Where Science Meets Innovation

Organising Technology Research Groups in Response to Mandates for Societal and Economic Impact

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The candidate confirms that the work submitted is his own, except where work which has formed part of jointly-authored publications has been included. The contribution of the candidate and the other authors to this work has been explicitly indicated below. The candidate confirms that appropriate credit has been given within the thesis where reference has been made to the work of others. The chapters indicated below were the candidate's contribution to the specific peer-reviewed articles noted:

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Abstract

Scientific knowledge resulting from university research is an important feature in the contemporary landscape of technology innovation. However, the terms on which such science contributes to innovation have recently been the subject of contentious policy and public discourse. These debates have both challenged scientists' position to pronounce on the standards by which their work should be judged, whilst at the same time requiring them to more actively contribute towards the beneficial outcomes of their research. In a space that I describe as *where science meets innovation*, these various discourses have coalesced under the headings of "Research Impact" and "Responsible Innovation".

This thesis reports an inductive case study of how two nanotechnology research groups have sought to respond to this shifting policy landscape. Recognising established modes of realising economic impact through the commercialisation of science, these rich case studies shed light on the dynamics of such innovation work. Furthermore, framing the innovation challenge as one comprising the development of organisational capabilities, allows the response to new policy mandates for societal impact to be discerned.

In this thesis I argue for the importance of managerial agency in both sustaining established capabilities and developing new capabilities for science-led innovation. The analysis of empirical case material reveals the importance of the level of uncertainty that exists for innovation actors in connecting their research actions to innovation impacts. In conditions of low uncertainty, then a strong, stable professional identity for scientists, allied to known standards of excellence, provides a sure guide of action. Contemporary discourse about the contribution of science to innovation has destabilised such identity and standards. These case studies reveal that scientists, in a more forward-looking and reflective display of managerial agency, have engaged in the development of new capabilities that allow their work to speak to a wider constituency of interests.

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Preamble

I feel like I have been here before; at least in a manner of speaking. My first formal education was in the natural sciences and culminated in the award of a PhD in chemistry, in the (then) nascent field of nanotechnology. The experience of that first training in science and research practice now seems very distant. There is an obvious connection in that I am now trying for the same degree in another discipline. In addition, my very research subject is taking me back to the same type of research labs as in my twenties. But delving deeper, there is a feeling that through my current studies I am exploring questions of what we can know through scientific inquiry that remained unanswered from my first efforts as a research student. For me the process of research has become a personal one. I no longer wish to detach myself from the subject of my curiosity. I aim to bring something of myself into the research, as this opens the possibility of a research process that is creative and not just systematic. However, such aspirations, whilst laudable for any individual, risk being too idiosyncratic to be of either interest or value to an academic (or practitioner) community. The challenge then is for the research process to be both creative and rigorous. This challenge seems a stark contrast to my experience as a research student in chemistry. I came to think of my PhD studies as an advanced apprenticeship in which I learnt the tools of my trade in a very practical hands-on manner. Following the example of senior colleagues, trial-and-error laboratory work and becoming a mentor in my turn to junior colleagues, were the conventional elements of learning the scientific method. It never occurred that there might be room for a more personal contribution as my subject of study did not seem to need it.

The world of material things existed (exists) independent of my interest. This philosophy, which I've come to understand as *realism* (Easterby-Smith *et al.*, 2008), seemed deeply ingrained through my early education in the natural sciences. My practice of science took place in a laboratory abstracted in many senses from the distractions of the real world. My subsequent working life has taken me physically and philosophically out of the laboratory. Research that seeks to exclude the messiness of the real world no longer holds an interest. Therefore, in order to ground my work philosophically and practically, I need to introduce my research with a brief account of my experiences of management and innovation. In another sense this also summarises my journey from the natural to the social sciences.

Following my PhD studies I moved directly into industry, becoming a research chemist in a small company working on problems and opportunities of an immediately practical nature. These projects involved either the continuous improvement (management jargon was apparent from the outset) of existing manufacturing processes or the design of completely new ones in response to customer enquiries. Early experience suggested that (intellectual) scientific breakthroughs were insufficient to make winning projects. All manner of extraneous factors contributed to success of an endeavour. Although I did not yet have the language to articulate it in these terms, I was attracted to the management of the whole innovation enterprise. The complexity of co-ordinating the multitude of extraneous factors, the intellectual challenge of creating an engaging path forward and, above all, the challenge of addressing things through the efforts of many people, meant that I pursued a career in management.

I made conventional progress from research through manufacturing and into leading a whole business unit within the chemical industry. Along the way I picked up an education in management, and first became introduced to the practice-friendly management literature that one encounters on MBA programmes. This experience, feeding perhaps on my early research training, has created an important legacy that impacts my current studies. The study of management only makes sense to me in relation to its consequences for practicing managers, and it will ultimately only prove of value to them if it is the result of thorough intellectual enquiry. And again, although not having the appropriate understanding at this time, this is something I now recognise as the dual hurdles (Pettigrew, 1997) of rigour and relevance of management research. It is the aim of this thesis to build the possibility of achieving these dual hurdles from the outset.

My interest in management and innovation gained further refinement following a period of time at a Regional Development Agency. This work involved both developing practical support programmes to encourage business innovation, and participation in Government working parties developing innovation policy. The final piece in this particular biographical jigsaw saw my return to a university environment and participation in what was being called the "Knowledge Transfer" (KT) agenda. This exposed me to a range of projects that aspired to accelerate the knowledge generated through university research into some practical use. It is within the arena of university science and its role in the creation of new technologies that this PhD research study is concerned.

An understanding of innovation has emerged for me that incorporates my experiences of original scientific research, the management of innovation within industry, the policy environment and practical support to connect university research to practitioner communities. Being mindful of these experiences, and not wishing for such pre-understanding to adversely influence my research, I adopted a reflexive approach throughout the study. Alvesson and Skoldberg explain that using reflexivity or reflection in research "draws attention to the complex relationship between process of knowledge production and various contexts of such processes, as well as the involvement of the knowledge producers" (2009, p. 8). Whilst I adopted research methods that immersed me in the worlds of my study participants, I also sought to conduct a critical self-exploration of the interpretations that I made of those worlds. My professional life has played out in similar contexts, and my aims here are to do more than codify two decades of reflections. Therefore, in the midst of the familiar I actively sought the unfamiliar. Most notably I joined a group of PhD students from a range of social and human sciences who were also studying science-led innovation and adopting a reflexive methodology. The practical details of this collaboration are described in Chapter 6 (Research Methodology) and the consequences for my research outlook are discussed in Chapter 14 (A Social Scientist in the Lab – A Reflexive Analysis).

And so to make a start. The importance of the construction of research questions in order to prepare the ground for interesting research is a very contemporary issue (Alvesson and Sandberg, 2013); and I would add that care in question construction is important for research that aspires to relevance as well as rigour. Therefore, the opening section of this thesis is positioned as an extended justification of the research area. It seeks to answer the question why this topic is worth researching at this moment. The answer to this question is sought in a series of different perspectives. Chapter 1 is rhetorical in tone and concerns public debates on the role (responsibility?) of science in making a contribution to a wider society of interests. In it I introduce the focus of the study as being that of individual scientists (and their research groups) seeking to respond to a changing policy and public discourse for greater societal impact from innovations that originate with their research. Chapter 2 is a conventional review of the academic literature in this area. This literature foregrounds the *phenomena* of university research being used as the starting point for innovation work; and it is a literature invariably not framed by any strong *theory*. Therefore a second academic literature survey is included (Chapter 3) that concerns an element of strategy theory (organisational capabilities) in order to frame changes in how research groups are organising for innovation as a strategic response to shifts in innovation policy. I argue that this theory is both useful in aiding my problematisation of the issues raised in the earlier chapters, and it is one to which the empirical analysis of this research may contribute. Chapter 4 draws together the threads of these perspectives to articulate the research questions for this study. The remaining sections of this thesis incorporating research design, findings and implications are introduced at appropriate places in the text.

Let us begin.

“It is best to begin, I think, by reminding you,

the beginning student, that the most admirable thinkers

within the scholarly community you have chosen to join

do not split their work from their lives”

“On Intellectual Craftsmanship” in

The Sociological Imagination by C Wright Mills

PART I
JUSTIFICATION OF RESEARCH AREA

Chapter 1
When Science Met Innovation

During the late 1990s, in what they expected would be a successful move, the Monsanto corporation launched a series of products in Europe based upon their genetically-modified organisms (GMO) technology platform: their expectations were proved wrong. The popular response to the incorporation of GMO technology into the food chain was one of vociferous opposition (cf. OST, 2000). Such opposition was widespread and not simply a product of yet another environmental activist campaign. Governments took notice: the EU issued a moratorium on the adoption of GM crop technologies, six African nations turned down US food aid suspicious of its GMO nature and senior members of the Bush administration referred to Europeans as “Luddites” because of their rejection of the technology (Weasel, 2009).

The story of the introduction of GM Crops in Europe (Bonny, 2003) has become a classic illustration of the widely different responses evoked by the introduction of a new technology. Interviewed at the time by Harvard Business Review, the Monsanto CEO (Robert B. Shapiro) framed his company’s science and innovation strategy as “growth through global sustainability”, noting “I am one of those techno-utopians who just assume that technology is going to take care of everyone” (Magretta and Shapiro, 1997, p.83). Scientists themselves invariably reacted to the adverse response in a similar fashion. Evolutionary Biologist Richard Dawkins again in an interview for Harvard Business Review commented on the response to GMO technology: “Part of the reason for Monsanto’s troubles is that the company came up against an extraordinary amount of unfortunate, even malevolent, media hype. And people were more or less misled by one scare story after another, into stampeding” (Dawkins, 2001, p.162). This narrative of a duped or scientifically-illiterate public overreacting to new technology was common amongst scientists and technologists. It was roundly criticised as patronising as it assumed a deficit in understanding on the part of the public; something to be corrected by bringing the public ‘up to speed’ with the scientists rather than have their own concerns and understanding being treated as equally valid (Wynne, 2001).

Such polar positions on science, and particularly on efforts to apply science, are recognizable in debates that have entered the popular imagination. The outset of this PhD study coincided with events marking the 50th anniversary of C.P. Snow's famous "Two Cultures" lecture (Snow, 1959). In this talk Snow warned of a gap between the sciences and humanities and the adverse consequences this split posed for solving the contemporary global challenges. In the concluding remarks of his talk he said "Closing the gap between our cultures is a necessity…. When those two senses have grown apart, then no society is going to be able to think with wisdom" (Snow, 1959, p. 50). Setting aside the peculiar (to me at least) categories of the split he identifies, there is a generalisable argument that continues to resonate today, and was made manifest in the debates on GM crops. Solving the complex problems faced by the world is going to require contributions from different disciplines of thinking. The position outlined above by the Monsanto CEO that technology will solve all our problems seems increasingly naïve: other 'cultures' (in the sense used by Snow) should be brought to bear on these problems. Integrating diverse cultures of thinking is likely to be difficult as Monsanto found to their cost. Snow, in his own review of the "Two Cultures" debates noted that "escaping the dangers of applied science is one thing. Doing the simple and manifest good which applied science has put in our power is another, more difficult, more demanding of human qualities, and in the long run far more enriching to us all. It will need energy, self-knowledge, new skills. It will need new perceptions into both closed and open politics" (Snow, 1963, p.99).

In this study I explore one aspect of the challenge laid down by Snow, namely what contribution are individual scientists to play in realising "the simple and manifest good which applied science has put in our power". Such work exists at the intersection between the discovery of new knowledge and the realization of a value from that knowledge. This value is frequently expressed in financial or *economic* terms. However, the GMO debates include a wider notion of value, to incorporate social, environmental and even cultural issues. In this thesis I use the term *societal impact* as expressive of such notions of value that cannot be stated in purely economic terms (at least not by their advocates). In the title words of this thesis, these issues play out in the space in which *science meets innovation*; and this has become a space in which academic scientists are increasingly expected to operate. Because this study is embedded in the world of scientists and their research groups I use the term *science-led innovation* to describe their work in this space. It is also an arena that has been subject of major innovation policy developments. The discourse behind these developments has been given labels that are resonant with meaning, and two in particular have provided the backdrop to this PhD study: *Research Impact* and *Responsible Innovation*. Whilst cognate in the sense of being concerned with the widest range of consequences, their approach to these issues is from different directions. The discourse of research impact starts with science and examines its consequences. The discourse of responsible innovation starts with a notion of the world as we might want it, and examines the implications for a science that could contribute to its realisation. However, whilst this thesis explores very contemporary concerns, it must be owned that the underlying issues are anything but new.

Debates concerning the responsibilities of scientists for the consequences of their work have a long history. As long ago as the 16th century Francis Bacon argued that a key justification for the pursuit of science was to realise "the relief of man's estate" (1605, Book 1, v, 8). However, the more dominant view that emerge during the enlightenment was that science was an amoral endeavour that should be judged purely in its own terms: science was to be judged good or bad in relation to the quality of its methods, the originality of its ideas and the rigour of its reasoning. And judgement on whether such standards had been attained was to be established by disputation amongst peers. These debates continued to be refined through the 20th Century. On the one hand J.D. Bernal, notable for his contributions as both a scientist and historian of science, suggested (1939) that science must work actively towards realising a value for a wider society than itself. In contrast, the classic position of disinterestedness of science was re-stated by another polymath Michael Polanyi (1962) who drew an analogy between the conduct of science and the workings of a free market: it was by pursuing its own self-interests, free from any interference, that science could best realise societal benefits.

Such debates amongst thinking people about the relationship between science and society have also played out in the more practical world of science and innovation policy. Such policy work coalesces around the question of the benefits that accrue to society as a result of Government investments in science. Whilst the example of the GMO affair of the 1990s suggested that the issues at stake are more complex, science policy enactments have invariably been expressed within a rhetoric of economic benefits. Thus, the science and innovation policy of the last two decades has included a recurrent narrative of the 'knowledge economy'. This posits that the general prosperity of a modern developed country is considered to be founded increasingly upon its stock of intellectual capital rather than natural resources. This outlook has provided a renewed impetus to the argument that scientific research should be more explicitly driven by some notion of wider economic or social well-being. During the last five years the phrase used in the UK to express this mandate has been "Research Impact". Scientists (indeed academic researchers of all kinds) are being required to work towards, and then demonstrate, the "Impact" of their research. Whilst the development of this agenda (and cognate policies) by the UK Government is considered in more detail in Chapter 8, for now we might consider something of the response of scientists themselves evident within the popular press.

In response to a requirement that proposals for UK Research Council funding be accompanies by a "statement of research impact" a distinguished group of natural scientists complained in a letter to the Times Higher Education newspaper on 12th February 2009 that: "…. In research worthy of this name, we are not aware of anyone who would be competent at foretelling specific future benefits and therefore in complying with the request [for an impact statement] in any meaningful manner".

A call by the same authors for peer reviewers to ignore Research Council requests to take considerations of research impact into account during assessment of grant proposals, received a muted response. In a related move an excess of 16,000 signatories were collected to a petition organised by the University and College Union to remove research impact from the assessment of UK academic's performance that takes place every 6-7 years (at the time of writing the results of the next assessment exercise are due in late 2014, and is call the Research Excellence Framework or REF for short). A variety of arguments were marshalled in criticism of the proposals to institutionalise Research Impact, and the debate played out very publically on the pages of the Times Higher Education newspaper. An archive of the debate is available on the newspapers website (www.timeshighereducation.co.uk) with the main criticisms of the policy being:

- Measures of research impact do not simply correlate with measures of research quality
- There may be a time delay greater than the REF assessment period before the real impact of a piece of research is realised
- It will inhibit the pursuit of research that is recognised by peers to have intrinsic worth and interest
- 20% of the total REF score (in the 2014 exercise) being based on research impact is too high because the measures of impact are untested, and it is unnecessary if the challenge to is encourage a behaviour of communicating basic research to a wider audience
- there is no unambiguous way of assessing the impact of a piece of research that stopped something happening in the wider world

Whilst such popular opposition, and more formal responses to funding agency consultation exercises, has led to a refinement of plans to enact Impact policies, the core idea remains that science will be judged in terms of a wider set of contributions than the purely academic. What then of the individual scientist? If both funding decisions for new projects and formal research assessment are to be made (in part at least) on the basis of Impact, then what are the implications for what individual scientists actually do? How are they to make practical connections between social and economic concerns and their research? Who should they be working with to make such connections? What new skills or competences will this require of them? Where, indeed is the work of science to take place? The overarching aim of this thesis has been to explore such questions with scientists who have sought to actively engage with the *research impact* agenda.

The need to demonstrate Impact is felt across all disciplines in the UK academy. However, as the story of GM crops illustrates, the stakes for scientists whose research is associated with new to the world technologies are particularly heightened. For such domains the possibilities of impact may be cast in extreme terms, with advocacy of polar positions. The resulting complexity of issues has become apparent under the discourse of *Responsible Innovation*. For this study the wider range of actors involved make the context of emerging technologies particularly interesting. Having decided to work within this arena, it seems to me that an important early decision for this PhD study ought to be the contextual choice of which area of emerging technology should I research. Questions concerning the roles and responsibilities of scientists have not been confined to GMO technologies. Nanotechnology (Bozeman *et al.*, 2007), Geoengineering (Macnaghten and Owen, 2011), and Synthetic Biology (Rabinow and Bennett, 2012) have all witnessed similar debates. For this PhD study I choose to work within the domain of nanotechnology. At the outset I own up a personal dimension to this choice. My first PhD was in an area of science that would today be labelled as nanotechnology, but in the 1980s that term was net yet in common use. However, there are also more objective reasons for suggesting that UK research groups working in this area, at the interface between science and innovation, constitute an interesting field of study.

Nanotechnology entered the public arena occupied by GM foods with the publication of “The Big Down: Atomtech – technologies converging at the nanoscale" (Etc_Group, 2003). This report by a NGO that had formerly contributed much to the GM food debates deliberately sought to widen the terms of that debate to include the societal impacts of nanotechnology. Their aims gained a marked impetus in the UK with the intervention of the heir to the British Throne. HRH Prince Charles, writing in a national newspaper (The Independent on Sunday, Sunday 11th July 2004) included elements of both measured rhetoric and careless scaremongering that continue to characterise much of the public debate of these issues. Echoing CP Snow's sentiments he writes "Discovering the secrets of the Universe is one thing; ensuring that those secrets are used wisely and appropriately is quite another. What exactly are the risks attached to each of these techniques under discussion, who will bear them, and who will be liable if and when real life fails to follow the rose-tinted script?" However, less helpfully he made the connection, reinforced by subsequent headline writers, that "thalidomide-style[1] disasters could result" from this technology. Following Prince Charles' intervention, the issue was always likely to remain a perennial topic when discussion in the popular media turned to the applications and impacts of new technology. A more considered discussion of the subsequent debates and their relationship with the UK Government's innovation policy is presented in Chapter 8.

Having outlined in this chapter the contemporary intellectual climate pertaining to responsibilities during the emergence of new technologies, the remainder of Part I of this thesis seeks to refine and justify a study of the innovation work of individual university-based nanoscientists. Studying the work of such scientists might more usually be found in the arena of science and technology studies (STS). Indeed during the course of this PhD programme I have sought out the company of such scholars for the insights they bring to these contemporary issues. However, my interests have a more practical dimension than is normally evident in STS. At the outset I hold out the hope that my research might have some practical value for the nanotechnology researchers with whom I engage. This is a

[1] A drug prescribed during the 1950s to ease morning sickness in the early months of pregnancy that subsequently caused birth defects.

familiar aspiration in management studies (and one examined more fully in Chapter 5). It is not unusual for journal papers in management studies to end with a paragraph or two suggesting how the paper's ideas might be useful to practitioners. I cannot recall a single instance of an STS paper that include comparable suggestions. The acrimonious debates of the "Science Wars" of the 1990s (cf. Labinger and Collins, 2001) suggest that the work of human or social scientists would not be an obvious source of ideas for natural scientists seeking changes in their innovation practices. The non-academic audience for STS work has proved to be policy-makers, and much of the innovation policy developments examined later in this thesis (Chapter 8) was influenced by STS scholars.

It should be no surprise then to learn that STS scholars have long been interested in debates concerning the relationship between science, the economy and a wider society. Their approaches reflect the multidisciplinary nature of the field (cf. Sismondo, 2010) and a few examples serve to illustrate the diversity of perspectives. From some sociologists closely involved in EU science policy came the idea that science in the 20th century had become context-driven and interdisciplinary (Gibbons *et al.*, 1994); thinking that was extended to suggest that the relationship between science and society was dynamic rather than demarcated by strict boundaries (Nowotny *et al.*, 2001). Mirowski and Sent focussed on the influence of funding in arguing that "alternative forms of funding and organization have shaped both the conduct and content of science throughout its history, characterized by shifting alliances among the commercial corporation, the state and the university" (2008, p. 673). On this basis they identify three funding regimes through the 20th century that represents different ways of organising science. Finally Berman uses institutional theory to argue that the expansion in patenting by universities was less to do with the Bayh-Dole Act of 1980, an can be understood as a process of institution-building that had been in progress since the 1960s (2008).

From a more theoretical perspective then the complexity of different voices contributing to critiques of science-led innovation suggests that *Actor-Network Theory* (ANT) might be an insightful STS approach to adopt in a study of the contemporary debates outlined in this chapter. Described as the “best known of STS’s theoretical achievements so far” (Sismondo, 2010, p.92), ANT explains the relationship between science and technology in terms of the creation of ever more complex networks of human and non-human actors. Framing my study using ANT would have involved understanding the changing interests and power relations between the different actors involved with my research site, and the way their being brought together made them act (as the actors of ANT are actants: humans or non-humans that are made to act). Bruno Latour’s “Science in Action” (1987) is possibly the most well-known exposition of ANT, and his prescription to *follow the scientist* has to a degree informed my case study methodology (see chapter 6). However, in treating human and non-human agents alike, it seems to downplay more subjective phenomena such as culture. These phenomena are viewed solely in terms of the networks of actors that produce them, and there is no place for institutional or macro-level forces. And yet as I entered this research field during my pilot research (see chapter 7) the influence of such forces seemed intuitively apparent. In a similar vein the suggestion (by ANT) that everything results from the relations within networks seems to deny the intuition that scientists and other innovation actors discover either new natural properties or market opportunities; rather than merely help to construct them. Therefore, my approach during the PhD work was to remain within the mainstream of management studies, whilst cultivating a familiarity with science and technology studies.

Within management studies the distributed nature of knowledge has been examined within the notion of innovation networks. Less theoretical in its nature than ANT, this literature is presented in Chapter 2 as the starting point for examining different organisational perspectives on the commercialisation of university science. Taken together, this literature has revealed a range of practices that may be adopted by university scientists wishing to pursue the commercial application of their research. However, the emerging mandates for economic and societal impact (outlined here in chapter 1, but explored in more detail later in Chapter 8) are widening the terms on which such applications are to be judged. No longer simply a matter of realising economic value, considerations of social and environmental (and even cultural) value must be brought to bear within innovation practices. This implies change, and Chapter 3 introduces a different management literature that offers insight on how organisations (in this case research groups seeking to do innovation) change in their capabilities to do innovation. The different threads of my arguments are then brought together in the close of this section (Chapter 4) in order to articulate this study's research questions.

Chapter 2
Academic Studies of Innovation and Universities

In the last chapter I outlined the contemporary twist that has been placed on the centuries old question of the relationship between science and its role within society, following the introduction of major new-to-the-world technology platforms. I explained how the response of individual scientists to such debates has been placed in stark relief as a consequence of a series of policy initiatives that require them to take a more active interest in the application of their research. Whilst I suggested that there is something distinctly new in these recent mandates that is obliging scientists to re-examine their approach to their work, it must also be acknowledged that there is extensive evidence of many university scientists doing innovation work. The academic studies of such work are the subject of this chapter. In reviewing such studies I am not so much trying to identify gaps in the management literature that this PhD programme is trying to plug; although this indeed may happen. Rather I aim to identify literatures that offer important perspectives on contemporary issues. I acknowledge the need to explore important gaps in the literature, but my purpose here is to ensure that same gap resonates with a challenge faced by university scientists themselves. Gaps in knowledge identified after an extensive and even systematic literature search may actually exist for a reason: they are of no interest to anyone. This is not to argue that research must slavishly follow the needs of practitioners. There should always be a need for management research to create completely new ideas or to challenge practice, but these goals are more likely to result from the identification of new and imaginative perspectives than gap-plugging (cf. Sandberg and Alvesson, 2011).

The literatures that I outline in this chapter have a strong empirical content and are invariably presented without being framed by any strong management theory. The majority of this chapter concerns itself with studies of university research groups and how they manage innovation. Universities do not have all the resources and expertise to undertake innovation alone, as the core of their mission remains teaching and research. Therefore a good area of the literature to consider at the start is that which deals with innovation networks. A sub-set of this literature is that which deals with the inter-relationship of Government-Universities-Industry and which has come to be known as the “Triple Helix”. Moving then towards the research group as a unit of analysis the chapter explores the two dominant mechanisms studied within this literature: academic entrepreneurship and university-industry research collaborations. This discussion starts very broad with a consideration of the innovation landscape and gradually focuses on the level of university researchers and their groups. This structure is shown figuratively below.

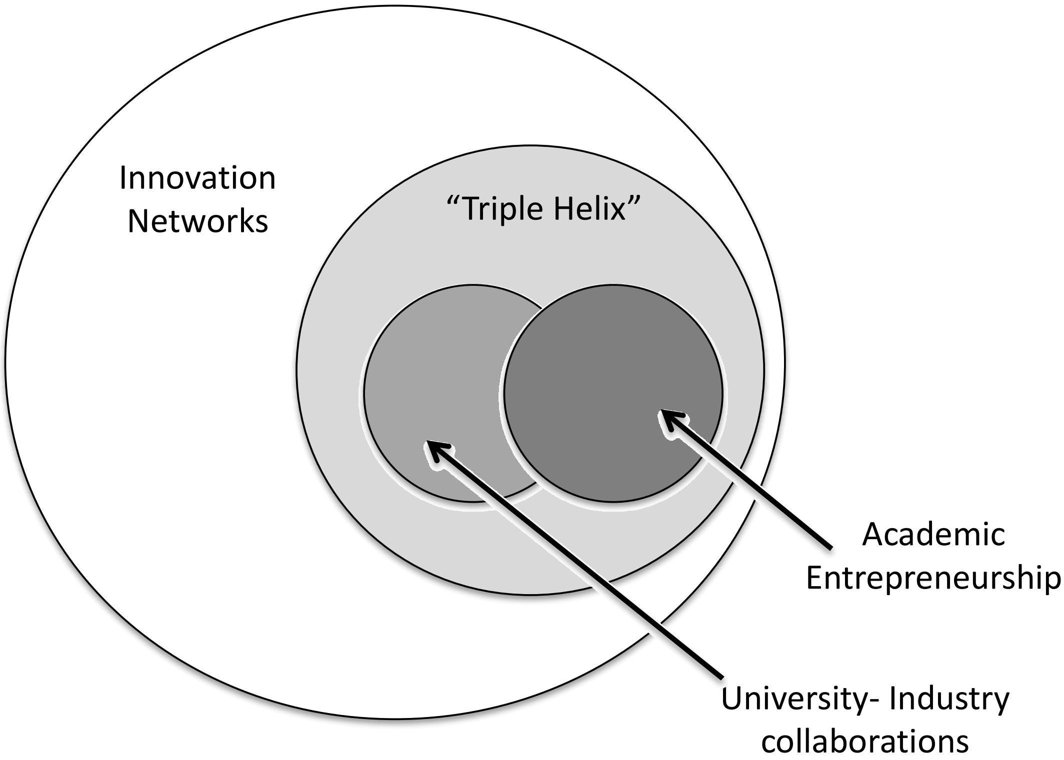

Figure 1 The nesting of literatures reviewed in this chapter

Innovation Networks

The creation of modern scientific and technological knowledge has been recognised as a complex process (Powell *et al.*, 1996). Breakthroughs occur at different times and within different organisations. The transfer or diffusion of knowledge between these places becomes critical for the management of innovation. This complex picture of knowledge creation has led scholars to study the emergence of innovation networks as the means by which dispersed advance knowledge is crystallised into new products and services. Such scholarship emphasises the personal nature of these interactions rather than a more impersonal combination of new knowledge. Kogut and Zander (1992) stress that key organisational knowledge resides in individuals and is manifest by how they interact within communities or networks. Nahapiet and Ghoshal (1998) discuss the mechanisms by which social capital leads to the generation of new ideas and possibilities. Other authors (Singh, 2005, Sorenson *et al.*, 2006) have shown how such intellectual capital generation is facilitated by the geographical proximity between individuals within networks. Overall from the perspective of such networks a picture emerges of a division of Innovation Labour (Fleming and Waguespack, 2007).

From the perspective of participating organisations such networks may open up possibilities beyond securing new intellectual capital. Exposure to not only new knowledge but alterative mind-sets inherent in the personal nature of network life may make for a more creative organisational dynamic (Fleming and Marx, 2006). In a study of the PC and the workstation market (Lee and Lee, 2003) the exploration of unlikely technology combinations was found to yield greater growth when the market was dominated by active IT enthusiasts. Birkinshaw, Bessant and Delbridge (2007) suggest exposure to the thinking offered by completely fresh partners is essential if firms are to adapt to discontinuous change.

It is therefore tempting to consider how organisations might manage their participation in networks. Birkinshaw *et al.*'s prescriptions (2007) suggest organisations might exert significant control over network formation. Other authors (Fleming and Marx, 2006) indicate a more responsive approach that acknowledges these networks as fundamentally between individuals and not organisations. This apparent tension between efforts to design networks and a reactive response to emergent network formation may simply be a reflection of managerial style. The challenge for organisational managers seems to be one of encouraging their scientists and engineers to participate in networks in ways that return value to the organisation. Studies suggest a variety of approaches are possible. In a quantitative study of 87 cellular service providers and manufacturers, Rosenkopf (2001) demonstrated that collaborative activity between companies was more pronounced in situations where their managers had forged personal links through participation in sectoral technical committees. Furthermore, such a route to collaboration was more significant when those companies did not have more contractually-based routes to exchanging information. Considering the generalisability of these conclusions, Rosenkopf herself suggests (2001) this mechanism may be more important in sectors for which formed intellectual property is used less. Sectors characterised by strict IP policies (e.g. biotechnology) may exert constraints on their managers' tendency to share information.

That this contrast between open sharing of information and careful management of intellectual property defies simple prescription was further evidenced by Spencer's study (2003) of the flat panel display industry. By undertaking a citation analysis of the scientific and patent literature produced over a twenty-year period, Spencer made findings that confound the norm of IP protectionism within a high tech sector. In sectors (such as flat panel displays) in which pre-commercial competition exists to establish a dominant design, Spencer argued early sharing of information between firms leads to enhanced performance benefits. The firm's driving force for such openness is to have their technology adopted as an industry standard.

However, encouraging employees to network within the context of a knowledge-sharing strategy may be insufficient to ensure benefits return to the firm. A significant thread of research has explored the impact of different structural features of networks (Borgatti and Foster, 2003), on firm innovation strategies. In a study of the Boston Biotech Cluster, Owen-Smith and Powell showed (2004) the importance of non-relationship factors. By applying visualisation tools to probe the structural features of the innovation network, they identified two other important determinants of information flow:

geographical proximity and organised ties to key actors within the network. In a study of the chemicals industry, Ahuja (2000) explored the relationship between different types of network-ties and disconnections (known as structural holes), and innovation performance (measured by patenting activity). His works suggested no simple relationship between network structure and performance, rather "identifying the benefit sought from a social structure is … likely to be critical in identifying the form of social structure that is most likely to be facilitative" (Ahuja, 2000, p.452). In a detailed study of the Application Specific Integrated Circuit Industry, Vanhaverbeke and colleagues (2002) explored the impact of both geographical connection and balance of direct/indirect ties for technology sourcing. The strategic choice between forming alliances and making acquisitions was found to relate to the centrality of a firm's network position, and the amount of direct ties.

Investigating the impact of different network positions has proved to be an important avenue of research in the literature. In a longitudinal study of the semi-conductor industry Stuart (1998) identified two aspects of structure, which he called "crowding" and "prestige", that both positively impacted a firm's ability to form alliances. The former consisted of network structures comprising many innovative firms, whereas those occupying "prestige" positions were those with a reputation for delivering major innovation. More recent work (Kafouros and Buckley, 2008) has however emphasised that contextual factors associated with the firm play a significant role in determining whether that firm derives maximum benefit from its position in an innovation network. Using data on 138 UK manufacturing companies from DataStream, models demonstrated that firm size, the intensity of competition and technology opportunities presented, could all impact the effectiveness of research alliances (Kafouros and Buckley, 2008).

What are the implications of all these findings for the University Science research groups that are my area of interest? The most obvious connection would seem to be that networks might provide one means by which such research groups could participate in some wider innovation endeavour. Universities, of necessity, do not possess the resources to unilaterally execute the commercialisation of their own research. One challenge for science research groups might then be to participate in appropriate innovation networks. Following Fleming's ideas (2006), university scientists might start to build relationships with their peers in industry or those in sectors that aim to facilitate innovation (e.g. Venture Capitalists). It might reasonably be expected that such relationship building would take time and an individual university researcher might only ever be able to exert a limited influence over the wider network.

This reading of the literature suggests that executing an innovation project is rather like pulling together the right bits of a jigsaw; a metaphor recognisable in Chesbrough's earliest articulation of open innovation (Chesbrough, 2003). Chesbrough notes how large firms often adopt a central co-ordinating role which sees them building new innovation networks in situations where they do not own all the necessary knowledge resources (Chesbrough, 2006). However the literature on Innovation Networks indicates that other types of research alliance structure can crystallise out of a wider network. From the perspective of a participating university researcher, how does he know that it is a good emerging project in which he might wish to participate? An academic might reasonably be expected to identify more with his science than a peer in industry (who will also identify with his firm's goals and mission). If a wider set of restrictions is relevant for the individual university research scientist, what is their nature and how do they influence the types of project pursued? In terms of alliances that emerge out of innovation network, then how might a participant know it was going to be a good alliance? How do questions of wider social and environmental impacts influence the character of emerging innovation alliances?

In starting to examine the influence of policy mandates for networks in which university research participates, this review now focuses on a stream of literature that is known as the "Triple Helix".

Triple Helix of University-Industry-Government Relations

From research exploring the relationship between modern university and wider societal and economic institutions, there has emerged an important thread of literature known as the “triple helix of university-industry-government relations” (Etzkowitz and Leydesdorff, 1997). This compelling metaphor aims to capture the essence of a relationship that is dynamic and complex. It is not intended to relate too closely to the double helix of biological evolution. Where the latter is built upon variations in a highly ordered dynamic system, at the heart of the “triple helix” metaphor is a notion of instability (Etzkowitz and Leydesdorff, 2000). Unlike the case in the biological helix, there are no structural laws governing the way in which the individual strands of the triple helix bind together. This lack of a priori order captures the complex nature of the challenges faced by representatives of the institutions as they work together on projects. The evolution of these relationships is then understandable less in terms of random mutations (as in the biological case) than mutual institutional transformations in response to environmental stimuli. The dynamic interplay between Universities, Industry and Government is viewed by advocates of this model as providing an overarching framework for understanding the societal context for new knowledge production. In this regard Etzkowitz and Leydesdorff have compared Triple Helix ideas with those of “Mode 2” (Gibbons *et al.*, 1994) knowledge production which he maintains was the original mode of science before its “academic institutionalisation in the 19th Century” (Etzkowitz and Leydesdorff, 2000).

Two strands are discernible in this literature, which may offer useful perspectives on my research interests. Firstly, there is an economic development thread that explores (amongst other issues) how innovation policy is enacted in different national contexts. Secondly, there is the emergence of the “entrepreneurial university” in which some of the commercial activity traditionally conducted by industry is witnessed within universities. This notion has become important not only for the implementation of innovation policy, but also has implications at the level of individual research groups. Papers from these two strands of the Triple Helix literature are examined below.

A comparative study of the economic policies evident in Korea and Brazil (Etzkowitz and Brisolla, 1999) positions the Triple Helix Model as providing a more relevant (in the context of technology production) alternative to either a free market or centrally planned approaches. Technology policy in such developing economies contained a strong element of importing technology. The involvement of Universities, Industry and Government was suggested to offer an alternative technology transfer mechanisms that also enabled the internal augmentation of those technologies as well as the development of new ones. In a later study of Brazilian technology policy, Etzkowitz et al. (2005) critique the country's incubator movement in relation to the Triple Helix Model. Compared with a Science Park movement of the 1970s, the co-operative nature of the incubators had, it is argued, led to their success. By active engagement with incubator projects, different Triple Helix actors were able to shape the form of the incubator in relation to their own science and technology needs. Such collaboration work led to the wider development of institutions beyond the purely economic; a process Etzkowitz terms "meta-innovation" (Etzkowitz *et al.*, 2005).

The three-fold contingency-based partnership that is the essence of this idea is proving to be an economic development framework that resonates in different national contexts. Thus studies have invoked this concept in order to describe economic development in national settings as different as Canada (Langford and Langford, 2000), Russia (Sedaitis, 2000), Zambia (Konde, 2004), Sweden (Coenen and Moodysson, 2009) and Japan (Anttiroiko, 2009). A significant feature of these papers is that they involve the partnership of all three institutions for understanding regional economic development activities. It is the institutional transformations posited within the Triple Helix Model that become the drivers for regional economic reform. Indeed, Etzkowitz and Klofsten (2005) argue that the Triple Helix model provides a generic framework for knowledge-based regional economic development that goes beyond the marketing rhetoric inevitable in regional promotion and case studies. Rather, the economic development mechanism at the core of the Triple Helix idea is that the three institutional actors collaborate in order to pursue roles traditionally undertaken by only one of them. Etzkowitz argues that the most fundamental change in this regard is the emergence of the “Entrepreneurial University” (Etzkowitz and Dzisah, 2008). Where once, the argument goes, entrepreneurship might be associated with private industry, a new partnership of Government, Industry and Universities has led to the creation of a new institutional category capable of generating new knowledge-based enterprises.

From a university perspective then the “Entrepreneurial University” represents a new institutional logic in addition to its familiar ones of teaching and research: one in which it has assumed a role in economic development (Etzkowitz *et al.*, 2000). This new role is sometimes called the “third mission” and it has emerged with the increasingly complex, knowledge-based character of innovation policy. The Triple Helix Model sees this mission being enacted through collaboration with economic partners. Thus, even when new knowledge is created in an industrial setting it is often done so in collaboration with Universities (Godin and Gingras, 2000). The two principal mechanisms associated with this new mission of academics, creating spin-out companies and university-industry partnerships, will be considered in later sections of this chapter. To draw this outline of the Triple Helix to a close, I will now explore the implications these changes for university researchers.

In parallel with the emergence of the “Entrepreneurial University”, Etzkowitz has noted an emerging group of “entrepreneurial scientists” (Etzkowitz, 1998). Born of increased interactions with industry, this idea is not to be narrowly understood as scientists becoming involved in commercial activities, but concerns how they perceive the role of scientist. From the perspective of my research interests, the most significant extension of this idea is the notion of university science research groups being understandable as “quasi-firms” (Etzkowitz, 2003): “Research groups operate as firm-like entities, lacking only a direct profit motive to make them a company. In the sciences, especially, professors are expected to be team leaders and team members, with the exception of technicians, are scientists in training” (Etzkowitz, 2003, p.111).

The tensions inherent in balancing the familiar role of teaching and research with a new role in economic development has been explored will reference to the literature on ambidexterity in organisation theory (Ambos *et al.*, 2008). These tensions have become manifest with the response amongst UK academics to Hefce's proposals for the Research Excellence Framework that I outlined in Chapter 1. Based upon a dataset of 270 EPSRC-funded projects, Ambos et al (2008) studied this ability to manage dual roles at the level of the university (organisation level) and individual researchers. Using the formation or not of a spin-out company, these authors demonstrated that the challenge of being ambidextrous was being met at the level of the University, but not (to a first approximation) by individual researchers. They found, in keeping with other groups (Shinn and Lamy, 2006) that some scientists are capable of achieving both academic and commercial success, but such people seem to be in a minority. Consistent with an important theme of the organisational ambidexterity literature, universities have managed these competing demands by the creating "dual structures". This has usually meant the formation of "Technology Transfer Offices" to complement the establish Research Support Offices. It remains an open (and important) question to understand how individual scientists manage the dual challenges of academic excellence and commercial relevance.

In order to explore how natural scientists surmount their own dual hurdles, some research has focussed on the phenomenon of "star scientists" (Zucker and Darby, 2001). Other research (Shinn and Lamy, 2006) has identified the need for a more nuanced consideration of the behaviour of "scientist-entrepreneurs", and suggests different paths of commercial knowledge creation may be appropriate. I suggest that the question of how exposure to the commercial environment informs the management of academic research is worthy of further study.

It is worth pausing to consider the implications of these studies for the Research Excellence Framework (see Chapter 1) that will be used to assess in 2014 the performance of university research and its impact. Hefce's proposals operate primarily at the level of university "units of account" (defined in terms of cognate disciplines), and at this level the teachings of Ambo's research (2008) would suggest that it is possible to successfully reconcile academic and Research Impact aims. However, the analogous message from the UK Research Councils has been targeted at the level of individual researchers where Ambos shows the achievement of these "dual hurdles" is more difficult. One contemporary reading of Ambos' research suggests that universities should seek to develop REF strategies around balanced portfolios of academic excellence and wider impacts based on the differing contributions of individual academics.

Bringing this sub-section on the Triple Helix to a close then a key lesson is that there are no *a priori* best approaches to connecting university research with government or industry. That is to say, university researchers should not simply respond to calls from government or demands from industry: the relationship is mutual and dynamic. The whole derives strength from the re-enforcing nature of interactions between different strands; but they do have to interact. At the level of the individual university research group then I adopt the view that such groups represent an entrepreneurial activity in itself regardless of the extend of actual commercial connections (cf. Etzkowitz, 2003). This analogy opens up the gamut of management and organisational thinking that is normally brought to bear on commercial enterprises. This perspective of 'quasi-firm' makes it natural to questions how these the research enterprises are managed in order to respond to calls for societal and/or economic relevance. Before pursuing this perspective further I now delve more deeply into the two dominant mechanisms for the commercialisation of university science: the formation of spin-out companies and university-industry partnerships.

Academic Entrepreneurship

Introducing the discussions in this chapter I noted that contemporary scientific knowledge is complex and dispersed. Making use of it to do the work of innovation must increasingly involve recourse to networks. At a macro or policy level, the Triple Helix literature argues that it is possible to discern new institutional forms being created at the intersection of Government, Industry and Universities. These forms seek to complement, and in some cases, extend the existing structures of innovation practice. In the remainder of the chapter I turn attention to more micro- and organisational-level phenomena in order to discuss how individual academics (and their research groups) have experienced the above changes in the nature of innovation work. In the first instance I consider the impact of policy mandates that universities adopt a more active position regarding the commercialisation of academic research. Such commercial activity was given a significant impetus in the USA with the passing of the Bayh-Dole Act in 1980. This act gave universities responsibility for exploiting intellectual policy generated during the course of their research programmes. The policy idea quickly crossed the Atlantic and became an increasingly notable part of the university landscape in Europe. The history of these policy developments and the emergence of the practice of academic entrepreneurship has been described for the USA (Slaughter, 2004) and Europe (Wright *et al.*, 2007).

The primary literature on academic entrepreneurship has been the subject of a very extensive review (Rothaermel *et al.*, 2007) structured around four major themes:

1. entrepreneurial research university
2. productivity of technology transfer offices
3. new firm creation
4. environmental context including methods of innovation

Within each of these major themes, Rothaermel *et al.* identify specific categories of research as well as suggesting areas for future studies. More generally, they concluded that this area has reached a sufficient mass to constitute a research field for more general topical questions in management research. They finish with the call that "by addressing key disciplinary questions in the context of entrepreneurial universities, scholars can help to move this fascinating research stream into the premier mainstream journals in their respective disciplines, and in turn enhance the visibility and build impact for this important new field of research" (Rothaermel *et al.*, 2007, p.778).

It is not my intention here to conduct a literature review that is as comprehensive as that of Rothaermel and his colleagues. My aims are to understand how this literature might inform the practice and policy issues identified in the earlier chapters. I also nest this particular literature within ones that I see as operating on a wider scale (Figure 1). Nevertheless I note that with innovation networks and the triple helix I have considered literatures that overlap with Rothaermel's first and fourth themes.

In Table 1 I outline some of the themes that have emerged so far in this thesis document and draw connections with areas of future research suggested in the course of Rothaermel's review.

Table 1- Research themes emerging in this report and those identified in the Review of the Academic Entrepreneurship Literature of Rothaermel et al. (2007)

Research themes emerging in this thesis	**Connections to areas of future research suggested by Rothaermel et al. (2007)**
How do individual research groups balance the competing demands for academic success and research impact?	"A deeper understanding of these issues (i.e. the effectiveness of certain [balancing] strategies, impact of entrepreneurial activities on university governance etc.) calls for a more rigorous analysis, such as longitudinal studies across different universities and different contexts" (*ibid*., 2007, p. 739)
How do research leaders ("entrepreneurial scientists") influence these activities?	"How do the nature and stage of the technology affect the process and outcome of linking surrogate entrepreneurs or the effectiveness of coaching by the original founder"? (*ibid*, p. 763) "Prior research, however, has not addressed what particular advantages a spin out firm would gain from one type of spin out versus alternative firms given the founder's social network". (*ibid* 2007, p. 764)
How do diversity of networking links affect the research groups' projects?	"Other future areas for study in this research stream [environmental context] pertain to a deeper understanding of the diverse linkages in networks of innovation". (p. 776)

In the remainder of this sub-section I will concentrate on the three themes in Table 1 and draw upon literature appearing since Rothaermel's review (or literature not used by them).

It was noted in the section on Triple Helix that the ability of universities to balance traditional research/teaching missions with an emerging entrepreneurial agenda is not always realised at the level of individual researchers (Ambos *et al.*, 2008). The necessity of organisations to display ambidexterity in order to manage the conflicting demands for current operations and innovation is well established (Duncan, 1976). Duncan's core argument was for "structural ambidexterity" in which different structures are created within the organisation to manage (at least in our interests) innovation and traditional activities. Developing this further, Birkinshaw has coined the term "contextual ambidexterity" (Birkinshaw and Gibson, 2004) which involves employees making behavioural choices during their day to day work. Chang and co-workers (2009) have applied these ideas to the context of academic entrepreneurship in a study of academic patent inventors in Taiwan. This study concludes that whilst both types of ambidexterity foster academic entrepreneurship, the more personal elements (e.g. entrepreneurial capability) that underlie contextual ambidexterity seem to be more significant. Adopting a citation analysis (a methodology that seems to dominate studies in this area) of researchers at Max Planck Institute Directors Buenstorf (2009) has provided further evidence for the conclusion that inventing does not harm the publishing record of leading academic researchers. Such quantitative analysis, whilst demonstrating that balancing traditional academic demands with the emerging entrepreneurship agenda is possible, give little insight into how the difficulties encountered by individual researchers identified by Ambos et al (2008) may be resolved. In this regard other research methodologies may have new insights to offer.

Through the compilation of rich case studies in two areas of university research, Tuunainen and Knuuttila (2009) have detailed the conflicts inherent in an effort to maintain boundaries between the traditional and entrepreneurial agendas. In doing so they have argued that the nature of these boundaries is complex and cannot be created at will in order for researchers or universities to pursue varied agendas. They draw particular attention to enduring commitments at universities that are part of their culture, as well as the existence of legal mandates that prescribe what is possible. These arguments for the former certainly resonate in the positions adopted recently in the UK by the voices opposed to the REF proposals outlined in Chapter 1.

Adopting a cognitive psychology approach, Jain *et al.* (2009) identify different strategies that university researchers adopt to make sense of these emergency dual roles. Their argument is that such researchers have a core identity built around their understanding of the traditional academic's role. Two mechanisms – "delegating" and "buffering" – can be adopted to ensure the primacy of the traditional role whilst at the same time engaging in activities associated with the entrepreneurial agenda. Delegating involves "establishing appropriate interfaces with other actors" (ibid, p. 929), and buffering involves scientists taking "steps to protect their role identity from the influence of norms typically associated with commercialisation" (ibid, p. 930). By placing the behaviour of the academic scientist/entrepreneur at the heart of their study, these authors take a perspective that seems to have been relatively neglected in the literature on academic entrepreneurship. This same neglect was noted by Link et al. (2007) and was the starting point of their investigation into the factors that drove academics to participate in technology transfer activity. This study noted that male, research active academics in secure university positions display a greater propensity to participate in such entrepreneurial activities. The gender bias was also noted in a separate study by Murray and Graham (2007).

In efforts to generalise such findings, there have been a number of typologies proposed to characterise individuals engaging in academic entrepreneurships. Etzkowitz and Leydesdorff (1997) suggested a framework based along dimensions that firstly considered the compatibility of the individual's approach in relation to traditional university norms, and secondly he viewed the willingness of the individual to build new institutional structures in order to realise their entrepreneurial goals. These dimensions are presented in Figure 2 along with the names he gave to the ensuing categories.

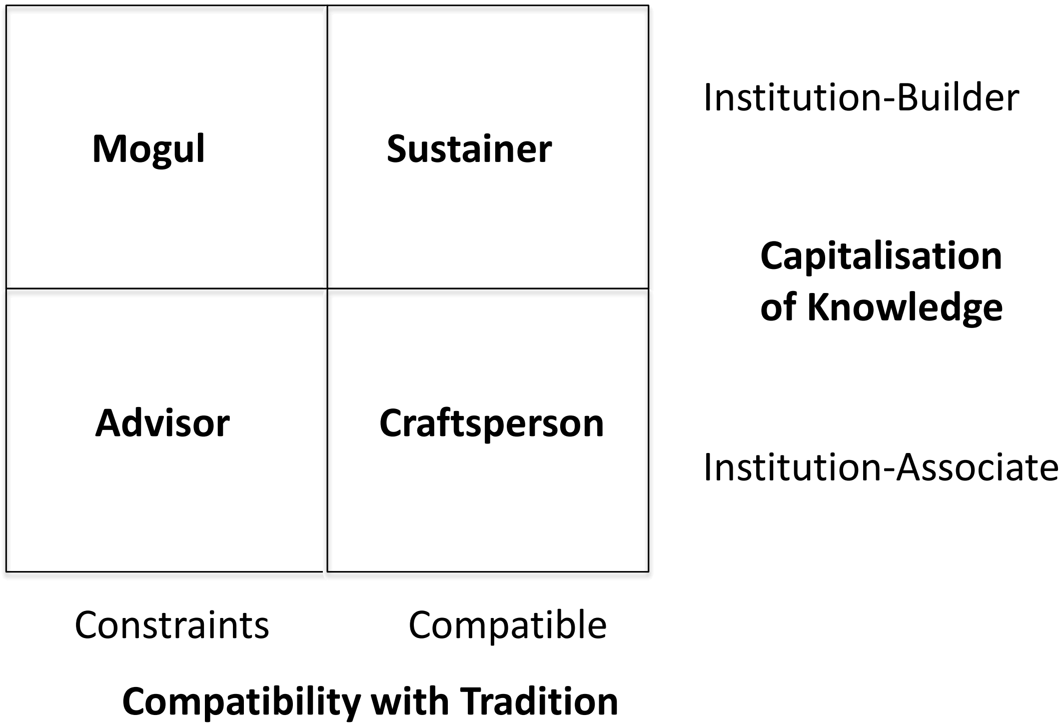

Figure 2 - Typology of scientist-entrepreneurs (source: Etzkowitz and Leydesdorff, 1997)

Etzkowitz's 'Mogul' seems to be an entrepreneur at heart who happens to have started his career in the academy. His focus is to maximise personal returns (financial and otherwise) from his business endeavours. The 'Sustainer' also wants his company to develop but primarily to support and inform his university research. The "Advisor" looks to others to grow the company, preferring himself to remain in a consultative position. The "Craftsperson" has a more exploratory mind-set that seeks the integration of science and business. Through this discussion a picture has emerged in which the new entrepreneurial agenda is something which either supersedes traditional academic roles, or for which the academic must find strategies to pursue without compromising his traditional role. It therefore becomes interesting to consider whether these two channels for intellectual scientific activity need to be viewed as just that, discrete avenues, or whether they can in fact inform each other.

This idea of mutuality was explored by Shinn and Lamy (2006) who, based on a study of scientists within the French public research agencies, present a more nuanced understanding of how scientists navigate amongst the demands of academic science and its commercialisation. Their four year longitudinal study of scientists engaged in commercial activities allowed them to identify three “paths of commercial knowledge” that differ in the way in which the competing demands on the academic’s time are co-ordinated. Shinn and Lamy maintain that the boundaries between science and business are real, and their typology of academic-entrepreneurs could be thought to represent an example of the contextual ambidexterity discussed earlier. They distinguish between the co-ordination modes in terms of issue of business-university synergy and tension plus the relative autonomy (from the business) of the scientific field. Their typology includes three terms:

- Academics who privilege their university work and take a ‘strategic’ perspective on their enterprise activities (i.e. what advantages can it bring to their university research)
- Pioneers are the opposites of academics and privilege their enterprise activities
- Janus types alternate their efforts between university science and enterprise activities.

Regardless of the specific typology that one might advocate, they all suggest a complexity of positioning of the scientist-entrepreneur that, as noted by Ambos et al (2008), defies a simple structural solution like those possible at the wider level of the university. By extension, simple (structural) solutions to the recent calls for UK university science to be more responsive to the needs of the economy and society, may only be possible at the level of the university itself. The response of individual scientists might be expected to be less straightforward. The expectations of policy-makers and university administrators may need to be similarly nuanced.

These policy challenges at the level of individual researchers beg the question of how they develop the competencies needed to participate in such commercialisation activities. By employing a questionnaire methodology, Krabel and Mueller (2009) recently explored the attitudes of 2,604 German scientists in Max Planck Institutes. This study unearthed a variety of supportive factors, including personal outlook about the commercialisation of science and experience of working with industry. Such connections with industry will be the subject of the next section of this chapter. For the current discussion it is simply worth noting that academic entrepreneurs are known to gain significant benefit from the experience with industry. This might involve direct support to overcome the resource and expertise shortfalls (Wright *et al.*, 2004). Alternatively, interacting with a range of commercialisation actors has been shown to build absorption capacity necessary for successful innovations (McAdam *et al.*, 2010).

The connection between an academic's enterprise activities and their network of experience was noted in a special themed section in the *Journal of Management Studies* (Markman *et al.*, 2008b). In their introductory paper the Editors of the special edition comment that "our synthesis calls for additional research on the heterogeneity of academic entrepreneurs' social capital, business acumen and academic discipline IP, and how this feeds through into the process of searching for scientific discoveries that can be commercialised" (Markman *et al.*, 2008b, p.1413). In light of the policy discourse of Responsible Innovation, I suggest the challenge of their research suggestion can be widened to explore creation of societal value.

University – Industry Collaborations

In this final sub-section I examine a complementary set of innovation practices that do not involve the academic creating a new enterprise. These mechanisms are ones in which academic researchers have sought to realise their innovation ambitions by participating in collaborative projects with industrial companies. Macro-level ideas relating to such collaborations are evident in the Triple-Helix literature and innovation networks literature discussed earlier in the chapter. In the following discussion I continue the micro-level and organisational-level examination of the innovation practices adopted by individual academic researchers and their research groups. Studies relating to the specific characteristics of industry-university collaborations have been the subject of a number of published literature reviews. This sub-section will summarise the key features identified in these reviews, along with areas their authors argue merit further investigation. Finally, primary literature not covered by the reviews that relates particularly to my emerging area of interest will be discussed.

The first review considered here adopts the perspective of "university-to-industry knowledge transfer" (Agrawal, 2001) and divides the literature along 4 themes:

o Firm characteristics: these studies, Agrawal argues, invariably considered a single industry and were dominated by efforts to understand how firms have increased their absorptive capacity (Cohen and Levinthal, 1990). He felt more work was needed on other mechanisms for establishing innovation connectedness.

o University characteristics: this area was dominated by the effects of the Bayh-Dole Act (as the literature considered dealt largely with US universities), and the management of patents by universities. He highlighted the need for more work on the entrepreneurship behaviour of academic inventors (as seen in the last section, this is a research field that has become increasingly popular).

o The importance of geography: here he drew attention to the large body of work demonstrating the benefits of being located close to the seat of knowledge production, even in the face of the growing use of the internet as a communication tool. Such economic impacts are sometimes called "spillover effects", i.e. benefits to the local economy that result from a major investment: in this case, investments in university research that is subsequently the basis for industry collaborations.

o Channels of knowledge transfer: as noted above, the use of patents seemed to be the dominant instrument of universities, and so had attracted most study. He felt it would be interesting to know more about how such mechanisms varied with university policy and national intellectual policy regimes. He also advocated the need for more research on other channels.

The second review considered here adopted the perspective of the economic benefits of basic, public-funded research (Salter and Martin, 2001). In keeping with a keen interest from policy makers this report was commissioned by UK Government Treasury Department. This review categorises the literature along methodological lines, i.e. distinguishing between studies based on econometric methods, survey instruments and case studies. Despite technical and conceptual challenges associated with the econometric methods, there is general consensus that economic benefits are substantial. This stream of literature has also drawn attention to “spillover effects” of publicly funded knowledge creation that was one of Agrawal’s (2001) literature categories. Examining the research that employed surveys and case studies, Salter and Martin identify six different ways in which economic benefits of university-industry collaboration are manifest:

1. “increasing the stock of useful knowledge;
2. training skilled graduates;
3. creating new scientific instrumentation and methodologies;
4. forming networks and stimulating social interaction;
5. increasing the capacity for scientific and technological problem-solving;
6. creating new firms. “

(Salter and Martin, 2001, p.520)

In this work Salter and Martin show that the realisation of economic benefits from university research operates in varied and nuanced ways. They find no evidence for a simple linear transfer of knowledge from university to industry. As Salter concludes "these [economic] benefits are often subtle, heterogeneous, difficult to track and measure, and mostly indirect" (Salter and Martin, 2001, p.528). These are difficulties are salient for current debate on articulating impact for the Research Excellence Framework proposals (see Chapter 1). The ways in which academics will realise an impact from their research are unlikely to involve simple (even if new) strategies involving the dissemination/transfer of research. The extent to which this is appreciated remains unknown, as the outlook persists that views knowledge production, transfer and use as a linear process. This view contends that academics produce new knowledge, it is transferred to industry (by some means) and industry puts it into use. Salter and Martin's work confirms that the emerging innovation practices are more complex than that. Varied mechanisms requiring new collaborative skills (on the part of both academics and industrialists) are implied.

The apparent breakdown of a linear model of production-transfer-use has been evident throughout this chapter. From the complex and distributed nature of modern scientific knowledge requiring networks of innovation actors, to the new institutional arrangements of the Triple Helix literature, to the micro-level innovation practices of individuals. Such changes have implications for how new academic knowledge is created in the first place. This is a thought that Salter alludes to briefly by introducing the idea of a shift in the nature of knowledge production to a “Mode 2” orientation (Gibbons *et al.*, 1994).

In “The New Production of Knowledge”, Gibbons *et al.* (1994) characterised changes in the manner in which science is being conducted. They identified five dimensions of knowledge production along which this change could be tracked (Table 2).

Table 2 - Characteristics of Mode 1 and Mode 2 knowledge production (Source: Gibbons *et al.*, 1994)

Mode 1	Mode 2
Academic Context	Context of Application
Disciplinary	Trans-disciplinary
Homogeneity	Heterogeneity
Autonomy	Reflexive/socially accountable
Traditional quality control (peer review)	New forms of quality control

Thus, Gibbons and colleagues argue that in a Mode 2 approach science is inseparable from the context in which it is produced. No longer is the challenge one of producing rigorous academic science and then following it with some process of knowledge transfer. Modern science, they maintain, is now conducted in different locations, linked through networks that differ institutionally (heterogeneity attribute) and by discipline (trans-disciplinary attribute). This complexity naturally gives rise to new criteria against which to assess the quality of science. Finally, the networked nature of knowledge production makes for a process in which the views of different voices must be incorporated. Individual scientists are thus expected to become more aware of these other voices and build the others' needs into their research. The policy discourse of *Responsible Innovation* and networked nature of modern innovation practice being explored in this thesis might readily be expressible in the language of Mode 2 knowledge production.

The conceptual basis and empirical evidence for such a change in the mode of knowledge production has been subject to much criticism. Hessels and van Lente (2008) have produced an excellent critique of the "Mode 2" ideas that includes a literature review of alternative perspectives and "systematic reflection" on each of the central attributes (Table 2). They conclude their review with a research agenda to "re-think new knowledge production". In relation to the emerging research questions being considered in this thesis, one of their three research questions seems particularly relevant: "Are university scientists in general increasingly reflexive, in the sense that they are aware of the potential societal effects of their research and take these into account in their choice of research objects, methods and approaches?" (Hessels and van Lente, 2008, p.758).

The most comprehensive literature review of university – industry relationships has been provided by Perkmann and Walsh (2007). They frame their review by suggesting a typology for university-industry links based upon the "extent of relational involvement" (Perkmann and Walsh, 2007, p.263). Their focus is then upon mechanisms with a high degree of such involvement: research services and research relationships. Most specifically they concentrate upon university-industry relationships as they involve a degree of interactivity that is more connected to the wider literature on innovation networks (neatly bringing us full circle in this current overview of relevant literatures). The main conclusions of their literature review are as follows:

o a wide variety of mechanisms operate

o innovation partnerships that originate with a piece of codified intellectual property are only of "moderate importance"; relationship-based innovations are more significant

o knowledge that originated at universities is not simply used at the outset of an innovation project but is also used at its later stages

o firms have a variety of motivations (not simply finding readily commercialisable ideas) for participating in such relationships.

Interestingly, from a methodology perspective, the authors note that most of the existing research in this area preferences quantitative data: often based on citation analysis of patents and publications. They maintain that more emphasis is now needed on unearthing the rich details of university-industry collaborations. Having completed this literature review, Perkmann and Walsh propose a research agenda. In this they are particularly influenced by the open innovation research agenda suggested by Chesbrough and colleagues (2006). In keeping with the core ideas of open innovation they primarily take the perspective of the firm in their proposals, and suggest a number of research topics under the headings of: (1)

search and match processes and (2) organisation and management of relationships (Perkmann and Walsh, 2007, p.273). These authors pose the question of whether institutional-based changes should be made in order to make academics more responsive to industry; something that speaks to the heart of the current Responsible Innovation debates. It is interesting to also consider their research agenda from the perspective of an individual university researcher (and his/her research group). How do the challenges of search/match and relationship management fit within the wider management of their research effort? In this regard, it is relevant to consider the effect of participation in such activities on fundamental research; a question over which the literature is divided; there are those who view the relationship as reinforcing (Van Looy *et al.*, 2004, Buenstorf, 2009) and others who find unintended negative effects (Geuna, 2001, Thursby and Thursby, 2004). As I noted in Chapter 1 the most vociferous opponents of the REF argue that such activities fundamentally compromise the core rationale and contribution of universities.

Concluding Remarks

In this chapter I have explored academic literatures that parallel the earlier contemporary debates about science and its wider consequences. In doing so I have uncovered ample evidence for the importance of university science in the modern innovation landscape. I note here that the majority of the general literature on innovation management necessarily focuses on firms because they have the resources and capabilities necessary to execute innovation projects. And yet this chapter has also shown how changing institutional arrangements have created an opportunities for individual university researchers and their groups to do innovation work. These trends are noted particularly in advanced new areas of emerging technology such as biotechnology (Murray, 2002, Kleinman, 2003, Owen-Smith and Powell, 2004, Rosenberg, 2009) and nanotechnology (Foster, 2006, Bozeman *et al.*, 2007, Pandza and Holt, 2007). However, this importance is largely expressed in economic terms; at best societal benefit is assumed to automatically flow from economic improvement. Emerging policy discourses of *Research Impact* and *Responsible Innovation* place an increasing emphasis on addressing wider societal and environmental interests. How then are these widening areas of concern influencing the conduct of the innovation activities described in this chapter? How are emerging policy discourses influencing the innovation activities of research groups operating in these emerging technology fields? Answers to these questions imply a study of change: what are the ways in which the organisation of innovation by research groups are changing? In order to progress an understanding of these phenomena the next chapter reviews a management literature of organisational change.

Chapter 3
Organizational Capabilities and their Development

The empirical literature discussed in the last chapter is rarely framed using strong theory. Indeed some writers have drawn attention to what they label the 'atheoretical' character of papers published in academic entrepreneurship (Rothaermel *et al.*, 2007, p.706) and the “predominantly phenomenon-focussed studies” of university-industry collaborations (Perkmann *et al.*, 2013, p. 425). However, the same authors argue that stronger theoretical underpinnings hold out the prospect for a deeper understanding of the empirics as well as contributing to discussions of theory. With this same ambition in mind, this chapter introduces a literature that may be instructive in both a consideration of the strategic challenges felt at the level of individual research groups, as well as examining how individual innovative groups approach the requirement to change in the face of new external mandates. The emerging questions from this study can be expressed as follows:

How are university research groups responding to mandates that they should be actively concerned with the economic and societal impacts of their research?

What new research and innovation practices are evident amongst research groups working in areas of emerging technologies?

These questions would benefit from a theoretical framing that explains both the ability of research groups to do innovation, and for them to change those abilities in the face of new policy mandates. Such a framing may be possible by invoking the strategic management literature on organisational capabilities (Dosi *et al.*, 2000). Organisational capabilities exist (conceptually) at the interface between the occurrence of external changes (e.g. Peteraf and Reed, 2007) and the associated reconfiguration of internal resources

(Barney, 1991). Whilst recognising that the configuration of a firm's resources may have become established as a set of firm-specific practices, this concept allows us to understand the change in those practices in response to external influences. Crucially the idea of capabilities allows an explanation of change in conditions where the exact nature of that change remains ambiguous, but still requiring of a response from organisations. In this study I suggest that the policy debates concerning research impact and responsible innovation constitute such an external driver for change for how university research groups are to *do* innovation.

Strategic management researchers have invoked and developed the concept of organization capabilities in order to explain how firms (for it is largely firms who are studied) maintain a competitive advantage over their rivals in the face of changes in their operating environment. This concept may prove instructive in my study particular if one adopts the perspective (noted during the Triple Helix discussions in Chapter 2) that university research groups might be understood as 'quasi-firms' (Etzkowitz, 2003). That is to say, organisations engaged in purposeful and imaginative activity, in relation to which they compete with their peers for resources and rewards. Such activity is founded upon their research, but increasingly as we have noted in this thesis, the range of activity is being extended into new arenas. However, I do not wish with this perspective, to automatically imply overt commercially-related activity (e.g. academic entrepreneurship or university-industry research collaborations). Such activities may be part of the enterprise mix, but I would also view the academic pursuing an extreme "blue-sky" research agenda as leading an enterprise. University research groups manage resources that mean, regardless of research philosophy and agenda, they may usefully be viewed as enterprises. This perspective is one that creates an opportunity for insights from more strategic management research literatures that result from studies of commercial firms.

This thesis is concerned with the response of research groups/enterprises faced with calls to make their innovation endeavours more responsive to a wider range of influences than the purely economic. My contention is that these putative changes in innovation practice may be understood and explained if framed in relations to their organizational capability for technological innovation (cf. Eisenhardt and Martin, 2000). Recognizing that major reviews are available for the conceptual and empirical literature in this subject, this chapter offers only a short introduction to the area. My aim is to introduce some of the key ideas concerned with this strategic management concept, and in particular how capabilities have been linked with strategic change. I also seek to show how the idea of a capability for change itself (known as a *dynamic capability*) remains a confusing concept, and one that seems to have been the subject of more conceptual elaboration than empirical study. In trying to resolve these difficulties I suggest that the idea of capability development being understandable in terms of life-cycle changes is most useful *for this PhD project*. However, because any application of these ideas to my research setting should remain cognisant of the extent to which research groups/enterprises are invariably identified with a single research leader, I finish the chapter by briefly examining questions of managerial agency. I note the influence of managerial agency on capability development remains contested in this literature.

Organizational capabilities are a well-established concept in the field of management and organizational studies (Dosi *et al.*, 2000). They have emerged from the “resource-based view of the firm” (RBV), which maintains that advantages accruing to firms result from the distinctive internal organization of resources within the firm itself (Barney, 1991). They have been variously defined in relation to this core idea. Such definitions are reproduced throughout this chapter in an effort to convey nuances in the conceptual apparatus that has been built over time; and because different terms are sometimes used to label the same concept. However, at the outset of this discussion Winter’s definition identifies key features of the concept and is a good place to start: “an organizational capability is a high-level routine (or collection of routines) that, together with its implementing input flows, confers upon an organization’s management a set of decision options for producing significant outputs of a particular type” (Winter, 2000, p.983). In this definition we read that capabilities are built of smaller units of activity (routines), and that they give options for strategic action. More intriguing from the networked perspective of university research groups is that offered by Pandza and Thorpe: “collectively held and action-oriented knowledge that enables firms to get things done” (Pandza and Thorpe, 2009, S118). However, again in this definition we read the idea that capabilities are built through some collective endeavour and that they are geared toward action. In relation to this thesis, the pertinent questions to ask are: what constitutes an organisational capability for a university research group/enterprise to do innovation?; and how are such capabilities changing options for action in light of policy mandates for them to be more responsive to both economic and societal needs?

The work of research groups has at its heart the creation and development of new knowledge. Knowledge and knowledge-creating activities are the starting point invoked by Iansiti and Clark (1994) for how new capabilities are built. Recognising the complex nature of a firm’s total capability base these authors emphasise treating the evolution of those capabilities in an integrated manner rather than piecemeal. They draw attention to the firm’s capacity for action and the associated knowledge, which provides the ultimate basis for this action. They investigate how generating new knowledge to undertake actions in the development of new products involves a host of problem-solving activities. Such problem-solving provides a dynamic character to their model as it is the means through which the firm builds a capacity for action, and is able to response to changes in its operating environment. The outcome of such actions may be expressed as the dynamic performance of the firm and defined (Iansiti and Clark, 1994, p.564) “by the consistency of its achievement of positive performance in critical competitive dimensions”. On the basis of extensive case study data drawn from the new product development projects in the automobile and mainframe computing industries, they identify a capability for integrating activities as being significant for the realization of dynamic performance. This integration capability is comprised of elements that are both internal and external to the firm. Internal integration is concerned with the execution of projects, and involves cross-functional and problem-solving activities. External integration is focused on new product concept development and involves routines to incorporate knowledge from customers and new technology. They conclude (Iansiti and Clark, 1994, p.602) that “the essence of integration is the generation, fusion and accumulation of knowledge: the capacity to merge new knowledge about the impact of possibilities with deep accumulated knowledge of the complex capability base of the organization”. The work of university research groups may readily be understood in these terms, and it is the very act of doing such work that their capability for innovation resides.

Winter identifies learning within the organization as being the essence of capability development (2000). In doing so he initiates a thread of the capabilities literature that has been developed extensively . The core of his position is that capabilities result from experimental learning, but he asks what happens when such overt learning stops? Re-ignition of aspirations may prompt further overt learning as a result of a crisis or pursuit of a continuous improvement programme. He also suggests that capability development is responsive to the surrounding social system: “As they learn new capabilities, organizations draw on the society around them for both means and ends….The ends include, at the highest level, socially legitimated organizational goals” (Winter, 2000, p.994). This speaks directly to the new policy pressures for university research groups/enterprises to be more responsive to a wider set of societal drivers.

However, if as some of the more inflammatory rhetoric suggests, the impact agenda represents a fundamental threat to universities (e.g. Collini, 2012), it could be that current capabilities for innovation risk obsolescence. For example, the inability to articulate 'pathways to impact' in grant applications (cf. Chapter 1) will make it increasingly difficult for research groups to pursue their research and innovation agendas. More generally, if the research enterprise’s capability for innovation has become obsolete (because of radical policy change) then explanations of capability change based upon prior learning may still be necessary, but insufficient to account for how new capabilities are developed. To address such questions of obsolescence Teece and Pisano (1994) sought to extend the capability concept by exploring how firms both acquire and sustain advantages over their rivals. They start by identifying capabilities as the distinctive and hard-to-replicate strategic foundation of the firm. Such capabilities are understood as the “managerial and organizational processes, its present position and the paths available to it” (Teece and Pisano, 1994, p.541). A different type of capability labelled “dynamic capabilities” are then conceptualized as “the subset of the capabilities which allow the firm to create new products and processes, and respond to changing market conditions” (Teece and Pisano, 1994, p.541). In their explanation of what activities actually constitute dynamic capabilities, they draw particular attention to the integration of different assets or “positions” (technological, financial, complementary and locational). Such efforts at coordination provide an opportunity for organizational learning and thereby the creation or development of new capabilities. However, the authors strike a note of caution that “a firm’s previous investments and its repertoire of routines (its ‘history’) constrains its future behaviour” (Teece and Pisano, 1994, p.547). For these authors technology, including both that derived from its own R&D work and that exogenous to the firm’s sector, provides the opportunity for breaking free of their path dependency (Sydow *et al.*, 2009).

Teece's explanation of how organizational capabilities may be changed is to suggest that they exists on multiple conceptual levels within the firm. There are everyday 'organizational capabilities' which are the very stuff of a firm’s operations, and then there is another level of capability ('dynamic capabilities') which come into play at times of change. This notion of different levels of strategic capabilities is one discussed by a number of authors, and is examined here as it is important to a discussion of how organizational capabilities might be changed. Table 3 summaries the authors who have argued for organizational capabilities existing at different (conceptual) levels within a firm.

Table 3 – Different levels of organisational capabilities

Study	Level 1	Level 2	Level 3
Collis, 1994	Capabilities (static, dynamic or creative) whose main characteristics are that they are embedded in firm routines and involve transformation of inputs to outputs within the firm.	Capabilities concerned with dynamic improvements to the activities of the firm.	"meta-capabilities" or capabilities to develop capabilities that "allow the form to overcome…the inimitability of lower-order capabilities" (p.149).
King and Tucci, 2002	"operating routines" from "static experience" (p.172)	"change routines" from transformational experience" (p.172)	
Danneels, 2002	First order-competencies are the "tangible resources needed for…addressing a certain group of customers" (p.1112)	"integrative competencies" are "the ability to combine first order competencies" (p.1114)	"second-order competencies [are] the competence to acquire first order competences". They refer to "the ability to learn new domains" and may mitigate against path dependence (p.1097)
Winter, 2003	"Zero-order capabilities. Also known as Operational or Ordinary "make a living" capabilities (p.993)	Dynamic capabilities which "operate to extend, modify or create ordinary capabilities" (p.991)	Higher capabilities that operate on dynamic capabilities.
Zahra *et al.*, 2006	A substantive capability refers to the "set of abilities and resources that go into solving a problem or achieving an outcome" (p.921)	A dynamic capability refers to "the ability to change or reconfigure existing substantive capabilities" (p.921)	They draw attention to the agency of the firm's key decision-makers to envision and enact a reconfiguration of a firm's resources and routines.
Danneels, 2008	"first-order competencies" specific to "a certain domain of knowledge and skill" defined as a "configuration of resources that enables the firm to accomplish a particular task" (p.520)	"second-order competencies" involving an "ability to learn new domains" (p.520)	

In the earliest of these papers, the idea of capabilities existing at many levels is problematised by Collis (1994). Whilst defining capabilities as “socially complex routines that determine the efficiency with which firms physically transform inputs into outputs” (1994, p.145), Collis discerns three different categories of definition in the contemporary literature. These are categories of definition that emphasise basic firm functions, the incremental improvement to the organizational work within the firm or the creation of novel activities. He further identifies a discussion of meta-capabilities, or capabilities to develop capabilities. Conceptualising such meta-capabilities he argues might lead to an infinite regress in which capabilities are sought that “innovate the innovation that innovates the innovation that innovates…” (Collis, 1994, p.148) and so on *ad absurdum*. For him, such infinite regress is a fatal flaw for an explanation capability change that invokes different levels. His proposal to overcome this problem is to emphasise the contextual dependence of valuable capabilities.

Notwithstanding these early criticisms of conceptualising capabilities at multiple levels, other authors have continued to work with the idea that a special category of capability is a useful explanation for how an organisation responds to and effects change. For example, Danneels (2008) is not as dismissive of the value of such higher order capabilities and believes that they can be built by leveraging other organizational competencies. He proposes a second-order competence which he defines as “the ability of the firm to engage in exploration, that is, to build new competences” (Danneels, 2008, p.520). Using panel data from US manufacturing firms he identifies five organizational antecedents that foster such exploration: willingness to cannibalise, constructive conflict, tolerance of failure, environmental scanning and resource slack.

Teece has continued to develop his early ideas on dynamic capabilities and positioned them in relation to the other major theories of strategic management: attenuating competitive forces, strategic conflict and resource-based perspectives (see Table at Teece *et al.*, 1997, p.527). In doing so, he and his co-authors argue that dynamic capabilities are in essence “resident in the firm’s organizational processes, that are in turn shaped by the firm's assets (positions) and its evolutionary path" (Teece *et al.*, 1997, p.524). This paper seems to have provided the springboard for a research agenda that has proved one of the most popular in the whole strategic management literature (Helfat *et al.*, 2007). The literature has been the subject of a number of substantial literature reviews (Zahra *et al.*, 2006, Wang and Ahmed, 2007, Ambrosini and Bowman, 2009) and so space is given here only to some of the more influential studies.

Eisenhardt & Martin (2000) hold a different view of dynamic capabilities than Teece (1997). They have argued that because "functionality of dynamic capabilities can be duplicated across firms, their value for competitive advantage lies in the resource configurations that they create and not the capabilities themselves" (Eisenhardt and Martin, 2000, p.1106). In this manner dynamic capabilities directly impact firm performance rather than doing so indirectly by means of them acting upon (lower-level) operational capabilities. In this conception dynamic capabilities are specific processes like product development (Eisenhardt and Martin, 2000), inter-firm partnerships (Ettlie and Pavlou, 2006, Kale and Singh, 2007) and strategic decision making. Crucially they also argue for an evolutionary dependence on market dynamism. In less dynamic markets these capabilities are similar to routines and the emphasis for their development relates to variation mechanisms. In dynamic markets they are highly experiential and unpredictable: in such situations managing selection is critical to the evolution of capabilities. The emergences of dynamic capabilities are path dependent and fundamentally based on learning. Particular learning features include repeated practice, mistakes and the pacing of experience. In these terms one might expect that the slow rate of change evident with the University sector means that the phenomena explored within this thesis are not concerned with dynamic capabilities as they do not operate in dynamic markets.

Experiential learning that is cumulative was also invoked by Zollo and Winter (2002) in their theorizing of how organizations develop dynamic capabilities. However, they argued that firms also sought to reconfigure their resources and competencies in environments that were not characterised by rapid rates of change. As a consequence they defined dynamic capabilities as “a learned and stable pattern of collective activity through which the organization systematically generates and modifies its operating routines in pursuit of improved effectiveness” (Zollo and Winter, 2002, p.340). These authors theorized that the co-evolution of three key learning mechanisms (experience accumulation, knowledge articulation and knowledge codification) were crucial for the building of both operational capabilities and dynamic capabilities which might then in turn modify operational capabilities.

Whilst the mechanisms of path dependency and experiential learning formed the core of theorizing about dynamic capabilities, the concept continued to attract different theoretical interpretations; a development that caused problems for some scholars. Kraatz and Zajac comment that “while the concept of dynamic capabilities is appealing, it is a rather vague and elusive one which has thus far proven largely resistant to observation and measurement” (Kraatz and Zajac, 2001, p.653). Similarly Zahra and co-authors bemoan the fact that “the emergent literature on dynamic capabilities and their role in value creation is riddled with inconsistencies, overlapping definitions, and outright contradictions” (Zahra *et al.*, 2006, p917). It is partly in response to such criticisms that Teece (2007) boldly asserts that they are central to our understanding of strategic management. His thesis is that “while the long-run performance of the enterprise is determined in some measure by how the (external) business environment rewards its heritage, the development and exercise of (internal) dynamic capabilities lies at the core of enterprise success (and failure)” (Teece, 2007, p.1320). He further maintains that the concept is most relevant to multinational companies operating in highly competitive global markets; conditions found most notably in high-technology sectors. Whilst seeking clarify the vagueness (Kraatz and Zajac, 2001) of the dynamic capabilities concept by distinguishing their micro-foundations, Teece suggests that there must necessarily be a degree of opaqueness to the idea and practice. For without such opacity, he argues, all firms would be able to copy them and the competitive benefits that accrue from them would be lost! Notwithstanding the inherent vagueness in (at least) the practical consequences of the concept he is advancing, Teece develops a comprehensive model of the foundations of dynamic capabilities based around three key categories of capability (Teece, 2007, p.1342): “Sensing”, “Seizing” and “Managing threats/Transforming”. Whilst each of these categories are described in terms of “micro-foundational practices”, the dynamic capabilities themselves operate at a meta-level that transcend the operational. Involving hard-to-replicate know-how, they are inherently entrepreneurial, rather than managerial, in character.

Although Teece’s microfoundations of dynamic capabilities are helpful to those (including me) interested in studying the development of capabilities, the overarching concept continues to attract criticism. Danneels maintains that “notwithstanding its current popularity, the notion of dynamic capabilities is abstract and intractable” (Danneels, 2008, p.536). However, the most sustained attack on the “logical consistency, conceptual clarity and empirical rigour” of dynamic capabilities view is made by Arend and Bromiley (2009). These authors suggest “four major problems that limit the potential contribution of the [dynamic capabilities view]: (1) unclear value-added relative to existing concepts; (2) lack of a coherent theoretical foundation; (3) weak empirical support; and (4) unclear practical implications” (Arend and Bromiley, 2009, p.75). In their response to this criticism Helfat and Peteraf suggest (2009) that the complexity of the concept has contributed to confusions in its development. In addition they also note that other theoretical work on capability building, including their own (Helfat and Peteraf, 2003), does not invoke dynamic capabilities. I shall turn to this work shortly. However it is worth noting that the clarification of the concept by senior researchers in this field (Helfat and Winter, 2011) contemporary with this study’s empirical work, acknowledges that the distinction between operational and dynamic capabilities is inevitably blurred given that change is always occurring to some degree. They define operational capabilities in relation to how “a firm makes a living in the present” and dynamic capabilities as those that enable “a firm to alter how it currently makes its living” (Helfat and Winter, 2011, p.1244). They emphasise that it is not the rate of change of the external environment that is the defining characteristic of a dynamic capability but rather the importance of the change for how the firm will earn its living in the future. Ambrosini and Bowman in their review of the dynamic capabilities construct argue a similar point in suggesting that ‘dynamic’ neither refers to dynamism in the environment nor in the capabilities themselves, but rather “to the renewal of resources” (Ambrosini and Bowman, 2009, p.35): something that can happen in relatively stable environments. In contrast to these views Wang and Ahmed in their review of the dynamic capability literature draw particular attention to the role of market dynamism as an antecedent to firms’ dynamic capabilities (Wang and Ahmed, 2007, p.40). The extent of the influence of market (or more generally environmental) dynamism for capability creation and development is important in relation to the area studied in this thesis. Change in the world of higher education is inevitably slower than that in the commercial world, and yet an ability to maintain competitive advantage (over peers) is no less important. In viewing such capability change in “slow-motion” (as it were) the empirical study of the innovation work of university research groups might reveal ideas that suggest insights for the faster changing world of commercial markets. However, even allowing for the clarifications offered in the core articles (as defined in Helfat and Peteraf, 2009), Teece *et al.* (1997, 2007) and Eisenhardt and Martin (2000), the concept of dynamic capabilities is sufficiently confusing[2], to merit considerations of other theories of capability development.

Other researchers have continued to work with the idea of (ordinary or operational) capabilities and explored other mechanisms for their development. A model for understanding the options for the different paths of new product introductions is used by Helfat and Raubitschek (2000) in order to explain how organizational knowledge, capabilities and products co-evolve over long periods of time. This longitudinal view of the process of new product introductions is labelled the “Product Sequencing Model” and is conceptualized as proceeding

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[2] At the time of proof-reading this thesis a further layer of confusion was added to this literature, with the suggestion (Hine *et al.*, 2014) that the hierarchy of capabilities be extended by distinguishing between *dynamic functional capabilities* and *dynamic learning capabilities*.

through the dynamic interplay of a “system of knowledge” (comprising both core and integrative knowledge) and a “system of learning” (comprising both incremental and step function learning) (Helfat and Raubitschek, 2000, p.968). Vertical chains (Porter, 1985) of complementary assets (Teece, 1986) and activities needed for product sequencing become important for the development of organizational capabilities and knowledge as well. In this manner the authors suggest that the “model provides a dynamic framework that enables us to track, step by step, how knowledge, capabilities, activities and products co-evolve over time and across key markets” (Helfat and Raubitschek, 2000, p.974).

Population ecologists argue that initial conditions are the principle determinants of capabilities (Hannan and Freeman, 1989). This emphasis has been challenged by Cockburn et al. (2000) with their modelling of the adoption of a particular innovation practice based upon an extensive longitudinal dataset for the pharmaceutical industry. Their analysis acknowledges the importance of initial exogenous conditions but also suggests a role for purposeful strategic adjustment. They identify the process of “convergence” (Cockburn *et al.*, 2000, p.1125) of best practices, and note that those firms who are initially most disadvantaged are the ones who work hardest to catch up. In addition, they find that firms differ in their ability to sense and respond to environmental cues. In this manner, initial conditions are manifest as a constraint on a firm’s strategic response to exogenous change.

A further illustration of relationship between starting conditions and strategic agency is found in the analysis of case histories from the semi-conductor industry by Holbrook et al. (2000). These authors argue that performance differences can be understood in terms of the different technological goals of firms and of their manager’s ability to integrate key functions. However the capability to leverage such activities in response to a recognized need to change appears constrained by past experiences. This leads the authors to suggest the importance the varied expertise of the firm’s founders and the conditions upon their entry into the industry (Holbrook *et al.*, 2000). In contrast to the study of the pharmaceutical industry (Cockburn *et* *al.*, 2000), Holbrook and co-workers find little evidence for the “sort of decision-making process, commonly assumed by economists, that would lead firms to converge rapidly to some similar set of R&D and production activities” (Holbrook *et al.*, 2000, p.1038).

Helfat and Peteraf argue (2003) that all capabilities can in principle change and can do so without having to invoke dynamic capabilities acting upon them. They are concerned with explaining how the heterogeneity of resources and capabilities within a population of firms evolves over time. Their aim is a dynamic resource-based theory that is not dependant on dynamic capabilities, and they do so by introducing a capability lifecycle model. At the outset they define a capability as “the ability of an organization to perform a co-ordinated set of tasks, utilizing organizational resources, for the purpose of achieving a particular end result” (Helfat and Peteraf, 2003, p.999). These authors describe their model by imagining the evolution of a new enterprise. They draw attention to the natural endowments of such an enterprise at its foundation. These are born of the attributes of individuals in the firm, and through a mechanism of path dependency they will become the initial source of capability heterogeneity. Such reasoning is important in the analysis of Holbrooke *et al.* (2000) of the development of major firms in the semi-conductor industry, and Raff (2000) with regards to US book retailers. Raff also finds that while there is a persistence of some core capabilities, then incremental adaptation also occurs leading to different outcomes from the starting positions.

From an initial stock of natural endowments, Helfat and Peteraf’s model (2003) sees capabilities develop through the conscious effort of individuals within the enterprise at process improvement and problem-solving. This model does not require the existence of any meta-capability (i.e. dynamic capabilities) operating upon the firm’s ordinary (i.e. operational) capabilities, but rather learning-by-doing is the core development mechanism. In this manner, capabilities evolve until they reach a level of maturity associated with some inherent limit of the capability or as a result of manager’s satisficing at some level of performance (Winter, 2000). In the absence of exogenous shock a capability might be expected to hold at this mature level with capability maintenance being the challenge for the firm.

The impact of exogenous shocks gives rise to “capability branching” in the life-cycle model and Helfat and Peteraf describe a number of possible evolutionary trajectories that might result. At such a juncture, these authors suggest six generic strategies for capability transformation that alter the shape of the subsequent lifecycle trajectory, and which they term “the six Rs” (Helfat and Peteraf, 2003):

- *Retirement*, or withdrawal of the capability
- *Retrenchment*, or the gradual decline in the level of the capability
- *Replication*, or copying the capability in some other context (e.g. a different geographical market for the same product)
- *Redeployment*, or applying the capability to a different product or market (e.g. Helfat and Eisenhardt, 2004)
- *Recombination*, or adding the original capability to another
- *Renewal*, or improvement of the capability in some way

All the different stages in a capability lifecycle are shown schematically in Figure 3. The horizontal axis is a proxy for time and represents the cumulative amount of activity associated with the capability. The vertical axis is intended to show the level of the capability employed per unit of activity. A move up the vertical axis would represent an increase in the degree of competence at the given capability. The diagram is generic in nature and illustrates a number of different evolution scenarios. The branching of the lifecycle and points of scenario departure that result will be influenced by a number of factors: the antecedents in the capability evolution up to that point, strategic change decisions taken internally by the organisation’s managers and the impact of changes in the external environment of the organisation.

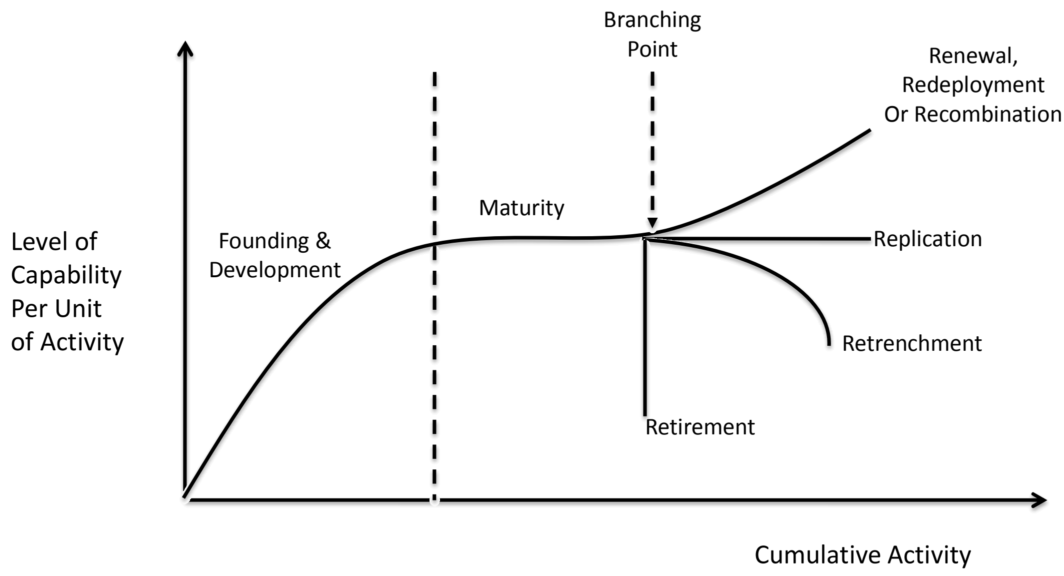

Figure 3 – Stages of the organisational capability lifecycle (adapted from Helfat and Peteraf, 2003, p.1005)

Schreyogg and Kliesch-Eberl (2007) also emphasis path dependency, structural inertia and commitment as the basis for the rigidity of capabilities in the face of environmental drivers for change. And yet accept that capabilities do adapt. In critiquing ideas of dynamic capabilities they identify three theories of dynamism in the extant literature: radicalized, integrated and routinized. They argue that none of these conceptualizations of dynamism provide a coherent account of how capability rigidities might be overcome. As an alternative approach they suggest that "instead of dynamizing the capability conception, capability evolvement and system dynamization are conceived as two separate countervailing processes which are performed simultaneously" (Schreyoegg and Kliesch-Eberl, 2007, p.925).

How then do these ideas of organizational capabilities (and their development) help us in understanding how university research groups are responding to new mandates regarding innovation? Put another way, what explanations do the management theories outlined here suggest upon re-reading the empirical accounts of universities and innovation in chapter 2? One point to note at the outset is that with the exception of George's historical case analysis of technology transfer at the University of Wisconsin (George, 2005), organizational capabilities have not been invoked in this empirical literature. This in turn begs the question of what actually constitutes an organizational capability for the conduct of science-led innovation, and this is one of the questions with which this thesis will be concerned.

The whole domain of university science-led innovation is founded on stories of change whether in the use of innovation networks (Rosenkopf *et al.*, 2001), modes of coordinating research and innovation (Shinn and Lamy, 2006) or the extent of relational involvement with research users (Perkmann and Walsh, 2007). The typologies of scientist/entrepreneurs that have been advanced (Etzkowitz and Leydesdorff, 1997, Shinn and Lamy, 2006) and the ways in they have reconfigured available resources and processes (Markman *et al.*, 2008b), could be read in terms of the “sensing”, “seizing” and “recombination” logic comprising Teece’s theories of strategic change through dynamic capabilities (Teece, 2007). In looking for evidence of the underlying mechanisms for capability development then we can find the path dependence of the enduring cultures of academic life presenting barriers to scientists participating in commercial work (Tuunainen and Knuuttila, 2009). In his historical analysis of the Wisconsin Alumni Research Foundation George (2005) draws attention to the importance of experiential learning not only for the primary capability development of patenting, but also in the development of complementary capability of licensing. However, whilst this literature might be profitably recast, - and has been in the case of George’s paper (2005) - in terms of organizational capability development, there are significant threads within the university science innovation literature that cannot as easily be accounted for within the capabilities literature. Most notably in this respect is the dominant role of the research leader in the development of research group capabilities and the implications this carries for the role of agency in capability development.

The idea of agency does not seem to sit comfortably within the mainstream capability literature. In their 1997 paper on dynamic capabilities Teece and his co-authors argued that agentic theories of the firm “do not recognize the opportunities and constraints imposed by processes, positions, and paths [of dynamic capabilities]” (Teece *et al.*, 1997, p.524). However, in a literature review that relates dynamic capabilities to issues of new venture creation Zahra et el. (2006) argue for the importance of the strategic agency of leaders, and how organizational capability development can be prompted by conditions other than market dynamism. As a result they define dynamic capabilities as “the abilities to reconfigure a firm’s resources and routines in the manner envisioned and deemed appropriate by the firm’s principal decision-maker(s)” (Zahra *et al.*, 2006, p.924). Their emphasis is on the dynamism of the capability and the agency of the manager(s) central to the change process. Managers work to keep capabilities relevant and active through processes of organizational learning. It is by this means of refreshing ordinary capabilities, with its associated acquisition of new knowledge that dynamic capabilities evolve.

An over-emphasis on organisational-level of analysis in the capabilities literature was challenged by Felin and Foss in provocative fashion (2005). In this polemic they ascribed the persistent confusions within this literature to result from an over-focus in organisational level concepts at the expense of ones relating to individuals. However, a related thread of literature had started to examine the role played by managerial cognition in guiding the search for new organisational capabilities (Gavetti and Levinthal, 2000, Gavetti, 2005). Here strategic agents use analogical reasoning in order to leverage their experience and respond to new competitive challenges (Gavetti *et al.*, 2005). The creative nature of such reasoning is emphasised by Winter et al (2007), who maintain that strategic search is primarily a matter of human imagination, with individual experience being the wellspring of creative thinking.

In further developments, Pandza and Thorpe make a link between managerial agency and dynamic capabilities by exploring the complementarities of the former with experiential learning (Pandza and Thorpe, 2009). They define dynamic capabilities “an organizational phenomenon accountable for the creation of novel knowledge that significantly deviates from a firm’s existing knowledge trajectories” (Pandza and Thorpe, 2009, S119). Their argument then develops by examining how discontinuities in a firm’s knowledge trajectory are generated. Where most of the dynamic capability literature uses a “selection-adaptation” mechanism (Volberda and Lewin, 2003), these authors introduce path creation (Garud and Karnoe, 2001) as a mechanism not widely employed in the dynamics capability literature. Their argument is “the novelty is not a result of accumulated experience; rather experience accumulates as a consequence of novel directions” (Pandza and Thorpe, 2009S120). However, discontinuity events are rare (except perhaps in very turbulent environments) and it is unlikely that dynamic capabilities result from the accumulation of learning from rare events. Two cognitive mechanisms are posited in order to account for the deviation in knowledge trajectories that would result simply from experience accumulation: creative search and strategic sense-making. Creative search is a divergent cognitive process concern with opportunity exploration. Strategic-sensemaking operates when understanding of the causal links between existing knowledge and the external environment are broken; something unexpected has happened which cannot be explained through a reliance on previous experience. These authors suggest that “the coexistence between patterned and experiential processes and less patterned managerial agency is still an unexplored area in the field of dynamic capability research” (Pandza and Thorpe, 2009, S128) calls for a “theory-driven and process-oriented case study design”. This call holds out the possibility that a study of how university research leaders and their groups are responding to mandates for greater societal relevance may also offer insights for the role of managerial agency in the dynamics of capability development.

In a related study within a large pharmaceutical company Pandza (2011) examined the autonomous strategic action (Burgelman, 1983) of groups that resulted in capability development. That is to say their strategic actions are “outside the scope of the corporate strategy and that typically involve engagement with new capabilities or capabilities perceived as less familiar to the firm” (Pandza, 2011, p. 1016). Pandza argues that the origin of capability development of groups acting autonomously lies in the collective cognitive frames held by the groups. These frames give rise to particular socially complex practices that included collective search of new practices, followed by legitimacy seeking and consensus seeking behaviour, including practices to “seed” ideas and people into other groups (Pandza, 2011, p. 1032). Such practices were motived by the groups perception that they compare unfavourable to others, as well as seeking to increase their own distinctiveness which in turn resulted in capability development. Again there are clear analogies between this case that that of the university research groups being study in this PhD project. The latter are able to act in a strategic fashion that is autonomous from their host universities: indeed it is expected of them.

This chapter has shown how the capabilities literature has been used to describe and explain strategic change of organisations. Whilst primarily invoked in relation to private firms, I have suggest throughout the chapter implications and analogies for use capabilities to frame a study of strategic change for university research groups. The literature is not without its disagreements, in particular issues of levels of analysis seem to bedevil theoretical and empirical progress of the construct. Regarding this issue, then a study of groups less complex that corporate firms (i.e. university research groups), who are identified with one key agent (i.e. the research leader) may prove interesting.

Chapter 4
Research Questions

This chapter brings the first section to a close by drawing together the threads of the discussion so far, and by suggesting it is an area of sufficient contemporary importance to merit researching. In proceeding to research questions in this manner I seek to make the additional point that research that aspires to contribute to both practitioner and academic debates must commence with a view of the challenge taken from both domains, rather than start with only the former in mind, and add on the latter at the end.

This thesis focuses on the work of research groups already active in the arena of nanotechnology. These groups have already taken some steps to pursue the economic impact of their research, and now need to respond to the demands of new policy mandates. That is, not only are they concerned with the disinterested pursuit of natural science, or *scholarship of discovery* as Boyer (1990) has labelled it, but also the *scholarship of application*, by being active in both research and innovation. That they should be so is the philosophy that has informed a mounting body of policy pronouncements. As introduced briefly in chapter 1, science-led innovation discourse in recent years has given greater force to the position that research groups be active in the application of their research. In many ways the basic message of a knowledge-based economy rhetoric has not changed during the last 15 years: if knowledge and scientific expertise are the basis of economic success then sustainable efforts need making to connect university expertise to industry. The mandates for such academic involvement have steadily acquired more urgency culminating in the Research Excellence Framework of 2014, which will (at the time of writing) involve a selection of university research to be assessed for impact. Over the same time a change in the articulation of impact has widened beyond the purely economic to include wider societal and environmental concerns has also been evident. For researchers in emerging technology areas like nanotechnology such policy debates have gained further momentum under the label of *Responsible Innovation*. And whilst the meanings of this term might be open to different emphases, the growing importance of these mandates for winning public research funds is evident (Owen and Goldberg, 2010). I suggest that this policy landscape for science-led innovation constitutes a macro-foundation (Hahn, 2003) that influences the activities of research groups and their leaders, and has obliges them to change the ways in which they realize the challenge of *Research Impact*.

Such mandates for Impact might be viewed as the latest manifestations of debates on the social role of science (cf. Chapter 1). However, their explicit linkage with the realisation of increasingly scarce research funds gives such policy pronouncements greater import for scientists themselves. It is no longer tenable for scientists to pay lip service to possible applications of their science (something I witnessed and practiced during my own forays in nanotechnology during the 1980s). As outlined in Chapter 2, placing an economic mandate on the conduct of university research groups has been the subject of an extensive empirical management research literature. New institutional arrangements for economic development have been described under the heading of the 'Triple Helix' literature, and the advent of a new character of university suggested: the *entrepreneurial university*. At the organisational-level of the innovation practices of individual research groups, then the mechanisms of 'academic entrepreneurship' and university-industry collaboration' have been the subject of extensive study. This PhD study is prompted by the developments in UK science and innovation policy that have changed in both character and scale. Economic benefits are no longer enough, research groups (and particularly those in areas of emerging technology) must be mindful of a wider set of societal and (natural) environmental needs; such responsibilities must be evident in innovation practices. And no longer solely the remit of early enthusiasts, introduction of measures of 'impact' in the assessment of all academics puts this policy onto the agenda of all academic researchers. How then are university researchers and their groups responding to these heightened pressures? Research groups must change in order to work more actively towards the realisation of research impact. In doing so I recognize the distinctive role played in such groups by the research leader.

In order to generate insight into the response of university research groups to new policy mandates, this study frames the changes in innovation practice in terms of 'organisational capabilities'. This concept has been developed within the strategic management literature in order to account for how firms create, develop and sustain the advantages they hold over their rivals. By treating university research groups as 'quasi-firms' (cf. Etzkowitz, 2003) this study aims to explore the nature of their organisational capabilities for innovation, and how these capabilities are changing under the new policy landscape. The ideas developed in the opening chapters and the research questions they beg is shown schematically in Figure 4.

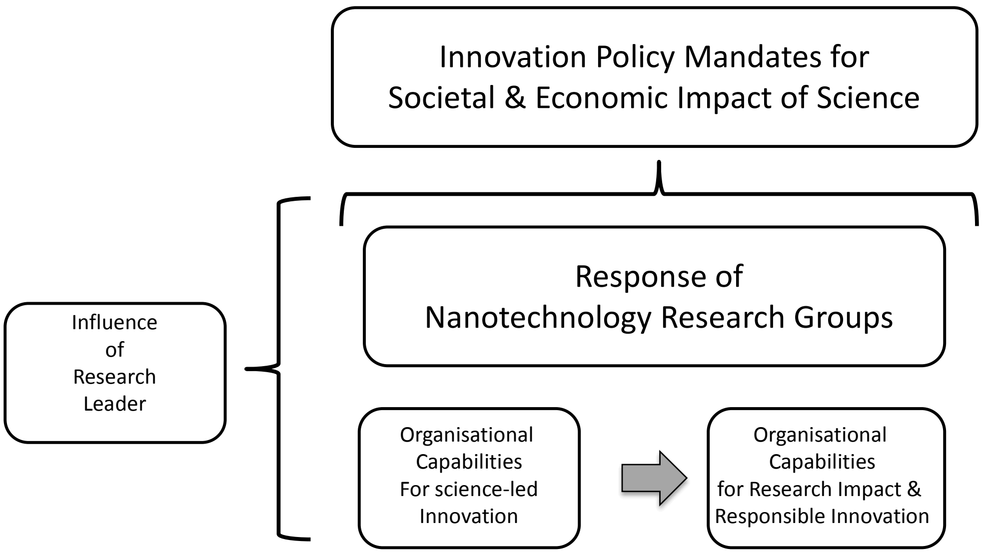

Figure 4 – Schematic summary of research themes

The principle question from this PhD study is:

How are emerging innovation policy mandates for societal & economic impact of science influencing the innovation practices of nanotechnology research groups?

Ancillary questions are:

(i) what are the organizational capabilities in science-led innovation?

(ii) How are capabilities changing in response to new innovation mandates? and

(iii) What is the role played by the research leaders in effecting such developments in capabilities?

The structure of the remainder of this document is as follows.

Part II of the document concerns the design of the research. Chapter 5 outlines the philosophy of science that has informed my research, as well as discussing the implications of social research that aspires to practitioner *relevance*, as well as scientific *rigour*. Chapter 6 then details the methods by which data were generated.

Part III includes a series of chapters that together constitute the research findings. Chapter 7 reports on some pilot research that served to orientate me toward the research field, and aided my development as a management researcher. Chapter 8 is a critique of recent UK innovation as it concerns university research groups. This review was conducted contemporary with the main original data generation for the project, and provided an important contextual understanding of the research field. Chapter 9-11 report the main data generated as part of a case study of two university nanotechnology research groups. Chapter 9 includes the main case descriptions, along with a comparative analysis. Chapter 10 focuses on the work of an individual scientist within one of the research groups. Chapter 11 includes an inductive analysis of interviews with people in leadership positions within the two groups.

Part IV involves a discussion of the research findings in relation to the themes raised in Part I. Chapter 12 examines changes in the innovation work of the two research groups in terms of the development of new organisational capabilities, and presents answers to the study's questions. Chapter 13 describes efforts during the course of the project to realise impact from the research. Chapter 14 brings the thesis document to a close by offering some personal reflections on this PhD research project.

PART II
RESEARCH DESIGN

Chapter 5
A Philosophy of Research

Philosophical foundations

If I were to repeat the experimental section of my PhD in Chemistry (Ashton *et al.*, 1991, Ellwood *et al.*, 1992) then, allowing for a certain rustiness in experimental technique, I would get exactly the same results. Those results are not a function of the University of Sheffield chemistry labs in the 1980s. In fact the methodology is such that anyone could repeat the work at any time; and get exactly the same result. Ultimately, this is because (I believe) there are laws describing the nature of material things that operate independent of our interest in them. This idea is built upon a philosophy that the nature of the material world can be understood objectively and independent of people observing it: logical positivism. I believe that this philosophy cannot simply be extended to the nature of reality as it concerns interactions involving people: the social world as distinct from the material world. In this Chapter I outline the philosophies of management research and related methodological positions that have animated my PhD studies. This chapter starts with a short discussion on the philosophy of science that underpins my approach to research. I then outline a number of ideas, that I call methodological orientations that have also influenced my work. These ideas do not seem to merit elevation to the status of 'philosophy', but they have nevertheless informed my practice of research. These conceptual discussions are made concrete in the next chapter which explains my actual research design. Importantly, both chapters explain how I have tried to build both rigour and relevance into the very fabric of the study: the way it is framed (Part I), designed (Part II), implemented (Part III) and made worthwhile (Part IV).

As I have journeyed from natural to social science, my firm conviction is that they are different, and that that difference should be embraced rather than ridiculed (Sokal, 1996a, Sokal, 1996b). Unlike many of the positions taken during the “science wars” (Weinberg, 1996), I believe it is misguided to view social science as a shoddy science mired in relativism. I maintain that a study of the natural and social world is both necessary for an understanding of our lived experience, and that the sciences needed for those studies are different. Both are sciences in the sense of having a strong logic connecting questions about their worlds and research methodologies. Those methodologies can have claims to validity but by different means because of the very natures of the subject of study. With natural science, repeating experimental procedures is possible (in principle, if not always in practice), whereas social scientists may demonstrate validity and reliability through clear and transparent accounts of their methods; procedures that may then be judged by those practiced in the same methods.

If I have a similar outlook to the natural scientists that are highly critical of social science (Sokal, 1996a, Weinberg, 1996), it is only to the extent of a lack of sympathy towards social science that seems to be aping its natural cousin (Thomas and Wilson, 2011). Natural science has been immensely successful in the production of theory that can make predictions of new phenomena, and can lead to the creation of technologies that have improved our quality of life. It is therefore perhaps unsurprising that, inspired by such success; social scientists have sought to copy the positivist methods of natural scientists. However, philosophical assumptions underlying this logical positivist approach to science remove, as far as possible, the researcher from the subject of inquiry (Chalmers, 1999). And as I noted in the Preamble to this thesis, my professional and scientific experiences have led me to seek a more personally engaged form of scientific inquiry. The idea within logical positivism that I wish to deny is that science should be value-free, and that the scientists should seek to be a neutral observer. This positivistic shunning of considerations of personal values and interests, whilst prevalent within the natural sciences, actually creates a space for understanding the contribution of social science. Flyvbjerg (2001) provides a strong argument for this perspective by considering the different Aristotelian types of knowledge pursued by the different sciences. Thus, whilst natural science is unparalleled in *episteme* (analytical knowledge) and *teche* (know how), social science matters most in relation to *phronesis* (which Flyvbjerg translates as “practical wisdom”) with its emphasis on more reflexive forms of analysis. My own expectations of social science were that it would be distinctive in both purposes and methodology from natural sciences. Such differences were to be welcomed, particularly in the context of a study of the work of natural scientists; distinctive disciplinary foundations seem to make interdisciplinary research a more realistic goal (Huutoniemi *et al.*, 2010).

Whilst wanting to move away from a philosophy that was objective in both its ontology and epistemology, I found that the relativist philosophy of being subjective in both these regards was a step too far. The position that not only theory but reality itself (the two can seem blurred in this philosophy) is socially constructed does not fit with my lived experience of management in that there do seem to be enduring patterns in the social world. I sympathise with the relativist’s rejection of the objective certainty of positivism, and the emphasis on the theory-laden nature of observation. Both of these positions lead me to subscribe to a subjective epistemology. However, my ontological position remains that there are discernible patterns to social life; they are just more emergent in character than the more concrete ‘laws’ that explain the material world (Danermark *et al.*, 2002).

A number of philosophies of science have trodden this middle way between positivism and relativism, and they may be placed under the broad umbrella term of “realism”. The realist philosophy posits that there is a real world that exists independent of our interest in it, and that there is no single way of acquiring knowledge of that world. As a philosophy it is part metaphysical and part empirical in that it goes beyond mere experience but it can be tested by experience (Leplin, 1984). The different realist positions that have emerged that can be understood in terms of the relationship between the metaphysical entities (or theory) and empirical observations. So-called weak forms of realism posit a loose connection where stronger positions suggest that the relationship is isomorphic (Chalmers, 1999). For instance, scientific realism pursues statements that are true at the level of both theory and empirical observation. In this way science progresses not because “our current theories are true, but they are truer than earlier theories, and will retain at least approximate truth when they are replaced by something more accurate in the future” (Chalmers, 1999, p.238). A more moderate position, sometimes called conjectural realism, acknowledges the fallibility of scientific knowledge (Popper, 1959). However, the most pertinent of the realisms for this study is that of critical realism originated by Roy Bhaskar (1998).

Critical realism is frequently presented as something of a reasonable compromise between the excesses of logical positivism and social constructionism (cf. Easterby-Smith *et al.*, 2012, p.29). In this regard it has been presented by both researchers whose natural sympathies involve working with qualitative data (Alvesson and Skoldberg, 2009, p.29) and those who work with quantitative data (Jones, 2011). Critical realists maintain that social construction is too limited in its reach as things happen which are beyond both individuals’ consciousness of them and their efforts to change them. Within organizations there are real things called ‘managers’; they are not just socially constructed phenomena. However, unlike the logical positivists, they argue that reality is not simply a matter of material objects and observable ‘facts’; ideas and discourses are both real and matter. Critical realists have no interest in formulating ‘laws’ to explain reality, rather they are interested in the “complex network of theoretical and observable elements characterizing efforts going beyond the surface of social phenomena” (Alvesson and Skoldberg, 2009, p.40). In this philosophy reality operates at three levels: the *empirical* (or that which we can observe); the *actual* (or that which pertains independent of the researcher or an observer); and the *real* (which involves deeper lying mechanisms which are taken to generate empirical phenomena). The job of the researcher is then to explore the real in order to discern how it relates to the empirical and the actual.

My own position is to pursue a form of scientific inquiry that will lead to a useful model of real management practice; one that is not simply dependent on my interpretation or the practitioner’s experience, useful though both of these elements are. As Rescher argues (2000) we actually need the presumption of a mind-independent reality (an objective ontology) to make scientific inquiry meaningful in the first place: “what is at issue here is not so much a product of our experience of reality as a factor that makes it possible to view our experience as being ‘of reality’ at all” (Rescher, 2000, p.127). If my research aspires to inform management practice then it is, at very least, pragmatically useful to assume that there is such a thing as the reality of management practice that exists beyond the attention I give it as a researcher. In this matter the radical streak evident in the philosophy of critical realism is pertinent: “[critical realism seeks] not only to explain the world but to change it” (Alvesson and Skoldberg, 2009, p.39). In a similar vein, Andrew Van de Ven argues that his highly developed methodology of engaged scholarship is founded on a critical realist philosophy (Van de Ven, 2007, chapter 2). The pragmatism inherent in the realist philosophy allows us to progress our research with a surer scientific footing. The question of whether and how management research might influence management practice are ones that have animated much debate within the management research community. Some of the ideas generated in these debates have provided key methodological orientations in my research, and so I now give space to a brief overview of the arguments that have been conducted under the banner of “Relevance” of management research. It is also important to note the parallel that exists between concerns for the *relevance* of management research and the *Impact* agenda.

The Relevance of Management Research

What's the point of management research? One might argue that managers constitute a large, distinct and interesting modern tribe, worthy of anthropological or sociological study. While true, this would probably not account for the size of schools of management (certainly compared with those of anthropology or sociology). It is of course true that Business Schools have an additional role in the education of this large body of managers. The debate on the effectiveness of Business Schools on the development of management as a profession is very rich and nuanced (Khurana, 2007) beyond the scope of this thesis. And yet we might expect that informing management practice with rigorous academic research might be relatively uncontentious. Given the wealth of management research undertaken it therefore seems reasonable to ask how much of it influences the work of practising managers. For some time now this seems to have been a question occupying the community of management researchers (Wensley, 2011).

In his history of Manchester Business School Wilson (1992) notes the tensions that existed during the early days of UK business schools between the relative priorities given to teaching, research and consultancy (what today we might call Impact). These uncertainties were resolved in part by initiating a research programme to examine the very question of how managers learn, and through the advocacy of research methods that fostered an immersion in the very real problems of management practitioners. However, this tension between priorities in Business School world may be inherent in the nature of an applied discipline practiced in the higher academy: they have certainly never been too far below the surface of debates about the contribution of Business Schools to the wider economy. The debate has been conducted on both sides of the Atlantic with US academic opinion invariably running slightly ahead of that in the UK. In 1992 Harvard Business Review considered the "complex case of management education" (Linder and Smith, 1992). In this discussion both academics and practitioners were in agreement that the contribution played by management research was usually minimal. Professor Harold J Leavitt wrote ".. a lot of business school research is overly esoteric and done with more of an eye to academic peers than to utility. I fully agree that much business school research (like all research) turns out to have been a waste of effort" (Linder and Smith, 1992, p.26).

The topic of relevance has been a perennial theme in the annual valedictory address from the President of the Academy of Management (AoM). Such speeches invariably focus on one aspect of Relevance and have included: “Mode 1.5” research (Huff, 2000); the co-production of knowledge with practitioners (van de Ven, 2002); greater overlap between scholarly knowledge and folk wisdom (Pearce, 2004); Evidence-base management (Rousseau, 2006); a relational scholarship of integration (Bartunek, 2007). This flurry of AoM Presidential advocacy (angst?) since the Millennium can be traced in many ways to the corresponding address in 1994 by Donald C Hambrick which had the provocative title “What if the Academy Actually Mattered?”: "Colleagues, if we believe highly in what we do, if we believe in the significance of advanced thinking and research and management, then it is time we showed it. We must recognise that our responsibility is not to ourselves, but rather to the institutions around the world that are in dire need of improved management, as well as those individuals who seek to be the most effective managers they possibly can be. It is time for us to break out of our closed loop. It is time for us to matter" (Hambrick, 1994, p.13). Strong words and ones that still seem disappointingly appropriate today.

One consistently argued position is that management research suffers from not having a coherent overarching paradigm (House, 1975, Pfeffer, 1995). This theme was taken up in the UK and developed with the publication of a paper on the nature of management research by Tranfield and Starkey (1998). This piece ended with a series of propositions for the management research community to debate. The sixth and final proposition strikes a particular chord with me: "A distinguishing characteristic of management research is that it engages with both the world of theory and the world of practice. Management researchers can locate themselves at different points in the cycle but they cannot stay fixed in either the world of practice or the world of theory. The problems addressed by management research should grow out of the interaction between the world of practice and the world of theory, rather than out of either one alone" (Tranfield and Starkey, 1998, p.353). One consequence of their paper appears to have been that the British Academy of Management adopted the challenge of relevance and has made it central to its work. A policy report was subsequently commissioned from Starkey in order to develop the implications of his paper. An abridged version of this report was published in the *British Journal and Management* (Starkey and Madan, 2001) and a series of essays were commissioned to debate the ideas raised by these publications (Hodgkinson, 2001).

In many ways the ensuing debate on the active pursuit of relevance in management research foresaw the debates on the importance of impact in Hefce's proposed Research Excellence Framework (discussed in Chapter 1). In the camp opposing greater relevance Grey (2001) argued that it was only by maintaining their independence from practitioners that management researchers might make significant contributions to practice. Pursuing relevance too purposefully may achieve short-term gains (for practice) but the legacy would be a less vibrant academy. In a similar vein, Kelemen and Kilduff offer two examples (2001) of "esoteric knowledge" that ultimately proved of significant practical worth for managers in difficult organisational situations. They argue that greater priority should be given to engaging with other academic disciplines than with practitioners. A similar point was made by Pettigrew (2001), although the tone of his critique seemed more supportive, arguing rather for a widening of Starkey's perspective to include engagement with not only other disciplines, but also with other Management Researchers in the USA. He felt the key challenge in realising such engagement lay in issues of capacity and being open to new forms of delivery. A similar conclusion was reached by Hodgkinson et al. (2001) by drawing on experiences in the field of organisational psychology. He argues that achieving research that scores highly both in terms of rigour and relevance will require more than simply adopting a "Mode 2" approach. Achieving such high quality research generally will mean that another gap needs bridging first - "a competency gap".

Hatchuel (2001) is the author in the Special Edition most supportive of Starkey's positions and argues that operationalising it requires new generic approaches to research design than the "classical laboratory and field models". He proposes a third model of "research-oriented partnerships" to address the challenge of relevance. Also generally supportive of Starkey, Huff et al. (2001) make suggestions for how "Mode 2" knowledge production may be extended. She develops her arguments and characterises a “Mode 3” type of knowledge production whose purpose “is to assure survival and promote the common good, at various levels of social aggregation”. In her discussion she relates the importance of knowledge production to higher questions of human existence in a manner articulated by the eminent management thinker, James March (Schmotter, 1998). A related higher perspective is also invoked by Weick (2001) who notes that "there is already a relevance bridge … but it is a lousy bridge " (Weick, 2001, S72). Faced with such circumstances, he argues for a retreat into an ideal of what a university aspires to be. In this he is inspired by John Gardner's description of a university as something that "stands for things that are forgotten in the heat of battle, for values that get pushed aside on the rough and tumble of everyday living, for the goals we ought to be thinking about and never do, for the facts we don't like to face, and the questions we lack the courage to ask" (Gardner, 1968, p.90). Weick maintains that by pursuing the wisdom inherent in these aspirations business schools will make better Mode 2 partners. Amongst all these apparently divergent perspectives there seems to me common ground to the extent that nobody is advocating irrelevance or even complete indifference to the concern of practitioners. Rather the differences lie in how best relevance should be pursued: by attending purposefully to the concerns of practitioners as articulated now; or by shunning such considerations for the time being, in favour of academy-constructed problems lest they compromise the distinctive contribution we might make for practitioners at a later date.

This debate resurfaced in the pages of the *Journal of Management Studies* (Fincham and Clark, 2009) just prior to the empirical work of this thesis. This briefer encounter was prompted by Kieser and Leiner claiming that the rigour-relevance gap is actually unbridgeable (2009). Invoking systems theory, they argue that because the worlds of management practice and management research are entirely distinct, this means their respected 'communication systems' cannot be integrated or converted. They maintain that "researchers and practitioners cannot collaboratively produce research, they can only irritate each other. However, sometimes irritations and provocations turn out inspiring" (Kieser and Leiner, 2009, p.516). Hodgkinson and Rousseau provide a stark refutation of Kieser's conclusion by noting that bridging the relevance gap "is already happening" (Hodgkinson and Rousseau, 2009). Starkey's own response (Starkey *et al.*, 2009) to Kieser allows him to extend some of his earlier arguments, and most crucially to my mind "re-imagine relevance as a necessary condition for rigour" (Starkey *et al.*, 2009, p.547). There is much that I find compelling in Kieser and Leiner's arguments, but I remain unconvinced by the logic of their "unbridgeable" conclusion. Rather it seems as though their differences may, once again, revolve around the mechanism of pursuing relevance and in particular the timing for introducing considerations of relevance into the research process. Their arguments could be read as recognising the importance of the specific nature of researcher-practitioner interface; whether it is a harmoniously sounding "partnership" or a more provocative "irritation". It could be that the important point to note here is that without some manner of attempted engagement at some point during the research process, then relevance remains highly unlikely.

Reflecting on these rigour-relevance debates (and by analogy that concerning the REF proposals - Chapter 1) I find that I can sympathise with many of the arguments presented by the polar positions that emerge in these debates. Management research is a practice-based discipline (law and medicine seem to me closer cousins than economics or anthropology), and could not exist if the world did not have management (something we can at least imagine). And yet such management research is likely to have very limited value if it slavishly addresses the needs of managers. The challenge of surmounting the 'dual hurdles' of rigour and relevance (Pettigrew, 1997) is based upon a metaphor that is both engaging and disarming in its simplicity; leaping both is far from being simple.

Answering, "when is rigour realised?" seems straightforwardly to be in the acts of establishing a logic that connects the research questions to the research method and in the discipline of data collection and analysis. Rigour is not something that can be realised retrospectively at a later stage. However answering, "when is relevance realised?" is less clear cut, and the most significant difference in the various positions articulated in this debate could be in the response to this question. No one is arguing in favour of irrelevant research, but rather when (and how) is the challenge of relevance best served. My own view is that attempts to deliver relevance retrospectively (i.e. apply something conceived as being purely theoretical to a practical solution) are likely to have a limited chance of success. This will be for all the reasons of incommensurability that Kieser and Leiner articulate (2009, 2011): retrospectively reconnecting different framings of an issue is likely to be very difficult. In the research conducted for this thesis I have sought to build from the outset, the conditions that at least make relevance possible. In doing so, my guiding principle has been that if one aspires to research that is relevant to practitioners then one should go to where practitioners operate in order to do that research. Therefore, in the final sub-section of this chapter, I consider another important methodological orientation for this thesis involves engagement with research users.

Engagement with Research Users

There are a number of established frameworks that may inform social researchers engaging with their user groups. In examining these models I have discerned three broad categories into which they can be placed, and which I describe by using a variant of Stokes' matrix (1996) to classify scientific research. This matrix is shown in Figure 5 has axes that reflect the primary concerns of the researchers and users that might be thought to exist during a given collaborative project: whether these concerns are mainly oriented towards academic ends or have a practical goal. Of the four possible quadrants shown in Figure 5, three represent serious scholarly practice, with the bottom left-hand quadrant being dismissed as not involving serious work. In representing broad approaches to user engagement in this manner, I do not mean to suggest that there are strictly defined distinctions between the three scholarly quadrants; there will inevitably be overlaps between these categories. However, this typology does have the virtue of highlighting the varied commitments in user-researcher collaborations. I note in passing the similarity between this representation and the 'four-fold typology of research' developed by Hodgkinson et al. (2001, S42) as part of their contribution to the *British Journal of Management* Special Edition on "Bridging the Relevance Gap". I have used the axes labels suggested by Stokes (1996) because they better convey the motivations of the researcher and user, where the labels used by Hodgkinson *et al.* speak more of methods and outcomes.

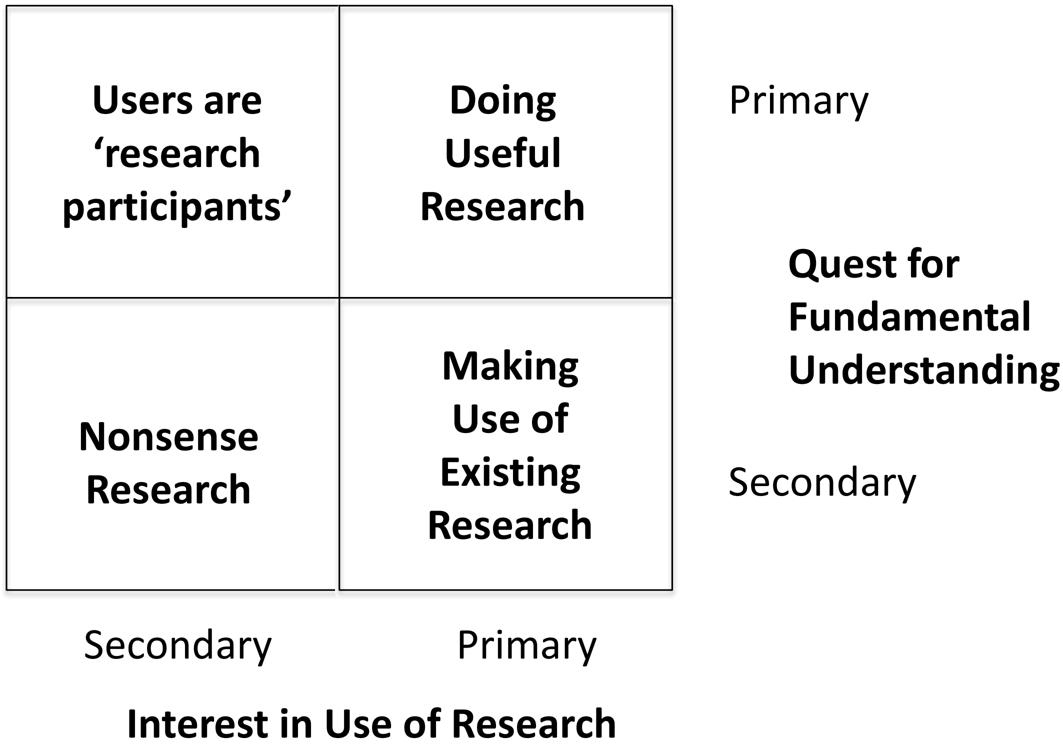

Figure 5 – Categories of user engagement models (after Stokes, 1996)

Engagements located in the top left quadrant are principally geared toward the production of academic outputs, notwithstanding that the achievement of those outputs involve the participation of stakeholders. This quadrant embodies the mainstream of research emanating from contemporary business schools (cf. Thorpe and Rawlinson, 2014). In contrast, research located in the bottom-right quadrant is concerned in the main with achieving practical outcomes: there may even be no contribution to fundamental understanding. Important approaches to collaboration in this quadrant include systematic review (Petticrew and Roberts, 2006) and the emerging practices of the Evidence-based Management movement (Rousseau, 2012). The third (serious) quadrant on the top-right of the matrix represents collaborative work of researchers and users that is equally concerned with academic and practical goals. Herein lies the much advocated dual hurdles of rigour and relevance. At the level of outputs, research in this quadrant may be followed by the production of both academic and practitioner papers (Pettigrew, 1997).

However, a range of extended collaborative practices is becoming evident, which seeks outcomes (and not just written outputs) in both theory and practice (Reason, 1988, Reason and Bradbury, 2006, Mohrman and Lawler III, 2011). The most well-developed model for this quadrant is the *engaged scholarship* framework of Andrew Van de Ven (2007). This framework conceptualizes collaborative research in terms of a balance of motivations: academic and practical. In addition, it is important to have a conceptual model of the very act of collaboration. In this regard, I have been influenced by the idea that outputs or artefacts of the research process may act as 'boundary objects' (Star and Griesemer, 1989) that allow new knowledge to be developed in more focussed terms. Such boundary objects may work by aiding the creation of 'trading zones' (Gorman, 2002) in which researchers and practitioners engage in a dialectical discussion based upon their knowledge claims for particular problem or issue. This 'intellectual arbitrage' (Van de Ven and Johnson, 2006) requires the presence of a boundary that throws into relief the different value assessments inherent within the knowledge claims. Whilst this process often draws upon the tacit knowledge of the researchers and users (Howells, 1996), I have endeavoured in my research practice to be mindful of the emergence of boundary objects (e.g. drafts of papers arising from this research) and the opportunities they create for user engagement.

Concluding Remarks

The research in this thesis aspires to both rigour and relevance. It is a practice widely acknowledged that the conditions for rigour are established in the methodological design and execution of research. In addition, care must also be taken to place rigorous research within the extant management literature and academic debates. In a similar vein, it is my contention that the conditions for relevance should be created in both a study's framing and its execution. That framing should be in relation to an important contemporary problem, but one that is of widespread concern rather than narrowly focussed. It is the job of management consultancy not management research to solve immediate and pressing organisational problems. And whilst professional doctorates (Banerjee and Morley, 2013) may take the real and specific challenges for the practitioner-scholar as their starting point, PhD research ought, I would argue, address more broadly defined practitioner concerns. Therefore, in this thesis I have invoked popular debates that have impinge on the contribution of science to contemporary economic and societal issues. In the next chapter a research design is presented that, whilst academically rigorous, seeks to create the conditions for meaningful engagement with practitioners and policy makers interested in Research Impact and Responsible Innovation.

Chapter 6
Research Methodology

The research questions developed in Part I of this thesis are exploratory in nature. The policy discourses for the responsible development of emerging technologies have largely operated at an institutional level and it is not clear how individual research groups are changing their innovation practice in response to the associated policy instruments. The academic literature has produced an extensive literature examining the commercialisation of science (and the two principle mechanisms of academic entrepreneurship and collaboration with industry). However, these studies have concerned how economic goals have been pursued and the requirement that such groups now incorporate societal influences into their innovation endeavours remains unexplored. In the absence of existing empirical or theoretical considerations it is appropriate to invoke a case study methodology to address the questions raised in Chapter 5. Adopting a case-study strategy allows the development of new constructs and the study of their inter-relationship (Eisenhardt, 1989), and in order to strengthen the inductive logic, then this thesis presents findings from two case studies (Eisenhardt and Graebner, 2007). Qualitative case study research is an appropriate methodology where a phenomenon is difficult-to-observe, or as in the case of responsible innovation, has not yet been codified into a set of accepted practices (Stake, 2005). If well-developed then case research for an emerging phenomenon might serve as prompts for further study (Flyvbjerg, 2006) or at least make that phenomenon more interesting to the prospective researcher that a purely theoretical treatment (Siggelkow, 2007).

This chapter explains the design of my case research. Firstly in introducing the two sites for my study, I draw attention to particular general features in order to provide a rationale for their selection: why they represent good venues to explore the issues raised in Part I. Secondly I outline the methods used to generate data. In this I was conscious of wanting to understand innovation at an operational level within the groups as well as how those in leadership positions approach innovation more strategically. Finally, I sought to make more of being embedded with these groups by including a small element of action research in order to generate data on the reflexive awareness of scientists to the changing policy landscape.

Sample and Context

The research settings chosen are both nanotechnology research groups within research-led UK universities. As noted in Chapter 2 it has become conventional to view such groups as ‘quasi-firms’ (Etzkowitz, 2003), as they increasingly operate as small enterprises pursuing objectives and competing with similar groups for acclaim and research income. They have a clear organizational structure built around an identifiable research leader, who largely sets the strategy for the group. Such research groups therefore represent an appropriate setting for the study of the development of an organizational capability for innovation. Conducting fieldwork in such laboratories is an established research venue for the transparent observation of practices (Latour and Woolgar, 1979) and social interactions. Moreover, their well-documented hierarchy based on seniority (Owen-Smith, 2001) supports the study of managerial agency. My selection of research groups working in nanotechnology specifically was prompted by the association of this field with the evolving debates on Responsible Innovation (cf. Chapter 1). Nanotechnology involves the manipulation of materials on an atomic scale, and has attracted much discussion for its potential to deliver new performance-based products (The_Royal_Society, 2004, Bozeman and Gaughan, 2007). As evident in policy debates the radical possibilities represented by nanotechnology have prompted polar responses of concern and excitement (Romig *et al.*, 2007). Therefore, nanotechnology research groups have significant potential for the exploration of issues relating to societal influences and responsibility within science-led innovation (Robinson, 2009).

Both research groups studied during this PhD programme operate in the broad area of nanotechnology, but in different areas of basic science. ColloidCo[3] has expertise in the design and manufacture of microparticles composed of nanoscale materials. The team at MicrosCo is expert in the field of high-resolution electron microscopy, and participates in a wide variety of collaborative projects with other scientists. Thus, whilst the scientists who are members of ColloidCo identify primarily with that research enterprise, membership of MicrosCo principally involves associates whose primary identification is with another research group. The project portfolios of the two groups range across a variety of project types. Whilst some projects can be understood in terms of the traditional labels of 'pure' and 'applied', most are located somewhere between. These projects aim to achieve breakthroughs in fundamental science, but are also concerned with the application of that research: a type of project has been labelled 'use-inspired' basic research, after the categorisation proposed by Stokes (1996).

In seeking to explore the development of new innovation practices in response to emerging policy mandates, it was important to select cases in which those research enterprises were already active in the established mechanisms of science-based innovation (cf. Chapter 3). Therefore, each of these research enterprises already exhibits a significant external orientation, and in which the research leader has commercial experience. It is illustrative of such experience to note that the leader of ColloidCo has created two spinout companies, and the two principal directors of MicrosCo (as well as having been Professors at a leading research-led university) worked for 20 years in leadership positions within a large science-based multinational company. A more complete description of the innovation work of these research groups is presented as part of my case research findings in Chapter 9.

One final reflection is worth making on the prospect of "re-entering" university nanotechnology laboratories twenty years after I left one. It seems to be something of a mantra amongst ethnographers that the critical challenge with this particular method is that of becoming too

[3] Research Groups names are annonymised

close to the subject of study (Thorpe and Holt, 2008). Given my initial research experiences in the natural sciences I am entering a world with which I am perhaps overly-familiar, and certainly the possibility of being an “anthropologist in the lab” (Latour, 1987) is not possible. Whilst familiar with some of the literature on “insider ethnography” (Labaree, 2002), my initial response to any concerns of being too close to the subject of study was to seek advice from other social scientists who have undertaken lab ethnographies. Such advice emphasised the importance of maintaining a high degree of self-reflection not only whilst inside the lab, but throughout the process of interpretation and analysis. The reflexive tone of this thesis should then be understood as not merely something that suits my personal objectives in undertaking a second PhD (although it certainly fulfils this function), but also as a crucial contribution to demonstrating the validity and reliability of the research.

Data Generation

This sub-section presents in outline the methods that were employed to generate data for the case studies. Each of the chapters in the Findings section that relate directly to the case research (Chapters 9-11) also include a more detailed description of methods for data generation and the subsequent analysis of that data. In the general manner of qualitative case study research (Stake, 2005) data were collected from a variety of sources in order to build rich pictures of the activities in each group. Unobtrusive documentary evidence (Webb and Weick, 1979) was collected that was both useful for describing the context for the research groups (e.g. research group websites) and relevant for this study’s questions (e.g. research papers and grant applications, including statements of “pathways to research impact”). Such data were obtained (where possible) in electronic form so that they might be processed in the same manner as the interview data (described in more detail in Chapters 9-11).

Each case study was founded upon a 12-week period of participant observation within the laboratories of the research enterprises (Hess, 2001). The aim of employing such a method was to understand the micro-practices and innovation routines evident within the research groups. Data were captured in the form of ethnographic observations and interviews (both unstructured and structured). Unstructured interviews involved conversations with scientists that at the start of my placement sought simply to describe their work, but increasingly became guided by my exploration of questions relating to Research Impact and Responsible Innovation, and associated capabilities. In addition, advantage was taken of more formal gatherings to collect further data: examples of such events include research group meetings, researchers' presentations of their work at departmental seminars, and supervisor–student interactions. All such data were captured in field notes and transcribed into electronic format. If, as part of such interactions with the group, a subject arose that seemed relevant to my study, then a more formal (i.e. taped) interview was held to explore that subject in detail. All these data constitute the core findings of the case research. These findings are presented in a descriptive account of each case, and comparative analysis, in Chapter 9.

In addition to the opportunistic collection of data afforded by being amongst working scientists, I also held a range of semi-structured interviews. Interviews with the research group leaders sought to explore the strategic behaviour of the group. At the outset these conventional interviews sought data on familiar strategic management issues such as: differentiation from comparable research groups; alignment with mandates for greater relevance of research. These interviews were transcribed and analysed in order to discern the nature of the groups' capabilities for innovation. This analytical process and subsequent findings are described in detail in Chapter 11.

As the final element in my study I sought methodological insights from the literatures examined in Part I of this thesis. In this I hoped to derive an added benefit from being embedded within the groups and to introduce a degree of novelty into my case research by including an element of action research (Reason and Bradbury, 2006). The nature of the method I adopted, and the rationale for its inclusion within my overarching case research strategy are explained below.

The practical concept of upstream engagement of stakeholders informing innovation decision-making necessarily operates at some aggregate level (i.e. government, professional bodies or technology fields): it is not likely that a research group leader might organize his own engagement activity in order to gather different opinions from stakeholders on the future course of his innovation activities. Participating on a personal level in deliberative engagement (Wilsdon and Willis, 2004) in order to shape their own ideas about societal issues, might be one way in which the research leader could better connect with societal debates. This approach resonates with the recommendation of the Royal Commission on Pollution when they examined the case of nanotechnology: “we recommend that is desirable to move beyond one-off public engagement ‘projects’ to recognise the importance of continual ‘social intelligence’ gathering and the provision of ongoing opportunities for public and expert reflection and debate” (Royal_Commission_on_Environmental_Pollution, 2008, p.73). However, it is not evident that such practices of generating ‘social intelligence’ operate at any level, let alone that of the individual research enterprise. The response required of social research into these phenomena is suggested to be one that “is developmental, experimental, exploratory and therefore also multidisciplinary and ‘messy’” (Davies *et al.*, 2010, p.422). In this light it is possible to view the social researcher as another ‘public’ with whom the nanotechnologist might derive ‘social intelligence’. What then are the methodological possibilities for a deliberative engagement between social and natural science researchers?

In attempting to make connections with these questions of methodology, the public debates of chapter 1 and the literature on capability development, I note Sidney Winter’s comment that “as they learn new capabilities, organizations draw on the society around them for both the means and ends…[which]…include, at the highest level, socially legitimated organizational goals” (Winter, 2000, p.994). Collis’ argument (1994) that the contextual unpredictability of capabilities means that efforts to build them will “remain elusive” offers an action-oriented possibility. In the mode of deliberative engagement (cf. Wilsdon and Willis, 2004), how then might the methods of management researchers aid the exploration of new capabilities rather than advise simply on what to do? In this regards one method might be the creation of a “decision support tool” whose use Zollo and Winter note (2002, p.349) in “[facilitating] a relatively infrequent and heterogeneous task may be more valid (or at least as valid) as a capability-building exercise as for the benefit derived from the actual use of the tool”. Similarly could the management researcher stimulate an entrepreneurial venture’s central decision-maker with the provision of tools that aid the deliberate learning for the ‘invention’ of new solutions (Zahra *et al.*, 2006)? As well as the direct stimulation of new thinking, engagement with a social researcher might provide the opportunity for a nanotechnology research leader to reflexively monitor to check the ongoing workability of their research group capabilities (cf. Schreyoegg and Kliesch-Eberl, 2007). All of these perspectives speak to the tradition of action research as the methodology for generating insight when developing a capability to undertake science-based innovation with a heighten awareness of societal (and not just economic) imperatives. And yet it will be important to employ an action research method that creates the space for the nanotechnology researcher to reflexively explore their own positions and strategies, rather than one that stimulates them in a more direct manner. An opportunity to explore a suitable action research method was presented by my participation in an international research consortium led by the Centre for Nanotechnology in Society, based at Arizona State University.

Funded by the National Science Foundation (NSF) in the USA, the Socio-Technical Integrated Research (STIR) project was conceived by Erik Fisher and David Guston of Arizona State University in response to a variety of government policy pronouncements that science research ought to be more responsive to wider societal concerns. They were particularly conscious of policy developments in the USA (US_Congress, 2003) and Europe (European_Commission, 2005), but there is a clear analogy with the rhetoric of the UK policy landscape regarding the *Research Impact* of university science. The objectives of the STIR project were as follows:

- To identify and compare external expectations and demands for laboratories to engage in responsible innovation.
- To assess and compare the current responsiveness of laboratory practices to those pressures.
- To investigate and compare how interdisciplinary collaborations may assist in educating, enhancing and stimulating responsiveness.

To undertake this research programme, PI Fisher built a network of 10 PhD students (including me) from a variety of disciplines (science and technology studies, philosophy, policy science, anthropology, political science and management research) who have research interests encompassed by the *Responsible Innovation* policy discourse. At the heart of this study is the notion of interdisciplinary collaboration between natural and social scientists. Conscious of the long tradition of laboratory ethnographies in science studies (Sismondo, 2010, Chapter 10) and the dominance of cultural forms of empirical analysis (Knorr-Cetina, 1999), Fisher's own research had sort to explore the extent to which social researchers could engage with these epistemic cultures. In particular he became concerned with the extent to which laboratory decision processes might provide a vehicle for natural "research scientists to more reflexively attend to the integration of technical and social considerations" (Fisher, 2007, p.157). In order to probe such decision process Fisher developed a research protocol that he positioned (and named) in relation to upstream engagement as *midstream modulation* (Fisher *et al.*, 2006). Employing the stream metaphor of technology development enables him to place the work of the natural research scientists as operating at the interplay between upstream funding decisions by policy makers and downstream public processes of acceptance/modification/rejection of technology outputs: "viewed this way, the midstream corresponds to the implementation stage of a large, distributed and dynamic decision process" (Fisher *et al.*, 2006, p.490). From his perspective, the midstream constitutes an unexplored area of potential innovation governance; from mine, it is the place at which the individual innovating enterprise enters the arena of emerging technologies.

My participation in the STIR project and testing of the *midstream modulation* protocol represents an added element to my case research. As well as introducing an element of action research into my case study strategy, membership of a group of social and humanities scholars drawn from a variety of disciplines (philosophy, political science, policy studies, sociology, science and technology studies) added significantly to my development as a social researcher. This group provided a forum for testing the ideas emerging from my analysis as well as introducing me to their own perspective on the Responsible Innovation discourse. Whilst the interviews I conducted using the midstream modulation protocol were incorporated into my general analysis of the cases (in Chapter 9), I present separately (in Chapter 10) a detailed account of my engagement with one scientist.

Thorough a combination of these methods, both conventional and novel, I collected a rich dataset through my engagement with the research groups. A summary of all the data generated by these means is given in Table 4.

Table 4 – Summary of data generated in case studies

Method	ColloidCo	MicrosCo
Observations	Laboratory life, Group meetings, departmental seminars, supervisor–student interactions	
Regular interviews with selected researchers	19 interviews	20 interviews
Semi-structured interviews with research leaders	3 interviews	8 interviews
Semi-structured interviews with collaborators	1 interview	5 interviews
Semi-structured interviews with researchers	8 interviews	4 interviews
Documentary evidence	Websites, research papers, new research proposals, formal contracts with collaborators	

The findings derived from these data are presented in Chapters 9-11 in Part III of this thesis. These case research chapters are preceded by two chapters that explain my introduction and orientation towards the research field. The structure of the Findings section and the content of each of its chapters is described below.

Chapter 7 concerns my introduction and familiarisation to the research field of innovation and universities, and is presented below as “Pilot Research”. This chapter also serves to mark my transition from management consultant to management researcher. The projects described in the chapter were ones that were commissioned from me as a management consultant. However, during the course of the projects (and with the permission of the participants) I started to collect and analyse data that related to the research questions of this thesis. On a practical level this allowed me to practice the methods I would use in my main case research, as well as to validate the basic rationale for conducting that research.

Chapter 8 examines innovation policy in the UK as it impinges upon universities and university researchers. This exercise prepared me for the field by making me familiar with the wide ranging policy discourse that might have been influencing the innovation practice within the research groups.

Chapter 9 presents a detailed account of the way in which innovation is organised with each of my research group cases. In this it draws upon all of the data summarised in Table 4. A narrative style of case description is used and includes discussion of issues relating to: development history of the groups; their physical location; science and innovation interests; organizational and governance structures; introductions to key actors and how they describe their work; research and innovation project portfolios; commercialisation practices (industrial collaborations and spin-outs); internal group process that relate to innovation; and any other distinctive features of their innovation work. A comparative analysis of the two case studies is presented that suggests important features for how research groups already active in this agenda are responding to the changing policy landscape.

In Chapter 10 I report on my observations when closely following the work of an individual scientist. Based upon data collected using the *midstream modulation* protocol this chapter contributes to the overall case study by observing the micro-level processes of innovation at work. Furthermore, it explores (in line with the objectives of the STIR study noted above) whether a natural scientist and social scientist engaged in a process of mutual enquiry can enhance reflexive awareness during innovation work.

Finally chapter 11 examines the strategic management of the research groups. Based upon an inductive analysis of interviews with those involved in the management of the research groups, I identify constituent features of their organisational capabilities for innovation: both established and those emerging in response to new policy mandates.

The relationship between these chapters and the research questions articulated in Chapter 4 is summarised in Table 5.

Table 5 – Research questions and associated Research Findings chapters

Research Question	**Findings chapters addressing this question**
How are emerging innovation policy mandates for societal & economic impact of science influencing the innovation practices of nanotechnology research groups?	7, 8, 9, 10, 11
what are the organizational capabilities in science-led innovation?	9, 10, 11
How are capabilities changing in response to new innovation mandates?	9, 10, 11
What is the role played by the research leaders in effecting such developments in capabilities?	11

PART III
RESEARCH FINDINGS

Chapter 7
Pilot Research

In order to gain a better understanding of the contemporary challenges faced by university researchers, a small amount of pilot research was conducted. The research was opportunistic in the sense that it resulted from consultancy projects within universities at the same time as I started these (part-time) PhD studies. This pilot work provided an appreciation of the way in which the framing of mandates for societal and economic impact was being experienced by academics in research-led universities. In this manner it provided a productive grounding in the field prior to the case research proper. The discussion in this chapter is not the product of a detailed research design. Rather they are more in the nature of practitioner reflections: both of the university researchers and managers with whom I was working, and also my own reflections in the intermediary role of management consultant. Whilst each of the projects had its own practical aims, each one also provided me with the opportunity of asking university researchers to reflect on their experience of participating in Research Impact activities. Each project was commissioned independently and no pretence is made that they form part of some wider coherent research design. The account I offer is rather an attempt to draw out practitioner insights along three broad themes:

- stakeholder views of the role of university research in wider economic or societal innovation;
- the changing nature of the management of university research.
- the perspectives of individual university researchers grappling with changing priorities.

However, rather than simply present a number of brief stand-alone cases, I have created a framework for categorising the impact projects that I observed in an effort to create actionable knowledge

(Argyris, 2005) for university knowledge transfer professionals. The four projects under discussion may be placed on a 2x2 matrix (Figure 6) whose dimensions are the “Research Impact Maturity” of participants and the scope of their projects. The term “Research Impact Maturity” is intended to represent the experience and sophistication of the participants in various projects. Many researchers were completely new to this agenda (low Research Impact Maturity), whereas others (high Research Impact Maturity) were early adopters of the emerging agenda, and can point to experience across a number of projects. The second dimension concerns the scope of their (Impact) projects. On the one hand there were many projects concerning the development of “focussed” ideas that result from defined research programmes and were usual identified with an individual researcher. In contrast are projects I describe as ‘broad’ in which the aims go beyond the development of a specific idea, and concern some aspect of capacity building for the practice of Research Impact.

The four case studies to be discussed may be positioned in three of the quadrants, with the fourth project (HEIF3 Review) covering all four elements of the matrix.

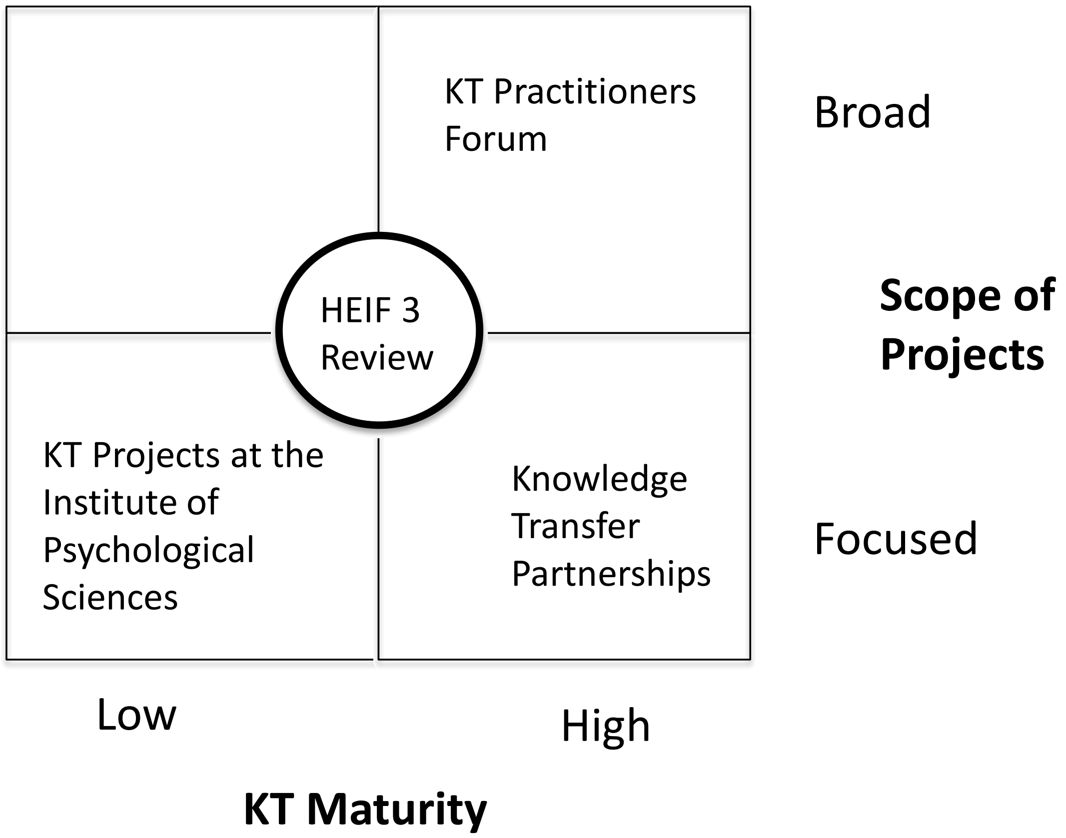

Figure 6 – Categorisation of knowledge transfer (KT) projects

Each of these projects is considered in turn, prior to my suggesting emerging themes and problematising the practice of pursuing research Impact at the micro-level of individual researchers.

Research Impact Projects within a single Research Institute

This project was situated at a single Research Institute within a UK research-led university. My brief was to support academics within this Institute who wished to build upon their existing research and develop related Impact projects. My support in this instance involved activities such as market research, patent searching, brokering new research contacts and helping with funding applications. Whilst my project work was focussed on providing such practical support, there were many opportunities in my interactions with researchers to explore the challenges of the research impact agenda. In this I was transparent about my practical and research motives. I made my own research agenda clear from the outset and received their permission to record a number of semi-structured interviews. These recordings have been transcribed and analysed in order to provide this account. A total of 16 such interviews were conducted. Each was intended to be a personal reflection of the Impact agenda and was based upon the following questions (at this time, and within the context of this Institute, research Impact was referred to as “Knowledge Transfer”):

- What does the term knowledge transfer (KT) mean in relation to your research?
- What ideas have you had for KT projects?
- How do you identify opportunities for new KT projects?

The approach to sampling during these interviews was purely opportunistic as it was a function of the project itself and decisions by the Institute’s “KT Champion” regarding which researchers would be “interesting to talk with”. Nevertheless, the researchers interviewed covered a wide range of perspectives from those who were only interested in their science (and shunned the Research Impact agenda), through those interested in exploring the practice, to those who had significant experience of KT projects.

The first theme to emerge from the interviews concerned the meaning of the term “knowledge transfer” as applied to university research. Whilst the academic literature on the subject may have identified a wide range of mechanisms (Perkmann and Walsh, 2007), this fact had yet to filter through to all academics. The strongest association of the term was with an activity with a clear commercial orientation: *“For the sort of research that I do [KT] is very rare. I think that the KT model rests on something commercial, or something marketable really applies. So, the sort of KT work I’ve done is, for example, new consultancy work*”.

In a similar vein, some researchers identified their experience of commissioned (and so in a sense commercialised) research: *“I’ve* *done bits and bobs of things I suppose in the past that would probably count as knowledge transfer. I've done some work with local councils where we evaluated an intervention that was already being used in schools"*.

Time and again during the project, this commercialisation theme seemed to be a touchstone. Whilst much of my work provided guidance on such projects, other researchers did become interested in developing other mechanisms such as advising a Government working party (on road safety) and influencing the professional development of primary school teachers (in relation to the relationship between childhood motivation and scholastic achievement).

In relation to generating new opportunities to realise the impact of research, emphasis was often placed on the value of inter-disciplinary and practitioner networking opportunities: *"When I think back to the two experiences uppermost in my mind, had it not been for that serendipitous networking, the head teachers and staff responsible for trying to meet government targets in terms of boys' improvement would simply have carried on in terms of [their] on-going experience. It was only when there was, in a sense, an intrusion of some sort of specialist knowledge that thinking started to change"*.

Although this particular researcher described the encounter as serendipitous, his very attendance and participation at such an event might be evidence of a key opportunity-seeking behaviour. Again, as we might have expected, the personal style of researchers influences whether they are prepared to place themselves in the unfamiliar environment that such networking implies. While some would actively seek out opportunities to meet new people, others preferred the comfort of their own discipline.

Such conclusions are ones that we might reasonably have expected without conducting detailed interviews. The question of significance might actually relate to how a new generic mandate to have greater involvement with user-groups throughout the research process will play out amongst an existing diverse community for whom such research does not come naturally. As one researcher expressed it: *"[my work] is becoming more like a business and I don't like it. If I wanted to be a businesswoman, I would have gone into Business"*.

For those who felt motivated to actively participate in this new agenda, then there was clearly no shortage of ideas. When providing practical support for such idea-rich researchers, then there were obvious (and known) challenges associated with finding funding for new ideas. However, beyond this there were other challenges that were not reflected as much in the support being offered to researchers. The most significant of these was the daunting (to researchers) prospect of securing early adopters. It is known (Chakravorti, 2004) that innovative ideas progress through a phase of needing to secure early adopters who can not only support the idea during its practical development, but are influential enough to ensure that the idea is taken up more widely. University researchers who are well networked amongst practitioner groups were adept at both foreseeing significant future potential impacts whilst being sufficiently grounded in the practicalities of innovation to understand the hard work required to secure the first active user. However, such researchers seemed in my experience to be in the minority and it was common to find that projects suffered from a lingering death if they are not given sufficient marketing focus to drive a "go/kill" decision (Cooper, 1993). In some cases a commercial firm could be found in order to provide the appropriate marketing focus. More generally universities have responded by creating a range of mechanisms to marshal projects through a "proof of technical concept" or "proof of commercial feasibility" assessment. Whether based on internal or external (e.g. Venture Capitalist) expertise, these mechanisms are resource-intensive at the level of individual projects, and as a result only a relatively small number of projects can be pursued. This begs the question of how decisions are taken on which projects to support. In a policy environment in which all researchers are being actively encouraged to pursue research impact opportunities, the pressure on conventional support mechanisms (e.g. business development advisers such as the role I adopted in this project) will increase.

Important questions thus emerged from the new impact agenda regarding how research is to be managed. If the university research process does not stop with publications, but continues with enterprise and knowledge transfer activities, then how is this extended process to be approached? At what level are key decisions on research resource allocation to be taken: individual researchers, departments, faculties or at the level of the whole university?

My project at this Institute concerned itself with individual researchers (and their research groups), and it was evident that at this level there is no shortage of ideas to boost research impact. Often the very nature of university research makes for a *scalability* challenge to extend the influence and tacit knowledge researchers bring to their networks of research participants. This challenge of scalability of ideas founded heavily on tacit knowledge brings an extra layer of complexity to the issue of early adoption mentioned. The adoption of new ideas/products/services may never be easy, but will be facilitated if that new idea/product/service can be made available to new users at their convenience. When the new innovation is crucially dependent on the tacit knowledge of a particular researcher, then their availability (as well as that of new users) becomes crucial (Agrawal, 2006). In this next section I briefly explore an established and exemplary innovation scheme that seeks to facilitate the transfer of such tacit knowledge.

Knowledge Transfer Partnerships

Knowledge Transfer Partnerships (KTPs) are a government-funded programme that seeks to connect organisations to new knowledge being created in universities. The partnership between an organisation and a university researcher is facilitated through a young researcher (known as the KTP Associate). The Associate is employed by the programme to work on a knowledge transfer project within the partner business, whilst at the same time receiving research project supervision from a university researcher; expert in the project area. This three-way relationship between the business project manager, Associate and university researcher has proved an extremely successful knowledge transfer mechanism, and has given the overall scheme a reputation for effectiveness throughout its 25-year history.

I undertook a short research project (with Prof Richard Thorpe) on behalf of the ESRC that addressed the following question: can the training and development opportunities for KTP Associates continue to be delivered if ESRC recognition of the Higher Education Institutions' (HEI's) capabilities was reviewed and, if it was, what would be the effect on the quality of that training?

Whilst having a specific brief, the research involved interviews with all the scheme's stakeholders, and this gave insights to the knowledge transfer process in the context of specific projects. The scope of these KTP projects was focussed on particular questions and involved stakeholders (and a KT process) that had a high degree of maturity, and so they may be positioned in the bottom right-hand corner of Figure 6.

The principal conclusion from the ESRC perspective was that there was no clear evidence to suggest that HEIs on their approved (research training provider) list have delivered better quality ESRC KTP projects than those that are not listed. It was apparent through the research that the key success factor for the projects was the KT competence displayed by the three people at the heart of partnership (Associate, Business Manager and University Researcher). Facilitating the development of such competence of KT is the key goal of those stakeholders in a supportive role (e.g. Government-funded KTP Advisers and trainers). This work of people in support roles is implemented within the context of a well-established and extensive project management framework. The framework details stakeholder meetings, supervision requirements and Associate training courses. The KT project itself is thus implemented within a comprehensive network of support that is resource intensive. The Government's KTP programme is demonstrably successful at the level of individual projects, but the extensive nature of the support means that it is necessarily limited in the number of university researchers who can participate. What then are the implications for such researchers outside this cocoon of intensive support? KTP projects show that there are effective means of transferring both the codified and tacit knowledge embodied in new research. The Research Impact agenda is requiring all university researchers to proactively pursue comparable projects. How then, in the absence of intensive support, can they be expected to succeed? What are the new competencies they need to develop? How is their overall research process to be managed? In the next part of this chapter I will outline a project in which I facilitated a focus group of KT-active university researchers as they explored the challenges of the new impact agenda for all researchers.

Yorkshire Universities' KT Forum

Yorkshire Universities was a network body representing all the HEIs in the Yorkshire and Humber region. In one element of their work they convened a forum of researchers who were experienced in the practice of knowledge transfer with universities. The purpose of this forum was to explore the possibility of innovation in the very practice of knowledge transfer. The importance of such debate was judged in relation to the Research Impact agenda and its implications for the widening of researcher participation in knowledge transfer projects.

My role in this project was to facilitate the discussion, and for this I choose a process for exploring Innovations in Management (Hamel and Breen, 2008). The account offered here is a very brief summary of the output from workshops and two benchmarking visits to innovative organisations operating in very different domains to those of university researchers. In relation to the framework in Figure 6, this project may be placed in the top right hand corner. The perspective of the Forum's members was broad in that it encompassed all aspects of KT practice. Furthermore, membership to the group was by invitation and limited to people who not only had a track record in KT projects, but also were known for the sophistication of their practitioner thinking. If researchers of such KT maturity are identifying particular issues as being important, then management researchers need to give these issues some attention. In the account here I will emphasise aspects of the whole project that relate to my own emerging research interests.

In the first of this Forum's workshops I got the researchers to explore the key current challenges facing universities aiming to increase their research impact. They articulated these challenges as follows:

- How do we incentivise or reward researchers to undertake KT projects?
- How do we move beyond science/engineering projects and involve other disciplines in this agenda?
- How are researchers to manage funding risks (e.g. research grants vs. KT income)?
- How do we increase the recognition for KT (i.e. scientific papers vs. KT outputs in research assessment audits)?
- How do we leverage the whole (multi-disciplinary) offering of universities?

The level on which the group chose to concentrate was that of the university as a whole. However, they would frequently move between this level and that of individual researchers and their research groups. At the researcher level key themes from the discussions were:

- Need for creativity skills and overcoming limited personal frames;
- The challenges of finding "real problems" (societal or economic);
- Leadership of the research effort;
- Competing demands on time;
- Departmental obligations placed upon researchers;
- Making connections with researchers in other disciplines;
- A shortage of role models.

The questions suggested here, arising as they do from conversations from seasoned KT practitioners, have an important influence on my case research. However, in order to appreciate how change is being felt across all disciplines and levels of familiarity with the impact agenda, a final project is now examined which encompasses all quadrants of the framework in Figure 6.

HEIF3 review at a Research-led UK University

The final project considered in this analysis of my KT practitioner experience involves a review of the campus-wide range of KT projects at a research-led UK university funded as part of the HEIF3 (Higher Education Innovation Fund – Round 3) programme. My project brief was to summarise and present the range of projects under a number of categories (i.e. public policy, consulting, continuous professional development, applied research, commercialisation, enterprising culture and partnerships) of KT mechanism in order to show how HEIF3 investments leveraged other activity and funding. In the account that follows I offer my analysis of the differences I noted in the types and KT maturity of the programmes that I observed. As might be expected from such an extensive round of funding, I encountered projects that might be placed on all 4 quadrants of the framework in Figure 6. This exercise also suggested a development pathway for the facilitation of KT projects that allowed me to characterise the quadrants as in Figure 7.

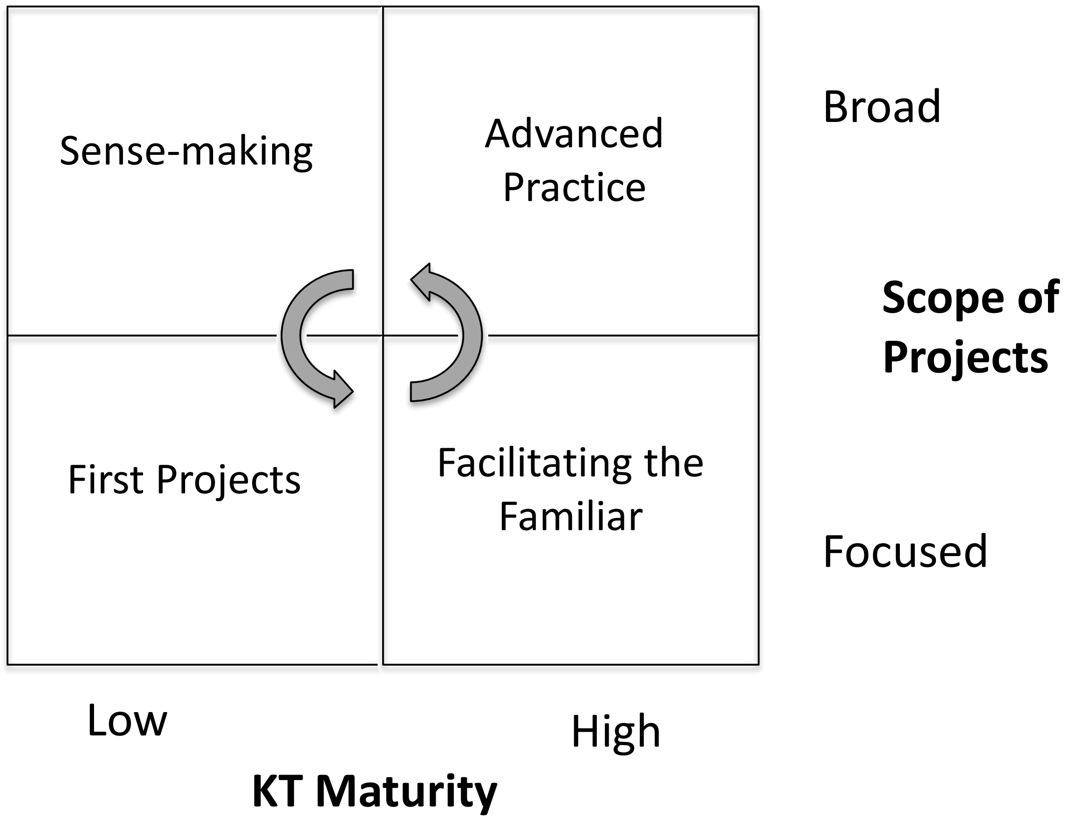

Figure 7 – Labelling of quadrants on matrix of KT projects

Many of the projects funded involved departments and people new to the KT agenda. These projects sought answers to questions such as "What is the KT potential of this Department/Faculty?" and even "What does knowledge transfer mean in our context?" Typically activities in this regard included interviews with academics, marketing studies and building a local KT management infrastructure. These projects are notable for their broad scope and the low degree of KT maturity on the part of their participants. They are placed in the top left-hand quadrant and can be thought of as "sense-making". The characteristic rationale for projects in this quadrant is to build commitment for the Research Impact agenda.

The next stage in the support pathway concerns the first projects endeavouring to realise an impact from a specific research idea. Such activities involve finding early enthusiasts amongst the research community and funding their ideas. The stage also sees the continued development of an enterprising local culture attuned to the

KT philosophies. Activities in this regard include the production of an internally oriented website and holding networking events to communicate the potential of KT projects for academic researchers. I have labelled this kind of activity “First Projects” and success is built upon a rationale of leveraging the efforts of early advocates of the KT agenda.

As the degree of experience with these projects increases, then a stage is reached that is characterised by the breadth of the projects supported. These projects tend to fall into recognisable categories noted in my project brief above as practitioners pursued mechanisms that have worked for others. The work of people supporting these projects acquires a more external orientation as websites become more outward facing, and brokerage between disciplines and external organisations becomes critical. The phase of development is located in the lower right quadrant in Figure 7 and is labelled “Facilitating the Familiar”.

The final stage in development sees a move from facilitating specific KT projects to broader ideas that seek to embed KT practice into academic life. Those in support positions become concerned with building capacity within the academic community for KT itself. In this regard, indicative projects include the creation of postgraduate training courses that aim to instil the importance of impact at the outset of academic careers. We have reached the top right-hand corner of the 2x2 matrix and whilst I have labelled it “Advanced-Practice”, it can also be thought of as a renewed phase of sense-making. Except that the sense-making is now at a deeper, experience-informed level than that which started the development pathway. A summary of this suggested pathway is given in Table 6.

Table 6 – Pathway of development of KT management at one research-led university

Stage	**Challenges**	**Typical Projects**	**Characteristic**
Sensemaking	What does KT mean? What is the KT potential	Interviews with existing KT-active academics. Marketing studies. Building KT infrastructure.	Building *commitment* from the researcher community
First Projects	Supporting individual projects of early enthusiasts for KT policies. Promoting the agenda.	Internally-oriented website. Showcasing events. Publishing case studies.	*Leveraging* the efforts of the early adopters of the knowledge transfer agenda.
Facilitating the familiar	Effective supportive mechanisms for consultancy, CPD, applied research, policy work	Externally-oriented website. Brokering links with new collaborators. Funding individual projects, whilst developing applications for large programme grants.	Increasing the *breadth* of the KT project portfolio.
Advanced Practice	Embed KT into academic life. Renewed sensemaking	Capacity building. Personal KT development programmes. Strategic partnerships	Creating projects of greater *depth*.

My meaning in presenting a wide range of different KT projects in this manner is not to suggest a simple stepwise process, but rather to express the knowledge gained through my analysis in a manner than allows KT professions to reflect on their practice and take action. The matrix is a simple device to aid reflection on the project portfolio of different Faculty teams. Positioning such teams upon this matrix allowed me to suggest a number of problems or challenges for KT practice (at least at one research-led institution):

- Why do different faculties progress at different rates (when all received the same degree of central support)?
- Why are the applied, professional disciplines (e.g. management, law and education) apparently the slowest in moving along this project development pathway?
- How does the role of the central support function change as KT maturity increases?

At the level of individual projects then the challenge of scalability of tacit knowledge and securing early adoption of new ideas noted earlier in this chapter were seen across a range of disciplines. Taken as a whole these projects bear witness to a changing agenda in which all university researchers are being asked to proactively pursue the realisation of impact of their research. This begs significant questions for how university research is managed. Such management, as may be evident from the policy and practice perspective covered so far in this section, operates at a number of different levels: Government/macroeconomic, university governance, faculty management and individual research teams. This PhD study concerns itself predominantly with the latter.

Before bringing this chapter to a close, I beg the reader's indulgence and offer a short account of what has transpired at this same research-led institution since my study. When presenting the completed study to the central university team overseeing the HEIF budget (and using Figure 7 and Table 6 in the process) I suggested that they faced a number of strategic challenges. These were consistent with the trends in activity, and the movement towards a renewed round of sense-making within the institution regarding knowledge transfer. I argued that the observed increase in maturity of KT practice amongst academics could lead to pressures for a transfer of KT administrative power from the centre to the faculties. If this happened then their continued role would be dominated by the provision of specialist services, such as patenting/licensing and consultancy support, for which there could never be economies of scale within faculties. These arguments met with polite thanks. Whilst I cannot claim to have forecast the details of the subsequent organizational change, the broad thrust of that change has seen a move away from central management of KT within the institution, with KT now being organised around discipline or faculty-related “Hubs”. At the centre reside a number of specialist support functions (including patenting/licensing and consultancy support), and the team who commissioned my study no longer exists.

The conduct of this Pilot Research (within the context of real efforts to advance the practice of Research Impact) reinforced the topicality of the questions being explored in this PhD research. In combination with the more generalised experience evident from the management literature discussed in Chapter 2, answers to this study’s questions speak to both scientists working to realise the Impact of their research and management researchers in this field.

Chapter 8
Critique of Recent UK Science & Innovation Policy

At the outset of the case research described in the following chapters, I undertook a critique of UK innovation policy as it affected university researchers in emerging technologies. I continued to stay abreast of policy changes throughout the 18 months when I was engaged with my chosen research groups. Where in chapter 1 I sought to articulate the overarching rhetorical themes in the policy discourse, this chapter seeks a more detailed examination of the practical consequences for researchers from the application of these innovation policies. The importance of these policies was quickly apparent during my case research, as researchers made efforts themselves to keep abreast of the latest policy pronouncements; particularly as they affected the likelihood of attracting research funding.

This chapter considers the evolution over the last 15 years of innovation policy in the UK as it affects innovators operating in and with university research groups. This review will show that, whilst this policy development has explored many aspects of the UK innovation system, a consistent feature has been a narrative that universities have a new and important role to play in the competitiveness of a modern economy (cf. Triple Helix literature in Chapter 2). I will argue that despite all this policy work, the challenges of how to connect university research with wider economic and societal imperatives have not all been completely resolved. As alluded to in chapter 1, much of the discourse has been conducted at a macro-level (universities, research councils, Government and its agencies) and the institutional responses to a changing innovation landscape. This chapter aims to work through the detail to the more micro-level guidance and obligations for researchers to the various policy enactments and the organisational-level implications for the research groups they create.

During the last 15 years an increasingly prolific stream of policy research has been conducted founded upon the belief that innovation is the economic imperative of a modern developed economy. Defined by the UK Government as the “successful application of ideas” (U.K._Government, 2006a, p.3) the growing importance of “innovation” may also be discerned in the changing name of the business-facing department of Government: The “Department of Trade and Industry” has passed through two rebrandings during this period, first to the “Department of Innovation, Universities and Skills”, and then to the “Department of Business, Innovation and Skills”. Under the Labour Government of the turn of the century a series of high profile reports were commissioned that explored different elements of the innovation landscape. Named after the leading figures in public life that were asked to chair the studies, these reports played a dominant role in enriching contemporary Government innovation policy. At the outset of this PhD project all this policy development reached its peak with the publication of a white paper entitled “Innovation Nation” (U.K._Government, 2008). This paper included in its provisions the creation of an “Innovation Research Centre in order to ensure “a steady supply of high quality innovation research into the UK innovation policy community” (ibid, p. 7). This capability in policy development continues to be used by the current (in 2014) Coalition Government.

Contemporary with the development of business-focussed innovation support another policy discourse was gaining momentum that was concerned with the societal dimensions of science-related innovation policy. Led by think-tanks, professional institutions and NGOs, this work explored and experimented with the idea of democratising science and innovation policy; extending the influence on decisions about which research to support and which innovation challenges to address.

This chapter will outline the development of these strands of science-based innovation policy and suggest that one contemporary point of intersection surrounds a series of debates taking place under the headings of “Responsible Innovation”. Rather than present the business-focussed and societal-aware strands of policy separately, a broadly chronological account will be offered of both together. This will hopefully serve to show how at various times during the last 15 years, these debates have acted both in ignorance and in sympathy with each other. The current debates concerning Responsible Innovation may then emerge without privileging one set of interests over the other. It should not be a question of “big R” (Responsible innovation) with an over-emphasis on societal influence nor “big I’ (responsible Innovation) with its stress on business concerns. Giving voice to both domains of concern holds out the hope that they can be reconciled in some practical sense.

Charting the course of any policy narrative is not an exact science. Reliance is made on a “grey literature” of white papers, commissioned research, pamphlets and reports from NGOs and professional institutions. Add to this mix the growing complexity provided during this period of blogs, and it becomes near impossible to identify all the lines of influence in the creation of a given policy position. Therefore, what follows in this chapter is this author’s reading of this multifarious literature. At the outset of this account we must note that implying one particular date as constituting the start of any policy development would be misleading. In reality a policy discourse as complex as that for science and innovation has varied antecedents. This account begins in 1998, as this was a time of significant recent change in the UK political landscape. In addition, the impending millennium heightened political rhetoric as many science and technology stakeholders contemplated innovations for the 21st Century.

Accepting these caveats, then the study for the Higher Education Funding Council for England (Hefce) entitled “Industry-Academic Links in the UK” by Howells et al. (1998) is a good point of departure, as it identified factors that were to re-emerge as important for the role of universities in innovation policy. Based upon a rich data set including surveys of industry liaison officers and interviews with senior university personnel, this study provided a comprehensive account of the state of interactions between industry and UK universities. Findings included: a growth in research funding by industry, noted to be a key motivating factor for academics; an apparent growth in income from codified intellectual property; closer involvement with local and regional economies noted; initiatives to meet industry training needs; evidence of both forming consortia between HEI and increased competition for funding. While an interesting feature of this report is the attention given to teaching and training links with industry, we see that 15 years ago basic terms of these debates were already set out: industry as a source of research finance; role of universities in regional development; careful management of intellectual property. The report also contains a short discussion (Howells *et al.*, 1998, p.63) of the UK Universities Research Assessment Exercise (RAE) and how it was forcing academics to concentrate on publications at the expense of developing links with industry. This debate on assessment and on how the contribution of universities to the wider economy was to be an important theme over the coming decade.

Some of the same themes were re-stated in 1999 as part of the *Baker Report* exploring the “Economic Potential of Public Sector Research Establishments” (U.K._Government, 1999). This study recommended “leadership in the PSREs be committed to drive commercialisation as an explicit part of their mission” (*ibid,* paragraph 1.19). Surfacing an issue that continues to resonate in this area today, Baker highlighted the importance of clarifying ownership and responsibilities for managing intellectual property generated in these organisations. And yet recognising the inherent difficulties of commercialising scientific research, further recommendations advocated new connections for scientists to experts in the business community.

At the start of the new millennium the UK Government produced a detailed statement of its innovation policy in a white paper entitled “Excellence and Opportunity: a science and innovation policy for the 21st Century” (U.K._Government, 2000). This paper announced major investments to promote science and innovation and including £1bn investment in science infrastructure and the establishment of the “Higher Education Innovation Funding” (HEIF) dedicated to applied work and thereby ensuring no dilution in the use made of pure research investments. A further support for business-relevant university work, saw the Government signal its intention to monitor RAE 2001 to see if guidance was followed to give equal weighting to basic and applied research. In an interesting development in the UK innovation policy narrative a whole chapter in the paper was devoted to the role of consumer in innovation. This included consideration of risks and the "social and ethical challenges" of scientific advance (*ibid.*, p. 50). This narrative was couched in terms of "public trust [in] innovation", and made reference to the (then) recent GM foods controversy. The white paper drew attention to the critical role of science in assessing the risks of emerging technologies, with the policy response being one of regulation, commissions for public dialogue and science communication initiatives.

The ways in which these issues related to the emerging field of nanotechnology was the subject of an Economic and Social Research Council (ESRC) funded critique (Wood *et al.*, 2003). The report started by outlining basic nature of nanoscience & nanotechnology, before assessing the current perceptions of commercial applications and considering social and economic dimensions to such developments. In a series of recommendations relating to the implications for social science, the report's authors argued that the pace of change of nanotech research meant that a gap was opening with the slow progress of associated social research. Such social research, they believed, should be more than just a conduit or facilitator of public debate. A summary of the social research that followed this clarion call is outlined below. However it is worth noting that most of this research has been conducted within the domain of science and technology studies, and their suggestions for management studies such as "managing the unforeseeable nature of problems; organizational development; managing change" (Wood *et al.*, 2003, p.41) have received comparatively less attention.

In order to further explore the uses of nanotechnology and their implications, The Royal Society and Royal Academy of Engineering jointly commissioned a study (The_Royal_Society, 2004) which took evidence from of a very wide range of stakeholders (scientists, professional societies, industrial companies, Government agencies, NGOs) and also include workshops and surveys amongst the general public. Its aims included the identification of the “health and safety, environmental, ethical and social implications…[that] may arise from the use of such technologies” (*ibid.*, p. vii) and to identify where extra regulation was needed. This exercise was the first extensive research and engagement exercise for attitudes towards nanotechnology in the UK. Four issues emerged which relate to different perceptions of risk: the need for accessible commentary on any long-term uncertainties; governance of nanotechnology; enthusiasm for potential benefits; ethical concerns over messing with nature. The positioning of the recommendations for action resulting from the study were very much upstream on the innovation journey, that is to say they concerned decisions of what innovation challenges and research projects should be given public investments. Specific recommendations included: incorporating public values into decisions; improving decision quality; resolving conflict between stakeholders; improving trust in institutions; informing/educating people about science and technology. And a variety of approaches to public dialogue were suggested (*ibid.*, p. 65); all of them positioned upstream.

The theme of upstream engagement then became a dominant innovation policy discourse. The Think-tank Demos produced an influential pamphlet (Wilsdon and Willis, 2004) in which they argued “we are on the cusp of a new phase, in which public engagement moves upstream [because] most immediately, policy makers and the science community are desperate to avoid nanotechnology becoming the next GM” (*ibid.*, p.19). Their commentary identified the considerations to be borne in mind when designing a particular engagement exercise: should it be deliberative or snapshot? Representative? Hierarchical or non-hierarchical? Consensual or exploratory? This advocacy from Professional Societies and Think-tanks resulted in a host of exercises in public dialogue including “Nanodialogues” (www.demos.co.uk/projects/thenanodialogues), “Nanojury UK” (www.nanojury.org.uk), “Nanologue” (www.nanologue.net), “Nanoforum” (www.nanoforum.org). These exercises (and others covering other emerging technologies) were subsequently reviewed in a report (Gavelin *et al.*, 2007) commissioned by the Joseph Rowntree Foundation. Whilst being in support of all efforts to democratize science and innovation, the report criticized the lack of understanding and capacity for public engagement amongst policy-making institutions. A more thorough academic critique of upstream engagement (Davies *et al.*, 2010) agreed with this assessment, memorably describing it as “an unending buzz of conversation, acting merely as a soundtrack as, by design or default, decisions are made elsewhere” (*ibid.*, p. 421). In conclusions relevant for research such as the current PhD study, the same authors examined the role of social science concluding “in order to deal with these challenges [current upstream positioning of debates and public invisibility] we might suggest that we need a social science which is also emergent; that is developmental, experimental, exploratory and therefore also multidisciplinary and ‘messy’” (*ibid.*, p.422).

A measure of the challenge faced by advocates of public engagement may be judged by the recommendations of the UK Government’s 2002 strategy for nanotechnology manufacturing (U.K._Government, 2002). A contemporary to the emerging consensus on engagement with publics over nanotechnology strategy, this report may be read as advocating the selling of benefits from nanotechnology to an otherwise confused public. In a study panel composed largely of engineering professors, this report sought to “offer to Government the considered views of academic and industry experts on the steps that need to be taken if the UK is to build on its current investments in nanotechnology research and become a world class player in nanotechnology applications” (*ibid.*, p3). The obstacles to achieving this goal were delineated as: lack of strategy for public support; fragmentation of research and industrial capabilities; absence of level playing field for international competition; access to people with appropriate skills. Whilst this particular study appears to have had minimal influence, it serves to demonstrate the ambivalence towards public engagement in UK policies circles and the relative lack of sophistication in understanding the societal dimension to emerging technologies compared with contemporary policy debates in the USA (National_Science_Foundation, 2001).

The theme of risk management continued to be developed within Government and emerged in the 2003 report from the Department of Trade and Industry (U.K._Government, 2003a) as “Outcome based regulation” (*ibid.*, p92). Examining the relationship between regulation and innovation, this framing was essentially that regulation could stifle innovation if it is too heavy-handed. The proposal of “Outcome-based regulation” would operate based upon “the fundamental principle of the New Approach [of confining] legislative intervention to a set of ‘Essential Requirements’ that are of public interest” thereby affording “maximum flexibility… to manufacturers over the choice of technical solutions they use to meet the requirements” (*ibid.*, p92). Whilst not completely antithetical to upstream engagement (the latter could inform outcome-based regulation), the focus is clearly different. Outcome-based regulation is fundamentally a downstream process and the report contains no discussion of public involvement; rather experts in government agencies in consultation with business and NGOs were set to trial the approach with pollution legislation.

The theme of commercialising university science last noted here in connection with the Baker Report was further developed with the 2003 *Lambert Report* on business-university collaborations (U.K._Government, 2003b). Taking a demand-led (i.e. industry-led) approach, this study explored the practical challenges for businesses working with the university research base. Extending the arguments of the Baker Report on the commercialisation of research, Lambert highlights the importance of personal interactions, non-linear models of innovation and the importance of proximity between business and the university. The report has proved to be pivotal, as it obliged all stakeholders of university-related innovation to raise the sophistication of their innovation strategies. For universities this led to greater resources (most notably the HEIF monies) to develop their knowledge transfer management function (variously called outreach departments, technology transfer offices, commercialisation services, etc). Not all universities were supported equally as the principle was established that those universities who proved themselves most successful, across a range of measures, would be rewarded with continued funding.

Lambert had also recommended further reviews of the contributions made by university research to business innovation, and so Peter Warry was invited in 2006 to chair a study on the economic impact of research council investments (U.K._Government, 2006b). Following this report, the issue of *Research Impact* (as the knowledge transfer agenda now came to be known) was placed firmly on the agenda of all research councils. Initially recommendations were made to include assessments of impact on the peer review process of grant proposals, and regular reviews were scheduled to evaluate how each research council performs in terms of making investments with impact. Another change saw one of the research council chief executives assuming responsibility for this agenda across all eight councils. The action plans that resulted from these recommendations first became manifest to researchers with the requirement after May 2009 to produce a two page "Impact Statement" in support of new grant proposals. At the time a short guidance note was produced to help researchers in writing these statements. It was founded on the idea that the onus was to be placed on researchers to identify who will benefit from their researches, how these benefits will be realised, and what actions will be undertaken in order to connect potential beneficiaries with research outputs.

This responsibility for academic researchers to articulate the usefulness of their research was now combined with the criticisms (noted earlier) of the Research Assessment Exercise to produce the policy initiative that has subsequently dominated debates in research in UK universities: the Research Impact agenda. The consequences of this policy thinking became evident in a consultation document issued by Hefce, the Higher Education Funding Council for England (U.K._Government, 2009a) regarding its proposals for the Research Excellence Framework (REF); the successor to RAE. Their initial proposal was that the 2013 (this was subsequently changed to 2014) assessment of university performance should take into account considerations of research impact, and a total of 25% of the available assessment score would be based on impact. These proposals were met with a chorus of opposition from academics that was noted in Chapter 1. In this opposition we see familiar lines of opposition being drawn The ‘disinterested’ nature of science (Merton, 1973) is presented as sacrosanct: any attempt to link it with external interests is to risk its compromise. Whilst the potential benefits of science were not questioned, the involvement of those who had a stake (economic or social) in those benefits, in the direction and conduct of research, would weaken the very foundations of that research.

However, there was a sense of implacable change in the response of Hefce which was to commission a pilot exercise (U.K._Government, 2010) across five disciplinary areas to explore and make recommendations on how an assessment of research impact would work in practice. This pilot exercise was conducted at the same time as the data generation for the second case study in this thesis. The scientists and engineers I encountered during my research were making sense of the early policy proposals (U.K._Government, 2009b) about the implications for the assessment of their work. This consultation document listed a “common menu” (*ibid.*, p. 41) of types of impact: highly skilled people, creating new businesses, attracting R&D investment, better informed public policy-making, improved health outcomes, progress towards sustainable development, cultural development, improved social welfare, and a catch-all “other quality of life benefits” (*ibid.*, p. 42). In short, influence and progress in any area of human endeavour. For the scientist researching an emerging technology, there was much here that was familiar in terms of the economic impacts of their work. However, the breadth of other social or cultural indicators on the impact ‘menu’ hints at the possibility for differentiating their own claims for impact.

Contested positions on the subject of Research Impact operate at a number of levels. In addition to those scientists (noted above) who found objectionable any notion of potential impact being a judge of what counted as good science, there was a more subtle tension involving any discourse that went beyond the economic. The reading of innovation policy development offered in this chapter suggests an interweaving of economic and the societal possibilities for nanotechnology, but also of concerns. We have seen throughout this discourse that these two dimensions are not natural bedfellows. The economic-oriented discussion looks clearly towards what is possible, where the heartland of the societal contribution is in voicing concerns. The final grand statement of the Labour Government's science and innovation policy entitled "Innovation Nation" (U.K._Government, 2008) makes this tension manifest. Whilst covering all the (by then) familiar commitments to supporting innovation the white paper positions such endeavour within the context of a wider view that is not purely economic: "the secretaries of state have set out their aim to achieve 'a society that is excited about science, values its importance to our economic and social well-being, feels confident in its use and supports a representative, well-qualified scientific workforce'" (*ibid.*, p. 31). And yet the associated "science in society" strategy did not form part of the same white paper, but rather followed on as a separate initiative later the same year. Keeping the societal dimension of innovation separate from the economic dimension in this manner reinforces a view the societal contribution is to surface issues of concern which must then be addressed by economic agents. An alternative view that the social and the economic could be integrated can be seen in the emergence of a discourse cognate with the ones already covered in this chapter and called *Responsible Innovation*.

The very label of this discourse with its the notion of innovating responsibly indicates a shift in attention from very tangible economic and financial benefits to more vague concept of adding value to society (Enderle, 2009). Whilst the term is resonant with meaning (and perhaps because of that) no one definition of responsible innovation has achieved wide currency. A suggestion by Von Schomberg (2011) is presented here as it offers a good outline of the different threads of this discourse: "Responsible Research and Innovation is a transparent, interactive process by which societal actors and innovators become mutually responsive to each other with a view to the (ethical) acceptability, sustainability and societal desirability of the innovation process and its marketable products (in order to allow a proper embedding of scientific and technological advances in our society)" (2011, p.9).

These debates have resulted in a number of policy enactments and public investments in social research in order to complement technology developments (e.g. US_Congress, 2003, European_Commission, 2005). Such social research has generated a number of distinct responses to the challenge of a transparent and iterative process between innovation actors defined by Von Schomberg. Formal approaches to technology assessment have been developed with the methods with “Constructive Technology Assessment” (Rip *et al.*, 1995) and “Real-Time Technology Assessment” (Guston and Sarewitz, 2002) attracting the interest of policy-makers in the EU and USA respectively. Karinen and Guston (2010) advocated more adaptive or anticipatory forms of innovation because of the time lag between conducting original research for emerging technologies, the development of new products based upon that research and the realisation of full impacts of that technology. In the UK we have seen a continued interest in the methodologies of risk assessment (Shatkin and North, 2010). And as discussed earlier in this chapter, ideas on upstream engagement have led to large organised public debates on the future course of nanotechnologies (Stilgoe, 2007, Jones, 2009).

A thorough critique of policies relate to the “responsible development” of nanotechnology was commissioned by the EU to examine how policymakers might “regulate in such a way as to enhance innovation, but remain sensitive to public concerns and potential risks in the environment and human health” (Davies *et al.*, 2009, p. 3). Involving researchers from sociology, political science, philosophy and ethics, this three year study examined the varied approaches being taken to the “responsible development” of nanotechnology: upstream engagement, ethical analysis and new governance mechanisms. The study’s findings were extensive but summarised in the idea that current activity was “still dominated by limited and limiting modes of thought [which] require reconfiguration in order to fulfil the promise of socially responsible nanotechnology” (*ibid.*, p. 3). Examples of such modes of thought include ingrained views on the division of moral labour or the limiting characterisation of public attitudes as being either “pro” or “anti” nanotechnology. The report concluded with 10 recommendations for public policy aimed at reconfiguring responsibility. These recommendations might equally be a guide for social researchers in this field who themselves need to develop “more innovative methods of engagement” (*ibid.*, p. 33).

All these elements of discourse on responsible innovation continue to play an evolving role in the development of regulation and the public funding of research and innovation with nanotechnologies. And yet in spite of all this important and necessary work, a question remains on the implications for innovating organisations beyond responding to new regulation. My interest in this PhD programme is to explore how responsible innovation might be built into the organisational strategies and practices of innovating enterprises so that they might deliver upon the promise of the policy discourse (Von Schomberg, 2011). Whilst some large “Constructive Technology Assessment” programs have involved a range of innovation partners including industry (e.g. Rip, 2005), more generally, efforts to connect innovating enterprises to this agenda have been limited. As responsible innovation policy instruments are developed, increased efforts will be needed to involve commercial partners. As a first step in this direction, and contemporary with my own empirical work, efforts to use funding incentives to engage university research groups in responsible innovation (Owen and Goldberg, 2010) and the development of tools to encourage socio-ethical reflexivity (Fisher *et al.*, 2006) showed promise for widespread applicability. Since the completion of the empirical work for this PhD an excellent framework for understanding responsible innovation has been published (Stilgoe *et al.*, 2013) that draws together different threads of this policy discourse. Coincidentally, it was published at the same time, and in the same journal, as the principle research paper resulting from this PhD programme (Pandza and Ellwood, 2013).

At the outset of this research, questions of the economic and social benefits of emerging technologies were conducted, for university scientists in the UK at least, under the auspices of the Research Impact agenda. Such intentions may be readily understood as flowing from the innovation policy discourse over the previous decade and sketched out in this chapter. And yet the very rationale of the proposed changes remained contested with important practical issues still unresolved. The REF2014 assessment exercise (as was the case with its predecessors, the RAE) was to place the spotlight on individual researchers who are required to submit their work for scrutiny. How has this evolving policy discourse about both economic and societal benefit played out at the level of the individual researcher or research group? Policy instruments and funding streams (e.g. the Higher Education Innovation Fund) had been developed that significantly enhanced institutional-level resources within universities to support Impact, but how has this influenced the work of researchers themselves? No longer simply a matter of suggesting possible applications of research as part of the writing and securing of research grants, the REF policy meant that Impact would need to be demonstrated in order to secure core research funding for Universities. How has this mainstreaming of the Research Impact agenda affecting the organisation of innovation activities within university research groups?

Chapter 9
Case Descriptions

The aim of this chapter is to provide a descriptive account of the research and innovation work in both of my case studies. It is derived from the 12 weeks I spent amongst the researchers in their laboratories: speaking with them, listening to their presentations, reading their papers and participating in their group meetings. At the very outset my practical work of data generation was guided by a few simple questions rather than any detailed theoretical lens:

- Where did the work of research and innovation take place?
- Who was involved?
- What innovation projects did they participate in?
- How did they organise their research and innovation activities?

As my understanding of the case research enterprises grew, and as I iterated between the empirical material and ideas resulting from the literature/policy critique/pilot work, more focused questions guided my search for data. This chapter sets the context for the more analytical consideration of the data I generated in the two chapters that follow.

ColloidCo

ColloidCo are based in a School of Engineering that is part of a large research-led UK university. Physically, the School occupies a prominent position near the front of the campus. It has a striking architectural front built in the 1950s of Portland stone with art deco reliefs. Behind the façade however one finds the conventional (functional) lecture halls, labs, meeting rooms and corridors; and one could be in any university of the period. ColloidCo are located in such rooms, on a corridor housing a number of particle engineering research groups. Opposite is a Seminar room, and there is the regular footfall of students, researchers and administrators going about university life.

The science at the heart of group's research interests is what I would recognise (as an erstwhile natural scientist) as colloidal chemistry. However, dealing with the precision engineering of material systems at the nano-scale, their science is positioned within a cross-disciplinary nanotechnology research institute that operates at the University. In scientific terms they express their research interests as involving: surfactant and polymer adsorption; co-polymer capsules; inter-particle forces; dispersion rheology and slurry transport.

ColloidCo has a conventional university research group structure (cf. Lynch, 1985). The group has been developed by, and is identified with, a single professor. His leadership sets not only the scientific direction of the group, but also dominates the collaborations in which the group participates. He takes the lead in securing funding; both from research councils and from industry. He claims, in an early interview with me, that his success in the latter has placed him previously within the top 5 researchers in the university at securing industry funding. Even on first meeting his interests in pursuing an economic value from his group's research is apparent in his conversation. In addition to industrial collaborations he has also been instrumental in creating two new companies from his research. The nature of his leadership and its implications to the questions considered in this thesis are taken up in subsequent chapters.

The group is organized along two main threads of activity; shown schematically in Figure 8.

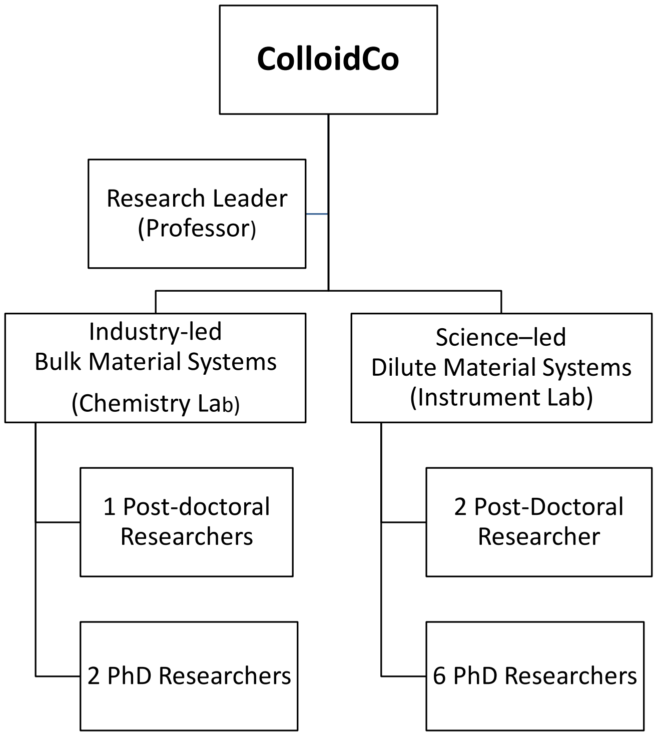

Figure 8 – Organisational structure of ColloidCo

The largest thread of activity concern the long-standing interest in colloidal science: it is the thread labeled “Science-led” in Figure 8. In the manner of such science the researchers worked on small quantities of material held in dilute solutions or suspensions. The research involved studying the physical properties of such materials using specialist scientific instrumentation (example shown in Figure 9). These instruments were located in a “dry lab” and the work of research involved preparing samples, placing them within the instruments and the expert setting up of the instrument in order to collect data (cf. Collins, 2001).

Figure 9 – Atomic Force Microscope (AFM) in the 'Dry Labs' at ColloidCo

The second thread of activity had emerged out of the first and was associated with a long-term collaboration with an industrial company working in the nuclear industry. The collaboration was organised around a particular challenge within nuclear fuel reprocessing. In this sense this aspect of the groups work was highly practical. It was intended to solve an intractable industrial scale problem; but one which required an understanding of the basic science of the materials involved. The researchers handled materials in larger quantities, with this work taking place in a conventional chemistry "wet lab".

The researchers themselves were a conventional mix of experiences. Post-doctoral researchers took some supervisor responsibilities for the PhD students. However, in the absence of the research leader on a day-to-day basis, ideas were shared and advice offered in a manner that bore no influence of a formal organizational hierarchy, but rather was a function of experience and scientific know-how. All such interactions are familiar from the many ethnographies that have taken place within such settings (e.g. Lynch, 1985). My own observations of this day to day work of the lab informed a background understanding of their scientific interests and commitments. However, my research design adopted the practice of trying to 'follow the scientists' (cf. Latour, 1987), and so whilst speaking with all of the researchers, I concentrated on the work of two scientists in particular. It was with these scientists that I adopted the *midstream modulation* interview protocol (discussed in the "Data Generation" section of Chapter 6).

The first of these researchers was a mature experienced postdoctoral fellow called Bob[4]. Easy-going with a wide variety of artistic interests, he was widely curious and reflective. Although having a rather idiosyncratic presentation style and being the butt of many group jokes he was respected as an independent researcher. In such circumstances building trust, credibility and a common language was easy. Practically he was always willing to make himself available for conversations. The second researcher was a PhD student nearing the end of his first year. Andy was the least experienced of the whole group, and he rather lacked in confidence as he was going through that phase of the PhD cycle recognizable to many students; wondering if anything is going to come of our research. Both of these scientists were in that part of the group labeled "Science-led" in Figure 8 as it was this dimension of the group's work that could be described as nanotechnology.

Speaking with all members of the group, it was noticeable how in introducing their research they tended to lead with narratives of the potential application of that research. In his very first words to me Andy explained: "*The purpose of my work is to create responsive capsules that will be used as targeted drug delivery systems".* Pete (another PhD student) was collaborating with a cosmetics company and in his project he was *"trying to use stabilised emulsion droplets*

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[4] All names in this thesis are changed in keeping with the anonymity I agreed to provide my research participants as part of my Research Ethics commitments

as particles, to see if you can get those particles to respond to pH and temperature, and see if we can release the perfume when we want to". I had the sense that such explanations were not in deference to my lack of understanding of natural science as they were all aware of my having a PhD in chemistry. Rather this was a way in which they were becoming accustomed to speaking about their research: it was evident in the PowerPoint slides I saw them produce and the papers they wrote. In a paper he was writing during my time with the group, Bob opened with a statement of a practical (commercial) challenge: *"In applications such as inkjet printing an ability to control the uniformity and structure of pigment particle deposits after the liquid has evaporated is of vital importance"*. In more visual forms of expression Andy used his skills in graphic design (he actually had his own small business) to produce a poster for a Departmental event for PhD students (Figure 10). While he felt that the comic-style presentation did not meet with the approval of the academics in the School, his efforts may still be interpreted as efforts to develop his own narrative for introducing his research.

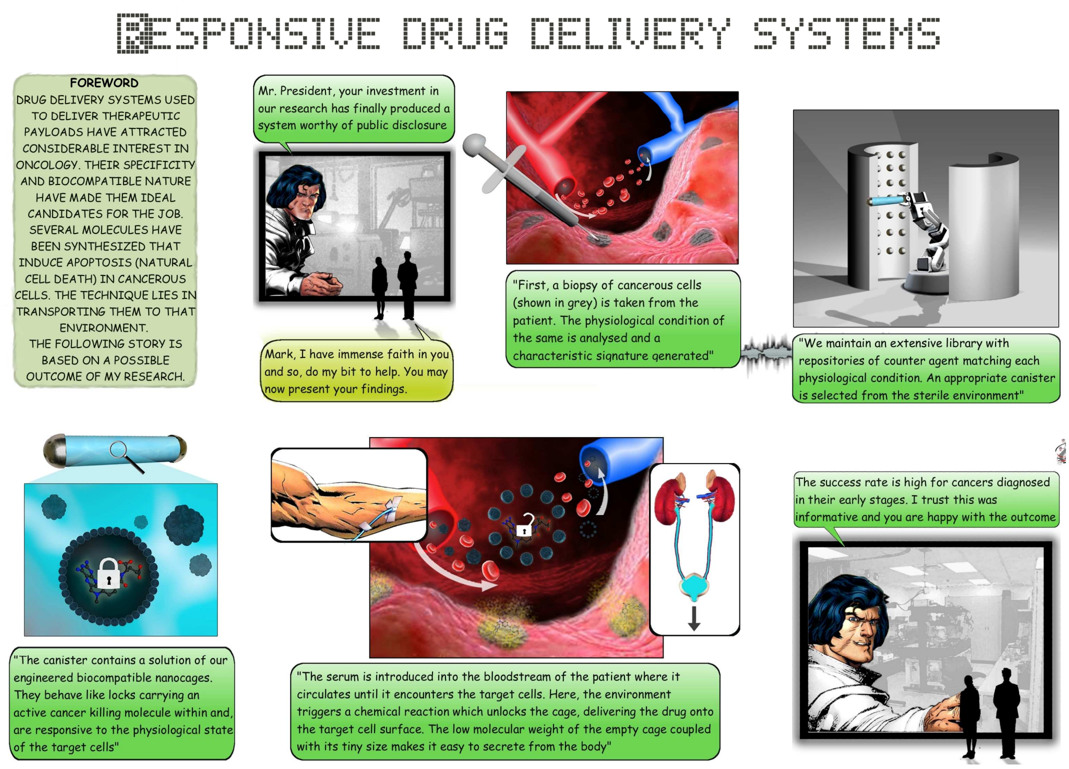

Figure 10 – Poster representation by Andy (PhD student) of his project's objectives

Dave, the senior researcher in the Industry-led section of the group, was highly conscious of the need to contribute to solving an important and intractable problem: *“I was asked to look from an overall perspective what characterization properties are easy to get and why…Sampling [of materials] in this situation is very limited…so I had to focus on the properties that we might be able to actually get [rather than like to have]. And so from that I worked out that technique we could use*”. Bob (notwithstanding the earlier quote from his paper) was less welcoming of this trend for industry relevance whilst acknowledging that it was necessary in order to fund university research: *“As you will know it used to be that EPSRC [Engineering and Physical Sciences Research Council] would fund the most things. And the involvement of companies was miniscule therefore the range of projects could be seen as being purely academic. But the range of projects was very broad, you had things that were very easily applied into an industrial setting but you also had a lot of blue sky stuff and lots of stuff in between. EPSRC's budget cuts are squeezed and I think the government are gradually trying to shift the spread of money from the public sector through to much much more, I guess I’d go to at least 50% towards the industrial funding”.*

The PhD students display a similarly varied response. Jim’s PhD was sponsored by a large multinational company with whom he appeared to have an ambivalent relationship: *“I don’t hear anything from them unless I contact them, and more often I have to contact them three times in a row to get anything from them”*. By contrast Andy, who received no industrial sponsorship (and in sense had the most freedom in the course of his science), was highly motivated to achieve a commercial impact from his research. Even though he had not yet achieved any experimental results of note his poster (Figure 10) and conversations always touched upon what he thought “industry” would expect of his research: *“[Having optimised the experimental technique] then we will start thinking about what can we build with these things. And when you try to market a product to industry they will ask you: exactly how long will it take to develop? How much material do you need? Can you use less material? We have to explore every critical factor for the coverage of polymer below which the system won’t work and also how stable is it? You have to* *answer all these questions"*. All these attitudes had been formed or highly influenced by the research leader, but within this attitude there was a clear sense that their research was not simply industrial research: *"While the group has a lot of links with industry the group is not beholden to have to make commercially successful products per se. [Research Leader] always insists that our role is not to make things that you can ever sell; but to show how things could be done and to let others take it from there"* [Jim, PhD Researcher]. The Research Leader's philosophy is considered in more detail in chapter 11 where the interviews with him are incorporated into a more formal analysis of the case data.

The various research projects are summarised in Figure 11 using Stokes categories of science work (Stokes, 1996). The diagram was produced by both questioning the researchers on the objectives and conduct of individual projects as well as examining (where possible) original project proposals. My interpretation of these data as expressed by my placement of projects amongst Stokes' four quadrants was checked with the research leader. The awareness of the potential commercial impact of basic research evident in the conversations with the researchers is borne out by the clustering of projects toward "Pasteur's Quadrant" (Stokes, 1996). As the research leader confirmed: *"We always worked well with industry, without compromising the ability to do good-quality science. We have always managed to publish in good journals, but we have also managed to generate excellent interactions and contacts with people from real world, with real problems".* Dave provided me with a good illustration of this philosophy when he explained: *"The [industry] guys always say 'well you used a model system and that's not 100% the type of particles that we've got in these plants', so what [knowledge] actually transfers? That's a tricky matter because it's a complex system. They have managed to work out the right sort of simulants that have the right sort of mixture of particles. The way we approach it is to say that the basic connectivity between particle charge and bed stresses are the same, so we've actually just written an [industry] conference paper detailing how the basic lab techniques can be used to predict performance"*.

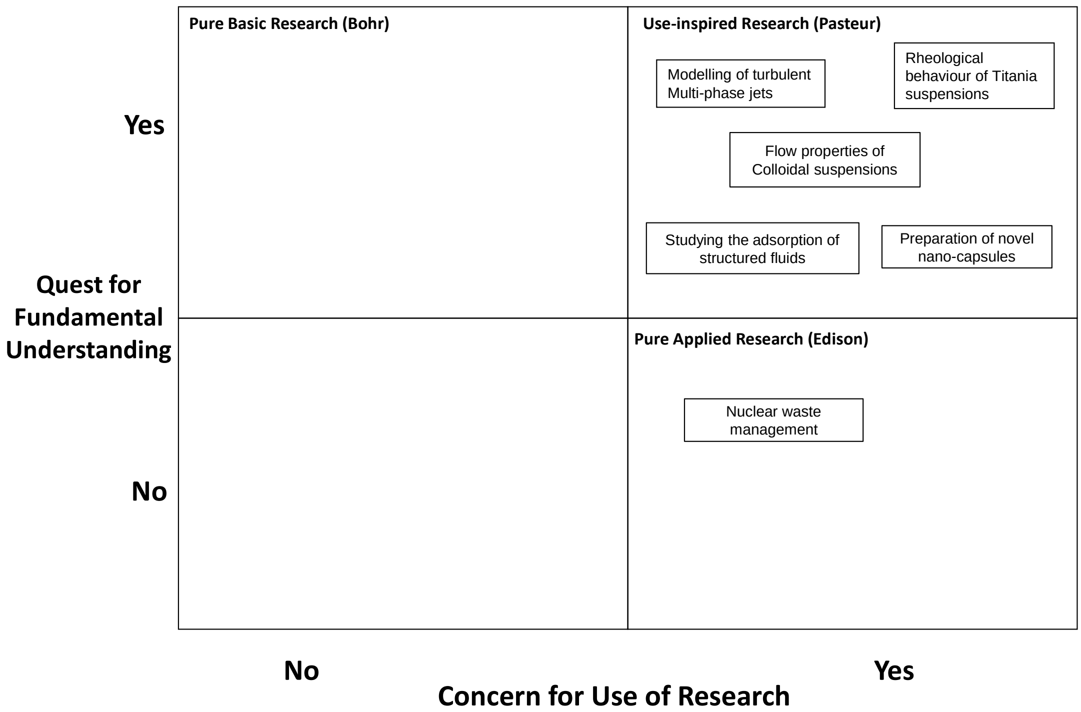

Figure 11 – Project portfolio at ColloidCo

ColloidCo has a significant track record of pursuing research that relates to industrial (and not just academic) needs. This is manifest not only in their collaborations with industry mentioned already, but also in their spinning-out of new enterprises. The first of these companies had been in existence (at the time of my time with the group) for 5 years and was in receipt of first round investment from a venture capital company. The second company was relatively new, having only been started in the previous year by a PhD student who had graduated at that time. The established company had its premises at a business incubator located on the same university campus as ColloidCo. There was regular interaction between the scientists of ColloidCo and those working in the spin-out company. These scientists had different expertise and would regularly do scientific favours (e.g. preparing some materials or running analytical tests) for each other. The two groups also met socially for a lunch date every two months. From the company's perspective then not only had ColloidCo provided the founding intellectual property but their on-going research was viewed as a potential intellectual property pipeline. The ColloidCo Research Leader played an active role within the company speaking with customers and participating in technical meetings. In describing this work with the spinout company its CEO suggested of the research leader of ColloidCo that *“of all the academics I have worked with in these roles [Venture capitalist working with Universities] then the [ColloidCo Research Leader] is the most commercially astute. He is very creative commercially. In a different life I could see him being a Sales Director”*.

ColloidCo had regular (fortnightly) research meetings to which everyone was expected to bring something (usually experimental data and its analysis) that the research leader had not seen before. Critique and discussion of the data tended to follow a hierarchy of experience with the research leader starting, followed by the post-docs and finally some (although never all) of the PhD researchers. This process was a highly recognizable from other accounts of scientific group meetings (e.g. Lynch, 1985) and fundamentally operated as a first level of peer review; with both the giving of feedback and the response to critique being fundamental for their development as researchers. In discussion with me later on the format of group meetings, the research leader explained that he consciously sought to offer *“leadership as distinct from management”*. This means that within an agreed problem space he allows the researchers a great deal of freedom to pursue their own ideas. He described this as giving them the *“freedom to nearly fail”*, by which he means keen a light check on them so that they don’t drift too far off track to the detriment of their personal aspirations and those of project partners. Increasingly this check took place during the group meetings as other institutional responsibilities (he was promoted to the level of Pro-Dean of the School of Engineering just before my time with the group) meant he was not available for more informal interactions.

I contributed to the group meetings by offering my immediate interpretations/reflections derived from my work with the group. This was partially successful as invariably only the leader or one of the senior post-docs would respond. The response of the research leader often saw him expounding on his general orientation towards research and innovation. He seemed to welcome to opportunity to do this in front of the group as he acknowledged that normally there was little natural opportunity for such discussions. In this manner he used the opportunity to explain: *“there is no longer such a thing as ‘gentlemen scientists’: everything we do must be mindful of economic considerations as you need to have funds to do research”*; *“you might knowingly do something that you know will never be scaled up and scaled out just to proof a concept; because that could frame iterative thinking to try to find an alternative which is cleaner or less harmful”; “I think you are probably missing a word: it is the responsible use of innovation [that we should consider] rather than responsible innovation, as innovation is innovation right”*.

Outside of group meetings I was struck by how little interaction I observed between group members themselves (compared the research labs in which I have worked). When not working at the bench or at an instrument, the researchers are sat looking at their computer screens, often they are also plugged into their iPods, and thereby disengaged from their peers. In such circumstances setting a time to have a conversation felt rather formal. I had expected to make use of group coffee times, for more informal chats, but such coffee times did not exist. It was easier to catch informal chats when they were in the laboratories; when sat at their PCs which commanded their undivided attention and it was harder to grab a chance to talk. In general each researcher also had a laptop computer which meant that there are a number of places at which they can do the work of data analysis, reading and writing. For some the lab is the place they come to in order to collect data, and because the limiting resource is time on the instruments, it is often the case that researchers work at night times and over the weekends. Such practices meant that laboratory life felt subdued. My 25-year old memories are of more active intellectual discussion. The latter might often have been wrong, ill-informed or lacking sophistication, but we used to argue about science and its importance. I remain curious about the pacifying effects of the computer screens (and iPods) that sit on everyone’s desk and compete (successfully) for attention. In the context of the current study, I observed no informal discussions of matters relating to the Research Impact agenda.

When prompted by me it was clear that there was no specific policy mandates for societal or economic influence/impact of which the PhD researchers are conscious in their day-to-day practices. My time in the laboratory coincided with the launch of UK Research Councils' "Impact Agenda" (launched fully in May 2009). As explained in Chapter 8 this policy required scientists seeking funding to articulate the potential beneficiaries (both academic and social/economic) of the research and explain what the scientists proposed to do in order to connect these beneficiaries with the practice and fruits of the research. The *Research Impact* agenda aims to get researchers "out of their" labs, taking responsibility (in the widest) for innovation. In other words doing the best science of which they are capable, might be viewed as a necessary but not sufficient role in their position as publicly-funded intellectuals. As we started to explore what these new policy mandates might mean for their work their first recourse was to a notion of a division of innovation labour was in which scientists simply do science which others then have the responsibility to develop. However, there was also evidence of a realisation that things were changing. Quoting Jim (A PhD student) above I noted his awareness of that his *"role is not to make things that you can ever sell"*, but as a result of a University initiative for young scientists to engage with the public about their research he felt: *"that It is not enough to merely explain science to people it has to show how the science you are doing is relevant to health and well-being…and this is a big pressure on science as a whole"*.

By virtue of having been closely involved in the writing of grant proposals, the post-doctoral researchers had had the opportunity to reflect on the implications of these new mandates for their careers. Jack, the senior post-doctoral researcher in the group, had the previous year won an "Enterprise Fellowship" from his regional economic development agency. This scheme was designed to provide young scientists with the skills and confidence to start their own businesses. Whilst he did not actually aspire to create his own business, he judged the innovation training and mentoring offered on the scheme as vital for his career development. He had started to apply for lectureships and fellowships to allow him to form his own group. In these applications he was positioning his own research as relevant to industry: *"I tried to focus on what I intend to do has great relevance for industry and these departments that have lots of links to specific industries. I think you have to put this in from the start, and the [Research Council] will ask for it any way"*. However, it was also clear that not all the post-doctoral researchers welcomed the emerging imperatives to commercialise science. In this regard the attitudes espoused by Bob are discussed in Chapter 10.

All researchers were originally unaware of the policy debates surrounding the idea of *Responsible Innovation*. In our conversations about these policies the researchers tended to see key decisions on the innovation (as distinct from academic) fate of the research as the responsibility of their industrial collaborators (going back to that traditional division of innovation labour). There was a belief (that was untested) that companies would be cognizant of their ethical, legal and safety responsibilities and would act accordingly. Jack commented *"we wouldn't be aware of small issues [in the industrial collaborator's ethical performance]. Obviously, if something was in the news then we would be aware of it. But in general this is not something we would discuss with the company"*. The researchers were highly conscious of local health and safety regulations with which they had to comply, but doing so did not in any sense inform their innovations. Nor was there any clear sense of a wider public's expectations of their responsibilities: Jack again *"Ethical issues in terms of the public are not so much present to us. There are basic things, such as we wouldn't try to use carcinogenic substances to prepare capsules which would be eventually injected into humans"*. To the extent that researchers did think about such issues, Jack continued to say *"I assume Research Council calls for proposal are informed by what the public wants…it is probably a complicated chain [of consultation]"*.

MicrosCo

MicrosCo is located on a University Science Park, some distance from its associated departments. Its near neighbours are companies that operate independently of the university, and serve customers either in the private sector or the public sector. The building in which it was housed was one of a number of identical plain one storey constructions that lined two sides of a large square car park. A single two storey building occupied the third side. The whole had the conventional look of an early twenty-first century business park that could have been anywhere in the UK. On the approach to the science park I would cross the path of undergraduates walking in the opposite direction from the halls of residence to lectures. To walk towards the MicrosCo facility felt like going to work in a regular business, rather than being part of some grander intellectual pursuit that one might associate with University life.

MicrosCo occupied this distant location, on specially constructed foundations of 16 metres of concrete, because it housed a state-of-the-art electron microscope whose performance benefitted from peace and quiet. With this instrument scientists could study nanoscale phenomena, with a particular expertise in dynamic phenomena: materials undergoing change. The difficulties of focussing the instrument to capture images of moving nano-scopic particles placed demands for a facility engineered to minimise vibrations. In addition to this large microscope the facility also housed a range of other, smaller microscopes that had recently being relocated from other parts of the university to create a single centre of expertise in high-resolution microscopy.

MicrosCo was created by a top-down intervention from the university's Vice Chancellor who himself had been prompted by the availability of large Government funding for research infrastructure: *"I wanted to have a commitment within the range of things that we did to a few big exciting, many-group, multi-lateral areas of science where we could really make a difference and be at the leading edge"* [University Vice-chancellor]. It was subsequently developed through a classic triple helix partnership (Etzkowitz, 2008) of the University a private company manufacturer of electron microscopes and a regional economic development agency. The latter had created a "Nanotechnology Programme" believing that it would benefit the sustainable growth of small and medium sized technology businesses in their region. In an analogous move to the University itself this strategy led them to invest in showpiece facilities recognizing that *'if there is so much good work going on in the universities in the region, we have to make sure it is all connected up [with businesses] properly"* [quotation from Regional Development Agency Manager]. The motivation of the private company was more pragmatic and (in their terms) conventional. An established marketing strategy of the company was to sell, on reduced terms, a new electron microscope to a university in exchange for being able to show other potential customers a working instrument. However, one distinctly new feature of this enterprise was that it was being created from scratch, and not at a University with a major tradition in this science. This presented a critical challenge as the Project Manager at the private company partner noted: *"We were empowered by my managing director and the vice chancellor to basically get on with it, and that gave us the confidence to say that ok if the vice chancellor wants to do it we'll go ahead…Without the* [Regional Development Agency] *input we wouldn't be able to progress in the way that we did…I guess the key element was that the people at the ground level that were going to operate the instrument weren't actually in place at the time: we only had this medium level which were myself and Professor G at York. That meant that we had to get the right staff in".*

Two notable electron microscopists (a husband and wife team) were recruited to lead the new facility. Each had been professors at a leading UK research-led university before being recruited by one of the world's largest science based companies, which had seen them work for 20 years within large well-resourced corporate labs. Both had leadership roles within the wider electron microscopy community, and their status allowed the facility to attract Nobel Laureates speak at its seminars. They were a showcase appointment to match the showcase facilities. As the Project Manager at the private company partner reflected to me: *"This [is] an amazing centre in the sense that they started from scratch… they didn't have the staff at start-up… and have now got equipment and staff that now puts them up with the highest level of research at the UK".*

The organizational structure of MicrosCo (Figure 12) owed more to that of a business than a conventional University research group. A leadership board had responsibility for the financial performance of the centre, and the Heads of the three scientific departments that had a stake (both financial and academic) joined the two Directors in constituting the board. However, the overarching academic strategy of the facility was largely determined by the two Directors: a situation I was to find was not without its disagreements with the wider Board. Of the two Directors then one (Director B) was one of the world's leading electron microscopists with an active research agenda; the second (Director A), although still participating in the research, led the organizational development of the facility. The operations team reported into this Director. During my time at MicrosCo there was an Operations Manager who oversaw the maintenance of all the instruments and coordinated access to the instruments from scientists across the Campus.

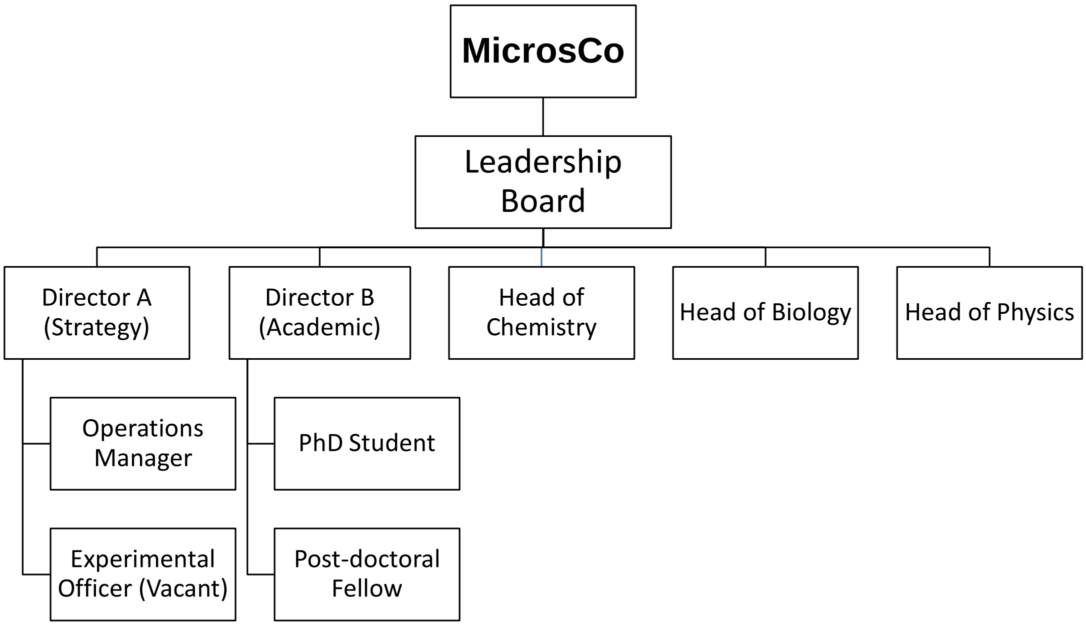

Figure 12 – Organisational structure of MicrosCo

The partnership with the regional economic development agency had created the role of “Experimental Officer” in order to co-ordinate access from regionally-based companies to the facility. This position was vacant during my time with MicrosCo, in part because the role (or as it seemed to me, the regionally-based companies who were to be the objects of this work) were not well defined. Details of the role were being written by a manager in the University's Technology Transfer Office (TTO) working with Director A. The TTO Manager explained *"I envisage that this person will be doing a mixture of: internal management to ensure that the requests for work [from regional SMEs] are carried out; quality control; brokering new relationships; and a certain amount of PR. Most of the connection with the outside world will be with this person and me".* This work was to be crucial in achieving the ambition of MicrosCo becoming "self-funded", but as the TTO Manager suggested *"[MicrosCo was] set up as outward facing but has been slow in getting going"*. This Experimental Officer initiative was funded in part with an EU grant obtained by the Regional Development Agency, but it was not simply one of providing a scientific service. In addition it had a networking dimension that was central to the business model of MicrosCo: *"We have a target for the number of SMEs that we must assist, but part of the spirit of [the RDA Programme] is to engage the local business community in the technology"* [TTO Manager]. These external networks might be viewed as an extension of the internal (to the University) research community that MicrosCo were developing. Thus, in addition to their formal governance structure MicrosCo was the focal point for a university network of associates for whom it provided the scientific instruments necessary to their research. This network would get together on the first Friday of each month for a lunchtime event comprising scientific presentations and a networking lunch.

The leadership board met on a quarterly basis and whilst there was a meeting during my time with the group, I was not invited to observe. In individual interviews with the board members I learnt about the issues on which there was disagreement. The facility is running at a deficit which was being underwritten by the three sponsoring departments. However, the real discord concerned the scientific strategy being pursued by the two Directors. This strategy involved modifying the new instrument in order to perform *in situ* experiments concerning dynamic phenomena: studying nanoscopic systems under conditions in which they were undergoing chemical and physical change. As I spoke with more of MicrosCo Associates I would frequently hear the refrain that the enterprise's *"unique selling point was to be a capability of performing in situ experiments"* [TTO Manager]. However the modifications to the large electron microscope that this required were not universally supported in private. Speaking at a later date (when his scientific strategy had been successfully implemented) Director A explained to me "*They were worried that we had invested £7M in this new microscope that I was then going to drill holes in the side*". He felt confident in *"cannibalising a state-of-the-art"* microscope in this manner as he claimed to have been instrumental in establishing that art in the first place. Possibly as a result of having worked in a US corporation for 20 years, his conversations were often managerialist in tone. He justified his strategy of modifying the electron microscope to create a new technology capability in these terms: *"if anyone is going to disrupt the innovations that we have created in the past then let's make sure it is us"*. These issues relating to the technical development strategy for the facility touch only obliquely on the research questions I was exploring, and I mention then only to help set the context. Here was an organisation that presided over a scientific facility that included a state-of-the-art electron microscope. However the Directors were not content to simply work with an "off-the shelf" (cf. "state-of-the art") instrument, but rather sought to innovate its technical capabilities and thereby provide a scientific niche for MicrosCo.

As with the ColloidCo case study my aim was to follow the work of two scientists. Because the facility had only been operational for 1 year it did not have many researchers directly based within it. A post-doctoral fellow had just returned to Japan before I started, leaving a single PhD student who was supervised by the two Directors. Joe was a 1st year PhD student, but one far more confident of his (undoubted) abilities than his contemporary in ColloidCo. He identified himself as a physicist and was comfortable to reflect on his research and the factors that animated it. We used the *midstream modulation* protocol as a basis for our conversations as regularly as possible (see Chapter 6, "Data Generation" section). I found that the protocol's effectiveness was constrained by the inherent experimental timescale of his science. His research was built upon the large electron microscope, and experiments could take a long time to both set up and execute. These limitations meant that a time period of a couple of weeks could easily pass between explaining a decision, and completing the research (and thereby being able to reflect) associated with that decision. Therefore my 12 week period with the group proved too short and our conversations never progressed beyond straightforward reflections on the immediate influences on the research. However, I always felt with this scientist that if my interaction had extended beyond 12 weeks, then the easy way he had when exploring ideas meant that we might of reach a more reflexive space, and examined a wider range of influences. In order to find a second scientist I had recourse to the wide network of researchers on campus that used the facility. With introductions from the Director, Steve agreed to work with me. He was another accomplished Post-doctoral researcher and one on a track towards full lectureship. Ambitious and focused, our conversations rarely strayed from the purely material dimensions of his research. Whilst being comfortable with the funding council's requirements for collaborating with industry he saw the main motivation of such collaborations as a means to access interesting materials that have useful properties or applications: *"We will have access to new materials which are interesting for energy applications, or biomedical applications. Some of these properties can be tested and all of these properties are a function of their size at the nano level"*. More action-oriented and less reflective by nature, in many ways with him I made least progress of all midstream modulation participants across both case studies.

Being sat in the MicrosCo offices during my mini-ethnography was like being sat in the university library; only quieter. Being distant from its associated departments there was no footfall of students. Researchers booked specific times to work on the various instruments in the facility, they came at those times, ran their experiments, collected their data and left. The instruments are available to use around the clock, and speaking to one researcher at 9 am one morning, it was clear that he has just finishing a 'night shift'. The facility has a dedicated operations manager who balances the demands from different research groups, undergraduate projects and external customers. However in coordinating access to the instruments the Operations Manager is mindful to ensure that there is sufficient capacity for collaborative work with regionally-based technology companies: *“anyone from outside who is a paying customer gets preference…it’s always been the case since the centre opened that outside paying customers would get to top of the list for machine usage”*. This agenda was of primary importance to the regional development agency’s investment in the facilities, and filling the available instrument time with university research would not be well received.

It was quickly apparent that lurking around MicrosCo waiting to speak with scientists not absorbed in their experiments would have proved of limited value in my study, and so I made arrangements to visit them in their own laboratories. Therefore, in addition to the various interviews and conversations I had within MicrosCo itself, I also conducted semi-structured interviews with a range of their collaborators. The list of interviewees is presented in Table 7 along with an outline of the distinctive perspective I gained.

Table 7 – Summary of Semi-structured interviews at MicrosCo

Interviewee	**Particular Perspective**
Heads of Department (Chemistry, Biology & Physics)	Strategic management of facility. The ways in which demand for societal and economic impact influenced their strategies
University Vice-Chancellor	The antecedents of the decision to create the facility, and the relationship between that strategic decision and impact agenda
Individual scientists	In Electronic Engineering, Physics, Biology, Chemistry, and Art Heritage. How MicrosCo was helping them respond to calls for societal and economic impact. How they organised their collaboration with MicrosCo.
UK Project Manager of private sector founder of MicrosCo facility	The antecedents of their decision to invest in MicrosCo, and what they expected from the collaboration.
Project Manager at Regional Economic Development Agency	The antecedents of their decision to invest in MicrosCo, and what they expected from the collaboration.
University Technology Transfer Manager	Nature of their work in support of MicrosCo’s engagement with external stakeholders.

On the basis of these interviews I constructed (and agreed with Director A) the summary of the MicrosCo portfolio (Figure 13). In contrast to the corresponding portfolio of ColloidCo (Figure 11) a wider variety of project types (cf Stokes) is evident. Whilst there were 8 projects in the Pasteur quadrant, then there are also 5 projects in the quadrant for “pure basic research”. The presence of such projects is not a surprising finding given that the MicrosCo facility houses a state-of-the-art piece of scientific instrumentation. Of more interest for the issues that I was exploring are the emergence of a cluster of projects that could only be described as “pure applied research”. One of these projects (“Nanoparticle characterization”) was a direct result of the investment by the regional economic development agency in the project. A second project involved MicrosCo extending the agenda of engaging with small technology companies, and in this they were collaborating with a nanotechnology networking organization. Funding for both of these projects had been secured, and the implementation of the projects was at a planning stage during my time with the enterprise. A third project, and in many ways the most interesting of the all, was a collaboration with the stained glass workshop of the cathedral in the same city as MicrosCo. Through the intermediation of an art history researcher at the university, this project was the subject of a major collaborative research proposal to the Arts and Humanities Research Council (i.e. nothing to do with advancing the science of nanotechnology). This project was highly revealing of Director A’s philosophy of collaboration and community relevance: it merits a fuller description here.

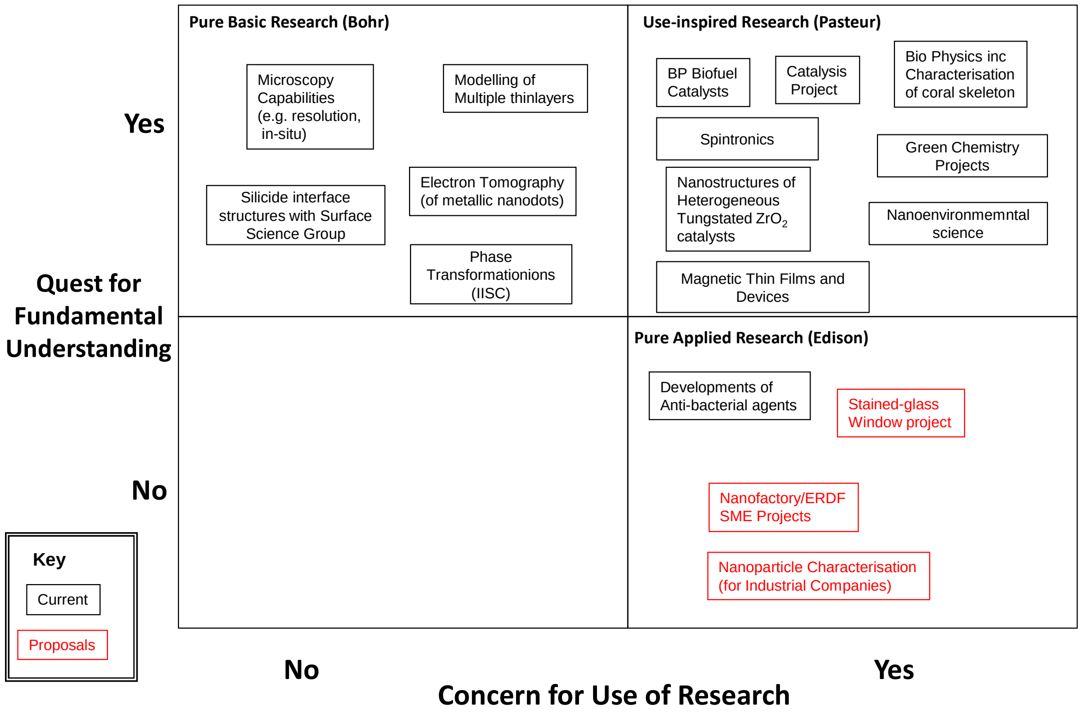

Figure 13 – Project portfolio at MicrosCo

MicrosCo's most unlikely research collaborator was an Art Heritage lecturer named Sue. She held positions both in the same university as MicrosCo, and also in the stained glass workshops of the host city's cathedral. This cathedral was a defining feature of the city having been at the heart of its economic, cultural and spiritual life for nearly 8 centuries. Conserving the glass in the cathedral's 128 windows was a significant undertaking in both historical and economic terms: the cathedral is central to the City's major tourist economy. However, conserving stained glass that has been the subject of 800 years of English weather and previous efforts at conservation represents a major technical challenge. Showing me around the cathedral's stained glass workshops she was able to point to different conservation challenges from windows from: the 19th Century (*"cataclysmic failures in paint is an all too common occurrence for glass of this period"); 14*th Century *("How can we reverse the browning of this glass"); and 13*th century *("how can we safely add a protective layer")*. In seeking answers to these very practical questions she had already collaborated with scientists capable of conducting chemical analyses on the glass and its paint. She wanted to extend this work in collaboration with MicrosCo in order to *"look in very great detail at what is happening at the interface* *between the glass surface and the application of glass paint. This will allow us to tie together earlier chemical analysis with how these conservation practices manifest themselves at the surface"*. Aside of the art historical benefits of this collaboration she also spoke of everyday economic considerations: *"We are in the process of convincing the trustees of the [cathedral] that this is what they should be doing. And so certainly it would help us a great deal if we could offer them a more coherent explanation of what generates this problem"*.

Despite working in departments only a few hundred yards apart Sue came to learn of MicrosCo through a collaborator at another university. She got in contact with the enterprise, explaining the practical issues she faced with stained glass, and *"they welcomed me with open arms. My understanding is that they are keen to work with historic material like this in [City] where stained glass is so much a part of the historic environment: they had already identified this as something that they would like to do"*. As well as the initial conservation challenges that had prompted her approach it was clear that other research opportunities had been identified for this collaboration. Such projects included scientific detective work that allowed them to confirm how particular images within windows which "looked odd" in artistic terms were in fact a composite of different segments of stained glass. In addition the possibility of developing a "glass fingerprinting" techniques opened up the possibility of further collaborations with stained glass workshops in Europe to trace economic trade routes for glass in the Middle Ages. It was clear in my own conversations with Director A that he judged there to be great value for MicrosCo in this collaboration. I also observed that in presentations about the scientific capabilities of MicrosCo he alluded to this collaboration with an opening rhetoric of *"nanotechnology has been a part of this city for 800 years"*.

Returning to the daily work of science and innovation at MicrosCo then spending time with Joe as he set up experiments on the large electron microscope, was very similar to being with Bob at ColloidCo as he worked his Atomic Force Microscope. All the talk was of the importance of sample preparation and *"finding an interesting bit of the* *surface"* from which to collect data. The intuitive way in which he quickly skimmed across the surface of his sample, producing images that to my eyes were only hazy blobs, spoke to the importance of tacit knowledge in making this instruments work (Collins, 2001). Reminding myself that mine was not a Science Studies project, we talked about the layout of the facility.

The large microscope was housed in its own shielded room visible from the control panel through a set of sliding double-glazed doors (Figure 14). Having carefully placed his sample within the microscope, Joe retreated to the control room from where he could adjust different parameters of the instrument and collect images (Figure 15).

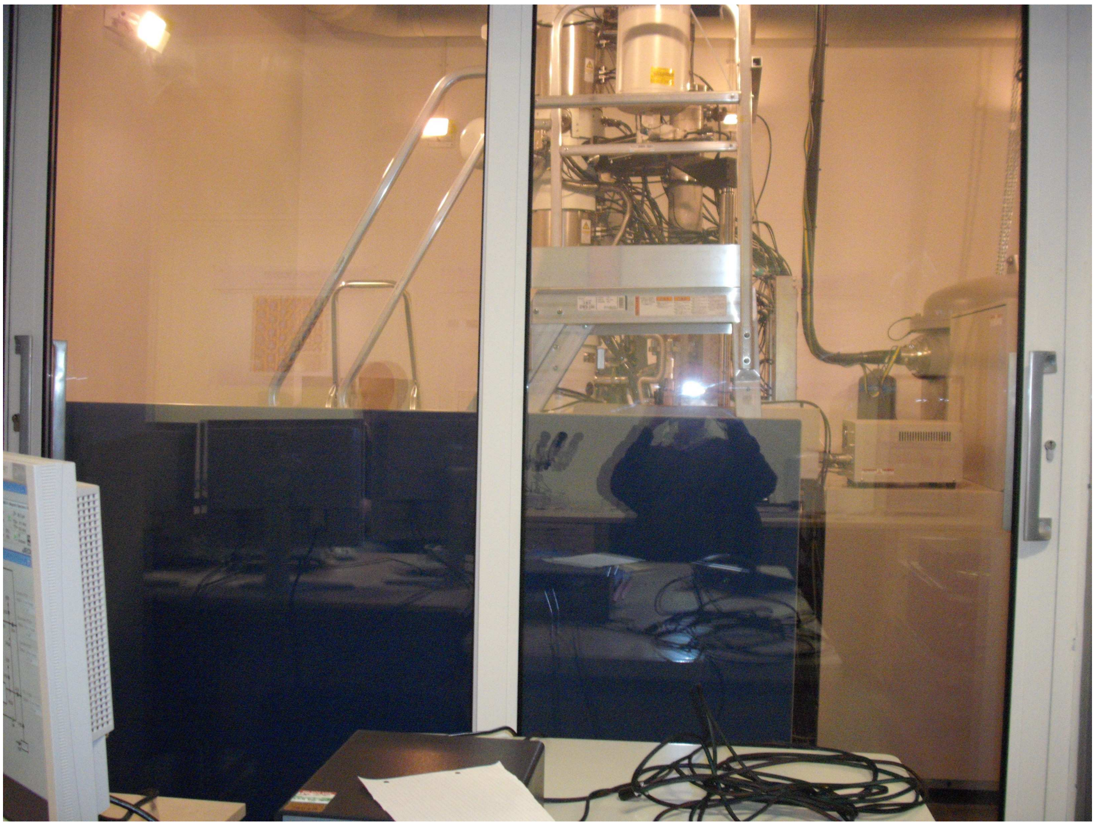

Figure 14 – Large electron microscope at MicrosCo

Figure 15 – Control room for large electron microscope at MicrosCo

There were posters on the wall showing some of the capabilities of the microscope along with some explanation of the basics of their aberration correction, along with nice photos of nano-structures. The science in these explanations was rather simplistic and certainly far below the level of Joe and the other researchers who were allowed to use the instrument. Joe explained that these posters are there to introduce the basics of electron microscopy for the many visitors to the facility. Indeed a photo in one corner shows the Directors with the Vice Chancellor and dignitaries from the founding company and regional development agency. Director A explained to me later that he redesigned the control room in order to make it more pleasing and conducive to receiving visitors: much to the annoyance of the engineers who then had to install the microscope. Joe commented that they are frequent visitations, including frequent instances of the Vice Chancellor showing off the facility to his visitors. Whilst it might be expected for the VC to show off the university's latest piece of scientific kit, it is also interesting that Joe himself has become involved in hosting visits from high school children as part of an initiative to encourage children to study Physics at university.

The policy mandate of which everyone was aware was the Research Impact agenda. Policies under the label of *Responsible Innovation* were never raised by the scientists themselves. My time at MicrosCo coincided with the first anniversary of the Research Council's launch of their *Research Impact* policy, and Hefce were running a pilot project to assess how Impact was to be assessed during the Research Excellence Framework; then planned for 2014. Indeed, one of the sponsoring departments of MicrosCo was taking an active part in the pilot assessment.

In many of the interviews with MicrosCo research associates the conversation turned towards "impact statements" that they had written or had in preparation. Art Historian Sue was planning on writing an impact statement for her grant proposal with MicrosCo at the time we spoke. She was aware that "*In terms of the economy, stained glass is never going to be a big contributor to the economy, but in [MicrosCo's City] as in no other city in Europe, it is more relevant…If we are able to say that we can come up with a rating system that says well this window scores highly because it is historically, aesthetically and even spiritually important, this one scores because it is actually made of materials that make it especially vulnerable to be exposed, then we have some kind of objective mechanism for mediating between these conflicting demands. And that is all about making good use of public money, and there is never enough of that*". Another MicrosCo collaborator, this time closer to their core scientific capabilities spoke to me about her own plans for submitting a REF Impact case study. In this she planned to draw upon her activities in trying to introduce physics to primary school children. In a collaborative proposal he was preparing with an Industrial company, post-doctoral researcher Steve had deferred to them in the writing of the Impact statement as he judged them primarily concerned with *"why and how it is going to help them"*. Given this widespread interest in the Impact agenda it was no surprise to me that it formed one of the recurrent topics of conversation for me and Director A. His thoughts in this matter are analysed more fully in Chapter 11.

Comparison of the two Case studies

In this chapter I have presented a descriptive account of the two case studies, interspersed with occasional interpretations on my part. I have used my research participants own words in order to express the challenges of organising for innovation in a changing landscape of mandates for societal and economic impact. In closing the chapter I will suggest a number of important themes that relate to how research enterprises working in emerging technologies organise for innovation. These themes either came to my attention during my researches in the two centres or became evident through comparison of the two cases: they are summarised in Table 8.

Table 8 – Thematic comparison of ColloidCo and MicrosCo case studies

Initial Case Themes	ColloidCo	MicrosCo
Immediate built environment	University Department	University Science Park
Operating model	Bounded organisation centred upon a single professor, and whose members self-identified with the group	Networked model comprising Associates from many groups. Governance structure spanning a number of different university departments.
Life-cycle position	Well established following conventional academic growth	New venture established by top-down Triple Helix collaboration
Strategic leadership	Single professor with track record for academic entrepreneurship.	Two Directors with long academic and industrial research pedigrees
Project portfolio (cf. Stokes 1996)	Use-inspired basic research. Includes collaborations with both industrial companies and other research groups	Pure basic, use-inspired basic, and pure applied research. Growing network of academic research associates, industrial companies, government agencies and civic institutions.

The physical locations of these two case studies were strikingly different. It was manifest to me each day as I simply walked to the site of my research, and it necessarily influences the way in which they approach innovation. Whilst there is a literature that explores the importance of place in scientific practice (e.g. Galison and Thompson, 1999, Henke and Gieryn, 2008), I do not wish to overstate the influence of the built environment on the innovation endeavours of these two cases. However, the operational model of the two enterprises was, of necessity, influenced by their location. ColloidCo has a conventional location for a university research group and its approach to industry collaborations and academic entrepreneurship were recognizable from accounts of such groups in the literature (cf. Chapter 2). There was a wider intellectual life in which all members of the group could easily participate. Interactions with researchers from other groups or those visiting the department were simply a matter of "popping their head around the door". In such circumstances regular scientific exchanges between researchers is easy and allows researchers (and therefore their research group) to build personal networks. Interestingly in this regard, there was no longer a common room within their department: a place where they had formally gone for coffee breaks. A recent refurbishment had created a modern looking café bar selling "*over-priced lattes*" [Bob]. In my time with the group, I observed none of its members (or those of other research groups) regularly using the facility. The haunt of undergraduates and staff holding visitor meetings, the change in coffee arrangement were not welcome and predicted locally to have an adverse impact on inter-group working.

MicrosCo, by virtue of being somewhat distant from other parts of the University had to work consciously in order to foster an intellectual life, and not be viewed as simply a service organisation or a building that housed a specialist scientific instrument. Forming and participating in innovation networks (cf. Chapter 2) was a more significant strategy as a consequence. They created a "First Friday Seminar Series" involving presentations from campus-based collaborators, and utilising the well-worn strategy of enticing people to attend people with a nice buffet. I attended a number of sessions and spoke at one, finding them lively affairs bringing together researchers from many different areas of science who might not otherwise meet. The potential for new collaborations was evident, but possibly their newness meant that I did not encounter any being formed during my time with the group.

Under the guidance of Director A the need for MicrosCo to network had acquired an exploratory (almost experimental) character. They were engaging in different collaborations not to some defined plan, but rather in the spirit of "*doing good things*" [Director A]. The enthusiasm with which the stained glass collaboration was pursued was noteworthy. The (nanotechnology) scientific value of the partnership was negligible for MicrosCo requiring, as it did, only a very routine use of its expertise. Similarly, the financial value was minimal. Rather, the enthusiasm can be read in terms of being able to fashion stories about what MicrosCo wanted to be. I interpreted their involvement with the stained glass project as allowing them to tell the story that they had 'arrived' and made a connection with the 800 year-old nanotechnology tradition in the city (colouration in stained glass, it is now known, comes from nano-particle forms of the pigments). Such stories are a type of *cultural resource* (Swidler, 1986) for organisations, and developing such resources, as they would financial or material resources, was one way in which I came to interpret the response of these research groups to mandates that their work have an impact on a wider society than that of their scientific peers.

The two research enterprises differed significantly in their maturity. ColloidCo had an established pedigree within their scientific community, which was evident in the roles its Research Leader had assumed within their scientific and academic community. MicrosCo, by contrast, was a start-up business. Their different life-cycle stages placed different demands on the work of leadership and these may be explained using the ideas of Helfat and Peteraf (2003) discussed in Chapter 3. ColloidCo had for some time been at the maturity phase of their life-cycle. Their innovation work developed from a conventional research group, and owed much to the attitudes and behaviours of their focal professor. Like most similar research organisations, this was his group. His development of the group can be described as entrepreneurial in the way that term has become associated with managers (Morris *et al.*, 2008): he identified opportunities for research and marshalled resources to ensure that they continued to be in a position to do "*interesting things*". For example, he spoke of how running an in-house science course in a large industrial company provided him with *"that file up there [pointing to file on his shelves] that contains a hundred ideas for projects that I might use with the company when the time is right"*. His long-standing interest in the science of real world problems was evident in the project stories I heard from the group's researchers. It meant that the emergence of the *Research Impact* and *Responsible Innovation* agenda was less of an exogenous shock to ColloidCo than many of the peers. Nevertheless I found evidence for "capability branching" in the form of a "redeployment" and "renewal" (Helfat and Peteraf, 2003, p.1006) in their capabilities for science-led innovation. This "redeployment" took the form of the groups researchers extending their familiar experience of industrial collaboration to pursue new experiences of economic development and public engagement. Such experiences subsequently informed a "renewal" of their innovation discourse within the group.

MicrosCo by contrast was at the "Founding & Development" stage of their life-cycle (Figure 3). Using their initial stock of "endowments" (Levinthal and Myatt, 1994), here including scientific expertise, personal networks and commercial experience born of 20 years in industry, the Directors at MicrosCo were forging a distinctive scientific capability (*in situ* electron microscopy experiments) as a foundation for all their other innovation endeavours. Achieving such a unique technology capability would place them at the leading edge of their scientific community, and attract funding and industrial collaborators. At this life-cycle stage policy mandates for societal relevance have less of the appearance of exogenous shocks and more simply seem the contemporary operating environment; and this was an environment to which MicrosCo were possibly more naturally suited given the circumstances of their creation.

Where ColloidCo's development had been ground-up by dint of the efforts of a single strategic leader, MicrosCo's was initiated in a top-down manner from a classic Triple Helix partnership. It was also possible to discern a transition from such a macro-level alignment of institutional (university, private company and regional development agency) agendas to a more conventional research group. The data I generated suggested three phases of development. The first phase involved the university Vice-Chancellor, a private company managing director and senior officers in the RDA (a photograph of the group still hung in the control room of the large microscope). Their discussions involved the exploration of three institutional strategies to find common ground and craft a new strategic narrative of partnership that aligned with the purposes of all three organisations. The second stage was delegated to senior managers within the organisations: a University professor; the UK Sales Director; and a RDA programme manager. This stage was concerned with securing the material resources: university real-estate and existing equipment; a new state-of-the-art electron microscope; and Government grants. The third stage was in progress during my time at MicrosCo and it revealed some of the practical tensions of a Triple Helix partnership. In this regard different threads of strategic action were evident, each aligned to the institutional agendas of the founding partners. However, under the leadership of the two MicrosCo Directors a research and innovation strategy was being developed that was highly distinctive. At this stage in its development it was possible to observe similarities to a conventional research group such as ColloidCo. True, the business model of MicrosCo was built upon a network of associates and had a broader governance structure than ColloidCo. And yet a distinct scientific identity was being created by the two Directors based upon a new technological capability for large electron microscope. This strategy, unproblematic in an organisational context such as that of ColloidCo, was creating evident tensions amongst the Triple Helix Partners.

As noted in my case narrative for MicrosCo, the implementation of the RDA's primary interest in the partnership had been slow to start. The recruitment of an "Experimental Officer" was seen as key to implementing the agenda of opening the work of MicrosCo up to regional technology companies. And yet whilst my time with the enterprise was some 18 months since the start of the RDA project that was to fund this position, a job description of the role was still being drafted. My observations and discussions with MicrosCo indicated that this part of the agenda was simply of lower priority to one of establishing a distinctive technological identity for the facility. However, during a visit to the enterprise two years later I learnt that two such Experimental Officers were then working in the company.

At first glance the strategic intent of the private company partner was being met: they had a new electron microscope housed in a well-resourced facility and were able to show its capabilities to potential customers. However, it also seemed to me that unresolved intellectual property issues lurked not far below the surface: who exactly owned the intellectual property associated with the new capabilities of the electron microscope (in prospect during my time with MicrosCo, but realised one year later)? These capabilities were born of the expertise and imagination of the two Directors, and yet their work was carried out on an instrument in which a private company retained a stake. Similarly the balance of intellectual property rights between academics and their host universities remains a perennial issue (e.g. Markman *et al.*, 2008a, Damsgaard and Thursby, 2013). The distributed governance and stakeholder relationships at the heart of MicrosCo's business model make these tensions more acute.

The three phases of the development of the MicrosCo enterprise outlined above have changing consequences for the objectives and strategies of the three founding partners. Triple helix accounts of organizational innovation have been criticised for paying insufficient attention to the manner in which institutional-level partnerships play out at the micro-level of researchers and the organisational-level of their research groups. In a study of how individual scientists negotiate the boundaries between academic and commercial worlds, Lam (2010) encountered a spectrum of role responses and questioned the claim that there has been a fundamental reorientation toward the *entrepreneurial university* (cf. Etzkowitz, 2001). Tuunainen, on the basis of a longitudinal case (2002, 2009) of a plant biotechnology group, criticised the Triple Helix model for not "pay[ing] close enough attention to the problems and contradictions that come into the world as university results are commercialised" (Tuunainen, 2002, p.36). In his case study difficulties were observed that involved intellectual property ownership, transferring research findings to commercial propositions, failing to create a hybrid community between academic and commercial stakeholders, and managing at the boundaries between the different partners. Whilst some of these observations are also evident in my own case study, the two Director leadership model can be read as an innovative strategy to address the difficulties of the split leadership role observed by Tuunainen (2009). The theme of strategic leadership is inevitably important, and addressed in a more thorough manner in Chapter 11.

The final theme of Table 8 was that of project portfolios which were also summarised in Figure 11 and Figure 13. ColloidCo's projects are clustered in the Use-inspired research category whilst MicrosCo's are spread across the three main quadrants. This difference may readily be seen as flowing from the other themes discussed in this section. ColloidCo's maturity means that the Research Leader knows the type of projects he wants them to conduct, and he has built strong relationships with industrial companies that have been sustained across different rounds of funding. The wider spread of project types at MicrosCo reflects both their Triple Helix origins and the explorations of an enterprise progress through is "Founding and Development" stage (cf Figure 3).

The accounts of the case studies in this chapter, and the comparisons between them, draw attention to important aspects of how research groups are responding to the challenge of societal and economic impact. One of the ideas that has animated this study has received some vindication, namely that the organisational-level dynamics of managing innovation in such groups, are underappreciated in policy enactments designed to promote such innovative work. The circumstances of MicrosCo's formation appear at first glance a model Triple Helix Partnership. The spin-out companies and industrial collaborations at ColloidCo are also exemplars of their class in many ways. However, these case studies reveal underappreciated issues in sustaining these initiatives. These challenges were evident both at the way in which individual researchers were making sense of the changes in the policy environment for science, and at the level of leaders pursuing strategies to develop and sustain their innovation aspirations. These issues are further explored in the next two chapters. Chapter 10 considers the possibility of bringing societal concerns for impact to bear at the level of lab decisions through a process of mutual inquiry involving a social scientist (me) and a natural scientist (Bob). Chapter 11 then adopts a more strategic perspective examining how organisational capabilities for science-led innovation are developing in the face of calls for societal and economic impact.

Chapter 10
Reflexivity in the Lab – a Critical Incident Analysis

This Chapter explores in detail the work and reflections of an individual researcher (encountered during the case research) in the face of mandates for his science work to be responsive towards societal and economic concerns. This work forms part of my case research that involved collaboration with the STIR programme led from Arizona State University (See Chapter 6, "Data Collection" section). The method employed in this part of the study was the *midstream modulation* protocol (Fisher et al., 2006) employed within the STIR project. This protocol is used to structure regular interviews with a natural scientist as part of a social researcher being embedded for a period of time with the natural science research group. It involves exploring the lab scale decisions being taken during the course of research, by iterating between four distinctive categories of conversational prompt: opportunities; considerations; alternatives; outcomes. These categories of prompt are defined by Fisher in the following manner (Fisher, 2007, p.158):

- *Opportunities* are "a perceived state of affairs characterizing the imminence of a decision". Such situations might not necessarily be understood as "opportunities" by the natural scientist, and might be thought of as problems needing to be solved or ideas that had occurred to them to try out. In conversation I found that saying something as simple as "*please tell me what you are working on at the moment*" would surface such opportunities.
- *Considerations* are "internal (cognitive) or external (social or physical) selection criteria that may operate as enablers or constraints, and that potentially influence or determine the response to the opportunity". Examples of internal considerations might be objectives or personal commitments. Examples of social criteria might be the purposes of the wider research enterprise, including the economic objectives of industrial collaborators. Finally, examples of physical considerations might be the material or resource constraints of the innovating enterprise, including their previous experimental

data. In practice these considerations could be identified by the social researcher using simple questions to pursue the different types of possible considerations: "*what factors have influenced your work at this point?*"

- *Alternatives* are "perceived options on courses of action for selection in response to the opportunity". More simply, this category is the ideas that occur to the natural scientist either on their own or in conversation about what they ought to do next. In practice, the interviews at this point would involve revisiting each consideration as a potential prompt for an alternative course of action.
- *Outcomes* are "the decision, understood as a particular response to the opportunity, through selecting one or more alternatives". The outcome itself might be subject to further elaboration and lead, in turn, to new opportunities being brought to light. The outcomes category provides the practical link between repeated interviews. At the time of the interview it might frequently be the case that the natural science researcher had not yet made his decision: the start of the next interview could then be "*what did you decide following our last conversation? And how did that work out? What new opportunities does this suggest?*"

Applying the protocol involved me engaging the natural science researcher in a conversation that iterates between the four categories. These interviews are linked in the sense of being conducted regularly. In this manner then a web of lab-scale decisions (articulated as opportunities-considerations-alternatives-outcomes) is constructed that make these decision more transparent to both the social researcher and the natural science researcher themselves. The action research modality operates in the very act of rendering these decisions more transparent, and thereby facilitating reflection of the interplay between the societal, the economic and the purely technical influences on the research/innovation practices. The selection of the two scientists to take part in this study, which followed a presentation of the study's aims at a research group meeting, was made by the group itself. My initial requirement was that the two volunteers were engaged in different projects, and that they had different levels of laboratory experience.

At ColloidCo this involved the experienced post-doctoral researcher Bob, and the 1st year PhD student Andy. Andy was the least experienced of all the researchers I encountered during the study and rather lacked confidence in his work. It was suggested (by the research leader) that the opportunity to reflect on his work by being a participant in my study would help his confidence: it did not. While we had very friendly relations he started missing our agreed appointments. Although not to avoid me (I think) he started keeping odd lab times, working late in the evening and at weekends rather than during the day. For him our conversations were perhaps surfacing his own lack of confidence and competence. The net result is that we only held a handful of midstream modulation conversations during all my time with the group.

At MicrosCo the first participant (Joe) also a 1st year PhD student, who was confident enough to explore the limits of his perspectives, and we used the midstream modulation protocol as regularly as possible. The tool's effectiveness was constrained by the inherent experimental timescale of his science. His research was built upon the use and extension of electron microscopy, and experiments could take a long time to both set up and execute. These limitations meant that we never move beyond simple reflections on his work: what Fisher calls *de facto* modulation (2006). There was little indication that these simple reflections were feeding back into the research or influencing the decisions taken during that research: what Fisher calls *reflexive* and *directed* modulation (2006). However, given more time, I am sure we would have reached a more reflexive space. The second participant at MicrosCo was another accomplished Post-doc (Steve) and one on a track towards full lectureship. Ambitious and focussed, our conversations rarely strayed from the purely material dimensions of his research, and with him I realised the least progress of all four participants. It was always suggested by Fisher that the midstream modulation approach was something of an experiment (Fisher and Guston, 2008), and he hoped during the course of his wider project to discern those contexts in which its use was most effective[5].

Of the four participants with whom I used the midstream modulation protocol to induce reflective observation on their innovation practice, the only one with whom I made significant progress was Bob the postdoctoral researcher at ColloidCo. What now follows is a detailed account of our conversations that owes something to the method of critical incident analysis of Flanagan (1954) in order to gain the fullest understanding of the issues surrounding a marked shift in the course of Bob's work; presented from the perspective of the researcher himself. This chapter comprises a narrative that outlines the main thrust of his collaborative innovation project with an industrial firm, and builds towards a decision point at which he was confronted with a choice of future directions for his work. The options at this point are suggested in terms of issues that had surfaced during the course of the earlier midstream modulation interviews. The choice of research direction and subsequent findings are described in the words of the researcher himself.

This decision point merits such detailed consideration because, as will be explained in the narrative, the researcher was faced with a degree of freedom over this decision that is unusual in collaborative work of this kind. Whilst not completely alone (he had the ear of his research mentor throughout), the influence of the industrial research partner was markedly relaxed at this stage in the project. This degree of exposure in this context, I suggest, makes for a heightened exemplar of the issues confronting researchers that have surfaced in the policy discourse of responsible innovation (Chapter 8). During the course of the four individual researcher interactions outlined above, this was the only opportunity at which there was the possibility of their reflections influencing, during the very conduct of the research, the

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[5] The research findings described in this chapter have contributed to a multiple author paper involving fellow participants in the "STIR project" and currently under review in *Nature Nanotechnology* (Fisher *et al.*, in review)

course of the innovation project. Whilst my very presence in the lab and my use of the midstream modulation tool were successful at making visible socio-ethical issues of the research which might normally remain unexpressed (Fisher *et al.*, 2006), there was only this single decision point affecting the course of the innovation project which could be read as having been informed by the researcher's critical reflections. The capacity to reflect in this manner should rightly be thought of as valuable of itself (Schuurbiers, 2011) but my motivation in exploring one decision point in detail is to suggest how through a collaborative form of inquiry between a natural scientist (Bob) and a social scientist (me), societal issues might be brought into reflexive focus during the everyday course of laboratory work. In this it also contributes to a growing list of tangible changes in the course of innovation projects facilitated by such collaborative inquiry (Fisher *et al.*, 2010).

The structure of the remainder of the chapter is as follows. Firstly, the researcher is introduced along with the particular innovation project in which he was a key partner. A summary is then offered of the range of opportunities explored during the interview series. Recurrent themes are identified and a picture created of how the researcher understood his role within a wider network of innovation. This account builds to a critical decision point in the project at which the researcher experienced an unusually high degree of freedom in his choice of the future course of the project. A number of readings are offered on the subsequent decision that range from the straightforwardly technical, to the rather more intriguing possibility that the decision was consistent with reflections on the project work, that had been built through repeated rounds of using the midstream modulation tool.

Introducing the Researcher and the Innovation Project

Bob is a postdoctoral researcher with many years of experience covering a variety of different research projects, both involving pure science and collaborative projects. Trained as a physicist, but now working in a chemical engineering lab, he felt his distinctive contribution was based on his experience with using specialist instrumentation in order to study the surface properties of materials. His project during the time of our lab engagement concerned the mechanism of interactions between nanoparticles and surfaces to which they are adsorbed. He generalised this as an interest in the relationship between bulk properties and interfacial phenomena, and what this might reveal for how we might understand macro-scale effects in terms of nano-scale mechanisms:

“I'm quite interested in adhesion because the study of adhesion in the mechanical properties at interfaces is to a certain extent poorly defined. There is a whole area of how do you define mechanical properties once you start to look at only a few tens of molecules by a few tens of molecules type of volume. Properties like hardness and toughness they don't really exist in their current definitions at that scale”.

Since joining ColloidCo his work had evolved in a new direction and he found himself often arguing for not using their specialist instruments as “black boxes”. Rather, he advocated that an appreciation of the science behind the instrumentation is necessary for a proper understanding of what they are capable of delivering:

“I must admit that from my Physical type of background I quite like the mechanical properties ideas. Since I have been in [ColloidCo] I think I've been addressing more kind of physical chemistry type of problems and the idea here is usually, well mostly, based around the control of adsorption and the understanding of it. My side of it has been largely can we understand what these instruments deliver, which is a slightly different take on what many of the other guys do; they use the instruments to generate data on their samples and the assumption is if they see a certain response from the instrument that means [emphasis in original tone of voice] something”.

Such a conscious awareness of the challenges of interpreting signals from these instruments was evident in discussions of data within research group meetings where we might expect such critical review. However, such scepticism over the meaning of data made for significant challenges of communication with the project’s industrial partner. His primary interests and motivation lay with a rigorous pursuit of basic science, and he expressed worries at the trend he sees in science policy towards greater funding of applied science projects:

“I see the way in which those funding bodies operate as having a core that needs to be maintained somehow and I hope the EPSRC [Engineering and Physical Sciences Research Council], or whatever they get called in the future, will maintain that. Engineering is an interesting one by definition. We are at a crossover, so how do you include some hard science and some applications within it? How do you share that money around? I think that's quite a difficult one to solve and there seems to be an interest in more short-term projects…which is interesting, but it's also a bit of a shame to anybody who's dedicated to pure science who sees that as part of the spark for why in the subject in the first place that's what keeps you going”.

He did not have natural sympathy for industrial research, in the sense of not wanting to do that kind of work himself. This viewpoint informed his understanding of the contribution of academic science in collaborative projects as eliciting underlying mechanisms. It is a position built upon natural curiosity and an interest in exploring unexpected avenues:

“ [we] are naturally curious and we say, well this has come our way, that's not what I expected, why is that? Let's investigate. It doesn't matter if I don't get as many industrial type data, answers to their problems as I did in the previous month, because maybe the intention is for the publication”.

In contrast he represents the industrial mind-set as being strongly instrumental:

“what you find is that presenting those results to industry is very difficult because they need to have something that they can understand and that is a fairly clear message, either yes it works, no it doesn't work, yes we see something, no we don't see something, that type of thing is what we are looking for and very often science, as you know, is different shades of grey”.

Such perspectives are not born of the current project but rather reflected a variety of experiences across a number of collaborative projects and research settings.

Bob's attitude to such research suggested he believed it is possible to strike a balance between academic and industrial interests. He believed that coping with this "*uncomfortable middle ground*" is influenced by the attitude of the researcher; there is still a high degree of academic freedom within such collaborations, and realisation of both industrial and academic goals is possible:

"You get some that only do what is required because they don't have an enquiring mind-set. But there are plenty of other people in the research establishment who are naturally curious, trained to an extent that they want to investigate that phenomenon that they see and as they go along investigating it, it invites other questions. So whilst keep one set of things in mind, trying to answer the industrial problems, they are also trying to find out what is happening down the other route as well. Perhaps the two can come together further down. It's there as a possibility. It depends on the character of the person who is doing the work".

Bob's work during the time of the STIR lab engagement concerned a collaborative research and innovation project between a large private company (HomecareCo), ColloidCo, another research group (ModelRes) within the same Engineering School as ColloidCo, and a third research group (ToxicRes), based at a different University. The project involved a new dishwashing product that incorporated nanoparticles. The technical responsibilities of the different research groups were clearly delineated. The role of ColloidCo was to uncover the basic cleaning mechanism of the new product, and in this work Bob was supported by a PhD student. The role of ModelRes was to undertake computer modelling of the cleaning action and a post-doctoral research fellow performed this function. The third research group ToxicRes completed toxicological tests relating to the new product. HomecareCo themselves undertook research using real-world conditions (e.g. using dishwashers). Whilst Bob interacted with the research manager at HomecareCo and knew his peer in ModelRes, the personnel and work of ToxicRes remained completed unknown to him.

The project was led and controlled HomecareCo; they set clear objectives and met with research partners on a regularly basis to review progress and to agree the future project direction. The Project was characterised by a clear division of labour, with each partner understanding the experiments and analysis they were to conduct. Project meetings involving ColloidCo and the company also involved ModelRes. However, throughout the work, researchers in ColloidCo received no communications about the nature and progress of the toxicological project being conducted at the other university. The relationship between ColloidCo and HomecareCo is characterised by a high degree of trust based upon research and teaching collaborations spanning a number of years. HomecareCo has a well-established reputation for operating in a highly ethical manner. The belief held by the ColloidCo researchers that they would be informed of any toxicological issues of concern, seems well founded.

Based upon previous industrial research involving dishwashing tests, the cleaning step was thought to occur during the wet stage of the wash cycle. For this reason the study of the mechanism of action of the nanoparticles was conducted in an aqueous environment. Surface studies used two specialist instruments in the ColloidCo labs: a Quartz Crystal Microbalance (QCM) and an Atomic Force Microscope (AFM). These instruments were used to investigate the nature of binding between a range of different surfaces and the nanoparticles present in the dishwasher tablets. The period of time of my lab engagement took place at an important stage in the course of the project. After two years of work amongst the project’s partners, Bob had the impression that HomecareCo were undertaking an important internal review that would result in decisions about the future commercial direction of the range of products associated with the project, and by implication, the project itself. A whole series of closely-spaced (relative to practice established earlier) project review meetings had been scheduled with ColloidCo and their colleagues in ModelRes. This meant that the experiments conducted during the

STIR engagement study were undertaken with the next review meeting never far away.

Midstream Modulation within ColloidCo

The midstream modulation protocol involves “four conceptually distinct but, in practice, highly iterative components – opportunity, considerations, alternatives, and outcome” - associated with research decisions (Fisher, 2007). In working through this protocol with Bob it was clear that the technical decisions he took on the next course of action within his project could take weeks to execute. This was a consequence of the time taken to set up his experiments (and allowing for securing time on key instruments), run them, and analyse the data produced. From discussions with other colleagues involved in other STIR lab engagement studies, it seems that the time period for working through the opportunity-considerations-alternatives-outcome cycle can vary with the nature of the science. Whilst some research settings may make for almost daily decisions, within the project considered here the timescales might stretch across several weeks. However, as in other STIR studies (Schuurbiers, 2011) the protocol may be used to surface other innovation related issues such as lab health and safety procedures, relationship management with project partners and intellectual property rights. This account offers examples of the opportunities discussed with Bob and the ideas that surfaced through their exploration. In this manner a number of important themes emerged that speak to shared (amongst the project partners) beliefs about how the work of innovation projects should be organized.

One of the first opportunities discussed with Bob concerned some samples that the industrial partner (HomecareCo) had asked him to investigate. These samples were glass surfaces that had been coated with dishwasher tablet nanoparticles and then “soiled” using real food; in this case spaghetti sauce. A strong sense of frustration came through Bob’s reflections on this work:

“ And as for the AFM well you really need close to ideal conditions to get the AFM to try to understand and that's what they don't seem to

realise. They see us as specialists in surface analysis who have the ability to get access to mechanisms. They're asking mechanistic questions based on real systems which are probably combinations of different mechanisms put together and acting on a particular type of soil, where we want to say under what conditions does this particular mechanism operate, compared with this other mechanism, and we want to isolate those mechanisms. The only way you can isolate them is to take things systematically. Is this a simple polymer? With a more complicated mix of polymers, this is one that is being prepared this way, with this the other way. This one with a certain thickness, this one is a different thickness. This one's been baked dry, this one's been kept wet. And I ask them basic questions like have you taken the spaghetti samples for example, and instead of letting it dry, just tried the same experiments as you have when wet for different amounts of time. To my surprise they hadn't".

In considering his contribution to such work Bob's view is grounded in his deep understanding of what the scientific instruments (in this case the AFM) are capable of delivering and this is something that the industrial partner "[doesn't] seem to realise". Where they want a statement of the mechanism of cleaning, Bob sees multiple mechanisms that require "isolating" through systematic inquiry. This, he acknowledged separately would be an interesting study given the time. In suggesting ways in which their complex system might be reducible to constituent parts, he is trying to find an accommodation with their desire for a simple explanation from the expert, with his scientist's desire to model complexity. His most straightforward course of action is to do as he is asked; this he did, and he had previously reported inconclusive, "messy" AFM data. However, in this quote, we can also read him trying to engage with them on their (experimental) terms by suggesting work more appropriately completed in their labs. His suggestions, in this instance were not taken up, and for his part, he was asked by the industrial partner to continue studying the adsorption of the nanoparticles under aqueous conditions. This work to mimic the wash phase of the dishwasher cycle was demonstrably (based on washing tests at the industrialist's labs) important in the cleaning process.

The next stage of the project found Bob studying the adsorption of the nanoparticles on different surfaces. Results had been rather inconclusive, but if anything, appeared to show little adsorption of the nanoparticles under aqueous conditions; something that did not fit with the understanding of the cleaning mechanism that was borne out of the industrial research. Bob and his PhD colleague had completed a lot of work using silica surfaces and a series of academically-interesting but complex results had been obtained. Again Bob felt that this could be another point of departure for an interesting academic study in its own right. However, the decision he took reflected obligations to the industrial partner.

"This project is a fair amount of money as far as [HomecareCo] is concerned. When I say a fair amount I mean the amount of money invested by the company in-house in products that are related to what we are looking, and so there is quite a lot hanging in some ways as to what we find. So if we find something that is interesting we need to be very confident that what we are finding is right. I mean we do that anyway as scientists from the point of view of reporting it to them. So we have this issue of trusting your data versus the time spec of needing to deliver quickly."

That decision was to put to one side the academically interesting work on silica surfaces and run some experiments on gold surfaces. Whilst gold is hardly representative of consumers' crockery and cutlery, his rationale was that gold would provide mechanistic clarity following a lengthy series of experiments that had proved conclusive. This consideration may be read as Bob trying to bring an academics contribution to a very practical problem.

At this time he had a sense that behind the scenes at the industrial company, the wider innovation project had acquired a degree of urgency. He was aware of the "fair amount of money invested by the company" and conscious of needing "something that is interesting" to report at the next (and imminent) project review. On the day before this interview (from the quote above), the PhD researcher on the project had obtained a very interesting result following some QCM investigations: unambiguous evidence of binding of the nanoparticles to a gold surface. Bob had decided to follow up this finding with some AFM experiments, and our midstream modulation interview took place as he prepared this experiment. Exploring this decision surfaced a number of other considerations that spoke to the practice of innovation within this project. The most important related to questions of "*trusting your data versus the time spec of needing to deliver quickly*". Practically, this meant validating the findings by conducting comparable experiments on other instruments and performed by another researcher; in this case AFM experiments performed by Bob. Previously such work had resulted in "*images that were not brilliant*" and so he was planning on using some sharper instrument tips this time.

The research prompted by this decision took over six weeks to work through, and in the end the initial "interesting" adsorption experiment proved to be a rogue result which couldn't be replicated on other gold crystal surfaces. Even though research decisions taken during this period could be understood in terms of clarifying and validating an initial experimental result, repeated use of the midstream modulation tool allowed us to surface socio-ethical issues for a range of other innovation practices (cf. Schuurbiers, 2011). Examples of such issues are presented in Table 9, which uses headings drawn from the four phases of the midstream modulation tool.

Table 9 – Summary of general innovation project issues explored with Bob using the midstream modulation protocol

Opportunity	Considerations	Alternatives	Outcome
Bob could contribute his emerging knowledge of the cleaning mechanism to inform the parallel toxicology project performed by ToxicRes	• There was an established clear division of labour within the project • There we no direct channels of communication between ColloidCo and ToxicRes • Bob had a full schedule of his own work and tight deadlines to meet • Material difference in toxicity of a chemical and the toxicity of a nano-variant of that chemical • Importance of understanding of cleaning mechanism and associated life-cycle of nanoparticles for the mechanism of toxic action • Complicating factor of the other ingredients in the dishwasher tablet formulation	• Contact ToxicRes group directly with his ideas • Raise these issues at new review meeting with industrial partner • Bob could learn more about the work at ToxicRes by looking up their publications. • Continue with own work, meeting deadlines set by industrial partner	• He continued with own work, and met deadlines set by industrial partner
Bob had written up a piece of the research for a special edition of an academic journal, and was intending to send it to the journal without first getting the paper cleared by the industrial partner	• Sending it to industrial partner may cause delays and make him miss journal deadlines • Bob feels sufficiently experienced with this partner and his own judgment of what would be acceptable to them • Bob not aware of the details of contractual obligations regarding agreement on publication in academic journals • It is important for Bob's academic career that he produces papers published in peer-review journals	• Just send paper to journal without any consultations or without seeking permissions • Check paper with research leader and send to journal without consulting industrial partner • Seek industrial partner's permission before sending it to paper	• Bob sent the paper to the industrial partner with details of the journal deadline. They reviewed it within a couple of days and returned it with no changes, and permission to publish
The production of a joint report with the industrial partner, in which their research and that of performed at ColloidCo with collated to produce a coherent account.	• Ensuring mutual understanding of technical language produced in two very different research settings • Bob felt a political need to demonstrate that much effort had been put in by both parties • Ultimate control of the editing of the report lay with research manager at HomecareCo • A desire to give due credit to the work done by the PhD student during the project at both the ColloidCo and industrial labs	• Produce an academically rigorous account of his own work that works in its own terms • Produce an account of the academic studies that shows connections with the industrial research, and for which all parties felt ownership.	• Bob tried to work with the research manager at HomecareCo to produce a coherent account. He this by making suggestions on how the academic work and the industrial work should be presented. He felt most of these suggestions were not followed, and the final report met the political needs of the industrial research team.
Are any societal issues that could be related to this project (e.g. the relationship between dishwasher tablets and water quality) completely mediated by HomecareCo?	• Bob would be aware of any big issues through the news • It's not a topic of conversation in any meetings • Bob's experience with other partners/projects is that partners can have hidden agendas • Partner might want a degree of secrecy as its competitors also work with others in university and information might be passed on inadvertently • Formal confidentiality agreements are now the norm.	• None identified as Bob comfortable with the company and its reputation for fair business dealings	

In addition to allowing the exploration of specific “opportunities”, the midstream modulation tool was used in interviews to draw out connections between different issues. This was facilitated by feeding back into the interviews observations and ideas from earlier in the engagement study. One example, which is important for the development of this narrative, followed the discussions noted above on the mechanism of cleaning action, the co-ordination of the overall project and the toxicological study at ToxicRes.

PE: *“is there a surface chemistry dimension to this? I'm thinking of a life cycle analysis where these nanoparticles have been constantly introduced to the surface of the plate or knife and fork. Are questions like ‘do these things build up over time?’ being addressed by the separate study? Who brings together the work of ColloidCo and ToxicRes?”*

Bob: *“HomecareCo. They do. I'm not exactly sure what's been looked at ToxicRes today, I just know they're looking at toxicity. I could probably look at publications but there probably aren't any. As to whether it builds up that will depend on the surface adsorption characteristics, the type of stuff we’re doing….So if you’ve got the surfaces that have been through washing machines 15 or 20 times, has it got 5 layers or 10 layers on? Does it build up in that way? Or does it reach 2 layers and that's it?”*

PE: *“and those are questions that HomecareCo are asking you to look at?”*

Bob: *“Indirectly. Layer build up is something they were interested in. However it becomes a bit more complicated than that unfortunately, because part of the studies that were investigating so far are just nanoparticles adsorbing onto interfaces directly, but in the real product there's also polymers there, there are different salts there and several other bits and pieces like enzymes and things…It comes down to the time frames within the project, because all projects are too short. If they say our primary goal with your project is to try to understand the mechanisms of adsorption of nanoparticles on different surfaces and the mechanism of removal. That's already two major questions so we can start to look at those processes and will* *probably find that it is a three year study …To then say okay how does it layer? That would really be a follow-up project."*

This extended quotation illustrates a number of important features of the innovation practice within this project that became evident through repeated use of the midstream modulation. A central characteristic of the project is that it is only HomecareCo who have sight of all the different strands of the project. There is a very clear division of labour with defined sub-project goals for each partner. For Bob, there is an ever-present challenge to relate the complex mode of action of the real-world product, with the models of action identified through academic research. One consequence of this is seen at the end of this quote where, as on many instances in our interviews, he identifies another interesting study (in this case layering of nanoparticles on plates) that he has not had the time, or freedom within the project, to pursue.

Becoming "Master" of the Lab

Having worked through the rogue finding of adsorption of nanoparticles to gold surfaces, Bob's work concentrated on the completion of a major report noted briefly in Table 9. This report was to inform a major internal project review at HomecareCo, and whilst the university funding commitments were secure, Bob's HomecareCo contacts informed him that the future of the wider company-based innovation project was in doubt. Whilst this review was taking place, the sense of urgency that had characterised the project over the previous couple of months was relaxed. Project managers at HomecareCo were no longer closely specifying the type of work they wanted performed at ColloidCo. Bob was confronted with a degree of freedom over research direction that had not previously existed in the project. Having identified a number of interesting studies during the course of our interviews that he had not previously had the time to pursue, which would he now select?

The absence of being constrained by the needs of the industrial partner was not a regular occurrence within ColloidCo. At the outset of the 12-week engagement whilst exploring the decisions on what research was conducted, the constraints felt by the research leader that prevented him from doing exactly what they might want to do, was immediately apparent. These constraints were not simply a reflection of the challenges of securing funding, but also recognised scientific fashions and dominant paradigms within his discipline. Not to work with the grain of these trends was to court obscurity. These constraints on freedom for action were reminiscent of Isaiah Berlin's two concepts of freedom (1958). Berlin explores two notions of freedom that he labels positive and negative. Negative freedom examines activities in which an individual has freedom from interference and is therefore restricted when the number of choices they can make is restricted. In this manner negative freedom is concerned with "opportunity for action". On the other hand, positive freedom concerns the freedom to do something rather than freedom from interference. Whilst notions of negative freedom would judge the availability of opportunity as determining the extent of an individual's freedom, this positive dimension adds some element of personal capacity to take advantage of the opportunity.

With science-led innovation, negative freedom is most closely related to issues of governance and regulation, with the efforts of innovators being restricted by the funding they can secure and the law of the land. However, through a number of interviews both the research leader and researchers showed a desire to be Master (to borrow Berlin's phrase) of their own projects, and pursue challenges that mattered to them. Indeed when discussing decisions on which experiments would be useful for the industrial collaborator, Bob actually used this very phrase:

"When you're in the lab, you're your own master to a certain extent. You know at the next meeting [with the industrial partner], you got to present some results about this, that and the other which hopefully should be relevant, so work is semi-driven. But the way in which you get there depends on the individual".

In reality there is no simple choice between positive and negative freedom, but the evidence of my engagement with Bob suggests that they characterize modes of innovation that might operate at different times within a project. At the level of lab-scale practices, then safety regulations were strictly adhered to, and the importance of toxicology for the subsequent chances of a particular material being commercialized formed a strong feature in the thinking of ColloidCo researchers. While accepting laboratory-level safety strictures and other constraints created by industrial collaborators, the experimental nature of the sciences being considered create many opportunities to pursue avenues of inquiry whose outcome is not clear. In such circumstances the researcher may become “master” of the innovation process in the sense of being able to open up a new direction for the projects. This suggested that these ideas of a freedom to innovate could not be separated from of simultaneous consideration of the likelihood of the outcome or impact for a particular course of action.

The starting point for our interviews at this stage was the progress of the report that was being jointly produced, and one of the threads taken up from earlier interviews was the issue of the lack of communication of findings from ToxicRes.

PE: *“So you have received no information on that [toxicology] side of the project? It's a very much need-to-know basis almost? Did you get the sense that the report covered those and you just didn't get to see those chapters?”*

Bob: *“No. I've seen all the report and the toxicological aspect is not covered at all. I think it is envisaged as a completely separate part of this work. As a consequence anything that comes out of that that may or may not be relevant to what we find here, we wouldn't know about it until it was published in the open literature. Which obviously delays things a little bit. It could mean some duplication although that's unlikely in this case, but it is possible that they may have done some similar types of studies. For example I have been doing some drying work recently, a newish area since I spoke to you last. This is closer to the area in which the toxicological people will be involved in, that's to say human contact with things that are on objects already…I'm starting to look at systems where I have deliberately tried to dry particles down onto the surfaces, and so because I'm drying them it* *means there's nowhere else for them to go. They are going to go on surfaces and I see them with the AFM, I know they are there. Deposition is very different to what we have been doing previously. That side of things is still being investigated; the wet systems still have scientific interest. The dried point of view is aiming more towards the structures that are found and possibly, I'm guessing here, and possibly what we find in terms of the structures that are deposited on surfaces may have some impact from a toxicological point of view. In other words the ease with which they can be transferred into the human body and what concentrations are relevant. So if I find a surface that is very patchily covered with particles at a particular concentration that would be of relevance for the toxicologist as you can imagine."*

PE: *"So what's behind that changing direction?"*

Bob: *"It's my idea really. I thought that based on all the work we've done both here and the bits and pieces that we've learnt from the HomecareCo stuff, it's quite clear that except under very few circumstances do you get adsorption directly in the liquid state of these nanoparticles particles onto the any particular surface. You need the surface to be positively charged and that's not very common. So most times you don't get any adsorption. That being the case and also knowing that HomecareCo have reported many times to us that we know particles are going down then the obvious way that that can take place is through drying."*

Of all the possible further studies that Bob could have chosen, he had been investigating the effects of drying solutions of the nanoparticles onto surfaces. Furthermore, in his introduction of this new direction he suggested *"this is closer to the area in which the toxicological people will be involved in, that's to say human contact with things that are on objects already"*; an element of the wider project in which he had hitherto shown no interest, as suggested by the absence of any attempt to learn of the ToxicRes findings. Research on adsorption under aqueous conditions was continuing with his PhD researcher colleague, because the *"wet systems still have scientific interest"*. And so here was a postdoctoral researcher who had always identified his primary interests as being with academic science, ignoring his unanswered academic questions from those aqueous studies, in order to pursue an entirely new dimension of this project, and making a connection with its possible relevance for human contact and the toxicology project. He speculates that there may be *"duplication"* and *"it is possible that they [ToxicRes] may have done some similar types of studies"*. Rather than trying to find out if this is the case, the project practice of clear demarcation lines of research in effect gives him permission to proceed with his own experiments.

It is interesting to speculate on the influence that repeated rounds of midstream modulation interviews contributed to this. Throughout these interviews a number of ideas reoccurred in Bob's reflections:

- The focus of Bob's work is always the contribution he makes through his own experimental work, and it is within the confines of his own laboratory that he feels most empowered
- He feels intellectually constrained by the obligations towards the industrial partner
- Whilst articulating contributions to the other elements of the project (and most notably that of the toxicology study), he makes minimal efforts (and then only in formal review meetings) to build relationships with the other researchers working on the project. In other words, he respects project practice of a strong division of labour.
- He is always thinking of the academic interest within the context of working on industrially-important problems. This is manifest in the number of "further [possible] studies" he articulates and his efforts to bring academically rigorous mechanistic thinking to bear on the project.

In light of recurring ideas it is possible to view the situation Bob experienced during this review of the wider project as giving him permission to be the "master" in the lab that he had previously reflected upon. He was, temporarily at least, freed from the obligations of doing work specified by HomecareCo; he could now pursue any of the "further studies" he had suggested during our interviews. That he chose one that connects with the toxicology study is interesting given that it surfaced (through my repeated introduction of the subject) in the midstream modulation interviews, and that he expressed a general disinterest in such studies, and had made no attempts to learn more of this aspect of the project. The work itself might generate important structural information concerning the extent and strength of deposition of nanoparticles, and hence the extent to which they may "break off" plates and attach to food. Such work is consistent with Bob's approach of bringing detailed scientific insight to bear on a real and practical problem. Following the quote above he went on to explain how such a study would plug empirical gaps in the academic literature, as well as connecting with a novelty area of science: the "physics of coffee rings"!

Concluding Remarks

The STIR research proposal (Fisher and Guston, 2008) identified three tasks for the human/social science scholar embedded within a technology group: "(1) observe *de facto modulation* in action (2) stimulate *reflexive modulation* by communicating observations, and (3) document any resulting *directed modulation* over time". *De facto modulation* concerns the surfacing of societal influences on the innovation process that arise through the reflections of the natural scientist during the course of the interview process. The experience of my collaboration with Bob was that single applications of the midstream modulation protocol succeed in rendering visible a range of socio-ethical issues relevant to the innovation process (cf. Schuurbiers, 2011). Repeated applications of the protocol saw the natural scientist making reference to ideas from earlier interviews, and in doing so refining his understanding of wider issues, and practising the manner in which he articulated them. In other words, repeated use of the protocol seems to encourage *reflexive modulation*. This interweaving and refinement of arguments was noted through simple cross-referencing of interview transcripts. In considering the question of whether I observed *directed modulation*, or the modification of decisions in light of reflexive awareness, we turn to the most challenging aspect of my analysis. This challenge has provided the motivation for producing this detailed account relating to a single decision on a change in research direction.

Across both of the case studies and the four scientists whose work I followed over a 12-week engagement study, Bob's decision to start a new stream of research concerned with the layering of nanoparticles upon drying, is the only candidate I have for evidence of directed modulation. It could be that 12 weeks is an insufficient time-period over which to observe such change. In this regard one should also note the relevance of the type of laboratory work being observed and its associated timeframe of decision-making. Both our case studies involved scientists performing experiments using complex and advanced instrumentation. The timescales for such experiments are invariably long, and the timescale of the decision-cycle should be borne in mind when designing the length of a particular lab engagement study. Allowing these explanations of the relative scarcity of directed modulation candidates, the interpretation of Bob's decision presented in the last section seeks to offer a coherent rationale for how the repeated use of the midstream modulation protocol might have influenced his thinking. However, I acknowledge that a more mundane interpretation is possible in that after a protracted time of studying the wet phase of the dishwashing cycle with no meaningful results, an obvious aspect to study next might be the drying phase.

I continued to track this aspect of Bob's work after the 12-week period and report that the research continued even after the HomecareCo review ended their wider innovation project. Bob and his supervisor eventually published the research in a leading academic journal in their area. In it they referred to the "coffee rings" literature, but also drew attention (in the first line of their paper) to the application of such science to an understanding of the drying process inkjet printing; a new area of research and innovation within ColloidCo. Bob himself, in answer to a direct question of whether the repeated rounds interviews had influenced his thinking, felt that it had not, that he was naturally "a thinker" (which seemed in context to be his preferred word for "reflective") and that he would have made that decision regardless of my engagement with ColloidCo. Demonstrating the "truth" (in some positivistic sense) of such assertions seems not to me the primary issue. Whilst the work should continue for methods to enable a more thorough analysis of directed modulation, the engagement described in this chapter provided a hitherto unavailable practice to enable a self-identified “thinker” to work through a wider range of innovation issues than those that were emanating from his innovation partners.

Chapter 11
Organisational Capability for Science-led Innovation

Having presented a comparative account of my two case studies (Chapter 9), and examined innovation work at the level of the individual researcher (Chapter 10), this chapter examines the organisational-level work of innovation strategy. The approach to generating data for my two case studies was informed by my reading of the literatures discussed in chapters 2 & 3, pilot research (Chapter 7) and the critique of policy literature (Chapter 8). I distilled this work into three broad literatures: policy discourse on Research Impact and Responsible Innovation; practices of science-led innovation; and organisational capabilities for innovation. The nature of these literatures recognises the intersection of policy, practice and theoretical ideas that influenced my approach to the research field. These ideas were used to guide my search for data during the case research: they influenced the formal interviews and informal conversations I held with different participants in the two case studies.

The policy discourse on Research Impact (including the emerging discourse on Responsible Innovation) expressed growing government rhetoric on the knowledge economy and the role of universities in it. Conducted and enacted at an institutional level, this discourse begged questions for how individual research groups would organise their research and innovation work. In my pilot research (Chapter 7) I heard a great deal of uncertainty from researchers regarding how they were thinking of responding to these new mandates. However, I also encountered examples where groups and individuals had found ways to realise new impacts from their research.

There is a rich academic literature built upon extensive empirical evidence that has examined innovation involving university scientists (Chapter 2). These studies described and explained changes at the institutional level of universities that had created new organizational mechanisms (funding and knowledge transfer intermediaries) for researchers to pursue Impact. Two routes to impact dominated literature discussions: academic entrepreneurship and university-industry collaborations. However, extensive reviews of the field (Abreu *et al.*, 2009, Perkmann *et al.*, 2013) had discerned a wider variety of ways in which researchers were connecting with economic agendas linked to their research. I had also encountered a similar breadth of possible *pathways to Impact* (to borrow the phrase employed by the UK Research Councils) being explored by researchers.

Studies of innovation involving university researchers might be explained in more theoretical terms by invoking the concept of organisational capabilities (Chapter 3). Within this literature ideas about how a capability for innovation might develop or change (Helfat and Peteraf, 2003, Teece, 2007) suggested frameworks to guide the search for evidence amongst university research groups of their response of mandates for societal and economic relevance. In addition the strategic influence of the research leader on the organisation of the research enterprise held out the hope that this research field might contribute to theoretical ideas about the relationship between strategic agency and organisational capabilities (cf. Pandza and Thorpe, 2009).

These literatures were used to direct my search for evidence as I engaged with the researchers in both ColloidCo and MicrosCo. I developed a semi-structured interview protocol for use with those people able to influence leadership in my two case studies, in order to better understand the strategic behaviour within the Research enterprises. In the case of Colloid Co these questions were used with the Research Leader and senior Post-docs. In MicrosCo the questions were used in interviews with the two main Directors, other members of the leadership group, and their strategic partners in industry and the regional development agency. With these questions I hoped to gain insights on how this strategic behaviour influences the emerging capability to innovate responsibly or achieve impact. I was interested to learn about how emerging socio-political aspirations for Responsible Innovation and Research Impact get transferred into the groups' innovation strategies (this was the organisational level complement to the lab-scale practices discussed in Chapter 10). In particular I was interested in exploring the role of leaders (strategists) in mediating between wider social aspiration and micro-practices that constitute organizational capability.

The Protocol uses four big concepts in strategic management relating to: differentiation; alignment; opportunities; and leadership. The issues relating to these concepts were informed by my reading of the three literatures are now considered in more detail. The first of these questions relates to strategic differentiation:

In what ways is this laboratory different from other comparable laboratories and do you think any of these differences account for its research and innovation performance? If so, in what ways?

Strategic differentiation assumes heterogeneity amongst organisations, and advantages that might accrue to an enterprise are a result of different endowments with resources, capabilities, knowledge *etc.*, in comparison to other organisations (e.g. competitors). Here I was interpreting competition in relation to scare resources (e.g. funding, attracting top-class PhD students) rather than simply in terms of some commercial advantage in a product marketplace that resulted from the groups' innovation activities. I assumed that a research lab has a peer group with whom it competes for resources (e.g. space in journals, funding, etc.) in order to pursue its own research and innovation agenda. This peer group could be characterised in different ways, perhaps narrowly in terms of specific research interests, or more widely in terms of a broad discipline or even locally within the same institution. In exploring strategic differentiation I sought to understand the advantages (over its peer group) that the lab might have in respect of its own resources, expertise and capabilities. My reading of the three initial literatures suggested that possible examples of factors that would contribute to strategic differentiation are presented in Table 10 below. When asking this question I listened for these and other unique features in both groups,

Table 10 – Indicative sources of strategic differentiation explored in semi-structure interviews

Unique Resources	Unique Expertise	Unique Capabilities
• Equipment • Technology • Software	• Research competency • Technical expertise • Disciplinary expertise • Specific knowledge	• Interdisciplinary collaboration • Inter-institutional collaboration

Asking this question invariably generated a lot of interesting information, but I was particularly interested in understanding how any of these factors might contribute to an understanding of Responsible Innovation as a source of strategic differentiation. In other words, I sought to explore whether (and how) the aspiration for Research Impact and Responsible Innovation had been incorporated into the resources, expertise and capabilities of the research group. None of the research leaders identified Responsible Innovation as source of advantage or differentiation vis-à-vis its peer group of laboratories, as those specific policy mandates were very new to them. However, the influence of the Research Impact agenda was already apparent in how they were thinking about the future development of the two groups. In both cases, we were able to explore together the possible consequences of unique practices and capabilities for Responsible Innovation.

The second principal question in the semi-structured interview protocol was:

Are there changes that have been made that have produced significant improvements in work of the laboratory and its innovation performance? If yes then how have these changes occurred? (e.g. from where/who did the change emanate?)

This question was founded upon a notion of strategic alignment, which argues that successful enterprises must operate in ways that are congruent with its organisational environment. To do otherwise would engage the enterprise in conflicts that distract it from achieving its purposes. In contrast, achieving congruence with its operating environment allows the enterprise to garner complementary support for its agenda. Possible elements of strategic alignment that I had discerned from the three initial literatures are shown in Table 11.

Table 11 – Indicative sources of strategic alignment explored in semi-structured interviews

University Innovation Policy	National or Trans-national (EU) Innovation Policy	Innovation Partnerships
• Innovation priorities • Innovation performance (publications, patents, external funding) • Internal Innovation Funds • Policy on Intellectual Property	• Research & Innovation Funding • Targeted areas of Innovation	• Intellectual Property Collaboration • Inter-institutional collaboration • Proximity of other research institutes or innovation centres

Typical supplementary questions explored those elements in the organizational environment of the research lab that complement the possibility of responsible innovation: how had these elements changed the behaviour of the research group? This interview question sought to probe into laboratories' capabilities to change or adapt to external dynamics. Together with the research leaders I explored if societal aspirations for Research Impact and Responsible Innovation had or might trigger changes in laboratories' practices. In doing this we would invariably considered whether such aspirations were considered as a liability or opportunity, which in turn introduced the next question.

What are the opportunities that are perceived by those who work in the laboratory that serve to shape the direction of the research agenda?

This question sought to explore the nature of strategic opportunities to which the group's leaders had given attention. In this I regarded strategic opportunities as ones in which there is a fit between external challenges and internal resources and capabilities. As ever, I was particularly interested in understanding how these opportunities might relate to shifting innovation policy mandates, and some initial avenues for exploration are shown in Table 12. I probed awareness of these (and related) policy agendas as a prelude to discussing how they had or might relate to the development of the group's own innovation strategy.

Table 12 – Indicative sources of strategic opportunities explored in semi-structure interviews

Grand Societal Challenges	Innovation Trends	New Research & Innovation Agenda
• RC-UK "Grand Challenges" • "UN Millennium Goals" • "Copenhagen Consensus"	• Trends within the research discipline or associated industry sectors	• Finding Council Impact agenda • Government Innovation Policy • Responsible Innovation policies

Finally, and although implicit in much of the foregoing discussion, I explored the influence of the research leaders.

Could you please describe the role (activities) of a Research leader in shaping the Group's innovation agenda?

My reading of the three initial literatures suggested a number of possible avenues for exploration (Table 13).

Table 13 – Indicative leadership behaviours explored in semi-structured interviews

Enterprising Behaviour	Strategising Behaviour	Collaborative behaviour
• Opportunity identification • Autonomous action • Knowledge integration • Momentum building/forcing for change	• Decision-making • Resource allocation • Sense-giving • Facilitating • Championing • Positioning	• Boundary spanning • Managing inter-disciplinary relations • Managing inter-institutional relations

With this question I listened for what considerations were made (if any) to take account of wider social/economic concerns in developing the Group's research and innovation direction.

Not only were these questions use in formal interviews, but they also informed the everyday conversations I had with members of the research groups during my time with them. These data were supplemented were appropriate with data drawn from group documents (e.g. contracts with innovation partners, "Impact statements", research proposals, reports, research papers, PowerPoint presentations and unobtrusive observations). All data was extracted or transcribed into text format and analysed as described below.

Data Analysis

The analysis of these data proceeded in an iterative manner by moving between the initial literatures, the data and the emerging structure of the theoretical argument (Miles and Huberman, 1994, Locke, 2001). The dynamic nature of this analytical process means that it is not possible to define strict (linear) analytical steps. However, it is possible to describe two broad phases involved in the progression from collecting data to theoretical inferences (cf. Corley and Gioia, 2004, Pratt *et al.*, 2006). This process and associated database was managed with the use of NVivo software (Bazeley and Jackson, 2010).

Phase 1 – Creating provisional themes and first-level categories.
The data were examined for my research participants' descriptions and explanations of their innovation work. These were captured in their own words via open coding (Locke 2001) and resulted in a long list of statements. Those statements expressing similar ideas were then clustered into *first-order themes*, and summarised in a short sentence to articulate the theme. In this manner a long list of first-order themes was produced. Examining these first-order themes in relation to the *initial literatures* allowed a further consolidated into a

shorter list of *first-level categorisations*. The choice of labels for these categories again involved iterating between the language of the participants and ideas from the literature. This abductive process (Van de Ven, 2007) of moving between the data and initial literatures, clustering participant statements, and naming the clusters meant the emerging fist order categories have a more theoretical character (Van de Ven, 2007). In the terminology of NVivo the analysis progressed from the *open coding* that creates provisional first-order themes to the *axial coding* producing first-level theoretical categorisations. This process is shown schematically in Figure 16.

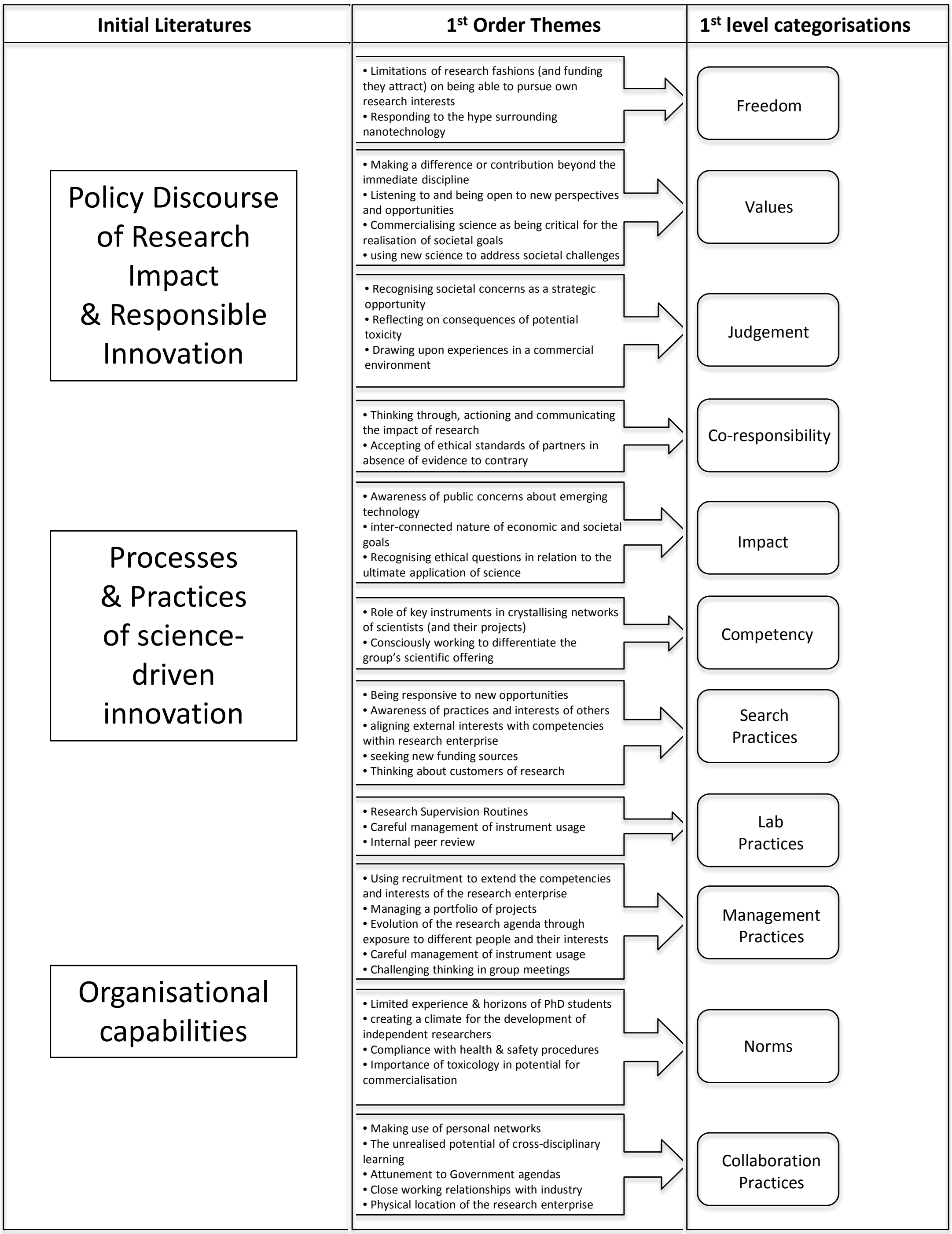

Figure 16 – Phase 1: data structure of first level categorisations

Phase 2: Second order aggregate categories. The next phase of analysis involved working with first level categories to construct an explanation of the influence of mandates for Impact and responsible innovation on the work of research groups in emerging technologies. Once again, this process is abductive in nature and involved the interplay of empirical material and theoretical ideas (Van de Ven, 2007). As a means of exploring how different categories could be combined, dimensions that seemed to underlie the first-level categories were identified. For example some categories seemed processual (e.g. ‘search practices’), some routines (e.g. ‘lab practices’), whilst other seem to represent a particular state (e.g. ‘Freedom’ or ‘Values’) or relate to personal skills (e.g. ‘competency’ and ‘collaboration’). On this basis different ways of combining these categories were tried and tested in discussions with research colleagues and at presentations at research seminars. Different conceptual models were critiqued in this manner both for their internal coherence and in relation to established organizational theories. Once a possible second-level framework had been created it was also critiqued in relation to its fit with the case data (Locke, 2001). Thus, the first-level theoretical categorisations represent abductive links between the case data and existing theoretical constructs, and suggested further literature that we might consult to guide theory-building (Eisenhardt, 1989).

Figure 17 is a summary of the second phase of the data analysis process and shows the first-level theoretical categorisations and the second-level aggregate categorisations. Such diagrams often incorporate arrows to show how the aggregate categories have been built from the first-level categories (cf. Pratt *et al.*, 2006). However, in this case I found the resulting schematic could be highly confusing with many arrows criss-crossing the figure. Therefore the construction of the second-level is presented in Table 14.

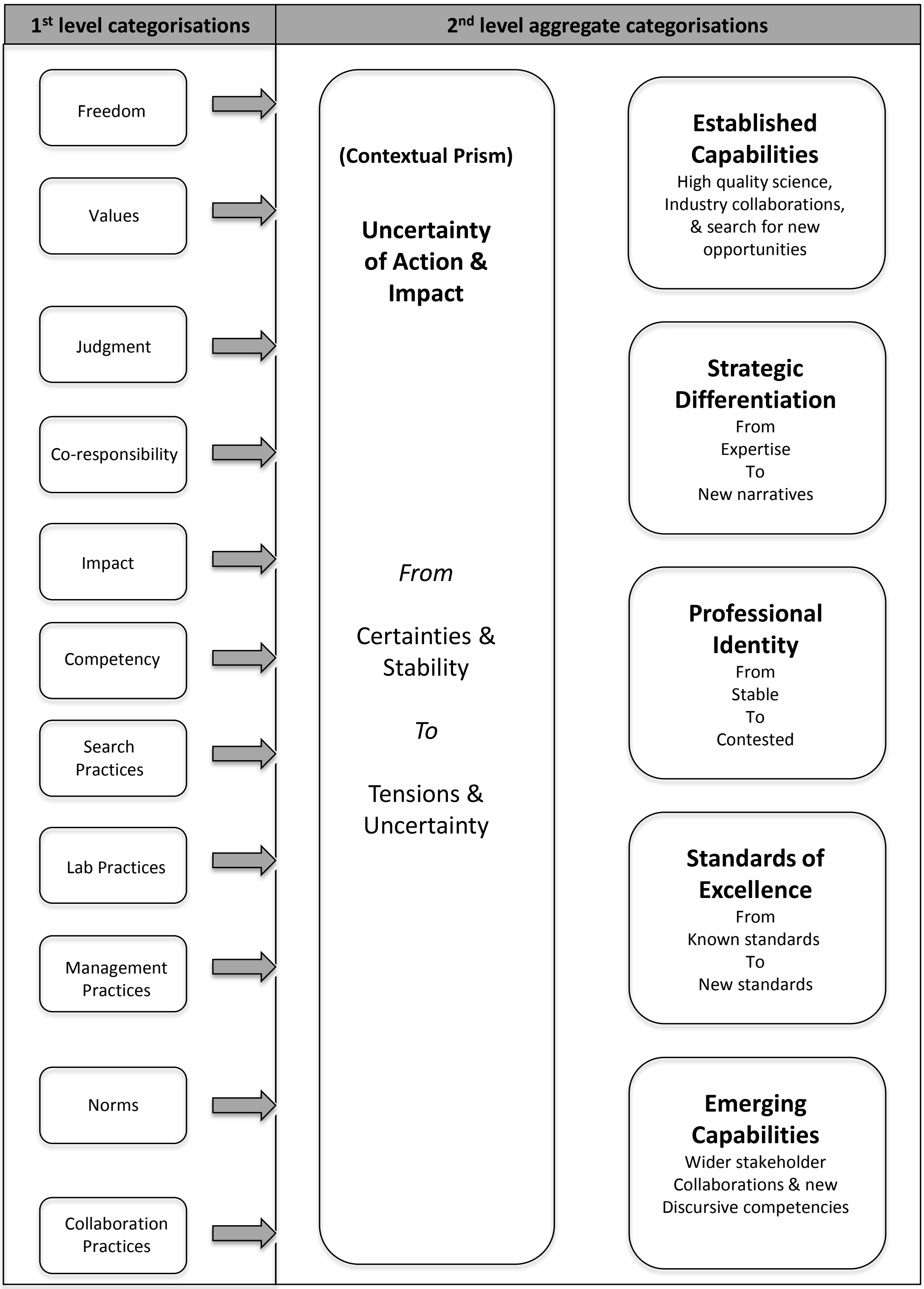

Figure 17 – Phase 2: data structure of second level aggregate categorisations

The aggregate category of “uncertainty of action and impact” is constructed from the first level categories of “Freedom” (the limits and obligation associated with needing funding in order to pursue a research and innovation agenda) and “Impact” (the interconnected nature of economic, social and scientific goals). With some evidence this aggregate category suggested stability and continuity, whereas with other data there were clear tensions and uncertainties. In examining the relationship between this category and the other emerging aggregate categories, then “Uncertainty of Action and Impact” acted as a kind of contextual prism. In other words it suggested a changing perspective as contexts moved between situations characterised by low uncertainty to ones of high uncertainty. As the degree of uncertainty changed, then so did the nature of the other second-level categories. The transition between low and high uncertainty, the corresponding changes in other aggregate categories and the constituent first level theoretical categories are all shown in Table 14.

It is in the nature of this type of inductive analysis that some established theoretical concepts emerged as being important during the very process of research. For example, I did not start my fieldwork with the expectation or orientation that the professional identity of researchers would be relevant for a study of research impact and responsible innovation. The importance of this established theoretical concept only became apparent from the analysis described above. Once evident that it is important for an understanding of responsible innovation it becomes possible to draw upon established ideas of professional identity in order to develop theoretical inferences.

Table 14 – Constitution of second level aggregate categorisations

Second-level Aggregate Categorisations	Contextual "Prism" of Uncertainty	Modulation of Aggregate Categorisation	Constituent First-level Theoretical categorisations
Organizational Capabilities	Low	Established capabilities	Competency, Collaborative practices, Search practices, Management practices, Lab practices, Norms
	High	Emerging capabilities	Values, Judgment, Co-responsibility, Impact, Search Practices, Collaborative practices
Strategic Differentiation	Low	Expertise as differentiator	Competency, Management Practices, Lab Practices, Judgment
	High	New discourses for differentiation	Values, Judgment, Co-responsibility, Impact
Professional Identity	Low	Stable identity	Competency, Collaborative practices, Management practices, Lab practices, Norms
	High	Contested identities	Values, Judgment
Standards of Excellence	Low	Known standards of excellence	Competency, Collaborative practices, Management practices, Lab practices, Norms
	High	Emerging new standards	Values, Judgement, Search practices, Co-responsibility, Impact

A description of the research findings that emerged through this analysis are organised in two sub-sections suggested by the contextual prism of “uncertainty of action and impact”. However, in presenting these sub-sections it is not my intention to suggest there are two discrete contexts in which the work of research enterprises in emerging technologies may be placed. Rather they are both aspects of the same lived experience of doing innovation work as part of a university research group. “Low Uncertainty” should be understood as the familiarity that scientists have with their methods of their craft and not the outcomes of applying those methods. They proceed with their experiments with a confidence born of their training and experience, just as they do not know the results of those experiments. “High Uncertainty” expresses a new space in which they find themselves as a consequence of the new policy mandates of Research Impact and Responsible Innovation. It is their *response* to this new innovation environment that is uncertain. Developing new strategies in response to emerging policy mandates does not negate the need for the established certainties of their research craft. These two sub-sections are not two scenarios in which they might find themselves. Rather, the changing policy environment requires them to develop their innovation capabilities in order to respond to a wider range of opportunities. In this manner the new policy mandates of Research Impact and Responsible Innovation can be thought of as a “capability branching” point (Helfat and Peteraf, 2003) discussed in Chapter 3.

Stability and Certainties of Established Capabilities

The analysis and coding of the primary data suggested categories of activity that might be considered as *established capabilities* for science-led innovation. These are manifest in the conduct of high-quality research, collaborating with industrial innovation partners, and searching for new opportunities. Evident in each of these domains were activities that exhibited clear strategic features; illustrative examples of such elements are presented in Table 15.

Table 15 – Indicative quotes and evidence for established capabilities of science-led innovation

Established Capabilities	*Description/evidence*	*Dimensions of Certainty and Stability*	
		Strategic Differentiation	*Professional Identity & Standards of Excellence*
High-quality science research	MicrosCo – Two main directors hired to lead new initiative are acknowledged international leaders in field. ColloidCo – Group has produced large number of frequently-cited papers over a sustained period.	MicrosCo – 'We are aiming for a fully differentiated intellectual and practical product in the UK and hopefully also global reference frame.' [Director A] ColloidCo – the enterprise equates the highest technical standards with being able to make an impact: 'To be truly useful, you've got to make sure that your tools are right up to date, and of the highest quality.' [Research Leader]	MicrosCo – 'As Directors we have the responsibility of keeping ahead, we have to lead by example. We have to get involved hand-on in running experiments. If we want to keep [MicrosCo] ahead, we [the Directors] have to be number one also' [Director B] ColloidCo – '[Our] main purpose is to produce ´independent researchers´. Above all else. Even above me writing papers. What you want is a bunch of really good people getting out there and contributing.' [Research Leader]
Industry–university Collaboration	MicrosCo – New enterprise created in collaboration with leading Japanese science-based company. Already it has major collaboration with blue-chip industrial companies. ColloidCo – Organization has track record in collaborating with leading multinational companies throughout various projects.	MicrosCo – Research directors have recruited an 'experimental officer', whose role is to generate new collaboration with small and medium-sized enterprises. ColloidCo – 'We always worked well with industry, without compromising the ability to do good-quality science. We have always managed to publish in good journals, but we have also managed to generate excellent interactions and contacts with people from real world, with real problems.' [Research Leader]	MicrosCo – 'Why are we going down this route? It's what we all want to do, but also because we've been told by industry, British industry, very clearly, that they're sort of interested in working with us, but if we follow this route, they're really interested in working with us and we've begun to garner some significant support on this basis.' [Director A] ColloidCo – 'So I don't see myself as someone trying to solve the problems of drug delivery using nanotechnology. I see myself as someone who is interested in developing knowledge, then if someone comes along and asks me can you apply this to drug delivery, then I'll say, 'Yeah, sure, we can do that.'' [Research Leader]
Search for Research Opportunities	MicrosCo – Clearly articulated 'differentiated intellectual product' forms basis of transformative search for new customers via conference presentations and participating in business networks. ColloidCo – two new enterprises have spun out of the research from ColloidCo, and opportunity-seeking behaviour is evident in all researchers.	MicrosCo – The group has set up a series of multi-disciplinary seminars, with a view to identifying research challenges for their microscopy service. ColloidCo – 'And the other way that you get challenged is by bringing people into your group with new skills. So when I've had a sabbatical visitor, I've made them sit in with the students. Why? Because it's good for them and it's good for the students, and it challenges everything you're doing and they start asking really good questions.' [Research Leader]	MicrosCo – 'A subsidiary interest of mine is actually passive energy reduction for buildings. So, you know, though it doesn't have a direct tie to this at the moment, it will have. I'll figure it out.' [Director A] ColloidCo – 'I think, for me, I've found the open approach is much better, because I don't think I've got the wit to keep coming up with new ideas. I'm pretty good at ... making something into a new idea, but I need to have the bits of Lego … I've got to have other people and I like challenges. I like people coming in.' [Research Leader]

The conduct of research judged by peers to be of high quality might reasonably be expected to be an important feature of science-led innovation. Any scientific idea will be thoroughly tested through an innovation process, and found to be wanting if it is not based on the high standards of those practised in the conduct of that science. The extent of the recognition of both enterprises is manifest in the leading positions that their research leaders occupy within their professional communities, along with the strength of their published research.

Collaborating with major industrial companies is the principal means by which these research enterprises realise their innovation aspirations (cf. Chapter 2). Over many years and rounds of projects, ColloidCo has worked with blue-chip multi-national companies. MicrosCo was itself formed by a partnership that included a leading Japanese company. Core aspects of the enterprises' strategic activities are geared towards realising innovation projects through collaboration with science-based companies. Such projects have proved successful in terms of both research (e.g. the production of peer-reviewed research papers) and innovation (e.g. the realisation of industrially-relevant knowledge). The case descriptions in Chapter 9 presented a number of examples of efforts to extend such established strategic approaches, e.g. efforts to recruit an 'experimental officer' at MicrosCo to connect with small technology companies, as well as the large companies with whom the research enterprise already collaborated.

The search for research opportunities evident in the case studies suggests a degree of entrepreneurial behaviour within both organisations, and this seems influenced, in part, by the attitudes of their leaders. The very creation of this new research enterprise resulted from the convergence of the three (Triple Helix) partners' individual searches for nanotechnology opportunities. At ColloidCo, two new enterprises have been 'spun out', and current researchers have regular professional and social contact with their counterparts in these start-up companies. An orientation towards the challenge of commercialisation was evident in the way new researchers framed their study questions, and Jack's (the Senior Post-doc within the group) participation in an entrepreneurship programme.

The other second level aggregate categorisations (i.e. strategic differentiation, professional identity and standards of excellence) suggest important features of the established capability for innovation as applied to issues of Research Impact and Responsible Innovation. The manner in which these categories contribute to a richer understanding of the research groups' capabilities is presented in the next two sub-sections. Structuring the findings in this way also acknowledges the close relationship I discerned in the empirical material between the categories of *professional identity* and *standards of excellence*.

Strategic Differentiation

Both research enterprises compete with similar groups in their peer communities for resources (e.g. grant funding, and the best young researchers). The recognition both groups have received from within their community not only reflects high standards in the conduct of science but also suggests the importance with which their contributions are viewed by funders, industrial collaborators and prospective students. This performance is consistent with the strategic efforts they have made to position themselves within their areas of science. The research leader at ColloidCo equates the highest technical standards with being able to make an impact: *"To be truly useful, you've got to make sure that your tools are right up to date, and of the highest quality"*. And despite representing a much younger enterprise, the directors at MicrosCo were also able to clearly articulate "*a fully differentiated intellectual and practical product on a UK and hopefully also global reference frame*" [Director A, MicrosCo]. This was the strategic rationale for extending the technical capability of a state-of-the art electron microscope: the technical strategy noted in Chapter 9 that was causing some disagreement with their leadership board.

The Case descriptions in Chapter 9 include many examples of specific initiatives that can be understood as the research leaders pursuing strategies to differentiate themselves from their peers. An indicative example for ColloidCo was the efforts to facilitate the interactions between the group's researchers and the innovation professionals in spin out companies. A further illustration of how the research leader sought to position the group with respect to the needs of their industrial collaborators can be gauged by the use he made of a course in colloidal chemistry for one of his major partners. Delivering this course allowed him to create a file that *"contains a hundred new project ideas"*, as the participants used real and present problems during the discussion in the course. For MicrosCo, then in additional to differentiating the capability of the microscope, then the propose recruitment of an "Experimental Officer" to engage with SMEs was recognised as something that would distinguish them from other groups.

Maintenance of a vibrant attitude towards new opportunities appeared to be the driver behind a number of activities. The directors at MicrosCo have set up a series of research seminars to which users of their facility are invited. These events reflect the multi-disciplinary character of the work with which they are involved, but also represent an opportunity to connect researchers that might not otherwise meet. It is anticipated that any new projects created from such networking will result in more income for MicrosCo. At ColloidCo, recruitment of new experienced researchers was oriented more towards the introduction of novelty of experience (for the enterprise), in order to maintain a flow of new perspectives.

Professional Identity and Standards of Excellence

The most common innovation activities within the enterprises could be understood in terms of the routine practices of science. Experiments and trials were designed on the basis of the knowledge held within the group, and the impact of such trials was uncertain, to the extent that the group's knowledge allowed for assessment of risks, and likely adverse consequences could be designed out. The conduct and standard of such activities was informed by the researchers' training, and reflected their identity as scientists. Undertaking the safe design and efficient conduct of experiments was what they considered scientists do. Such professional identity, and particularly the extent to which such an identity suggests responsibilities (vis-à-vis societal concerns), finds expression in the norms of laboratory life (e.g. performing risk assessments for experiments or the handling of new chemicals, maintaining scientific instruments to the highest standards, and rigorous internal peer review of new thinking).

Industry–university collaboration are the means by which the researchers are able to access the resources needed to apply the ideas generated through their research. The industrial partner is likely to give access to specialist commercialisation services, for example laboratories able to test the toxicological properties of new materials. This creates a situation in which the responsibility for the tasks of innovation is shared. In this respect, researchers at ColloidCo exhibit an absolute trust in their main industrial collaborator – to the extent that they allow the conduct of toxicology tests without enquiring after the results. Such trust has development over time and across a number of different projects. However, it is reasonable to ask at what point such trust tips over into disregard for matters being dealt with by the industrial partner. At no point had the researchers in ColloidCo demanded information about the findings of toxicological studies conducted at facilities. As I discussed in Chapter 10, the toxicology testing being conducted as part of Bob's project (ColloidCo) did not feature in his thinking until he was free to decide on the future course of the project. His decision to do so under those circumstances is open to the interpretation that he became aware of the harmful potential of nanoparticles deposited on plates, and felt a responsibility to act (Chapter 10).

The activities undertaken to aid the search for new activities are readily understandable in terms of the organisations' efforts to sustain and enhance their position compared to research rivals. And yet interviews with the research leaders in both enterprises revealed the value set that underpinned their action. The main director at MicrosCo spoke frequently of an interest in environmental challenges, and indeed a number of their existing projects reflect this interest. Whilst these projects also had significant scientific drivers, more intriguing were environmental challenges (e.g. passive energy reduction in building) for which the director did not yet have a solution in mind. Scientific drivers or industrial partners are not the only point of departure for the development of new projects; the awareness of a societal challenge is sufficient to orient the enterprises' search for new opportunities.

Tensions and Uncertainties of Emerging Capabilities

During the time I was engaged with the research enterprises, two aspects of their context were prominent in our various interactions: the extent of uncertainty in the impact of their work, and the degree to which their freedom to operate was constrained by external interests. To reiterate, uncertainty of impact here means the manner in which they dealt with the consequences of their work. At the level of laboratory decisions regarding science, then it mattered less (to my observations) that they could predict outcomes than that they had the intellectual resources with which to discuss and act upon likely outcomes. As the consideration of impact extended beyond the immediate to further downstream in the innovation journey, then the assurance with which they discussed the implications of their research decreased. Such circumstances required them to develop new capabilities so that some consideration of outcomes might inform their activity.

In conversations with research leaders they expressed the view that the innovation policy discourse as it affected universities was acquiring a new character. My engagement with the groups (during 2009/10) coincided with the most vigorous period of discussion about the Research Impact agenda (cf Chapter 8). The Hefce pilot project on Impact case studies (U.K._Government, 2010, HEFCE, 2011) included one of the sponsoring departments of MicrosCo. Similarly the idea of Responsible Innovation was starting to appear (Owen and Goldberg, 2010) in research calls from the EPSRC: the primary research council for both ColloidCo and MicrosCo. I had selected these cases on the basis that they were already active in different types of innovation work. What I heard from their leaders was that they had a sense that they needed to respond in new ways in order to continue to differentiate themselves from peers, and secure funding from (what seemed at the time) a shrinking government budget. ‘Pathways to impact’ statements had become required with all new grant applications. However, the practice of writing such grants had not become institutionalised. There were no models of best practice and the contemporary guidance notes from the research councils were judged of limited help. Director A at MicrosCo opined that research councils did not know themselves what constituted a good “Impact Statement”.

During my engagement with both groups I became involved in a number of discussions related to the production of Impact Statements. I was presented with early drafts of such statements and asked to critique them. An analysis of the data generated in the case research allowed me to discern two particular dimensions to a changing capability for innovation. The first involve the participation in new types of collaboration with innovation stakeholders. Both Research Groups participated in these collaborations in the full expectation of some benefit, even if the benefit was not yet articulated. However, such partnerships also carried with them obligations or constraints on the actions of the research enterprise; they were no longer masters of their range of interests. The second feature of their emerging capability involved the development of new discursive resources in order to offer an account of who they were and what they did. Indicative evidence for these emerging features of their capability for science-led innovation are shown in Table 16, and further explained in the subsections below.

Table 16 – Indicative quotes and evidence for emerging capabilities for science-led innovation

Emerging Capabilities	*Description/evidence*	*Dimensions of Uncertainties and Tensions*	
		Strategic Differentiation	*Contested Identities and new standards*
Collaboration with wider Stakeholders (other than research institutes and industrial companies)	MicrosCo – Pursuit of unusual collaborations to demonstrate their civic relevance, e.g. with the stained glass workshop of the local 13th century cathedral. ColloidCo – researchers doing extra work to engage with new stakeholder with public engagement with science event and winning a place on an entrepreneur development programme.	MicrosCo – 'I was very struck by how open and how quickly [MicrosCo] were there. They were so quick to say Yes. Yes we can, in fact exactly offer you this kind of service. They already had a sense of what stained glass was and potentially wanted to investigate and so I didn't really have to work very hard in convincing them that it was worth working together' [Sue, Art Heritage collaborator]. ColloidCo – 'The challenge is that a lot of people aren't good at relating their work to others coming from a different angle. My interest [in communication] was born during my degree in nanotechnology which spanned five departments and required us to make a common language between disciplines. We found that scientists can struggle to tell each other what their work is about' [Jim, PhD researcher]	MicrosCo – 'We really like to enthuse students, in particular with the sense that they are actually going to do something that's worthwhile … What is the real value of what you are doing? What's your life mission? Is it to keep producing five scientific papers a year?' [Leadership Board member] ColloidCo – 'My main disappointment is that we don't as a community, science community, spend enough time taking account of who pays us and what they want to see for what they are paying and we should have a responsibility to communicate with them in a language they can understand. You know, in the end it's your money that I'm spending, so it's my responsibility to try and help you understand what I'm doing.' [Research Leader]
New Discursive Competency	MicrosCo – Crafting of Impact Statements that seek to go beyond the familiar rhetoric of industrial collaboration ColloidCo – Postdoctoral researchers contemplating creation of their own research groups coming to understand that the familiar model will be insufficient to attract funding	MicrosCo – 'is the economic and, if you will, moral case, enough? Or do we have to have a broader context into which to put these things. And actually I have to realise that I haven't seen a model impact statement from anybody' [Leadership Board member] ColloidCo – [in submitting proposal for a major early career researcher grant]:'to try to stand out I focused on the relevance for industry. I focused on this part rather than the science itself, which is a risk, as the referees may want more science. I still think it is a good thing to do this from the start as I think this is what the [funding body] is looking for' [Jack, Postdoctoral researcher fellow]	MicrosCo – 'I think anybody who's thinking about these industrial processes must now have the environment as a licence to operate – freedom to operate, we would call it – in front and centre of what they are actually planning to do for the future.' [Director A] ColloidCo – "I see the way in which those funding bodies operate as having a core that needs to be maintained somehow and I hope the EPSRC [Engineering and Physical Sciences Research Council], or whatever they get called in the future, will maintain that. Engineering is an interesting one by definition. We are at a crossover, so how do you include some hard science and some applications within it? How do you share that money around? I think that's quite a difficult one to solve and there seems to be an interest in more short-term projects…which is interesting, but it's also a bit of a shame to anybody who's dedicated to pure science who sees that as part of the spark for why in the subject in the first place that's what keeps you going" [Bob, Postdoctoral research fellow]

New Opportunities for Strategic Differentiation

Most external engagement activities at both enterprises are concerned with stakeholders who have a direct interest in the research outputs (e.g. industrial companies). Interviews with the groups' leaders, and the external activities of some of the researchers themselves, provide evidence of awareness that contemporary university research groups have a wider set of stakeholders. Such awareness has not yet translated into norms or organisational practices, but instead is characterised by a collection of exploratory activities.

As described in Chapter 9 Jim (a PhD researcher at ColloidCo), acting through personal motivation, secured a scholarship to participate in a dissemination-of-science event for the general public. This activity required him to produce a poster explaining the rationale of his research project and its findings, to the general public at an open science fair. In a similar vein, researchers at MicrosCo have hosted visits to their facility by schoolchildren, with the aim of encouraging the study of physics at university. Such engagement events are supported by the rhetoric of the Leadership Board Director at MicrosCo, challenging the next generation of scientists to ask: *"What is the real value of what you are doing? What's your life mission? Is it to keep producing five scientific papers a year?"* These are questions for which answers were not readily evident in either Research Group, in terms of a range of different stakeholders participating routinely with their innovation projects. Rather they are the first signs that the rhetoric of public engagement with science (cf. Stilgoe, 2007) is initiating the first steps towards a wider conception of stakeholders in the research enterprise.

Such engagement with a wider range of publics might be interpreted as simply following a particularly contemporary trend. Importantly, my observations suggested there were indications in both research groups that such activities might represent a way to differentiate the offerings of a modern research enterprise in the face of changing innovation policy mandates. In my critique of an Impact Statement associated with a major new grant application at MicrosCo, I discerned 11 different domains of Impact (beyond the academic impacts associated with the sciences of electron microscopy and new materials) identified by the Directors for the work of the enterprise. These were: recognition for a previous investment; new academic collaborations; enabling the study of new material systems; new areas of industrial application; the recognition of UK science; articulating the ultimate beneficiaries; working with small companies; financial gains for collaborators; specific service offerings; creating new academic networks; and improving UK competitiveness. While each of these was associated with a different stream of activity, the large number of them may be read as the Director of MicrosCo testing out different expressions of Impact looking for which ones gain traction with funders. Finding new ways to talk about their impact was not limited to Impact Statements. As the vignette in Chapter 9 about the collaboration with the stained glass window workshop showed, Director A actively sought opportunities to connect with a wider range of stakeholders, and find new ways to tell stories about the work of MicrosCo: "*nanotechnology has been a part of this city for 800 years*".

The need to develop and practice new discursive resources was not limited to experienced strategic leaders, but also evident in the next generation of researchers. In this vein Jack (the senior post-doctoral researcher at ColloidCo), in his application for a major early-career fellowship, was cognisant of the emerging landscape for academics as he sought to articulate his research agenda. This particular scheme required him to articulate an agenda for his science work, and referees feedback from the earlier rounds had requested that he wrote more about his science. And yet he continued on the basis that his agenda would have "*great relevance for industry*" something he "*assumes is not the same for all candidates and this will make me stand out*". Whilst the referees' comments suggested that this was a risky strategy he saw the research leader at ColloidCo as a role model and believed that *"it is a good thing to do this from the start as I think this is what the [funding body] is looking for"*.

Contested Identities and Responsibilities

An important and recurrent theme throughout these findings is the collaboration between both research groups and industrial companies. The core exchange at the heart of these collaborations was that for access to intellectually stimulating and complex materials, the Research Groups were required to deliver solutions to defined commercial challenges. The direct consequence is that opportunities to follow scientifically interesting avenues are limited by the requirement to deliver commercially relevant findings. However, we have seen repeatedly throughout these case studies that many scientists are comfortable in the roles that industrial collaborations require. Mechanisms such as university-industry collaborations and academic entrepreneurships have become institutionalised (cf. Chapter 2), and many scientists have adopted these practices. Indeed, the wide-ranging experience of the research group leaders suggested a conflation of identities: scientist/entrepreneur at ColloidCo and scientist/industrialist at MicrosCo. And at the level of the next generation of research leaders (e.g. Jack, in the example outlined above), collaborations were enacted as if it was part of what modern scientists should do. However, in the context of research groups developing Innovation strategies in a new policy landscape that emphasised impact and responsibility, it was interesting to observe the new personal demands that such policy discourse made on the scientists.

It is to be expected that both partners in any collaboration would feel the uncertainty of impact, with the practical consequence that the responsibility for resolving emerging issues might not always be clear. In the case of ColloidCo, this was shown in relation to the toxicology study discussed in Chapter 10 that was conducted by a third party with whom only the industrial collaborator was in contact. Researchers at ColloidCo worked in the expectation that they would be informed of any significant toxicological findings. Whilst this belief was reasonable, it does not follow that they have no capability, or even responsibility, to contribute to the thinking behind the toxicology study; their research insights might conceivably have a significant impact. Such circumstances suggest that matching professional identity with external interests might be more problematic when there is an expectation of co-responsibility.

In discussion of these issues with research leaders then issues of identity were cast at the level of what science or scientists should do *as a body* in response to a more broadly defined set of demands. Table 16 includes a quotation from the Research Leader at ColloidCo in which he opined that the scientific community needed to do much more to explain themselves to the people that were ultimately paying for the work. He felt that the answer lay not only in the very process of commercialisation and the discipline provided by markets, but in the way innovators spoke about such work outside their professional communities.

Director A at MicrosCo displayed less confidence in existing innovation practices and was more reflective on the challenge of incorporating new voices in the mandate for science-led innovation. Reflecting on his time working for a global chemical company he commented that *"we looked at what happened with the genetically modified foodstuffs. I mean we were just appalled about the loss to the world of a giant opportunity to improve the world food supply, but also the way it was mishandled. So that people – particularly in Europe – got frightened about genetically modified food"*. At one level this quote is reminiscent of the words of Richard Dawkins in the Harvard Business Review article (Dawkins, 2001) that was discussed in the opening chapter of this thesis. However, in light of my observations of the innovation strategy at MicrosCo and the engagement with new stakeholders, an alternative interpretation is suggested. Director A commented that *"people facilitate good things happening, [that] is my belief, and the open process is part of it, and if you're not prepared to indulge in an open process, because you haven't got a clear enough story, you haven't got enough self-confidence"*. The new stories evidenced in his Impact statements and novel projects (e.g. the stained glass project) suggest that MicrosCo's strategic ambitions were made in conjunction with considerations that speak to the values of the scientists and the way this is bound up in their sense of a professional self.

One common motivation in the engagement of both enterprises throughout my time with them (and possibly one of the reasons they agreed to host my study) seemed a desire to explore the uncertainties and opportunities presented by the emerging policy climate for responsible innovation. As Jack (senior post-doc at ColloidCo) commented at the start of my engagement with his Group: *"I think there are ways of integrating them [social and ethical concerns] but it depends on specifically what they are…..we need more opportunities like this so you can discuss this sort of thing as the main problem is lack of interaction."*

PART IV
DISCUSSION

Chapter 12
Responding to Mandates for Research Impact and Responsible Innovation

Research Groups pursuing innovation projects with a focus on economic impact is not new. The academic literature discussed in Chapter 2 has examined this phenomenon in some detail, and the two Research Groups in this PhD project were selected precisely because they were actively working to this agenda. In particular they have experience with the two main innovation mechanisms in this domain: industry-university collaboration and academic entrepreneurship. However, as discussed in Chapter 8, innovation policy as it impinges on the work of university Research Groups has been acquiring a new character. The *Research Impact* agenda in the UK has sought to mainstream the idea that university research should be pursued not merely with economic impact in mind, but in relation to a wider set of societal concerns. Cognate with the *Research Impact* agenda, policies advocating *Responsible Innovation* have emerged in the USA and mainland Europe, and have started to impinge on funding calls from the UK Research Councils. This policy discourse has given impetus to the notion that the work of science-led innovation should be informed by a range of voices (or publics), and not simply those of scientists themselves or those who finance the commercialisation of science. It was evident in my study of the innovation work of ColloidCo and (particularly) MicrosCo, that their leaders not only sensed the changing character of innovation mandates but that they had started to explore new ways to respond. Therefore, my research seeks to contribute to the existing literature in this area (cf Chapter 2) by describing new innovation practices of university research groups, and explaining how those practices are evolving.

To recap, the main question for this PhD study is: *How are emerging innovation policy mandates for societal and economic impact of science influencing the innovation practices of nanotechnology research groups?* This question was framed as one involving a change in organisational capabilities for science-led innovation. This framing then suggested a number of underlying questions: (i) what are the organizational capabilities in science-led innovation? (ii) How are these capabilities changing in response to new innovation mandates? And (iii) what is the role played by the research leaders in effecting such developments in capabilities? This chapter presents answers to these questions by relating the research findings in Part III to the literature on capability change discussed in Chapter 3. The chapter is structured as follows. Firstly, I offer a brief outline of how each of the chapters (7-11) in the Research Findings section has contributed to my emerging understanding of these questions. Each of the three underlying questions is then discussed, in order to provide a comprehensive answer to the overarching question.

My pilot research (Chapter 7) was pursued in order to provide a practical orientation to the field. Already familiar with the public rhetoric of *Impact* and the emerging roles of science (Chapter 1), I gained a more practical appreciation of how this public discourse was influencing the private thoughts of academics across a wide range of disciplines. I learnt that there was a wide range of existing impact-related activity (cf. Abreu *et al.*, 2009), but it was also evident that such activity was yet to be institutionalised into widely-accepted practices. I discerned a development cycle in the sophistication in thinking about impact, and concluded that my own case studies would need to be with research groups who were already sophisticated in their engagement with these agendas. In a similar vein, the framing of innovation capabilities as a life-cycle (cf. Helfat and Peteraf, 2003 and discussed in Chapter 3) received some vindication. Finally, and at a personal level, this pilot work provided my first practical steps in social research, and progressed my transition from management consultant to management researcher.

A review of recent innovation policy as it affects university research groups (Chapter 8) suggested that issues of economic contribution and societal impact do not sit together comfortably. While the former have been largely institutionalised and aligned with long-standing modes of private sector commercialisation, the latter remain aspirational; with mechanisms for voicing concerns seemingly better developed than ones to actually identify courses of action.

Nevertheless, the idea that university research should address a wider set of concerns than the purely economic has attracted increasing attention, and been extended by the discourse of *Responsible Innovation*. UK Policy also continues to place a significant onus on the individual research scientist and his or her research group. Whether it is articulating “Pathways to Impact” in grant applications, or producing “Impact case studies” for REF2014, policy enactments are requiring individual research group leaders to develop responses to a set of expectations from a widening set of constituencies (cf. Chapter 1).

The two research enterprises chosen as case studies displayed an active engagement with innovation mandates for impactful university science; although this was not without its tensions and difficulties in implementation. The case descriptions and their comparison in Chapter 9 indicated the importance of a number of strategic dimensions in the response to policy pronouncements. The two enterprises were at different stages in their development; while ColloidCo was mature, MicrosCo was still in a start-up phase. This meant that their response to the changing environment for university research groups had a different character. The operating models and project portfolios of the two enterprises reflect, in part, their position in a development cycle. The innovation practices at ColloidCo were more established and readily recognisable as those discussed in literature reviewed in Chapter 2. At MicrosCo everything other than the processes of science was new, and their innovation work had a more exploratory feel. At both enterprises, I encountered individual, early-career scientists who were reflecting on the changing context for university science and what it would mean for them. Their research leaders were similarly reflective of the more strategic challenges of sustaining their groups and the scientific communities with which they identified.

It should also be noted that the actual dates I spent with the research groups coincided with different phases of policy enactments. My time with ColloidCo started (May 2009) with the first formal announcements of the requirements for grants to include “impact statements”. By the time I started with MicrosCo (April 2010), the initial publication of the requirements for “Impact Case Studies” in REF 2014 had been made and the consultation of what they should include was underway (and included one of MicrosCo’s sponsoring departments in the Impact case study pilot trials). These different timings necessarily influenced the thoughts and actions of research leaders. Whilst the response of MicrosCo may seem to be more varied and imaginative than that of ColloidCo, it must also be viewed from the position of the timing of data generation within a changing policy landscape.

Chapter 10 concerned the response of individual scientists to the changing landscape of university science and innovation. I sought to explore how these innovation debates played out in the work at the laboratory bench. My approach was one of mutual inquiry, and I experimented with a new method of engagement between social and natural sciences: midstream modulation (Fisher *et al.*, 2006, Fisher, 2007). My extensive interaction with one scientist (a post-doctoral researcher at ColloidCo) gave me a rich understanding of the practical scientific work within a collaborative innovation project with industry; and how this changed in response to cues from the industrial partner.

In its more analytical examination of the strategic challenge of responding to external changes with the reconfiguration of internal resources and practices, Chapter 11 brings together the threads of the earlier findings. In the strategy literature *capabilities* have been used to explain how organisations can adapt to their changing environments (cf. Chapter 3), and so it is the theoretical lens through which the changes in research group organisation have been examined in this thesis. The empirical material from the case research suggests that the emerging policy landscape is manifest for university research groups in two different dimensions. These differences turn on the level (low or high) of uncertainty felt regarding the actions to take and the resulting impact of science-led innovation work. Table 17 summarises the theoretical categories, inductively derived from the case material, which are important in explaining these distinctions. The relationship between these different categories is expressed schematically in Figure 18 and forms the starting point for further discussion in this chapter.

The policy discourse on Research Impact and Responsible Innovation suggests mandates for science-led innovation that have resonances beyond the purely economic. I argue that these changes constitute a shift in the strategic macro-foundations (Hahn, 2003) for science-led innovation, and they are placed at the top of diagram in Figure 18. Whilst the diagram is split in two by the ‘prism’ of the level of *uncertainty of action and impact*, the elements with each half are the same categories inductively derived by the analysis in Chapter 11. This analysis suggested the importance of professional identity for the emerging changes in capabilities for science-led innovation. Chapter 2 included mention of studies that have advanced typologies for scientists who become actively involved in innovation work (e.g. references from chapter 2). Whilst recognising the importance of key individuals in one of the study’s subsidiary questions, my initial framing of my case research took no account of such typologies or their possible relationship to professional identity. My research was framed primarily at the level of organisations and viewed through the frame of strategic *capabilities*. That this organisational level construct and its relationship to the agency of strategic actors has been an area of heated debate within the strategy literature (Winter, 2003, Felin and Foss, 2005, Winter, 2013), served to place any possible initial discussion of identity further into the background. However, it is to these debates that I now turn in order to explain the constitution of an organisational capability for science-led innovation.

Table 17 – Summary of second level aggregate categorisations (cf. Chapter 11)

Second level aggregate category	Low uncertainty of action and impact	High uncertainty of action and impact
Organizational capability	Established capabilities for science-led innovation, including: high-quality science; university-industry collaborations; and search for new opportunities.	Exploratory efforts to develop new capabilities, including: working with a wider variety of stakeholders; and new discursive competencies.
Strategic differentiation	Applying existing capabilities in science and innovation practices in order to gain advantage over peers for resources.	Developing new capabilities in order to differentiate on the basis of responding to the changing character of innovation mandates.
Professional identity	Stable and homogeneous.	Unstable and contested.
Standards of Excellence	Aspiration to achieve known standards of scientific excellence as recognized by professional community.	Development of new practices of science and innovation that are informed by concerns of communities wider than the scientific and industrial.

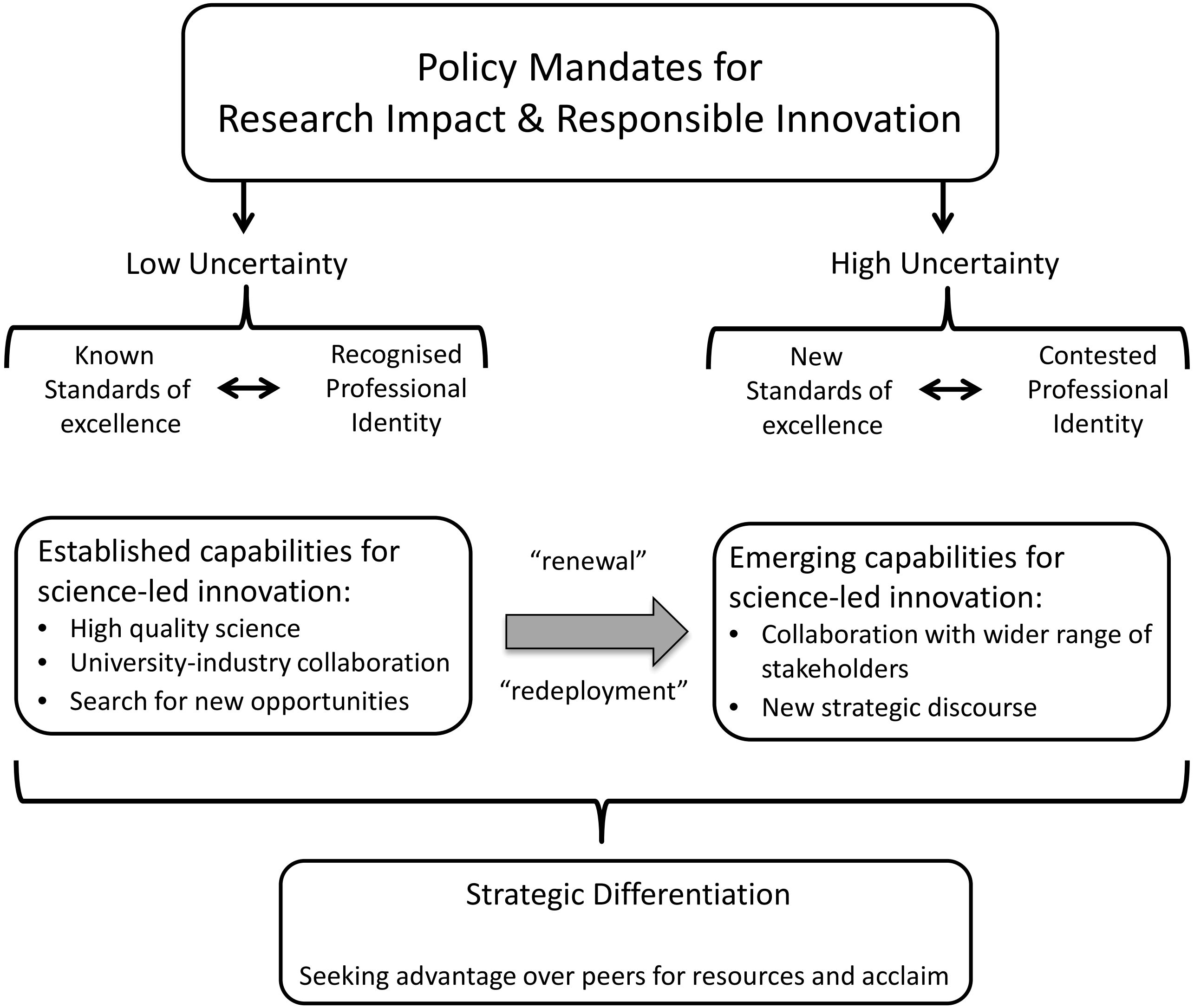

Figure 18 – Schematic showing relationship between second level aggregate categories

What are the foundations for an organizational capability in science-led innovation?

The idea that firm-level behaviour and strategic performance might be profitably explained in terms of concepts at a lower level of aggregation than the organisation or institutional fields has led to debates about the *micro-foundations* of management. These debates have become very topical with two leading journals devoting special editions to the subject in recent years (Felin *et al.*, 2012, Devinney, 2013). Both of these editions are broadly supportive of the prospects for adopting a microfoundational approach to management and organisational studies. The collection within the *Journal of Management Studies* suggested examining the origins of capabilities with a focus on three categories of microfoundations: (1) individuals; (2) processes, and (3) structure (Felin *et al.*, 2012). The symposium in the *Academy of Management Perspectives* (AMP) drew attention to the importance of explaining the mechanisms by which microfoundations are aggregated, and not simply stopping with the identification of those foundations (Devinney, 2013). Dissenting voices were also given space within each edition (Hodgson, 2012, Winter, 2013). In this there was a questioning of the value in adopting a reductionist approach in explanations of management action partly for the theoretical problem with reductionism that “there does not seem to be a guiding principle on where to stop” (Winter, 2013, p.124). However, both dissenting authors also argued on the basis of their reading of the lack of progress over many decades of linking macro and micro economic theory. Existing disagreements (cf. Felin and Foss, 2005, Felin and Foss, 2011) over whether accounts of capabilities that do not include individual behaviour (e.g. Nelson and Winter, 1982) are theoretically satisfactory, was more apparent in the AMP Symposium (but that may simply be because it was the smaller of the two editions, and the main protagonists in this debate loomed larger as a consequence). Nonetheless, there seemed general agreement that microfoundations (irrespective of their ultimate importance for management research) were to be found in ideas relating to individual behaviour.

In seeking an explanation for the construction of capabilities in terms of microfoundations, I take as my starting point Devinney's editorial summary of the AMP Symposium (2013, p.82). Devinney represents (See schematic in Figure 19) management theories existing at the top (i.e. macro) level of the strategy ("S-theories"), a middle (i.e. meso) level of the organisational ("O-theories") and the lowest (i.e. micro) level of the individual. Taking his philosophical lead from Kincaid (1996), and supporting the symposium practical contributions of Barney and Felin (2013), he emphasises the need for theories of aggregation ("A-theories") in order explain how higher levels may be composed of lower ones.

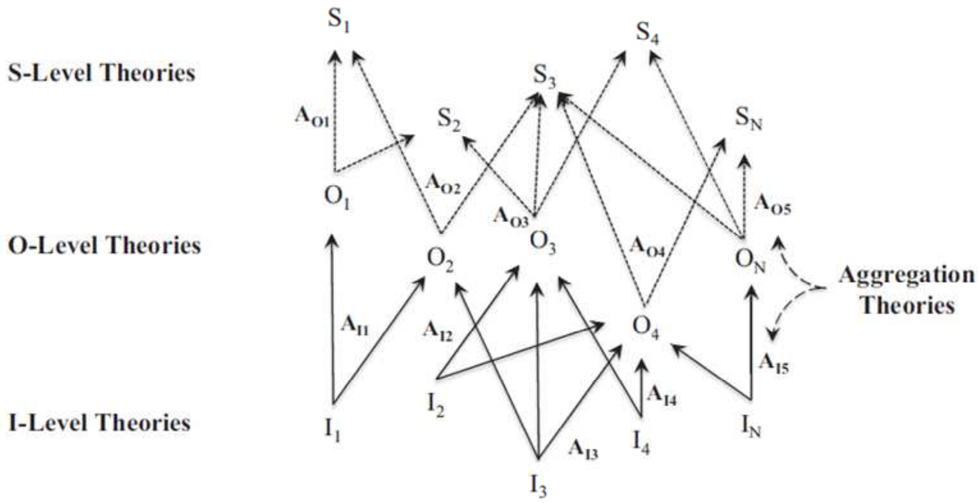

Figure 19 – Microfoundations as I-level, O-level and S-level Theories Bridged (Source: Devinney, 2013, p.83)

In my case research, the *S-theories* might relate to explanations of the how differentiating their offering, would make MicrosCo and ColloidCo more successful in attracting funding and acclaim. However, my research has not been primarily concerned with this level of analysis, and it is not discussed further here. Rather, I have studied an organisational-level response to changes in the innovation policy landscape, and framed this change in terms of the *O-theory* of organisational capabilities. In building an explanation of capability evolution, this section develops both the *I-theories* emerging from my analysis and the way in which they are aggregated (*A-theories*).

These *I-theories* are those relating to key individuals in both research groups that emerged in empirical analysis of Chapters 9-11. In articulating the means of aggregation I interpret the suggestions of Barney and Felin (2013, p.149) in light of the case research findings. The two questions for which an answer is now needed are: what are the microfoundations of capabilities in this case? How are the capabilities, in the aggregate, built from these micro-foundations?

In explaining the founding stage of the capability life-cycle model Helfat and Peteraf (2003) draw attention to the natural endowments (Levinthal and Myatt, 1994) held by individuals. This includes knowledge, skills and experiences of individuals. The empirical work for this thesis uncovered such human capital in every aspect of the work of the research groups. Individuals were recruited to join the groups for having specific *technical knowledge and competencies*. Most commonly these were competencies that aligned closely with the research and innovation work of the groups. At times the recruitment was prompted by an intention to bring a specific new competence to the groups: "*then we'll bring in someone with a whole bunch of different skills and they'll bring a different mix into the group*" [ColloidCo Research Leader]. As illustrated in the conversations with Bob (the postdoctoral researcher whose work was discussed in Chapter 10), individual scientists also had a clear notion of the technical skills and distinctive knowledge they brought to the group. Therefore I posit that individual's *technical knowledge and competencies* constitute a microfoundation for capabilities in science-led innovation.

Prior experience is another aspect of human capital that Helfat and Peteraf note is salient for the founding of capabilities (2003). Research within entrepreneurship studies has shown how experience of different industries gives entrepreneur firmly-held views on what drives performance in different settings (e.g. Baron and Ensley, 2006). The two research leaders in my case studies both had extensive (and different) commercialisation experiences that coloured their approach to innovation. The ColloidCo Research leader had participated in a number of entrepreneurial ventures: commercial experiences he could bring to collaborations with large industrial companies. The Directors at MicrosCo actually had very extensive industrial experience having worked for over 20 years in a large science-based multinational company. These experiences were never far from the mind of Director A. It is no exaggeration to report that I never had a conversation with him that did not include some reminiscence or lesson that he held from his time with the multi-national company. In his case the depth of his experience (Gavetti *et al.*, 2005) shows a degree of entrenchment and possible projection of those experiences (cf. Cornelissen and Clarke, 2010) onto his approach to innovation at MicrosCo. He had already drawn upon some of these experiences (e.g. in drafting the job role of the "Experimental Officer" who was to interact with local technology firms), whilst others were still being expressed as aspirations or opportunities (e.g. the desire to do more work related to energy conservation). This suggests a stock of prior experience that can be drawn up to extend current capabilities as well as develop new ones. Overall I argue that individual's *prior experience* constitute a microfoundation for a capability for science-led innovation.

Beyond elements of human capital there seems general agreement in that microfoundations are also associated with individual behaviour (Felin *et al.*, 2012, p.1359, Devinney, 2013, p.81). In a manifestation of the generic problem with reduction (cf. Kincaid, 1996) this guidance still leaves scope for going deep into psychological explanations of behaviour (as Foss and Lindenburg (2013) do to a degree in the AMP symposium). However, most research stops at a level corresponding to what Barney and Felin describe as "individual-level concepts" borrowed from other disciplines (2013, p.142). Examples of such concepts are "motivation", "learning" and "cognition". These authors argue that simply referencing such concepts will not suffice as an explanation of microfoundations, in the absence of a theory concerning how they aggregate. In this project the construct of *identity* is one example of such 'borrowed individual-level concepts' that emerged from the analysis to inform the microfoundations for science-led innovation capabilities.

Identity has been defined as "the conception which each individual develops in relation to others, of who and what they are" (Watson, 1987, p.195). However, discussion of *professional* identity is seen as problematic by Watson who views the term *professional* as a resource used by spokespersons to advance the status and influence of certain occupations by endowing them with an "elite" label (Watson, 2002). He argues that it is more useful analytically to differentiate between *self-identity* ("being an individual's own notion of self") and *social identity* ("being the notion others have of who and what the individual is") (Watson, 1987, p.195). Whilst I cannot claim to have evidence for any change in the *self-identities* of the research leaders in my case studies, I did encounter their awareness of changes in the cultural or discursive notions of the "scientist": their social identities. The innovation policy discourse presented in this thesis can be seen to have challenged the *social identity* of scientists as experts uniquely equipped to pronounce on what ought to be researched and developed. Such questions should now be opened up to a wider range of voices (cf. Chapters 1 and 8). This contestation of the social identity of scientists has implications for the standards against which their work should be judged. In conditions of low uncertainty, both self and social identity of scientists are stable and the standards of excellence to which they work are known. As the *social* identity of scientists becomes contested then new voices have started to impinge on what constitutes excellence in science-led innovation. This relationship between standards of excellence and professional identity are represented in Figure 18 using double-headed arrows. In this I am suggesting that the influence of professional identity and standards of excellence are mutually informing. Together they represent the *professional cognitive frames* (cf. Cornelissen and Werner, 2014, p.186) of scientists undertaking science-led innovation. In conditions of low uncertainty such frames are clear and guide familiar actions. However, under conditions high uncertainty the established frames represent a less reliable guide for action. The third microfoundation for a capability for science-led innovations is *professional cognitive frames*.

In suggesting future research on microfoundations I repeat the assertion that the context of research groups has the advantage (for preliminary studies of aggregation) of not being as complex (in organizational terms) as private firms. For in this context the influence of individual actors (and in particular the research leaders) is most apparent. A key development aim of these research groups is the creation of independent researchers: something I heard both leaders in the research groups advocate on a number of occasions. The scientists in these groups are expected to act autonomously and pursue their ideas. The extended narrative in chapter 10 of one postdoctoral researcher affirms his ability to change the course of research, even within the context of a project that was tightly controlled by an industrial partner. Therefore the importance of the *agency* of the scientist-innovators is something that drew my attention throughout the study It is another of Barney and Felin's 'borrowed individual-level concepts' (2013, p.142), and it is germane to any discussion of microfoundations of capabilities (cf. Felin *et al.*, 2012, p.1366).

Emirbayer and Mische (1998) theorise agency as comprising three dimensions: iterative; projective; and evaluative. Iterational agency involves the selective repetition of patterns of action that become stabilised into standard collective activities or "routines". Such "routines" are viewed by some authors as constituting "first order" organisational capabilities (Winter, 2003). However, such routines are organisational-level phenomena, and "first-order" should not be confused with the microfoundations being discussed in this chapter. Projective agency is concerned with future possibilities and entails the creative recombination of existing patterns of action to achieve new goals. Such agency may manifest in the strategic search for distant opportunities (Bhardway *et al.*, 2006) or the strategic sensing of change (Teece, 2007). Finally, evaluative agency concerns the capacity to make judgements that speak to the values and duties of the actors. Emirbayer and Mische note that such judgements are often manifest in newly emerging, ambiguous situations (1998, p.971).

In studying the ways in which research groups are responding to new mandates for science-led innovation, my case studies suggest that the immediate reaction is to ensure innovation work is free of negative connotations for the environment and human health (Swierstra and Jelsma, 2006). Within early conversations in both groups I heard the importance of complying with regulatory standards as evidence of responsibility (cf. Shatkin and North, 2010). However, it has been argued that there can be significant delays between the launch of a new technology and an appreciation of its impact on health and the environment (Owen *et al.*, 2009). Whilst acknowledging the response to this challenge at the institutional level of science in the guise of risk management (Owen *et al.*, 2009), technology assessment (Rip *et al.*, 1995, Rip, 2005) and new forms of governance (Guston and Sarewitz, 2002), this study has focussed on the organisational level of research groups and the individual level of their leaders. In deciding on this focus I am suggesting that the uncertainty inherent within the dynamics of technological innovation (Tushman and Anderson, 1986) cannot be resolved only with respect to institutional-level mechanisms. It is also important to understand the response of innovation agents embedded within a network of social interactions (Weaver, 2006). Therefore, the discussion of the microfoundations of the innovation capabilities in this case must explain the relationship between iterative, projective and evaluative agency. Furthermore, distinguishing between the two key contextual categories of low and high uncertainty may shed light on the relationship between agency and the evolution in those capabilities.

Low uncertainty and persistence of professional identity

My engagement with the two research enterprises shows that challenges associated with responsible innovation and the societal impact of research are starting to be recognised as an issue. However the response of such groups to these mandates has not been so disruptive as to start the deinstitutionalisation of existing values and practices (Greenwood *et al.*, 2002). Rather than a more significant change in innovation practices, I observed that the initial response to emerging policy mandates was one of researchers emphasising current approaches towards the commercialisation of science. The economic impact of such approaches remain part of the policy landscape. The associated capabilities for science-led innovation are familiar and may be viewed as institutionalised within universities (cf. Chapter 2). In such circumstances I observed that the everyday activities of science-led innovation have a low degree of uncertainty, in the sense that innovation practices were approached and executed in line with accepted standards of excellence.

In these circumstances questions of responsibility were perceived as uncomplicated. The leadership evident within both research enterprises was consistent with the view that established capabilities for science-led innovation were sufficient to meet renewed demands for an economic impact. In this vein responsibility in innovation can be understood as extra efforts to sustain established capabilities. Whether by means of either the reiteration of existing routines, their projective development, or the values underpinning judgements on new projects to pursue, the strengthening of existing capabilities for science-led innovation may be read as an act of responsible innovation. This congruency between iterative, projective and evaluative agency on the part of research leaders is guided by a stable and homogeneous sense of professional identity (both self and social) and accepted standards of excellence. Aspiring to, and delivering on, such standards is highly likely to be perceived as responsible behaviour by all research groups' members.

Professional identity issues that touch upon collaboration with industry (one of the elements of a capability for science-led innovation in conditions of low uncertainty) are worth noting. Whilst such collaborations have long been a feature of the innovation landscape in universities (cf Chapter 2), some notable scholars have argued that such collaborations do not make for excellence in science (Dosi *et al.*, 2006). University scientists may share the same notions of professional identity as their industrial collaborators (Murray, 2002, Kellogg *et al.*, 2006), but organizational identities (Ashforth *et al.*, 2008) resulting from different institutional logics across the university-industry divide still make for tensions. Despite these contradictory tensions the case studies suggest reasons why collaborative innovation still merits being viewed as associated with the pursuit of excellence.

In conditions of low uncertainty the case data revealed a strong sense of a division of innovation labour aligned with each collaborator's strengths and desired benefits (cf Ibarra et al, 2005). Such divisions do not work by reducing the strength or stability of professional identity, nor the way in which it guides agency towards the pursuit of excellence. What is changed in such circumstances is the homogeneity of professional identity. With the minor exception of administrative frustrations, then participation in industrial collaborations appeared unproblematic to the scientists within each research enterprise. Indeed, the experience of Bob (the ColloidCo post-doc and detailed in Chapter 10) suggested that his experience of collaborating with industry made him more aware of his identity (and the contribution to innovation projects) as a scientist. More generally, there was no evidence to suggest that members of either research group had undergone a process of professional adaptation (Ibarra, 1999), and sought to better align themselves to their work environment. In short, such collaborations left the professional identities of scientists as uncontested. It was the judgement of the research leaders (in a show of projective and evaluative agency) that industry collaborations could be a source of excellence. With such endorsements the increased heterogeneity of identities resulting from different institutional logics proved unproblematic, and allowed industry-university collaborative practices to feature as a constitutive element in the pursuit of excellence in science-led innovation.

High Uncertainty and the contestation of professional identity

Even while recognising that research groups have limited their initial response to calls for research impact and responsible innovation, it was evident that research leaders and senior members sensed a change in emerging policy mandates. Growing policy calls for science-led innovation to be increasingly informed by a wider set of concerns than the purely economic (cf Chapters 1 & 8) is changing the strategic outlook of research leaders. The message is that science-led innovation may not simply be understood as involving a familiar division of innovation labour between known and identifiable partners. The emerging institutional field of science-led innovation comprises a varied collection of voices and an absence of clearly articulated positions. The challenge of societal impact or responsible innovation has been capable of mobilising multiple communities not normally associated with innovation. The result on the field of science-led innovation has been to make it, in part, more uncertain and less institutionalised (Battilana *et al.*, 2009). The case data suggest that members of research groups (and especially research leaders already engaged with the agenda of economic impact) experience tensions when they sense high uncertainty between action and impact. This may be explained by recourse to ideas relating to agency, professional identity and standards of excellence

In conditions of high uncertainty innovation actors cannot rely on iterative agency. In other words institutionalised practices and familiar innovation mechanisms (cf Chapter 2) are not sufficient for guiding decision-making. Furthermore, the discourse of responsibility in innovation challenges established notions of professional identity and standards of excellence. Responsible Innovation mandates bear witness to changing perceptions of both of what it means to be a professional scientist (i.e. the *social* identity is contested), and what constitutes good science-led innovation (i.e. standards of excellence). And yet the persistence of a strong *self* identity of scientists (cf. Scott, 2008) risks entrenchment of epistemic cultures (Knorr-Cetina, 1999) and inertia in the face of change (Brown and Starkey, 2000). The very ambiguity of much of the responsible innovation and Impact discourse means that new, clearly articulated normative standards of excellence are not forthcoming. Such standards are being socially constructed by a discourse involving a shifting collection of constituencies with identities that differ markedly from scientists. In such circumstances it is inevitable that the professional identities of scientists become contested and the possibility of professional adaptation (Ibarra, 1999) emerges.

To be clear, these two case studies do not provide evidence for a major shift in the professional identities of scientists (cf Pratt et al, 2006). Neither did I observe processes such as conflict, resistance and subversion (Doolin, 2002) in the relationship between the groups and external stakeholders. What I did observe was an increasing awareness on the part of research leaders (both established and the ‘next generation’) of innovation policy mandates that are redefining their roles vis-à-vis a wider society of interested parties. These changes were sensed even as they were not fully understood, or perhaps understandable given the ambiguity of discourse relating to emerging standards of excellence. The crucial observation was that research leaders were willing to respond and experiment with new approaches to their innovation work.

In both case studies I clearly observed the development of new organisational capabilities in response to innovation policy mandates for greater societal impact and responsible innovation. The very contestation of profession identity and uncertainty about the standards expected of science-led innovation mean that established foundations for existing capabilities offer little guidance in the development of new capabilities. *Iterative* agency is less relevant. Rather innovation actors come to rely on their *projective* and *evaluative* agency. The former is strategic in character, involving the recombination or redeployment of resources, or drawing upon previously unused experiences, in searches for new opportunities. It contains an imaginative dimension as connections are constructed between possible capabilities and the aspirations inherent in much of this new policy discourse (cf. Chapter 1 and 8). The latter suggests a moral dimension to this whole enterprise, and serves to draw attention to the *evaluative* agency that I could hear in the research leaders' conversations about the type of projects they would like to pursue if they could secure the funding. The judgment of research leaders within this ambiguous emerging policy landscape is both strategic (cf projective agency) and moral (cf evaluative agency).

In summary, my analysis of research findings suggests five capabilities for science-led innovation in the changing policy landscape confronting university nanotechnology research groups. I have characterised these as "established capabilities" (which are evident in conditions of low uncertainty) and "emerging capabilities" (evident in conditions of high uncertainty), viz:

Established Capabilities (in conditions of low uncertainty)

- High quality science
- University-industry collaboration
- Search for new opportunities

Emerging Capabilities (in conditions of high uncertainty)

- Working with a wider variety of stakeholders
- New discursive competencies

I have argued so far in this chapter that these capabilities are constituted of a number of microfoundations, viz:

Microfoundations

- Individual's prior experiences
- Technical knowledge and competencies
- Professional cognitive frames
- Human agency

The challenge remains to articulate how these microfoundations are aggregated to give the empirically-derived organisational capabilities.

Aggregation of Microfoundations – areas for future research

For Barney and Felin (2013), identifying the microfoundations of capabilities is insufficient explanation: it is also necessary to explain how they aggregate to give capabilities. However, whilst identifying a number of possible mechanisms of aggregation they acknowledge that much work is needed: "the question of how behaviour "scales" or aggregates is a central one – scarcely addressed in the extant organizational literature" (Barney and Felin, 2013, p.146). This current PhD research was not designed to specifically elucidate the mechanisms of aggregation of the microfoundations of innovation capabilities. This would require data generation more focused on key innovation activities and a more fine grained analysis of that data than has been possible here. The aim of this study is to explore an organizational level response to emerging changes in innovation policy. However, the rich case data generated in this study together with Barney and Felin's suggestions for aggregation mechanisms (*ibid*, p.145), offers hints that might guide future, more fine-grained research.

In their AMP Symposium paper Barney and Felin divide aggregation mechanisms for microfoundations into two types (2013, p.145-148): "Additive", and "Complex". The features of human capital that I consider microfoundational in my cases (i.e. *prior experience* and *technical knowledge & competences*) are suggested by Barney and Felin to aggregate in simple additive mechanisms. They draw attention to mechanisms by which individuals decide to join organisations that align with their interests and skills. These mechanisms include the attraction-selection-attrition model of Schneider (1987), or theories of *homophily* (McPherson *et al.*, 2001) that examine how similar individuals decide to interact. These mechanisms are pertinent for the way in which scientists had been recruited into my case study groups. From the perspective of the ColloidCo research leader quoted earlier in this chapter, the building of scientific and innovation capability involves, in part, the conscious recruitment of people with known experiences, knowledge and competences. From the perspective of individual scientists (discussed in detail for one scientist in chapter 10), then the challenge is finding a contract with a research group with whose interests they can align.

Simple additive mechanisms are unlikely to be sufficient to explain the aggregation of behavioural microfoundations to capabilities given the complexity of social interactions in organizations: even those as simple as my case studies. They do not seem relevant when considering the complexity inherent in the *professional cognitive frames* or individual *agency* that I argue constitute other microfoundations of innovation capabilities. Recognising the influence of organisational complexity Barney and Felin suggest the notion of "emergence" is helpful in identifying those "collective outcomes that are surprising and not necessarily reducible to constituent individuals" (2013, p.147). Their suggestion is for researchers to examine features of organisational design and structure that determine or facilitate social interaction between actors. Examining my experience of being embedded within research groups in light of this advice suggests a fruitful avenue of future research would be a fine-grained analysis of innovation project meetings. My own access to the research groups did not include their industrial collaborators, to the extent of being present during such project meetings. Capturing and analysing the dialogue from such meetings may offer significant insight into the aggregation of microfoundations of innovation capabilities.

My experience of participating in research group meetings (which were largely concerned with scientific rather than innovation projects) leads me to suggest there may be an interesting analogy between the way in which science itself can be explained as socially-constructed, and the way in which microfoundations build to give capabilities in science-led innovation. A detailed examination of what happens in research laboratories prompted a major thread of research during the 1970s and 1980s in Science and Technology Studies (Sismondo, 2010, chapter 10). These studies were largely concerned with the nature of scientific knowledge, or more prosaically, *how facts are made*. Although there were no straightforward answers to this question, it is something of a given for STS scholars (if not the scientists who they study – cf "science wars" referred to briefly in Chapter 1) that scientific knowledge is constructed in the social and material spaces in which scientists work. In this vein then during research group meetings I observed: the presentation of data and the critique of its quality; the challenging of logics connecting evidence and claims; examination of underlying warrants and assumptions. I also witnessed similar micro-processes of argumentation (cf. Toulmin, 1958) in routine conversations between supervisors and students, and between peers. The way in which research groups organise and enact their internal social interactions is informed by these micro-processes of argument construction. I speculate the same happens in their social interactions with external collaborators, as these micro-processes appear deeply ingrained in the way the scientists interact with others during the course of their work. When Director A at MicrosCo showed me his draft Impact case study (see Chapter 9) he asked me to critique his ideas and their logic. By analogy to the way these social micro-processes of argumentation are brought to bear on the construction of scientific knowledge, I speculate that they are significant in the way that knowledge of how *to do* collaborative innovation or opportunity search is arrived at collectively. An interesting area for future microfoundations research would involve the fine-grain analysis (e.g. by means of discourse analysis) of a single manifestation of science-led innovation capabilities (e.g. a single industrial collaboration project).

A summary of the foregoing discussion in terms of Devinney's of microfoundational levels (Devinney, 2013) is shown schematically in Figure 20.

O-level	From empirical research	Capabilities for Science-led Innovation	High quality science. University-industry collaborations. Search for new opportunities. Wider stakeholder collaborations. New discursive competencies
A-level	Suggested areas for future research following Barney & Felin (2013)	Aggregation theories	Selective addition of human capital. Organisational & social interactions through vehicle of argument construction.
I-level	Conceptual discussion of micro-foundations literature in light of case findings	Micro-foundations	Individual prior experiences. Technical knowledge & competences. Professional cognitive frames. Human agency.

Figure 20 – Microfoundational levels for capabilities in science-led innovation (cf. Devinney, 2013)

Whilst explicitly addressing subsidiary question (i) the discussion so far has also touched on issues of change and the role of research leaders (subsidiary questions (ii) and (iii) respectively) to the extent examining the agentic responses to different degrees of uncertainty surrounding innovation actions and their impact. The dynamics of capability development are explored further in the next section by revisiting the life-model of Helfat and Peteraf (2003) introduced in capabilities literature review in Chapter 3.

How are capabilities changing in response to new innovation mandates?

The life-cycle model of capability development presented by Helfat and Peteraf (2003) is generic in nature. Their aim is to provide “a frame within which subsequent research can examine the process that shape the [capability life-cycle] in greater detail” (Helfat and Peteraf, 2003, p. 1000). The case studies provide evidence for research groups experimenting with two related organizational capabilities. This work of exploration may be explained with reference to Helfat and Peteraf’s “6 R’s” of capability development (see Figure 3 in this thesis). However, the nature of the change being experienced by these research groups cannot be expressed as neatly as the one caused by the “branching point” in this diagram. In Figure 3 the branching point appears abrupt and change in capabilities immediate. The changes under consideration in these case studies are slow to emerge. In other situations, such as a private technology company experiencing a change in market demands, then the “branching points” might indeed be more abrupt. My time with both case enterprises coincided with an important, but slowly emerging, shift in the policy landscape in which they operate. I observed their response to that shift in terms of a number of new innovation activities with which they were experimenting, and I have characterised this as the development of emerging capabilities.

However, I also noted that not all the innovation work of these groups is subject to change. Much of their work operates in a context I label as “low uncertainty of action & impact”. Here their established innovation activities still pertain. As I noted in the analysis of research finding in chapter 11, the established capabilities have not all become redundant as a result of policy changes. Innovation capabilities of *high quality science*, *university-industry collaboration* and *search for new opportunities* are still required. In short there is not in the case of nanotechnology research groups, one single trajectory of capability development. Helfat and Peteraf’s model (Figure 3) in these cases might be better re-drawn as Figure 21.

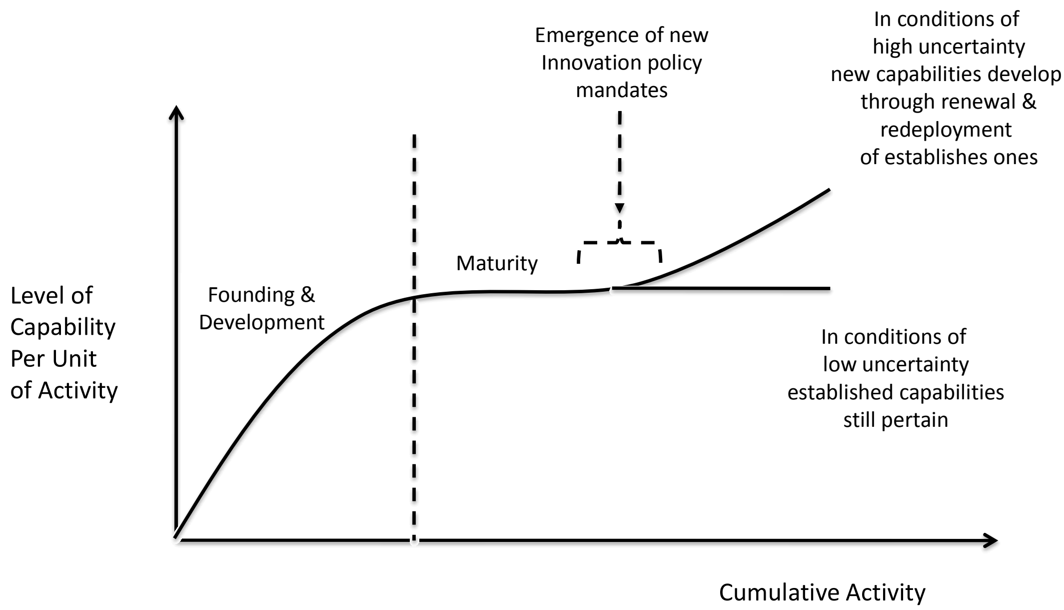

Figure 21 - Schematic representation of evolution of capabilities for science-led innovation following emergence of new innovation policy mandates (after Helfat and Peteraf, 2003, p.1005)

Helfat and Peteraf's life-cycle idea serves to remind that these *new* capabilities (in the trajectory of high uncertainty) emerge out of existing capabilities. In this manner, what I have described hitherto as "emerging capabilities" might also be thought of as new *attributes* to established capabilities. Which of these two modes of labelling is adopted seems a moot point. In this thesis I have adopted the label "emerging capabilities" in order to draw attention to the new innovation work that I observed. In Helfat and Peteraf's terms (2003) the engagement with new stakeholders in both research groups can be explained as a *redeployment* of the established capability to collaborate with industrial companies. Such new collaborations contained both established and unfamiliar elements. In MicrosCo's collaboration with the stained glass workshop one can see the usual division of labour evident in industrial collaborations. The stained glass experts select and define the windows to work with, and are responsible for placing the results within the wider realm of art heritage interests. MicrosCo certainly delivers a technical result, but theirs is not simply the work of an analytical service. Indeed this service is merely a means to a greater end, which is announcing their arrival on a civic stage. What such participation will involve was still unclear during my time with the group: this whole enterprise had an exploratory character. The research leader's actions in developing this new type of collaboration were animated by a sense that the role of the scientist was becoming more public, and so MicrosCo had to have a presence in a public arena. I observed the same movement towards dialogues with publics at ColloidCo, but in this case it was the research students who were making the move. All such new modes of engagement may be understood as efforts to reduce uncertainties of actions and impacts in the highly complex field of science-led innovation.

The development of new science-led innovation narratives can be seen as a *renewal* of the discursive resources they already have at their command to both communicate the quality of their science and search for new opportunities. Creating a discursive competence in order to connect with new constituents is consistent with the idea organisational arenas are actually discursive spaces where participants must become involved in the consumption, production and distribution of discourse (Hardy and Maguire, 2010). My time with MicrosCo coincided with the time scientists in the UK were starting to produce to contemplate the "Impact Case Studies" that would be needed in REF 2014 (cf. Chapter 8). Conscious that no clear models for such case studies were available, MicrosCo's own efforts in this regard involved experimenting with multiple new narratives of impact. In this manner they were behaving as *political actors* (Scherer and Palazzo, 2007) participating in the political process of the construction of new norms. I had been at ColloidCo one year earlier exactly when the need for grant applications to contain "Impact Statements" had just been announced. Although this agenda was perhaps too new to expect the level of narrative invention I observed at MicrosCo, it is interesting to remark that, once again, it was the next generation of scientists who were the first to respond. In this vein the determination of Jack (the senior postdoctoral researcher, then submitting an application for a major early-career research award) to pursue a narrative of impact despite feedback of reviewers to the contrary is revealing: maybe his own sense of the emerging landscape for science-led innovation was more prescient that that of his more senior reviewers[6]. In both case studies it is interesting to note that the developments of new narratives for their work is directed towards the strategic differentiation of the groups, and gaining advantage over their rivals for new resources.

What remains unexplained from the capability life-cycle model is the generative mechanisms underlying these changes. In doing so, I continue to offer an explanation at the level of the microfoundations of these capabilities. As a prelude to examining the underlying (microfoundational) mechanisms of *how* these science-led innovations have changed, it is useful to first re-state *why* they have changed in the same terms. Whilst a high level reason for the changed could be expressed as ‘in response to new innovation policy mandates’, it is instructive to emphasise the impact of such macro-level changes on the micro-level foundations under discussion in this chapter. Thus, I have argued that a new policy discourse of Responsible Innovation and Research Impact has given rise to a contested professional identity for scientists and disputed standards of excellence. Taking these two concepts as constituent of the *professional cognitive frames* of scientists, I posit that the it is the destabilisation of these frames that has provided the trigger for new innovation actions. Furthermore, the motivation to embed these new collective actions as emerging capabilities is motivated by a desire to differentiate the groups from their peers: and thereby win in the competition for innovation resources.

Piecing together the threads of this discussion suggests that the generative mechanism for capability development might be found at the level of changes to their microfoundations. This mechanism might be expressed in simple terms as involving: a change in macro environment (emerging policy discourse) impacts microfoundations of capabilities (professional cognitive frames); these microfoundations

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[6] Three years after my time with ColloidCo I learnt that Jack had been successful in gaining his own grant funding, and had also gained a full-time lecturing position at the university.

aggregate as before, but because they are different, then the aggregation now gives new capabilities. Thus what we observe as an organisational level of change actually results from the crucial change happening at the microfoundational level. This explanation is intriguing in that it stands in contrast to explanations of capability change that invoke *higher order* dynamic capabilities acting upon organisational capabilities (Teece, 1997). However, the explanation also has clear weaknesses. Not least of these is that the theories necessary to explain how microfoundations aggregate into organisational-level capabilities remain uncertain. Additionally, the relationship between the macro-level (e.g. policy environment) and the micro-level (e.g. behaviour of individual scientists) does not operate in the simple one-way linear relationship expressed by this mechanism. In the AMP symposium on microfoundations Barney and Felin (2013) emphasise the interrelationship between macro-level and micro-level phenomena. They note that while the whole microfoundations enterprise makes a "methodological point about the power of looking at lower-level constituent units when explaining higher levels of analysis", there must also be "room within the microfoundations program of research to study and understand…macro to micro causation" (Barney and Felin, 2013, p.144). In the cases under consideration here, then whilst scientists were in the position of having to respond to new innovation policy mandates, future research might examine how that (individual-level and organisational-level) response informs the development of future (macro-level) policy change. Nonetheless, the possible that organisational-level capability developments may result from changes the microfoundations of those capabilities, adds impetus to research efforts to elucidate aggregation theories.

In explanations of the process of capability development, then the strategy literature is dominated by the concept of *dynamic capabilities* (as discussed in chapter 3). I did not use this concept to frame my research as (through application of *Ockham's razor*), the dynamic view of organisational capabilities offered by Helfat and Peteraf's life-cycles (2003) seemed sufficient. Whilst the issues at stake in the public discourse of emerging technologies suggested major changes were afoot (cf. Chapter 1), my exposure to university scientists doing innovation (in the literature of Chapter 2 or the pilot research in Chapter 7) gave no indication of a rapid reorientation of their innovation activities. Nor did I encounter anything from those sources that let me to believe that a *special* capability was at play effecting change. Nothing has disavowed me of this original choice through the course of the research project. The capability changes observed in my cases are incremental in nature, and may be explained by the evolutionary processes sketched out by Helfat and Peteraf's life-cycle model. The quantum of change effected may prove to be profound if the rhetoric of the responsible innovation discourse (cf Chapters 1 and 8) is taken at face value, but the pace of that is not fast compared to the contexts in which *dynamic capabilities* are invoked (cf. Chapter 3).

Limitations of the Research

In its reliance on a small number of cases the findings of this research inevitably face the challenge that they may have limited generalizability. Nonetheless, there have been features of the analysis and discussion that suggest the findings are not completely contingent on the circumstances of ColloidCo and MicrosCo. For instance, whilst nanotechnology was selected as the emerging technology of choice for this case research, none of the explanations offered have been peculiar to that science. Similarly in pursuing generative mechanisms at the micro-level of the skills, general technical competencies and professional identity of scientists, suggests associated generalizable contexts. And so this research may offer insights into other specific situations in which the identities of innovators are challenged in some manner.

It is also a moot point as to whether this study has been a case of responsible *research* or responsible *innovation*. My data generation took places largely amongst people directly connected to ColloidCo and MicrosCo, with only a few interviews taking place with those involved in commercialisation stages of the innovation process. However, it has been my argument that enterprises such as MicrosCo and ColloidCo operate at the interface between science and innovation, and success at making this transition is built upon a distinctive set of capabilities (which I have labelled science-led innovation capabilities in order to convey that they exist on the cusp between science and innovation).

Implications for Future Research

There are a number of interesting avenues for future research that are suggested by this study. The most notable of these concerns a general move *downstream* in order to encompass the commercialization phase of the innovation process, and the work of commercial firms. This is a shift in which research interests may be closely aligned with further developments in innovation policy and practice. The latter are discussed at the end of chapter 13, and so I limit myself here to related ideas for new research that develops our understanding of how ideas from the responsible innovation discourse are manifest in the innovation routines of private firms.

There is some evidence that this shift toward the downstream of innovation has started. Two books have been published recently in which the responsible innovation debates are starting to widen to include commercial firms. With the benefits of an edited volume Owen, Bessant and Heinz (2013) are able to present diverse perspectives of the contemporary prospects for the idea of Responsible Innovation. In relation to the challenges face by firms innovating in the 21st century I note that Bessant argues that the "key to long term innovation management success is to build *dynamic capability*" (Owen *et al.*, 2013, p.3). In positioning their conception of responsible innovation in relation to business ethics and corporate social responsibility, Pavie, Scholten and Carthy (2014) place their examination of responsible innovation more specifically within the for-profit business world. However, many of their case studies seem to pre-date the responsible innovation discourse (e.g. Their re-telling of the Ford Pinto case study – Pavie et al., 2014, p.48) whilst some of their other cases are of firms one does not immediately associate with responsible innovation (e.g. the Starbucks case study – Pavie et al., 2014, p.151). On the technological terms in which the responsible innovation discourse has been described in this PhD (cf. Chapter 8) then the first studies in commercial firms have only appeared in recent years (e.g. Flipse *et al.*, 2013). These studies involve ethnographic and collaborative methods appropriate to an area for which either normative guidance or research insight is only just emerging (cf. Flipse *et al.*, 2014). More studies with comparable methods in different technological settings are required to build our understanding of how private firms integrate notions of responsibility into their innovation routines.

The ideas explored in this chapter touching upon the microfoundations of organisational capabilities merit further empirical investigation. One research avenue could be to consider single university-industry collaboration in some detail; capturing the interactions between partners in reports, observations and transcripts of interactions between the collaborators. Taking the current PhD study as a starting point, the examination of the microfoundations of a capability for such collaborations might be extended to include the industrial firm. Fine-grained analysis of the data (e.g. by means of discourse analysis) should be pursued with the aim of building theory for the aggregation of microfoundations.

Finally, although not necessarily purely research-related, one interesting extension of this PhD study would be to make use of the midstream modulation protocol in more conventional managerial arenas. Whilst this protocol has been used in this study to elicit reflexive awareness of societal issues in relation to nanotechnology innovation, an analogous goal is the aim of many management development initiatives. There are well established ideas in the field of management learning that argue for the promotion of greater reflection (e.g. Schon, 1983) and critical reflexivity (e.g. Cunliffe, 2004). More recently there has been the imaginative suggestion that reflexive engagements between academics and practitioners, might provide a mechanism through which management research becomes more relevant (Paton *et al.*, 2014). Whether in teaching or research environments, the prospects for using the midstream modulation protocol with managers merits investigation.

Conclusion

The main contributions to knowledge from this thesis have been made with respect to the literatures touching on the role of universities in the contemporary innovation landscape (Chapter 2), the dynamics of organisational capability change (Chapter 3), along with the emerging literature on Responsible Innovation (Chapter 8).

Regarding the contribution university research groups make to contemporary technology innovation, then the rich case studies in this thesis speak to calls for more detailed accounts of the dynamics of such research (e.g. Perkmann and Walsh, 2007); as well as those requiring an examination of the reflexivity of scientists (e.g. Hessels and van Lente, 2008). More significantly, this research has contributed to understanding how research groups are working to respond to a wider range of contemporary concerns than purely the economic impact of their research. Finally, offering a strong theoretical framing (in this case using *organisational capabilities*) is distinctive in a literature that has been described as “atheoretical” (Rothaermel *et al.*, 2007, p.706), and comprises “predominantly phenomenon-focussed studies” (Perkmann *et al.*, 2013, p.425).

The organisational capabilities literature is complex, and whilst overwhelmingly concerned with the strategies of private firms, a study of the strategic work of university research groups may provide interesting relief. This PhD research contributes to our knowledge on the dynamics of capability development, by revealing some of the mechanistic features underlying the life-cycle model of Helfat and Peteraf (2003). The qualitative examination of practices within a simple organisational system (i.e. a university research group) contributes to contemporary interests in the microfoundations of capabilities (e.g. Devinney, 2013).

An increased scholarly interest in Responsible Innovation (rather than only an interest from policy makers) has been evident during the seven years of this PhD study[7]. This PhD research contributes a perspective from management and organisational studies, to a literature that is still dominated by STS scholars. In the main academic conclusion to the thesis, this research has shown the importance of managerial agency for sustaining established capabilities and developing new capabilities that can lead to responsible innovation. The analysis of empirical case material revealed the importance of the level of uncertainty that exists for innovation actors in connecting their actions to impacts. In conditions of low uncertainty, then a strong, stable professional identity allied to known standards of excellence provides a sure guide of action. The destabilisation of such identity and standards has been a consequence of contemporary debates about the role of science in society. In such circumstances these case studies reveal that scientists, in a more forward-looking and reflective display of managerial agency, have engaged in the development of new capabilities that allow their work to speak to a wider constituency of interests.

[7] An academic journal dedicated to this topic, the *Journal of Responsible Innovation*, published its first issue in early 2014.

Chapter 13
Towards a Contribution to Practice and Policy

Whilst it was not a primary objective of this PhD research to make a contribution to practical knowledge (cf. Argyris, 2005), a few opportunities presented themselves to do so, and these are described briefly in this chapter. Having a business school academic (and one with experience of innovation management and management consultancy) in their presence seemed to prompt people in both research groups to ask for general management advice. To the extent that this did not touch on the substantive content of my research I was happy to oblige: it seemed a reasonable *quid pro quo* given the extensive access they were allowing me. Nevertheless, in keeping with my methodological commitments to foster reflection, I always fell short of presenting outright recommendations, and rather gave them ‘something to think about’. For example, at ColloidCo at the behest of Jack (senior postdoc within the group) I ran a workshop on the subject of *creative climates* (based upon Hunter *et al.*, 2007). In this manner I challenged the researchers to think about those things under their control that contributed to the creative climate within the group itself. In the days after the workshop, they rearranged their offices to create a small space where they could have coffee together. It was prompted by *their* reflections that they rarely made time during the working day to sit and talk with each other.

Whilst suggesting that my workshop had a small impact, the subject of creative climates is not part of my research, and so I will not elaborate further on this incident here. The possible implications for the relevance of the management *researcher* rather than his or her management *research* are discussed in the next chapter. In a similar vein discussion of the contribution to practice through the exercise of the midstream modulation protocol (presented in Chapter 10) will be discussed in Chapter 14. In the current chapter I will describe two examples that presented themselves during the course of the research to make a contribution outside of my two case research groups. The first of these involved participating in a workshop for science policy makers in Washington DC, and the second concerned producing materials for the ethics training of young engineers.

Through my membership of the network of researchers participating in the STIR project (described in Chapter 6) I was invited to participate in a workshop to discuss the findings of that project with US science policy makers. Such events seem to be a usual feature of large projects funded by the National Science Foundation: tax dollars got spent on this project and so what had the taxpayer got in return. Taking place at the Woodrow Wilson International Center for Scholars in Washington DC, the workshop combined paired presentations (social science researcher and a natural science researcher for one of their case study groups) and open discussion of implications. Unfortunately, an urgent last minute departmental commitment for the research leader at ColloidCo prevented him from joining me and so my own presentation did not have the benefit of a formal response from a natural scientist with whom I had engaged.

My own presentation was in essence the research findings presented in Chapter 10, along with a few reflections on using the midstream modulation protocol. The presentation was well received, with its shift in direction of a research agenda being particularly noteworthy. Indeed, of all the presentations at that workshop from STIR researchers mine was the only one which concerned a decision that brought about a change in the research being conducted. Other changes were noted but these concerned the practicalities of laboratory work, e.g. health and safety matters or the waste disposal route used by university labs. As a person experienced in innovation in an industrial setting, I confess to being less concerned with the ability of protocol's use being able to influence such everyday change. Practical matters such as these are resolved by simple innovation routines in an industrial setting, and do not require greater reflexive awareness of young scientists. Nonetheless there remained a general interest amongst the three policy managers from the National Science Foundation in the STIR experiment.

The plan agreed in discussion at the end of the workshop was that Erik Fisher would (in addition to other project reporting obligations) combine the work of the STIR researchers into a “white paper” (in the UK we might call this by another colour: a “green paper”). This paper would be submitted to the NSF participants in the workshop and a decision taken on how best to then progress the policy debates. As events played out, this paper never got written, and in personal communication with Erik he acknowledged that other (university department) pressures to produce papers for academic journals took precedence. The STIR project is still a part of the policy discourse in the USA (Presidential_Commission, 2014), but the influence of one study (mine) amongst many is impossible to discern.

A second opportunity for the research to be put to some practical use occurred with the Department for Business Innovation and Skills (BIS) in the UK. A programme funded by the “Science and Trust Action Group” involved compiling a series of written case studies exploring ethical issues within science and engineering. The *Centre for Excellence in Teaching and Learning* in Interdisciplinary Applied Ethics (IDEA Cetl) at the University of Leeds won this contract, and I worked with them to produce a case study based upon my engagement with Bob that was presented in Chapter 10. This written case was one of ten written that were planned to be used in professional development workshops for young scientists and engineers. Having been presented with the case study, they would be invited to debate the different ethical issues provided by the situation.

Whilst only a teaching case, it is interesting to consider how the ethicists at the IDEA Cetl framed my research findings. Here is the closing case summary as they wrote it: “This case explores the responsibilities of scientists, and how far they should go in taking responsibility for what they discover during their research. The need to specialise in science can create situations where multiple people work on a single project, but only one group or organisation are in a position where they can see all the information, and therefore, are the only ones who can know all the risks, but they may not understand all the risks. It’s therefore important for scientists to try to make sure the right people know about their research, as much as possible, as failure to do this could impact on the public".

The disappointing postscript to this story is that shortly after receiving all the case studies, and before they could actually be used on development programmes, there was a reorganisation within BIS and the commissioning group was disbanded. To the best of the knowledge of the IDEA Cetl, the cases have never been debated by young scientists, or otherwise put to practical use.

In both these short accounts of my attempts to progress opportunities for impact for my own research it can be seen how such efforts may be frustrated for the most mundane of reasons. These stories are presented here as doing so allows an interesting juxtaposition with the case studies of nanotechnology research scientist pursuing impact from their research.

Having acknowledged my own limited success in pursuing changes in innovation policy and practice, I close this chapter by offering more general thoughts on the practical implications of this research. In relation to innovation policy, then the review in Chapter 8 suggested that the discourses touching (separately) on economic and societal consequences are not yet completely aligned. I argue that both are necessary in attempts to characterize this confusing space where science meets innovation, whilst acknowledging that the main thrust of technology innovation involving universities is dominated by economic considerations. And yet to adopt a strategy of responding to crises of public confidence in new technology only as they erupt would seem to risk cementing intractable positions. It is not hard, for example, to draw parallels between contemporary debates in the UK on the introduction of "Fracking" technology (Jaspal and Nerlich, 2014) with the GM Crop protests of the 1990s. Therefore, renewed efforts are required of innovation policy makers to incorporate both economic and societal voices at earlier stages in the innovation process. This includes those stages at which universities become involved, and could involve a greater foregrounding of non-economic types of research impact that already exist in guidance notes from research councils (e.g. EPSRC, 2014).

I suggest there is also a need to shift policy work, and associated practical enactments, more *downstream* towards firms who commercialise technology. The discourse of responsible innovation has largely revolved around questions of what areas scientific research should receive public funding, and societal implications for the technologies that are suggested by such investments. Firms and their innovation mangers have been largely divorced from such upstream debates, and pursue their commercial goals within the context of known regulation. However, regulatory restrictions can change rapidly as GM crops companies found to their cost in the 1990s, and hydraulic fracture drilling companies are experiencing at the moment. How then can firms incorporate the principles of responsible innovation (cf. Stilgoe *et al.*, 2013) into their practice of innovation? There are the first signs of research in this area including suggestions for how firms may incorporate wider notions of responsibility into their innovation routines (e.g. Asante *et al.*, 2014). In an analogous downstream move in the policy sphere then the commitments of the largest UK research council to a framework for responsible innovation (Owen, 2014), could be followed by comparable developments in the more business-facing departments of Government.

Finally, what recommendations might I offer to university research leaders seeking to increase their repertoire of research impacts? Having a social scientist in residence is never likely to be a realistic option. In thinking about practical alternatives I am put in mind of an experience at the start of my time with MicrosCo. I was scheduled to introduce my research at the second of their newly inaugurated networking seminars. As I sat in the audience, the agenda for the seminar was projected onto the screen at the front, along with the title they gave for my talk: “A collaborative project with social scientists”. At this point I overheard a PhD student nearby ask his friends “what’s a social scientist going to tell us?” to which his friend replied, “that we should get out of the lab more”! At the time I smiled at the idea of social-scientist-as-counsellor implied in the answer. However, now I find the reply rather prescient. To stay in the lab is to default to a traditional division of innovation labour that this research shows is becoming less tenable. Whether it is policy makers, schoolchildren, NGOs or stained-glass window experts (!), university scientists should increasingly participate in science-related conversations with non-traditional audiences (i.e. not other scientists or commercial business managers). Where this may lead will be uncertain, but taking part in what the Research Director at MicrosCo called an “open process” with all comers will increasingly be seen as part of what university research groups do.

Chapter 14
A Social Scientist in the Lab – A Reflexive Analysis

The interfaces between university research and the world outside the campus has been manifest throughout this thesis. It constitutes the substantive topic of the work which has examined how university research groups are organized within the context of an emerging requirement for their work to be judged in relation to both societal and economic impacts. In the rhetoric of the thesis title this interface is a place where science meets innovation. Whilst nanotechnology research groups have been the focus, analogous debates have a significant history within business schools. The issues at stake with nanotechnology (and other emerging technologies) have prompted fractious debates (cf. Chapter 1) and witnessed a discourse of responsibility. By contrast the challenge of business school researchers to be more relevant (cf. Chapter 5) seems positively tame by comparison. Nonetheless, this final chapter turns the researcher's gaze inward and explores the lessons I have drawn from this project of working at the interface between management research and the world outside the business school. In this I aim to offer suggestions for the researcher who works at these interfaces.

Reflecting first on the tensions and congruencies of nanotechnology research and innovation, it is pertinent to ask what have I learnt from my research subjects (or should that be collaborators?), about how I should go about realising an impact from my research? Can I extend to *my own* research practice, the innovation practices at ColloidCo and MicrosCo as I have described and explained in this thesis? At the organizational level one direct lesson would be to suggest that, along with my business school collaborators, I should seek to extend our current capabilities by working with a wider variety of stakeholders, and develop new narratives for explaining our work. There are some (Thorpe and Rawlinson, 2014) who might argue that simply to work with businesses would represent innovation within our business schools. In this they note that we have some way to go before we reach the levels of practitioner collaborations already common practice with professional schools of medicine and engineering. Chapter 13 served to illustrate that my own efforts at realising an impact from this work have met with limited success. However, the lesson I draw from my research subjects is less one of instrumental impacts (at least in the short term), and rather one of the narratives and frames I use to explain what it is that I am doing. Part 1 of this thesis is my attempt to position my research to appeal to both business school colleagues and the practitioners with whom I have engaged. I have come to view such positioning as a necessary first step in sketching out the boundaries of the academic-practitioner interface of interest. And so my first lesson is to ensure that I have identify the debates within both academic *and* practitioner communities to which I hope the research will speak.

Another interpretation of the seeking new collaborators might be to ask what new venues are possible for management and organisational research. Just as the stained glass workshops of cathedrals and nanotechnology research might not immediately seem obvious bedfellows, what are the novel research venues for business school colleagues? The new (in 2013) editors of Organisational Studies make a similar call reminding their readership that "Organization Studies does not limit itself to the study of business firms" (Holt and den Hond, 2013, p.1588). And whilst their aim is most certainly not to encourage an simple pursuit of managerial relevance, they argue that "good scholarly work...can also evaluate and argue for a change; it can bring researchers into wider social settings, addressing questions that matter to people in their lives" (*ibid.*, p.1596). And so my next lesson is to be open to the unusual in research setting.

Reflecting on the *research process* itself there are a number of boundaries of which I became conscious. Perhaps mostly obviously I was an innovation researcher working amongst innovation professionals, and so there was an academic/practitioner interface. There were also a number of interdisciplinary boundaries. There was the social science/natural science boundary inherent in my study of nanotechnology groups. However, through my work with the STIR project (Chapter 6) I also became conscious of boundaries between management studies and other social and human sciences. I reflect upon my work at each of these interfaces below.

In considering management researchers working with management practitioners then we are in the familiar territory of the *relevance* debate. I have discussed important features of this debate already in chapter 5 in relation to aspirations at the outset of the study and the implications for my research design. My outlook on how relevance should be approached, and indeed whether it is even possible, has changed during the seven years of the PhD programme. Just before the start of the empirical work for this thesis a "Point-Counterpoint" debate in the *Journal of Management Studies* considered whether management research and practice are, in fact, unbridgeable. In chapter 5 I offer my contemporary response to this debate: whilst having sympathy with Kieser and Leiner's argument (2009) that the two worlds represent self-referential communication systems that can never truly understand each other; I came down on the side of the we-can-do-it-with-the-right-methods camp (cf. Hodgkinson and Rousseau, 2009, Starkey *et al.*, 2009). Five years later I feel less secure of that position. Despite having adopted research methods that embedded me in the world of the innovators; despite having engaged with individual scientist-innovators over an extended period of time; I am not convinced that much bridging of the management theory-practice gap actually happened during my study.

Again looking to the researchers at ColloidCo and MicrosCo as exemplars; they displayed an openness regarding what was expected of them in collaborations with new stakeholders, and to which they brought the totality of their scientific competence. The approach was not simply one of ‘how can I apply my research’, but rather what can I contribute as a scientist. Much of the relevance debate in management studies (cf. Chapter 5) is conducted with a research project being the unit of analysis. The relevance challenge is often framed in terms of realising both academic and practitioner outputs from a single piece of research. This framing seems to place too high a burden for a single piece of management research to bear. One lesson from the cases in this PhD may be to consider whether a better framing is one of the relevance of management research*ers*, rather than a piece of their research. The practical implication for engagement with practitioners becomes one of bringing the whole gamut of our knowledge and experiences to bear on the issue at hand. In this case research then maybe the modest impacts I effected through in running workshops on creative climates (outlined in Chapter 13) and bringing other management research ideas (and not just the ones in my immediate research project) are worthy of further reflection.

During my professional life I participated in innovation projects based upon chemistry research. While I believe that these experiences allowed me to empathise with the challenges faced in my case groups, the insights I offered from my management scholarship failed to connect with them. Whilst I proved useful (as discussed briefly in Chapter 13) on occasion as a management consultant, my management research seemed to have little impact. Copies of the main publication from this research (Pandza and Ellwood, 2013) were shared with research leaders during the drafting and before the final submission. Conversations about what they had made of my conclusions were met with phrases such as "interesting" or "I'll need to think about that". This leads me to conclude that management research whose questions are informed (in part) by practitioner concerns, and engages with practitioners directly in the generation of data, may be necessary for relevance, but are not sufficient. This does not lead me to suggest some notion of joint analysis of the data: I am more of the mind of Kieser and Leiner that these two worlds speak different "languages" and cannot be bridged through some simple co-production process. Rather knowledge production processes are needed to which practitioners and academics bring their own type of knowledge, and which are then allowed, in Kieser and Leiner's phrase, to "irritate each other...[something which might]..turn out inspiring" (2009, p.517).

In recent years a number of scholars have sought to develop academic-practitioner engagement processes that involve such knowledge production through *friction*. Kieser and Leiner themselves revisited their earlier papers and described engagement process that entail mutual inspiration (Kieser and Leiner, 2012). These include action research, consulting and executive education. Paton and colleagues coin the term "relevate" (signaling a sight-raising quality to relevance) in describing a process that “deliberately disrupts the established boundaries of a problematic with a view to expanding the scope and range of issues considered relevant or irrelevant to a particular decisional imperative” (Paton *et al.*, 2014, p.269). However, Beech *et al.* (2010) sound a note of caution noting that many dialogues between academics and practitioners remain self-defeating as they unwittingly promulgate a separation between the two communities. For these authors the answer also lies in longitudinal engagements; but ones framed as if between equals as these make possible dialogues that “generate resonances and ongoing ripples” (Beech *et al.*, 2010, p.1364). Such dialogic approaches are illustrated in a Special Edition of *Management Learning* (MacIntosh *et al.*, 2012) Here management *practicing* and *knowing* are imagined as entwined rather than distinct activities; dissolving in the process the labels ‘researcher’ and ‘practitioner’. In this perspective all participants are practitioners who bring different experiences of practicing to these encounters. All these authors suggest the importance of continuing engagement beyond data generation in a manner that is not simply (academic) knowledge transfer, but more cognisant of different knowledge generation processes that operate simultaneously as practitioners and academics talk.

These papers are positioned in relation to debates on the relevance of academic knowledge for practitioner knowledge and practice, which is understandable given the popularity of this debate in academic journals. Possibly as a result, it is invariably the influence on manager’s practice that is described in these methods. The impact of these extended methods of engagement on the academic’s theorizing of management is less clear. The difference for scientific outcomes, of an academic engaging with practitioners as collaborators in an extended dialogue, compared with engaging with them as research subjects would be worth more detailed examination. I am aware of only one study that has examined the influence of practitioner thought on academic theorizing. Barley *et al.* (1998) examine the practitioner and academic texts on the subject of organizational culture that appeared over the course of a decade. Following a detailed content analysis they concluded that “over time…academics appear to have moved toward the practitioners’ point of view, while the latter appear to have been little influenced by the former” (Barley *et al.*, 1998, p.24). Whilst this study takes as its unit of analysis communities of academics and practitioners, rather than individuals, it clearly demonstrates the benefits to theory from close acquaintance with practitioner knowledge. If the same could be demonstrated for individual engagements (cf. Schultz and Hatch, 2005), it might serve to enliven the interests of those academics skeptical towards all calls for greater relevance (e.g. Learmonth *et al.*, 2012).

It is pertinent to revisit my own extended dialogue with a post-doctoral researcher called Bob (reported in Chapter 10) in light of these ideas. This engagement was structured using the *midstream modulation* protocol that I encountered through participation in the STIR project (chapter 6). The purpose of this protocol in the STIR project was to test whether a natural and social scientist engaging in a mutual inquiry about a specific innovation project could engender a greater reflexivity regarding the prospects for Responsible Innovation. Chapter 10 offers *my* interpretation of this extended encounter. In this I report a significant shift in the course of the project: a completely new avenue of research was started that had not hitherto been part of the project’s scope. In chapter 10 I am mindful of other influences on Bob, and argue that whether the encounter (or the *midstream modulation* protocol) influenced this change remains open for debate. I also note that whilst effecting an abrupt practical change, this did not in turn influence the larger innovation project which was discontinued for other commercial reasons. An individual’s reflexive awareness of outcomes may not always change something as complex as an innovation project. Nonetheless, the encounter reported in chapter 10 might reasonably be added to the examples (cited above from the management research literature) of practitioner’s thinking being influenced by an extended encounter with an academic.

What influence then did this engagement with an innovation practitioner have upon my own efforts to conceptualise the changes that I was studying? One straightforward answer is to affirm that it contributed to my understanding of how innovation was organized at ColloidCo. The engagement revealed the work of individuals within larger innovation projects. In this my assertion that the microfoundations for innovation capabilities includes individual's *prior experiences*, and *technical knowledge and competences* (Chapter 12), was strongly informed by my encounters with Bob. However, in this matter Bob appears to be a conventional research subject from whom I am generating data. Accepting the STIR project premise that Bob and me were engaged in a process of mutual inquiry facilitated by enhanced reflexivity, it is pertinent to examine the influence that my presence was having on the inquiry process.

Alvesson and Skoldberg suggest that reflective social research involves a critical self-exploration of one's own interpretations. In this they suggest that are four elements of reflective research (Alvesson and Skoldberg, 2009, p.8ff): systematics and techniques in research procedures; a clarification of the primacy of interpretation; awareness of the politico-ideological character of research; and reflection in relation to the problem of representation and authority. I examine each of these four elements in seeking insight to the influence of my presence on the research process.

Whilst Fisher had developed the "midstream modulation" protocol within his own research (2007), the STIR project constituted something of an experiment in its widespread use. Therefore, over a series of five research workshops, participants in the programme compared our use of it for facilitating mutual inquiry between social and natural scientists. These reflections and an evaluation of its outcomes in individual studies have been compiled in a paper under review (at the time of writing) in *Nature Nanotechnology* (Fisher et al, in review): a distinguished journal in the natural sciences. The general conclusion across a range of 30 studies is that repeated use of the protocol can elicit societal responsiveness in natural science research. My own experiences (outlined at the start of chapter 10) would nuance this conclusion by adding that the chance of effective interactions are enhanced if dealing with experienced scientists, and having an engagement period sufficiently long to encompass the natural experimental cycles of the scientist collaborator. The opportunity for such a detailed comparison and reflection upon my use of a research method, contemporaneously with its use in the field, is unusual. I argue that this experience has provided a useful corrective in examining the impact of my own presence on this case research. This has informed my interpretation in Chapter 10 of my engagement with Bob and the openness it displays to other influences than my use of the protocol.

The political undercurrent to my interactions with Bob was something of which I was acutely aware throughout the research; perhaps because of my own background in natural science. The origin of this aspiration for mutual inquiry came from the social scientist. It was me who approached both ColloidCo and MicrosCo with the idea to collaborate, and the method of our interaction took place on my terms. In the instance of my work with Bob, then I was the one controlling the use of the midstream modulation protocol. The framing of the interaction as one of mutual inquiry facilitated by enhanced reflexivity was (ironically) not conceived mutually. In a similar vein, I must acknowledge that the interpretation presented in Chapter 10 is mine. Bob has read a copy of this account and I acknowledge his response: he believed that he would have made the shift in the direction of the project without our conversations. My response to these possible weaknesses in my interpretation has been one of transparency: laying out the practical progress of the engagement, using Bob's own words as far as possible and remaining open to other interpretations (including Bob's). In summary, reflecting upon my influence upon this research project (cf. Alvesson and Skoldberg, 2009), with particular emphasis on my engagement with practitioners, I believe that my research conclusions are sound, if conventional. I have generated data with practitioners in a manner that is sensitive to their perspective, and produced my own interpretation of how they organize for innovation. In this the research project is conventional rather than being one of mutual inquiry. In thinking how this research might have been less conventional, it is intriguing to imagine a practitioner-academic engagement in which both made use of the midstream modulation protocol to facilitate reflexivity in the other.

The engagement with innovation practitioners in a process of mutual inquiry remains for me an aspiration. I have learnt that such inquiry is certainly aided through longer term collaborations, and have the hunch that a single research project may simply be too narrow a frame, through which the aspiration can be manifest.

Postscript

The places where science meets innovation are ones I have occupied throughout my professional life. As I have progressed through this PhD I have consciously changed my perspective to this space: from natural scientist to social scientist; from management practitioner to management researcher; from a leaning towards the relevance of academic research towards a greater valuation of theory. Such shifting landscapes have increased my enjoyment (and hopefully my learning) in the process. PhD programmes ought to expose candidates to a rounded view of research; affording them a sight of different conceptions of the researchers job. Laying out such options at the outset of their careers will help them think through the type of researcher they aspire to be. Such choices are influenced by the company we keep, and I end with some lessons learnt from the social researchers with whom I have worked, and who have influenced my own development.

Firstly, I should like to acknowledge the intellectual stimulation of working with peers from a variety of social and human science disciplines as part of the STIR project. In many ways the interdisciplinary interface with these sciences was stranger than the one with the nanotechnologies. This could be readily explained by my background in the natural sciences, but that is not the sum of it. In other social and human sciences I encountered a tendency to deconstruct phenomena to a greater degree than I have yet seen in management studies. At times this was personally challenging as my own efforts at theory-building from case research was likened to the shallowness of theory-building from natural scientists! From these peers I have learnt to check a natural impulse to order data, and stay with the complexity of the phenomena for a little longer.

My last word must be for my supervisors. My interest in the double hurdle of rigour and relevance was unwittingly (in the sense I cannot claimed to have planned it that way) personified in the allocation of my two main supervisors. Whilst both having made contributions to both theory and practice across the body of their research, the *centre-of-gravity* of their interests seems different; with one leaning to theory and the other more passionate about relevance for practice. As a consequence my seven years of study have been characterized by the widest range of experiences that they have facilitated: from progressing a paper through the review process of a leading journal; to giving evidence to a parliamentary select committee on engaging with small enterprises. The venue for my own researcher development throughout this PhD has been conducted at the interface of theory and practice. And so looking forward I remain committed to those places in business schools where science meets innovation.

List of Abbreviations

AFM	Atomic Force Microscopy
ANT	Actor Network Theory
AoM	Academy of Management
AMP	Academy of Management Perspectives
EPSRC	Engineering and Physical Sciences Research Council
ESRC	Economic and Social Research Council
GM	Genetic Modification
GMO	Genetically Modified Organism
Hefce	Higher Education Funding Council for England
HEI	Higher Education Institution
HEIF3	Higher Education Innovation Fund – 3rd Round
IDEA Cetl	Interdisciplinary Ethics Applied Centre of Excellence in Teaching and Learning
IP	Intellectual Property
JMS	Journal of Management Studies
KT	Knowledge Transfer
KTP	Knowledge Transfer Partnership
NGO	Non-Governmental organisation
NSF	National Science Foundation
PRSE	Public Sector Research Establishment
QCM	Quartz Crystal Microscopy
RAE	Research Assessment Exercise
REF	Research Excellence Framework
STIR	Socio-technical Integration Research
STS	Science and Technology Studies

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