the intervening decades. My appropriation of this framework then is not concerned with further advancing a thesis about the construction of scientific facts through inscriptions, as this is a position already well advanced. Instead I look to explore how the notion of translational research is made visible and workable in the context of mundane writing actions, and whether such actions are transformed, accommodating and/or resistant of this label. How I do this and the implications of doing so will be set-out now in brief detail.

The section on inscriptions in each empirical chapter of the thesis focuses on the practices of writing, particularly in relation to two institutions of scientists’ credibility cycle: grant applications and journal publishing. The aim is to consider the extent to which the emergence of translational research has transformed these mundane writing practices. This approach borrows in part from Hessels et al’s (2009) heuristic for studying impact of wider historical struggles for relevance experienced by disciplines have had on local practices of scientists. But rather than synthesise all actions on the credibility cycle with historical developments in the organisation of disciplines, I focus on two writing practices from the credibility cycle, in order to query whether suppositions that translational research is transforming the institutions of science is supported by empirical findings. The impetus came from wanting to explore a scenario put forth by Rip (2011) about the future of research in Europe over the next decade. One institution which Rip
identified for coming under increasing pressure from external audiences was that of grant writing and journal publishing. The implication was that translational research marked wider changes in ‘re-contextualised science’, whereby the context is increasingly ‘speaking-back’ and becoming more pushy in its funding of public science (Gibbons et al., 1994, Nowotny et al., 2001). In this scenario, in order to receive funding, researchers across the sciences would be obliged to follow a new convention of including in these texts plans to convert preliminary findings from the study on which the application was being based into further proof-of-principle studies. In other words, researchers have to demonstrate (at least the potential for) translational research.

Scientists, it has been shown, have for a long time had to tie their work into a broader ‘web-of-reasoning’ with which those funding them are interested (e.g. Knorr-Cetina, 1981). Knorr-Cetina examined how in journal articles published by applied scientists terse statements about potential applicability of their product were included into texts otherwise saturated with technical discourse relating to narrow problem choices in their specialities. Such statements were carefully constructed to suggest only a plausible script for future action in relation to their product, rather than definitive statements about its future progress. Furthermore, statements were organised within the texts in such a way that authors themselves would not be accountable for developing these products further in a practical direction (ibid). By contrast, the implication of translational research for the institution of grant writing presented by Rip appears to be that scientists must include plausible plans for further development of findings and that they themselves will be held to future account for the promises they make. In other words, extending Nowotny’s ‘re-contextualisation’ thesis, these particular institutions are likely to undergo transformations as a result of interested parties becoming ‘pushier’ towards scientists. The sections thus offer preliminary findings on whether this scenario has materialised. If indeed translational promises do indeed now constitute a function of the texts these respondents must
write, then it could mark a departure from some earlier STS findings reporting the persuasiveness of statements according to standards of representationalism.

Instead of following the whole process of writing journal and grant articles like earlier laboratory studies, where possible I conducted an analysis of statements found within successfully mobilised grant and journal articles (insofar as they had withstood deconstruction and enrolled intended gatekeepers like journal editors and grant review boards). In order to distinguish between texts whose persuasiveness is based purely on representational claims and those in which statements about translational research also feature, selected documents were coded using Latour and Woolgar’s five statement types (marking ‘representational’ claims) and ‘translational’ statements (marking instrumental, performative claims). The materials gathered, procedures followed and findings with regard the presence of each code and their relationship within written texts produced by each case is discussed in the respective empirical chapters. Overall patterns in the coding can be used to make inferences about the extent to which this important institution of science now requires these respondents to extend persuasive skills to include promises of relevance and legitimacy.

As well as focussing on the content of mobilised inscriptions, analysis also focuses on the additional work scientists performed in order to mobilise inscriptions. After all, alone inscriptions do little- they must be supported by inscription devices. One of the sociological questions of interest here is the extent to which ‘traditional’ institutional outputs written by scientists carry credibility in contexts of ‘translational research’ which are thought to incorporate actions of agents with multiple interests and disciplinary approaches. Is mobilisation of additional techniques and efforts appropriate in order for inscriptions to travel in processes of production and/or development?
Microprocessing of Facts

In the fourth chapter of *Laboratory Life*, Latour and Woolgar (1986) argue against received views of philosophy of science stating that sociological processes of decision-making are remote, if not irrelevant, to the ‘logic’ of scientific discovery. Instead, the chapter sets out to show how contingencies become certainties in the course of action and interaction. Through empirical observation of ‘routine exchanges and gestures which pass between scientists’ in the laboratory (p. 151) they reach the conclusion that the association between ‘facts’ on the one hand and ‘nature’, ‘logic’, ‘scientific method’ on the other (characterizing much of the received discourse in philosophy of science) are simply rhetorical effects achieved at certain stages in the career of a fact. These couplings rely on tautological claims, evidence for which appeared almost entirely absent from the forms of argumentation they observed being used by participants in the course of laboratory activities (1986, 151-152). The chapter provides a conceptual framework through which to analyse how facts get constructed as ‘logical’.

The authors reported four exchange types they had observed from interactions in the laboratory which served an ‘information spreading function’ between participants (1986, 161). The first (heron Type I) set of exchanges involved discussion between scientists about the facticity of a statement, referring either to ‘new’ or ‘long established’ facts. Type II exchanges occurred mainly between technicians at the ‘coalface’ of experimental activity and involved real-time discussions about correct ways of doing things (ibid.). Type III exchanges were largely strategic in character and were reportedly less common, concerning long-term development of things they were working on and how they might relate to future developments in their discipline (1986, 163). Type IV exchanges were those in which claims about the status of statements as fact or artefact was supported by reference to ‘non-technical’ characteristics of other researchers with whom claims were being associated (ibid.). As will be made clear in each empirical chapter of

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15 Not to be confused with exchange types in *Literary Inscriptions* section
this thesis, some cases provided better opportunities for original application of this framework than others.

Overall my aims in using this framework in the empirical chapters differs slightly from the scope of their original objective, as it is not pre-occupied with elucidating the tautological character of much scientific reasoning. It simply uses the framework as a sensitising concept to delineate important ‘mundane’ aspects of scientific activity: real-time negotiations required in order to make science workable. The kinds of elements that are mobilised in order to stabilise exchanges then are key concerns in adopting the framework. This framework also makes implicit the assumption that places are important in the coordination of such exchanges. My analysis therefore considers in addition the function of place in production processes of these cases and builds towards further theoretical development of this issue in the concluding chapter of the thesis. One of the problems which emerged in applying this tool was that the exchanges traced by respondents did not occur simply inside single production-sites as in Latour and Woolgar’s study of interactions in the Salk Laboratory. Instead the nature of the questions being asked here necessitated movement into sites of production which were geographically distributed and ‘brought in’ heterogeneous compositions of human and non-human elements which did not fit comfortably within certain of the ‘local’ boundaries respondents in each of the cases drew for themselves. One prominent a priori definition of what makes translational research visible and workable is the composition of research teams which include not only scientific researchers, but also prospective developers and end-users. In order to work in practice, this ‘technoscientific’ discourse requires extensive effort and negotiation by stakeholders to arrive at local coordination whilst retaining some of their global differences (Galison, 1999, 138). How then negotiations are coordinated and what function places play in ‘technoscientific’ production are fascinating and important questions opened-up in the course of applying and extending this framework in the context of these cases.
Cycles of Credit

Perhaps compared to other frameworks introduced by Laboratory Life, the *cycles of credit* chapter addressed more traditional sociological questions concerning the social structuring of laboratories as organisations and the calculations and decisions made by scientists in respect to their careers. It did so through making a quasi-economic analogy with an extended notion of *credit*. This was expressed through a two-dimensional graphic subsequently labelled the *credibility cycle* (see figure 3). Their account of credit incorporated a critique of a number of disparate themes and arguments in previous sociology of science literature with regards how and why credit and reward gets bestowed amongst scientists. This is not the place to rehearse these criticisms in full, merely to give a general critical overview of the framework, and explain how it has been taken-forward in relation to accommodation of two mundane concerns—scientists’ group structure and careers—around practical issues provoked by translational research.

The framework states that scientists are constantly struggling to mobilise resources (human and non-human) which can be associated with one another and eventually be converted into new facts primarily through the medium of publication in scholarly journals (see figure 3). Scientists gain and enhance reputation from publishing and being cited by institutions in reputational work organisations of scientific fields (Whitley, 2000). Scientific practice is therefore concerned with utilising and extending the cultures in which scientists are embedded (Pickering, 1995; Knorr-Cetina, 1999), and primarily actions can be understood as amenable to pursuit of credit. Forms of credit displayed on the graphic are not particularly useful in and of themselves, but in relation to other actions to which they can be linked in the cycle. As a primary form of ‘social capital’, institutions of credit exhibit something approximating a ‘use-by-date’: if not reinvested in subsequent cycles then it can be eroded. Accumulation of credit can have a kind of reinforcing effect in attracting further resources and enabling more rapid and extensive conversions of
credibility (Mulkay, 1977). As a product stabilised through networks, credit is not evenly distributed. The processes by which credit is accumulated and distributed, far from being harmonious, is characterised as one of social struggle. Indeed Latour and Woolgar liken scientists to military generals who must exact strategies about which positions to occupy on a given battlefield (e.g. carving-out an association with a problem which will force rivals in their agonistic field to bestow credit upon them). Senior scientists such as the heads of laboratories have accumulated greater levels of credit and can mobilise much larger and stronger networks than junior members of a laboratory, who are seeking to gain enough credit simply to be accepted as competent practitioners within their communities. As such, various members of the laboratory make quite different calculations and definitions of their careers. For instance, the head whose name is frequently attached to the laboratory as a formal organisation and location is able to speak of their career as paralleled to that of laboratory. Conversely, the junior post-doctoral researcher or PhD student will seek to associate themselves with larger, established, stronger allies, such as senior scientists or the laboratory, so as to strengthen their own credibility as ‘commodities’ in the job markets for scientists (Knorr-Cetina, 1981). The asymmetries of credit distributed amongst individuals in a laboratory warrants one of the main bases for the divisions of authority observable within these settings (1986, 229). As senior members have accumulated greater credit, they are able to assume the position of head and spokesperson over other entities with greater levels of trust and authority than would those with a lot less credit to their names.
Part of the appeal of this framework in approaching group structure and careers is to pose these units as the outcomes of performances and organising, rather than properties of individuals and the organisation. This is in keeping with the general ontological positioning favoured in this thesis of the world as one of flux and performances, not as fixed and Platonic (Pickering, 1995). Thus group structures are reconstituted in the course of work organisation, as credit is variously distributed and contested amongst members of the laboratory, for instance in relation to a specific claim or a piece of work. As such ‘the scientist’ as an individual is best seen as an outcome of the organising done by associated actor-networks, rather than a kind of discreet subject existing prior to collective work. Credit in its extended form also offers a felicitous
concept through which to explore careers of groups and individuals as it often occupies a central place in how scientists come to tell of the activities of themselves and others. This account of scientific practice provided a much stronger general explanation of the day-to-day actions observed by scientists in the laboratory, than earlier versions of scientists being driven by norms or intellectual puzzles. These explanations are not wrong per se, however they explained only a very small fraction of observed behaviour (Latour and Woolgar, 1986). Furthermore, without calculating issues of credit a given selection would deliver, it seems highly improbable a scientist would ever be able to mobilise the capital to satisfy these wants. One of the appeals of the credibility cycle is that in addition to being generally applicable, it can also accommodate variation in observed actions. For example it gives a plausible connection to seemingly disparate actions one can observe scientists performing such as discussing results from an inscription device, hiring new staff, and writing a grant application. The applicability of the credibility cycle as a general model for (de)constructing individuals and small groups of scientists in this study became clear during observations of weekly team meetings (in Chapter 8). The definition it provided of scientists’ investment decisions and actions as mobilised around an extended notion of credit enabled me to make sense of exchanges I observed during meetings, for instance over which grants to submit for, how to write project reviews, which deadlines to prioritise and so on. Here the primary problems being voiced by participants regarded short-term problems in converting forms of credit, as opposed to what constitutes an important societal problem their science can address (i.e. normative or truth-seeking motivations) (Knorr-Cetina, 1981). The priority of producing publications in scholarly journals, credit for which could subsequently be re-invested in further cycles was also reinforced through interview accounts with respondents pointing to the formal assessment standards of the REF placing varying levels of constraint on individuals and the group.
As such, the credibility cycle could be used as a general model through which to explore the actions and selections of scientists. However, the original model has not been without its criticisms. Part of these can be related to the fact that how science is organised in Britain today is different from the situation of scientists in California in the late 1970s. The emergence of the REF as an important disciplinary control measure in respondents’ accounts and recent modification of the original credibility cycle graphic to include such performance measuring devices (dubbed ‘organisational devices’ see Hessels et al, 2009; Figure 4), meant that application of this framework would have to feature this issue. This and other governance initiatives have been designed and implemented with a view to ‘auditing’ academics, part of what some Neo-Foucauldians see as an expression of neo-liberal governmentality practised by contemporary universities (Shore and Wright, 2000, Deem and Brehony, 2005). One of the questions I had been interested in exploring concerned gaps between producing work deemed ‘excellent’ and work which would have an ‘impact’. This simple sounding interview question (see Table 2 in Chapter 5) enabled me to get respondents to talk about the difficulties of putting the idea of translational research into practice whilst retaining commitment to more ‘traditional’ objectives like publishing in respected scientific journals.

Likewise it has been argued (I think fairly), that the original model did not pay sufficient attention to the funding landscape in which scientists struggled to capture resources (Slaughter and Rhoades, 2004). Thus Rip’s (1994; see Figure 5) model which includes struggles for ‘legitimacy’ and ‘relevance’ in addition to ‘facticity’ was taken as a version of the framework which promised to navigate certain institutional pressures which informed scientists’ practical mundane struggles. This appeared appropriate not only from readings of emerging data, but also at the outset given how narratives which typically surround strategic questions of science funding frequently latch onto concerns for relevance and legitimacy.
Figure 4: Latour and Woolgar’s Credibility Cycle now including ‘Organisational Devices’

Source: Hessels et al., 2009, 396
Conclusion on Sensitising Concepts

This section has set-out various motifs and concepts which empirically-derived studies in the STS canon have offered the research in this thesis. Despite their widespread influence, much of these contributions have yet to make their way into studies explicitly concerned with translational research. The actor-network theory approach has arguably been the most influential of STS research programs, bringing into focus the importance of networks in the studies of knowledge practices in activities like science, technology and medicine. Its emphasis on associations has done much to advance the focus of STS scholars and many outside the field on the practices and contingencies of knowledge production, in terms of bringing allies into the
laboratory and subsequently ‘translating’ (or ‘transforming’ as I put it) the interests of further allies beyond their immediate centres-of-computation. Indeed the term translational research itself has a curious Latourian ring to it. In addition to ANT, STS research on boundary activities have done much to contribute to an understanding of how scientists and their allies forge and maintain stability despite local differences of interest. In addition to these resources, the empirical chapters draw-in insights from laboratory ethnographies of scientists at work. In particular, four frameworks set-out in Latour and Woolgar’s (1986) influential text *Laboratory Life*, have been put to use here for the purposes of locating the mundane work practices of the scientists forming the basis of the case studies. It is through its empirical case studies accentuating the mundane aspects of laboratories, field sites, museums, observatories, and so on that STS has been so successful in challenging the grand narratives of science. This literature has therefore provided a logical basis on which to study translational research in a way which demystifies and circumvents any hype which might accompany its ‘common-sense’ usage. Together with sociological research on translational research, this conceptual apparatus provides a strong – although not altogether indestructible- basis on which to conduct the empirical inquiry in this thesis.
4. The Philosophy of STS

Introduction

This chapter seeks to capture how laboratory studies have challenged some of the standard tenets of received philosophical and cultural thought about science, and in doing so, provides a compelling case for using constructionist theories and methods to study the forces of translational research in this thesis. This section provides the intellectual background to and justification for the relativism behind theories and methodology discussed in Chapters 3 and 5 respectively, and put to use in the empirical case studies.

The style of STS has been to take revered and standardised ideas and concepts—science, technology, law, the market—and convert them into objects of study (Woolgar et al., 2009, 22). These ‘black-boxes’ thus move from ‘from matters-of-fact’ to ‘matters-of-concern’ (Latour, 2005). In this vein laboratory studies have typically taken phenomena like facts and machines, and used ethnographic sensibilities to explore how they are made (Latour, 1987). This approach appears counter-intuitive to modernist discourse, which assumes such phenomena to be both objects and objective (Law 1994). But by retrieving the practice from science, laboratory studies have shown how objects and objectivity are made rather than given. Following this tradition, this thesis seeks to give translational research the laboratory studies ‘treatment’—in problematising this concept by opening it up to constructivist methods and concepts. In doing so, it is expected that the study will produce a very different account of translational research to those found elsewhere, e.g. in policy rhetoric.

The chapter begins by focussing on what these studies have said about the manufacturing (read ‘construction’) of knowledge, as the complex, contingent outcome of local contexts. Attention of
the section then moves towards clarifying an aspect of constructionist thought that laboratory studies have helped to develop, namely its rather unique ontological-epistemological stance. I then consider the sometimes uneasy relationship laboratory studies-derived STS claims have had with philosophy of science before finally advocating the use of constructionist methods in studying translational research.

**Background**

Since the 1970s, STS Laboratory studies have reported first-hand about the *social processes* of scientific observation and experimentation, activities which had previously been deemed too uninteresting or irrelevant by sociologists. Laboratory studies can be described as ‘the study of science and technology...at the root where knowledge is produced, in modern science, typically the scientific laboratory’ (Knorr-Cetina, 1995, 140). Traditionally the method for observing this production is ethnography, with some discourse analysis (ibid. 141). Laboratory studies were influenced by earlier social constructionist traditions in sociology and anthropology concerned with the cultures and practices of groups in their work contexts (Knorr-Cetina, 1981, Collins, 1985, Latour and Woolgar, 1986, Traweek, 1988, Pickering, 1995). Like anthropologists, laboratory studies wanted to treat science as a form of culture on which insights could be opened-up through qualitative inquiry. These studies followed and helped extend the social constructionist perspective in the social sciences (Berger & Luckman, 1967), by applying its ideas to the very ‘factories’ of contemporary knowledge production (i.e. laboratories). This paradigm perspective holds that although patterns are the persistent feature of all social life, no one pattern is necessarily persistent or stable (Barnes, 1995, 67). This is the foundation of a relational metaphysics.

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16 Although there are some differences between STS versions of constructionism and social constructionism (discussed in Knorr-Cetina, 1993), on the issue of using qualitative methods they are commensurate.
The laboratory studies agenda differed from earlier philosophy of science accounts, in rejecting the need to ask cognitive questions\(^{17}\). Instead the former were interested in analysing the processes through which knowledge is constructed in its everyday settings, thereby retrieving ‘context’ (‘non-rational’ things: personal, social, cultural), as a central feature in the organisation, management, and performance of science (Knorr-Cetina, 1991).

**Are STS Laboratory Studies relativist or realist?**

Clearly this methodological approach projects a relativist epistemology. Following a number of constructionist traditions, particularly ethnomethodology, it is assumed that all categories, routines and habits are socially constructed, no matter how stable they may appear through common-sense readings. For Garfinkel (1967), this statement about the social world is evidenced by the observation that routines evolve and alter over time. Categories get constructed but become problematic, as shown in his case study of Agnes, a trans-gendered person taking part in a census. As such, one should not lose sight of the social construction of phenomena, however routine they might appear.

Thus, STS can be seen as an empirical and methodological extension of earlier social constructionist writings. This is because, despite some discontinuities STS has continued to fulfil what constructionists wanted social scientists to do: study **things as contingent accomplishments of people** and to study **how these things get done** (e.g. by looking at ‘speech acts’ and ‘discourse’ rather than, say, statements or deduction). As a general paradigm for social science inquiry, social constructionism is often (somewhat stereotypically) criticised for its alleged irrealism: upholding the naive position that **everything** in the universe is socially constructed. However, speaking of Berger and Luckmann’s (1967) treatise on socially constructionism, Hacking rebuffs these criticisms:

\(^{17}\) What statements/methods should be considered scientific/technically feasible.
“They did not claim that everything is a social construct, including, say, the taste of honey and the planet Mars - the very taste and planet themselves as opposed to their meanings, our experience of them, or the sensibilities that they arouse in us... They did not claim that nothing can exist unless it is socially constructed.” (Hacking, 1999, 25)

STS constructionism has fleshed-out this point empirically, and in doing so has set-out its own niche position on scientific knowledge; one that is, simultaneously, epistemologically relativist and ontologically realist (Woolgar, 1988, 54). Contrary to some criticisms, then, like more moderate accounts of social construction, STS does not refute the existence of an independent reality, or even of scientific facts (Latour & Woolgar 1986, 80, 82). It also acknowledges that the objects ‘natural’ scientists study differ, compared with say actions studied by sociology (Sismondo, 1993, 531). The dual position enables the articulation of some sort of distinction between natural and social sciences. In particular, natural scientists’ knowledge claims are said to allude to external referents (material objects) (Barnes et al., 1996), whilst social scientists study self-referring elements of knowledge that people hold of how their societies work (e.g. actions) (Cicourel, 1974, 45) and therefore lack this relation to an ‘independent’ reference point. Social science knowledge is therefore validated by looking at its incidence among the very people who believe in it (Barnes, 1995). An important implication of the realist position is that it helps to overcome certain reductionist weaknesses in earlier sociology of science, whereby ‘the content of science becomes transmuted into the simple reflection of social interests’ (Callon et al., 1986, 8). For example, ‘interest models’ of science (e.g. Barnes, 1977) gave little consideration towards how laboratories are constrained by access to and control over material resources, which are vital ingredients in investigating scientific problems (Latour,

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18 Giddens (1990) frames his discussion of social science’s self-referring quality under the term ‘double hermeneutic’.
It is not just humans that provide ‘resistances’, but also non-human elements (Latour, 1987).

Despite acknowledgement that the natural and social sciences are constrained differently by the realities they study, STS retains the relativist position that scientific knowledge is constructed rather than discovered. This is captured most clearly in their stance towards the ‘pre-existence’ of material objects, which become social objects as they begin to enter the meaningful parts of the social world of a scientific community (Sismondo, 1993, 524-525). Hence scientific knowledge is always constructed because its objects cannot pre-exist. Quite simply, ontology is the consequence of science (Knorr-Cetina, 1993, 558) and it is only in alienated social world that existence of objects, and indeed statements about them, are posited outside the realms of human intervention (Bauchspies et al., 2006, 14). Overall, the dual-position is seen as significant, because it accommodates empirical distinctions between natural and social sciences, whilst reinforcing the latter’s mandate to investigate all of the sciences (as ‘fair sociological game’). This methodological commitment to symmetry is credited to the Strong Programme in SSK, who held that successful and failed claims should be treated the same way by the sociologist (Bloor, 1991). This logic was also extended towards claims being made inside and outside of the established institutions of science, for instance by treating now discredited para-psychology claims the same as one would established ‘scientific’ claims (Collins and Pinch, 1982). Such provocative positions are illustrative of STS’s propensity to subvert taken-for-granted assumptions held about scientific knowledge.

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19 From an organisational sociology perspective, this factor can also constrain the development of scientific fields more generally (Whitley, 2000)

20 Incidentally this is usually a feat which takes much effort and investment on the part of the explorer (or laboratory) (Latour, 1987).

21 By negating the reductionist fallacy that everything in the universe is socially constructed, there is no longer good reason why sociologists should be deterred from opening the ‘black-box’ of scientific institutions.
**Conclusion**

This chapter has argued that reflexivity of STS constructionism has done much to challenge the ‘naïve realism’ underpinning the rhetoric in received views of science, which ‘speak about theoretical objects as if they were on the same level as the banal realities of everyday life’ (Collins, 1998, 878). In this sense STS marks a radical departure from essentialist philosophies of science. As Randall Collins notes, the reflexivity of STS constructionism is felt by many practitioners and philosophers as an affront to their ‘sacred object’ of truth. The typical emotional response to reflexive statements about the social construction of knowledge is therefore one of embarrassment. Conversely, STS has not escaped critical feedback from philosophers of science:

> “Their ambivalent relationship with philosophy...gets these studies into trouble - the trouble consisting in the belief that constructivism must be understood, or must prove itself worthy, in relation to philosophical doctrines.” (Knorr-Cetina, 1993, 560)

Philosophy of science has been concerned with demarcating the special nature of scientific knowledge. This is incompatible with STS, which sees such a task as sociologically useless and based on out-dated myths. Given this state-of-affairs, most relativists have maintained that it is not their responsibility to bring philosophy back into constructionist STS:

> “It is hardly conceivable that a phenomenon like modern science, which is so intrinsically linked to modern society as an institutional and collective arrangement, should not itself display social features which philosophy must come to grips with if it is ever going to be au courant with the world in which it lives.” (Knorr-Cetina, 1993, 556)

Hence although STS does not conform to certain standards set out by philosophers of science, given the lack of reflexivity apparent in these standards, perhaps this is no bad thing. As STS is
concerned with the problems human beings face in producing knowledge inside the world, its constructionist toolkit is surely appropriate in opening-up the complexities of translational research. Indeed if STS has demonstrated success at tackling the ‘hard cases’ of scientific and mathematical knowledge (Bauchspies et al., 2006), then this would suggest it is equally well equipped to tackle other cases related to science, technology, medicine, and increasingly, law, economics and business (Doing, 2008). I think that in the context of studying knowledge and innovation, constructionist methods are helpful and useful. It is the applicability and efficacy of its methods and theories which has made me appreciate STS. Quite simply, case studies and concepts developed by previous generations of STS scholars has helped me to better understand science, and deploying them in this study promises to help better understand translational research. Furthermore, as few practitioners, policymakers, or members of the public appear to think in these terms, constructionist methods provide a valuable resource in the context of social science intervention (Whitley, 2000, xi-xii).
5. Design of the Thesis

Introduction

The purpose of this chapter is to describe the research design and analytic procedures used in conducting this research. It will do so first by outlining the methods used, the key theoretical assumptions behind these approaches, and why these were deemed appropriate. The chapter then describes how the design was implemented in practice.

The methodological paradigm underpinning the methods procedures in this thesis is social constructionism. Paradigm wars between different approaches have received a vast amount of coverage in books and articles on social science research methods, and this is not the place to regurgitate well-worn arguments. Suffice to say, the constructionist approach adopted here has its roots in the works of philosophers like Schutz and sociologists like Berger.

Despite research being a practical activity, textbook representations of methodological routines, agreements, and debates have the habit of removing the practice from research (Seale et al. 2004, 1). As such, decontextualised methodological rules, advice and general principles have a tendency to appear rather abstract and removed from the day-to-day experience of conducting research (Ibid., 5). For Seale et al. (2004) there are limits to the use of methodological frameworks, which are helpful insofar as they fulfil two functions: social and political. Their social function is in encouraging principled choices to be made in the course of research practice, such as appropriate use of methods and adhering to good practice. The political function is in allowing researchers to make claims about the importance of their work, through attaching it to a particular paradigm position (Seale et al., 2004, 3).
This chapter starts by looking at how these two dimensions have shaped the research design of this thesis. Much more attention has been paid towards the social function of the methodology, as it is these considerations which traditionally fall within the ‘frame’ of a methodology chapter (see Goffman, 1974, Chapter 1). Hence although these are not hard-and-fast rules for guaranteeing good social science research, they have nonetheless provided useful guidance in shaping the design and conduct of the research in this thesis. After this section, the chapter will shift focus towards the types of research devices used and the rationale for doing so, then to recounting the practical steps that went into conducting fieldwork and analysis.

**Political and Social Functions of Research Methods**

In a reflexive, ‘confessional’ frame (Van Maanen, 1988), I will outline briefly some of the political motivations in following the methods/procedures adopted hitherto in this research.

In short, one of the main reasons qualitative methods and a case study design were identified early-on as strong candidates for conducting this research is that they have historically found favour in the ‘normal science’ of STS. Kuhn (1970) observed that as a scientific field matures, researchers tend to become increasingly reliant on standard problem-solving techniques that become part of the ‘normal science’ of a field. Having widely accepted ‘search routines’ is helpful in enabling fields to amass knowledge and grow, as it helps to simplify complex tasks. Although recognising that paradigms can constrain researchers, Kuhn also understood that they enable people to think (Kuhn, 1970). As STS has matured, it has developed tried-and-tested ideas and methods that are very helpful in opening-up an understanding of science. Qualitative research and case studies are two such features. The worldview underpinning qualitative research, that data should be ‘generated’ (read ‘constructed’) through forms of inquiry that are ‘open-ended, flexible, opportunistic, and require constant redefinition of what is problematic’ (Jorgensen,
is seen in STS as much more conducive to studying how knowledge develops amongst scientists.

Making arguments in the form of case studies was one of Kuhn’s many legacies on the field of STS (Law, 2008, 626). Part of Kuhn’s impact then was to shape the very normal science proceedings of STS itself. For instance, case studies documented failures and struggles ‘to bring objects and effects into existence, in the laboratory and in the wider context’ (Knorr-Cetina, 1995, 559). Case studies also enabled sociologists to capture the lines of controversy and disagreement that marked efforts to establish new knowledge as facts, and the social forces shaping these outcomes (Collins, 1985). These studies of scientists at work also provided exemplary empirical case studies of organisations from which organisation theorists could benefit (Czarniawska, 2009). The aim of laboratory studies was to open up the cultures of science to ethnographic sensibilities and methods. This would allow for the complex, non-linear dynamics of innovation to be captured. All this, of course, reveals that STS has its own history and agendas which are separate from those of whom it studies. STS scholars have their own professional interests, audiences, and problems in which its members are concerned with engaging: this study is no different. In following these standards, it is hoped that the results of the work will attract attention amongst audiences interested in STS and organisational sociology. Indeed both the empirical and theoretical literature from which this study has been inspired (and to which it hopes to make a novel contribution) have used qualitative case study approaches, rooted in social constructionist thought. Hence following tradition and standing on the shoulders of earlier giants in the field of STS seems like a reasonable and logical choice to make.

Good research practice does not exist in a vacuum but is determined by a research community. As members of a collective (e.g. paradigm, field, discipline) it is important that research is well crafted and incorporates good practices, whilst recognising that standards are always more or
less contestable. The irony of doing social science research as a social constructionist is recognising the situatedness of these standards, whilst remaining compelled to follow them (Hacking, 1999, 23). It is important for collectives to have these methodological guidelines, as it enables identity formation, responsible behaviour, and higher quality research to be crafted. This section will now link the mode of inquiry with wider epistemological assumptions with which they are associated (‘Case Studies’ and ‘Research Methods’). This is an important aspect of crafting good research, as it enables one to specify the strengths and limitations of the procedures used to gather and interpret empirical data. The term qualitative research encompasses a very wide umbrella of techniques, methods, paradigm assumptions, research traditions, and disciplines. Three that have been highlighted for this study are qualitative interviewing, direct observation, and document analysis. Although these techniques enable different forms of interpretation, they are often used in conjunction under the banner ‘ethnographic research’. The interactive nature of qualitative research is helpful in exploring phenomena that one believes to be non-linear and multi-formed. When performed successfully, it can produce various forms of data that facilitates rich insights into people’ experiences, accounts, memories, opinions, understandings, thoughts, ideas, emotions, perceptions, practices, actions and activities (Mason, 1996, 36-37), as well as about discourses, narratives and organisations permeating their local situations (Mason, 1996, 37).

Research Methods

For the empirical case studies qualitative interviewing has been identified as important means for generating data about translational research. Given the diffuse and under-theorised characteristics of this phenomenon, the qualitative interview appears particularly apt, as:
“Interviews are a highly efficient way to gather rich, empirical data, especially when the phenomenon of interest is highly sporadic and episodic.” (Eisenhardt & Graebner, 2007, 28)

As a research method the qualitative interview is taken to be based around relevant, purposeful conversations that are conversational, flexible and fluid in style (Mason, 2002, 225). Generally speaking, the approach has found favour in traditions that privilege accounts of social actors, agents, individuals, or subjects over other data sources and that propound the centrality of talk and text in illuminating the social (ibid.). Interviews with scientists frequently yield data on how they move from ‘tacit assumptions to explicit articulations’ about science and what norms/values are enrolled in accounts that defend/prosecute their interests (boundary work) (Gieryn, 1983). The perennial criticism that interviews produce knee-jerk reactions is scarcely problematic, as this would support the accepted STS point that scientists draw on norms that suit their situation at hand (Mulkay, 1976).

Constructionists believe that evidence and theories they themselves produce is also context-bound, situation specific and rooted in interaction (Mason, 1996, 41). Qualitative interviews are thus mobilised on the basis that actions and actors are social processes about which researchers ‘co-construct’ data with participants (Mason, 2002, 226). The qualitative interview technique has traditionally invited a level of flexibility and reflexivity largely absent in methods like survey research. Rather than being an embarrassment to the researcher, lack of ‘objectivity’ is accepted as an inevitable and indeed useful component of the qualitative interviewing technique (Latour, 2005). This does however make the issue of reliability contentious, as given the highly interpretive nature of the methodology and technique, it is not always clear whether other researchers in the same context would draw roughly the same results (Prior, 2003, 149). The same criticisms apply to observation and documentary analysis, which will now be discussed. The relative strength of these criticisms is considered at the end of the section.
Direct observation is useful for exploring phenomena as they occur in real-world settings bounded by time and space (Jorgensen, 1989, 9). This is apt as the research questions in this study are concerned with how a phenomenon ‘works’ as a social accomplishment in particular situations and settings. Such characteristics are at the very ‘foundations’ of (non-)participant observation’s inquiry and method (Jorgensen, 1989, 13). Like interviews, this approach has a propensity to generate data that captures the non-linear characteristics of phenomena. Observation methods resonate with the broader theoretical and methodological thrust of this thesis and its own intended scope, as:

“Direct observation is especially appropriate for exploratory studies, descriptive studies, studies aimed at generating theoretical interpretations... [and] findings of participant observational research certainly are appropriate for critically examining theories and other claims to knowledge.” (Jorgensen, 1989, 13)

Insights from data gathered using observation can be used to corroborate and cross-validate other forms of data, as well as develop existing theories and suppositions. This can include any of the dimensions mentioned above (see Mason, 1996, 36-37), but as they occur ‘naturalistically’ (rather than ‘artificially’ via the interview process). This technique is often thought to have the advantage of generating data which is less guarded than interviews, although the notion one can be thought invisible in observation settings is dubious (see below section ‘Becoming Immersed?’).

As Latour and Woolgar (1986) note, scientists routinely produce texts to be read, cited, and broadly influence (at least they hope) others in their tribe. As texts are akin to a form of material currency in this activity, documents provide a very useful resource to consult and/or examine. Following a very general definition, ‘documents are things that we can read and which relate to some aspect of the social world’ (MacDonald, 2001, 196). In essence then analysing how people
are represented in documents is another means of observing ‘what people do’ (Prior, 2003, 114). I would add that they also enable observation of how humans attend to non-humans. All documents are socially produced and are thus never neutral statements of fact (Macdonald, 2001, 196). Most documents also aim to be persuasive and performative in some form, in that they do not intend simply to represent aspects of the world but to change them (ibid.). The breadth of the above criteria meant a lot of the information sources drawn on before and during the pilot and main case studies qualified as ‘research documents’. In researching the study a number of policy documents and trade journal articles were consulted in relation to translational research. Likewise hand-searches and full database searches of scientific journals were carried out to develop ideas about the definitions, emergence, changes and understandings of this term over time. The other major resource one can consult in this respect is the internet. Search engine searches were routinely carried out, albeit with some scepticism as to the quality of sources.

There are a number of strategies for scrutinising documents. This thesis draws on Latour and Woolgar’s five-fold typology for studying argumentative techniques in scientists’ statements (see Chapter 3), which was deployed in order to code formal texts produced by members of the cases. This approach is akin to discourse analysis (DA) (although they do not call it this), as it was adopted in order to study the kinds of entities members of the cases recruited into textual accounts and how referents and entities were interlinked (Prior, 2003, 118). Hence in DA:

“One has to attempt to get a picture of the ways in which the network of references interlock. It is, perhaps, what we might call a matter of intertextuality.” (Prior, 2003, 122)

Although useful, one cannot understand how problems are resolved in practice using documents alone (Prior, 2003, 121). To answer such questions documents should be supplemented with
interview or observation data. Another way of generating data about documents is not by looking at their content, but by observing their use as resources in naturalistic settings. Prior urges researchers to observe how documents (as intermediaries) are made to carry agency (following ANT) in organisational encounters and settings. This lends an extra-dimension to use of documents in qualitative research that goes beyond simply treating them as inert receptacles of text (see Prior, 2008).

In sum, each of these methods should enable the researcher to construct interesting and insightful stories about how the phenomenon is made to work within different settings and situations.

**Case studies**

Although no single format exists for carrying-out case study research (Eisenhardt, 1989, 40), it has come to be seen as a particularly useful tool for building theories in new topic areas and/or developing further insights into phenomena on which little is known (Eisenhardt, 1989, 532, Eisenhardt and Graebner, 2007, 26). Hence case studies will be deployed in an effort to contribute to our understanding of the dynamics of translational research, as well as to make an ‘idiographic’ contribution to theoretical issues proposed in the literature review (Platt, 1988). Case studies generally involve investigating phenomena in a naturalistic context a feature which distinguishes them from experimental research designs (Yin, 1994). According to Miles & Huberman’s definition:

“Abstractly we can define a case as a phenomenon of some sort occurring in a bounded context. The case is, in effect, your unit of analysis.” (Miles and Huberman, 1994, 25)

One of the most important features of case study research is the scope of its sampling logic. As with quantitative research, there is usually an implicit belief that the cases selected somehow belong to the population of interest (Patton, 2002, 238). But in terms of scale, the case study
strategy is ‘ill-equipped to address the questions “how often”, and “how many”, and questions about the relative empirical importance of constructs’ (Eisenhardt & Graebner, 2007, 27). These sorts of questions have been more the domain of survey research, where the interest is to look for certain features across a relatively large number of cases (Hammersley et al., 2000). Instead with case studies, the ‘overall idea is to become intimately familiar with each case as a stand-alone entity’ (Eisenhardt, 1989, 540).

The approach one adopts should rest largely on the types (and scope) of questions one is asking. Some models, like Yin’s, are based upon realist epistemologies and designed with a view that case studies should attain generalisability. By proxy claims about generalisable knowledge depends upon the social world being structured, and that cases can be fixed, stable, and are ‘out-there’ in the social world for researchers to capture. In this vein, with regards sampling, Yin (1994) argues that the researcher must determine in advance what constitutes the main unit of analysis (e.g. ‘the organisation’), and must not stray too far away from this towards embedded sub-units (e.g. individuals’ experiences) (Yin, 2003, 45). This realist version of cases is one which constructionists would refute.

The case study strategy being adopted here is one that seeks to (re)construct each case as an end in itself (Becker, 2000). I intend to tell stories about each case and how they got to be that way (ibid., 229), and to draw comparisons between important aspects of each case. Miles and Huberman note that ‘studies may be of just one case or of several’ (Miles & Huberman, 1994, 24). An advantage of performing two or more case studies (rather than one) is that it allows comparisons to be made (Becker, 2000, 227). Recognising distinctions enables in-depth analysis of how a phenomenon varies according to particular situations and contexts, therefore hopefully contributing ultimately to a more nuanced understanding than had previously been available. In the context of this study, it enables us to open-up and complexify linear, reified innovation concepts.
Not only is Becker’s design more conversant with the emergent and iterative mode of inquiry that ethnographic methods inspire, but it also refutes the existence of a fixed reality ‘out-there’ that researchers can simply capture (e.g. the ‘organisation’). Becker’s cases are not treated as a priori structures of the type found in survey research, but as things that are co-constructed over the course of the study (Becker, 2000, 227). It follows outliers should be taken as potentially important aspects of cases rather than inconveniences or irrelevances (Strauss, 1987). As a constructionist researcher I believe Yin is incorrect about maintaining ‘the organisation’ as one’s main unit of analysis. With ANT and other constructionist approaches to organisations it is the process of production (‘organising’) in which one is interested, not ‘the site’ per se. Yin’s approach implicitly assumes the phenomenon and the site to be somehow the same thing. But these three sites were not the only ones which could logically have been sampled and a great many things could have been studied about each of them which were not. Following constructionist organisation theory (Czarniawska, 2008), the formal organisation is just one of many products of *organising* and its relative importance should emerge through (not prior to) a description of the production process. With this in mind, the main sampling approach used to construct each case was to ‘follow the actants’ (Latour, 2005) in order to describe organising which produces the phenomenon, rather than sticking to the production sites as the main units of analysis. This has the advantage of bringing into focus organising which occurs above, below, and beyond the formal organisation: important inputs which Yin’s pre-occupation with formal organisations renders opaque. Hence although for practical purposes of presentation each of the three chapters appears to focus on a formal organisation, the actual content deviates from this framing by attending to ‘organising’. Becker’s model is more sympathetic to the specific concerns of STS, particularly as it does not require the analyst to impose a unitary framework on their cases (as Yin’s does). Instead the fluidity and flexibility of this style is conducive to mantras like ‘follow the actant’ (Latour, 2005), whereby the researcher describes the scientists’ efforts to
build a structured world, rather than forcing them to conform to abstracted criteria of demarcation imposed by previous researchers (Callon et al., 1986, 9). Hence:

“Knowledge of an organisation resides in the first place with the actors; observers may have knowledge about an organisation, which does not result from any privileged access to reality.” (Czarniawska, 2008, 7)

The observer’s view will always be theory-impregnated, but in order to be reflexive one should be transparent about introducing existing literature into the case. Although literature helps identify salient features of each case and its parameters (Miles and Huberman, 1994, 26), it should produce ‘sensitising concepts’ rather than definitive concepts with which to construct cases. Indeed analysts should avoid, where possible, trying to apply rigidly the relationships between variables and explanatory theories from the outset (Eisenhardt, 1989, 536). In this thesis the ‘context of the case’ follows an empiricist definition in which ‘context’ encompasses literally those texts brought before the researcher, primarily in the form of materials displaying the actant’s knowledge (e.g. data), but also some existing social science resources (Czarniawska, 2008, 7).

In selecting a constructionist approach one is making a particular set of commitments towards the treatment of scale. Clearly Becker’s model makes an explicit epistemological statement favouring depth over breadth. It follows a logic that the complexity of the social world and small sample sizes are such that it is inappropriate to expect significant generalisations from case studies (Stake, 2000). The only form of ‘external validity’ it can hope to produce realistically is ‘naturalistic generalisation’ (ibid.), or ‘generalisation within the case’ (Creswell and Miller, 2000). The design may also be transferable to other case studies being carried out at a later date (Lincoln and Guba, 2000). Case studies form an appropriate basis for a ‘phenomenon-based’ contribution (Eisenhardt & Graebner, 2007) to translational research. Whereas the lack of
breadth is often pointed as a weakness of case study research, in the context of STS intervention in science policy, depth of studies is taken as a considerable asset, given that policymakers frequently lack detailed understanding of complex innovation concepts (that become oversimplified and linear).

One blemish often accused of this type of cross-sectional design is that one cannot be confident in determining whether findings about patterns collected from one moment in time will apply at another (Bryman, 2012, 390, 392). For this purpose a longitudinal design would clearly be preferable. Yet observing patterns at different points in time is not desirable because it renders the findings more ‘valid’ as Bryman seems to be suggesting, but because it provides for more interesting stories about shifting ontologies of objects and how social relations get ordered around them over time. This benefit is exemplified by case studies of innovations in the social construction of technology tradition (Bijker, 1995). Intellectually then whilst longitudinal studies constitute a formidable ‘trial of strength’ (Latour, 1987), I do not believe it lends itself well to the practical demands and finite resources of PhD research. The decision to carry-out cross-sectional studies is thus one driven as much by pragmatism as any normal science ideals. Clearly the selection of case studies will always be determined by people’s willingness to take part and the general feasibility of conducting fieldwork (e.g. within a reasonable time-frame) (Yin, 2003).

Encountering ‘resistances’ when scaling-down is after all an inevitable part of doing research (Latour, 1983). Another criticism typically levelled at this kind of study is that its ethnographic methods are ‘overly subjective’. On the contrary ‘being limited to one’s own perspective’ should be seen as a position of strength rather than weakness, as it allows movement to take place at relatively little cost between one’s theory-impregnated position and reality that is documented by respondents (Latour, 2005). As Latour notes, this ‘flip-flopping’ describes how all scientists go about ‘discovering’ knowledge and is therefore a particularly weak criticism of constructionist
methods. Hence this ‘subjectivity’ need not impede the production of robust knowledge, as robustness itself is a relational concept.

Methods: Putting the Design into Practice

Research Ethics

In his account of what it means to be a good qualitative researcher, May argues the importance of accounting not only for what has been produced, but also how it was produced (May, 2001, 29). This is important in considering the wider contribution of research to society and the kinds of responsibilities it should carry (May, 2001, 29-30). One frequently overlooked dimension of this ‘how’ question is the issue of research ethics. An ethics application was submitted to the University of Surrey’s Ethics Committee prior to the pilot study, which was granted on 15 February 2011 (see Appendix 2). This was then carried over into the main case studies. As professional adults familiar with the world of research, the respondent populations have not been deemed a vulnerable group. Similarly, lines of questioning are not intended to focus on issues that could be deemed particularly ‘sensitive’. Providing information sheets (Appendix 4), gaining informed consent (Appendix 3), storing data according to the 1998 Data Protection Act, anonymising accounts, and being sensitive to questions that might cause distress or offence are measures deemed satisfactory to protect the research subjects’ wellbeing and ensure the ethical robustness of the study. Details such as names of individuals, organisations, research papers and other clues which might unravel the identities of informants have been changed or omitted. For instance, the names of individuals in the empirical chapters are made-up pseudonyms. Likewise I have sought to make opaque the identity of the universities with which the groups were associated- all that I have indicated is that these can be considered research-driven, as opposed to teaching universities.
In making such decisions it is clear that research is not a value-free activity, as it involves considering and developing a critical stance on moral questions. Hence the increasing prominence of research ethics would appear to support the STS constructionist view that knowledge production is not ‘freely produced’ but reliant on the resources ‘out-there’ in nature and society (Czarniawska, 2009). Positivism produces an objectifying representation of human beings which appears to run counter to contemporary research ethics discourse. An inability to confront questions about research ethics, owing to its objectivist perspective and passive style of writing, is another flaw in positivist views of knowledge production. The use of reflexive, ironic styles of writing should put constructionists at an advantage over others traditions, particularly positivism, as they can openly acknowledge problems encountered during the research process and how these were resolved.

**Pilot Study**

The aim of this section is to report on procedures followed in the pilot study and reconsider how this exercise informed the design of main studies. The prospect of doing a pilot study was proposed by my supervisors near the beginning of the PhD. At this point in time my knowledge of the social world of biomedical science, medical schools, and universities was quite sketchy. The pilot provided a means for expanding my understanding and generating further research questions (Yin, 2003). What was needed was an organisation that was relatively local, would allow access to do a small case study, and corresponded to the study’s population of interest. By the end of the first year the following ‘purposive sampling’ frame had been constructed (Patton, 2002, 238):

- Research-active units

- Located broadly within the biomedical sciences

- Claiming to be carrying out translational research (in public or private)
- Have agreed to take part in the study

We decided to get in touch with a university-based clinical trial unit (CTU) carrying out drugs trials on behalf of commercial clients. The Director of the centre was contacted and agreed to meet, enabling me to learn about the centre’s activities before formal interviews began. She agreed the study could take place once favourable ethical opinion had been obtained. An application to the University Ethics Committee at Surrey was submitted and given favourable opinion.

Respondents were recruited via a ‘snowballing’ technique (Morse, 2007, 238), with the help of the director, a critically-minded ‘gatekeeper’ familiar with the inner workings of the organisation and therefore able to advise who it would be appropriate to interview (Hammersley and Atkinson, 1983). Two of the members identified were shortly to leave, thus meetings were promptly arranged. Prior to interviews scientific publications by the researchers and their colleagues were read. This allowed me to ask about translational research in relation to examples from their own work. Interview schedules were drawn-up for the sessions. These were constructed abductively out of theoretical literature, how to interview books, discussions with supervisors, and informal meetings with the director. However writing the schedules themselves required translating theoretical questions into interview questions (Wengraf, 2001) (See Table 2 in Chapter 5). The supervisors offered reassurances that that the schedules were adequate and made additional suggestions.

The pilot study ended-up consisting of interviews (n = 5) with members of the CTU based within a research university in the south of England. Appendix 1 displays the roles of CTU members and the duration of the interviews. The pilot interviews were helpful and revealing in a number of ways. They provided me with confidence in the sampling criteria, including reassurance that cross-sectional interviews with academic scientists about translational research would be a
fruitful means of generating relevant data about this underexplored phenomenon (Eisenhardt and Graebner, 2007). In addition, capturing the experiences of different members of the organisation appeared to be a useful strategy that has been taken forward in later case studies. Shortcomings in my knowledge about this professional world were also greatly improved by the pilot process. Despite earlier apprehensions, there appeared some merit to Strauss & Corbin’s argument that not having deep prior knowledge of a phenomenon enables researchers to see important things that would otherwise appear ‘obvious’ (Strauss and Corbin, 1990, 42). It was recorded in post-interview fieldnotes how I felt I was questioning much of what the respondents took-for-granted, and that this tactic should be taken forward. Academic units appear suitably convenient bounded entities along which to begin constructing and distinguishing case studies. However, the fact that structures and parameters of the CTU only became clear to me once had fieldwork commenced, has reinforced the appropriateness of Becker’s instruction to determine the boundaries of the cases during the study rather than before (Becker, 2000, 227).

The sessions suggested that some lines of questioning in the interview schedule had been more successful than others. For example, respondents were able to talk at length about their career structures and how translational research intersected with the specific problem choices. Most were much less knowledgeable about the provenance and national policies on translational research. Hence subsequent schedules in the main studies were adapted to incoming findings in the data, further reading and practical experiences. When analysing the interviews, I found there to be some ambiguity and interpretive flexibility towards translational research across situations. For instance, reading the website of the centre, its faculty, and the university’s technology transfer office was very revealing in terms of the promissory rhetoric that shrouds translational research, particularly when organisations are attempting to project a favourable outward-facing image. ‘Backstage’ however, there were some reservations. When asked about the term, two respondents (physician and marketing officer) expressed scepticism towards translational
research, adopting a tactic of ‘dismissal’ (Oliver, 1991), citing it as a cosmetic relabeling of what existed before. It was seen simply as part of the games being played, and of little interest or importance to them. Notably in their roles, these two respondents were not constrained by institutional pressures to the same extent as the other respondents, who were more inclined to show ‘compliance’ (Oliver, 1991), possibly because of the managerial responsibilities their roles demanded. This indicated that it would be undesirable to restrict sampling in the main studies to managerial figures alone, but to access a range of respondents. The multiple local ontologies of translational research also appeared to reinforce the utility of ANT in providing a set of devices with which to make sense of this variability.

**Main Case Studies: Designing the Sample**

Having completed a pilot study I was looking to move onto cases which would constitute the main empirical basis of the research in the thesis. The precise number of case studies had not been decided upon, but I knew I had wanted to do more than one so as to lend the study a comparative angle. One of the most important phases in the course of ethnographic fieldwork is designing a sample frame and gaining initial access into the population in which one is interested.

Constructionist approaches to case study research stipulate that the research design should aim to be exploratory and accommodate surprises. This theoretical position impacted on the sampling approach adopted here. Once initial access had been secured, the main sampling logic was to ‘follow the actors’ (Latour, 2005) and organising they did in relation to the phenomenon. Prior to the fieldwork on the main studies, my sampling criteria was to conduct case studies on researchers and their teams whose research agendas related to at least one of the definitions of translational research set-out in a medical review article (Khoury et al, 2007). This helped provide inclusion-exclusion criteria for populations relevant to this phenomenon (Patton, 2002). The articles’ scripting of different roles for academic researchers in relation to translational
research concurred with both NIH’s translational research policy in the U.S. (Woolf, 2008) and MRC-NIH’s in the UK (Cooksey, 2006, DH, 2007). Given that academic researchers working on related problem areas were being positioned as responsible for taking-up this challenge within the formal policy discourse, for this reason this population bracket was deemed relevant to this phenomenon. When names of candidates were suggested to me by colleagues, I checked on websites whether their work met with Khoory’s framework and/or whether they had declared an interest in translational research. When either of these criteria could be met, I then made contact with the researchers. Although there was this initial rationale for going into these sites, at the early stages of fieldwork the character of their relationship to the phenomenon was uncertain. As a practical step in identifying case studies, I first encountered formal organisations as ‘candidates’, because these were sources of information that were publicly visible at the outset. But it was through the writing of the case studies that the relationship between the sites of production, actors’ organising and phenomenon became known. Indeed the very rationale for doing empirical studies was that I did not know the answer to these questions.

The aim of this thesis is not to generalise theoretically as with most statistical probability and some qualitative non-probability studies (Saunders, 2012, 42). The aim of the case studies is to present an authentic account of the practices of populations in the studies. There are no hard and fast rules for what constitutes exactly an authentic qualitative account—this is something to be negotiated between author and reader of a text. Authenticity can be enhanced through gaining access to information provided by an appropriate cross-section of elements relevant to the workings of the groups. This number cannot be determined in advance, but must be decided, as it were, after one’s initial entrance into the field, as one’s knowledge begins to increase. The dictum ‘follow the actor’ is useful in this regards, as starting by contacting and conducting interviews with senior employees can leave traces of other elements on which they depend for getting things done. In addition to people, I could also access non-humans such as
grant applications and journal articles discussed during interviews and/or meetings, although this brought with it different kinds of access issues, such as confidentiality restrictions or institutional payment barriers. These various traces can then hopefully be pursued further through the course of fieldwork and into analysis.

Once initial interviews with seemingly important individuals\(^\text{22}\) had been conducted, adopting a ‘snowballing’ approach was deemed compatible with the ANT logic of ‘follow the actor’. Hence, after these initial interviews the range of people eligible for the study was broadened to anyone these initial gatekeepers had earmarked as relevant to this problem, who I predicted would be relevant, and who would likely be accessible for an interview. In sampling interview respondents \textit{within} each of the cases, the sampling frame resembled a form of \textit{heterogeneous purposive sampling} in which ‘participants with sufficiently diverse characteristics [were selected] to provide the maximum variation possible in the data’ (Saunders, 2012, 45). This was a decision informed by experiences of conducting the pilot study, where I found differences in respondents’ answers towards certain questions about translational research, where different versions about actor-networks emerged. In practice such ‘technical’ sampling considerations must also be offset against practical concerns for expediency and pragmatism. The snowballing logic where respondents were included on the basis of their being recommended or volunteering themselves into the sample (Saunders, 2012) did not always yield participants. This I found to be a frustrating feature of doing qualitative research. Of course one of the reasons this causes anxiety is because it injects uncertainty into decisions about when to cease sampling. This situation, often associated with non-probability, qualitative studies in real-world settings, brings the controversial voice of the researcher into the foreground, who must exert their own judgment about when to cease sampling rather than follow ‘objective’ standards/conventions. In

\(^{22}\) They adopted positions as spokespersons for their organisations and occupied positions of formal seniority within the institution of the university e.g. as professors.
the cancer case study (Chapter 6) I was forced to end sampling before the full population had been sampled, which was a decision made out of necessity owing to the sluggish response rate to adverts for participants. In the other two studies, close to a full sample of group members had emerged, thus removing such a dilemma. In the case of gaining access to observe weekly team meetings of the obstetrics group (Chapter 8) a date was agreed in advance for when I would stop attending for which both parties were content.

With such intellectual and practical constraints in mind I decided to cease with sampling in each case based on the following criteria: when it became clear that there were no more volunteers available/forthcoming, when I had already sampled enough of a heterogeneous cross-section of those working in the groups, and having made a judgment that raw data transcripts generated hitherto would provide materials of sufficient quality to proceed in re-telling (through the medium of my written text) how the problems of translational research are worked-out within these production sites. One means of judging the latter criteria was through referring back to fieldnotes and listening back to audio recordings to ensure I had been able to ‘return’ from the field with responses which I thought helped both answer my initial questions and helped me to understand their practices better than I had done before. On each occasion I was largely satisfied with the quality and quantity of data. Clearly this approach to designing and conducting empirical research is not completely infallible. But in following certain standards of ethnography and in trying my best to pursue important values such as honesty, transparency and clarity, it does at least mean that my methods are not weak. This is all that one can ask of a science.
Journeys into the Field

As expected, it was not easy to enrol informants into the study: it required perseverance, nous, compromise, and luck. In his description of gaining access to a police department, Van Maanen states that any researcher must undergo ‘a lengthy process of examination’ before s/he can ‘penetrate the back regions’ of such an organisation (1988, 85). This was not my experience on entering the settings for this research: my experience was one characterised more by apathy than mistrust. Perhaps this was due to variations in the types of professional organisations involved in his study and mine. The processes of applying the sampling frame and gaining access in each case study will now be recounted.

Two colleagues acted as important brokers in putting me in touch with the gatekeepers who would become crucial in helping me gain access to the three cases. I had discussed informally the criteria for inclusion of cases in my study. When names of candidates were suggested to me by these brokers, I checked on websites whether their work met with Khoury’s framework and/or whether they had declared an interest in translational research. When either of these criteria could be met, I then made contact with the researchers. But at this stage I knew little of the details of how their work was translational, beyond my visits to their websites and reassurances from the fact that they had volunteered themselves to my colleague that they were eligible for the study. Hence although there was rationale for going into these sites, at the early stages the character of its relationship to the phenomenon and the actors-actants implicated in this work process was little known. As such gaining access for interviews and/or observation was crucial to fulfilling the aims of the research.

To become more familiar with each case, the tactic was to gain initial interviews with seemingly powerful individuals²³, then once these were conducted adopt a further ‘snowballing’ approach.

²³ They adopted positions as spokespersons for their organisations and occupied positions of seniority within the institution of the university e.g. as professors.
This was deemed compatible with the ANT logic of ‘follow the actor’, as interviews pointed a trail towards other elements gatekeepers were reliant on for getting things done. Hence after these initial interviews the range of people eligible for the study was broadened to anyone these gatekeepers had earmarked as relevant to this problem and who would likely be accessible for an interview. This approach did not always work: potential gatekeepers would lose interest, turn out to have no one ‘beneath’ them, and/or could not persuade others to take part. This I found to be a demoralising feature of doing qualitative research. Nonetheless, it was only through entering the field and writing of the case studies that the relationship between organising of actors-actants, the sites of production, and phenomenon could be known. Therefore, the snowballing technique was deemed to be both theoretically consistent and practically useful for the purpose of designing case studies. The finer details of gaining access to each of the three case studies will now be discussed.

In the *Obstetrics* case, interviews were arranged initially with two GP/academics (both professors) researching in the field of primary care. The first colleague had spoken to them about the possibility of my carrying-out interviews, which had paved the way for me to contact them by email. Interviews were subsequently held. It was hoped that these one-off interviews would ‘snowball’ into main studies (Morse, 2007, 238), but soon it emerged that one of them did not have a large enough team to merit conducting a full case study (although his interests and experiences of translational research provided a great number of interesting questions for exploration). The second professor, when asked, could not think of anyone else in his team who I could talk to. I asked him about this in a follow-up email a few days later to which I received no reply. However, this interview was useful given that he was the head of the department hosting what would later become my obstetrics case study. As such, the interview provided important insights into the strategic vision members of research groups in the department (including obstetrics) were expected to follow.
Shortly after carrying-out these interviews, my colleague put me in touch with the head of Obstetrics group, a nascent collective that had recently moved into the same public health department as the primary care professors. The Centre Lead was an academic clinician (consultant/professor) who was expecting an email from me, having spoken to my colleague. An interview was arranged. Before the interview I had been planning to inquire about the size of his group and what opportunities there might be for further fieldwork, including interviews and observation. However, to my surprise, I did not have to wait until the end of the interview to find out: during his interview he invited me to observe meetings in order to learn more about how their research was coordinated. His role as gatekeeper was thus crucial in the story of how this case study came to be framed. I recognised that observation could potentially provide another dimension to the case study, by allowing me to observe their organising in situ. I took up his offer, which resulted in attending weekly team meetings as a non-participant observer. This lasted for longer than anticipated (8 weeks), owing to the suggestive insights being thrown-up and their willingness to accommodate my needs. I also attended the group’s inaugural week of lectures and dinners, as well as data-monitoring and trial-steering committee meetings. I was also sent minutes of meetings and their clinical trial protocol documents.

The cancer case study started when I was introduced to the Director of a large Cancer Faculty by a colleague at a lecture we had both attended. This belonged to the same large medical school as the obstetrics group, although they were located in different faculties. The two faculties were identified within the school’s portfolio as being primarily research-led, although they did also perform teaching duties. The head agreed to be included for an interview in my study and a meeting was promptly arranged the following day. After the interviews, the sample ‘snowballed’ (Morse 2007, 238), as he put me in touch with a professor who was principle investigator (PI) of a laboratory in the faculty. He was interviewed and afterwards put me in touch with someone who worked for a major charity that had just recently become part of the institute (and is
physically located in the main headquarters). I also asked the PI to suggest other members of his laboratory who could be interviewed. Although he was willing in principal for me to do more interviews with members of the lab, he was apprehensive about me entering as an observer. In a conversation we had immediately after our interview, he cited concern that this would take-up too much time and cause too much disruption to the work flow of the laboratory. Likewise he told me that he did not want his PhD students to be considered for interviews, as he was concerned about their time-management skills and saw involvement as an unnecessary distraction.

The interviews I did manage to conduct took a long time to arrange, but eventually a senior lecturer, two post-doctoral researchers, and a technician were interviewed out of the 23 people in the laboratory. One post-doctoral student replied to a circular email I had written that was sent around the lab by the PI for the second time. After our interview another post-doctoral student came forward one week later, apparently having spoken with her interviewed colleagues about the experience. Once this interview was completed, nobody else came forward. The senior lecturer had been mentioned several times in the PI’s interview, and appeared to be presented as a key player within the laboratory. I decided to ‘cold call’ him with an email, to which he obliged. After our interview, I asked him whether he could put me in touch with a technician in the lab. The importance of the technician’s role emerged as important within the senior lecturer’s interview as well as in STS laboratory studies (Shapin, 1989). The senior lecturer showed me to a technician at work in the laboratory, asking him in front of me whether he would be interviewed, to which he agreed. At this point I became satisfied, as well given the time constraints and difficulties in getting access, that I had interviewed a reasonable cross-section of the laboratory.

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24 No one came forward on the first occasion.
The gatekeeper for what became the *diabetes case study* was the Dean of a medical school. This was based in a separate university to the aforementioned cases, providing reassurances that findings about the travel of translational research and associated concerns were not simply peculiar to one particular university. I contacted him via email after a colleague who had worked with him before recommended him. The past interactions of my colleagues with this person was (I suspected) key in getting him to partake in the research. He responded positively to the email and we arranged to speak over the phone. During this meeting he stated that he would be willing to circulate an email to colleagues requesting *phone interviews*. He was less receptive to the idea of me entering the field as an ethnographer, as he was conscious of placing a burden on colleagues who were already very busy, and suggested that I would be unlikely to receive any willing volunteers for this request. Furthermore given the considerable distance of the university from where I lived, he suggested it would be unpractical to return for face-to-face interviews at different times. With this in mind I sent him an email describing the research and what it would involve, which he initially forgot to send around, doing so once I had sent a polite reminder.

After two weeks there had been no replies, at which point I contacted each of the people to whom the gatekeeper had carbon copied into the email. At this point I received two replies from the PI of a diabetes group and a clinical researcher sometimes affiliated with the group, who would subsequently emerge an important respondent in the diabetes case study.

Phone interviews were conducted on consecutive days. The clinical researcher recommended I contact a post-doctoral student who he co-supervised with the PI. The PI said he would ask two more people in his group- a full-time PhD student an academic-clinician doing a PhD part-time - to participate in the study. These three people came forward and interviews were promptly arranged over the telephone. At this point it became clear that the diabetes group would constitute an appropriate entrance point around which to build the third case study. Despite only having informal links to the diabetes group, there was still strong reason to include the
clinical researcher in the sample: the PI made it explicit that the clinical researcher was an important ally linking his basic research to his translational operations. Finally I conducted an interview with the Dean of the medical school who had acted as gatekeeper to the laboratory. The sample ceased here after it became apparent that the group was actually being newly assembled and there were no other formal members of the group apart from other PhD students, a category of respondent with whom I felt I had reached ‘saturation’.

Interview Schedules

Wengraf recommends that within semi-structured interviews, interviewer’s questions should, in most circumstances, be kept to a minimum (2001, 72). In practice this meant trying to allow the respondent to get to an issue of their own accord and introducing a number of prompts rather than pointed questions per se. Nonetheless, the schedules served as a reminder of issues I wished to see covered during the interviews. At various points I would scan over the sheet in order to check which themes they had addressed in their responses. Below is a table which seeks to distil questions discussed in interviews and link these back to theoretical issues in which this research is interested (Table 2). The aim of the table is to show that the interviews, whilst flexible, were directed rather than aimless conversations. The Theme column lists some important concepts/ideas (based around relevant literature, notably Latour and Woolgar’s frameworks) explored through the interviews. Following Wengraf’s dictum that ‘a theory question is never an interview question’; the tables present a Theory Question column which are then translated into Interview Questions. Unfortunately the original schedules have been omitted, because they were written as a practical reminder for me to use in the sessions, rather than to make for easily accessible documents to be read by others at a later date. The table therefore presents a more user-friendly version. I recorded in my interview fieldnotes that the schedules appeared to work well on most occasions. On some occasions they were little needed, as the more verbose respondents covered a lot of these sub-themes of their own volition
without being prompted. For interviews with more taciturn respondents I was more reliant on the schedules. The order of questioning would be modified slightly according to the flow of the conversation and the extent to which respondents were able to answer the initial interview question/theme. The fieldnotes also state that all of the questions were asked in every interview, except once, where Turin in the third case study was in a hurry and had to conclude the study before I had chance to ask all of the questions. The issue of ‘rapport’ will now be discussed further with reference to people from each case study.
<table>
<thead>
<tr>
<th>Theme</th>
<th>Theory question</th>
<th>Interview question/prompt</th>
</tr>
</thead>
<tbody>
<tr>
<td>History of the Laboratory/ ‘Object Lessons’</td>
<td>Local meanings of translational research? In what networks is it made visible? What work do these associations do? What are limits of involvement?</td>
<td>What, if anything does translational research mean to you in the general context of your work in X… And what about on project Y? Who would be you collaborators and what do they do?</td>
</tr>
<tr>
<td>Inscription</td>
<td>Is translational research made present in writing actions? Whose agenda does this conform to?</td>
<td>Where does funding generally come from? Do you follow vision of a particular council? Where would you publish?</td>
</tr>
<tr>
<td>Microprocessing</td>
<td>What are the issues in collaborating with others? How are boundaries negotiated?</td>
<td>What processes are involved in working with others? Do you agree about everything? Where is the common ground?</td>
</tr>
<tr>
<td>Cycles of Credit</td>
<td>Group Structure; Careers of individuals and formal organisation</td>
<td>Is your role tied into translational research? What are your priorities? Are you concerned about the REF? How well-funded is your area? Is translational research going to become more or less important?</td>
</tr>
</tbody>
</table>
Becoming Immersed?

This section reports, from my fieldnotes, experiences of conducting fieldwork and impressions I had of the people and what I thought was their impression of me.

When I first entered the obstetrics group as non-participant observer I was not at all familiar with the local context. I had gone in wanting to get a ‘sense’ for the day-to-day articulations and negotiations involved in doing research, and how translational research intersected their organising. However, I did not know how to look for this, or which utterances or actions were pertinent. To compensate I scribbled down as many fieldnotes as I could during and after the meetings. The first two meetings were very confusing experiences in a number of ways, as I had little idea of what to expect and felt as though I had been thrust into the deep-end of their organising activities, equipped with little knowledge of their local idiosyncrasies, acronyms, or in-jokes (the group would sometimes laugh at things someone said which I did not understand). In the days following the second meeting I spoke informally with my supervisor and told him that the meetings seemed quite routine, mundane, and that no mention was made of the issue at the centre of my research. He replied with something I had not considered: perhaps this was the point. Not much does happen at these meetings (at least not much of any excitement); people may be bored; the day-to-day realities of these people in this ‘backstage’ setting may not be permeated with sexy, fashionable ideas. This triggered the realisation that even if the time spent as a non-participant observer yielded little by way of direct insight into how translational research ‘works’ in this naturalistic setting, this in itself should be treated as an empirical finding, rather than reason to dismiss the case. As such I decided it would be worth continuing to attend the weekly meetings and made a request to the Centre Lead about making further visits, which he granted.

Over time, although still not fully embedded in the local knowledge of the group, I became more attuned to the meetings, their format, content, and some of the general problems these people
were facing. Being able to record and listen back to recordings was obviously helpful in this regard. Even though I had believed that the informants were never particularly riled by my presence, I felt as the weeks passed my turning-up became more familiar part (albeit temporary) of their meetings. I had recorded people seeming less surprised on entering the room to find me sat there, an inference based upon the types of greetings I would receive from the informants, who had also come to know me on first-name terms. This was reinforced by the fact that they had also invited me to an important week of events they had planned which included the Centre Lead’s inaugural lecture, two trial steering committee meetings, education program meetings, and CLRN meetings. This was also complemented by my attendance at ‘social’ events, such as two evening meals in restaurants and two wine receptions. The familiarity I had built-up meant in interviews I was able to pinpoint specific studies they had conducted of which I had some prior knowledge and integrated these specifics into the interview schedules (for instance, I asked the Senior Clinical Lecturer to relate translational research to the epilepsy multi-centre trial for which I had observed the steering committee meeting). Again, perhaps owing to the rapport I had built, I found respondents were helpful in engaging with the questions I presented to them in interviews.

As the cancer and obesity case studies did not afford me access to observe meetings or other occasions for organising in situ, I was unable to generate similar data sets to those reported on in the Obstetrics case chapter (Chapter 8). As such it is my impression that I never got as close to these people’s contexts and was not able to become as saturated as I was in the obstetrics. This is not a complaint, but a ‘confession’ as to potential limitation to parts of this research. However, the fact that interviews and documents were the main sources of data did not mean my experience with these cases was completely devoid of familiar relations. During my visits to carry-out interviews in the cancer laboratory, I was able to take cursory glances at the laboratory’s benches and offices, recording the lay-out in notes immediately after leaving. On
one occasion I spent approximately 4-5 minutes by the benches, having been introduced to their technician by the senior lecturer after our interview. This afforded me first-hand a ‘snapshot’ of the laboratory space, which complemented interview accounts describing the laboratory life in which informants were variously embedded. The inside of the building was very different to the offices of the obstetrics group: it was a molecular biology laboratory resembling many of those depicted in STS laboratory studies. This reinforced the expectation that the cases being studied in this thesis were embedded in distinct epistemic communities, so much so that the very physical built environments in which they worked looked completely different to an outsider. As an organisational unit it could be likened to a factory which ‘stocked’ the various resources, human and non-human, abstract and material, needed to assemble experiments (Knorr-Cetina, 1999, 27). As an organisation, the object-centred-ness of the bench spaces was one of the most striking features I noticed on entering this building, meaning it was an idiosyncratically-designed workspaces centred around assembling resources which would enact and respond to emergent demands of experiments (ibid.). The individual work spaces of the benches always appeared quite cluttered, with large numbers of bottles and containers stacked on the shelves, trays of pipettes, stacks of test-tubes, beakers, microscopes, micro-assay machines, sinks, and microwaves situated at the lab benches. There were computers situated at the end of the open-plan room, which along with the laboratory benches were assigned to those (technicians, post-doctoral researchers, PhD students) whose roles were more-or-less inextricably tied to material elements of the laboratory’s activity (Latour & Woolgar, 1986, 188). Away from this large, open-plan space there was also a kitchen area with a large table for carrying out meetings. Next to this were personal offices for senior members of the laboratory, less immersed in bench work. Swipe cards were required to access the laboratory, meaning I had to phone the secretary in order to be let in and which always reminded me of my status as a visitor.
Most of the people who came forward to be interviewed for this case study were enthusiastic about taking part (although one person was noted as appearing rather bored during the session itself). Each person asked me what I was studying after the interviews and appeared interested, if a little confused, about what I told them. The two post-doctoral researchers held longer conversations after the interviews had ended (approximately 5-10 minutes), about life in the laboratory, what they thought of their colleagues and how they enjoyed working in the area. Although I have deemed it inappropriate to draw on some of this data because of its possible sensitivity (e.g. complaints about work colleagues, conditions), having these informal exchanges did help to ‘paint a picture’ of the laboratory life and engender at least some degree of familiarity with this somewhat esoteric organisational unit. Other senior members I interviewed would inquire about my research, but also engage in small-talk conversations. These might include what university I was affiliated with, what discipline I was in, and, how data could possibly be generated from verbal exchanges taking place in interviews.

Retrospectively I believe I was unable to build the same levels of familiarity or closeness with the diabetes case as in the obstetrics case, or even the cancer case. Again this was surely due to the restrictions of access, with the fieldwork limited to interviews. Partly this feeling was also no doubt influenced by the fact that these had to be done over the phone, a medium which precludes face-to-face interaction. Unless one can access a parallel universe, it is difficult to determine whether carrying out phone interviews is to the ultimate detriment of the data that is generated. In some ways it was helpful, as I was able to follow the interview script more closely, without disrupting eye contact and other deferential gestures. Certainly all but one of the respondents were willing to give-up their time to answer further questions during the interviews (the information sheet stated they would last for approximately 40 minutes) and made an effort to give detailed answers. However this case perhaps did not elicit the range of local issues and subtle nuances in the same ways as observation fieldwork can. This was compensated for by the
more focused set of interview questions and deeper technical knowledge of translational research I had developed by this point. Indeed in the small-talk after one of the interviews, an informant even asked me if I was a doctor. Likewise, although this case perhaps did not permeate the deep spaces of this site, the interviews were very lively and did produce a lot of rich data about the challenges and boundaries these people face and the types of credibility and career investments they have made.

**The Role of Non-Participant Observer**

Although I gained rich access to the world of obstetrics over this period, I believe that the role of non-participant observer remained largely intact, at least in the context of meetings with the team, trial steering committee, and educational program. There are different possible explanations for the relative lack of attention or scrutiny I felt I received as a researcher entering these settings. Before the interviews and trial steering meetings I had provided participant information sheets which had given brief details of the study (see Appendix 4). Likewise I had given brief overviews of my research in the emails that the gatekeepers in the cancer and obesity cases had circulated to colleagues. Perhaps one of the reasons for the general lack of scrutiny was that they had read through this information and had their concerns/curiosities satisfied. Likewise they could have been satisfied that the study had received a favourable ethical opinion or that their participation would be unlikely to unearth any sensitive information.

Another factor which meant I could adopt the role of non-participant observer with relative ease was determined by the general lack of interest the informants displayed towards my research and the purpose of my visit. Most inquiries made about my research were usually in ‘small talk’ gaps after interview tapes had been switched-off and before we parted ways (when they had been given the ‘cue’ that the formal part of the interview had ended). A common theme in their inquiries would be how on earth the interviews could be converted into data, what stage I was at in my PhD studies, or how I could possibly derive any interest from observing, let alone listening
back to these meetings. Other sessions ended with small talk about the weather, or my journey to and from the location. As I was not asked for help on any issues I believe they did not consider me a member of the team: I believe I was treated more like an ‘accept able incompetent’ than ‘working patrolman’ (Van Maanen, 1988, 89). Perhaps the ability to act out the role of non-participant observer here was a mark of the continued existence of a ‘hierarchy of sciences’. My position as a social scientist was however never openly derided or ridiculed. Indeed on some occasions informants would even explain their willingness to participate in the studies by referring to common academic values, such as a willingness to help others in their research and to further science, thus stating an equivalence rather than difference. Perhaps though the general lack of interest I felt they displayed towards my research was summed up during the first meeting, when I was asked to ‘say a few words’ about the purpose of my visit and my intellectual background. I provided what the fieldnotes described as a ‘pithy’ off-the-cuff answer to this question (which I hadn’t been expecting). This was met with what seemed like general acceptance and polite indifference (judging by the fact that no one raised any questions). I was also to repeat this request during the later trial steering committee meetings I attended, where, similarly, there were no follow-up questions.

Overall, although the informants did not express a great deal of interest into the reasons for my being there, I realised that this perhaps also helped me to avoid resistances of the type that Van Maanen and other organisational ethnographers have encountered (where potential informants are mistrusting of the ethnographers’ intentions). I think we both recognised that as someone who is to all intents-and-purposes an outsider from their specialist community, it would have been ineffective and strange to ask me for advice over most matters arising in their work. Although this arrangement was rarely articulated, I think it was understood by both parties. In one meeting my presence did become more visible, when an informant turned to me and asked ‘any suggestions?’ This was met by laughter from the others, which I took as a sign that the very
notion of me contributing to the team’s actual work was absurd and worthy only of humour. As such my role as non-participant observer worked well for both parties. This ability to adopt the role of non-participant observer does not mean that I was completely invisible and that my presence had no effect on the way informants conducted themselves in the meetings. It would be disingenuous to report that I had no impact whatsoever on the course of the meetings and that the divide between my participating and not participating was always easy to distinguish. Likewise it would also be disingenuous to state that my being there never created any tension, as during first obstetrics team meeting one person complained to the Centre Lead that he had not foretold them about my presence. The latter apologised for having not informed other team members, and also apologised to me that this issue had arisen in the meeting. This apology appeared to diffuse the problem, much to my relief. I did not see this as a big problem, as they were objecting to the lack of communication rather than the idea of me being there to study them. But these utterances were rare and for the most part, as far as I could tell, the informants were content going about their ‘normal’ business whilst being observed. Overall, despite some initial nerves and some frustration at the amount of travelling required, the fieldwork experience was not particularly taxing on my emotions. Ultimately fieldwork was being carried-out in a comfortable, familiar type of setting (university) which risked little of the ‘occupational hazards’ and emotional traumas of the type Van Maanen described of doing research in organisations like the police force.

**Treating the Data**

The primary empirical materials for the case studies were sourced from interviews, observation and documents. Having taken informed consent, the interviews and observations benefitted from use of a digital voice recorder. This enabled me to record what was said with much greater accuracy than is available through relying purely on fieldnotes or, worse still, memory. Transcribing recordings will always provide the scribe with a great deal of control over how the
data in its raw form comes to be represented on the page. Although this is unavoidable there are some measures one can take to mitigate this issue. For the sake of consistency across the data I had decided beforehand to write transcripts in a verbatim style, which omits some of the minutiae that can be captured in approaches like conversation analysis, but still includes word-for-word what was said in the session (Bryman, 2012, 485). This is a common style used in ethnographic writings intended to persuade readers who were not present at the scene of the fieldwork of the authenticity of the author’s testimony. Quotations are thus deployed by qualitative researchers an ‘immutable mobile’ (Woolgar, 1988, 79) with which to support arguments in the text: providing mobile and accurate representations of what was said to them by respondents. The quotations are indicated through use of conventions like indented paragraphs and speech marks. Where the interviewee omits words, for grammatical clarity, I have inserted the missing words inside square brackets. Another convention has been to include three dots to signify where, for purposes of length and relevance, certain words or sentences uttered by interviewees have been omitted from the quotation (Bryman, 2012, 485). This rather linear style of reporting was deemed a sufficient level of detail and accuracy for the types of research questions being asked.

Once the transcription was completed, the interview and observation transcripts and other documents were exported into the Nvivo software package. The advantage of using this software will be made clear in the subsequent discussion of the approach used for coding the data. As discussed in the literature review and empirical chapters, existing STS materials have been utilised to accentuate certain topics and themes in the process of data analysis and writing. In terms of the data coding approach, I drew influence from Ritchie and Spencer’s ‘Framework’ for qualitative data analysis (Ritchie and Spencer, 1994). This identifies five stages:

1) Familiarisation

2) Identifying a thematic framework
3) Indexing

4) Charting

5) Mapping and interpretation (this being the stage at which key objectives of qualitative
analysis are addressed)

**Familiarisation** describes the early phases of listening, reading, and making brainstorm notes
about one’s data. As the name implies, the purpose is to become more familiar with the content
of the data. I tried to follow this advice by making fieldnotes immediately after each session,
recalling how I thought they went, what salient points emerged, and whether there were any
distinct surprises that came out. Such records provide ‘experiential data’ that is useful in coming
to write-up the research (Saunders et al., 2003, 227). Shortly after interview and observation
sessions I listened back to the tapes before transcribing them into word documents, all of which,
except for two interviews, I did myself. I subsequently read back over the two which were
outsourced to check for accuracy. Strauss and Corbin (1990) recommend first-time qualitative
researchers transcribe as a means of familiarising themselves with data. Although this did seem
to work, on a confessional note I should state that at times I found this to be tedious and
frustrating.

**Identifying a thematic framework** should occur before the indexing stage. At this stage a priori
issues guiding original questions and interview topics, notes made during the familiarisation
stage, emergent issues raised by respondents themselves, and recurrent themes or patterns of
particular views or experiences are all combined as inputs into the process of constructing a
thematic framework for indexing, sifting, and sorting material (1994, 180). The data was
examined and referenced against these framework(s). Ritchie and Spencer advise that it is
usually the case that one has at least an implicit idea of what this framework will be before it is
formalised at this second ‘stage’ (ibid, 179). Indeed having read around STS literature before
fieldwork had begun, a number of its key themes and motifs had already influenced the writing
of interview schedules. It was during the period of conducting pilot studies I first read Latour and Woolgar’s *Laboratory Life* and this undoubtedly had a big influence on the types of questions I was asking of respondents.

Once a framework was identified, it was applied to individual transcripts and documents. This *indexing* stage was still provisional: if applying the framework did not appear to work then I would have reconsidered. Yet I was persuaded of its utility when it became clear that it could be applied across large sections of the data. It also proved useful for building a common index across individual transcripts, thus enabling me to ‘identify common and divergent themes between populations’ (ibid, 180). For example, I was able to contrast with relative ease how different respondents answered similar questions, through using the ‘tree node’ in Nvivo. Indexing helps demonstrate that fieldwork and analysis were conducted systematically across each case, thus adding credibility to subsequent comparisons.

After indexing the transcripts with thematic framework, Ritchie and Spencer instruct that data be taken from its original context and rearranged into an appropriate theme. This *charting* process requires one to place distilled summaries of respondents’ views or experiences on charts. It is therefore described as a process of abstraction and synthesis (ibid, 184). Charts can be arranged by theme (each theme across all respondents) or by case (for each respondent across all themes) (ibid, 182-183). These charts enable one to ask the question ‘are there similarities/differences in theme within/between cases?’

This framework appears to pre-date advances in computer-assisted qualitative analysis, as this stage is actually done automatically if one uses the Nvivo package: node functions allow one to change interfaces from whole transcripts to node themes through a simple click of the mouse. Furthermore one can easily trace statements placed under theme back to their original source and produce multiple themes without ruining the original transcript. This was found to be one of Nvivo’s advantages.
For the final stage of mapping and interpretation:

“The analyst reviews and charts the research notes: compares and contrasts perceptions, accounts, or experiences; searches for patterns and connections and seeks explanations of these internally within the data.” (Ritchie and Spencer, 1994, 186)

It is this final stage which Ritchie and Spencer argue fulfils the functions and objectives of qualitative research, as it can encompasses: defining concepts, mapping range and nature of phenomena, creating typologies, finding associations, providing explanations, and/or developing strategies (ibid, 187). Each of these functions have been variously utilised in answering the questions of this research in the empirical chapters of this thesis, therefore providing me with confidence about the utility of this framework in providing a robust, systematic, and transparent tool for data analysis. I also found it to be user-friendly and easy to follow. Further credibility comes from the fact that the ‘Framework’ has been used many times before (ibid, 193), as a cursory glance at its citation numbers in Google Scholar concurred.

Checking Validity

The procedures to ensure validity will now be discussed further. Qualitative approaches to collecting and analysing data are often said to rely too greatly on individual intuition and judgment. Despite this view being naïve towards practices of knowledge production in all sciences (Latour, 2005), for any qualitative study the audience nonetheless are entitled to ask about the credibility and believability of the findings (Bryman, 2012, 389-390). Most criteria used for assessing validity of data have its roots in quantitative methodological traditions, and are perhaps ill-suited to qualitative studies. However, some of these criteria can be adapted to fit with qualitative research (ibid). One way of tightening the construct validity of case studies before and after the fieldwork is to draw suitably from existing theories (Gibbert et al., 2008, 1466, 1468). Latour and Woolgar’s (1986) framework was found to be helpful in this regards, as
it was itself formulated out of empirical findings of similar types of production processes and their sites. Reflexive researchers often question the ‘confirmability’ of their findings, i.e. whether their values have intruded too greatly on the findings (Bryman, 2012, 392-393). A limitation of interviews is that they can sometimes produce knee-jerk reactions and retrospective sensemaking (Eisenhardt & Graebner, 2007, 28); meaning findings can be accused of being skewed or cherry-picked. Constructing systematic measures for responses helps overcome charges of cherry picking data, persuading readers that a single or cluster of themes have been investigated and analysed rigorously. Again using Latour and Woolgar’s framework as sensitising tool in data analysis across individual transcripts for all three cases adds rigour to the findings. Clearly use of a rigorous coding system (like Ritchie & Spencer’s ‘Framework’) is also helpful in this regard. Interpretations of interview findings can also be sensitised through field notes recording ‘primary observations’ of what happened at the time (Saunders et al., 2003, 227), such as recording how respondents answered questions. One striking example was when people would respond in ways which, from where I was sitting, signalled ‘that was a stupid question’. Here I concluded that I had uncovered knowledge which was so taken-for-granted by respondents that it was ‘blindingly obvious’.

How findings are presented through writing is usually convergent with the type of paradigm in which the study is located. Different paradigms traditionally have their own genres of reporting that usually carry connotations of validity (for a review of validity measures across traditions see Creswell & Miller, 2000). Writing findings in ‘depth’ is the lens that has predominantly found historical favour amongst constructionist audiences, where ‘depth’ is a value-laden concept carrying post-modernist implications (Lincoln and Guba, 2000). In particular it implies using types of narrative genres different to the passive voice usually found in ‘scientific’ texts (see Gilbert and Mulkay, 1984). Van Maanen’s (1988) ‘storying’ encompasses styles refusing to project passive, disinterested voices. For instance, using his style of ‘illustrative tales’ to depict particular
aspects of the cases is consistent with the case study strategy and paradigm-position taken in this study, as well as arguably being more attention-grabbing. Descriptive writing styles synonymous with the ethnographic tradition are most appropriate for capturing complex phenomenon of the type in which this thesis is engaging (Latour, 2005).

In sum, claims of validity are usually presented to the reader based on reassurances that certain standards and criteria have been followed. Being transparent about the research design and fieldwork are considered important features which increase rather than reduce validity of the findings, reassuring the audience that the researcher is conscious of the strengths and limitations of the study.
6. Empirical Analysis: Transforming Cell Signalling?

Introduction

This chapter looks to consider how the idea of translational research effects the mundane work organisation of a laboratory working on processes of molecular cell signalling in a cancer institute belonging to a university medical school. The case study draws on the insights provided by a group of five respondents working as full-time members of the laboratory. They consisted of De Jong- a professor and principal investigator of the laboratory, Mendez- a senior lecturer in charge of the ‘dry section’ of the laboratory, Enfield- in her first year as a post-doctoral researcher having also completed a PhD in the same laboratory, Madeira- who was also in her first year at the laboratory having joined as a post-doctoral researcher, and Morales- a technician. The study also benefitted from gaining access to interview the Head of the Institute and a local engagement officer representing a major research charity within the Institute. This helped to provide information on the immediate governance and organisational context of the institute and university in which the laboratory was hosted.

As with each case study, data was collected transcribed into Nvivo and coded, accentuating especially Latour and Woolgar’s (1986) four frameworks- history of the laboratory, inscriptions, microprocesses, and cycles of credit- as sensitising concepts. As with the structure of the other empirical chapters, the findings will be ordered under these themes. Each section will elucidate both empirical findings and where appropriate link these with broader arguments and concepts in the theoretical literature. The chapter concludes with a brief discussion as to what aspects of
the idea of translational research have travelled and the extent to which it engenders innovative modes of organising within the context of this case.

**History of the Laboratory**

This section provides a general background into the case, by mapping-out how, when related to specific objects, translational research is made visible in relation to the networks on which the laboratory is subsistent.

Translational research was defined by the Head in view of the networks of relations assembled under the formal guise of the Institute. This brought into view the Institute’s strategy as being formulated around enabling translational collaborations of various sorts to occur between constituent elements like research laboratories, research charities and local NHS Trusts. This was framed as a response to wider signals and disciplinary controls being projected by external ‘macro-actors’ on whom they are dependent as a research-led organisation. Definitions given to translational research by the School’s management figures were made to act as ‘immutable mobiles’ sent back towards other ‘centres-of-calculation’ to whom they were variously dependent and accountable:

“I think at the moment we’re benefitting from the fact that government agencies in particular are interested in the idea of impact... That may not always be the case: we may go back to a culture where blue skies thinking and curiosity-driven research is afforded a greater priority and there is less of this obsession about, you know, driving things to the later stages of the translational pipeline. But for the moment, the searchlight is shining on the bit that we do best.” (Head)

Immediately then it became clear that translational research, as a governance device, was having some influence on the actions of the Institute. From the Head’s interview account the actor-network of the institute consisted of formal allies—research laboratories, administrators,
local NHS trust, cancer charity – all brought together under the label translational research. Translational research could thus be seen as a boundary object for framing their mutual interests. Each ally of this actor-network appeared to have their own expectations and interests in associating with the Institute. For example the charity was primarily concerned the goal of improving cancer patients’ clinical outcomes:

“I suppose in five years’ time what we’d want to see is that some of the local engagement will have translated into cancer patient benefits and patient outcomes, so people will be diagnosed earlier with cancer and people will have better outcomes from the disease, they will live longer. They will also have better understanding of how to prevent getting the disease in the first place.” (Local Engagement Officer)

The concern of the Head was not only to bring about clinical pay-offs, but to meet various standards expected of research-led organisations. In large part this meant ensuring researchers met the standards of ‘excellence’ set out under the REF. In addition each of the institute’s quasi-autonomous research groups were positioned within this actor-network and made to account for ‘impact’ of their work. Strategically this was defined in terms of the stages of the translational pipeline along which their work could (convincingly) be aligned:

“The way that the institute is designed is that we have the sort of infrastructure- those types of people with the right skills sets- to enable anybody to be able to plug into it and get the right levels of support, whatever stage they are in the translational pipeline.” (Head)

In view of the faculty’s portfolio each research centre was allocated a particular role in relation to their translational vision, framed in terms of their respective expertise in basic, pre-clinical and/or clinical research. More clinically-oriented researchers for instance were expected to develop a translational component to their work in view of an administrative centre headed-up
by the Head himself, charged with providing the governance necessary to scale-up findings into clinical trials that tested for novel diagnostics and therapeutics. The basic researchers were expected to demonstrate an ‘impact’ more through translating research into commercial opportunities such as patents, licensing and spin-out companies via the university’s technology transfer office. The laboratory at the centre of this case study was positioned as fulfilling those more basic elements of this pipeline metaphor. The Head recognised the laboratory as a legitimate player in this regard:

“You’ve got to have something to start. You’ve got to have an A to get to B. If all the work is just focused on the B, i.e. the patient, you won’t have anything to translate. It’s important to have a balance, but I think it’s important to cross-pollinate between people at both ends of the pipe, to educate, to have doctors and clinicians who are familiar with the language of science and understand at least the principles of what the fundamental research is about. But you’ve also have got to motivate, by exposing basic scientists to the big clinical questions. And out of that cross-pollination experience has shown that you get interesting new ideas which have taken both elements to make a practical reality.” (Head)

Translational research is thus transmitted as a label with internal marketing purposes, where the Head, as spokesman for the entities, looks to ‘nudge’ from-a-distance scientists like those in this case, to adjust their behaviour and ‘mentality’. But how do respondents in my case define translational research in light of the mundane activities and practices of their laboratory? This will now be elaborated through further discussion within which the idea can be located. As has been intimated, the laboratory is expected to meet certain criteria imposed upon them by the senior management figures in their host Institute, who are in turn responding to demands of others. Interviews with the Head and local engagement officer helped elaborate a general definition of translational research inscribed on the laboratory through the spokesperson of the
Institute. However, interviews with laboratory members brought into view more specific networks of relations through which translational research is defined (and transformed) at their local organisational level.

All of the definitions given over to translational research were intertwined with the laboratory’s central focus on a core family of proteins: PI3 Kinases (PI3K). As a basic research laboratory, the research agenda was focused around exploration of various facets of this phenomenon. This meant their research ‘started’ at the very basic cellular level, through analysis of cell signalling processes in the ‘dry section’ of the laboratory. With the help of technology transfer office Madeira and De Jong had co-founded a spin-out company which had begun to scale-up these techniques into novel inscription devices marketed towards commercial and academic laboratories. This company had then been sold on to big pharma, owing to the commercial promise of the developing technology in helping to screen for novel targets in pathways to disease. Despite relinquishing control, both were kept on as consultants by the company for their expertise about the science behind the technology. Thus as well as bringing money into the university and laboratory, the venture also brought about steady stream of income to be reinvested in the laboratory. This highlights two definitions of translational research circulated towards the Institute’s management. In one sense they were fulfilling commercial arm of the Institute’s translational ‘portfolio’ by creating profit for the university in financial terms. They also could claim to be delivering on ‘impact’ motifs by improving the technologies at the centre of search processes used in drug development, which might ultimately produce diagnostics and therapies which get brought to market, thereby constituting their work as a general ‘public good’.

As well as concentrating on these fundamental processes of cell behaviour, another work section of the laboratory focused on ‘wet’ research which scaled-up cellular findings into work on whole organisms. Mouse work was performed as a means of modelling diseases that affect humans.
This involved breeding mutant mice with particular diseases (cancer, arthritis) and conducting experiments in relation to possible therapeutic treatments. Accordingly if results show the efficacy of giving a specific small-molecule inhibitor to diseased mice then it may be taken-up by others elsewhere in the translational pipeline ('externalities'). Ideal typically this would mean seeing their findings taken-up by others through various phases of process-driven work at pre-clinical proof-of-principle stages, before entering human trials and stages of regulatory affairs, then eventually entering the market. As such, the work done in their laboratory is presented as an important \textit{initial stage} in the translational pipeline through which principles are proposed for others to test and exploit. The association with the ‘translation’ metaphor given over to their work on disease processes is identified by respondents as a key boundary object around which their collaborations with those beyond their specialty are forged. Publishing in esteemed scientific journals like Nature or Science was deemed by respondents as bringing attention and ‘presence’ (Latour, 1987) to these findings amongst potential suitors, especially pharmaceutical companies. Mutual interest in an aspect of disease is thus what brings them together. Such a definition enabled them to balance their primary interest in producing research of ‘scientific excellence’ alongside work which met ‘impact’ or ‘relevance’ criteria.

This broad ‘coming-togetherness’ definition also enabled respondents to frame some of the work they do which feeds-back from the clinic into the laboratory as ‘translational’. Hence as well as flowing in the direction of ‘bench-to-bedside’, their work can also focus on problems which are ‘bedside-to-bench’. This is especially apparent in some of the paid consultancy work De Jong did for big pharma, in which his expert knowledge about the role of molecules in cells means his voice is listened to in regards to what happens when drugs based on said molecules are given to patients. Likewise in partnership with another big pharma, the laboratory had gained a large MRC grant to test for haematological malignancies in patient samples, thus indicating flow of research materials from hospitals back to their academic laboratory.
Translational research is thus a means of labelling and communicating episodes of working with others out of mutual interest in disease. This was also a definition which the Head of the Institute accepted as legitimate and had integrated into the portfolio of the Institute.

One of the most notable themes coded in these interviews was the presence of ‘boundary work’ in demarcating the limits of their involvement in the translational process. In interview accounts respondents constructed a moral order which sought to carve out an enclave for their involvement in this type of endeavour whilst simultaneously justifying their locating of these boundaries. In particular although supporting the movement of their findings into clinical studies, they strongly resisted the notion that they themselves should become more strongly involved in clinical research, wishing instead to maintain their position as a basic research laboratory:

“I think translational work is a dangerous game because you can work with doctors, clinical fellows, but, you know, you still have to maintain your identity as a basic researcher. And I think what I prefer to do is work with clinical fellows or clinicians and so on, then they take the work, it is theirs, so you don’t have to try and become a clinician. I cannot become a clinician, I have no aspiration to. I’m in the wrong business.”

(De Jong)

Therefore, as well as being made accountable for their actions by others (e.g. senior management), the respondents are looking to delegate certain tasks to others in this extended (and fragmented) production process. Time and again the spatial and temporal distance of their findings from actual clinical application prompted respondents to bring into view network ‘externalities’ responsible for picking-up and developing their ‘grass roots’ work.
Inscriptions

Having mapped-out several ways translational research is made to order objects that are made visible within networks, I will now discuss the results which arose from coding data using Latour and Woolgar’s *Inscription* framework. Through intermittent visits to carry-out interviews, I was able to observe the lay-out of the space which constituted ‘the laboratory’. The actions I observed as particular to this space appeared to correspond with accounts of mundane activities and practices of scientists as centred on writing (in an extended sense) (Latour and Woolgar, 1986). Through the lens of laboratory studies, the space of the laboratory appeared like a production line (Knorr-Cetina, 1981, Latour and Woolgar, 1986) for assembling various forms of writing, from the work benches in which ‘results’ are read off inscription devices, to the library where books, journals and SOP guides were read and annotated, to the office spaces containing personal computers. To the outside observer, writing thus appears central to *organising* in the laboratory.

As discussed previously, Latour and Woolgar’s study of acts of inscription was greatly facilitated by rich access to their respective laboratory over an extended period of time. The restrictions placed on access to the spaces in which these actions took place necessitated some degree of pragmatism in considering this framework within the context of this case. It has resulted in a choice to discriminate interviews and publicly available documentation as key resources with which to make sense of this aspect of their activity. This means focussing on forms of persuasion enacted within important textual outputs by the laboratory, namely grant applications and journal articles, with participants’ interview accounts used to support inferences. As in the previous chapter, the aim is to consider the extent to which the idea of translational research appears to inform writing practices and whether this pressure requires a significant departure from writing practices described in earlier STS studies.
Grant Applications

From the interviews it was revealed that grant writing was the domain of senior members of the laboratory, who have acquired the know-how to acquit themselves successfully in practising this action. However, for less senior members of the laboratory learning to write grants is not something about which they yet had knowledge. De Jong and Mendez occupied managerial positions, whose roles entailed articulating alignment with the social world (Fujimura, 1987), including responding to the pressure to demonstrate ‘translatability’. For this reason the written outputs sampled for study here were authored or co-authored by these two respondents.

Using the Institute’s webpages and interview accounts I was able to identify those funding bodies from whom the two respondents had been successful in securing support since re-locating to the Institute. Grant applications are made publicly available on the websites of various funding bodies. I was therefore able to search the names of the PI and S/L in the funder’s websites. This yielded results from the BBSRC (n=3) and a research charity with which the institution had strong ties (n=2). Applications made to the MRC were not found. The texts were exported into Nvivo and coded according to Latour and Woolgar’s five statement types.

Findings support the argument that scientific writing is a form of practice (Latour, 1988, 218), as grant applications appeared as a type of game in which participants encode information in particular ways with a view to persuading their audiences to act in certain ways (namely agreeing to fund proposed research). Indeed De Jong reflexively acknowledged that he and others would routinely adjust aspects of the information content in grant applications to meet the expectations of a particular audience. For example, the Biotechnology and Biological Sciences Research Council (BBSRC) were thought to be less interested in contributions to disease studies and more in contributions to basic science problems and possibilities these might hold for commercial exploitation. Research charities on the other hand were very much interested in possible patient benefits (again ‘translational’ being identified with an interest in ‘disease’ within
the context of their work). ‘Forward looking statements’ were a prominent feature of the respondents’ grant applications, albeit the content of promises would differ according to the grant body to whom they were applying:

“I will put some forward-looking statements in there, that it helps the economy, that sort of stuff, you know. I’m not saying that I’m going to cure a disease, but I am saying that ‘look, based on this people might actually start to set-up a company’. But I’m not proposing to do it, you know, there is a different way of putting it.” (De Jong)

Inclusion of promissory statements was thus reported as an effort to second-guess what the grant body to whom they were applying would be interested in funding. Within several respondents’ interviews this aspect of their practice was framed as a ‘game’, with the participants proposing that despite inclusion of such statements being essential, it would be unlikely that they would be held to future account should these promises fail to accrue. Likewise there was a general attitude that these statements were not as decisive in decision-makers’ evaluation of these texts as those statements pertaining to technical problems to be addressed by the research. Speculative, performative statements have always been important functions of grant applications, as by definition gatekeepers are being asked to invest in the potential of proposed research. But the substance of these statements appears to differ according to the anticipated interests and expectations of the respective audience. Grant applications submitted to bodies with a greater interest in medical research were identified more explicitly with ‘translational research’ agendas by respondents than with the BBSRC applications. The former engages with disease processes (as opposed to fundamental cellular processes, as in BBSRC funded research). However, even BBSRC grants (thought to be a ‘basic science’ champion) include some ‘impact’ statements with regards the potential social utility of research. As the PI stated, he will include these ‘translational’ statements on more of an ad hoc basis, typically
towards the end of the content of BBSRC applications (see below italics). This statement was supported by analysis of grant writings:

“This is a fundamental science project that will enhance our knowledge about basic biological phenomena. In the past, the biology of PI3Ks has impacted on science far beyond our own field, mainly because these enzymes control fundamentals of biology. *This proposal also has the potential to benefit industry, as it might identify PI3Ks as new targets to develop medicines against.* In the longer term, it is very likely that this research may lead to a better understanding of disease processes and *to the development of new medicines.*” (BBSRC 2, emphasis added)

The difference between promissory statements referring to ‘scientific’ outcomes and ‘practical’ outcomes in this excerpt appears to be that the former would result from uptake of new facts within the scientific community, whereas the latter would require other externalities to come into play (e.g. pharmaceutical companies). Whereas one suspects promises about basic contribution have always been part of writing practices in many scientific fields persuading gatekeepers of the social utility of research appears to be a more recent addition within fields whose sponsors and gatekeepers engage in missions of strategic/translational science (van Lente and Rip, 1998b). Even though they are identified as a ‘basic science’ laboratory applying for money from a ‘basic science’ research council (BBSRC), there is an identified requirement that they include statements predicting consequences beyond the immediate context of their discoveries.

The interests of medical research charities in practical outcomes from research they fund means that research proposals must be centred on the disease(s) about which they are concerned. In applications to the research charity there was explicit systematic mention of specific types of diseases to which the research contributes, such as cancer, as opposed to general contributions to knowledge about cells work in relation to an open-ended number of disease processes.
Typically, the interest in a specific disease like cancer is not simply ‘bolted-on’ at the end of the text in an ad hoc manner, but is the central problem being studied and is therefore much more prominent across the statements comprising the text. As with the BBSRC applications, the research is also framed as ‘translational’ by way of making more specific promises (other than contributing to general stocks of scientific knowledge). For instance they make claims about how results might constitute the basis for later cancer therapy developments. This can be read from the following statements rich in grammatical modalities (italicised):

“This work will determine the contribution of the PIK3CA oncogene to energy metabolism in cells with similar genetic background, and may result in the discovery of new drug targets and/or biomarkers for oncogenic PI3K signalling... (Research Charity, emphasis added)

Thus we aim to systematically investigate the contribution of oncogenic signalling (with initial emphasis on the PIK3CA oncogene) to the deregulation of bioenergetic processes. In addition to increase our understanding of basic cancer biology, these studies may also have repercussions for the design of therapies that target cell signalling or bioenergetic pathways [in cancer].” (Ibid. emphasis added)

That respondents’ pointed-out these particular aspects of their writing practices to me when discussing ‘translational research’ in interviews supports the argument that this term is used to label, simplify, communicate, order and package more complex processes. As a label it is both immutable and mobile as it feeds-back between centres of calculation (e.g. research councils) and peripheries in the research system (e.g. laboratory scientists), helping to frame equivalence in these particular exchange processes
Journal Articles

Scholarly articles authored and co-authored by De Jong and Mendez were subjected to one time ‘advanced search’ on Pubmed online database. Those papers to which my institution granted access were selected, downloaded and exported into Nvivo. These encompassed ‘research’ types of article published in scholarly journals (n=14). Again each article was coded using the same five statement types. The results of this analysis will now be discussed further.

Of the fourteen articles coded for analysis, six were coded under Type I-IV statements only, with the other eight articles containing both representational and performative statements. Two of the six ‘representational’ articles had been published in a specialist journal, two more in another specialist journal, and one each in Nature and Science. These texts were replete with arguments saturated in technical discourse. This can be characterised by the low count of grammatical modalities, with statements instead being presented in such a way as to make various ‘facts’ speak for themselves, with reference to human agency excluded from statements. This ‘technical’ rhetoric can be juxtaposed with other titles in which more ‘pathos’-driven rhetoric was accentuated. The (hopeful) basis of projections about the practical pay-off of the work in the latter titles appears to be based on assumptions about the scientific validity of the research:

“Such an approach could be applied...to the pharmacodynamic validation of new drugs by enabling an accurate assessment of target inhibition in primary tissues. This technique has the potential to significantly contribute to making the concept of personalised cancer therapy into a clinical reality.” (Anon)

Notable within certain articles was the presence of statements containing modalities in the form of words like potential and future, which would undermine representational statements For instance:
“These results define PI3K as a potential therapeutic target in inflammatory disease.”  
(Anon)

Part of the expectation in these titles then appears to be that the persuasiveness of a text will be assessed not only in terms of contribution to an important intellectual puzzle in their given specialty or field, but also in terms of possible practical utility to be accrued from the results, for instance in terms of commercial or clinical developments. Notably, part of the literary strategy of performative statements is that possibilities are included (such as therapeutic targeting), but not known-barriers to such possibilities (Knorr-Cetina, 1981). The statements can therefore be understood as an additional type of attempt at ‘black-boxing’ (Latour and Woolgar, 1986). As with De Jong’s remark about grant writing, the promises in these texts are carefully articulated so as not to render the authors themselves accountable for driving forward subsequent translation. Part of the rhetorical strategy found amongst these performative statements was to silence agency of the researchers who have produced and championed these inscriptions from their laboratories and instead impute agency into the new facts themselves. This technique could be found in the above quotation and was repeated in another article:

“This technique has the potential to significantly contribute to making the concept of personalised cancer therapy into a clinical reality.” (Anon)

These performative statements are written in such a way as to suggest it is the new fact itself which, acting alone ‘have potential’ to be the driving-force of subsequent translations. Alternative ways of framing promising results, such as ‘we think it has potential’ or ‘in my opinion’ appear less solid because they would reveal it is the authors themselves who have attached this meaning onto the object for persuasive purposes. This follows a realist discourse in which objects (which represent nature) provide a firmer foundation for claims about the world
than meanings and opinions imputed by humans. This is a literary strategy of the text used to ‘talk-up’ the product, which is hyperbolic rather than misrepresentative (Knorr-Cetina, 1981, 101). Explaining the institutionalisation of this translational discourse within the function of the texts published by these titles is beyond the scope of this study. However, what is notable is that such requirements would appear to affect the need for respondents’ to extend their discursive repertoires, appealing not just towards audiences located in their own technical world, but engaging also with broader discourse of strategic (or translational) science. Without learning and mobilising these skills, they would be unable to bring in grant money and publish articles in certain titles, and therefore continue/advance as practitioners in their community. Hence struggles for facticity and fundability are met with wider struggles in the institutions of their fields (journals and grant applications) for relevance and legitimacy (Rip, 1994).

The need to include (clinical/commercial) translational statements might constitute a dramatic departure from writing practices depicted in earlier laboratory studies. But from the PI and S/L’s account, it became noticeable that the need to include such statements is not presented as a problematic shift in writing practices, but as something which has already become quite ordinary and routine for them. This particular component of writing practice thus does not constitute a significant boundary to their strategic engagement with ‘translational’ work: they consider it ‘doable’ (Fujimura, 1987). Empirical findings regarding the routinisation of these extended inscription practices appear to have been under-appreciated in STS. Theoretically this finding can be accommodated by existing theories, which postulate that scientists will pragmatically ‘speak to’ a range of audiences in the course of their practices: this just happens to be another interest for them to accommodate (Mulkay, 1976).
Microprocessing

Translational research requires subsistence on types of networks other than those covered in earlier laboratory studies literature. In these emerging contexts there appears a heightened focus on being able to communicate effectively amongst allies outside the parameters of the laboratory’s local expertise and focus. Data on microprocesses which occur in conversation during such interactions was not available to me. As such, synthesis of Latour and Woolgar’s original framework with empirical findings has been restricted. There were openings in the interview data which are suggestive of the local negotiated micro-processes which help constitute fact-making processes in this laboratory setting and therefore suggest the general utility of Latour and Woolgar’s framework. For instance ‘shop-floor’ talk is cited as a common feature of the processes of setting-up and running experiments:

“I: Can you talk to colleagues about where you might have gone wrong?

Enfield: Yes. It’s very common. I mean everyone tends to have their own project but it’s very much like... you troubleshoot with your friends, because that’s the only way to understand.”

It also became apparent that the members of the laboratory held regular meetings which were cited as important in terms of negotiating and articulating project work. Projects which were conceived both as ‘doable’ and likely to lead to publication were said to be the ideal type against which articulation processes were framed by laboratory management. However, despite these openings, the lack of access to observe such occasions in situ hamstrings any attempt at providing a rich description of this framework in relation to collaborative work in action. Instead then this section considers how in their interview accounts respondents describe articulation work in and around the laboratory, particularly with reference to projects they see as having translational ‘ingredients’ (i.e. those with strategic alignment to commercial/clinical interests in
the social world). This will mean highlighting from interviews those boundaries which facilitate and disrupt exchanges, and enable decisions within such assemblages.

As shown in the History of the Laboratory section, translational research signifies bringing together elements of an actor-network and that which enables them to communicate. In this sense it meets the formal definition of boundary objects. But this mutual interest is insufficient on its own to sustain collaboration throughout - other boundary objects come into play, some of which are more physical. For instance, mass-spectrometry instrumentation developed in the laboratory was scaled-up into commercially applicable technologies. Here representations of the technologies’ potential utility in commercial laboratories facilitated bringing members of the laboratory into contact with commercial actors, via a spin-out company and technology transfer office. After this coming together had occurred and partnership had been forged, there appeared to be much ‘repair work’ occurring in regular negotiations between the laboratory and commercial clients. Sustained periods of regular communication were depicted as central to the process of scaling-up their discoveries into a commercial context (by way of developing a spin-out company). As the technician reports, this required regular formal and informal meetings with those using the technologies, in order to ‘iron-out’ problems. In addition to these ‘business’ meetings, one of the means by which collaborations and translations are established was through ‘networking’ and presenting at conferences:

“I think what is satisfying is that I keep on being vocal about how to use it at big conferences and things like that, and some small companies have taken it forward and made it very successful, meaning some of the big boys have started to say ‘oh actually we also need to do that’ and they go back and do it.” (De Jong)

In these communication contexts face-to-face meetings are identified as crucial. Most respondents recognised face-to-face meetings as an important ingredient in articulating
translational projects, particularly as these were thought to carry greater risk to stakeholders.

This is captured in the interview account of the institute Head:

“So, for example, even today most productive meetings happen in person. Until a face-to-face meeting has occurred, then you know the possibilities of Skype, social networking, or whatever else electronically, just doesn’t work. We are talking about large investments here, not only in a financial sense, but also in a personal sense, in terms of time and reputation. It really depends on people trusting each other and understanding each other’s motives. It takes a lot of face-to-face meetings— but we do a lot electronically too.” (Head)

Trust comes into play as an important resource in these projects, which is best established by way of face-to-face meetings, supported via intermittent electronic communications by telephone and new media. Another aspect of making translational work doable is having a network in place so as to facilitate collaboration as and when it is deemed appropriate. This means having formal and informal ties with local clinicians, drug companies and in-house departments like technology transfer offices. Discourse on networks has often presented the idea that they enable the reallocation of action. In this regard De Jong and Mendez describes how scaling-up discoveries does not require him to possess significant expertise on clinical problems, as he is able to delegate this responsibility to clinicians within his networks (in this instance his informal ones):

“I remember during my career development I went to New York and they said to me what do you know about clinical trials? I said nothing but I have a good friend and that’s what he does for a living. I meet him every Sunday, so when the time comes I’ll find out.” (De Jong)
This technoscientific aspect of their networks is facilitated by recourse to commonly agreed upon techniques and standards. Mendez describes how having ‘productive methodologies’ in place which are commonly accepted by scientists and clinicians alike that facilitates interaction:

“Well, we all know that certain pathways are important for cancer: clinicians and scientist know this. We understand the genetics the same.” (Mendez)

Talking to clinicians and reading medical literature is one means by which the general processes of translational research is made possible for basic scientists. In terms of scaling-down clinical phenomenon into their laboratory work, this communication enables the scientists to focus their experiments on problems that are in the clinic- such as breast cancer treatments – before carrying-out cell-based studies of them. But despite ‘common ground’ there is also a negotiated division of labour within translational work along lines of local expertise:

“Clinicians know the pathology much better; and different types of breast cancers and different types leukaemia’s and the genetics of leukaemia, and how to classify them. And I know more about the physics and mathematics, the technologies that we use to do understand some biology and biochemistry.” (Mendez)

In sum, despite the lack of data on naturally occurring talk making it difficult to scrutinise Latour and Woolgar’s microprocessing framework, concepts like actor-network, boundary object and boundary work were useful in opening-up the communication processes occurring inside multi-disciplinary spaces of interaction comparable to those observed in case studies. De Jong and Mendez were careful to draw the boundaries of their involvement and defend this articulation during their interviews. Thus although the immutability and mobility of the name remains intact, much of the actual collective actions to which this term gives a name are marked by uncertainty. The concern for constructing boundaries of collaboration and expertise could be read as an attempt to preserve their ‘identity’ and strategic position in forms of work to which they had
become accustomed and had achieved some success. Exploration beyond the boundaries of their existing expertise was portrayed as strewn with risk, and led the Institute Head to describe translational research as ‘hard and dirty work’. As is discussed further in the next section, De Jong’s reported propensity to pursue such risks was framed within a repertoire of enterprise. It is such risk calculation which appears to constitute a significant boundary to more whole-scale adoption of translational research as the strategic position for the laboratory.

*Cycles of Credit*

Applying the *cycles of credit* framework generated an enormous quantity of coded data within the transcripts of this particular case. This section will provide an overview of these findings, starting with the sub-topic of group structure before focusing on the construction of individual careers of scientists in relation to the idea of translational research.

*Group Structure*

De Jong identified that shifting from ‘basic’ towards ‘translational’ types of problem choices would necessitate significant transformations in human and non-human elements constituting the laboratory. For instance, at present parts of the laboratory’s workforce, like technicians, are employed in order to process experiments focusing on basic cellular processes. One of the implications of this re-configuring could well be ‘downsizing’ the ‘human resources’ elements like technicians. In their accounts De Jong and Mendez evoked a sense of familiarity with their employees, owing to the relatively small size of the laboratory and the feelings of ‘co-presence’ this engendered. As De Jong states, one of the occurrences which ‘keeps me awake at night’ is running out of money to pay his employees:

“You see they come here from all over the place, they put trust in me, and then, you know, if I say ‘you can’t do your work anymore because I have run out of money’ then it is not fair on them because they have come from France or Italy or Greece or wherever. I
have a responsibility to them...I mean within reason. But for me to run out of money because I spent it too quickly…”

One measure by which he seeks to temper this risk is by not expanding the size of the laboratory to an extent that actor-networks begin to unravel:

“I think it can grow a bit too fast. And I talk with friends of mine who’ve got a big group of people and I say ‘do you enjoy your work’ and they say ‘no’. I’m in control. I’m in control. Do you think [the Head] is in control? His agenda is set by all sorts of organisations…” (De Jong)

Moving to occupy a more explicit ‘translational’ position over an area of scientific field risked coming at a heavy price for De Jong, namely loss of his managerial control, coupled with subsequent loss of reputation and betrayal of interpersonal relations.

Concerns those respondents other than De Jong and Mendez had towards problems posed by translational research can be read in relation to the types of roles they were accustomed to playing within the organisation of the laboratory. For example, the technician’s role does not infringe upon layers of organising concerned with aligning the laboratory to the social world. Here then the hierarchical dynamics of the laboratory become clear, as the technician for the most part is expected to obey and accommodate rather than pro-actively (re)write this sort of script. The notion of translational research is therefore kept largely ‘black-boxed’ by the technician, who can imagine only very indirect ways in which this idea permeates his mundane activities and practices:

“...I: Does that motivate you...

Tech: ...Yes if I think about my role and how it may influence on a more general sense, then yes I am aware of it. But on a daily basis I’m more aware of the routine aspects of
my job. But as you ask me these questions and I think about my role, you really start to realise about what I do and the real impact it has." (Morales)

Within the routine aspects of laboratory organising, Morales recalled being exposed to exchanges about translational research mostly within team meetings:

“I: Is it something that would be said in meetings in any shape or form?

Tech: Yes, we have lab meetings all the time and this is something that gets discussed a lot. ‘What are the potential impacts of the work that we do? And will it get developed into something that will help people?’ So it does get discussed, but I really see what we do as grass roots - we’re actually a long way from getting it produced and onto the market.”

Within the accounts of the post-doctoral researchers, it is similarly on these occasions that the organisation’s concerns for translational research are brought explicitly into view. In these meetings they will be asked to report on the progress of their projects and suggest new directions they were considering. The meetings were said to provide an opportunity for members to ‘troubleshoot’ amongst colleagues. For the post-doctoral respondents, much of this advice will come from more senior members of the laboratory. Interestingly, both respondents described seniority as relating to degrees of knowledge members had acquired rather than simply a function of their formal roles within the employment organisation. In other words seniority is determined to some degree by credibility, not just formal rank. Yet the member of the group to whom they reported showing greatest deference in these occasions was the PI. It is he after all who is seen as possessing the know-how about aligning experimental and social world levels of organisation. In this way the post-doctoral researchers see themselves as being
dependent on the PI to help them further their own capital accumulation as scientists. Such practices appear to provide strong intimations about a power-knowledge nexus enacted within the microprocesses of the laboratory (Foucault, 1978, in Latour & Woolgar, 1986, 229). From the various interview responses it can be inferred that on such occasions the post-doctoral researchers and others have been asked by De Jong to account for how they intend to articulate an alignment between their proposed experiments and translational agendas.

“You have the ability to take your project in whatever direction you want for quite a while without him interfering. So he isn’t a micro-manager, he sort of isn’t necessarily leading. It’s more like a conversation. He might be holding the more strategic part of the conversation but the conversation is not necessarily him saying ‘you do this’, or ‘you have to make the project this way’. He might just give you these ideas.” (Madeira)

Typically for post-doctoral researchers this ‘type of conversation’ appeared to occur in later stages of project articulation than for more experienced researchers, who are expected to have this strategic know-how incorporated more readily into their work practices. For the post-doctoral researchers then, the problem of translational research was an intermittent one occurring at various points in the life-span of a (4-5year) project. As well as filling the portfolio of the laboratory with ‘translational’ projects, part of the rationalisation De Jong gave of this management tactic was to afford early career researchers an opportunity to build-up their own credentials and know-how with regards articulating project work in a way which was responsive to this ‘growth area’. As well as giving the so-called next generation of scientists a ‘flavour’ for translational work, the PI described how he would - where appropriate- permit them to take ownership of this work and pursue it further in their subsequent careers beyond this particular laboratory. Here then is one way in which the idea of translational research is said to be
translated into existing practices of the laboratory, namely by informing and justifying the hiring and training of staff.

As implied by earlier discussions, the PI is a key linking-pin of the actor-networks of the laboratory through which translational research is made visible. There are aspects of the laboratory’s work which fall on his shoulders. For instance, it is he who consults with drug companies, interacts with clinicians, and accounts for the laboratory’s academic performance. One aspect not yet covered by these earlier discussions is the struggles the PI has faced in being made to account for aligning his laboratory’s research output with the formal standards of the REF. When discussing publication targets this alignment was black-boxed by respondents, as each displayed confidence that the regularity with which they would publish in top journals like Nature and Science acquitted them well for this process. But one aspect of the process with which De Jong had encountered some difficulty was accounting for their output in terms of ‘impact’. An ongoing debate at the time concerned which particular examples of work done by the laboratory could fulfil the ‘impact’ criteria of the REF. It emerged that what the PI had thought worthy of impact and what was deemed satisfactory by the Head of the Institute were two different matters. For one, work which had eventually been scaled-up into commercial developments and clinical trials, had been accomplished elsewhere, before his lab had moved to the current institution and was thus disqualified from the criteria. Here then was one aspect of the REF to which he displayed criticism:

“So I think I would find that silly because everybody knows it takes ten years. Everybody knows that to get it to work you have to move places. So I think it is a bit short-sighted in a way to look at it like this. I don’t want to take all the credit for this clinical work, but it’s something I have at least facilitated.” (De Jong)
In contrast then to publishing basic research in top journals in order to meet ‘excellence’ and acquire (re-investable) credit, clarity of the means by which they could be credited for meeting ‘impact’ criteria was more opaque. Another problem in aligning their work to this agenda was that the criteria does not formally acknowledge commercial exploitation of research, one of the ‘trump cards’ the laboratory’s ‘translational research’ activities he felt entitled to play. Finally, the publication of ‘Review’ articles in prestigious scientific journals- commenting on existing state-of-the-art in scientific knowledge and proposing directions in which an academic field can and should move (including its clinical potential)- was not able to meet REF standards (as interpreted by the Head):

“Then I asked him [Head] what if you write reviews in Nature/Science and he answered ‘that’s just vanity, it doesn’t count’. But, you know, not everybody writes reviews in Nature, there are only a few people in the world who are allowed to write in Nature- this is influential stuff. You are setting out the directions for the field as a whole- clinical, basic, whatever- I mean this is impact-type stuff so I don’t know why this would not count.”

From this excerpt it can be deduced that periods of time in which the PI (as author and spokesperson of the laboratory) was positioned under this ‘impact’ script appears to be relatively short and the techniques management use for implementing this script can be informal (through conversation). As such, the REF’s criteria of impact appears to De Jong less like an ‘iron cage’ in which the laboratory is constantly trapped, and more like a leaky roof they cover-up on occasions which accentuate this problem. At the time of the interview this controversy had not been resolved, although the PI intimated that he considered this matter more pressing for the Institute than his laboratory whose major objective in meeting the REF was still its publication and grant targets.
In sum the group structure of the laboratory is adjusted slightly to accommodate pressures of translational research. This is done intermittently and in a way which appears to (re)affirm existing circuits of power. The post-doctoral researchers had the task of aligning their projects with ‘translational’ agendas delegated them by senior members of the laboratory at various points in the life of a project, namely through conversations in meetings. This form of task delegation was reasoned to be appropriate to the levels of competence post-doctoral researchers were expected to possess. For their part, the post-doctoral researchers appeared content to distance themselves from such responsibilities and concentrate on their main work tasks. By common consent much of the responsibility for constructing a vision for the laboratory’s engagement with translational research was said to be the responsibility of the PI, whose role was to manage and build the reputation of the laboratory. He therefore had to ensure the continuing operations of the laboratory and account for the research being done against the expectations encoded in certain scripts like the REF. Efforts to stabilise definitions and stories about the laboratory’s activities were not completely accepted by the Institute Head. This suggests that arguments about robustness and relevance have to be fought and negotiated and when common definitions stabilise, this is a relational achievement (Latour, 2005, 138; Nowotny, 2006, 5). As will be argued in the next section, this positioning within the group distinguishes De Jong’s account of alignment between translational research and career from those of the other respondents. This general topic will now be taken-up for further discussion.

**Careers**

The extended notion of credit will now be applied to study scientific careers and the orientation of researchers towards translational research as a capital investment. There were diffuse patterns across respondents’ accounts in relation to their careers as individual scientists. Much of this can be attributed to the individual’s positioning within networks of the laboratory
(‘hierarchy’) and the credit each had accumulated to date. The post-doctoral researchers accounted for their current employment positions as a procedural step in their intended career paths as scientists. They told of being incentivised by the goals of learning a range of skills which would further qualify them as competent practitioners of science and equip them with necessary forms of capital to be mobilised in pursuit of paid employment in academia. In doing so they would also fulfil their responsibility towards the laboratory of producing interesting research which would result in publications in scientific journals. Both during their PhD research and into their post-doctoral training these two researchers had focused on kinase proteins. Alignment with a particular family of kinases promised a broad range of advantages from which their careers might stand to profit. By becoming associated with a formal organisation (the laboratory) with a reputation in this topic area the post-doctoral researchers hoped to boost their own credibility as scientists. In particular the post-docs told of how the reputation of the laboratory with using particular techniques in particular problem areas would reflect well on them as individuals in the eyes of prospective future employers recruiting expertise in this problem area. In terms of explanations which chime with the credibility cycle model, respondents spoke about investing in areas which would yield doable research problems—i.e. promise a return on their investment:

“I: So what attracted you towards this change?

Post-Doc 2: There were a few things— I was looking to gain a new skill-set when I started my post-doc. As well as my cell biology background I also wanted to have new skills so that if I wanted to go find another job or I wanted to go move into industry then I also now know x, y, and z. This lab does a lot more animal work and I had never worked with mice before. So that was one aspect. Also the mass-spectrometry aspect I thought could be quite applicable if I wanted to move into industry. It’s more of a new
technique, so I thought it could actually look good on my resume to have this entirely new skill set.” (Madeira)

How they had come to occupy this strategic position in the field was narrated as much in terms of accident as design, but having moved to occupy such positions they had come to consider some of the potential pay-offs it might afford them as individual scientists:

“Post-Doc 1: There are so many interesting questions to answer and to do experiments with, especially with the system that I work with. These enzymes are phenomenal, in terms of how little is understood yet how important they are— they’re very highly conserved within evolution, they are important in diseases— but we still don’t know what they do. So for me I’m trying to work on different aspects of it.” (Enfield)

One of the potential pay-offs of focusing their growing expertise on this topic was the relative ease with which they could align problem choices with growing external demands for translational research they anticipated would occur in future. Growing interest in this idea within their own and other scientific fields was interpreted as potentially leading towards opportunities for them to accumulate capital as individual scientists, by joining forces with a PI and laboratory with a reputation for translational research which they hoped would make them a more marketable commodity in future:

“I: Interviewer: So is that something you are conscious of yourself, or…

Post-Doc 2: I’m not as much yet, but that’s part of the reason I came to this lab was because I thought it would be a good place to learn how to think that way— because I never did that in my old lab as much. And I think that is an important skill just from a purely financial point of view, from a security point of view, for so much more funding
nowadays you have to be able to have translational research and be able to have ties
to industry or potentially want to go into industry.” (Madeira)

For Enfield, the need to articulate her interest in basic science with external translational
agendas was something which she believed would be advantageous, if not obligatory, to her
career. Yet her lack of know-how as to how to go about doing this was the cause of some
insecurity, both in terms of long-term future employability and her ability to acquire competent
member status in her tribe. For some then translational research connotes a certain degree of
disembedding effecting loss of ‘ontological security’ (Giddens, 1990).

The career-paths of post-doctoral researchers were modelled on typified versions of scientific
careers ‘laid-out’ within their field: where success was measured in terms of ability to move
away from laboratory bench work towards acquiring responsibility for more managerial aspects
of work organisation (ultimately becoming PI of their own laboratory). Conversely for the
technician whose role is attached to ‘routine’ experimental processes, translational research was
something that management worried about. His own career path was constructed away from
academic science:

“I: Is there a trajectory in terms of career development that you look to?

Tech: I guess so, I’m part of a service and I’m becoming more involved with the
administrative side of it too. It would be towards maybe managing a service facility for
example. That’s where I see myself going in the long term, in the short term it’s more
expanding my knowledge in the area. The past three years I have really been focusing
in particular techniques and machines, so now my focus is to become more of an
expert rather than a generalist.” (Morales)

Translational research was very much part of the ‘bigger picture’ to which he was contributing in
small part, but had little influence on the kinds of investments he would make as an employee of the laboratory.

For the figure of the PI, unless they take-up an altogether new type of role at the level of the field or a more managerial position within the university, further career development is typically tied into the fate of the laboratory in terms of expanding its operations and reputation within a given problem area. As one might expect, the concerns of De Jong (as a tenured professor) with regards career development were not centred upon questions of financial security, so much as loss of reputation he had built up over time in his field. Therefore ensuring the laboratory was organised in such a way as to ensure continuing success through credibility cycles was much more of a priority than getting individual experiment to run properly (as with post-docs and technicians).

The fieldnotes recall that interviewing De Jong was first suggested by the Institute Head on the grounds that he was an enterprising figure with ‘a strong commercial vision for translational research’. His reputation for this vision also arose amongst his colleagues with whom I spoke and this theme emerged regularly (I felt largely unprovoked) from our interview discussions. Conversely I was surprised to find that my interview with Mendez- who had after all co-founded a spin-out company with De Jong just prior to the fieldwork- did not produce this kind of account. For Latour and Woolgar scientists often begin to describe their careers in such an entrepreneurial way when they come to take charge of their own laboratories, as this is when:

“...they may begin to cash credibility in their own name. They may thus say they ‘have had ideas’, that it is ‘their’ laboratory, and that is they who have managed to attract money and equipment in the interest of securing the basis for their operations.” (Latour & Woolgar, 1986, 229)
Mendez had not yet reached such a position. Conversely then an entrepreneurial repertoire emerged at various points in data from De Jong’s interview, notably during passages discussing his career development and the risks associated with pursuing translational work. Notable features exhibited in De Jong’s account include unwillingness to play by the rules expected of him by more conservative colleagues (‘laggards’) remaining embedded in basic science:

“You know, I have to say that what you do not know you find a bit strange and I think it is fair to say that I go to basic science meetings and I feel that I am sometimes a little superficial compared to my colleagues. Because, you know, you can’t do everything, there are only 24 hours in a day. I work on fifteen subjects. I do mouse work, cell work, I talk to companies. This comes at a cost. You are broader. I think some of my colleagues are so specialised in a very small area. I was in that area before, but now, when I talk to them I obviously don’t have that depth of knowledge anymore. And sometimes, you know, you are looked upon as if ‘ooh that was a stupid question’. But it’s a small price to pay, I don’t mind. I don’t mind.” (De Jong)

Conversely those basic scientists who stuck relentlessly to exploiting and specialising in existing forms of knowledge were brandished as conservative and risk-averse:

“There are people here in the cancer centre that have a mouse which may have a metabolic phenotype but they don’t look at it because they are nothing to do with cancer...I think people should look a little broader in their work and think a bit more translational. I think this is all about translational- you don’t necessarily have to do it yourself, but, you know, you can actually give it to someone else or talk to another person...I think colleagues of mine that have no translational inclination, they say ‘I can’t be bothered, I don’t want to do it, I want to look through my microscope, nothing is going to happen’.”
Disposition towards exploring translational research (a ‘new’ institution) as opposed to ‘playing safe’ by specialising in basic research marks him out as an enterprising figure: a leader rather than follower. Similarly on several occasions he remarks about his propensity to ‘stick to his guns’ and ‘see through’ decisions after periods of deliberation. The opportunism used to portray his past actions was applied to his interest in translational work and more generally to the very family of PI3Ks on which he had built his reputation. For example the ‘decision’ to focus on experiments using blood cells was a pragmatic one emanating out of ease of access to this tissue type. Likewise, the interest in exploring translational aspects of his basic research was reported as emerging serendipitously as a result of an unexpected linkage between the family of kinases he had discovered and human diseases:

“I have to say that some of the targets I worked on were highly expressed in blood cells and it was like a natural door if you like.” (De Jong)

The know-how he acquired from this time in his career with regards aligning his basic research to disease processes was something he claimed he had pursued ever since. Hence although not exactly a ‘Eureka moment’, he was able to pinpoint a particular event which triggered his interest in translational work. This interest was defined in terms of the strategic advantages it would bring in terms of increasing the size and reputation of his laboratory. As the kinases he had discovered had been a growth area of research in his scientific field over the past 15 years and he had received credit for this, it was perhaps not surprising to find that the organising of his laboratory was very much centred around running experiments which would lead to publication on this topic. Out of this came many pay-offs. Pursuit of translational research was therefore just one benefit afforded to him by the ‘interpretive flexibility’ of PI3K as an ‘epistemic object’ (Knorr-Cetina, 1997):

“I have a real passion for science. I think it shows and I want to use it. I want to use it to make business, to do good papers, to train people.” (De Jong)
In terms of future development, he described a desire to remain close to the laboratory and increase his profile as ‘a basic scientist with an interest in translation’. He was however also conscious that to do so would mean adapting to growing demand for work in the lab to demonstrate ‘translational’ qualities, which in the short term meant becoming more acquainted with expectations of funders like MRC (as well as BBSRC). It would also mean having to negotiate various political pressures from the institute calling him to expand the size and scale of the laboratory. He had so far resisted this on the grounds that it would bring about more variables over which he would have less control.

As discussed above, interacting with stakeholders outside of basic science can be time-consuming and potentially risky in terms of detracting from other forms of work for which they might well receive more credit. The ability to profit financially from undertakings outside of the laboratory such as consultancy work, patenting and royalties for books can be said to add credence to the model of the scientific-entrepreneur (Knorr-Cetina, 1981, 79). Yet despite the emergence of authorship and risk in accounts of more established individuals, the notion of scientists as entrepreneurs is a dubious one. In part this is because they remain to all intents and purposes dependent on the government (as ‘employees’) (ibid.) in order to work. Ability to work ‘may depend on decisions made at top organisational levels’ (Knorr-Cetina, 1981, 77), thus restricting the extent to which scientists can be said to have ‘control over their ability to work’ (ibid, 78). In advancing in their careers scientists adhere to a set of institutions stipulating how individuals are to pass through the ranks of laboratory hierarchy, suggesting in fact that they are anything but rule-breakers.

In sum what forms ‘translational research’ is expected to take and how important it will become as their careers develop are ambiguous, open-ended questions. As well as the potential advantages of pursuing translational agendas, for respondents the sheer ambiguity of this pressure also appears to carry threats of ‘ontological insecurity’ (Giddens, 1990), with regards
shifting the laboratory’s problem choices in this direction. The improvisation of aligning translational research with work they had been doing on Kinases might suggest more broadly that under present conditions in order to travel in a way which ‘speaks to’ scientists and engenders changes in practice, the idea of translational research must be re-embedded alongside existing mundane activities and practices, rather than altogether displacing them. This might mean that in settings where this would risk bringing disruptions to existing operations and symbolic attachments, the idea would be more likely to encounter degrees of avoidance and/or defiance (Oliver, 1991). Making sense of this problem was thus a matter of improvising based on what they already knew and had come to expect about this given problem. Methodologically, interviewing researchers at varying stages in their careers provided the advantage of revealing the uncertainties and ambiguities which beset members of this laboratory. What became clear is that these respondents’ deal routinely with overdetermined information out of which they are trying to make some sense. Against this process, it has been found that translational research is but one pressure of which they are expected to consider.

Chapter Conclusion

This chapter has presented findings from a case study of a molecular biology laboratory’s engagement with the idea of translational research. Each of Latour and Woolgar’s four frameworks for studying the mundane aspects of scientists’ activities and practices were used as initial sensitising concepts through which the data was coded and analysed.

The history of the laboratory section found that in general translational research was defined in terms of that which brings them together with others (a boundary object), notably clinicians and commercial actors. The term also circulated as a boundary object between members comprising the host cancer Institute and was promoted as a management tool for internal marketing purposes. Through interview responses I inferred that sustained embedding and positioning of the laboratory’s research in norms and activities seen as becoming of basic research appeared to
sustain degrees of resistance towards the institutionalisation of translational research. Senior management figures like PI and S/L, instead projected strategies of compromise with which they sought to guide actions towards these field pressures (Oliver, 1991). Hence aligning basic cellular experiments with specific classes of diseases was seen as one means of meeting translational agendas whilst retaining their identity as a laboratory interested in basic science whose main currency was still in ‘Science/Nature Papers’.

Owing to types of data generated from fieldwork on this case, some of the analysis reported in this chapter does not mirror that of the previous case. This is particularly the case in terms of applying Latour and Woolgar’s notion of microprocessing. In the absence of data gathered in situ, this framework was instead used as indicative of a broad set of motifs against which to explore and code for patterns in the interview data. In doing so it was found that certain important boundary objects came into view in instances where respondents described the processes of working with allies in the commercial and clinical worlds (i.e. when doing ‘translational’ work). In a commercial capacity, one such mutual interest can be inscription devices into which the university’s technology transfer office and big pharma had expressed interest. The shared goal of seeing the mass spectrometry technology they developed scaled-up into commercial usage was clearly a key reason for interacting with commercial actants. In terms of interacting with clinical actants, mutual agreements about the factual status of knowledge on genetics enabled them in part to ‘speak each other’s language’.

Despite identifying ‘common ground’, the respondents were also keen to (re)draw boundaries of their involvement in such processes, by way of ‘boundary work’ tactics. This would allow them to retain sufficient levels of local expertise so that they could ‘retain their identity’ as a basic science laboratory and ‘stick to what we’re good at’. Compared with most aspects of the previous case study, here the discourse of translational research appears to have engendered boundary-perturbing effects which are more problematic to participants in this local
organisation context. Reasoning from laboratory studies (Fujimura, 1987), it can be argued that dramatic alterations in categories of problem choice (for example from very basic to very translational types) is likely to meet some resistances where this would necessitate step-changes in the places and practices which are said by participants to constitute ‘the laboratory’. One of the boundaries then in moving towards translational problem choices in a ‘purer’ form (measured by carrying out research with patients) is the disruption this would likely cause to those routine aspects of organising to which respondents have become accustomed and attached.

Applying Latour and Woolgar’s *inscription* framework to written outputs of the laboratory yielded some illuminating findings. Their account of scientists’ concern for producing statements evaluated as factual and fundable via a series of ongoing interconnected actions (see for instance their credibility cycle diagram), although still current, clearly does not cover all of the struggles now faced by scientists in this case study. Latour and Woolgar gave little attention to the struggles scientists faced in enrolling support of funding, possibly because the laboratory they studied received regular income stream from the U.S. NIH, therefore having rarely to justify the utility of their research. Rip’s (1994) extended model goes some way towards compensating for this in reporting increasing pressures for relevance and legitimacy at the field level, arguing this has meant scientists are now required to justify their local research in these widened terms (Rip, 1994, Rip, 2004). This requirement was pertinent to certain inscription tasks observable in the data in this case, particularly the practices of writing grant applications and journal article reports. In engaging with translational types of research, certain respondents required confidence that projects could be funded and would lead to publication. This means they must mobilise a range of persuasive skills to enrol gatekeepers like medical research funders and journal editors, to an extent that was not appreciated in Latour and Woolgar’s study. Although speculative Type I statements have long been present within scientific journals and grant
applications, these were usually made in relation to contributions towards puzzles recognised inside the scientific specialty/field. Many of the texts included here also included statements with regards the wider practical utility of the findings. These sit alongside existing Type IV and V statements, rather than displacing them altogether. For Latour and Woolgar and for Rip, this part of writing practices was defined in terms of struggles for facticity. But demands for relevance implied by these particular findings suggests that discourse of strategic (read ‘translational’) science has become more widespread and influential across stakeholders in the research system since Rip’s (1994) article was published. Perhaps as a result, at the local level scientists have had to become more accustomed to making promissory ‘utilitarian’ statements, so much so that, for some, it is considered already an ordinary part of routine practice. In the context of these case study findings, it might therefore be prudent to include struggles for relevance within the parameters of credibility cycles, and not external of them.

From the cycles of credit section, it was found that translational research was a concern for certain respondents more than others, owing largely to the function of their respective roles in the organising of the laboratory. Notably this was a problem the PI was expected to deal with as the most senior member in charge of managing research agendas and formulating strategy. As such the problem of translational research appeared not to disrupt the hierarchy of the laboratory to any significant degree. The PI framed translational research as carrying some risk to their (main) pursuit to publish on basic science, as it could potentially detract from the scientific reputation of the laboratory and its members. However, he and Mendez saw commercial involvement as beneficial for bringing money into the laboratory in the form of licensing and consultancy fees and further successful grant bids, as well as enhancing its reputation. Hence where it could feasibly bring in credit without disturbing knowledge-power dynamics, translational research was earmarked as ‘doable’ area of problem choices for the laboratory to focus upon. Positioning their interest in PI3K within the parameters of a cancer
institute relieved ‘struggles for legitimacy’ in large parts for this group compared with researchers interested in other diseases which carry stigma like obesity (discussed in next chapter). This is not to say that the ‘struggles of legitimacy’ segment of Rip’s (1994) model is necessarily entirely redundant, simply that surprisingly it was less pressing in the context of this case.

In considering their career development individual scientists recognised increasing demand for translational work over recent times and anticipated this becoming a growth area over the coming years. Such an orientation is highly suggestive of translational research as a self-fulfilling prophesy (Merton, 1948). Strategically this meant needing to become more outward-facing towards social actors in commercial and clinical settings. For the post-doctoral researchers, exact details of how they would do this were not particularly clear, as they did not yet have the practical know-how or network of relations in place to do so. Establishing such linkages with the social world is something ‘De Jong does’ and is not something they are expected to do. One post-doctoral researcher stated one reason for her joining the laboratory was influenced by its reputation as a basic research laboratory with some success at translational work: therefore associating herself with the laboratory might give her exposure to this type of work and transfer credit onto her as an up-and-coming scientist in search of tenure. However this instrumental account of her decision-making in relation to translational research was coupled with other investments in mouse-work and mass-spectrometry techniques. For the more senior members of the laboratory the investment in translational research was not expressed so much in terms of securing employment and professional status. De Jong’s priorities were to continue day-to-day concerns of keeping the operations of the laboratory running, something at which he felt he had achieved. However, aligning the laboratory output with the ‘impact’ agenda of the REF had been somewhat problematic.
Whereas certain of Latour and Woolgar’s sensitising concepts appear to have retained their utility in explaining the activities of these scientists, aspects of these frameworks, including scientists’ inscription practices and struggles for relevance, appear to effect mundane practices in a way not captured in earlier analyses. The extent to which this should lead to theoretical synthesis will be discussed further in the concluding chapter.

In the context of this case, the aspects of the idea of translational research which appear to have travelled go beyond simply that of a name. Rather, the problem appears to involve changes to certain clusters of practices distinct to some of those depicted in earlier laboratory studies. Although these findings might lead one to state that this idea has engendered innovative modes of organising practices in the context of this case (Czaniawska and Sevon, 2005), this must be tempered with the consideration that certain of these dimensions now appeared routine and unproblematic for some respondents. Those aspects of the phenomenon thought to pose a threat to existing routines were subject to intense boundary work tactics within the accounts of respondents. Therefore, in local research practices where novel forms of articulation work are required which pose risks to scientists’ current work routines, there is likely to be defiance and/or compromise, if not avoidance displayed in strategic responses of organisations and individuals towards this agenda (Oliver, 1991). Findings also reveal that concerns about how to accommodate translational researcher are also intermittent and off-set by other events and pressures in their environment of which they are seeking to make sense. Overall translational research appears to require integration into existing mangled practices (Pickering, 1995), thereby adding further layers of complexity into these already entangled webs.
7. Transforming Diabetes?

Introduction

In this chapter STS materials are utilised in order to explore how the idea of translational research ‘travels’ within the specific and contingent context of a research group’s mundane work practices. It takes a case study format telling of a mostly basic science group researching a prominent disease in ageing adults: Type II diabetes. The main body of the chapter is structured around findings generated through pragmatic application of Latour and Woolgar’s four frameworks in analysing data on this case. It concludes by considering the effects of translational research on this local context and by reflecting back on the utility of existing STS materials in studying and accounting for these findings. Before doing so, it will briefly introduce the ‘cast of characters’ around which the case study is centred.

The sample size for the study was six, with five researchers and a Dean of the medical school (see Appendix 1). The sampling of this case differs slightly to the other two empirical chapters, in that not all respondents identified themselves first-and-foremost as members of this formal organisation (which is referred to subsequently under the pseudonym ‘diabetes group’). One respondent in particular, Bateman, as a clinician with a laboratory in the University Medical School’s partnering hospital, did not affiliate himself with the diabetes group. He was brought into the case when earmarked by others as part of the informal network with whom the diabetes group would collaborate. Likewise- Kapoor- who had just started as a PhD student of Bateman’s on the NIHR’s academic-clinician track, was planning to learn his trade in the diabetes group’s laboratory space later into his PhD, but resisted speaking on behalf of the diabetes group when asked about specific organisational practices. Neither of these two respondents spoke in ways which would meet Douglas’s (1986) criteria of group members: defined through
identification of ‘we-ness’. However, as they were recognised as part of the network through which the diabetes group subsisted and were accessible for interviews, they were brought under the case. By contrast other respondents spoke of themselves as members of the diabetes group. These respondents worked in the group’s formal space, which was located in a large shared laboratory facility on the third floor of a building on university premises, situated directly opposite its NHS hospital partner’s building. They included McGregor, the PI, who had only recently founded the group. Another PhD student- Turin- who was also on the NIHR’s academic-clinician track, but 18 months into the course and by now more familiar with laboratory work than Kapoor. Finally there was Thompson, a PhD student in the first year of his course with training background in basic science. The study was also able to benefit from an interview with the Dean of the Medical School in which the diabetes group and Bateman’s laboratory were hosted. He shared similar research interests and in the past had collaborated with both McGregor and Bateman, identifying his role as linking-pin in helping to bring them together. His role of Dean also meant he was able to talk more broadly about translational research and the strategic responses formulated by the medical school and university in response to this pressure. The findings from analysis of empirical materials will now be discussed in further detail.

**History of the Laboratory**

As with treatment of data in the other empirical chapters, the objective of this section is to apply ANT devices in order to map out allies brought into play and local meanings given to the objects that were used to define translational research. The section begins by reviewing the definitions attached to the term by the Dean of the Medical School, then considers how this immutable mobile/boundary object was deconstructed by respondents situated at the ‘coalface’ of research activities.
From the perspective of the Dean, writing and implementing a translational research strategy was cited as a means of meeting the (co-evolving) institutional control of the REF, particularly its ‘impact’ criteria:

“Dean: I think in the REF one significant difference with the previous assessment is that they actually improve credit for impact. Now what that makes people like me think about is that we don’t look just to this next REF, but we look to say the next ten years, you know because if these same individuals are still here we have to demonstrate that these people’s research has impact and actually we have to show evidence that it has actually made a difference. Now that process involves translational research. As a school we have to think about the types of infrastructure and mechanisms that we have to put in place in order to ensure that we do have an impact.”

Part of the ‘infrastructure’ in place was the formal ties between the medical school and the NHS University Hospital situated across the road from their university site. It was hoped that this ‘co-presence’ (Goffman, 1967) would serve to bring social actors from university laboratories and the hospital closer together, so as one could benefit from the knowledge of the other. Again this coming-togetherness marks translational research as a boundary object, working to open-up communication between these two worlds, in this instance at a local geographical level. In the Dean’s interview, translational research was also defined in a way that went one-step beyond the older category of strategic science, in that it was no longer deemed sufficient for researchers to have promising basic science findings, but instead were being encouraged to pursue more explicitly ‘technoscientific’ (cf. Latour, 1987) modes of work organisation. Again this was in part a strategic response for accommodating demands of the REF:

“Dean: In the past we do the research and then we forget about it. We then have to wait for somebody else – an industry or someone else- to come and pick that research up and develop it further. The classic example is that you do research and
throw it across the track to the next trial, and hope that somebody else picks up and runs with it. But the thing is with that often people who have developed the trials have developed insights which may get lost once they are picked-up by someone else. You do not only need to involve someone else but it’s helpful to have some people involved who are further down the pathways as well.”

According to the Dean, the medical school’s research agenda was structured around engagement with the MRC’s vision for translational research (with its ‘T1’ and ‘T2’ gaps). As such the MRC is credited as a key macro-actor in transmitting this immutable mobile into the medical school. At the local organisational level, the process of aligning particular research practices of individual research units to different components of the MRC’s de-contextualised framework required some ‘interpretive flexibility’:

“I: So is the school’s vision tied in with the MRCs definition of translational research?

Dean: More or less. I think we will not cover every aspect of it... but I think in the areas that our research does cover, that MRC-definition is what we are trying to do.”

How these diffuse types of research corresponded with translational research was acknowledged to vary, but was nonetheless something for which each was said to be accountable. The Dean described how he wanted to get even fundamental scientists to start to respond to this agenda:

“And for me the challenge is trying to work out where that work can move, to work which could then be translated if you like. So I think trying to do things which are applicable in any kind of health context. At the moment they are simply working out ‘how does it work?’ So ‘what are the neurological implications for cancer?’- that’s what I’d like them to do.”
Another aspect of early stage translational work, which described a significant proportion of the work done in the school, was ‘bedside-to-bench’ research, in which human materials (from the local NHS hospital for instance) were transferred into the School’s laboratories in order to gain fundamental insights into disease processes. It would be hoped research findings could then feed-back in a ‘bench-to-bedside’ direction:

“Dean: But then it is a case of taking this type of work through ...trying to put a context to it and say ‘yes I have discovered this protein, but could it be about biomarkers of say cardiovascular disease?’ and things like that.”

In addition, the Medical School also encompassed research focusing on the MRC’s T2 gap, such as studies working on implementation and validation of interventions (which have already been tested and developed) into routine practice in the health service. Aside from being translated into the formalised strategy of the Medical School, a notable feature of the Dean’s account was how translational research was construed as a problem of adjusting individual behavior, with the subject position of a ‘good scientist’ being defined in part in terms of getting them to do more to align problem choices with this challenge:

“I: Okay and as a dean, are the researchers expected to pursue this agenda in some way or another?

Dean: Well I think they all understand that they are not working in a research institute; they are working in a medical school. So the purpose of a medical school gives us several things we have to do. Apart from doing excellent research and publishing good papers, getting grants, and teaching, there is an expectation that the medical school will also contribute to making a difference in terms of health, whether on our patch, nationally, or internationally. But I think that’s the general culture that we are trying to put into the medical school, where everybody- including the person doing research on
basic molecular processes-thinks that, ultimately, the work, the mission, has to be to change health in some way.”

Translational research is evoked here as a central idea in the institutional order of medical schools. Notably however the emphasis on practical contribution does not altogether displace other goals like ‘excellence’ in publication. Here again the idea of translational research appears to enable certain agents to act-at-a-distance (via talk and texts) encoding information which looks to ‘nudge’ scientists into acting in certain ways. For their part researchers in this case were aware of circulation of such messages and general (albeit ambiguous) instructions they carried:

“I: What kind of vision do they [the school] have then which you have to be aligned with?

Bateman: It’s a tricky one that. I think the usual things like innovative research, you know, patient benefits and so on. It’s all kind of general things which you’d expect, you know? Innovation and patient benefit I think are the main things. Clinical relevance and so on.”

Definitions of translational research given by ‘coal-face’ respondents frequently referenced what they assumed were taken-for-granted delineations institutionalised in the medical research arena. For instance accounts pointed to institutionalised ways of ordering research themes picked-up from attending large medical research conferences. Here translational research was reported as an intermediary stage at the intersection of basic and clinical categories of problem areas, which were claimed to be broadly accepted and legitimated within medical research discourse. But how was this de-contextualised label transformed by mediators when brought back into the particulars of the Diabetes Laboratory’s local research practices? Through which

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25 His account (re)defines how medical schools, as a type of institution, should ‘think’ (Douglas, 1986).
networks were definitions made visible? What forms of work did these connections seek to accomplish? These are the questions to which the focus of this section will now shift.

Within the MRC’s schema, the domain in which the group in the present case study was said to be operating was ‘T1’. The thrust of the group’s interest in linking basic research on diabetes to practical clinical outcomes was summed-up by the PI, McGregor, through the example of their interest in ‘dampening inflammation’ in body tissues:

“McGregor: We are very interested in inflammation and trying to dampen inflammation, because we feel if you have inflammation it has the effect of exacerbating diabetes. That may be one of the driving forces for type II diabetes and cardio-vascular disease. So if we can find a target to actually dampen that without causing any other side effects then pharmaceutical companies would be keen to develop that knowledge.”

Producing new knowledge of inflammation could, they reasoned, lead to greater possibilities for targeting this effect through new drug treatments. Hence the coupling of the group’s specific interests with translational research was defined first of all in terms of a near-market positioning with pharmaceutical companies: ‘bench-to-bedside’ or ‘lab-to-market’ research. Here the ‘bedside’ is constructed as pre-clinical then clinical stages of drug development performed by pharmaceutical companies. This focus was anticipated as helping to establish, via university’s technology transfer office, future opportunities for spin-out and licensing agreements. A consequence of pursuing this basic research is coming into contact with clinical materials and their associated clinical researchers. Here then a second illustration of translational research is brought to bear in terms of ‘bedside-to-bench’ research:

“So we would go from the clinical-type elements and then we would bring it back down to the basic science. So it would be going from something that is already developed
and is given to patients in terms of drug treatment, and we look at what the additional
benefits are and how that actually works at the molecular level.” (McGregor)

A particularly prominent feature of such ‘bedside-to-bench’ collaboration in the context of this
case was described in terms of formal and informal network ties with the local NHS hospital In
particular, the gatekeeper led me on a trail towards Bateman, clinical researcher with a
laboratory located in this hospital, with which the diabetes laboratory had particularly close
network ties. The role of clinician enabled the latter and his clinical colleagues to approach
patients coming into the hospital with diabetes or endocrinology problems as potential research
subjects. This in turn provided basic scientists in McGregor’s group with a convenient conduit for
accessing clinical materials, such as patient samples. Bateman’s laboratory also included certain
pieces of ‘hi-tech’ instrumentation and inscription devices, notably a ‘state-of-the-art’ machine
for monitoring body’s energy expenditure, the purchasing of which had recently gained some
local and national media publicity\(^{26}\). One of the bases of the diabetes group’s collaboration with
the clinical research laboratory had been over use of this equipment. Here then mutual interest
in running experiments using this specialised instrument and materials brought the groups
together, triangulating basic and clinical work on energy expenditure into complex research
processes subsequently labeled as ‘translational research’. These had been identified as such on
the basis of tackling problems which fed-back between typified modes of basic and applied
clinical research. This association brought a ‘presence’ (Latour, 1987) of relevant clinical research
to their basic studies on cell inflammation.

Much of the collaborative linkages pointed-out by McGregor and Bateman alike were defined in
terms of PhD projects co-funded and/or co-supervised across the two research organisations.
These boundary spanning-projects and their associated individuals were cited as providing

\(^{26}\) Incidentally funding for this expensive custom-built piece of equipment had been awarded through a
‘Translational Medicine’ scheme by a local strategic partnership between public sector and industrial
organisations.
concrete examples of ‘translational research’ projects conducted between basic and clinical contexts. One such project was Kapoor’s, whose PhD looked at the effects of certain drugs for controlling obesity paths, first by carrying out clinical observation study of the effects on the whole-persons, which would then be triangulated with findings from fundamental experimental studies on cellular responses to these drugs. Another clinical research fellow, Turin, was carrying-out PhD work across the two laboratories with McGregor and Bateman as co-supervisors. As an academic-clinician he was training in basic science laboratory in order to study the effects of food intake on weight control at the molecular level, and to study its pathological effects. The work was designated as part of a translational process because it took clinical samples and experimented on them with fatty oils. The process of taking samples from ‘bedside-to-bench’, growing them with and without fat cells and comparing results, it was claimed, could lead to promising findings which could then be ‘taken through’ to tests on animals and humans (‘the translational stage’):

“I can see if I do it in the lab and I see that chronic is worse than intermittent [weight loss] and which are good paths then I can use it to go and do research which would look at what course of action is better [for patients]. I would obviously need to do research which would prove that in terms of clinical trials, but I can see that what I did it in the lab and then go on to see its clinical context- so it’s following the process through- having a basic science and clinical interest means I can do both.” (Turin)

This PhD problem is framed as a means of linking Turin’s existing interest and knowledge about clinical issues with new skills acquired in relation to basic laboratory science. Translational research thus labels the bringing together of his basic and clinical focuses.

As well as the links with the hospital’s clinical laboratory, the diabetes group’s translational remit was extended to collaborations and interactions with pharmaceutical industry. For instance at the time of fieldwork there was an ongoing five-year collaborative partnership between the
group and a large multi-national pharmaceutical company. The basis of the collaboration was to explore the effects of fat tissue in cell stress, but with added focus on how such stress might be reduced through targeted interventions. This linkage brought their basic exploratory work into contact with those engaging in the ‘next stage’ of translational research (e.g. proof-of-principle studies). Again PhD training was an important boundary object in establishing the laboratory’s network connections with this ally. Thompson’s PhD was funded and co-supervised through this same company. In the context of Thompson’s work, translational research was brought into focus predominantly when referring to formal sponsoring of his work:

“Thompson: I guess you could say they are the kind of industry pharmaceutical company who are trying to translate my basic research into a more pharmacological output.”

The project thus involves in part a shift around the interests of his more immediate supervisory team in the diabetes group and industrial supervisors. From the management’s perspective, collaboration enables members of the group access to large and expensive equipment without themselves having to purchase and maintain this fixed capital and supporting infrastructure:

“I: Would you troubleshoot with them [industrial sponsors] or would that be more the academic supervisors’ role?

Thompson: No I could also go to them. In fact they’ve got some specialised techniques they use up in their lab. I’ve not gone there yet but I will be learning a few things with them. So they can show me further techniques as well.”

These illustrations suggest the network ties associated with translational research to be based on negotiation of what ANT calls ‘equivalence’: the drug company can use students as a relatively cheap means of doing research; in turn the academic group brings in various forms of capital through which to support and extend its operations. Likewise the diabetes group and
local clinicians are brought together through access to each other’s know-how, labour force, equipment, and materials. Translational research helps to rationalise such relationships, bringing in a normative justification and putting a dramaturgical spin on organising being done within the laboratory, for the consumption of various audiences: Deans, REF panels, potential allies (e.g. future sponsors), prospective staff and students, and so on. As a metaphor for innovation, translation thus appears to provide a post-hoc rationalisation of otherwise complex and messy local research practices in much the same way as metaphors were argued as doing in accounts scientists produce for describing their activities (Black, 1962).

Inscription

This case study is unable to draw on grant applications as documentary material on which to base analysis of this section. Likewise evidence taken from coding of journal articles (described in brief below) pointed towards ‘traditional’ passive modes of authorial voice used in the accounting for empirical phenomena in these texts (Gilbert and Mulkay, 1984), as opposed to more speculative, contingent, promissory statements regarding practical utility of findings (see examples in cancer case study). With these considerations in mind, the decision has been made to concentrate on how writing actions were accounted for in interviews with respondents. These actually provided a great deal of insights, especially from McGregor, who was able to reflect on first-hand experiences not only of submitting grants, but also assessing them.

One initial observation to report in relation to grant applications is that like the cancer case and unlike the Obstetrics case (See Chapter 8), this group did not concentrate on applying for funding from a single funding council or program. Instead they applied to a number of organisations: private sector firms like pharmaceutical companies, research councils like NIHR, and charities like British Heart Foundation and Diabetes UK. In describing the pursuit of grants the respondents with responsibility over this task, two senior researchers- McGregor and
Bateman described how they would make decisions about which funding body to court on the basis of likelihood of interest in a given research problem on which they were working:

“I: As a researcher do you generally follow the funding bodies’ statements, visions?

McGregor: Yeah, to be honest we only apply for the ones which sort of fit with the research that we’re doing. So the BBSRC are very interested in the sort of basic science elements. They don’t really want to hear about the medical research. But we can still do that element of the project without compromising what they wish to have.”

The instrumental orientation of this action and impression given of grant writing as a ‘game’ in which researchers must persuasively transform the interests of funders, so as to get the latter to follow the former’s interests (Type II and III transformation in Latour, 1987), is further reinforced in the accounts of respondents:

“I: Okay would they expect on say grant applications for your research to be aligned with their interests?

Bateman: Oh yes absolutely yeah. If you want to apply to Diabetes UK then you have to put a diabetes spin on it or else they won’t be interested.”

McGregor had sat on a board for assessing research applications at a major charity funding research into diabetes. He described in detail how translational research - the identification of the ‘next step’ to which the current proposal could lead - was an ‘obligatory passage point’ in winning funding from the committee:

“McGregor: I think it is because they can sell it to the patients or sell it to the sponsors, you know, and when they go to the meetings they will generally say ‘we are funding this, that we hope is going to lead to some clinical outcome’ ...So they are really critically reviewed because you know the people that are funding the studies – the
patients have spent a long time trying to get this money... So the committee is very conservative and wants to be absolutely sure that there can be an outcome from the research. It's not about being speculative. Blue sky projects don't work very well either. It has to be a very much step-by-step approach. Funding bodies- and I think this is across all of them actually- don't want to take that sort of risk very often.’

As spokespersons for this charity, the peer review board will not accept abstract promises of a clinical pay-off: applicants must position the proposal in relation to other stages of the translational spectrum towards which their findings might credibly be scaled-up. ‘Translational research’ was recognised as a prominent labeling device (‘boundary object’/’immutable mobile’) used by these sponsors to frame the basis of funding exchanges with researchers:

“I think you can do pure science if you want to but I think in terms of funding streams and particularly with what we are interested in, you have to put translational medicine in there.” (McGregor)

The presence of interest groups in the organisation of their research and the alignment of their field with these groups’ interests makes the question of whether the scientists here are driven by research problems or clinical improvement an ambiguous one. Suffice to say, although lacking textual data in the form of grant application forms, it appears from interview accounts that translational research is already something of an ‘obligatory passage point’ in order to secure funding from a number of major sources. As well as exhibiting credibility as scientists, applicants must mobilise rhetorical skills in being able to put ‘a good spin’ on the proposals as key to this particular writing practice.

With regards the presence of translational research in written journal articles, a search on PubMed yielded 9 papers accessible via my institution. These were downloaded and converted into NVivo software for coding. Again Latour and Woolgar’s five statement types were used
alongside identification of promissory instrumental statements in order to code the data. I was somewhat surprised to find no recorded instances of performative promissory statements in the texts, which instead displayed a prevalence of technical scientific discourse throughout. Part of this surprise perhaps came as a result of what I had been expecting, given the seemingly explicit requirements by grant awarding bodies for translational research to be demonstrated through these texts. It seems then whilst explicit focus on translational research is de rigeur in some institutions (e.g. grant writing) it can be less explicitly present in others (e.g. scientific journal titles).

As with grant applications, decision-making around journal submission were described as guided by consideration of which title will most likely accept the research, as opposed to moulding research from the beginning around a particular title’s interests. Thus identifying journals synonymous with translational research and then tailoring research problems to interests of these particular titles does not provide a convincing explanation of the processes of problem selections and writing made in the laboratory. Here again instrumental factors come into consideration when organising writing of articles for publication:

“Well as of yet I haven’t gone through that process. But I know where they send their work is an important decision. Other colleagues will discuss this in meetings and they always think of impact factor- that’s a key way of ranking journals by their effectiveness: how many people read the journals or the articles from those journals, and also their global distribution of the journal- whether it is available online, whether it can be accessed. There are several things they take into account.” (Thompson)

With regards audience, a number of cross-disciplinary titles were identified to which ‘translational’ work could be submitted, considered in terms of ‘impact factor’:
“Dean: Actually there is a wide variety. I think because of the nature of the work you can go anywhere from the best, which would be something like Nature: Medicine, then you have a long way down to all sorts of others [laughs]. The Journal of Clinical Investigation is another excellent one. The other specialist ones are things like Diabetes Care, Diabetes, Circulation, all of which I have published in over the years.”

In order to write papers for such titles, a division-of-labour would emerge, with different experts focusing on particular sections of the paper. McGregor would frequently play a role of lead writer, giving him greater authority for editorial control. Part of the tacit skills required in order to write in these collectives was expressed in terms of tact, deference and learning about areas beyond one’s immediate intellectual purview. This appears a qualitative departure from forms of journal writing implied as routine in Latour and Woolgar’s study, whereby scientists write disciplinary texts with a view to being read and cited by others in their specialist network. The findings of this section appear to support the assertion that scientists must become adept at incorporating a large number of demands into their practices (Fujimura, 1987, Pickering, 1995). The varying demands of translational research on their writing appears to be something to which they must adapt, albeit, at the time of fieldwork, respondents accepted such demands as already a largely routine, mundane, and unavoidable aspect of their practices. The coverage of translational research as an explicit matter of concern appears somewhat patchy across their field of diabetes research: explicit instrumental statements appear de rigueur in grant applications to research charities in diabetes, less so in submissions for basic research sponsors like BBSRC, and largely absent from content of published scientific journal articles. However, the findings in this section have led me to conclude that within the context of this case, growing demand for translational research requires extensions in the persuasive writing practices of scientists compared to the dynamics reported by earlier studies, notably Latour and Woolgar’s Laboratory Life.
**Microprocessing**

As with the previous case study chapter, this section reports on the results of applying concepts like ‘inscription devices’, ‘boundary object’, and ‘boundary work’ to the analysis of data. Specifically it is interested in how communication and consensus worked out in networks despite apparent demarcations between collaborators in ‘basic’ and ‘clinical’ research camps. Again this section utilises data generated through interviews. These sessions were helpful in terms of pointing to boundary objects which come into play in the course of collaborative work and in gaining insights from researchers about the challenge of engaging with researchers outside their own specialist networks. However, they do not provide detail of how boundary objects are actively (re)constructed in interactive settings. As such the section offers more of an opportunity to consider the utility of ‘boundary object’ and ‘inscription devices’ as abstracted sensitising concepts in the study of multi-disciplinary research contexts than it does an opportunity to engage per se with the ‘microprocessing’ framework presented in Latour and Woolgar’s book.

As a formal organisational unit the diabetes group does not have access and control over all of the resources needed to conduct important aspects of research, but instead relies on being able to redistribute action through networks of allies when required. As well as members of the laboratory not requiring legalistic ownership or legitimate control over all resources, they are not required to stock ‘in-house ’all forms of expertise embodied by varying social actors:

“McGregor: So we have the basic scientists, the bioinformatics, the pharmaceutical industry and also the clinicians. So that’s how we sort of build around our team. I don’t know all the clinical things so I can go and access them, and I don’t know enough bioinformatics so we can ask them on various elements as well. And we have collaborators as well who can help us on other aspects that we do that are different to bioinformatics. So basically there are lots of different people we call on.”
One key finding from accounts of respondents was the recognition that although working together, they did not necessarily share the same goals or sets of interests as allies. For McGregor one key facet in explaining conflict avoidance was their propensity ‘not to disagree’, citing common techniques, as well as shared normative frameworks to which both they and clinicians subscribed. Although norms as motivations are thought to provide weak indicators of scientists’ behavioral outcomes (Mulkay, 1976), it does appear feasible that ideas and rhetoric surrounding patient-oriented research may play some importance in helping to unify and rationalise the collaborative endeavours in which McGregor has been involved, albeit the context of this case provides little by way of further detailed empirical explication of this point.

Of more robust footing were references towards common themes (such as links between diabetes and obesity), methods and technologies shared across the practices of the diabetes group and hospital laboratory. In collaborating with clinical fellows, locating consensus around devices used to represent and analyse a given phenomenon in which they shared enough of a mutual interest was cited as important. As was mentioned in the History of the Laboratory section, use of hi-tech equipment in the clinicians’ hospital laboratory would provide reason for the basic and clinical researchers to work together. Furthermore, within this interactive space the running of experiments through PCR techniques was identified as something to which both sets of researchers can become attuned:

“Thompson: Well we all use PCR. RPCR, that sort of thing. The quantity of polymerase chain reaction to express the level of gene expression in our samples. We also used western blotting/glossing- so that’s also a similar technique but instead it’s to look at the protein content within a cell- for a specific protein. So we’ll both be using those techniques.”

There is recognised to be some asymmetries of expertise with regards knowledge of specific aspects of such boundary objects, however, chasms were not said to be so great as to distort
altogether information exchanged between members situated across the two worlds. The apparent ability for both to understand (or be taught about) readings from an inscription device helps form a basis for collaboration to work:

“Thompson: The results both basic and clinical scientists produce are very interchangeable and one would certainly understand the other. So maybe a few techniques which are a bit more abstract in the basic scientist’s work that a clinician may not fully grasp just because they also have the medical side to have to focus on. But I’m sure they would understand if they read around the subject— a bit of background.”

The availability of Standard Operating Procedures inscribed in manuals and shared intranet files is said to facilitate learning to operate shared machinery. Likewise it is accepted that at certain points some textually mediated engagement in clinicians’ ‘scientific literature’ is required in order to make collaboration ‘doable’:

“I: To do this kind of work do you actually need to understand what it is they are doing, or can you leave them to it with their own specialties?

McGregor: No we tend to know, or we tend to find out their expertise and to then use it. Especially when we are writing papers as a team, or I’ll be writing the papers, so I have to understand their knowledge. But obviously I would have to gain some of the knowledge through the literature.”

The issue of trust is also raised in relation to such collaborations, with recognition that reading clinical literature alone would bring basic scientists only partial familiarity with information needed to do such work. In addition, basic scientists appear to rely on the credibility of clinicians within their informal networks in order to produce knowledge about boundary-spanning problems:
“McGregor: So you do need some of their knowledge. Obviously, you know if you are selecting patients in terms of the criteria for certain studies, then you know, if they [clinicians] tell me that someone has a hypertension, they are hypersensitive and would have to be excluded from the study, then obviously I wouldn’t necessarily go and research that element because I would have to take their clinical expertise as given...So for those parts of the studies where they have clinical input, in maybe designing parts of the protocol for what patients should be selected or not, then I would leave that to them.”

McGregor then provides a hypothetical illustration of having ‘a sense for’ knowing when to intervene in relation to a finding, thus bringing a research problem back under his expert jurisdiction:

“If there is something about [a clinician’s] interpretation of ‘oh well we’ve found out that protein x does such and such, and that’s important clinically because of such and such’ then I might go and look at the data as well and see what that is.”

As such tacit inclinations appear still to be important facets of scientific practice (Collins, 1990), despite changes in context and production sites of science towards greater levels of technoscientific collaboration than was captured in situations described by earlier laboratory ethnographies. As with the previous case studies, concepts such as boundary object and inscription device help to account for formal and informal means through which ‘common ground’ is established when basic and clinical scientists are brought together in the course of articulating research projects designated under the label ‘translational research’. As such the STS-inspired notion that scientists rely on objects and devices in bringing them together with other stakeholders is one which is supported in the data from this case study.
Cycles of Credit

From utilising Latour and Woolgar’s *cycles of credit* framework to code and analyse data, it was found that accountability for issues of translational research was not distributed symmetrically across the group. The section will begin by describing how accountability towards this problem was spoken about and enacted by respondents, before moving on to consider how careers are constructed in relation to the problems of translational research.

Group Structure

In contrast with the previous case, it became clear that the forms of social control associated with translational research remained enforced from quite a distance and with little regularity, with the senior management of the Medical School allowing large degrees of autonomy and interpretive flexibility with regards how this group responded to this problem. There was little indication that the researchers themselves had (yet) been made to account formally for how their work would meet criteria of translational research, be it through face-to-face meetings or in mediated forms. Part of the explanation for such a flexible governance approach was attributed (by both Dean and researchers) to the relative ease with which, when called for, it was thought the group’s research could be aligned in a credible fashion with translational research. Despite general awareness, a notable feature in the data was the varying forms of responsibility and accountability distributed in relation to this object. The PI was the member of the group responsible for linking together tasks associated with this problem and formulating a strategic vision, in contrast to others whose day-to-day concerns focused around problems situated at the experimental level of work organisation.

One of the PI’s responsibilities was to apply for grants, which requires amongst other things the translation of general societal problems into specific research problems being addressed in the laboratory. As well as being formal leader of the group, through interviews with PhD students it became apparent that McGregor acted as legitimate head and spokesperson the group (Knorr-
The PI occupies a more managerial role in relation to the research, steering and articulating levels of work organisation in a way which delegates action to members of the group along formulations of ‘rank’, competency, and mundane considerations like time-tabling.

Insights into the effects of translational research on processes of grant applications were simply not forthcoming from more junior respondents’ accounts, as they had not (yet) begun to forge such networks. Staff and students were hired by the PI in order to work on distinct projects positioned around a common research theme in which (they hope) the laboratory will become a reputable player. Having a group focused around similar theme in the same place is also hoped to bring about opportunities for troubleshooting and internal collaboration, thereby building local expertise within the group (a theme in laboratory studies and communities of practice literature). In interviews the objective of McGregor as qualified spokesperson of the group was to build and extend their operations through cycles of credibility conversions:

“McGregor: I suppose personally I’m quite a high drive, get things done, get papers published person. I think part of that as well is to encourage students and staff. We have lots of PhD students I’ve supervised, fourteen that have passed and I think I’ve got five or six at the moment.”

Within this institutional context there were asymmetries of knowledge and accountability towards translational research. Interestingly, for example, it was Kapoor’s supervisor (Bateman) who foresaw a likelihood that his student’s research would develop in a translational ‘direction’, with Kapoor himself still unsure of the details how his work would progress in such a way. Hence although he agreed with the general goal of developing an interest in translational work through his PhD, the specificities of the problem choice and his contribution were still unclear:
“Kapoor: Through my work I would like to help the rest of the scientists already trying to find obesity through brown fat and aiding them and giving them tools to actually quantify and see whether their drug or method works. That’s my broad area of research. So I hope it will help all these researchers who are in the process of trying to find a drug or a cure, or a fat or a weight losing measure.”

As with the cancer case study, the practical problems posed by translational research appears to occur in ways which reconstitutes existing power-knowledge dynamics in the group (Latour and Woolgar, 1986, 229). This credibility appeared to reinforce McGregor’s overall position as leader of the laboratory, as it meant he was the only member qualified to manage across all main levels of its work organisation (Fujimura, 1987). From having sat on committees awarding grants, being lead author on scientific publications, supervising students, McGregor also had much greater degree of control in relation to organising actions in response to this problem. His ability to exercise control legitimately was based on his credibility as a more senior scientist and his more senior occupational role the institutional order of a university. But McGregor was himself manager of a division in a larger organisation, meaning he could be made accountable to senior management figures in relation to the outputs of his group, including its translational pay-offs. The REF, for instance, was a matter of concern for more senior academics, such as McGregor and Bateman. The PhD students interviewed lacked detailed knowledge of the REF criteria, with two even not initially recognising the name of the exercise. They dismissed explicit engagement with this concern as beyond their responsibilities, although they were brought under line with its goals through other means, such as career incentives (see ‘careers’ section below). Again a power-knowledge nexus appears in part to inform how accountability relations get worked out within the group. Although PhD respondents expressed a general normative commitment to the idea of translational research, they were ignorant of the co-evolving institution of the REF and its call for ‘impact’:
“Thompson: I’m only familiar with the outcomes of the previous one- is it RAE?- I don’t think I’ll be in a position for submitting that work because they really look for high quality experienced work. I might be able to contribute towards other members of the group or even supervisors, but no, personally I won’t.”

At the time of fieldwork McGregor and Bateman described how management of the Medical School had given only intermittent attention to their respective responses to impact demands. This control had been loosely enforced and respondents had not been hard-pressed to come up with an immediate set of solutions to these problem areas.

To conclude, within the group translational research and the co-evolving institutional control of the REF were more of a concern for those in managerial positions. This in turn consolidated the position of the PI as head and spokesperson of the group, suggesting that the problem of translational research does little to undermine the more traditional form of hierarchical organisation in the group. As will become clear in the next part of this section, translational research is a problem which early-career researchers anticipate having to provide solutions for in the processes of pursuing their individual careers in academic science.

**Careers**

One noticeable difference respondents here had from the other two cases was that they spoke of their group as having not yet fully established a strong strategic position in their field of research. By contrast then, much of the talk of careers- be it those of individuals or the group- were framed around goals of securing such position.

As a PI, McGregor’s career was aligned with success of his group. Much of his concern for the strategic positioning of the group centred on their ability to compete in markets for translational research in the near-future. In a recent scenario presented by Rip (2011), scientists unable to
demonstrate credibility for translational research will struggle to attract funds from a number of research sources in the next decade. This scenario was found already to be apt in the context of this case: the laboratory was applying for funding from small grant bodies for 'preliminary studies'. Until they had such studies in place, McGregor and Bateman claimed they would be unable to win funding from MRC, who fund projects capable of scaling-up such basic research findings:

“I: Where do the majority of your funds come in from- the MRC, NIHR?

McGregor: Not for mine no- with the MRC it is actually very difficult to get funding from them. You need to have preliminary data in place in order to be eligible. So at my stage we’re not really looking for that at the moment. We’re more looking for the grants to get preliminary data so as we can then get in a position to apply for funding from MRC.”

The market for research which did not follow this model was thought too small and therefore too risky for them to pursue:

“No, I mean I think you can do pure science if you want to but I think in terms of funding streams and particularly with what we are interested in, you have to put translational medicine in there, because the days of doing science for the sake of science, you know, are sort of gone. You know the days of looking to see what a molecule does have gone- it has to have some practical use or offer some practicality that you think has long-term future that you are looking into with your basic science.”

The relative lack of existing stockpiles of credibility in their strategic position was construed by the group’s leader, McGregor, as a problem to be met by encouraging rapid cycles of credibility conversions. As a relatively new group they were not yet endowed with the promising results or
reputation to compete for large MRC grants in translational research, which were seen as the domain of the ‘big players’ at other prestigious universities. Taking preliminary results through further translational steps was therefore a future goal McGregor hoped the group could achieve by way of rapid credibility conversions and moving towards grants at an ‘intermediary’ stage between their current preliminary studies and desired big program grants:

“McGregor: Well there is the NIHR program grant. They’ve got a development fund as well which is designed to develop projects- to then be in a position to apply for program grants.”

Hence translational research is positioned as an obligatory passage point in order to continue and extend operations in the foreseeable future. Translational research grants from MRC are seen as the ‘royal road’ for competing successfully in this hostile environment and building-up credibility required to apply for this funding is a strategic priority for McGregor and his diabetes group. Yet another concern relates to the field of diabetes research in particular. Predicted rises of obesity and diabetes to levels of epidemic provided respondents with some confidence that this field of research would continue to attract investment. Yet they were also wary that there was increasing competition with others moving into occupy a strategic position within the area of diabetes research:

“McGregor: I think the trouble with type II diabetes is that it is incredibly competitive, because you have Oxford, Cambridge, Imperial College London- lots of these universities that have very high calibre diabetes research departments...Those things are a lot more difficult now and you are competing against the same people each time who are very high calibre... I think money is a lot less than it used to be. And I think it will get a lot worse and that people are going to have to think about other avenues to try to support their research, look further afield and think about their research in a different way. I think it’s going to get tough for the next few years.”
In terms of short-term strategies, recruiting clinicians as PhD students to be trained in the ways of basic science was a strategic effort to forge networks with the local hospital and build-up expertise and reputation for research at the interface of these two worlds. Likewise having PhD students co-funded by pharmaceutical companies would add further layers of equivalence between themselves and the industrial partners needed to build a profile in ‘bench-to-bedside’ research. The group could also focus on articulating lines of experimental work deemed to be ‘doable’ in the context of their laboratory resources which could then be packaged as ‘translational’. For example, conducting a PhD project around fatty acids was convenient for Turin as he could gain easy access to these samples as a clinician working in the local hospital:

“I’m looking at oils and comparing saturated fats. I’m growing cell lines- I’ve done that and now I’m looking at clinical samples. If we get fat samples then we have a good access to fatty acids...clinical research is easier to get access to because we are on the ward seeing patients.”

Hence although modest in comparison to the contributions of large grants in translational research, Turin and his supervisors were keen to align basic experimental problems with resources in the laboratory and wider clinical problems in a manner which was ‘doable’ in the time it would take to do a three year PhD. As such, translational research is not necessarily made to denote research which is significant in scale of contribution, but can be used to rationalise and package quite modest projects which meet with its broadly accepted criteria.

As with McGregor, Bateman was looking towards larger program grants around translational research as something to work towards for his laboratory. This was seen both as ‘where things are going’ and as a ‘natural step’ in the trajectory of current preliminary research being carried-out. Thus current research is defined in relation to this prospective problem even though at the time of fieldwork his work did not fit easily into the category of translational research:
“Bateman: That is kind of basic work with a translational element – you can translate it to patient care actually, the management of obesity and so on. I think [the Dean] is more involved in that side of it at the moment. But I am trying to... I will be working more on that with time.”

Possible career trajectories varied considerably between, on the one hand two PhD students on NIHR’s academic-clinician program (Kapoor and Turin) and, on the other, one co-funded by a pharmaceutical company (Thompson). Kapoor had yet to become fully immersed in the diabetes group and therefore identified less with this group than the clinical laboratory. His career trajectory was constructed in terms of the NIHR’s formal academic-clinician path, with training in basic science seen as an interesting and necessary step on this path. The problem of translational research was one which he saw as consonant with his intended career trajectory, requiring little by way of ontological re-specification away from the career-type he looked set to follow. Translational research was therefore spoken of in a very positive and enthusiastic manner in relation to future career developments.

In being co-funded by a pharmaceutical company, Thompson’s possible career development could deviate significantly from his clinician colleagues, with the degree providing a possible platform for work in industry-based science. The status of his PhD work as basic science with the promise of scaling-up into industrial developments made ‘translational research’ seem a ‘good investment’ in in terms of building a reputation for a career in industry. The demand for scientists here is seen to be much greater than demand for reputable scientists in academia where there is oversupply competing for dwindling demand from state funding agencies:

“Thompson: Yeah well I think it is pretty tough for anyone at the moment. But I think the fact is there'll be more opportunities in industry, because the organisations are running more like a business which need new employees regularly. Whereas I think academic positions are a bit harder to come by.”
Investments were being calculated in his accounts according to levels of security they seemed likely to bring. Other considerations—strategic positioning, normative commitments, and personal motivations—were ranked as secondary in terms of importance compared with such issues. Although translational research would likely form an important problem category along which to align his future problem choices, he recognised that he might not find academic employment, which would thereby render this concern redundant. This enabled some deferral in terms of how confronted this problem within his account. For now he would try to publish as first author in order to boost his value as a commodity in the market for academic scientists (Knorr-Cetina, 1981).

By contrast, Turin did not appear to share Thompson’s concern for publishing scientific papers over the remaining period of his PhD, as he had already published systematic reviews whilst working as a clinician. Securing his place on the NIHR academic-clinician track provided some assurance of being able to continue to do research in an academic capacity after his PhD. He declared an interest in pursuing translational research in his future career in the form of follow-up studies to his PhD work. However, the opportunity to discuss further his career plans was cut short by the respondent, who had to finish the interview abruptly (see Methods chapter). The account of Kapoor gave was very tightly coupled to the institutional career structures of academic clinicians and associated norms. Translational research was taken to be consonant with such norms, which, Kapoor claimed, had been a strong influence on his behavior and would continue to be so:

“I am funded by NIHR so I definitely support the goals of NIHR and I try my best to incorporate the values of NIHR into my behaviour. There is an interest in patients. Which is largely in keeping with Diabetes UK and Wellcome- they are all the same – do ethical research; research that will have a health impact; do something from which patients and everybody benefits. I think that is what we are doing. Those are the right values.”
Carrying-out studies further up the translational ‘ladder’ was seen as a ‘logical step’ in terms of continuing to align his actions in this way. However, Kapoor was the one respondent across the cases, that, when asked questions about career did not speak in economic language, such as ‘investment’. This could be a potential weakness in Knorr-Cetina’s and Latour and Woolgar’s theories. However there was a strong possibility that he was ‘saving face’ within the interaction of the interview: as someone who had spoken of his primary commitment to patients, it would be embarrassing to then talk about his self-interest in forging a career. Indeed frankly it would be hard to imagine he had got to the position he was in without having made such calculative decisions at some stages of his career. Furthermore, he spoke at other points of keeping results secret until the point of publication, suggesting he was- like the other scientists in my cases- at least somewhat concerned about issues of credit and reward.

In sum, respondents in this case constructed versions of future actions in relation to their careers against signals from the market for diabetes research. The (arguably post-hoc) rationalisation, by some, of careers to date in terms of alignment of their problem choices with translational research was a means of strengthening their credentials as individual scientists in markets for future employment. Aligning future problem choices with external funding agendas in support of translational research was seen increasingly as an obligatory passage point in order for them to continue and prosper in a career as an academic scientist. Again this anticipation of translational research as representing ‘where things are going’ marks it out as a form of ‘self-fulfilling prophesy’ (Merton, 1948). The construction of individual career pathways formulated in accounts appeared to correspond with levels of accumulated scientific capital and various institutional trajectories in terms of career structures, publication requirements, and training needs.
Chapter Conclusion

The aim of this chapter was to describe how translational research ‘works’ in the mundane practices of a small group of researchers working in the area of Type II diabetes. In doing so it looked to extend existing empirical knowledge on the subject of translational research through use of STS materials. Simultaneously it looked not only to use these materials as a resource, but also as a topic whose utility could be considered in relation to this task (see conclusion).

The history of the laboratory section showed that translational research was defined along institutional categories prevalent in the medical research arena, as those studies which ‘plug a gag’ between basic and applied studies. It showed that researchers at the ‘coalface’ articulated an alignment between their own complex research processes and this abstracted label/metaphor. This section also brought into view a network of allies through which this idea is made visible and thus ‘doable’, which included collaborators such as clinicians and pharmaceutical companies.

Applying the inscription framework highlighted how translational research has become an important consideration (a ‘box to tick’) for researchers in this case responsible for writing grant applications. Part of the rhetorical skill needed in practising this action involves being able to transform highly specific, specialised scientific problems into something which can credibly promise to make a useful contribution to clinical treatment of diabetes. The need to write persuasively can also be extended to the task of making NHS ethics applications. As such, struggles for ‘facticity’ (cf. Latour and Woolgar, 1986) are also met with struggles for ‘relevance’ (Rip, 1994) and ethical robustness in the practice of writing. Yet despite an extension in the persuasive rhetoric deployed in these situations, the ability to adopt the passive voice of scientific author is still a necessary component of writing in others, such as for journal articles.

What was striking from this section was that seemingly innovative modes of organising (versus
earlier STS case studies) fitted alongside more familiar aspects in a way which was accepted, mundane and largely institutionalised.

The microprocessing section helps to build an argument for the importance of ‘inscription devices’ and ‘boundary objects’ in bringing together and forging common forms of understanding between the basic science group and its clinical and pharmaceutical allies. These could include items mundane as use of common pieces of laboratory equipment and readings from a PCR machine, as well as the suspected links between diabetes and obesity as a clinical problem. This finding suggests that social studies of translational research have been so far opaque with regards how basic and clinical worlds are able to work together at the level of mundane, everyday work practice. The literature has so far concentrated on questions of failure, such as why large-scale funding pushes in programs like stem cell research have failed to translate into clinical treatments, concluding that there are huge cultural differences between basic and clinical worlds. However this ignores how it is collaborations between these two worlds are able to function at all successfully (even if it is at just one phase of the pipeline).

Finally the cycles of credit framework helped to show how the travel of the idea of translational research had been transformed into a problem for which managerial figures were made accountable and responsible. This means they are able to author and then delegate certain tasks in relation to this problem. The lack of a strong strategic positioning in markets for translational research meant a priority for the group was to build a reputation through rapid movements through credibility cycles oriented towards this problem, for instance in the form of small grants which could be later exchanged for larger MRC grants in translational research. For early career respondents, the importance of translational research varied according to their existing credit levels as individual scientists, training needs, and career structures. The findings in this section suggest scientists will respond proactively to the challenges of translational research in situations in which it is calculated as being in their interest to do so.
In concluding this case study, it remains to ask what aspects of the idea of translational research have travelled to this site. The name has certainly travelled, as it labels some of the types of projects they have done in the past and look set to continue doing. Aspects of the practices consumed under this label are also present here and deemed ‘doable’, such as writing of grants and establishing ways of working with clinicians beyond their own specialist domains (as seen in sections on ‘inscription’ and ‘microprocessing’ respectively). Yet certain problems associated with translational research are not yet deemed ‘doable’ for the group and individual scientists, such as winning large MRC grants. As such they must extend their operations in respect to this problem in order to compete with any success. What is clear from the findings then is that translational research is deemed to be ‘where things are going’ and thus takes on the characteristic of a ‘self-fulfilling prophesy’ (Merton, 1948).
8. Transforming Obstetrics Research?

Introduction

This chapter uses empirical findings to describe how the idea of translational research interacts with the mundane work activities of a particular group of researchers specialising in obstetrics and gynaecology in the scientific and regulatory field of Health Technology Assessment (HTA). By the end of the chapter it is hoped that a convincing account of the ‘transformation’ (Latour, 1987) of this idea in the context of this case will have been provided. Latour and Woolgar’s four frameworks constitute the primary ordering devices around which the chapter is structured. The chapter will conclude by considering what aspects of the idea of translational research have travelled and the extent to which incorporating this idea entails innovative modes of work organisation in this particular case (Czarniawska and Sevon, 2005, 10). The case study is based on data generated from spending ten weeks following around six participants who at the time comprised the full membership of the research group. As elucidated earlier (Chapter 5), this case study was able to benefit from rich access to observe obstetric team meetings, clinician network meetings and clinical trial steering committee meetings. These six members included the Group Lead- Quereshi- an academic-clinician practicing gynaecology with broader interests in methodology and medical education; Matthews- a senior lecturer (S/L) specialising in systematic reviews; Kumar- a senior clinical lecturer (SCL) focusing on issues around obstetrics (her area of clinical practice); Pereira- an academic-clinician learning his trade as a post-doctoral (post-doc) researcher; and two others focussed more around getting the clinical trials to ‘work’, including Morrison- a clinical trials manager, and Aziz- an obstetrician in the local hospital. There were also interviews carried-out prior to the case study with a Dean of their host (mainly applied health research) faculty and an academic-clinician who had just joined the faculty.
History of the Laboratory

Taking the ANT definition of an object as the point of departure, this section analyses how translational research was defined in relation to their networks of allies by respondents in this case study. The intellectual ‘glue’ holding the group together was a collective commitment to evidence-based medicine (EBM). Norms and values of EBM and HTA were strongly supported by all members of this group and were found to be reiterated by Quereshi in his inaugural lecture and by respondents in interviews. As the Centre Lead suggested, this may well have derived from the NIHR’s general championing of T2 translational research since its inception in 2007 (having ‘trickled down’ from the ‘cosmopolitan’ to ‘local’ level (Rip, 1997)). Indeed Quereshi himself was open about the notion that he (and therefore the group) practised a type of ‘ritual conformity’ towards the vision of the NIHR (DiMaggio and Powell, 1983, Oliver, 1991):

“Our vision is very much tied into what NIH does, so we respond to NIHR’s calls and very often we succeed. Sometimes we don’t succeed, but as long as we are doing things on the NIHR line- the HTA line- then that works fine... I think they are...interested, if you like, in the extreme end of translation where you are figuring out this thing that is translatable already, whether it should be used in the NHS or not.” (Quereshi)

There appeared to be strong elements of ‘mimesis’ in their adoption of the idea of translational research (at least in its form, if not substance) (DiMaggio & Powell, 1983). This cognitive category might be extended sociologically and be displaced by the statement that they are following a fashion, which functions as a kind of collective choice mechanism in organisational and scientific fields (Czarniawska, 1997). In this case the label ‘translational research’ was used interchangeably with ‘applied research’. As such incorporating ‘translational research’ does not require significant disruption to existing resources, inscription devices and routines with which they operate in their day-to-day work organisation, as in a sense it is already internalised. The
versions of translational research presented in the course of interviews were very tightly coupled with EBM and HTA norms that new evidence should come from clinical studies, usually randomised controlled trials (RCTs), along with synthesis of existing evidence through systematic reviews and meta-analyses (Greenhalgh, 2006). The legitimacy of clinical trials and by extension the researchers who perform them is that they provide scientific evidence to support the ‘gold standard’ of clinical practice (Timmermans and Berg, 2003). The meaning given to translational research by Quereshi in response to the interview question ‘what does translational research mean to you in the context of your group’s work?’ was related primarily towards the type of clinical trials they had performed historically:

“For me it means we conduct clinical trials which test the applicability of something that has emerged from lab research, to patients. It is about doing research directly in patients, with newly observed molecules, devices and so on.”

There are a number of different types of clinical trial and study. This group carry-out Phase IV clinical trials, largely in the form of RCTs, that assess the ‘overall risks and benefits’ of interventions which have already received licenses and are already part of clinical practice in the NHS (UKCRC, 2006, 15). Hence the metaphor of translational research- originally used in pharmaceutical drug development- is being picked-up and appropriated within the context of applied clinical research. In the context of this case ‘discoveries’ refer to the potential shortcomings of existing interventions on the market (discovered for instance through systematic reviews), which they then translate into ‘proof-of-principle’ studies in order to produce scientific evidence for relevant decision-makers. The clinical trial functions as a means of ‘scaling-up’ from promising discovery towards workable intervention (Dehue, 2001). For the obstetrics group, the research they do simply occurs at a later stage in this translational ‘pipeline’ than pharmaceutical development. Systematic reviews were also framed as part of
their translational process, as they could potentially contribute to changes in clinical guidelines and constitute a basis for follow-up primary studies (e.g. trials).

The translational hope of this research is to ‘displace’ (see, Latour, 1983) existing standards (or lack of standards) informing decision-making in clinical practice. Publication of guidelines was identified as the boundary at which the group’s involvement in the ‘career’ of the fact ceases and becomes the domain of an external network (Latour & Woolgar, 1986). In order to be translated into these spaces the researchers require subsequent acceptance and championing of the object by external gatekeepers. In affluent nations like Britain HTA and EBM are increasingly able to influences decisions of health agencies like National Institute for Clinical Excellence (NICE) (Webster, 2007, 34). For researchers such as these, operating in HTA field thus provides ready-made ‘infrastructure’ and ‘strongly-convergent network’ (driven by ‘macro’ actors like NIHR) in place to support (and legitimate) this type of translation (see Callon, 1991, 1995). The NIHR funds HTA research with a view to it being translated more-or-less immediately into evidence that will inform clinical practice. In terms of the translational potential of the group’s research, operating in this field means there is ready-made demand for the production and dissemination of the facts they produce:

“On a day-to-day basis we do this study on [Intervention X], and if it shows that this actually works then in the next six months the NHS changes its policy and says ‘everybody will get this test and in this condition’” (Quereshi)

This is not always typical of the situation of researchers in other fields: in comparison the infrastructure in place to scale-up the research for the cancer and diabetes groups was sparser and networks had relatively weaker convergence. Hence this field as a whole benefits from positive externalities in the form of ‘the state’s regulatory apparatus in regard to the public health-care system’ (Webster, 2007, 168). The institutionalisation of HTA within public health care systems means there is relatively high demand for the types primary and secondary
research they perform, albeit this also means they face competition from others supplying these ‘products’. In systematic reviews parts of a study are pre-specified to the researcher, sometimes with questions provided on a given complication, intervention or outcome. Once completed, synthesised evidence from systematic reviews usually gets written into guidelines and reports to inform clinical decision-making, like clinical practice guidelines, which are made available through international databases such as PubMed or Cochrane, and/or in Royal College or National Guidelines. Support from state apparatus towards HTA also enabled respondents’ claims that the speed of their findings’ translations into clinical practice gave their work greater legitimacy and relevance than other areas of the sciences, especially much basic research:

“This feeling in the academic community that you are a good scientist if you do basic science and talk about stuff no one can understand...if that’s supposed to be better than tracking thousands of women on a clinical trial then you know...It’s usually the case that, at things like conferences it’s dominated by basic scientists; it’s always been the case actually...and systematic reviews are sometimes not even considered as research, because they think you are putting together other people’s research as data and so it’s not your own...which is not the case. But it’s the case that if you do basic science, you are considered a serious researcher, you could call it.” (Kumar)

The majority of the group’s external funding was received from the National Institute of Health Research (NIHR), in particular the Health Technology Assessment (HTA) program. HTA is a relatively new form of science emerging since the 1980s concerned with overcoming deficits to existing evidence informing clinical practices and replacing it with more robust scientific evidence about efficacy and cost of clinical interventions. The stated practical outcomes from HTA programs are clear: improving patient outcomes and efficiency of allocating scarce resources in health care contexts (Lehoux, 2006, 2). Unlike the other case studies in this thesis which apply to different funding bodies for grant money, the group align their strategic vision
with just this one funding body. Following Latour’s (1987) influential typology on transformations, the group’s use of translational research as a metaphor for clinical innovation resembles his type I transformation, as the scientist (a relatively weak contender) can profit from ‘piggy-backing’ on ‘a vastly stronger’ ally (110), in the form of the state’s regulatory apparatus. Large bureaucratic public health systems are prominent allies for the scientists, who can profit from the HTA movement having in large part already recruited, drilled and forced the state’s regulatory apparatus to be ‘simultaneously interested and obedient’ to their facts (Latour, 1987, 172).

This framework, as appropriated by later ANT (Law and Singleton, 2005, 336) thus provides the blueprint for making sense of what is otherwise a particularly unruly object of inquiry. For this particular case, the cost of adapting their practices towards the problem of translational research is relatively low as they already produce findings which are readily ‘scaled-up’ via an extensive actor-network: HTA. It is ostensibly the name as opposed to the contents of the idea of translational research which has been transformed through the group’s mundane practices. Used as a label to package their work, translational research provides normative rationalisation which serves to further defend and legitimise their actions when brought under scrutiny (cf. Meyer and Rowan, 1977, Czarniawska, 1997).

Inscriptions

This section considers the persuasive skills enacted by scientists in the acts of producing inscriptions for external audiences. It was found wider pressures for translational research have effected a change in the situation of scientists, to the point where they no longer have to appear persuasive simply in terms of their claims for validity and credibility (as in Latour and Woolgar’s framework), but also in terms of the relevance of their work.
Latour and Woolgar’s *inscription* framework suggests a number of ways of studying the persuasive skills deployed by scientists in the course of fact creation. Some of these are more practical for my purposes than others. Unlike Latour and Woolgar, one of the problems I faced was gaining access to the full spectrum of writing activities performed by members of the group. As well as having rich levels of access, these authors also enjoyed the benefits of observing interconnected forms of inscription occurring within a single geographical production site: the laboratory. One feature distinguishing the activity of this particular group from that of scientists presented in earlier laboratory studies was that a great deal of the inscription activities occurred outside the traditional material setting of a laboratory (as the ‘workplace’). Indeed the work of this group may be likened to that of a ‘bioclinical collective’ whose knowledge production processes extends beyond the confines of the laboratory (Rabheriosa & Bourret, 2009). The meeting room I observed constituted perhaps just one centre in which acts of inscription occurred. This is a problem other ethnographers have encountered: where the action is perceived to be occurring elsewhere (Law, 1994). One aspect of Latour and Woolgar’s *inscriptions* framework I was able to take forward was analysis of the rhetorical construction of written outputs by the group’s members, in the form of journal articles.

I performed an author search for Quereshi on Pubmed Database and Cochrane Library and downloaded all of the articles for which my institution had access (n =8). I decided to select for analysis those articles reporting on clinical trials in which he had collaborated. The articles I accessed appeared to require little by way of (terse) promissory statements at the end of articles (see van Lente and Rip, 1998b). Articles reported on clinical trial questions about discreet clinical problems, the answers for which ‘should’ be translated rapidly into clinical practice on the basis that they have followed robust scientific procedures. This is corroborated by Quereshi’s interview account of translational outcomes as ‘part-and-parcel of what we do’. In reporting results Quereshi and his colleagues do not diverge from technical discourse that typically
characterises the genre style of scientific journals. For example, the conclusion section of a multi-centre trial on breastfeeding interventions is written not in terms of potential promises for further research, but with matter-of-fact certainty which typifies much of the discursive style typically reported of scientific journals in STS (Gilbert and Mulkay, 1984):

“A universal service for initiation of breast feeding using peer support workers provided within antenatal clinics serving a multiethnic, deprived population was ineffective in increasing initiation rates.” (Anonymous)

Statements in these text seek to persuade through use of type IV and type V statements that omit agency of the researchers from the accounts and instead make the ‘methods’, ‘findings’ and ‘conclusions’ ‘speak for themselves’ (Latour & Woolgar, 1986). Drawing tentatively on findings from the previous section, the relative proximity of their knowledge products to clinical application may mean the researchers are not required to make terse promissory statements about potential future developments of their work (of the type found in more upstream, basic science journals and grant applications) . In their field relevance is internalised in the applied character of the clinical problem choices their research field has been made to deal with (Rip, 1997). As the label ‘translational research’ is interchangeable with ‘applied research’ and somewhat implicit in the types of studies they do it does not appear to require the respondents to change how they seek to persuade these external gatekeepers on whom they are dependent for publication. Therefore in terms of their interaction with the social world (Fujimura, 1987), translational research does not appear to have disrupted the flow of practices in performing this specific aspect of textual inscription.

**Additional Coordination work**

At the level of production work (Fujimura, 1987) it appeared that contra disciplinary laboratories, this group was unable to ‘stock’ the resources necessary in order to perform their
experiments. This was because getting ‘technicians’ to set-up inscription devices and perform experiments was not something that was performed ‘inside’ by members of the group in a single production site. They were instead reliant upon outsourced markets of clinician and nurse volunteers situated in multiple hospital wards across the UK. As the clinical trials manager explained:

“I suppose if you have multi-centre trials you sort of have staff who are removed from... you know... they are not part of the central research team I suppose. What I mean is they are not employed as part of the research team. So there will be staff at each of the centres who we are relying on to recruit patients and complete data and everything. So potentially that could be difficult in that they are not directly employed by us. Obviously we are very much reliant upon them. So I suppose that’s a challenge to getting it done.” (Morrison)

Recruitment is a persistent problem within the NIHR’s HTA funding stream, with around 80% of studies requesting either more time or more money (according to Quereshi in his inaugural lecture). To make the trial team’s inscription devices (questionnaires) travel and act at a distance requires the enrolment of allies in the form of NHS staff and patients. On their own the researchers are not strong enough to enrol and drill these elements; they require institutional support. Comprehensive Local Research Networks were set-up by NIHR as a key initiative to support timely completion of clinical studies within the NHS (NIHR, n.d.). Prominent researchers conducting MCTs with regularity in the NHS are invited to submit applications to this ‘sales network’ (Pinch, 2008, 477) for recruitment of participants into clinical trials. The group I observed had successfully enlisted the support of the NIHR shortly before I began fieldwork and during my stay I was invited to observe the inaugural meeting for their new CLRN. Without this meeting, respondents would be unable to ‘scale-down’ the phenomena in which they were interested into a statistical form. What I observed here then was a sales-pitch to an audience
composed of clinicians, nurses, and NHS R&D department officials. Performing these kinds of projects with any success thus requires researchers to engage as salespersons at certain points. Salespersons can be seen as ‘mediators’ (Pinch, 2008): in this case helping to move about inscription devices (e.g. questionnaires) from their centres of calculation (e.g. offices, trial steering committee meetings) to the peripheries (e.g. NHS maternity wards) and back again.

One of the chief tactics used to build this network was conducting formal face-to-face meetings with prospective allies, which can be interpreted as a kind of co-present ‘interaction ritual’ (Goffman, 1967). The organising of the meeting can be read as an effort by the researchers to render themselves indispensable to the audience (and their absent colleagues), so as the researchers in turn will reach their own goals (successfully running MCTs) and associated interests. The intermediary object around which the meeting was framed was the clinical trial. The occasion of the CLRN meeting was even framed by some speakers as an effort to ‘sell’ involvement in their studies. They did so by presenting its benefits.

Public displays have long been important in the history of experimental sciences. Robert Boyle, for instance, used this ‘literary technique’ to demonstrate the uses of his air-pump technology to the Royal Society (Shapin and Schaffer, 1985, 57). Public spectacles are a means both of presenting products in front of agnostic audiences in order to show what they can be made to do and for supporting the spread of a particular ‘discourse’ or ‘form of life’ (ibid). The inaugural meeting of the CLRN provided a spectacle centred on presenting the utility of the researcher’s product- multi-centre trials- so as to enrol audience members into actor-networks needed to complete such projects. More generally it sought to institutionalise NHS clinician’s participation in research projects.

Like Boyle’s Royal Society demonstrations, those present constituted a very select ‘public’ (ibid, 58). Individuals from relevant NHS Trusts were invited on the basis that they themselves could be persuaded to participate in the researchers’ multi-centre trials and would also go on to help
recruit colleagues absent from the meeting. Like air-pump science, MCTs are expensive, ‘temperamental’ to run, and therefore a somewhat scarce commodity. In other words both resemble ‘big science’ (ibid, 38). On its own then the product appears rather cumbersome and unlikely to ‘sell itself’, meaning in order to travel from the centres to the peripheries much extra coordination work is required. Yet unlike public meetings held for Boyle’s air-pump, the product being promoted here could not be demonstrated immediately in this particular time and place. Instead various numerical and visual inscriptions were composed onto PowerPoint slides (as ‘immutable mobiles’) in order to re-present in a performative way successes of past clinical trials brought to patients, which were not directly witness-able by the audience. What was made present and absent was of central importance to the rhetorical construction of these presentations. For instance, the messiness of designing MCTs which I had observed during trial steering committee meetings was notably absent in the presentations given to this audience. A rather linear account of innovation was given instead, whereby past successes of clinical trials (stripped of all their transformation struggles and contingencies) in bringing about altruistic patient benefits were made present. Analogies with successes of past trials were then used as the basis for constructing promises about future trials in which the audience were being invited to join. In the narratives of the presenters, through following the short ‘detour’ proposed by the researchers and giving them what they want, the audience will be able to overcome the ‘blockages’ and get what they want: a return to the ‘main road’ (Latour, 1987, 111). In this instance, one of the main roads is the improvement of patient care. The audience were being defined as a professional group whose primary interest lies in the advancement of patient care. But in addition to the patient-benefits, throughout the presentations speakers imputed benefits clinical trials could bring in future as capital for the audiences’ departments, individuals’ curriculum vitae and NHS performance assessments. Furthermore, it was not only the brilliance of the product which was used to enrol the audience, but the competence of the team involved in replicating past successes (cf. Shapin and Schaffer, 1985, 56-59). Hence the success of the
product is inextricably linked to the credibility authors and champions of the trials, particularly their leader and spokesperson: Quereshi.

Once the merit of clinical trials was ‘established’, deficiency statements were introduced by speakers. Based on these accounts, the only problems associated with running clinical trials were the apathy of individual clinicians and the bureaucracy of NHS Trusts (all other contingencies were black-boxed). The presenters then offered themselves as solutions to these two problems across the course of the talks. This mode of persuasion follows precisely the formula of transformation presented by ANT: projection of a desirable goal, followed by a problematisation in reaching that goal, then presenting oneself as solution to that problem (‘if you do what we want, you will get what you want’) (Latour, 1987). Alongside the ‘sales-pitch’, the CLRN meeting also allowed for exchange of tacit knowledge to occur between those working on similar problems. Prompts for audience questions were purposefully introduced between formal presentations. In these spaces, the audience would project difficulties they had experienced in trying to carry-out research in their departments (e.g. NHS bureaucracy) and suggest solutions with varying levels of conviction.

Having occasions to meet face-to-face appeared important in order to coordinate these ‘big science’ projects. Over the time I spent observing this group, much discussion was centred on problems not only of how to attract the interest of busy NHS staff, but also maintaining it over the course of studies which could last for three years or more. Holding occasional face-to-face meetings could not guarantee the mobilisation of NHS trusts into their trials. Emails and newsletters were to be sent out every few months to ‘top-up’ enthusiasm among participating NHS staff. Even then, behind-the-scenes, collaborators on occasion would express a lack of confidence that formal meetings and written communication were sufficient to maintain enthusiasm towards the research. What were deemed more valuable were informal relations amongst the local clinicians. It was the contention in team meetings that holding face-to-face
meetings such as CLRN encounter would produce a certain level of enthusiasm amongst allies that could not be achieved remotely, for instance via forms of media. In a sense then the respondents appeared to exercise a vernacular version of interaction ritual theory, whereby face-to-face encounters enhance emotions and energise people, thereby influencing how they perform subsequent actions (Goffman, 1967; Collins, 2004). Indeed during intervals at the CLRN meeting, two clinicians with in attendance with whom I struck-up a conversation volunteered the opinion that holding such face-to-face meetings was vital in order to retain their enthusiasm for partaking in a clinical trial, given their typically high workloads.

Although Latour and Woolgar’s account of scientific production processes stated that skill and effort is needed to coordinate production and mobilisation of inscriptions, the cost of doing so appeared less constraining in their disciplinary laboratory than it did in this form of multi-sited ‘big science’ production context. I would like to emphasise here just how difficult and expensive it was to produce inscriptions even at the mundane level of running ‘experiments’, given the need to coordinate multiple forms of organisation, regulation, and paradigm. Nonetheless, the notion of scientific facts (and clinical ‘evidence’) as inscriptions is helpful for retracing the practised skill and efforts which go into producing and mobilising such black-boxes.

**Microprocessing of Facts**

Drawing on analysis of ethnographic fieldwork, this section reports on the findings generated through applying Latour and Woolgar’s microprocessing framework within the context of this case study. As this *microprocessing* framework was originally formulated out of observations of a single disciplinary laboratory site, the multi-disciplinary context of this case provides an opportunity to reassess the extent to which this part of existing knowledge about routine, mundane aspects of fact creation provide an adequate picture of scientific activity in contemporary sites of ‘translational research’. In the following passage I will use Latour and Woolgar’s theorising on microprocessing in order to explore how ‘translational research’
interacts with this ‘mundane’ aspect of work activity. In this particular case study I was able to explore an (a priori) property strongly suggested by translational research- multi-disciplinarity- as this was very much apparent in one of the important production sites to which I was given access: the trial steering committee (TSC) meeting. Before reporting on specific findings which emerged from applying Latour and Woolgar’s microprocessing typology to observed exchanges, I will set-out some additional findings and details about the composition and function of such meetings.

**Defining the Trial Steering Committee**

Trial steering committees and their ad hoc, quasi-independent Data Monitoring Committees (DMCs) can be seen as institutions for ensuring the production of regulatory and epistemic objectivity in clinical trials (Cambrosio et al., 2006b, Berg et al., 2000). The TSC meetings involved a heterogeneous array of participants from different professional and epistemic communities. One trial meeting for example was composed of obstetricians, neurologists, specialist nurses, clinical trial managers, epidemiologists, medical statisticians and social scientists. The main objective of the meeting was to establish agreement about the content of the institution around which the trials were being coordinated and regulated: the trial protocol document (CT-Toolkit, n.d.). Despite varying interests that one might expect these attending members to profess about their involvement in research- whether it be saving humanity, contributing to science, gaining recognition, fulfilling administrative goals- the key purpose of this meeting was to design a protocol that would ensure the ‘successful’ completion of the study (measured against rapid conversions between forms of credit). Indeed according to the faculty head, who had a stockpile of credibility in designing clinical trials in the arena of public health, this goal constitutes the ‘glue’ that enables these particular assemblages to work together:

“I think that the different disciplines who are involved in these studies can all see the common goal, which is to get the study done, to gather the data and to do so in a
robust way. I don’t think anybody has any particularly different or divergent views about getting the study done.” (Faculty Head)

The organisation of this work in these meetings did not hinge so much around structures of social authority, as they did through object-oriented structures. As discourse occasions, these meetings provided a space in which ‘the features, reactions, and requirements of technical objects are continually exhibited and expressed, and in which everyone can, in principle, assess and follow technical needs and co-shape the strategies adopted’ (Knorr-Cetina, 1999, 173-174). This ‘management by content’ repertoire which set the tone of the meetings meant that different people would step-forward as ‘spokespersons’ on behalf of problems posed by the object and potential solutions. If this repertoire fails, for instance in there being serious disagreements about the protocol design and implementation which emerge in the course of a trial, then there are international standards which can be referred to.

In assembling the TSC, Quereshi and Kumar brought together people, with whom they had familiarity, having worked with them before and even identifying some of them as friends. Hence these previous linkages with obstetricians and statisticians suggested the committee was an extension of existing networks of allies forged by the researchers. However, this existing association had to be introduced to new members on the committee. One of the first exchanges which occurred during each of the meetings was for each person to take turns introducing themselves around the table. This suggested that not all of the participants had met one another and that in order to work better, it would be helpful for members of this internationally composed team ‘to put a face to a name’ (Urry, 2003). During this ritual, the post-doctoral researcher, Pereira, stated that he was present at the meetings in order to ‘gain a feel’ for how clinical trials work. This is one example of respondents recognising that meetings provide opportunities to pass on tacit knowledge that is not made explicit in textbooks and protocols.
Meetings for trial steering and data monitoring committees thus appear a necessary action in order to organise the production of clinically ‘objective’ knowledge through MCTs. The meetings also help to establish familiarity and trust between different epistemic and professional constituents on the trials, and provide a forum for exchange of tacit knowledge. Without such occasions it appeared that clinical trials would not be considered workable.

**Observing Epistemic Conversations**

Observing such occasions proved conducive to exploring Latour and Woolgar’s *microprocessing* framework, as it provided the appropriate setting for participants in the clinical trial to air disagreements with parts of the trial protocol document before it was to be finally submitted. This contrasted with other types of occasions I attended during the fieldwork, like weekly team meetings, where I had recorded a scarcity of direct references to ‘technical’ procedures or arguments occurring in exchanges. Without opportunities to carry out extensive ethnographic fieldwork of the kind Latour and Woolgar enjoyed in production sites, it was not possible to observe all of the dimensions of this framework in relation to the process of constructing ‘logic’ and ‘proof’. Despite cross-sectional scope, the findings have nonetheless revealed certain potential weaknesses in Latour and Woolgar’s original formulation when applied to ‘hybrid’ research settings such as this one. I was able to observe and record TSC meetings for two studies which were about to begin recruiting patients. Passages transcribed from the meetings were coded under the general theme ‘microprocessing’ and sub-coded according to Latour and Woolgar’s four exchange types. This section is organised around discussion of empirical findings in relation to these exchange types, and where appropriate, considers how far the original framework is corroborated in this altered production context.

The most alarming finding from analysing these meetings was that only two of Latour and Woolgar’s four categories of exchange emerged in any kind of recognisable form. However, the two which had emerged—Types I and IV—were prominent recurring themes in the data. I found
Latour and Woolgar’s observation that Type I exchanges typically revolve around ‘new’ facts rather than ‘long-established’ ones was contradicted by my findings. The results will now be discussed in further detail.

Boundary Objects

During the meetings, the chair moved through different sections of the document so as to give an opportunity for those present to scrutinise its content. A lot of the time the statements made on behalf of facts would foster agreement amongst participants. Facts and proposed methods around which members appeared in agreement were thus interpreted as analogous with ‘boundary objects’ (Star and Griesemer, 1989) and were coded as such. I identified a range of boundary objects which functioned to simplify information and enhance communication between these people. The stabilisation of facts within the meetings could be inferred from the data when no one appeared to challenge a part of the document or a spokesperson’s statement about that part.

A prominent boundary object, first pointed out to me in interviews, was the mutual commitment to EBM methods. This point appeared to be supported by observation of TSC meeting given that the central role of statistical methods was never once questioned in exchanges in the meetings (despite being set-out in detail in the trial protocol documents they were discussing). Hence agreement about the facticity of Type I statements appears to be at least one source of establishing certainty in the course of interactions involving ‘hybrid collectives’. I also observed on several occasions Type IV statements where participants would defend certain data collection and reporting procedures proposed in the protocol document on the basis that these adhered to general standards for reporting clinical trial data within their scientific community. Participants would also support their efforts to assume a position as credible ‘spokespersons’ on certain standards by calling on the ‘presence’ (Latour, 1987) of reputable macro-actors behind the
standards, as with a discussion centering on the governance structure of the trial proposed in the protocol document:

“Quereshi: What is being proposed here is fairly standard—and it is based on what the MRC recommends.”

Such ‘Type IV’ statements appear to corroborate Latour and Woolgar’s basic point that the facticity of a statement does not arrive at this status because of its innate correspondence with ‘nature’, but because it has withstood a whole host of deconstruction efforts, both ‘technical’ and ‘non-technical’ in character. Hence whether studying exchanges in a disciplinary laboratory or a multi-disciplinary hybrid collective, the notion that the ‘logic’ of a scientific fact cannot be dissociated from the ‘socio-logics’ of its construction remains convincing.

New, Non-Controversial Facts

The importance of ‘trust’ in upholding the credibility of spokespersons (and by extension their statements) is especially important in instances where asymmetries in expertise within the group were made apparent. Here Type I statements, primarily in relation to new facts, were supported by Type IV statements regarding the authority of specific participants as qualified experts able to ‘vouch for’ new facts. There is one analogous example given by Latour and Woolgar, when describing the dependence one junior scientist had on the experiences of another in lending a statement its fact-like status. Similar deferrals to the expertise of others enabled potential ambiguities to be overcome relatively quickly during the TSC meetings. Closed controversies were subsequently re-told at the end of the sessions as ‘tying up loose ends’. Although there was disjuncture in experience levels of participants, an additional source of dependence clearly emanated from the fact that certain members of the TSC lacked local expertise around certain inscriptions proposed in the protocol document. Several times Quereshi reiterated that not everyone in the collective was in possession of expert knowledge of
all facts. Instead standards were suggested and ‘vouched for’ by members positioning themselves or being positioned by others as spokespersons, as occurred during the course of an exchange in one study meeting between the chair (an obstetrician) and statistician about the statistical modelling proposed for analysing data:

“SC member: I feel the same [as the chairman]. It is an important clinical question and as it stands the protocol from a clinical point of view seems appropriate. But the statistical model, and the way the question will be answered by that is beyond my comprehension. There is an assumption that this has been approved by the people who are experts... [Agreement]...

Quereshi: The knowledge lies elsewhere [points to statistician... laughter]. [Statistician] has acknowledged the appropriateness of this model.

Chair: I should just then add the caveat at this point that we’re going to need more info... I think the issue needs to be resolved ASAP before the study starts, we need a meeting, especially with statisticians.”

Here the statistician offers to ‘vouch’ for the scientific credentials of ‘standard’ methodological procedures proposed to carry-out the study, an endorsement which is taken as credible given his expert status both as a statistician knowledgeable about the latest standards for what passes as ‘fact’ within this field and someone who has had experience as a statistician on these types of studies. Trust thus appears a key aspect to the social structure enacted by this collective, as it is needed foster agreement about statements (and continue making the new fact). It was not always clear from the data collected whether the participants invested trust in individuals or in more abstract expert systems like scientific disciplines to which they would refer and could be associated. The statistical approaches remain ‘black-boxed’ in the course of the meeting, to be ‘opened-up’ at a later (more appropriate) time.
Established, Controversial Facts

Despite enactment of ‘multi-disciplinary’ skills, inscriptions and boundary objects, it is not always easy to broker total agreement on these occasions. At times efforts to form consensus around certain standards broke down and controversies emerged over particular efforts to do so. Notably the facts whose status was being questioned were those which constituted ‘soft’, taken-for-granted ‘parts of the furniture’ in the epistemic machinery of some respondents, but which were challenged as artefactual irrelevancies in statements by others in the meeting. In one meeting for instance a lengthy exchange broke-out amongst participants about how many times the trial’s data monitoring committee (DMC) should look at the data during the collection period of the study. The controversy occurred mainly between a statistician and trial chair, focussing on whether by looking at data regularly over this period the DMC would unduly influence (‘bias’) the results. Speaking on behalf of statistical standards, the former argued that this carried the risk of discrediting the findings in scientific terms (a type I statement). The chair disagreed with this argument, stating that it would be helpful in order to determine whether useful data was being generated, and that the study would be completed on time. In putting forward this opposing statement, the chair re-constituted the statement as an artefact, which although perhaps not meaningless per se, was simply not important in relation to the particular set of circumstances being discussed. The chair supported his position with a Type IV statement, arguing he had done many studies before in which checking during the study had been common practice and had never caused the findings to be discredited. The issue was not resolved directly in this meeting but deferred to another meeting, which appears to correspond to Boden’s (only half-joking) observation that the main outcome of meetings in organisations tends to be arranging another meeting (Boden, 1994).

As well as pointing to a common set of goals in place, participants could foster agreement and understanding via common sets of facts and standards upon which they could all agree
Type IV codes could be located prominently amongst those instances where claims about the facticity of Type 1 statements were being defended by their claimants in the face of uncertainty and criticisms being voiced by others in the course of exchanges. It was found that Type IV statements could settle uncertainties expressed towards ‘new facts’ articulated by certain members, who would entrust others to act as reliable spokespersons (‘experts’) on behalf of facts about which they themselves were ignorant. In instances where black-boxed facts appeared to be being opened potential controversies were closed very swiftly. These findings are analogous with observations Latour and Woolgar made, albeit one might speculate that the need to trust and defer to the statements of others and those uttering them becomes heightened in contexts where diverse epistemic cultures are brought together and conduct ‘epistemic conversations’. Another point Latour and Woolgar made was that Type I exchanges about long-established facts were much less common than those concerning new facts. I have found that this was not the case here, as a majority of the fact-oriented statements being challenged concerned those facts which were for certain participants ‘long-established’. Controversies emerged when other participants retold the statements as having artefact status, such as when claims about appropriate stopping points for DMC to enforce were challenged.

**Cycles of Credit**

The utility of the credibility cycle as a general model for (de)constructing individuals and small groups of scientists in this study became clear during observations of weekly team meetings. Here the primary problems being voiced by participants regarded short-term problems in converting forms of credit. The ultimate objective of conversions being discussed in these meetings was to produce publications in scholarly journals, which could subsequently be re-invested in further cycles. This finding was also reinforced in interview accounts, with respondents pointing to the formal assessment standards of the REF placing varying levels of constraint on individuals and the group. Interviews also provided a useful platform through
which to explore ‘inner thought processes’ of respondents concerning credit investments, particularly in relation to longer-term strategic considerations like careers.

As the focus of the original credit framework was on individuals and group structures as they arise from laboratory work, this section will also take as its unit of analysis how these issues are constructed through (and not prior to) processes of work organisation enacted by respondents. The section will thus be structured first around respondents’ backstage accounts of group structure and whether translational research constitutes a priority, before focussing on how careers of individuals are constructed in relation to this idea.

**Group Structure**

In participating in this form of social organisation the primary objective for these respondents to meet was publication. This seemed to be heightened through their assessment as employees by the university in the REF exercise. Pressures from the REF constrained the groups’ operations in a way which did not always align with the notion that research should be judged by its practical promises or outcomes. Although sympathetic to the general goal of the REF, when questioned about gaps between validity and utility of research the respondents certainly acknowledged that the REF’s goals were not necessarily always compatible with utilitarian goals, particularly within their specialty:

“I think there is some way in which it has to be done, but it is a little bit unfair to expect everyone to publish in high-impact journals, because there is differences in what impact a specialty has...if you are in medical education none of your journals are going to be good in high-impact terms. If you are in internal medicine then what are you going to do? You do not always get into the Annals of Internal Medicine or the New England Journal of Medicine? So if you take Obstetrics and Gynaecology, then the top journal has an impact factor of three or something like that.” (Kumar)
There was some concern that those studies which had most translational impact (in terms of being read by specialists in their own medical specialty) were not necessarily those which were being valued under the REF’s criteria:

“I: But is that the best way to make your research useful- to publish in a high-ranking journal- I think you mentioned the Lancet?

Kumar: No probably not, no, because the British Journal of Obstetrics and Gynaecology, it goes to every single obstetrician free of charge if you are a member of the Society. And people are more likely to read their own specialty journal than read the BMJ. So it may not necessarily be the case.”

Here the pressures to produce papers in ‘excellent’ scientific journals and pressures to produce work which had an important ‘impact’ on clinical practice were in contradiction at the level of their social worlds. The priority of publishing papers was also a goal Quereshi repeatedly reiterated to other participants during team meetings, for example as occurred during a discussion in with Matthews about an external project that Matthews had been pursuing outside of the group i.e. with a team at another university:

“Quereshi: The first thing is that they should lead to publication.

Matthews: Absolutely. Well this one is a HTA project.

Quereshi: The second thing is that if they are eating up a lot of your time then their impact on other work should be assessed. The third thing is that if it takes up a lot of your time then it should somehow be supported by funding. This project seems to meet all those criteria from what you have described.

Matthews: Oh yeah it does.”
This exchange can be read as an example of leadership by the Quereshi, who is setting-out (or re-iterating) this organisational goal and making sure it is being adhered to amongst members of the team (Perrow, 1986, 69). What I interpret from this short exchange is that publication is not just something that individuals should do in order to meet some external organisational goals, but is also being enacted as something that ‘we do’ as a collective: following this institution is projected as a key goal within the organisational storytelling of the participants. Another indication of this from meetings was that congratulations would be expressed when a colleague would announce they had a paper accepted for publication in a ‘good’ scientific journal.

Another agenda to which the group had had to respond was that of the faculty. From the faculty’s perspective, Quereshi was taken as a ‘sound investment’ because of his successful publication record and the promise of further capital accumulation. Another reason was that he was able to package his group’s research as compatible with the T2 type of translational research in which the department was looking to build its reputation as a major player in the general health research arena. In my interview with the Faculty Head, it was made clear that translational research (or the promise) was seen as a credible position for candidates which employers like him were looking to invest. Quereshi and faculty head acknowledged HTA infrastructures enabling research to be translated into guideline making, recommendations and public policymaking in a relatively straightforward fashion put them at an advantage in the REF compared with researchers operating in other fields. What constitutes a translational outcome in this field is also something for which they can receive credit (for instance as named authors on NHS reports or clinical guidelines) in fulfilling the REF’s ‘impact’ criteria. Publishing guidelines can be carried-forward as a form of capital to be reinvested in subsequent episodes of accountability like the REF or curriculum vitae. Thus being ‘translational’ does not detract as an opportunity cost from being ‘excellent’. The problems posed by the REF are thus posed as a facet of
individual behaviour: getting members in the group to meet its targets, rather than an ‘infrastructural’ issue.

How the careers of individual scientists are constructed out of this complex state-of-affairs will now be the topic of further discussion.

**Careers**

In public sciences incentive structures are typically oriented around reputations in intellectual specialty/field and promotion in employment organisation (e.g. the university) (Whitley, 2000). Most respondents at some point in their interviews acknowledged such rewards as one of the *moral purposes* for investing in work (see Perrow, 1986). Hence their stated reasons for publishing went beyond meeting the minimum requirements of organisational needs and into the realm of individual rewards. This section will focus on those respondents identifiable with academic roles, whose job requirements identified the need for them to publish in scholarly journals. It is worth noting that although not falling into this category, Morrison as clinical trials manager and Aziz as an obstetrician expressed ‘hope’ of becoming named authors on journal articles, so as to meet formal and informal assessments of their job performance by employment hierarchies and gain some ‘credit’ for the skill and effort they had invested in the making of the research.

Of the four participants eligible for analysis according to these criteria, one exceptional case emerged in the course of this study. When discussing her attitude towards publishing, Matthews claimed only to be motivated towards satisfying the minimum REF targets expected by the university. As the exceptional case, she expressed an attitude towards this work in more instrumental terms than her colleagues, as ‘just a job’ needed to provide security. She exhibited a general strategy of *compromise* towards the REF in order to retain employment status, consisting of more specific tactics like ‘placating’ and ‘accommodating’ goals of the REF, whilst
‘concealing’ her non-conformity to its guiding norms (Oliver, 1991). This frontstage compliance was coupled with a backstage ‘attack’ on the sources of the institutional pressure and how knowledge was valued within the REF framework (ibid.). The effects of such measures she claimed was that it created incentive structures which prioritised personal gain at the expense of research which could have a patient impact. The achievement of this latter goal was something she herself claimed to be motivated by in choosing a research field:

“...And that’s what we really need to know. Not ‘I’ve done a randomised trial and I’ve published it in the Lancet and isn’t that wonderful’, I’m actually more oriented towards what people really need- what patients really need.” (Matthews)

This could be best achieved through greater professional autonomy by removing the excessive managerial constraints of the REF. As a firm champion of EBM and HTA, Matthews professed that research she and the group did could be ‘truly’ translated into improved patient outcomes, however, the presence of the REF was creating a barrier to the institutionalisation of this translation. This account thus presents the dual goal of making research more ‘translational’ and making the workforce more ‘accountable’ as fundamentally incompatible. The resistance by professionals towards increased management controls will appear unsurprising to those familiar with research on new managerialism (Ferlie et al., 1996). Although not her primary ‘passion’, in her account systematic reviews constituted a reasonable compromise between research which was patient-centred and research for which she could attract funding and thus pass through cycles of credibility, at least on a short-term basis. Matthews’ compliance with this institutional control and continuing participation in the credibility cycle was thus justified as a temporary move before she could ‘escape’ (Oliver, 1991) from the clutches of the REF and carry-out a career in research assessed according to different criteria:

“Well it’s finances really. When I have the money to go part-time I’ll do it. And I’ll do research that I think needs to be done, not what ticks boxes for the hierarchy.”
Her complaints about the REF chimed with recent human relations and sociological commentary on the stresses and strains of work intensification brought about by increasing managerial control of professions, especially in studies of higher education (Ogbonna and Harris, 2004). In an actor-network sense, Matthews was struggling to position herself between goals of the REF and those who would be evaluating her. To avoid this route would mean to have her security undermined. This response differed markedly from the disposition of other researchers interviewed in the group, two of whom, by contrast – the Quereshi and Kumar were clearly invested in the group’s research not just as a means of continuing in employment, but as a strategy for gaining recognition and reward in their intellectual field.

Quereshi, as an eminent scientist with over two hundred publications against his name, had clearly invested more than the ‘bear minimum’ required by employers. Perhaps unsurprisingly he presented a very different disposition towards the demands of the REF in relation to his work requirements and his career ambitions, as a constraint he felt largely comfortable about meeting. The institution of tenure enabled a level of employment security for the professor which the others lacked. He also had the reputation and know-how to continue publishing and meet the REF targets with relative confidence. In his case one of the goals emerging for him was to maximise reputation through publishing in esteemed journals. As the individual in charge of long-term strategic direction of the group, Quereshi role requires he attracts attention of other scientists in his field ‘as a name, a location, a set of topics, and someone to be reckoned with’ (Knorr-Cetina, 1999, 221). He had founded the group and built-up its reputation in the field of EBM, meaning he could attract early career researchers into the group, who learnt to assemble trials and write systematic reviews, thereby bringing further publications and future collaborations to the group. Quereshi had a reputation amongst other respondents as a particularly industrious worker, as in his absence others would joke about the (exaggerated) number of projects he would be working on at any one time. He was also identified by other
respondents as someone continually looking to extend the scale and intensity of the group’s operations, for instance by re-locating to the new (respected) host institution and hiring a full-time clinical trials manager.

Like Quereshi, Kumar expressed attraction towards rewards from publishing in high-ranking journals, for instance, during our interview she described feelings of excitement gained as an early-career researcher on having had articles accepted by high-ranking journals:

“Kumar: I think it’s a good feeling when you get published in the high ranking journals, so if you never publish there you aren’t ever going to get that feeling…

I: So it’s a worthwhile achievement to be published in those top journals?

Kumar: It is, yes because it’s so difficult to do, that if you’ve done it you’ll think it’s worth it, so yeah.”

Kumar’s complicity with strategic ‘rules of the game’ could be read from the account she gave about the decision-making processes that goes into investing in either clinical trials or systematic reviews. Although multi-centre research is being increasingly institutionalised under the HTA program, not all researchers have the capital or motivation to invest in these studies. But for those who can compete successfully in this activity the rewards are clear. This type of study was seen by Kumar as a potentially rewarding area of investment, given demand of external actors like NIHR for these types of studies. From her perspectives then, although multi-centre trials are seen as much more difficult/exhausting and costlier to run than systematic reviews, they are considered more effective in building individual and group reputation and credibility (as well as, strategically, that of their faculty, university and local NHS Trusts):

“To change clinical practice, you do need collaboration in this day and age, you cannot function without it...you cannot do a credible single-centre trial. Unless that is the question can be answered within a single hospital. But think about it, how many
studies looking at women in one centre have been published in the BMJ or Lancet?"

(Kumar)

Systematic reviews were seen by Kumar as easier to assemble, however, the competition for these funding opportunities is seen as strong, meaning the team also had to develop competencies in running clinical trials, particularly in their specialty of obstetrics and gynaecology, as they could not meet and capitalise on organisational expectations by being reliant on funding from systematic reviews alone. Likewise systematic reviews are seen as less ‘sexy’ and therefore marketable towards high-ranking medical journals like the Lancet and New England Journal of Medicine, as well as to the REF, much to the annoyance of a number of the researchers. Deciding on which type of studies to seek funding for is not simply discussed as a matter of short-term planning and organising, but also a matter of strategic investment made by individuals and organisations. In this excerpt the Kumar focuses around the problem of how she as an individual and the research group might best be positioned within future markets for research funding and academic capital when making managerial decisions about investing in clinical trials or systematic reviews:

“I: Out of the reactive, and if you like, proactive research studies, which would you prefer?

Kumar: I think the reactive applications … [laughs]… I think… I’ve just put this one in this month and I am planning to put one in at the end of the month and it is extremely tempting to put in a reactive one because you think you stand a better chance. You do all the work in this area, then the brief calls, then there is the huge temptation to put the application in. But with the reactive one, your competitor will almost always be in the same field.”
As such research methods are not considered just in terms of their technical utility for answering scientific questions. Strategically early-career researchers look to learn skills which will facilitate future credibility conversions. Often this means calculating what is the path of least resistance and following the fashion culture. At the time of fieldwork two respondents were ‘learning the ropes’ with regards how to perform multi-centre trials, albeit at different stages of career development. Kumar had just undertaken the role of ‘Project Coordinator’ on two clinical studies, the first time she had undertaken responsibilities of this kind. At a different stage of early-career research, Pereira the post-doc sat in on the trial meetings in order to ‘get a sense’ for how these type of studies work, signalling that such skills will come in useful in order for him to become established as a professional scientist in this field (indication that multi-centre studies were considered ‘where things are going’). Although not committing his career to this particular group, Pereira described how he had applied for this temporary job in the hope of learning a new set of skills which would make him more ‘marketable’ as an employee in his home country; skills which he could re-invest in future credibility cycles.

Respondents thus did not invest in clinical trials exclusively on the basis that they will deliver best patient outcomes or constitute the best spending of public funds. What I observed here was that clinical trials were being invested in as (hopeful) means of gaining professional and employment status. I would make the tentative point that researchers are likely to invest in those sets of problems for which there is perceived demand and for which they can re-apply some of their existing skills and resources.

Despite normative statements to the contrary, it became clear across the course of the study that it is not necessary for the respondents to align every decision at the level of experimental and laboratory work with the pressures for them to produce ‘translational research’. Indeed this appears compatible with Fujimura’s argument that scientists will routinely ‘respond to the demands and concerns of several audiences’ (1987, 265), sometimes even in contradictory
fashion. Quereshi and the faculty head were those charged with aligning the local research practices of the group and the host institution (respectively) with broader patterns in the research environment. Outside of the ‘managerial’ roles, researchers rarely spoke of translational research itself explicitly in terms of investing in a position in the social world, but more as a normative framework with which they could broadly identify their work. Again this might be due to the relative success and power of the field in which they are positioned. The demands to produce translational research are perhaps just one of the external pressures exerted on them, yet compared to struggles of credibility the need to appear broadly relevant and legitimate appeared rather slight. Perhaps this is due to their ability to ‘piggy-back’ (Latour, 1987) onto state-sponsored NIHR-HTA programming and its associated infrastructure (see Rip, 1994, 11). For scientists in this case, who had already carved out reputations in HTA, translational research constitutes a re-branding of what they had been doing already and therefore was not depicted as high-risk. Indeed, it was barely even spoken of as an ‘investment’ by participants, perhaps because it was already internalised in their practices.

Chapter Conclusion

This chapter has reported the findings of a case study in this thesis. It was organised around applying Latour and Woolgar’s (1986) four concepts for studying mundane work activities of scientists to the primary empirical data collected on this case. Having set-out the findings in this manner, I will now delineate the key empirical and theoretical findings which in conjunction with other case studies will be used to set-out the original contribution of this thesis.

The findings from history of the laboratory framework elucidate how translational research is given meaning in the context of this group of applied health researchers. It also allows one to make the link tentatively to organisation studies concerned with how ideas spread in contemporary organisations exposed to management fads and messages, where people have the propensity pick-up and use ideas without necessarily being aware of their provenance or
original meanings (Clark, 2001). One of the puzzling factors of this case was that they had appropriated this label without being under any explicit calls to change the name of what they were doing. In all likelihood the name added normative justification to their existing practices (Meyer and Rowan, 1977). Although the interpretation that respondents were mostly doing the same things as before but under a catchier name may sound cynical, it belies the simple fact that ‘names travel with ease and with great speed’ (Czaniawska and Sevon, 2005, 10) and that in open systems organisations tend to mimic one another (DiMaggio & Powell, 1983).

Analysing the respondents’ activities in terms of acts of inscription reveals that the pressure of translational research does not require a significant extension of their persuasive skills to accommodate relevance in the context of writing journal articles. However, the framework’s application does suggest that those engaging in large-scale ‘translational’ projects like multi-centre trials must extend their persuasive skills in order to enrol the workforce necessary to perform parts of these studies (e.g. data collection). In such interactions as presentations to CLRN, the need to assure audiences of the relevance of their work is brought into view. This section has explored some of the forms of persuasion mobilised by participants in the course of producing facts from multi-centre trial studies. It appeared that in terms of interaction with the immediate social world of other scientists, the promissory language of translational research was not mobilised, with discourse of scientific journal articles reserved for more traditional forms of ‘passive’ rhetoric. However, ethnographic fieldwork produced traces regarding the tremendous effort and cost that goes into producing such inscriptions in the context of production work of the multi-centre trials. This type of project appears to require a shift in the coordination and organisation of inscriptions away from a single production site (in laboratory studies) towards a ‘bioclinical collective’ (Rabeharisoa and Bourret, 2009) model where researchers bid for outsourced labour to carry-out important inscription work (e.g. filling-out questionnaires) at the level of data production. To enrol this outsourced labour requires both a persuasive currency
and infrastructure to be enacted. The translational promise of their research was a rhetorical resource mobilised to market their projects to prospective allies needed to produce inscriptions from experiments which could be returned to their centre of calculation via immutable mobiles (e.g. questionnaires) (Latour, 1987). This appears to require an extension of the persuasive skills of researchers investing in these types of collaborative studies, that was not accounted for in earlier laboratory studies of single-site ‘factories’ (Knorr-Cetina, 1981, Latour and Woolgar, 1986). Although these respondents still appeared to be in the business of persuading and being persuaded by others in the production of inscriptions, the geography of this production appeared much more diffuse than in earlier laboratory studies. Although Latour and Woolgar’s account of scientific production processes stated that skill and effort is needed to coordinate production and mobilisation of inscriptions, the cost of doing so appeared less constraining in their disciplinary laboratory than it did in this form of multi-sited ‘big science’ production context. I would like to emphasise here just how difficult and expensive it was to produce inscriptions even at the mundane level of running ‘experiments’, given the need to coordinate multiple forms of organisation, regulation, and paradigm. Nonetheless, Latour and Woolgar’s notion of scientific facts (and clinical ‘evidence’) as inscriptions is helpful for retracing the practised skills and efforts which go into producing and mobilising such black-boxes.

Considered through the *microprocessing of facts* lens, the data suggested that the need to produce fact-like statements which draw consensus is vital in order for the heterogeneous assemblages involved in multi-centre trials to be able to collaborate. Negotiation was accomplished in the course of co-present exchanges through reference to common goals and boundary objects, such as methods and official standards. Not all of those present during the trial steering committee meetings held expert knowledge about certain aspects of the trial protocol document. During exchanges different participants would emerge as credible ‘spokespersons’ on behalf of statements in said document. However, this positioning was not
always accepted, in which case participants would deploy various micro-strategies in order to close controversies, such as reference towards experiences of a spokesperson, arranging another meeting, or ‘pulling rank’.

Finally the cycles of credit framework enables one to draw inferences about the construction of individual scientists and group structures in relation to the work they perform. The findings in this chapter show that in order for translational research to ‘work’, it must not interrupt the flow of credit conversions taking place in the context of mundane work activities. In this case, as only the name travelled, it posed no such problem. Likewise the idea must be consonant with longer-term investments made by individuals and groups, such the positions they wish to occupy in their field in order to build reputation and career. Rather than completely upending existing work activities, pressures to consider translational research become yet another aspect of the complexities of which these respondents have to make sense. How meaning is attached to this and other pressures like publication thus varied considerably across respondents. In this case, the idea affected an innovative mode of organising principally in terms of its form rather than content. This might feasibly lead one to predict that translational research may travel more seamlessly into contexts where it reaffirms existing practices and potentially improves consistency of operations, than those contexts where it requires upheaval of routines. Put simply, translational research represented for these researchers the most viable forms of research to do for disciplinary and economic reasons, and was synonymous with following the fashion culture of their field. However, within the group how respondents experienced and engaged with this collective path varied considerably. Some were content to play an instrumental role in fulfilling the aims of some other agent (e.g. HTA), rather than operate as autonomous researchers fulfilling their own intellectual curiosity, because it suited their own interests for security, career advancement and reputation. This is an example of ‘equivalence’ (Callon, 1995). One anomalous respondent however was disenchanted with the state-of-affairs
in which she felt to be the means to someone else’s ends. She appeared to identify much more strongly with the rhetoric and norms of the Humboldtian university than with the kind of neo-liberal, individualistic games in which she found herself caught-up. Although then the goal of translation was something with which she strongly identified, she denounced managerial performance measures and pressures exerted on academics for being counter-productive to this goal. This respondents’ account appears to exemplify the types of ‘culture clash’ between an ‘old’ logic of ‘academic science’ and a ‘new’ logic of imposing managerial devices from a distance, translational research being associated with the latter. Based on my readings of scholarly literature on academic’s responses towards re-contextualised science (Webster, 1994, Cooper, 2009), what was perhaps surprising here was not to have found more people who espoused this kind of sceptical and resistant attitude.
9. Thesis Conclusion

Introduction

This chapter starts with a summary of the original aims of the research, approach and design that have been taken. Attention then shifts towards an in-depth discussion of the main empirical findings from the thesis; its main theoretical contribution; the strengths and limitations of the frameworks used in the thesis; policy contribution of the work. The chapter then finishes with a series of statements about how sociological studies of translational research should be carried forward and what themes this piece of research contributes towards this topic.

Summary of Research

The research in this thesis explores how an abstracted, ‘flattened’ framework for science and innovation– translational research’ is transformed amongst the local practices of mediators its champions are seeking to conquer: academic researchers working in medical research institutes. The field of STS was drawn-on as it provides a compelling set of tools for exploring what it is like for scientists to work with this idea. Although the field of STS lacks definitive consensus about which concepts, case studies, or methods constitute the best way to go about the task of subverting translational research, most of the concepts appropriated here were nonetheless identifiable and well-known within STS. Two such commitments will now be mentioned briefly.

One was to treat claims respondents gave about translational research the same and not to distinguish between those deemed ‘good/bad’, ‘true/false’, or ‘successful/unsuccessful’. In a methodological relativist turn, the thesis considered how versions have been constructed in the contexts of each case, rather than searching to uncover a ‘definitive’ version(s) of translational research (Bloor, 1976). This selection was made for practical purposes as much as for
philosophical ones: given the variation and at times sheer confusion caused by different versions of translational research, it seemed to proceed with a closed, definitive version of what the phenomenon ‘is’ would be less productive. This symmetrical strategy was coupled with the theoretical selection not to pursue problems of generalisability in relation to translational research. Rather the approach followed was one developed in later ANT studies, which explores objects in terms of their multiple, situated ontologies (Mol, 2002). This shift towards transportability leads to a very different version of materiality than would be the case if one pursued a generalised explanatory theory of translational research (Latour, 2005). This is because the endurance of the idea is assumed to be contingent on its transformations and stabilisations amongst localised practices, rather than because of homogenous properties of the idea itself, or its inner glory. With such sensibilities in mind, the research questions were as follows:

How does the idea of translational research transport into local academic knowledge production sites of academic medicine which policy authors are seeking to enrol?

What aspects of the local production practices are adapted to accommodate this goal and where do resistances occur?

In short the thesis is about how translational research is performed and ‘worked out’ in real-world knowledge producing settings.

The empirical chapters were structured around three separate case studies of research groups hosted by medical schools in English universities. The first empirical chapter studied a group in a cancer research centre interested in cell signalling, the second chapter was of a diabetes research group, and the third chapter a group engaged in EBM-based forms of obstetric research.
The primary method used to generate data was qualitative interviewing, supported by documentary analysis. One case study (Obstetrics) also granted access for observation of meetings over an extensive period. In writing each case study the objective was to capture how translational research has transformed (or not) the mundane knowledge producing practices of researchers in these settings. ‘Frameworks’ deriving from Latour and Woolgar’s (1986) seminal text were appropriated in order to capture various dimensions of these ‘mundane’ practices in the data. These frameworks were supported where appropriate by sensitising concepts and findings from other studies in the STS canon, particularly other laboratory studies and later ANT writings.

Data from each study was transcribed separately onto the Nvivo software program and coded using Ritchie and Spencer’s (1994) ‘Framework’ for coding and analysing qualitative data. With the aid of this program data pertaining to each of the frameworks could be easily delineated, which was helpful in writing-up each of the empirical chapters. Each chapter focussed on the localised dynamics of translational research in respect to each of these frameworks, followed by a closing summary. The implications of this research will now be further articulated.

**Review of Key Empirical Findings**

The name translational research was read as a label well known amongst respondents in each of the cases studied in this thesis and arguably more widely across arenas of contemporary health research. Yet whether the practices captured under this idea also travel and transforms at locales of knowledge production was found to be affected by all sorts of institutional factors. Translational research marks another effort to redefine the science-society contract, which rests on the dilemma of how to harness and institutionalise innovation which science promises. It was said to me by respondents in my pilot studies that translational research was simply a case of scientists re-labelling what they already did in order to meet an agenda. But do the labels have some effect (e.g. from a governance level) in terms of priorities and ways research is organised?
The findings from my study would suggest they do. Furthermore, even if the transformation of local research practices only extends to the displacement of a name, this still signals some novelty in terms of how ideas are extended via networks into new sites. If the inventiveness of this device is approached in terms of a novel arrangement alongside other situated activities and devices (Barry, 2001, 211-212) then for each of these cases translational research marks an innovative mode of organising, albeit in different ways and to a greater or lesser degree. The thesis provides support to the theoretical assertion that in contemporary organisations sometimes names travel, sometimes names and practices travel, and sometimes only practices travel (Czaniawska and Sevon, 2005, 10). The practices implied by translational research were already institutionalised within the organising of the obstetrics group; therefore the cost of engaging in ‘translational research’ as opposed to say ‘applied research’ was minimal: it is just the name which becomes displaced. For the other two cases, the advent of translational research implies certain modes of practices being transformed as well as simply the name of what it was they were doing.

This variation in what has travelled will now be further explicated by comparing how aspects of the local mundane practices (history of the lab, inscription, cycles of credit, microprocessing) were transformed in relating to the problem of translational research across each case.

**History of the Laboratory**

The approach used in each chapter was to consider translational research as relating to multiple situated realities which were being enacted and performed (Mol, 2002). Each empirical chapter therefore began with an attempt to map-out the various definitions ascribed to this term within the context of each group’s working practices. The movement through space and endurance across time of an ‘idea’ is contingent on its reception amongst different hands needed for it to spread (Latour, 1987, 138). Methodologically one can study the materiality of an idea by looking at its changing shape as it is tied into different people, by exploring how it gets transformed into
their local operations (if at all) (ibid.). Conducting case studies of its transformation within
different organisational contexts is a logical and appealing way of performing this task. How then
was translational research transformed from an abstract idea in the context of each case? How
can one account for variations in results in terms of differing networks of practice? Stabilised
definitions given to translational research in each case therefore varied according to differing
configurations of institutions shaped by the networks in which the researchers could be located.
For instance, the varying *epistemic machineries* (Knorr-Cetina, 1999) in which their practices
were embedded delimited the kinds of translational work towards to which they could lay claim.
Certain actions would not befit the institutionalised order told through narratives about their
forms of science and behaviour appropriate of its practitioners (Czarniawska, 1997, 43). The
articulation of boundary work in the respondents’ account therefore varied across cases and
their existing institutional contexts. These findings will now be discussed further.

One notable feature of the cancer and diabetes cases was the lack of self-evident production
processes and strongly-convergent networks of practice to search for and scale-up their
products into novel interventions. The fragility and notoriously high attrition rates associated
with the extended networks through which their products had to pass in order to deliver market
or clinical pay-offs, led the respondents to articulate more workable boundaries regarding their
involvement in these processes. Questions about how they defined translational research in
relation to their work provoked difficult boundary issues with regards their appropriate role as
scientists versus engineers. In answering such questions they have come to negotiate several
institutionally relevant and legitimate practices such as consulting and collaborating in
partnerships with commercial companies, and licensing and commercialising intellectual
property. Likewise, articulating ‘disease-oriented’ or ‘clinically relevant’ problem choices in their
basic research activities enabled the groups to mobilise promises about potential utility of their
work, thereby contributing to translational research without necessarily having to perform
additional ‘engineering’ work themselves. This strategy resembles Latour’s type II and III transformations. In type II, the scientists get interested parties to shift their initial, hasty interests, and instead follow those laid-out by the scientists (1987, 111). As achieving such ‘explicit displacement of interests’ is rare, the scientists deploy another transformation (III) strategy, by stating to the interested parties: ‘You cannot reach your goals straight away, but if you come my way, you would reach it faster, it would be a short cut’ (ibid). In these contexts, the lack of established production and search processes and shifting, weakly-convergent networks led to difficulty in defining the boundaries of their involvement in translational research. This provides a compelling explanation of why the respondents traced-out a rather fragmented list of ways in which they could contribute to this general goal.

By contrast, in the obstetrics case, availability of well-known inscription devices (notably clinical trials) and strongly-convergent networks of practice for scaling-up its products made their local transformation of translational research reasonably self-evident. The institutionalisation of the HTA amongst the ‘state’s regulatory apparatus in relation to public health’ (Webster, 2007) provides a ready-made conduit for scaling-up and implementing the researchers’ products into routine clinical practice. Across these extended networks, regulation could be said to have facilitated (rather than burdened) ‘the production, circulation, and interchangeability of novel entities and practices’ (Keating and Cambrosio, 2003, 332). The focus of NIHR on prioritising their type of multi-centre clinical trials also meant there was additional support in place which the group had begun to successfully exploit. For example, at the time of fieldwork, with the formal backing of the NIHR, the group were in the process of setting-up a ‘sales network’ (Pinch, 2008) called CLRN to support the production and promotion of their clinical trials in the NHS. Having such equivalence with extended networks of HTA meant they were thus able to ‘piggy-back’ onto a single, much stronger ally than themselves (Latour’s type 1 transformation, see 1987, 110).
Of course there would still be practical obstacles to their translation, however, the presence of this ally provoked respondents into arguing their products provided a much more plausible script for action in relation to the problem of translational research than does more upstream experimental work in basic science. Unlike the other two cases, the obstetrics researchers were producing claims (‘evidence’) about the safety and efficacy of new or existing interventions, rather than championing the clinical potential of given interventions per se. Their scientific and regulatory field of HTA-EBM is disciplined around an ‘institution of objectivity’ (Cambrosio et al., 2006b). This made striking a balance between problems of representing and intervening, science and engineering, much less problematic than was the case for researchers in the other two case studies. They were behaving as scientists in making truth-claims (‘facts’) via standard inscription devices (clinical trials, systematic reviews). Simultaneously, in producing facts they were also providing ‘evidence’ which, because of modern regulatory institutions in healthcare, is indispensable in making interventions translate into clinical practice. They were therefore also acting as engineers, in helping to test whether translations could be made workable. The label translational research therefore did not perturb the boundaries of what they were said to be doing to anywhere near the same degrees as for researchers in the cancer and diabetes cases.

Translational research was defined by the researchers through processes of ‘analogical reasoning’ (Knorr-Cetina, 1981). But despite the flexibility and mobility of this label-metaphor, it is not infinitely malleable: instead respondents’ versions of this problem corresponded to local situations and accompanying institutions in which they located their actions. Respondents in each case defined translational research according to epistemic machineries with which they identified (and the mobility of the inscriptions they could produce), the types of allies with whom they could associate themselves, and the relative ease with which they could render invisible the subsequent detours to be taken by external allies in order to scale-up their products.
into clinical/market settings. Hence the objects into which this ‘idea’ gets associated can be rendered visible within the contexts of networks traced by respondents (Law and Singleton, 2005). The ‘translational’ success of such objects is very much contingent not only on the formation of such networks, but on subsequent work these associations perform (Czarniawska, 2004). Further issues in working with these associations will now be considered with reference to Latour and Woolgar’s three remaining frameworks.

Inscriptions

For Latour and Woolgar (1986) the products of the laboratory (facts/objects) are made to travel and circulate via various forms of writing. The ‘inscriptions’ sections of the case studies explored whether Latour and Woolgar’s depiction of the content of laboratories’ written outputs as being characterised exclusively by technical discourse and claims to facticity held-up. This question emerged because certain respondents associated translational research as now being de rigueur in respect to institutions of writing in science like journal articles and grant applications. This claim appeared significant in that, if true, it would suggest that in order for their products to travel through forms of inscription, scientists today must also explicate their potential to be translated into practical outcomes.

Primary analysis of the textual outputs produced by respondents in each case study only partially supported this hypothesis. Drawing on STS theorising, I will now try to make further sense of these results within and across the cases. The obstetrics group apply to one centralised sponsor, whereas others apply to more fragmented sets of sponsors. Hence the former simply have to ‘whistle the tune’ of a single pay-master, whereas others, so to speak, must extend their repertoires. The findings show obstetrics group as practising Latour’s (1987) type I transformation, where they can ‘piggy-back’ on a stronger ally (HTA) whose translational remit is broadly established and pre-specified (e.g. in the form of multi-centre trials and systematic reviews) and which has an ready-made infrastructure in place to scale-up the researchers’
findings. The group simply have to deliver a product which meet these criteria, rather than promise further proof-of-principal studies. One effect seems to be they do not have to justify anew the means by which the project will scale-up into clinical practice each time they apply for a clinical trial grant or submit a journal paper. This is a feature of operating within a field whose relevance is considered indispensable to a powerful ally, namely a state-supported earmarked programming mission: NIHR-HTA (following Rip’s ‘cycles of relevance’ model, see Figure 5 in Chapter 3).

Conversely as the work of the cancer and diabetes cases must take ‘detours’ (via lengthy periods of basic research) before its clinical pay-offs can materialise, the sponsors seek promises and reassurances from researchers that funding will indeed actually help them to reach their goals (Latour’s type II transformation). Furthermore, compared to the obstetrics case the other two groups lack institutionalised and strongly convergent networks of allies like the HTA (which in theory have enlisted and drilled a bureaucratic organisation like the NHS in a relatively standardised way). A more ‘natural’ ally for the cancer and diabetes cases is the pharmaceutical industry. This industry has its own interests, making it harder for the researchers to enlist support and render their product indispensable to its companies. Wary of potential to be perceived as lacking in relevance and legitimacy, elite members of the field (e.g. journal editors) introduce as obligatory practice in writing articles the need for inclusion of promissory statements about potential utility of findings. But findings here would suggest this ‘hinterland’ (Law, 2004) strategy appears to emerge at the discretion of editors and is not ubiquitous to all titles analysed in the different case studies.

The claims of certain informed respondents in cancer and diabetes cases was that reference to translational research constituted more of an obligatory passage point over recent times. The extent to which the inclusion of statements providing plausible scripts for practical action constitutes a new phenomenon in science remains doubtful. For example Knorr-Cetina’s (1981)
study of applied scientists’ strategies of literary reasoning in scientific journals drew attention to the inclusion of such statements in the introduction sections of articles. Although this was based only on a single case study, her findings would suggest this literary strategy has been a feature of applied sciences for some time. Indeed, as with ‘plausible scripts for action’ Knorr-Cetina identified, those forward-looking statements found in texts of my respondents were written in a way as to highlight only *possibilities* as opposed to practicalities of scaling-up their products. It may be plausible for instance that rather than constituting something qualitatively new, translational research has simply displaced other labels for describing this aspect of writing practice (such as ‘strategic science’). Also plausible is the notion that the forms of literary reasoning associated with Knorr-Cetina’s applied scientists has become more commonplace amongst more upstream modes of (techno)science, given the blurring of demarcations between science and engineering and the reported advent of changes in wider knowledge production regimes (Nowotny et al., 2001, Hessels and van Lente, 2008). This is a claim which deserves to be followed-up through further study. For now, I would simply add that although respondents recognised this intensification as an innovative aspect of the writing practices in cancer and diabetes cases, it was not necessarily one towards which they had resisted or struggled to adapt.

An additional empirical finding was that on their own inscription devices cannot be transformed into scripts for action without additional embodied efforts and skills of scientists and those with whom they seek to enrol. Notably in getting texts like grant applications and journal articles to enrol (potential) allies, scientists also had to attend and/or organise meetings with the allies. In the diabetes case study, submitting grant applications would not be sufficient in order to access funding, but was supported through an interview process. Likewise, before commercial companies invested in the products of the cancer laboratory, the PI and senior lecturer were required to present their work in person and sometimes undergo what they felt lengthy cross-examination processes. In the obstetrics study, it became apparent that the process of moving
from one form of inscription to another was hard-fought and costly. In particular in clinical trials moving from a device like a questionnaire towards composing a full data set on the relevant population required all sorts of mediation work on the part of hospital staff, which in a geographically distributed production process like MCTs was not always inevitably forthcoming. Holding face-to-face meetings was one tactic by which the scientists and their formal allies sought to enlist the support of said group. Common themes amongst each of these stakeholders were apathy and/or risks they attributed towards working on the products with which they were presented by the scientists. Hence although ‘translational research’ appear to be very much dependent on a range of inscription practices, these practices must be supported by additional embodied efforts, which, ultimately may fail to materialise on a case-by-case basis. This finding suggests that in these ‘translational’ contexts inscriptions were less effective at producing and circulating new objects than was the case for those scientists engaging in disciplinary networks of practice depicted in Latour and Woolgar’s study27. I suspect this was what one respondent was intimating when describing translational research as requiring lots of additional ‘dirty work’.

Cycles of Credit

If scientists subscribe to the ‘logic of opportunism’ (Knorr-Cetina, 1981), it follows that translational research must have practical use for researchers in order to be taken-up. The economic concept of cost is a helpful means of expressing levels of commitment, transformation, and resistances calculated by respondents in each case.

Cost calculations were considered in terms of the risk to existing operations. In the cancer and diabetes cases as only certain senior figures had the capital and know-how required to align translational research to other levels of work organisation, they became even more indispensable to the operations of the group. It appears intuitively convincing to state that

27The importance of place in coordinating exchanges will be discussed further in the theoretical review later in the chapter.
because technicians, post-doctoral researchers, and PhD students are hired by the PI in order to fulfil particular roles in the group’s production process (at the ‘experimental level’ of work organisation, see Fujimura, 1987), the leaders would not want them to take on responsibilities in relation to this problem. ‘Experimentalist’ members were less indispensable in relation to translational research, typically acting under the scripts authored by senior figures in the group. It is the senior figures who are knowledgeable and accountable for cost calculations. Hence the interaction of translational research with the groups’ traditional forms of hierarchical organisation did not antagonise and create a particularly strong source of resistance. For obstetrics researchers the problem of translational research could be addressed simply through cycles of credit and funding conversions. The principle disciplinary measure for this organisational motif was publication and acquiring research funds. This disciplining was formalised around the organisational device of the REF and imposed on others by the group lead.

Laboratory studies of scientists at work showed how like workers in other organisations, scientists worry about their futures. Projections about cost also entered as an important consideration with regards calculations about future career actions. As an ‘investment’ translational research was related towards scientific careers of the group and individuals in different ways, varying according to stage of career development, levels of capital accumulated to date, and interpretation of opportunities within their respective agonistic fields. The reputation of the group and its lead scientists was seen as significant in determining opportunities to compete in their adopted areas of translational research both hitherto and in future. For the diabetes group, translational research was seen as a struggle, but one which they needed to harness and exploit. The problems of strong competitors and lack of reputation for bringing in grant money beyond preliminary studies had the potential to make the terrain too treacherous for them to travel in. The aim for the group was to negotiate and eventually master
the ‘terrain’ through extensions of actor-networks, in order to make more frequent and rapid journeys across this ‘terrain’, therefore benefitting their future ‘empire’. In short, they had to overcome and tame initial resistances. For the cancer case study, prior successes in feeding drug development and instrumentation had brought a reputation to the laboratory and its leader for working on translational research. By associating themselves with the laboratory, post-doctoral researchers could enhance their own reputations as commodities in employment markets for scientific researchers in academia and industry. For the PI, the professed strategy was to balance the need to retain the reputation of the laboratory in this regard, so as to continue rapid movement through cycles of credit conversions and meet external agendas, whilst retaining their focus as a basic science laboratory whose main product was ‘Science/Nature papers’. Further expansion into areas of translational research could stretch resources and result in loss of control. As such, translational research marks one means by which the laboratory can attract funds and reputations, but does not replace sources associated with institutions of basic science. In the obstetrics case study the work of the group was very much aligned already with their definition of translational research. The focus for the group lead therefore was to consolidate and expand further his group’s reputation in the field of EBM-based obstetric research, particularly through conducting large MCTs. For others, such as the senior clinical lecturer and post-doctoral researcher, individual career ambitions were centred on mastering the design of multi-centre trials and becoming a ‘name’ within this translational territory. For members of this case, translational research did not require them to reconstruct their individual career trajectories, which I suspect is one of the reasons why the adaptation towards ‘translational research’ appeared relatively unproblematic for this group. One anomaly was a senior lecturer that was resistant towards the management of research in universities and the problems this posed for pursuing patient-centred research: formal performance measurement targets like publication and grant money displaced opportunities to orient selections and actions around
pursuit of broader patient-centred problems. As a result the individual questioned their long-term future in this kind of working environment.

Sociologically it is argued that certain individuals and groups stand out not because they are special or because they are somehow fated to do so, but because they have been able to enrol, mobilise, and drill larger numbers of elements, more effectively than others (cf. Latour, 1987). Following ANT logic, observed differences were a matter of greater or lesser capital accumulation and mobilisation of resources. Clearly then how groups of researchers organise their responses and how careers are aligned in relation to this problem is likely to vary according to the location of existing stockpiles of capital and calculations about promises of further successful accumulation. Where the costs for existing operations and credibility are anticipated to be too great, resistances are likely to occur.

**Microprocessing**

STS laboratory study instructs the analyst to treat scientists as following a ‘logic of opportunism’ (Knorr-Cetina, 1981). As the history of the laboratory section indicated, translational research was a means of packaging production processes which involved them working with certain allies. But getting these collaborations to ‘work’ requires hard-fought periods of negotiation. This was illustrated in the obstetrics case through observation of trial steering committee meetings in which social actors from various professional and disciplinary backgrounds met to agree upon the trial protocol document. In formulating this written text it was hoped that they could coordinate the clinical trial from a distance. The period of observation enabled me to capture some of the difficulties in coordinating and negotiating exchanges and the importance of articulating boundary objects in order to do so. Part of the equivalence was based around following international standards for conducting clinical trials and methods of EBM. The importance of coordination could be inferred also through responses given in interviews across the case studies, in which respondents provided detailed examples of how collaborative
processes are able to work. Instruments, theory, experiments were found to be amongst the mundane artefacts which brought them together and facilitated coordination of exchange (Galison, 1999). In the diabetes case certain pieces of ‘big science’ equipment brought their laboratory into close contact with a clinical laboratory in the partnering NHS hospital. In collaborating with clinicians, the basic scientists recognised certain experimental techniques as providing workable data sets which could be understood across their social worlds. Researchers in the cancer case provided a mass spectrometry machine for other academic and industrial laboratories to use and were positioned as expert consultants offering a paid service for facilitating its transition into these extended spaces. Here a locally developed niche product was able to develop and ‘translate’ amongst people with different qualifications.

Literature on translational research has equated the idea with the linear model of innovation. This is argued as being doomed to failure, for example because it fails to accommodate issues of incommensurability between various social worlds needed to make the process of translation possible. Whilst I share this broad perspective, one of the shortcomings of these studies is they render opaque the fact that it is possible to translate in science. Research on the significance of ‘trading zones’ appears to be supported by findings in the case studies. One of the most striking findings which goes against existing emphases in social studies of translational research is that the different participants within these assemblages need not always necessarily talk past one another (what Kuhn (1970) termed ‘incommensurability’). This renders opaque altogether how those from different domains might establish common ground. I propose, borrowing from Galison (1999), that translation process be studied not in terms of Kuhnian paradigm shifts, but as overlapping tectonic plates of ‘interlocution’. Galison argues that history of science demonstrates precedents of different branches converging and talking to each other, although managing such efforts is far from straight-forward and predictable. The importance of place in erecting spaces of interlocution required for translation to occur will now be considered further,
with special reference to how findings in the thesis can contribute to recent theoretical advances on this issue.

**Theoretical Contribution - On the Enduring Significance of Place**

Although STS literature remains sceptical to the notion that linear models will solve problems of how to ‘institutionalise’ innovation, it also shows how in practice innovation works as a recursive process between those with ideas and those who are searching for ideas to develop. It is search processes as much as provision processes which brings associations together and if there is some sort of mutual meeting ground then it is possible translate. But that mutual meeting ground is not self-evident: it has to be constructed. One theoretical current which helps to account for how such networks of practice manage to interact is recent work on ‘emplaced science’ (Henke & Gieryn, 2008). The authors argue that although focus on fluidity by Law, Mol and Callon is useful, it risks diminishing ‘the apparent significance of the specific geographical spaces where the actants pass through or end up’ (2008, 355). They contend that places of science still hold an importance in regards to how science travels (ibid.). The authors then conducted a literature review in order to theorise ‘how place has consequence for scientific knowledge and practices’. The findings from this thesis on the travels of translational research provide useful empirical flesh to support and extend these theoretical bones. I will focus in particular on the importance of scientific sites in terms of reputation and capital accumulation, and in terms of providing places in which ‘trading zones’ (Galison, 1999) can be articulated.

Henke and Gieryn mention in passing that ‘accumulation of people at a place serves as its own magnet’ for other scientists (2008, 357). Certainly this was the case for early-career researchers who recognised the likely importance of translational research in developing their future careers, and the ability as individual scientists to attach themselves to this problem through working in their group and/or host institute, given their existing reputations. But I also found that the articulation of faculties and institutes which drew together the ‘presence’ of hospitals,
charities, clinicians and laboratory scientists were intended by university management figures to serve as a magnet to enlist others in their pursuits of translational research. Speaking to respondents like Deans, it was clear such collaborations were designed to be seen, so as to proclaim the organisation as a ‘hotspot’ for translational research. Building this reputation would be an advantage for the institute and its researchers when pursuing further cycles of credit accumulation and investment. Each of the institutions (medical schools) in which the groups of my case studies were hosted pursued strategies analogous with this kind of model. Hence although translational research often implies a geographically and temporally distributed production process, assembling in the same location some of the different elements needed to drive forward this process was a strategy being pursued in each of the cases and is arguably a wider mimetic trend currently being followed by larger medical schools and institutes in England more generally. This is seen as the way to build the reputation of such institutes and to stand-out from the crowd.

Assembling these different parties was not just a matter of frontstage performance, but also of actually encouraging and facilitating greater degrees of interaction between such social actors. Having these ingredients in place was intended to build and reinforce networks and provide infrastructures so that promising ideas could be capitalised on where they arise. Also, by establishing formal partnerships between university and local hospitals it was hoped that this would enable peripatetic movement of people and flows of information and materials (what Latour, 2005 calls ‘mediators’) back and forth across the two-sites (Henke and Gieryn, 2008, 357). This can be seen as a form of ‘knowledge architecture’ in which the building of material spaces is at once material and social (ibid). To some extent this strategy was also underlined by a somewhat hopeful belief that simply bringing these diverse collectives together would engender feelings of awareness and co-presence, which would ultimately encourage them to do more to be ‘translational’ in their behaviours. Whether such compositions did in fact engender closer
sense of ‘co-presence’ (Goffman, 1967) was debatable, particular amongst junior scientists, several of whom expressed indifference and/or dismissed such efforts as ‘PR’.

In order for translation to occur, participants recognise a need to hold face-to-face meetings. The function of meetings appeared important across the case studies in a number of senses. First they were important in order to engender trust between participants. As success or failure of their enterprises was seen to rest on the ability of allies to carry-out certain actions, meetings constitute an important occasion to establish trust relations. The significance of trust is not lost despite the rise of geographically distributed production processes. This has been argued by Urry who has developed a notion he calls ‘meetingness’. He states:

“Because networked relationships are conducted at-a-distance so encountering, visiting and seeing networks members face-to-face is crucial. Indeed in some contexts the network will be made up of those who are not known to each other except by ‘name’ and hence it is essential to ‘put a face to a name’, as with many professional and business meetings.” (Urry, 2003, 162)

This appeared to resonate with the PI’s story in the cancer case study about drug company’s practices of arranging meetings to ‘suss out’ scientists with whom they were interested in collaborating. Additionally, in the obstetrics case, holding trial steering committee meetings enabled occasions for participants to debate and negotiate the credibility of the trial protocol document and by extension, the credibility of those with whom they were collaborating and the clinical trial itself. Hence the dawn of geographically distributed work production sites does not displace the importance of meeting face-to-face, but appears to reinforce it. Translational research is thus not ‘placeless’ science.

Another function of meeting face-to-face is to exchange tacit knowledge. Forms of knowing not explicated in standardised documents and other formal media could be shared on these
occasions, through talk and gestures rather than simply following printed texts like SOPs and textbooks. The most explicit acknowledgement of this idea came from observing the discussions which led-up to the post-doctoral researcher in the obstetrics case study deciding to attend the TSC meetings. It was assumed that in order to become a competent skilled practitioner eventually in running multi-centre trials, it was necessary to gain direct sensory experience of these meetings. The prevalence of meetings in the other two case studies also supported this assertion, as they were seen as providing occasions in which to ‘troubleshoot’ and ‘learn’ from colleagues and superiors.

A third function of place was to provide a protected space in which aspects of the experiments were allowed to fail. ‘Experiments without borders’ theses therefore received only partial support in these findings. This notion that the boundaries of laboratories as single, largely invisible production sites which have been undermined in recent times was corroborated by the stringent forms of regulation and accountability placed on researchers in this case, in terms of ethical scrutiny and accounting for full economic costing of their research. Likewise, for clinical research projects, particularly MCTs, it could be said that the hospital constituted a new type of laboratory in which experiments took place. But places where people met for ‘troubleshooting’ suggested a *backstage* in which various ideas were considered and tried out before selections were made public. For instance, trial steering committee meetings enabled debates to occur over the best means by which to design and implement a clinical trial in which ideas were suggested but omitted from the final submitted version. The absence of particular audiences from these occasions was conspicuous, as it became clear that, say sponsors, were interested only in formalised textual outputs of the research team and not the processes of ‘laboratory reasoning’ (Knorr-Cetina, 1981). Hence although multi-centre trials constitute geographically dispersed production processes (compared with earlier ethnographic accounts of single laboratory sites), the erection of a frontstage/backstage distinction remains important (Goffman,
To articulate this divide meetings are organised in which some are invited and others are not. On visiting the cancer laboratory it struck me that only members of the organisation were able to access (or even probably locate) this production site. This reinforced what respondents had been saying about translational research: that it was about trying to control at-a-distance. Hence those sponsoring their research did not make them accountable by way of regular spot-check visits into their sites of production. Instead these messages were communicated via forms of media like grant applications and forms of talk such as when applicants attended interviews at sponsors’ offices. In other words, the actual day-to-day performance of the research was not observed directly by the sponsors. The laboratory thus provided a private enclave in which various ideas could be tried-out and finessed before being made public.

My findings added fresh empirical support for each of the theoretical assertions that trust, tacit knowledge, privacy, and reputation are important functions of places in science (as well as perhaps others forms of business and professional activity) (Henke & Gieryn, 2008). But findings in the obstetrics case also led me to infer the importance physical meeting sites have in terms of engendering motivation/excitement for participation in large clinical trial projects: in a vernacular way respondents appeared to subscribe to the notion that co-presence brings about what some in interaction ritual theory call ‘emotional energy’ (Collins, 2004). This aspect relating to the importance of place which has been neglected by recent works on the importance of place in STS (Henke and Gieryn, 2008, Wainwright and Williams, 2008) and mainstream sociology (Gieryn, 2000, Urry, 2003). Inspired by Goffman (1967), research in interaction ritual theory argues that key to understanding and shaping social order and conflict are shared co-present encounters between people. In interaction rituals participants come together (as ‘emotionally charged bodies’), following rules and rituals which ‘not only show respect for sacred objects, but also constitute objects as sacred’ (Collins, 2004, 17). Although human beings generally crave
order from social interactions, the volatile combination of emotions that such episodes sometimes concoct can lead to unanticipated outcomes:

“Interaction rituals begin with emotional ingredients (which may be emotions of all sorts); they intensify emotions into the shared excitement that Durkheim called ‘collective effervescence’; and they produce other sorts of emotions as outcomes (especially moral solidarity, but also some aggressive emotions such as anger).” (Collins 2004, 105)

According to Collins it is the propensity of interaction rituals to reproduce and transform actors’ emotions and motivations that make them such interesting and important parts of sociological analysis. The process of shared emotions being carried-over by individuals from one interaction ritual to another is described as ‘interaction ritual chains’.

This work has been criticised for placing unsubstantiated faith in the primacy of interaction in the social order. I am not interested here in wider theoretical debates, but merely wish to point out that respondents in the obstetrics case were observed as enacting a lay version of this theory. As a tactic for motivating allies, the notion of meeting face-to-face was explicitly supported during team meetings. This belief appeared to be passed on in these encounters as a form of tacit knowledge about how to run clinical trials, which although not codified or formalised in normal science textbooks, was nonetheless earmarked as important for the success of running these kinds of clinical studies. They projected that enthusing NHS trusts and workers to help implement their clinical trials would require the semi-regular organisation of face-to-face meetings and conference-type presentations, alongside emails and newsletters to ‘top-up’ enthusiasm. Similarly I recorded in fieldnotes how during intervals of these meetings, two clinicians whom were the target audience of presentations had echoed the importance of co-present interactions in supporting their continued enthusiasm for such projects. This suggests that further research on place in science and organisational sociology more generally should
consider the importance of meetings in engendering ‘collective effervescence’ towards geographically distributed work production processes. How important such occasions are for marketing and enrolling allies into different types of translational projects is a question worthy of future attention. Interaction ritual theory may be useful in accounting for function of place in terms of its importance for generating enthusiasm amongst potential recruits. This insight has been overlooked in concepts like ‘trading zones’ and was also absent in Henke and Gieryn’s (2008) literature review, but seems something in which future researchers interested in emplaced science should take more of an interest.

Concepts like actor-network and trading-zone are useful for studying how scientists go about constructing the spaces necessary for translation. But then of course there is the question about the actual appropriability of new ideas: whether developers and users then have the capacities (in all sorts of sociological senses) to appropriate, use, exploit ideas is likely to have a significant influence on the ability to translate. This was indicated in the history of the laboratory section with reference to the diverse actor-networks on which the different cases were dependent for ‘scaling-up’ their ideas and the varying ‘transformation’ strategies available in getting them to do so (Latour, 1987). Although places appear indispensable in order for translation to occur, in and of itself constituting them anew does not guarantee translation.

**Critically Considering the Framework**

So far the chapter has attempted to assemble a strong case for using ANT and related STS resources for studying translational research. However, it would be unsatisfying to take the framework adopted here as a straightforward heuristic to be applied onto this target concept. Therefore in the spirit of STS I would now like to consider the approach used in this thesis as a topic and not just resource of inquiry. The theoretical utility of Latour and Woolgar’s frameworks and STS laboratory studies more generally can be understood in terms of the deductive inferences it allowed for in the context of the analysis. These were rehearsed above.
However, there were also some inductive inferences made during the analysis which expose certain issues and shortcomings in applying the original model to the context of this case study.

The appropriation of the *laboratory over historical periods* framework stands out as a useful means of sensitising the analysis of an otherwise promiscuous and unruly idea like translational research in the context of participants’ local research practices. The transformation of this idea into objects and associated practices which are made visible within network of relations made it possible to locate this ‘idea’ in time and space. In an effort to retrieve an abstracted idea from mobilisation processes, ‘translational research’ was studied as an immutable mobile and boundary object, albeit not necessarily an entirely successful one, which gets variously embedded into the local practices of each group. As these practices are always local, one should refrain from starting with a ‘real meaning’ of translational research, but rather take its definition as an empirical question.

*Cycles of credit* has also been deduced as a useful model in explaining the behaviour of respondents, who were observed as being persistently concerned with converting forms of credit. Yet the model, as originally conceived was ill-equipped to capture problems of relevance and legitimacy scientists in my study faced in respect to certain institutions. To temper this emergent shortcoming, I followed a version of the credibility cycle which accommodated these emergent struggles (Rip, 1994). But even working with this ‘improved’ framework, I have reached certain conclusions. In the case of the obstetrics group, the extension of the credibility cycle to accommodate struggles for *relevance* appeared less constraining than in other case studies, possibly due to the institutionalisation of HTA within wider apparatus of public health. Likewise in the context of the cancer case, the credibility cycle of respondents had not expanded to incorporate struggles for legitimacy, which could plausibly be explained by the researchers’ ability to associate their basis research activities with a more powerful and legitimate set of allies: cancer research. Thus the extent to which Rip’s model provides an apt model for framing
the situation of scientists should not be pre-determined, but considered over the course of inquiry. Rip does however provide a better working model than Latour and Woolgar’s original.

A further criticism of the credibility cycle (and by extension laboratory studies) is that it is not particularly adept at capturing field-level and discipline derived influences on scientists, such as the funding landscape of particular scientific fields (e.g. Slaughter & Rhodes, 2004, 78). This (plausible) criticism appears to have been corroborated by my experience of observing and interviewing scientists, who although providing numerous useful traces, through which their networks were organised, only occasionally pointed me towards this particular type of institutional constraint. Therefore, future analyses could benefit from following and developing versions of the credibility cycle model that have attempted to accommodate ‘cosmopolitan’ level concerns (e.g. Rip, 1997, Hessels et al., 2009).

Laboratories are atypical forms of organisational units which can often ‘stock’ certain elements for periods of time in order to utilise them when appropriate (Knorr-Cetina, 1999, 27). Conversely most modern organisations are thought of as having to respond to pressures in their environments almost at the moments they occur (Czarniawska, 2004, 777). This posed certain questions of Latour and Woolgar’s account of scientists’ ability to mobilise various forms of inscription. The need to extend powers of persuasion to enrol ‘technicians’ to run their trials suggests respondents in the obstetrics case must organise in a manner more akin to Czarniawska’s ‘modern organisations’, for instance by engaging with a fluctuating market rather than organised hierarchy of labour. In the context of this case it was found that respondents had to deploy persuasive skills in a manner beyond their counterparts in Latour and Woolgar’s original study: the hospital might even be described as an extended laboratory (Löwy, 1996, Mol, 2006). The ‘technicians’ themselves are now an important audience whom the scientists must seek to enrol through persuasive presentations/demonstrations and media like emails and
newsletters. The translational promise of the research appeared as an important rhetorical resource in alliance-building.

Methodologically the practical utility of Latour and Woolgar’s original description of the organisation of laboratories through *inscription* was also brought into doubt in the course of this case study. Whereas the laboratory constitutes a kind of production line for the creation of facts through literary inscription, many such inscriptions appeared to be performed ‘off-site’ in the context of the obstetrics case. At no time was I able to observe, for instance, occasions when researchers sat down to read journal articles, or at their computers to write-up their articles. At weekly team meetings, participants would simply provide a verbal summary of their progress on writing particular papers in respect to completion. This part of the writing act occurred elsewhere, in their offices or on personal home computers. This was also a problem in applying the *microprocessing of facts* framework, as the various routine exchanges Latour and Woolgar were able to observe in a single site, here occurred in geographically dispersed locales, such as hospital wards, and across extended periods of time (up to three years). For the observer this poses a range of practical questions about what to observe and where. In this section, what was observed was one phase of the process of fact production taking place in a centre of calculation (the trial steering committee) rather than the locales (hospital wards). Some have suggested that geographically distributed forms of technoscientific production might better be studied via multi-sited ethnographies (Hine, 2007), yet the efficacy of this in regard to processes as extended and fragmented as clinical trials needs to be explored further.

In addition to the methodological issues posed in relation to applying the *microprocessing* framework in its original form, there were also some empirical discontinuities in the conclusions the data allowed me to reach. Applying this framework has provoked a more substantial set of issues with regards the importance of place in performing translational research and the acknowledgement of this by existing literature, which will be addressed in the next section. For
now, reporting on observations made of the obstetrics case study’s trial steering committees, when controversies emerged in interactions regarding the validity of long-established facts, protracted debates ensued (contra Latour and Woolgar’s findings), not all of which were settled fully there and then. From this I have posited the tentative conclusion that shift from observing exchanges in disciplinary, ’laboratory’ settings to those multi-disciplinary ’hybrid’ settings will likely reveal greater prevalence of controversies regarding long-established facts, given asymmetries of expertise within these assemblages. Possible topics for further research include what kinds of closure mechanisms appear most prevalent when controversies break-out in such contexts. Furthermore, how new controversial facts are negotiated by hybrid collectives is a topic which may be facilitated by more extensive ethnographic fieldwork in sites where these hybrids meet. Again it is not clear the extent to which this question was difficult to answer here due to the practical limitations of the fieldwork, or because new controversial facts are simply less likely to occur in these multi-disciplinary situations. It is also instructive to note for purposes of future studies how the lack of data pertaining to Type II and III statements resulted from only observing one type of meeting in a centre of calculation as opposed to more extensive exposure to other, perhaps more peripheral, places in the knowledge production context.

One of the general appeals of ANT, in terms of its utility in answering the questions about mobility and durability of translational research was in helping to capture how people attribute different types of characteristics, parameters, dimensions towards actants that are involved in their production processes, which they regarded as being more or less workable with. This helps one to retrieve the practice from translational research which was rendered invisible in the discourse I had encountered amongst public statements of scientists and strategic policy documents. But in adopting some of the motifs of ANT, one of the challenges of the thesis methodologically was to try to identify with precision the boundaries of actants when those boundaries themselves are being constantly redefined all the time by different agents and
people. For example, translational research was not some sort of durable object in and of itself, but had variability to it. At this point one has to concede humility and acknowledge that any orders and stabilisations described from observations in the context of each case are more-or-less reversible. A lack of invariable reference points (accounts) therefore means that a single definitive version of the actants is not possible, instead one must settle for historically specific accounts on which to base findings and conclusions. However, as these accounts were generated through ‘following the actors around’, the lack of a definitive version of translational research is not necessarily a shortcoming of this thesis, but a consequence of respondents having communicated the sheer messiness of the realities they face in their mundane practices. Providing a definitive, generalisable version of translational research therefore might be professionally or politically expedient, yet it would surely provide an inauthentic version of what it is like to work with translational research.

In sum the sensitising concepts selected to aid analysis, derived mainly from ANT, have provided a largely useful and workable set of devices with which to confront the problems posed by my research questions. Yet these tools should not be immune from criticism, especially if STS is to refrain from becoming a bland formula to be applied readily towards emergent problems like translational research. By recalling some of the issues which have emerged in the course of inquiry, it is hoped that the thesis is adding to an existing set of approaches which can be used by others to provoke contention in relation to translational research in future social studies.

Policy Implications

Translation was found to be used metaphorically in discourse of scientists and policymakers (Martin et al., 2008). Part of its appeal is that it can be quite an ambiguous and empty concept, which is adaptable as a solution towards a number of problems. For example, in policy discourse it has been picked-up as offering a promising solution to significant problems with regards how publicly funded academic medical science relates to innovation. This can be seen in terms of
how it is related to both T1 and T2 ‘gaps’ in medical and health innovation. But research in this thesis has shown that when it becomes operationalised, the idea becomes contested and more-or-less problematic. In showing how academic researchers come to work with translational research it became apparent, for instance, that their primary concerns were connected to research, rather than translation. Inversely, it could be said that for policymakers, the primary orientation is towards translation as opposed to research. Crudely speaking, then, there appears to be some discrepancy between translational research vs. translational research. One of the questions I would pose towards policymakers in the UK is which of these constructs they wish to prioritise. The answer they give is likely to have a significant impact on how health research is organised in the next few years. If translational research continues to be influential then presumably this would mean researchers modifying their practices in order to accommodate this goal. However, if translational research took precedent, then it could imply a moratorium on those types of research which fail to relate to this goal. Of course, in practice, these logics co-exist and are subject to re-negotiation. However, at the time of writing I would argue that although the latter do appear to be ‘speaking-back’ to science, researchers still retain some autonomy from the intervention of interested parties. The situation is therefore one of ‘muddling through’. Which polarity to pursue is a difficult decision as both provide certain dilemmas.

If translational research is to continue, then expectations about the likely impact it will have should not be exaggerated. Continuing to promote the circulation of promising labels and metaphors may lead to modest impact in shifting priorities and ways research is organised, but is unlikely to provide a magic bullet that will ‘institutionalise’ innovation from the public medical sciences. One of the issues is that there are a number of other external agendas researchers in these cases were expected to meet, not all of which were conducive to working further with translational allies. Researchers, as logical opportunists, will likely seek to satisfy the
expectations of external audiences as best they can, rather than organise their operations around their goals.

The research in this thesis shows also how in practice the multiple goals policymakers have for medical sciences (as sources of health, wealth and excellence) do not necessarily complement each other. As respondents in each case made me aware, the goals of wealth promised by biomedical innovation and scepticism about the ability to adopt health technologies into routine clinical practice can often be in tension. Thus the dual goals of improving healthcare (T2) and producing wealth (T1) underpinning health research policy texts like the Cooksey Report are not always commensurable. By continuing to ‘muddle through’ it thus appears logical to state that any success in plugging one of these gaps would not necessarily be conducive to plugging the other.

A whole-scale shift in the medical sciences towards translational research would clearly require a systematic revolution of how these sciences are organised and what their stated goals would be. The strategy governments have exercised towards public sciences has typically been characterised as one of steering from a distance, often via intermediaries like research councils (van der Meulen and Rip, 1998). However, this approach has always been subject to continued re-negotiations, with the balance between autonomy and control, absence and presence undergoing transformations. In a period of so-called ‘re-contextualised’ science (Nowotny, 2001), the temptation of interested parties like governments would appear to be imposition of further, stricter unitary measures to control quite diverse knowledge production practices. But stricter control over research practices is unlikely to elicit the proliferations in translation policymakers and other ‘communities of promise’ hope will emerge. Indeed reified unitary frameworks like translational research may even prove counter-productive in their capacity to undermine existing successes researchers have achieved in organising their knowledge production processes (Whitley, 2000, Lehoux et al., 2010). Furthermore, exactly what constitutes
a satisfactory level of engagement and contribution towards this wider goal is still being contested and negotiated across different organisational levels of the sciences. Relevance and robustness of different areas of the sciences after all are neither relative nor absolute concepts, but relational ones (Nowotny, 2006, see also Latour, 2005). This means the act of associating with translational research does not guarantee relevance. Critics have already pointed-out that policy initiatives like the Cooksey Report threaten to sideline areas of the health sciences which do not fit its rigid criteria for what constitutes quality research (usually thought of in terms of biomedical innovation) (Shaw and Greenhalgh, 2008). Medical treatment in other words should not be considered the sole guarantor of future health (Collins and Pinch, 2005). Policymakers might therefore consider on more of a case-by-case basis the extent to which sciences are providing products which conform to their interests, rather than imposing a de-contextualised linear innovation framework emanating from pharmaceutical development onto the whole vast tapestry of health research.

Whichever of these two polarities takes precedent, there will remain certain challenges with regards institutionalising translational research. For one the interpretive flexibility around the idea of translational research, as well as bringing people together, can also make coordinating exchanges between different parties difficult as they attribute different meanings and resistances towards it, thereby shaping subsequent actions in ways which cause disagreements. Arriving at a successful stabilisation of translational research across the medical sciences will therefore be a contested process. This is unavoidable. There needs to be common ground between producers and users of novel ideas in order for translation to occur, but managing and predicting the appropriability of novel ideas is fraught with difficulties for all sorts of sociological reasons. Those cases exhibiting relative success at translation were able to draw-on largely stable, strongly convergent networks for scaling-up academic products into clinical pay-offs. But there is not a simple algorithm available for constructing such networks elsewhere in the
sciences. A difficulty then for scientists and policymakers alike will be design and implementation of institutions, organisations and infrastructures which are adaptable towards goals of interdisciplinary, translational research. Part of the difficulty with organisations like universities and NHS seems that they display signs of being non-adaptive to this purpose (for instance through many cumbersome performance measures). People working within these settings follow their own institutions and interests in ways which do not necessarily always match-up with those practised by other social worlds. 'Trading zones' appear indispensable in order for translation to occur, but efforts to construct them are not guaranteeing of success. Hence although there does appear to be some intuitive logic to some of the efforts to foster translation observed in these cases, whether ‘trading zones’ can be established through simply bringing different entities together in the same place is not certain. However, at present, erecting such spaces would appear to be one of the few promising strategies for coordinating greater levels of translation from the medical sciences. Of course whether existing organisations like universities and NHS and their institutions can be made sufficiently flexible and adaptable in order to foster innovation is a key question.

In sum there are no easy answers to how translational research can be made to ‘work’ better in contemporary research policy and practice, or whether indeed translational research will provide a satisfactory solution to longstanding problems regarding appropriating innovation from public sciences. Part of the contribution this research can make towards policymaking is simply to point-out a set of problems. Taking the ANT approach enables one to account for differences in versions of translational research between cases and in doing so problematise assumptions which lie behind prominent versions of translational research. By using a reflexive set of methods and concepts the thesis can be used as a tool for allowing researchers and policymakers alike to begin to think more reflexively about how they might better articulate and implement this idea. It does so by allowing them to retrieve the practices from translational
research and de-mystify some of its characteristics. Conducting further empirical studies using STS concepts is an important means for opening-up substantive knowledge on this topic, which without such concepts would almost certainly remain hidden.

Concluding Remarks: An Agenda for Further Substantive Studies

Having done an STS-style study it would seem inappropriate to stipulate what should be found by future research in social studies of translational research. Instead what I would like to set-out here are certain provocations with regards existing studies and themes with which future studies should benefit from engaging. I believe the task of social studies of translational research now should be to begin looking at performances of this idea in various other epistemic settings/places. Whether future studies are through single or comparative case studies, this thesis provides a theoretical/methodological tool-set with which to do so.

Previous studies have glossed over the problem of how to conceptualise the term translational research, accepting instead policy definitions like ‘bench-to-bedside’ as their point-of-reference. But I would argue that to appreciate better the dynamics of translational research in organisation of contemporary public science, social scientists need to consider the real-world performances of the idea. Having tackled this problem by introducing terms like boundary object and immutable mobile, I find aspects of these earlier studies dissatisfying in a number of ways. Part of the frustration comes because in these stories ‘policymakers’ seem to be deployed as strawman figures made to speak on behalf of linear models of innovation. This representation then allows authors to knock them down with arguments typically pointing towards the futility of linear innovation models and the presence of feedback between sites of production, development and use (‘bedside-to-bench’). One of the immediately obvious weaknesses was that it is not made at all clear who these policymakers are or how their attitudes have come to be known. Although I share many of these basic sentiments about how innovation works, I suspect that these arguments may be somewhat oversimplified for polemic purposes. This was
reinforced when, reading through policy texts like the Cooksey Report (2006), to my surprise finding them not to propound versions of innovation processes quite as naïve and linear as I had come to expect having read social studies of translational research arguments about ‘policymakers’ approach towards science-innovation. Parts of the text even acknowledged that translational research involves a two-way feedback processes. The term immutable mobile implies specific transformations of definitions as the idea gets extended and mobilised, meaning claims about policymakers’ supposed devotion to a singular, simplified definitions like ‘bench-to-bedside’ appear slight. Until further empirical work is done on how policymakers engage with this term, it appears equally plausible to state that translational research is seen by them in largely instrumental terms as a label to be ‘filled-in’ by scientists and their allies. It may be that they are prepared to promote simplified, singular, de-modalised versions of reality, not because they believe they are likely to emerge exactly as specified, but because they are seen as helpful for getting things done at a distance and provide renewed hope for bridging innovation paradoxes. Furthermore as translational research is no longer synonymous simply with promises of biomedical breakthroughs, but also incorporates ‘T2’ approaches articulating some scepticism towards biomedical innovation, can the term any longer be made interchangeable with linear, deterministic models of innovation? I would suggest a more nuanced approach is needed which is able to accommodate differences in the handling of the term as it moves across different networks of practitioners. My study provides a useful basis for an analytic framework which can capture such complex developments.

Previous studies have also paid little attention to how this labelling device is accommodated and resisted within the mundane contexts of scientists’ work practices. Averting analytic attention towards these activities is likely to prove fruitful in terms of exploring the barrier towards translation which is surely of central interest to all of those interested in this topic. Several of my insights can be usefully taken-forward on further research journeys into this topic. Translation
can be understood as a metaphor used by scientists and other interested parties like policymakers and industry to describe science and innovation (Martin et al., 2008). In particular, I found this metaphor to be mobilised in the extension efforts of technoscientific networks. For the scientists’ part, a metaphor like translational research was important in helping them to present an equivalence between what it was they were doing and what has been successful elsewhere (Knorr-Cetina, 1981, 57). Respondents recognised how this metaphor evokes something of a ‘hinterland’ for audiences (Law, 2004) to whom they were variously accountable, particularly interested parties like sponsors and evaluation regimes. Hinterlands are sets of existing statements which a novel statement must sit alongside, support and/or reinforce in order to be accepted and transform wider networks (ibid, 28). Translational research was as a label scientists attached to their products in an attempt to make them more accessible and transportable as they come up against the hinterlands of potential allies. The argument then that the metaphor translation performs a dramaturgical function in ‘packaging’ their activities has some currency. Certainly the term would appear to have some stylistic appeal to audiences. Dismissing ‘translation’ as mere decoration of scientists’ activities would be mistaken, given how decoration was recognised by them as important with respect to a number of facets of their production process, such as obtaining funding and meeting performance criteria. Metaphors, therefore, far from ‘having no serious place’ in discussions about organised activities, are in fact crucial for ‘[plugging] gaps in literal vocabulary’ and ‘supply[ing] the want of convenient abbreviations’ (Black, 1962, 32-33). As academic sciences are in a peculiar situation of being highly specialised and self-referential collectives whilst still being dependent upon ‘outsiders’ for support and legitimacy, efficient and effective means for ‘plugging’ gaps in their audiences’ understanding remain highly important. This is why metaphors in science like translation should not be treated as trivial matters. They help science to ‘work’.
Future STS research can follow the logic that researchers’ articulations of definitions for ‘translation’ is not self-evident, but must be worked-out in relation to all sorts of local organisational considerations and competencies. The local configurations of institutions found to be practised in these ‘peripheral’ places appears crucial for how (if at all) ideas get transformed and whether they are deemed ‘workable’. STS presents workability as a pragmatic and relational concept which points towards the scientific production process as being informed by a number of practical considerations by scientists, not least their existing situations in respect to ‘worlds of instrumentation, collaboration, publication, and investment’ (Knorr-Cetina, 1981, 58). As ‘logical opportunists’ scientists organise their actions and selections around unrealised solutions which they calculate as promising strong chances of success (Knorr-Cetina, 1981, 57). In (research) practice then there are multiple stabilisation efforts being enacted around a host of other, sometimes antagonistic objects (Mol, 2002, 150). My study’s contribution develops these STS insights to show how the ‘workability’ of translational research was shown to be very much related to the presence (or otherwise) of stable networks of allies for constructing and appropriating their ideas. Where such relations are weakly convergent the ‘workability’ of translational research is likely to recede. The empirical chapters also showed that when translational research marks a shift away from ‘what can be done’ (Knorr-Cetina, 1981, 58; Fujimura, 1987) in sites of scientific production, high costs of adaptation are likely to be coupled with emergence of resistances (cf. Pickering, 1995). Therefore, in order for some form of translational research to transform local research practices, it has to promise success as a solution towards locally-conceived problems (cf. Knorr-Cetina, 1981). Following this line of argument, it appears improbable that scientists ‘at the coalface’ will make a gestalt switch towards problems incommensurable with their existing institutional narratives, competencies and capital investments. The experienced and/or expected cost of extensions is therefore a central register in helping to conceive of scientists’ accommodation and resistance towards this problem (Latour & Woolgar, 1986). The empirical findings suggest that there were a number of
competing pressures and demands placed upon researchers in these institutional settings, and that in pursuing goals which are deemed economically, politically, or epistemically viable, researchers will indeed follow certain fashions.

As a topic of inquiry translational research should be understood by social studies analysts as it relates to the extension efforts and practices performed within networks indexical to contemporary research and innovation systems, particularly in domains associated with the ‘medical industrial complex’. Analysts should focus on struggles to stabilise and accommodate this goal within production settings and/or other centres-of-calculation thought central to research management and organisation. Of these, other promising sites for fieldwork might occur in institutions located elsewhere in the triple helix (university, government, industry), ‘hidden research system’ (hospitals and other health care settings), and/or civil society groups (e.g. medical research charities or patient activist groups), all concerned with this phenomenon in one way or another. In doing so one of the key aims should be to describe the coordination work through which translational research is made workable (or not) within such arrangements. Clearly any study which takes this task seriously should describe the diversity of institutions in these specific local settings, but there are some themes from this story of academic researchers to be taken-forward and engaged with in producing further accounts. As intimated in the theoretical discussion, one factor which appears to make translational research workable is the articulation of places in which to meet and communicate. Other themes such as the identification of appropriate networks of practice, mobilising promises and incentive structures appeared highly important to coordinating exchanges.

As with other devices, there are certain typical features required of a label like translational research in order for it to act at a distance with any success. One is that its definitions need to circulate in networks through everyday actions like writing, talking, and demonstrating, with the aid of mundane items to help inscriptions retain their shape, like strategic policy documents,
grant application forms, curriculum vitae, journal pages, websites, email, PowerPoint slides and so on. These constituted some of the mediating practices and ‘flat surfaces’ (Latour, 1990) necessary for this device to act at a distance. This would likely be a feature of studies of translational research located in institutional settings elsewhere. Another feature in common with immutable mobiles is that in order to work, definitions needed to be transformed in a way which was deemed useful, workable, and/or unavoidable at local sites of practice.

The set of themes presented here is not exhaustive, as it is hoped there are others to be identified through further empirical inquiry. Indeed this thesis should not mark the closure for social studies about the performative effects of translational research in contexts of practice, but a new beginning into this important topic of inquiry. The ‘appropriateness’ of various transformations by novel actors and therefore the success of immutable mobiles are not predictable prior to their mobilisation, as these evaluations are socially constructed and negotiated (Latour, 1987, 112). This means that over time, transformations of translational research by academic scientists could begin to be read as ‘treasonous’ by interested parties (cf. Callon, 1986). It should be noted that within this arrangement it is the customers of science who play an important role in determining the worth of research. At present, although translational research appears as an idea whose stock is on the rise in academic sciences in England, it is worth remembering that its stability is not given or absolute, but relational and therefore ‘dropable’ should networks keeping it stable begin to break apart. This is one of the reasons why the topic of translational research is shaping-up to be one of immense interest for STS scholars. If/when such developments occur I believe STS tools developed here and provided elsewhere should provide a highly useful resource for navigating through this complex terrain.
References


ATKINSON-GROSJEAN, J. 2006. Public science, private interests : culture and commerce in Canada’s Networks of Centres of Excellence, Toronto Ont. ; Buffalo N.Y., University of Toronto Press.


the governance and outcomes of a science/policy network in genetics. *Science and Public Policy*, 37, 737-750.


UKCRC 2006. Understanding Clinical Trials, London, UKCRC.


### Appendix 1: Interview Respondents

#### Clinical Trials Unit

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<th>Participant Role</th>
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<tr>
<td>Marketing Director</td>
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<td>48 min</td>
</tr>
<tr>
<td>Director CTU</td>
<td>Face-to-face</td>
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</tr>
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<td>Senior Medical Officer</td>
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#### Additional Pilots

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#### Cancer

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<td>PI Cancer Lab</td>
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<td>Local Engagement Manager</td>
<td>Phone</td>
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<tr>
<td>Post-doc 1</td>
<td>Face-to-face</td>
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<td>Post-doc 2</td>
<td>Face-to-face</td>
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<tr>
<td>S/L</td>
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<td>Technician</td>
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## Obstetrics

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<td>Senior CT Manager</td>
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<td>Consultant obstetrician</td>
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## Diabetes

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<td>PI</td>
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<tr>
<td>Clinical Collaborator</td>
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<tr>
<td>Clinical Fellow/PhD</td>
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<td>PhD</td>
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Appendix 2: Favourable Ethical Opinion

Mr Alex Rushforth

15 February 2011

Dear Mr Rushforth


I am writing to inform you that the Chairman, on behalf of the Ethics Committee, has considered the Amendments requested to the above protocol and has approved them on the understanding that the Ethical Guidelines for Teaching and Research are observed. Please be advised that the Ethics Committee is able to audit research to ensure that researchers are abiding by the University requirements and guidelines.

If the project includes distribution of a survey or questionnaire to members of the University community, researchers are asked to include a statement advising that the project has been reviewed by the University’s Ethics Committee.

Date of confirmation of ethical opinion: 15 September 2010.

Date of approval of amendment to protocol: 15 February 2011.

The list of amended documents reviewed and approved by the Chairman is as follows:-

<table>
<thead>
<tr>
<th>Document</th>
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<tr>
<td>Protocol Cover sheet</td>
</tr>
<tr>
<td>Summary and detailed protocol</td>
</tr>
<tr>
<td>Summary and detailed protocol (Track Changes)</td>
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<tr>
<td>Interview Schedule</td>
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<tr>
<td>Information Sheet for Participants</td>
</tr>
<tr>
<td>Consent form</td>
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<tr>
<td>Sample to participants</td>
</tr>
<tr>
<td>Risk assessment</td>
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</tbody>
</table>

*Sensitive information has been removed here

Yours sincerely

Glenn Moulton
Secretary, University Ethics Committee
Registry
Appendix 3: Consent Form

Translational Research in the Making

Consent Form

(Date)

- I the undersigned voluntarily agree to take part in the study on Translational Research.

- I have read and understood the Information Sheet provided. I have been given a full explanation by the investigators of the nature, purpose, location and likely duration of the study, and of what I will be expected to do.

- I consent to my personal data, as outlined in the accompanying information sheet, being used for this study and other research. I understand that all personal data relating to volunteers is held and processed in the strictest confidence, and in accordance with the Data Protection Act (1998). I understand that the data may be published, on the agreement that my identity is anonymised.

- I understand that I am free to withdraw from the study at any time without needing to justify my decision and without prejudice.

- I understand that should I need to complain about the study, I should contact Mr. Alex Rushforth (University of Surrey) email: a.rushforth@surrey.ac.uk

- I confirm that I have read and understood the above and freely consent to participating in this study. I have been given adequate time to consider my participation and agree to comply with the instructions and restrictions of the study.

Name of volunteer (BLOCK CAPITALS) ..............................................................

Signed ..............................................................................................................

Date ...............................................................................................................  

Name of researcher/person taking consent (BLOCK CAPITALS) ..............................................................

Signed ..............................................................................................................

Date ...............................................................................................................
Appendix 4: Participant Information

Information Sheet for Participants

Translational Research in the Making

What is the research about?

The research is about how individuals and organisations make sense of the challenges of translating research into a clinical 'pay-off' (understood here under the general term translational research). The study is to be based in part on interviews in which people are asked to describe their own experiences in setting-up and/or working on research that has this broad goal in mind, and reflect upon obstacles/challenges that they may have negotiated in their own work. The research is also interested in how researchers interpret the general prospects for this broad type of endeavour and any opportunities/difficulties they might foresee arising from it.

What will be the set-up of the interviews?

The interviews will be of a semi-structured style, meaning that it may be closer to a conversation than a rigid question and answer session. You will be asked to recall and describe certain things and further questions/clarifications may well be sought at certain points in the interview. Documents may be shown to you as prompts during the sessions. There will be pen and paper available should you wish to make notes or visual displays corresponding to your answers. These may be requested to be taken away after the interviews, but of course you retain the right to say no to this. The interviews will be recorded on a Dictaphone.

What will I be asked to talk about?

Semi-structured interviews are designed to be free-flowing, meaning it is quite possible that the sessions will cover a diverse range of issues. You may be asked to reflect on the wider field of 'translational research’ and where you place yourself and your organisation in relation to it. Examples of what you may be asked to describe include the type of professional activities you engage in, the challenges that come with working with partners on projects or how you might seek to promote the work of the organisation. Also of interest is how you perceive the future prospects for translational research and any possible obstacles to achieving this. Where appropriate, it may be useful to relate to your own experiences on these matters.

How long will the sessions last?

Preferably around 45 minutes, although if you can spare longer then this would be much appreciated. I recognise that you are all busy people, so if you cannot afford to give-up this much time shorter sessions can be arranged. If you need to stop the interview at any stage please say so.

What will happen to the data?

Once recorded the data on the tape will be transcribed, coded, analysed and where appropriate, published in the thesis and elsewhere. The tapes will be secured under a
lock and key, and be destroyed after transcription. Procedures will follow the Data Protection Act 1998.

Will I be made anonymous?

Yes, the details of the project, including its name and location will be anonymised, as will job titles and names of people.

Will I be able to withdraw comments at a later date?

Yes. You will have the opportunity to request to view any of the comments you have made before they are submitted into the thesis or any other publication in the consent form. Should you feel uncomfortable about the comments at a later date, but have not specified this on the consent form, then please contact me as soon as you can.

This information sheet has been given a favourable ethical opinion by the University of Surrey Ethics Committee.