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Institutional Change in the Natural Sciences

A thesis presented in partial fulfilment of the requirements for the degree of

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Andrew Gordon Dickson

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Abstract

This thesis investigates the Allan Wilson Centre for Molecular Ecology and Evolution, a Centre of Research Excellence financed by the New Zealand Government's CoRE fund, which was established in 2001. The CoRE fund represented a change from traditional science funding in New Zealand. Its aim was to make use of existing networks of scientists, from several institutions and disciplines, to form new 'Centres of Research Excellence', independent from any existing institution, but made up of members who remained in their existing positions.

The aim of this thesis is to investigate whether the formation of the Allan Wilson Centre has made a difference to the way its members carry out their science and, if so, how. To do this, an actor-network approach is used to analyse the various 'modes of ordering' the Centre, to make sense of the networks represented by it.

The results show an interesting shift in the way that science is carried out in the Allan Wilson Centre in contrast to the pre-Centre form. Although the focus of the Centre remains firmly on the science they do, they now also interact regularly with the discourse of management in order to better 'do' and 'encourage' their science, creating new successes but also new tensions.

The importance of this thesis is two-fold. First, it provides a mechanism through which to 'hear' the voice of the Allan Wilson Centre and its members; and second, it provides a means through which science policy makers can see how this particular policy mechanism may have changed the process of science.

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1. Introduction

This thesis is fundamentally about institutional change. In particular it is about the impact of a government policy, the Centre's of Research Excellence (CoRE) fund, on the formation and function of a science institution, the Allan Wilson Centre for Molecular Ecology and Evolution. Studying the impact of government policy on science institutions is not a new thing, John Law for example wrote '*Organizing Modernity*' (1994a) based on his experiences researching the impact of government policy on a UK public sector science institution, and this is just one example. Before Law works such as Thomas Kuhn's '*The Structure of Scientific Revolutions*' (1970) and Latour & Woolgar's '*Laboratory Life*' (1979) provided a strong basis for the social study of science institutions, it is upon these, and others, that I build my theoretical and analytical foundation. In spite of this international academic basis, not many studies have specifically considered government policy impact on New Zealand science institutions, some exceptions to this include Leitch & Davenport's work on the science funding framework (2005); Doolin's research into government policy impacts on a public hospital (1999; 2003) and Davenport & Daellenbach's recent research on the formation and function of another of the CoRE's, the MacDiarmid Institute for Advanced Materials and Nanotechnology (2006). This research adds to this small body of research by investigating the impact of the CoRE fund on the Allan Wilson Centre.

I chose the Allan Wilson Centre (AWC) as I have been interested in the precursors to and the current AWC since about 1998; when, as an undergraduate student within Massey University's College of Sciences, I was able to watch, with my student colleagues, the research strengths that Massey showed in the disciplines of molecular biology, theoretical genetics and mathematics that were a major factor in the awarding of the CoRE fund and establishment of the AWC in 2002. My wife was undertaking a Bachelor of Science honours degree in Molecular Genetics with David Penny (now Research Director of the AWC), and it was accepted that David and his associates were rather impressive scientists on an international scale. Particularly I was fascinated by the interdisciplinary nature of much of their investigation, watching my wife struggle with the mathematics

and computing that was a large part of her honours thesis made me realise that this science (molecular genetics) had become, by necessity, interdisciplinary. The fact that government policy around the same time (see Tertiary Education Advisory Commission, 2000) was gearing up to identify and specifically promote interdisciplinary and inter-institutional collaboration may have been coincidence, but this, along with the research strengths acknowledged above, seem to have led to the formation and funding of the AWC. What interests me now is how the institutional and disciplinary relationships have changed under the new ordering regime.

Before looking at the demographics of the AWC it is important to introduce its name sake: Allan Wilson. Allan Wilson passed away in 1991 during treatment for leukaemia at the age of 57; this was a tragic loss for New Zealand and for the study of molecular evolution. Allan Wilson was a pioneer of molecular techniques, bringing the study of DNA to bear on the scientific fields of biochemistry, genetics, palaeontology and archaeology amongst others. During his 35 year tenure at the University of California, Berkeley, Allan Wilson trained most of the current 'superstars' of molecular evolution; his ideas were revolutionary and transformed Humans' knowledge of their own evolution, particularly his 'out of Africa' Human evolution theory is still recognised today as one of the most significant scientific breakthroughs of the 20th century. Allan Wilson was a New Zealander, born in Ngaruawahia, and trained initially at Otago University, however this fact is not well known – particularly by people outside of New Zealand. By using his name, the AWC is doing two things; firstly recognising and celebrating the success of an extraordinary New Zealander on an international front by rhetorically drawing a link to Allan Wilson's identity as a New Zealander, and secondly, using Allan Wilson's name and profile to draw attention to the Centre's own successes in molecular evolution. To me, a proud New Zealander, these both are admirable efforts.

The institutional constitution of the AWC between 2002 and 2006 involves five universities; Massey University is the host institution and provides two of the main sites. Massey's Turitea campus (in Palmerston North) is the official headquarters but Massey's Albany campus (in Auckland) is also host to several collaborators. The other universities are: The University of Auckland; Victoria University of Wellington; Canterbury

University and Otago University. From a disciplinary perspective, in their own words, the AWC “comprises world class ecologists, evolutionary biologists and mathematicians who will work together to unlock the secrets of our plants, animals, and microbes” (Allan Wilson Centre for Molecular Ecology and Evolution, n.d.a, para. 1). From this clearly the AWC can be described as an interdisciplinary research centre, particularly one that brings together scientists from the disciplines of ecology, evolutionary biology and mathematics.

Although the breadth of science undertaken under auspices of the Centre is rather grand the actual Centre is quite small. In total it currently comprises less than one hundred members (including all scientists, students and support staff). Of these, ten are primary investigators with the rest made up of post-doctoral fellows, support staff and graduate students (many of which originate from outside of New Zealand); this is approximately similar in size to a small University department.

The AWC has four main research projects covering a broad spectrum of evolutionary science; project one looks at the rates and modes of evolution; project two at biodiversity; project three at human settlement in Aotearoa/New Zealand and project four is aimed at developing new ecological and evolutionary theoretical models. These four projects form a research programme which together address issues currently central within the fields of molecular ecology and evolution. Recent examples of their science include the breakthrough investigation of how the microevolutionary processes of Adelie penguins are impacted by environmental changes such as the movement of icebergs; and more generally the role of microevolutionary processes in macroevolution, such as the evolution of mammals.

Although the research programme is broken up the four major projects mentioned above, they do not appear to be in any way disciplinarily distinct, in fact the interactions between the biologists and the mathematicians can be seen in all of the work. For instance, the introductory blurb for project four states “we seek to exploit the dynamic interaction that exists in this group between mathematicians and biologists” (Allan Wilson Centre for Molecular Ecology and Evolution, n.d.b, para 1). Also the projects are not in any way institutionally distinct, all involve members from across the spectrum of universities

involved. In many ways this does not seem odd as prior to the establishment of the Centre many of the collaborations forming the current project streams were already in various stages of existence.

My research investigates the form and function of the Allan Wilson Centre using an actor-network model informed primarily by Law's modes of ordering analytical approach (1994a). Actor-network theory, with its emphasis on the 'process' of organising rather than the 'forms' of organising, is generally written utilising odd grammatical devices. In particular verbs such as 'embodying' and 'performing' are used to describe things that are more commonly portrayed as stable nouns such as an 'institution'. Within actor-network theory the emphasis is on the performance of the institution rather than the structural nature of the institution for instance. Consequently, I offer a cautionary note: At certain points some readers may find the text a little 'lumpy'; please be assured that this is a purposeful ANT inspired discursive strategy.

Briefly, the results show an interesting shift in the way that the scientific practices and the management of science are constituted together in the current Centre in contrast to the pre-Centre form. The Centre interacts with the discourse of management in order to better do, promote and encourage their science. This has interesting ramifications for the nature of science policy and management of science through the Centre of Research Excellence form.

I believe that the results of my thesis will be of use in several forums. Firstly, my thesis follows in a line of other studies of science institutions conducted using an actor-network approach, in this way it adds to the body of actor-network literature. Secondly, the results of my research will allow those interested in the science sector in New Zealand to 'hear' the voice of this particular Centre, and its members, at least through the prism of my analysis. Thirdly, I think that science policy makers may be interested in my interpretations of how this particular policy mechanism has changed the process of science, and how it appears to be revealing other things, such as how the tertiary research and tertiary teaching structures seem to be somewhat divergent.

This thesis is organised into seven chapters, following this introductory chapter, chapter two considers the literature supporting my thesis, this concludes with an overview of my theoretical position. Chapter three presents my research methods and methodology, actor-network theory, and gives more detail on the specific analytic framework through which I conducted the analysis of data. Chapters four, five and six are the results and discussion chapters; each of these presents one of the three 'modes of ordering' the Allan Wilson Centre, these are doing science, encouraging science and managing science. Chapter seven presents the conclusions that I have reached through the process of my thesis by looking specifically at areas where the different modes of ordering intersect and how this has changed things before looking at some of the limiting factors inherent within my research approach and providing some directions for future research.

2. Literature

This chapter presents and discusses a range of literature within which I will locate my research thesis. Several bodies of literature inform my thesis; this is mainly because the Allan Wilson Centre can be understood by looking through the different lenses of several different bodies of knowledge. I felt it was important, and useful, to consider broadly how these knowledges have influenced the formation of this type of research centre (the Centres of Research Excellence, or CoREs) before looking closely at several literatures that are more specific to my thesis. Accordingly this chapter is ordered along the following lines; beginning broadly in the first section I consider the concepts of public management and the funding of science on an international scale, and then more specifically how these have impacted New Zealand's science policy. Also in this section I consider the notions of disciplinarity and interdisciplinarity generally and their role in the production of knowledge and how this has specifically influenced science funding policy. The final part of the first section explores some literature around the nature of institutional collaboration. In the second section I narrow focus to consider three specific studies, all conducted in the actor-network genre, that look at organisations that for different reasons are exemplar informants for my research. The final section concludes with an examination of the research problem I attempt to address in this thesis.

2.1. The Public Management of Science Organisations

Science research is mainly funded through the public sector, either by funding universities or government run research laboratories. Traditionally these institutions, different from their private sector equivalents, conduct more fundamental research than directly applied research and therefore find it difficult to attract private funding due to the long-term and uncertain return on investment. This section is concerned primarily with considering how publicly funded science institutions, like the Allan Wilson Centre, operate within the environment created by the policies of government. The first three subsections look at how a change in the policies of western economies in government research has occurred since the late 1980s and early 1990s as a result of new public management practices and how this has shaped science policy funding in New Zealand.

The final two subsections look at academic disciplines, interdisciplinarity and the 'research centre' and the nature of institutional change in academia.

2.1.1. A Global Change in Government Research Funding

Government research funding for Universities has traditionally been focused in disciplinary areas; and this makes sense, as academic institutions are, in the most part, set up to function in discipline and research 'silos' concentrated along structural disciplinary boundaries (Awbrey & Awbrey, 2001). Over the past two decades there has been a considerable shift of focus in the academic, research and government communities towards a model of funding that focuses on interdisciplinary research centres. One of the main reasons cited by those that believe that traditional research funding structures cannot continue is that advances in research (particularly in the 'hard' sciences) must increasingly work across traditional discipline boundaries to solve the research problems they are facing (Klein, 1996; Grigg, 1999). Examples of these research problems are cited as being present in all facets of science; including cognitive neuroscience, nanotechnology and evolutionary biology, amongst others.

As a result of this emerging power shift over the past twenty years many governments have begun funding alternative institutional forms to foster interdisciplinary research. As Garrett-Jones, Turpin, Burns and Diment comment: "International experience shows that research is increasingly being carried out in organisational forms, such as university-industry collaborative research centres, built around cross-sectoral and transdisciplinary teams" (2005, p. 535). In the UK for instance university-based Interdisciplinary Research Centres were established in the late 1980s "to focus on strategic base science – particularly in interdisciplinary areas" (Hoch, 1990, p. 39). Closer to New Zealand, the Australian Government began funding Cooperative Research Centres (CRCs) in 1990 (Slayter, 1994) and continues to do so today. To date there are more than 70 CRCs in operation in Australia to which the Australian federal and state governments have together contributed a total of 3.2 billion in funding (Garrett-Jones, et al, 2005).

The Finnish Centres of Excellence (CoE) model is a particularly useful example to understand the mechanism used to implement New Zealand's own Centres of Research

Excellence scheme, as it appears that the original Tertiary Education Advisory Council (TEAC) reports (used to inform the current New Zealand tertiary education system) utilised the reviews conducted by the Academy of Finland on Centres of Excellence across 17 nations. The Finnish CoE model originally emerged in 1995 when the Finnish Ministry of Education announced the establishment of 12 Centres of Excellence; these were initially funded for five years through to 1999 (Gibbons et al, 2004). The scheme as it currently exists was established in proper in 1998 based on a strategy published in 1997 by the Academy of Finland. This strategy document reviewed the excellence programmes of several other OECD countries and concluded that the main objectives of Centres of Excellence programmes across these countries included “developing top national know-how and competitiveness in fields that can involve basic and/or applied research, supporting top-rank researchers, guiding multi-disciplinary cross-sector research programmes, marshalling scientific resources at universities and promoting results exchange and exploitation” (Academy of Finland, 1997, p. 18)

So, clearly there has been a significant change in the way that Governments in the OECD have chosen to fund science, the ‘Centre of Research Excellence’ has been an influential concept.

2.1.2. History of NZ Government Research Funding

Like all western nations New Zealand has its research funding roots in the same traditional structures of the disciplinary arrangement of higher education institutions and government run R&D organisations. Up until the 1980s the majority of research funding in New Zealand was provided to conduct research through two main mechanisms, firstly by funding universities based on numbers of equivalent full time students (EFTS) and secondly by directly funding the Government’s Department of Scientific and Industrial Research (DSIR); and the research arms of other Governmental areas such as the Ministry of Agriculture and Fisheries and the Ministry of Forestry (Ministry of Research, Science and Technology, & Crown Company Monitoring Advisory Unit, 2002).

The new public management reforms of the 1980s and early 1990s changed the nature of science research funding in New Zealand as dramatically as they did in the rest of the

western world. As Hammond and Devine note “the science reforms were fundamentally directed at separation of policy from the research purchasing and from the provision of scientific services” (1994, p. 120). Several new Government organisations were set up to manage the funding of research during this reform period, and the Government owned and run scientific and research departments were disestablished in favour of the establishment of the Government owned, but privately run Crown Research Institutes (CRIs).

The Ministry of Research, Science and Technology (MoRST) was established early on in this period to manage the allocation of the funds associated with the vote Research, Science and Technology (RS&T) portfolio. Primarily they do this through another entity established early (1989) in these reforms (Leitch & Davenport, 2005) the Foundation for Research, Science and Technology (FRST). The separation of the development of research funding policy and the allocation and management of research funds identified as important in the reform process is achieved through this split. The Royal Society of New Zealand (RSNZ) and the Health Research Council (HRC) also act as fund distribution and management agencies (Ministry of Research, Science and Technology, 2005).

The other main research funding portfolio is the Ministry of Education (MoE). The research funds allocated to Tertiary Education Organisations (TEOs) through the vote Education portfolio form a large amount of research funding in New Zealand (STEP, 2005). Primarily this allocation of research funding has been provided to the universities and other TEOs on an EFTS basis, this is being gradually phased out and replaced by a Performance Based Research Fund (PBRF) which allocates research funding to TEOs based on three factors; an assessment of the quality of their research outputs; the reported research degree completions; and a reflection of the external research income generated (Ministry of Education, 2005).

Although other research is contracted, funded and carried out in New Zealand, the two ministries discussed above provide the vast majority of research funding to New Zealand researchers and are powerful actors influencing the structural and political formation of

research institutes, mainly by defining and implementing research policy. The following sub-section identifies the current science funding policy initiatives utilised by the ministries and the research structures that have emerged as a result of this.

2.1.3. New Zealand Research Policy

As identified in the previous section the current research funding system in New Zealand provides funding for science research through two main ministerial portfolios. The Research, Science and Technology (RS&T) portfolio provides the majority of funding for scientific research in New Zealand, but the Education portfolio also funds research. This section considers how these ministries impact New Zealand science institutions.

Ministry of Research, Science and Technology

The Ministry of Research, Science and Technology (MoRST) manages the majority of government research funding in New Zealand. In 2005/2006 this totals approximately \$639 million dollars (Ministry of Research, Science and Technology, n.d). This is approximately 3.4 times the funding allocated to research by the Ministry of Education. MoRST does not directly fund research in New Zealand, but utilises a contractor relationship with certain funding agencies, three of these provide the majority of funding allocation to scientists in New Zealand, as mentioned above, they are: The Foundation for Research, Science and Technology (FoRST); the Royal Society of New Zealand (RSNZ), and; the Health Research Council (HRC) (Ministry of Research, Science and Technology, 2005).

MoRST's funds are divided into a set of output expenses, each managed by an agency contracted to MoRST. For scientists and research institutions in New Zealand the major funds managed by agencies contracted to MoRST include: Environmental Research; Health Research; the Marsden Fund; the New Economy Research Fund; the Pre-Seed Accelerator Fund; Research for Industry, and; the Supporting Promising Individuals Fund. MoRST assesses the 'return on their investment' into the various funding schemes by considering how they contribute towards four goal areas, identified in their 2005 statement 'Sustaining Strong Investment', these four goals areas are: Knowledge;

Economic; Environment; and Social (Ministry of Research, Science and Technology, 2005).

Over the reforms period in New Zealand between 1989 and 2004 the terminology used in the science policy agencies underwent significant change (Leitch & Davenport, 2005). Indeed, Leitch & Davenport describe this as a move from “excellence to relevance” (p. 896). It seems that accompanying this change in language was a change in ideology from a focus on basic research and ‘science for science sake’ to a focus on applied research and ‘return on investment’. This can be most readily seen in the CRI funding model; immediately following the 1989 establishment of the CRIs after the economic reforms, the funding that had been provided to the Government institutes was available to the CRIs under a semi-contestable scheme entitled the Public Good Science Fund (PGSF). However this funding approach changed significantly between 1989 and 2001, as the aim for the CRIs was to develop alternative sources of funding (Hammond & Devine, 1994) to become independent and commercially viable; this aim was achieved somewhat, in 1998 CRIs used approximately 85% of the PGSF and this had dropped to around 50% by 2001 (Ministry of Research, Science and Technology, & Crown Company Monitoring Advisory Unit, 2002). The funds that had been allocated to the PGSF have now been redistributed into the new ‘output expenses’, some of which I identified above, all that remains of the PGSF is two references to “public good” in the Health and Social Research output expenses (Ministry of Research, Science and Technology., 2006, p. 66-67). The Ministry’s focus on the CRIs now refers to ‘value creation’ and ‘investment’, for instance the following statement appears as a ‘key action’ in the 2006-2009 MoRST statement of intent:

Embed CRIs as leaders of innovation and value creation within their sectors, with an overall emphasis on delivering benefit to New Zealand... This Key Action will allow us to gain greater value from science investments made through CRIs (2006, p. 28).

One of the biggest concerns of scientists about the change in research funding towards an ‘investment and return’ model was the lack of long term basic research science funding allowed within the remit of MoRST. It is generally accepted that the ‘investment and return’ model is short-term focused, generally requiring a fund applicant to be able to

show a potential for significant return over a few years. By contrast, basic research often cannot show this, and therefore does not generally fit well within an 'investment and return' schema. The only fund within MoRST's remit that explicitly states it targets basic research is the Marsden Fund, which is highly contested, and only represents about 5% of the total MoRST budget, or \$34 million in 2005/2006.

Although MoRST is responsible for allocating the majority of specifically research funding in New Zealand, funding is also allocated through the Ministry of Education.

Ministry of Education

The vote Education fund reflects a large amount of the New Zealand Government budget; the total vote Education budget for 2005/06 is just over \$8.5 billion (The Treasury, 2005). The tertiary component of this is about \$1.9 billion, with the specific research funding component (provided by the Performance Based Research Fund (PBRF) and Centre of Research Excellence (CoRE) fund) totalling approximately \$189 million (The Treasury, 2005). The PBRF component is increasing in relation to the total tertiary allocation over the next few years because the traditional funding component, which is based on the number of equivalent full time students enrolled at a particular institution, is being completely phased out by 2007 (Tertiary Education Commission, 2005). Specifically, under vote Education for the 2005/06 funding year the PBRF is allocated at \$163,471,000 and the CoRE fund at \$25,407,000 (The Treasury, 2005).

The Centres of Research Excellence Fund

Generally, the new 'performance based' regime of funding (PBRF and CoRE) seems to have emerged from a series of reports commissioned in 2000/01 from the Tertiary Education Advisory Commission (TEAC). These reports were commissioned in response to a shared concern over a "range of urgent problems facing tertiary education" (Tertiary Education Advisory Commission, 2000, p. 1) during the late 1990s. The Commission's brief was to develop a strategic direction for tertiary education in New Zealand (Tertiary Education Advisory Commission, 2000). TEAC produced four reports, each subsequent report building on the others, constructing a picture and roadmap of the tertiary education

system that could be developed. The PBRF and CoRE initiatives can be traced to this group, though clearly, by suggesting these concepts they were following a trend that had already permeated much of the western education system. The first report, entitled 'Shaping a Shared Vision'; published in August 2000, identified the areas that the commission would investigate in order to recommend action. Within the research arena two particular areas were posited that appear to have led to the development of the CoRE concept. Specifically: "What balance should be maintained, within and across tertiary education providers, between provision of all fields of research and specialisation to create centres or networks of research excellence?" and "How can barriers to collaboration in research within the system, and beyond, be reduced?" (Tertiary Education Advisory Commission, 2000, p. 30).

In their second report 'Shaping the System', published in February 2001, the Commission (TEAC) more explicitly recommended "the establishment or recognition of national centres or networks of research excellence within the tertiary education system, with linkages to a national strategy and the international research community" (Tertiary Education Advisory Commission, 2001a, p. 53). This recommendation, amongst others, was seized upon by the Ministry of Education and the 'Centres of Research Excellence' (CoRE) fund was established in late 2001, with the first round of applications called for on the 2nd of October of that year (Maharey, 2001, October 2), and the consequent first five CoREs announced on the 6th of March 2002 (Maharey, 2002, March 6). The CoRE fund initially held \$60.6 million of operating and capital expenditure over four years. By October 2004 this had been extended to a total sum of \$123 million funding seven CoREs, with a further \$72 million committed through until 2008 (Maharey, 2004, October 13).

The original purpose for the establishment of the CoREs was twofold: To enable the sharing of research related resources; and to encourage networking between researchers within New Zealand (Maharey, 2001, October 4). To achieve this purpose the CoREs were envisioned to be "inter-institutional research networks, with researchers working together on a commonly agreed work programme" (Maharey, 2001, October 4, para. 8).

In the third TEAC report 'Shaping the Strategy', published in July 2001 the commission identified two potential types of centre of research excellence funding. The first 'Model A' type identifies the centre as conducting "world-class research at the creation/discovery end of the spectrum with no regard to discipline, theme, extent of collaboration or nature of research outputs" (Tertiary Education Advisory Commission, 2001b, p. 25), the second 'Model B' type adds the requirement for the centre to be more thoughtful of the application of research outputs and consequent association with industry groups. Although it appears that the first model was preferred by the Ministry of Education the resultant CoREs are a mix of interdisciplinary and inter-institutional centres some of whom are discovery-focused, conducting primarily basic research, and some of whom are explicitly applications oriented.

In August 2006 the New Zealand Government announced a further funding boost to the CoRE fund, with an additional \$30 million in expenditure allocated in the 2007/2008 budget (Cullen, 2006). This has resulted in another round of contestable CoRE applications, with the original seven CoREs reapplying for a further six years of funding and a potential for two additional CoREs to be established, the results of this round will be announced in June 2007.

The current (2002/3 – 2008) Centres of Research Excellence cover a broad range of research areas, including: Evolution and molecular ecology; molecular biology and medical research; advanced materials and nanotechnology; agriculture and biosecurity; growth and development, mathematics, and; Maori development and advancement. In addition to the Allan Wilson Centre the other six centres are: the Maurice Wilkins Centre for Molecular Biodiscovery; the New Zealand Institute of Mathematics and its Applications; the National Institute of Research Excellence for Maori Development and Advancement; and the National Centre for Growth and Development – all hosted by the University of Auckland. The MacDiarmid Institute for Advanced Materials and Nanotechnology hosted by Victoria University of Wellington and the National Centre for Advanced Bio-Protection Technologies hosted by Lincoln University.

The CoRE model is based around several 'primary investigators' whom remain employed by their host institution but are partially funded through the CoRE money. One could see this arrangement as simply another form of institutionalisation, one that creates self-selected institutionally backed cross institutional virtual 'department' like structures for the academic elite; offering 'protection' for them from activities such as teaching. However the funding model differs between the researchers in the CoREs; some are entirely funded through the CoRE whereas others remain partially or mainly funded (30-70%) by their university. It also seems that many of the investigators involved (at least in the AWC) choose to remain in teaching positions, possibly out of their desire to see the next generation coming forward suitably prepared. The CoREs self-select their own members; though the networks being supported certainly, to a degree, pre-determine the membership. Others investigators are invited by the CoREs originators, these people often add research or disciplinary 'clout' to the application. When funded each CoRE determines its own distribution of research money, the models for doing this differ significantly across the existing CoREs; from democratic models to contestable schemes.

As outlined in the introduction my research focuses on the Allan Wilson Centre for Molecular Ecology and Evolution, which describes itself as an interdisciplinary research centre that brings together scientists from ecology, evolutionary biology and mathematics to investigate issues of significance to New Zealand. The Allan Wilson Centre officially involves researchers from five different university institutions in New Zealand, and if successful in furthering their funding in the 2007/2008 round one Crown Research Institute.

This section has discussed the New Zealand research policy environment within which the CoRE fund has emerged, the following section discusses two key concepts that impact on the ordering of the Allan Wilson Centre and other institutes of scientific research, these being the nature of discipline and the production of knowledge.

2.1.4. Discipline and the Production of Knowledge in Research

This section discusses the history and current thinking related to academic disciplines and knowledge production. Knowledge production in this sense is specifically in reference to

the research activities conducted in academic, industrial and government R&D settings. The section begins by discussing the form, function, power relations and culture of disciplines, it then introduces notions of interdisciplinarity and finishes with a discussion of the contemporary nature of knowledge production.

Discipline

Often quoted is Richard Zare, co-founder of the BioX institute at Stanford University:

Knowledge is extracted from a fully integrated world. Knowledge is 'disintegrated' by disciplinary units called Departments in Universities. How can knowledge, discovery and dissemination be re-integrated? (Caruso & Rhoten, 2001, p. 4).

Disciplines are a relatively new emergence in the institutional structures associated with the production of knowledge. Klein notes that disciplines, in the form they are currently seen, emerged only during the late 19th and early 20th century when between 20 and 25 distinct disciplines emerged from the milieu of science (Klein, 2004). Prior to this 'science' was more likely to be done by generalist 'gentlemen scientists' who would not identify with a particular specialty other than the specialty of science. Klein believes that there are two main categories of features that differentiate different disciplines; a functional demarcation and a power demarcation. Functionally each discipline has separate and "specific objects and subjects in the curriculum, bodies of evidence, laws, concepts and exempla as well as methods, and even their own separate language systems" (2004, 'Elements of disciplining' section, para. 3). The power demarcation involves the use of power in the formation and reinforcement of disciplinary knowledge. As Klein notes:

Disciplines control not only accounts of their histories but the kinds of questions we ask and the kinds of answers that will be believed and accepted. The disciplines also control resources; they control identities, patterns of education; they constitute labour and employment markets; and they constitute economies of value determining what kind of work people will do and what kind of work is legitimate or not. (2004, 'Elements of disciplining' section, para. 5)

So not only do disciplines functionally divide an integrated natural world into 'subject areas' they also, as structures of power, can prevent the reintegration of knowledge by utilising their structures to define knowledge, history, education and scientific progress. As Max-Neef notes, members of disciplines (he calls them Professors and disciples) can have a profoundly developed sense of loyalty to their discipline; so developed that academics often perceive that their discipline is more important, than any other, to the success of their institution, be that university or government. (2005).

By looking at the history of the disciplines as they exist in contemporary academia we can see how their development has been as a result of the changing social conditions of the times. Klein identifies several main social forces driving the disciplining of knowledge during the late 19th century, these include: the evolution of modern natural sciences; the 'scientification' of knowledge; technological advancements and the industrial revolution (Klein, 1990). At the heart of these forces is the specialization of labour. Driven by the industrial revolution the concept of specialisation of labour pervaded all industry, including academia, in the 18th and 19th centuries. The results of the industrial revolution called for specialist academics that were knowledgeable in particular areas, often this was (and still is) for practical reasons such as requiring specialised individuals to utilise expensive and complex equipment (Klein, 2004). The result of this specialised disciplining is that many academic researchers become mono-disciplined actors isolated from other mono-disciplined actors, as Max-Neef describes "one person may, in fact, study biology and handle it well without the need for knowledge about physics or psychology." (2005, p. 6).

So it seems rather clear that the modern disciplines are built around dominant 'blocks of knowledge' about a particular subject matter. Biology and Chemistry for instance have different 'knowledge' about an integrated human object; they consider the object of their study in different ways, utilising different (complementary *or* contradictory) knowledge bases. It stands to reason that, just as a language is subject to cultural formation, a set of ideas or a knowledge base is likewise constituted within the bounds of a culture. In fact McDonnell argues that forms of knowledge are simply cultural productions: "a form of knowledge culture comes with, in fact is constituted in, a form of language, a custom of

practice, an economy of means, a structure of power, a rule of justice, an archive of narratives of identity and tradition” (McDonell, 2000, p. 27). Given this focus, different disciplines create different forms of knowledge which act like different languages or at least different dialogues of the same language. If I don’t speak that dialogue well, or at all, then how can I understand and share the culture? This would account for the difference and conflict between disciplines that has been noted since C.P. Snow spoke of “the two cultures” in his 1959 Rede lecture.

A good example of this concept in practice can be seen in the work of Sheldon Krimsky, given this lens through which to view discipline we would expect researchers from different disciplinary backgrounds to interpret things like ecological risk in different ways; Krimsky gives the example of scientists from the discipline of molecular genetics, reducing the risks associated with the production of recombinant DNA (DNA which has been genetically modified) to a specific molecular concern, whilst ecologists, whom are generally less molecularly and more whole organism focused in research approach consider the more general environmental concerns (Krimsky, 1991). As Krimsky notes:

“Each discipline and sub-discipline is serviced by its unique vernacular and conceptual architecture through which it describes and interprets some component of nature. It should not come as a surprise that representatives of the respective disciplines would approach the problems of risk differently” (Krimsky, 1991, p. 137)

This is just one example of how a scientist’s disciplinary background can cause cultural conflict when confronted by the view of another scientist who in effect ‘speaks another language’. This is considered more in the section on institutional collaboration, included later in this chapter, as Jakobsen et al (2005) found a similar effect when researching the formation of an interdisciplinary centre. The following section considers the notion of interdisciplinarity in more detail.

Interdisciplinarity

Despite the obvious functional, cultural and political barriers to interdisciplinarity described in the above sections, the concept is not a new thing. The ‘problem’ of interdisciplinarity is essentially a problem of knowledge integration, and this has existed,

in various forms, since the disciplining of knowledge first began (Klein, 1990). Although disciplining occurred in both the natural and social sciences at around the same time; the social sciences, during their development, drew from the natural science ontological and epistemic models to conduct their research. These models, primarily positivist in nature, accentuated the necessity for specialisation of knowledge through their reductionist philosophy, and amplified the speed and differentiation of the process of the disciplining of knowledge, it stands to reason then that some counts now put the number of disciplines and sub-disciplines at over 8500 (Klein, 2004).

The formation of interdisciplines in the natural sciences has been occurring as long as the formation of disciplines. I think this is because the social nature of science is one where disciplines naturally emerge as structural, social and political entities, most often called university departments. Palmer (1999) discusses Dogan and Pahre's (1990) concept of 'specialization-fragmentation-hybridization' to describe the formation of interdisciplines and interdisciplinary structures such as research centres. Dogan and Pahre's idea was that specialisation occurred creating a discipline that eventually was institutionalised into the 'university department' form. Over time fragmentation will split these disciplines apart and they reassemble into hybrid entities, taking "the form of crossdisciplinary research topics or networks of interpersonal contacts" (1999, p. 242) which eventually will coalesce into a socially accepted discipline, and so the process continues.

Along the lines of the thesis put forward by Dogan and Pahre (1990) above Klein discusses the example of the formation of biochemistry and molecular biology. Although Klein presents a fairly social constructionist representation of this process I think it is useful to see how disciplines and interdisciplines operate by using the analytical lens of actor-network theory – in this way we can see how the histories and present structures of the disciplines can influence the process of science.

Biochemistry (now a nominally distinct discipline) in the early twentieth century arose because of knowledge chemists required of the biology of metabolism, and over time was constructed into a discipline by the various actors promoting it as disciplinarily distinct. As Klein describes, biochemistry now has "its own level of inquiry (chemical reactions

involving macromolecules that perform physiological functions), its own theoretical schemes (most notably the citric acid cycle), and its own research problems and techniques” (Klein, 1996, p. 81-82). It also has departmental structures, degree qualifications and specific journals, all working to embody and perform its role as a distinct discipline in the project of science.

The formation of molecular biology can also be seen through the lens of actor-network theory. You could argue that molecular biology can provide us with a contemporary understanding of interdisciplinarity; by suggesting that it is simply a reformulation of fragments of existing disciplines into a new discipline, and that this process is based on paradigmatic knowledge advancement. In other words, a paradigm changed, because of the advent of molecular technology, and various pieces of a range of disciplines, including physics, biology, chemistry and biochemistry, were required to take advantage of this paradigm change (Klein, 1996). However, along with the technological advancement, a social and political element seems to have been important to molecular biology’s rapid rise, consider the following statement from Klein:

Molecular biology also illustrates the link between cognitive development and social control of institutional mechanisms and assets. The rise of molecular biology in the 1960s was predicated on a range of preceding and succeeding policy actions that enabled ongoing contacts between biological and physical scientists, especially bacterial geneticists and X-ray crystallographers of proteins and nucleic acids (1996, p. 83)

So clearly the various actor-networks supporting or resisting the formation of an interdiscipline play a significant role as it becomes integrated into the milieu of disciplines already in existence. Frickel (2004) provides an excellent example of this in action as he analysed how the interdiscipline ‘genetic toxicology’ emerged into the science world during the 1960s and 1970s. Frickel uses the actor-network concept of ‘translation’ to describe how the human actors who were potentially placed to promote or support the disciplining of genetic toxicology realised that their “own specific interests, for example, in understanding the causes of birth defects, were best served by adopting the similar goals or employing the similar practices as genetic toxicology promoters” (p. 274). I think this clearly shows how the disciplining of knowledge in the natural sciences

is heavily reliant on the interplay between the various, competing actor-networks involved in its establishment.

The interplay described above can also be seen in the formation of interdisciplines in the social sciences. Postmodernism, according to Klein, has accelerated interdisciplinarity within the social sciences (1990). Saussurean linguistics and its use by poststructuralist theorists' such as Foucault and Derrida brought to bear a social constructionist ontological framework on the social sciences since the 1960s, somewhat displacing the traditional positivist natural science models with a more postmodern critique. An example of this is the formation of social psychology; this required psychologists to discard the natural science models traditionally favoured by psychology and embrace postmodern alternatives such as narrative analysis, critical discourse analysis and textual deconstruction.

In the above section I have discussed, in broad terms, how interdisciplinarity and the formation of various interdisciplines' seem to be reliant on the social and political networks involved. In many ways the human actors involved in the institutional forms that promote interdisciplinarity embody and perform the role of the 'interdisciplinary scientist'. It seems that one of the main mechanisms used to perform this role is that of the boundary object and more generally the concept of boundary crossing; this is discussed in the following section.

Boundary Crossings and Boundary Objects

Boundaries seem to be a natural part of society. They emerge to separate and differentiate social systems, but like the systems they protect they are also social and political constructions that bend, flex and break as the circumstance that created them change. In this section I briefly consider two literary perspectives on boundaries and boundary crossing, the first is a more general view based on Klein's academic research into interdisciplinarity, the second is specifically in the actor-network genre and considers more fully the concept of the boundary object. Both of these approaches are useful as they better inform the 'how' of the question: 'How does change 'really' occur in the institutes of science'?

Klein, in her 1996 book *'Crossing Boundaries'* utilises Tom Paxson's four levels of disciplinary interaction to describe how boundaries between disciplines are crossed during academic enterprise. Level 1 involves the borrowing of "tools, instruments or techniques" (p. 62) from a different discipline, Klein gives the example of a Chemist using a Physicist's mass-spectroscope for instance. Level 2 involves a more symbiotic relationship where two disciplines exchange theory; the on-going relationship between economics and political science is given as an example. Level 3 signifies two disciplines "growing toward each other" (p. 65) by "forming an interface of theories and subject matters" (p. 65) and level 4 signifies the emergence of an interdiscipline – this process occurred between parts of biology and chemistry between 1920 and 1940 which resulted in the formation of biochemistry. This started as a sharing of tools, then a sharing of concepts and theories (such as the citric acid cycle discussed earlier in this chapter) and eventually they formed a solid disciplinary theory.

The work above, represented by Klein, mainly focuses on the concept of boundaries between disciplines in the traditional interdisciplinary research studies type literature. Star and Griesemer (1989) develop an approach for considering the notion of boundaries and specifically boundary objects in an approach informed specifically by the actor-network theory of Callon and Latour. To me this work is very complementary to Klein's as it talks about the specifics of the interactions across boundaries – the objects that exist at the boundaries of disciplines that enable interdisciplinarity to occur, and therefore provides a useful analytical tool, informed by actor-network theory.

Star & Griesemer define boundary objects as those things which translate scientific information between two or more different 'social worlds'. The concept of 'worlds' is important in Star & Griesemer's work, it represents the different social groupings of people and things involved in their research site, the Museum of Vertebrate Zoology at the University of California, Berkeley. Different world views are represented by: The Museum director, Joseph Grinnell; the financial patron Anne Alexander; various amateur collectors; animal trappers and the UC Berkeley's administration. Boundary objects, through a process of translation, transmit information between social worlds. Here they

describe the process of the development of boundary objects using Callon and Latour's notion of translation:

The trick of translation required two things: first, developing, teaching and enforcing a clear set of methods to 'discipline' the information obtained by collectors, trappers and other non-scientists; and generating a series of boundary objects which would maximize both the autonomy and communication between worlds (Star & Griesemer, 1989, p. 404)

California for instance was identified as a boundary object by Star and Griesemer (1989) as its geographical, spatial and ecological features could be used *between worlds* to communicate scientific information. To me the notion of boundary objects is essential to understand how disciplines, which I argue can be compared to Star & Griesemer's 'social worlds', communicate across their boundaries.

Having looked briefly at boundary crossing and objects that assist I want to expend a few words on 'border protection', that is the devices used, sometimes unwittingly, but sometimes not, by discipline affiliates to protect their borders. Sharon Traweek writes a compelling history of the development of discipline and institution form and concludes with the acknowledgement that although interdisciplinarity is growing in action, in society the disciplines still maintain a stronghold in the attribution of authority:

The departmentally based disciplines still appear to control the definition of intellectual authority: even faculty positions funded through the centers usually require disciplinary affiliation. Many faculty firmly situated in the disciplines smugly announce to their students that very few with an interdisciplinary degree can get good jobs (Traweek, 2000, p. 45)

From these comments I feel safe concluding that the disciplines attribute both intellectual authority and also *institutional* authority, it is dangerous and difficult for students and scientists alike to disrupt disciplinary boundaries; it has consequences.

Given the above discussion on disciplinarity and interdisciplinarity, and all that they encompass, I want to turn now towards the more general notion of 'knowledge production'. Disciplining knowledge and then trying to reintegrate it through processes of

interdisciplinarity is inextricably intertwined with the theories of knowledge production, the following section discusses 'new' theories of knowledge production.

New Theories of Knowledge Production

In 1994 Gibbons et al published a book entitled 'The New Production of Knowledge: The Dynamics of Science and Research in Contemporary Societies'. This work suggests that the production of knowledge in contemporary society is shifting from a traditional (Mode 1) to a new (Mode 2) mechanism. Gibbons et al portray Mode 1 knowledge production as that conducted within a traditional disciplinary context (Turpin et al, 1996), the characteristics of this type of knowledge production include the advocacy of the traditional 'scientific method' within disciplinary inquiry, including established methodologies, publication formats and peer review processes and a marked distinction between fundamental theoretical science and applied practical science. The characteristics of Gibbons et al's Mode 2 knowledge production by contrast to their Mode 1 are distinct. Mode 2 is naturally transdisciplinary; research, fundamental or applied, is conducted with a use in mind and the use of that research in turn drives further research in a continuous cycle (Gibbons, et al, 1994). This can be contrasted with Mode 1 knowledge production where fundamental research and applied research are discrete from each other, intellectually and institutionally.

It seems likely that a form of Gibbons et al's mode 2 knowledge production is increasingly becoming accepted, even driven, by research funding agencies. Certainly in New Zealand the marketisation of the research and science sector has resulted in funding systems focusing on the practical relevance of proposed research before 'investing' in a research project (Leitch & Davenport, 2005).

The Gibbons et al model has come under some distinct criticism, particularly for the lack of empirical evidence supporting their theory (Simpson, 2004) but also for the premise that Mode 2 knowledge production is in fact not anything new, but just a recapitulation, in a contemporary context, of the original format of scientific discovery prior to its 'institutionalization' by the academy in the 19th century (Etzkowitz & Leydesdorff, 2000, p. 116). Barbara Simpson studied four CRIs in New Zealand to try and assess how the

new public management reforms in the science sector had changed the organisation of science. Her conclusions point, not to a switch to a type of mode 2 knowledge production, but in fact to a different institutional format surrounding extant scientific process. In other words although the institutional form of science had changed the working practices of the scientists did not (2004). What is particularly interesting about Simpson's study is her conclusion that the transformation from mode 1 to mode 2 type knowledge production outlined by Gibbons et al is not a particularly relevant distinction in a New Zealand science sector context:

However, the changes undertaken by the CRIs in this study provide little evidence to support the single destination transformation implied by Gibbons et al. (1994). In as much as the CRIs were designed to bring together scientists from different disciplines to work with end-users on solving applied problems, their design criteria certainly fit with a Mode 2 orientation. However, the government laboratories that were the predecessors of the CRIs were equally charged with undertaking a broad spectrum of science activities to solve practical problems for the benefit of the nation. As a small country with limited science resources, active networks that spanned disciplinary and organisational boundaries were always essential to the conduct of research. This suggests that Mode 2 knowledge production was just as evident in the pre-reform government laboratories as in the post-reform CRIs (2004, p. 264)

Simpson prefers the Etzkowitz & Leydesdorff 'triple-helix' model for describing the changes undergone in the method of knowledge production in the New Zealand science sector post-reforms. This model provides an alternative explanation for the changes in knowledge production purported by the Gibbons conceptualisation; they describe an emerging 'triple-helix' of academia, industry and government relations (1997). This emerging model is driven by the changing role of the universities in contemporary society and the associated "withdrawal of government from its previous role of controlling the interactions between academia and industry" (Simpson, 2004, p. 264).

I am more currently more convinced by Simpson's perspective that the Gibbons et al's mode 1 to mode 2 model, mainly because I agree with Simpson's analysis of New Zealand's pre and post reform science institutions. But either way there has been an

alteration in the institutional forms that surround the production of knowledge, the following section discusses this institutional change in academia in more detail.

2.1.5. Institutional Change in Academia

From the above sections we can see that traditional research institutions (including universities and government research centres) are generally organised along disciplinary lines. It is also clear that specifically within the scientific disciplinary boundaries there occurs a further differentiation between fundamental and applied sciences. For instance Massey University, in 2006, within its College of Sciences, has an Institute for Fundamental Sciences (including chemistry, physics and mathematics), and then several other institutes for more 'applied' sciences, including food nutrition and human health, and veterinary, animal and biomedical sciences. This pattern is repeated in the structures of most universities, it is the dominant mode of ordering university departments. Government research centres have also been organised along traditional disciplinary lines, though in New Zealand since the economic reforms of the 1980s these structures have begun to change (Leitch & Davenport, 2005). Institutional theory supports this structural formation; DiMaggio and Powell for instance assert that homogeneity emerges in the evolution of institutional forms. Particularly they claim that once organisations in the same line of business are "structured into an actual field, powerful forces emerge that lead them to become more similar to one another" (1983, p. 148).

The brief consideration of institutional theory above seems to reinforce why universities and government research laboratories are ordered in the way they are, when you combine this with the power of discipline to order discussed earlier in this chapter it becomes obvious how these institutions managed to develop their disciplined boundaries. Clearly people perceive that there are substantial problems with this type of structure, as it does not enable 'mode 2' type 'knowledge' production to occur. In other words, transdisciplinarity is not encouraged by disciplined university and government research institutes, and transdisciplinarity is performed in the current research climate as the Holy Grail of exemplary research – Houston, we have a problem! It stands to reason therefore, that much of the research into institution change in academia focuses on the issues associated with collaboration between departments and institutions – because

collaborations break down disciplinary and institutional barriers, or so the story goes. This section takes a critical look at some of this literature to reduce its use in understanding what is 'really' going on in these inter-institutional structures.

In a recent book '*Facilitating Interdisciplinary Research*' published by the National Academy of Sciences, National Academy of Engineering and Institute of Medicine of the National Academies in the U.S. the authors report the results of an extensive research exercise involving surveys and interviews with academics and researchers in U.S. research institutions. This study was focused on the facilitation of interdisciplinary research within an academic or research institution. When asked to identify the institutional barriers to interdisciplinary collaboration within their institution the participants' answers were concentrated in several factors: Limited resources; the academic reward system; different institutional cultures; program evaluation; different departmental policies and procedures; lengthy start-up times, and; decentralized budget strategies (National Academy of Sciences, National Academy of Engineering and Institute of Medicine of the National Academies, 2005). The findings of the National Academies' research are not dissimilar to the literature identifying barriers to collaboration generally. For instance, Grey identifies: institutional disincentives; historical and ideological barriers; power disparities; societal-level dynamics; differing perceptions of risk; technical complexity, and; political and institutional cultures (Grey, 1989).

The main problem with the National Academies research findings is that they focus on the accounts of the people involved in the interdisciplinary research centres; and this is only one angle from which to consider collaborations between institutions. It is well known that human actors generally are acting within certain dominant discourses, for instance, the 'problems with collaborations' discourse might be well known within academia and therefore will be 'performed' by the human actors within the interdisciplinary research centres. Other forms of enquiry will allow a better understanding of what is 'really' going on.

Jakobsen, Hels, & McLaughlin (2004) add a different dimension to the understanding of barriers and facilitators in collaborative scientific structures. Their study was a cross country comparison of two interdisciplinary research projects. Their results show a more useful four way model for understanding those things that impact successful collaboration. Based on the concept of boundaries, which they define as the "intangible lines that separate person from person, team from team, team from organization, and organization from organization" (Jakobsen et al, 2004, p. 17) the authors identify those factors that act as barriers and/or facilitators to collaboration at each collaborative level; the levels being: individual interactions; group interactions; organisational interactions, and; mixed group and individual interactions. An interesting aspect of this model is the ability to extract the individual from the team and the team from the organisation to more easily identify where the barriers and/or facilitators 'really' exist within the collaborations.

What I find particularly salient about Jakobsen et al's findings is their identification of the randomness of applicability of barriers and facilitators to collaboration between institutions in their study, in that in two different collaborative enterprises some things identified in one as barriers were described as being facilitators in another. Jakobsen et al put this down to something they call a 'science culture' and its relationship to the organisational context, this they claim is a unique finding, stating: "we did not identify any previous research that discusses the importance of the science culture, and the fact that some influences can act as both facilitators and barriers depending on the context." (p. 29). To me what Jakobsen et al were dealing with was the well documented clash between the discourse of science and the discourse of management, often manifested in the government laboratory – university scientist conflict. Evidence for this can be seen in Jakobsen et al's writing, for instance one sentence reads: "As one of the interviewees in the ICBEMP study phrased it: 'Even though they (agency scientists) work for the agency, their peers are in the universities'" (p. 28).

Jakobsen et al also described a disciplinary identification that created issues for the management of the interdisciplinary collaborations, because language barriers emerged among scientists of different disciplinary backgrounds.

The vast majority of academic and government researchers have been trained in a university in one, or possibly two particular disciplines. Additionally, in most circumstances those same researchers are employed within a departmental or institutional structure that is organised around a discipline. As noted in the first section, funding for research has traditionally been dispersed along departmental (and therefore disciplinary) lines (Awbrey & Awbrey, 2001). However it is becoming more common for research funding to be dispersed to interdisciplinary research centres; anecdotally it seems that this improved funding is at the expense of the traditional department rather than in addition to it. Consequently one would expect to see more researchers being attracted to centres, rather than university departments, to gain better access to research funding. The following research considered the impact of this change on the institutions and the researchers within.

Garrett-Jones *et al* (2005) utilised a risk based model to try and understand how the formation and function of the CRCs in Australia impacted on the institutions and researchers involved. The authors report the results of a preliminary interview based research project aimed at identifying “how researchers manage to work effectively within the two spheres – that of the multipartner collaborative centre and that of the research agency or university department that employs them” (p. 536). The scene of their research, the Australian Cooperative Research Centres (CRCs), are described as collaborative, cross-sectoral, inter-institutional structures that seek to bring researchers together from different institutions in an integrated research team whilst retaining their official institution position (Slayter, 1994). The CRC model has significant similarities to the NZ CoRE model, as, like in the CRCs, the researchers within the CoRE centres retain their official institutional positions and both the CRC and CoRE programmes have a mix of applications orientated and discovery orientated research centres.

The Garrett-Jones *et al* model separates the risks associated with being a member of a CRC firstly into three risk categories; institutional; scientific and academic, each with associated components. They then transpose two further dimensions: The organisational and individual levels where the impact of the risk is felt. They represent the model diagrammatically as indicated in figure 1.1 below:

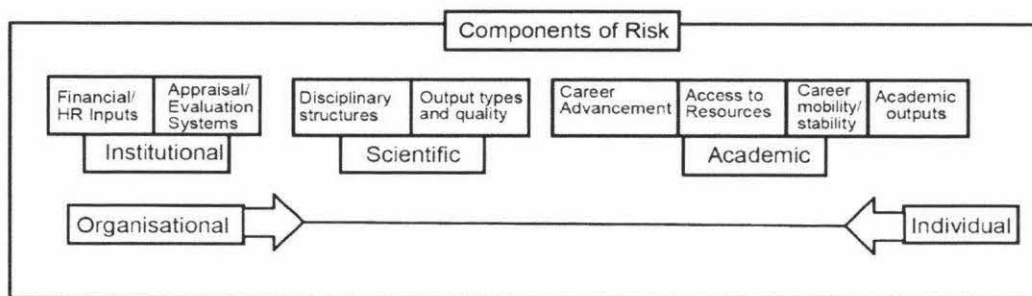


Figure 1: Components of Risk Diagram

Taken directly from: Garrett-Jones, S., Turpin, T., Burns, P., Diment, K. (2005). Common purpose and divided loyalties: the risks and rewards of cross-sector collaboration for academic and government researchers. *R & D Management*, 35(5): 535-544.

The results of their study are grouped into four categories; the benefits associated with the being in the centre; the problems associated with being in the centre; the effect on research careers and the management strategies used in the centres.

The benefits the members associated with being in the centre were concentrated in several main areas: Firstly the status that is associated with being a member of the centre was viewed by the participants with some pride. Secondly their association with the centre often supported a long-term research focus which is not possible with many industry partners. This enabled researchers to develop long-term research projects with an iterative cycle (p. 540) to facilitate the often lengthy process of science. It also enabled the researchers to be associated with any resultant commercialisation phase. Thirdly professional administrative and project management support was provided by the centre, this was not discussed as being provided by traditional departmental structures. Fourthly, particularly for PhD students and post-doctoral fellows, centres provided training and career development opportunities exceeding those of traditional departmental structures. Finally the ability to work within a multidisciplinary team and therefore the ability to access multi-disciplinary expertise was seen as a major benefit (Garrett-Jones *et al*, 2005).

The problems members associated with being in the centre can be concentrated into three major areas: Firstly the industry affiliation was perceived as potentially impacting negatively on the centres priorities, particularly by influencing the development of the

research programme and by trying to exert influence to increase the pace of commercialisation. Secondly resource issues, particularly researchers balancing the time commitments between their employing institution and the centre, and difficulties with accessing funds for conferences from the centres. These were more readily available to students and post-doctoral fellows, who were employed directly by the centre. Finally researchers could become inextricably intertwined with the centre, making their extraction very difficult (Garrett-Jones *et al*, 2005).

I think that these results, like Jakobsen *et al*'s, demonstrate the conflict that exists between the cultures of the university and of the more commercially focused government laboratory, managed according to new public management ideology. From an actor-network position we see that these two cultural positions could be represented as two modes of ordering competing for dominance in the inter-institutional structure.

The effect on a researcher's career was both positive and negative, there were benefits of being associated more closely with industry, particularly because the exposure opens the researcher's eyes and broadens their research experience creating more opportunities for the researchers and students with centre experience because their skills are more in demand (Garrett-Jones *et al*, 2005, p. 542). However negative factors included concern because promotion opportunities in the employing institution were not generally related to the researcher's involvement in the centre. Also research outputs were a source of concern as publications could be delayed by centre responsibilities (like commercial activities and IP issues), therefore affecting promotion and grant applications. Also certain publications promoted by the centre may not correspond with the performance indicators of the host institution, and therefore will not carry the same benefits (Garrett-Jones *et al*, 2005).

The above again represents two different and competing modes of ordering operating within the two types of organisation involved in the inter-institutional structure. One where the 'top journal' actor heavily influences publication path and one where the 'commercial product' heavily influences a different path, one where publication is nice if it doesn't interfere with Intellectual Property.

Finally the management strategies employed by the centres led Garrett-Jones *et al* to identify that the CRCs are ‘virtual’ organisations ruled by “consensus and mutually adopted research strategies and programmes” (p. 543). Centres often would establish a separate entity to house their research programme but commonly “their researchers [are] widely distributed both institutionally and geographically” (p. 537). Management effort then is generally focused on trying to negotiate agreement amongst members on strategic research plans.

Conclusion: What is ‘really’ going on?

The research that is presented in the above section does not really answer *how* these new institutional arrangements have or have not been successful; because it does not explore the intricacies of their ordering arrangements, and *does* privilege the role of the human actor over other informants. The research does however point to some useful points of analysis and is appropriate for gaining insight into employee relations. In contrast to the literature discussed above I present in the following section literature that utilises an actor-network approach to consider how individual organisations are ordered given the public management environment that has arisen in the past twenty years. It is through this literature that I develop my theoretical and analytical framework.

2.2. Ordering Organisations

There are several ‘types’ of organisation that can help to understand how the Allan Wilson Centre is ordered. These organisations share certain structural or functional similarities or have been impacted by similar organisational movements. In this section I introduce three studies that have used a mode of ordering approach to study such organisations; these researchers have established, at least at the time of study, how these organisations were ordered.

2.2.1. John Law: Narratives of a Public Science Institution

Law’s conceptualisation of the organisation is of one where organisational participants are ‘performed through modes of ordering’ and in turn ‘perform’ the organisation by subscribing too, performing through their organising routines, and therefore reinforcing,

several competing organisational modes of ordering. This conceptualisation of organisation and ordering has emerged with the uptake of a more critical perspective into organisational studies since the 1970s. As Law describes, this has had a decentring effect on what was previously 'known' as organisational certainties (Law, 1994b). This critical perspective has been influenced by post-structural and postmodern conceptualisations of the nature of organisation. Following, amongst others, Saussure, Foucault, and Derrida, concepts of language, discipline, knowledge and power have been cast in a more critical light exposing the myriad of complexities at play within organisational society (Calas and Smircich, 1999). An organisation is now "not one thing, but is many" (Law, 1994, p. 248). If bounded at all, organisations have at best uncertain boundaries and by their nature are "chronically incomplete" (p. 249).

This way of looking at organisation is in my view liberating, as it prevents blind oversimplification of what are highly complex organisational processes into stable entities. Law describes organising as a "reactive verb; its expression represses, or suppresses, sources of energy which it seeks to deny or cannot know" (p. 249). Although liberating, it does call into question how you can study these processes, if you can't apply the modern method of progressively building a picture of stable organisation, of what it 'should' be like, how do you make sense of an entity like the Allan Wilson Centre?

Law studies the modes of ordering organisational life; his influential ethnographic study of the management of the Daresbury SERC Laboratory, a publicly funded UK science organisation, identified at least four competing modes of ordering at play within the institution. These were: enterprise; administration; vision; and vocation (Law, 1994a). Law stresses that any attempt at ordering is a simplification, a deletion of everything else for the pleasure of adopting, at least for a instant, a stable form: "It's what ordering is about: ignoring; simplifying; fixing what is complex for a moment in a stable form, reifying" (1994a, p. 132). Consequently each of these modes of ordering the Daresbury Laboratory should not be seen as 'the four parts of the organisation' but instead as four competing orderings used by the actors in the network to perform the organisation. When they embody one, they by necessity ignore the others.

Organisations are not bounded entities; they are impacted by their history and their environment. Similarly orderings carry environmental and historical significance; this means that the modes of ordering the Allan Wilson Centre will embody the things that make up their history and their environment – including the impact of being formed primarily by colonial university scientists. As such the orderings Law experienced in 1990 in a UK publicly funded science institution are directly useful in making sense of the various narratives in operation in the Allan Wilson centre. The intricacies of the orderings experienced by Law in the Laboratory are discussed further below.

Enterprise

To restate a point from above; by emphasising a particular mode of ordering, by necessity other various modes of ordering are deleted to make room for the, often contradictory, intricacies of the specific mode to emerge. Law often talks about enterprise in opposition to the (scientific) ‘vocation’ mode of ordering, and it is in these terms where I find it extremely useful.

The enterprise narrative Law experienced is a narrative akin to an entrepreneurial discourse, as Law explains: “This kind of narrative told how agents – heroes and organizations – are or should be adaptable, sensitive, and able to capitalize on shifting opportunities and demands” (1994b, p. 255). Enterprise talks about the importance of keeping up appearances, of appearing to be enterprising. The Daresbury Laboratory, at the time of Law’s ethnography was in a state of turmoil, faced with the practical implications of Thatcher’s new public management reforms there was a clear struggle taking place between trying to perform the functions of a science lab at the same time as performing a picture of efficient organisation. Clearly the enterprise mode of ordering the Daresbury Laboratory is in line with organisations operating in unregulated markets, where performance can be measured and is rewarded.

Law tells a story about a visit to the Daresbury Laboratory by the Prime Minister of the time Margaret Thatcher. Daresbury’s management team found itself in a state of quandary as they wanted to show Thatcher ‘the SR ring’ (Synchrotron Radiation ring) which signified, to the scientists and technicians, the use and value of the facility. Law

quotes one of the senior managers: “the problem is the area is a pile of steel and concrete. It is a mess. It is not [meant] to be seen” (1994a, p. 172). As Law notes the problem was that the SR ring does not embody and perform an enterprise mode of ordering “surely it was intended to perform and embody quite other ordering arrangements” (p. 172).

It is obvious then that material things (such as the SR ring – which was designed, presumably, to be vocationally efficient rather than heroically enterprising) clearly gain agency. Just as certain organisational members would not be encouraged to talk to Thatcher as they, as actors, embody modes of ordering that do not fit the ‘enterprise’ mode that Thatcher represents for Daresbury.

One of the key aspects of enterprise as experienced by Law is its duality; front-stage heroic enterprise performances afforded by back-stage preparations. An example cited by Law is a decision to collaborate with other scientific institutions; the enterprise mode is embodied in both the back-stage negotiations that allow the formal front-stage collaboration to take place (1994a).

Administration

Following Max Weber’s concept of bureaucracy the administration narrative identified people within a hierarchical structure, with measured responses and concern for due process. This narrative “told of, embodied and performed the well-regulated organization with its smoothly-working arrangements of people, files and machines” (1994b, p. 256). Administration in the Daresbury Laboratory was embodied in actors performing “a particular version of due process” (1994a, p. 79), a case for the human, non-human non-divide is compellingly told in this mode of ordering the laboratory’s safety systems. Law describes the administration of safety as comprising a highly systematic combination of human and mechanistic elements:

Parts of the network are embedded in the bodies of the technicians – and in their actions. Some are built into the circuits, the switches, and the relays of the SR Source safety system. But, for the moment, it’s the *continuity* that impresses me most, the continuity between the different materials of the network as they perform the gradients that strain towards dualism. (Law, 1994a, p. 145)

It seems that, in Daresbury at least, the most stable ordering was embodied in the processes of administration, processes that order actors within the network in a highly regulated way.

Vision

In Law's ethnography this mode of ordering was the most slippery. At times, in *Organizing Modernity*, he questions whether vision is a mode of ordering in itself or instead a "mode of being" (1994a, p. 88-89) that informs the other modes, i.e. I can embody and perform a visionary administration role, a visionary vocation role and a visionary enterprise role. Law doesn't come to a conclusion on this – and this does not really matter. The aspects of vision that made it so prevalent within the Daresbury Laboratory related specifically to the attributes of the Director of the SERC facility, Giovanni Alberti. This narrative follows somewhat the traditional understanding of the 'visionary leader' or of 'transformational leadership' accepting that it allows for both the born charismatic and of a constructed visionary, one who "passed through a process of apprenticeship" on the way to becoming someone "of charisma, grace, single-minded necessity, genius, and transcendence" (1994b, p. 257).

To stand vision apart from the other modes of ordering Law tells of scientists (and managers – in many cases both) who treat mundane organisational (or administrative) matters with indifference because they hold some "privileged access to ultimate truths" (1994b, p.257). This access allows the visionary to delete things that are critical to other modes of ordering – for instance Daresbury's organizational charts – which are critical to the processes and procedures privileged in the administrative mode of ordering, as Law describes: "The single-minded grace of vision blots out other possibilities or complexities" (1994a, p. 117).

Law speculates that vision and vocation might be inextricably intertwined at Daresbury, partly because of the inherent hierarchical authority attributed by the vision mode, either you have vision, you partake of vision or you don't (1994a). Similarly in vocation, either you have a PhD, you are studying towards a PhD, or you aren't – the importance of hierarchy is the same.

Vocation

The vocation mode of ordering stands in stark contrast and in stark similarity to the enterprise mode of ordering the Daresbury Laboratory. This vocational narrative stressed the importance of scientific training (PhDs) and laboratory experience in performing an organisational role, as Law notes: “Here the agent is seen as a carefully trained puzzle-solver who embodied skills and sought solutions that were both creative and conservative” (1994b, p. 258). Enterprise and vocation do not sit comfortably in Law’s ethnography – though there is recognition that they are bound together. They do not sit comfortably because “enterprise is pressing on vocation; enterprise does not really understand profession” (1994a, p. 90, emphasis in original) but “a rub exists” because “in our brave new world, profession depends on enterprise: if there are no grants, then research is pressed ever thinner” (1994a, p.91, emphasis in original). The frustration for the Daresbury scientists and managers was that they were caught between the vocational world of professional science and the ‘new public management’ impact of Thatcherism. They were in effect, boundary objects, caught between two powerful and conflicting modes of ordering (Star & Griesemer, 1989; Law, 1994a).

However, vocation and enterprise do have elements in common, particularly hierarchical ranking and performance. As I have noted above, enterprise calls for performance and measures it – in a similar way vocation is also about scientific performance. Together with vision they emphasise organisational, but particularly individual performance. People who performed at Daresbury were promoted and achieved higher rankings – and they worked longer hours, and respected others who also did, as Law describes:

For when you perform, clock time loses its sense and significance. You work until you drop – a fact which also had ethnographic implications. Thus it was much easier to talk to people ‘out of hours’. It wasn’t that they were necessarily busier between nine and five, but inside hours they had no way of telling whether I was a ‘nine to five’ sort of person... Or whether, instead, I was in my own way, a member of the vocationally committed, or the entrepreneurially performing elite (1994a, p. 120)

Law’s ethnography has much relevance for my own research on the Allan Wilson Centre. Much of the ordering of the Daresbury Laboratory rang true when considering the Centre,

however other studies have also utilised the ‘modes of ordering’ approach to actor-network theory in contexts relevant to the Centre; Davies work is presented in the next section.

2.2.2. Gail Davies: The Natural History Unit of the BBC

As discussed briefly in the methodology chapter Gail Davies conducted a study of the Natural History Unit of the BBC using an approach informed by Law’s modes of ordering analytical model. There are many methodological and some case similarities between Davies study and my own, particularly her results exemplify how Law’s analytical mechanism can be applied, usefully, in another setting.

Davies, using a collection of ethnographic techniques (see methodology chapter for more details), established three modes of ordering the working practices of the BBC’s Natural History Unit (NHU). Given job role syntax these modes basically were the ‘Amateur Naturalist’, the ‘Producer’ and the ‘Manager’. Each of these modes offers different accounts of the working practices of the NHU, both current to Davies time in the unit and historically since its formation. It is important to note, before exploring the details of these modes, that Davies emphasises the interrelatedness of the modes of ordering she identified at the NHU. She explains: “Despite my separation of the three modes of ordering into distinct accounts, and the tendency of subsequent modes of ordering to erase the importance of former, all kinds of organisational ordering coexist” (2000, p. 549). For Davies, like Law, the coexistence of modes of ordering is a key feature – and the mechanisms that actors use to privilege one over the other form a key part of her analysis. In the rest of this subsection I consider, briefly, each of these modes, how they operated with their counterparts and their relevance for my thesis.

Three Modes of Ordering the NHU

The amateur naturalist Film-maker mode of ordering the NHU privileged the traditional aspects of natural film making. It contained moral and scientific integrity, emphasising the importance of scientists and film-makers working closely together to bring nature to the public. As Davies notes: “What is stressed in the ordering practice of the naturalist Film-maker is the importance of field craft, the relationship with the animal in the wild,

collaborations with scientists, and transmitting an enthusiasm for this vision of nature to the public” (2000, p. 543). People embodying this mode showed disregard for the modern tenets of television, things like audience share and production techniques were not considered important, instead, held in high regard was “an a priori belief in expert culture and the benefits of educated citizenship” (p. 544). It was important therefore to ensure the integrity of the nature being filmed and the science being communicated. This mode was historically situated, emerging from a time when the BBC had monopolistic conditions and was therefore mostly unconcerned about audience share (Davies, 2000).

This amateur naturalist mode worked in certain contrast to the television producer mode of ordering the NHU. This mode represented, to Davies, another stage in the development of the NHU where the production of television emerged as important to ensure the continuing success of the BBC. For people engaged in this mode there was “a strong belief in the autonomy and integrity of the producer to mediate between disparate voices and communicate directly to the public” (2000, p. 546). One of the voices mentioned is the amateur naturalist who in this mode represents only one of several important aspects of bringing a production to completion. In the producer mode of ordering the NHU producing good television becomes as important as producing good science (Davies, 2000) – and this issue creates a clash between the amateur naturalist aiming to retain the integrity of the science in distinct disregard for the integrity of good television and the television producer aiming above all to meet the needs of the audience.

In the story of the NHU here enters the television ‘Manager’, as Davies notes, this mode of ordering aimed “to increase efficiency by cutting production costs and increasing programme impact through monitoring and maximising the flow of material through the existing networks of natural history filmmaking” (2000, p. 548). The emergence of this mode signifies an extension of the commercialisation of the NHU, emphasising the efficiencies that can be gained through the application of techniques associated with managerialism.

Clearly these three modes of ordering are historically situated, but at the time of Davies ethnographic enquiry each was still very much alive in the NHU – all playing a part in

the development of natural history television. The follow section discusses how these modes coexisted and the tensions this placed on the NHU.

Coexisting Modes in the NHU

The impact of the history of the BBC and the permeation of new public management through the UK public sector offers a very interesting reading to the changing modes of ordering the NHU; this reading that is very relevant to my own thesis, as the Allan Wilson Centre has also been impacted by the permeation of the philosophies of managerialism. Particularly a consideration of how these 'business' philosophies impacted upon the other two modes of ordering the NHU is enlightening as the impact was considerable, take Davies comments for instance:

The NHU's associations with scientists, based upon trust, personal contacts and shared experiences, are threatened as media demands for efficiency demean the role performed by the naturalist film-maker. The ordering role of the television manager also permeates production offices where programme costs, previously impossible to account because of freely shared in-house resources, are now managed to allow direct comparison with external commercial competitors. (Davies, 2000, p. 549)

This clearly shows how managerialism has threatened aspects of the other two modes. In particular the associations with scientists, a key part of the NHU's core business, is severely challenged as the actors performing the amateur naturalist mode align more with the less commercially orientated informant scientists than they do with their own managers.

Davies talks of the hierarchies and rankings of modes where actors embodying a certain mode rank the others in a descending order of irrelevance. "These hierarchies emerge because some modes of ordering speak more clearly and more widely of more actors" (2000, p. 550). But they are all necessary to perform the NHU – from the collection of ideas and involvement of scientists to the production of 'good' television and 'good' science to the efficient management of time, space and resources.

2.2.3. Bill Doolin: Competing Narratives in a New Zealand Hospital

Bill Doolin conducted a study within a New Zealand public hospital undergoing reform between 1994 and 2001 (Doolin, 2003). Using an approach related to the actor-network theory inspired by Callon and Latour and following Law's modes of ordering approach he conducted interviews, reviewed texts and observed working practices over a six month period in 1996 and followed up with interviews in 2001 (Doolin, 2001; 2003). This study is directly useful to my own as it also involved an entity coping with government mandated change to support their own new funding regime (Doolin, 2003). Resulting from the impact of new public management originating in the UK, as Doolin describes, his case "can be placed in the context of wider changes in both the New Zealand and UK health care sectors" (2001, p. 235). Likewise the Allan Wilson Centre is a result of a Government mandated funding change, in this sense Doolin's experiences can be illuminating.

Doolin's research differs from the likes of Davies because he was looking specifically at an episode of change, rather than a history of institution – but this shows two things; firstly how compellingly an actor-network approach following Law can usefully inform an understanding of an enforced change episode, and; secondly it shows the breadth of research setting within which Law's analytical model can be applied.

Clinical Leadership

The results from Doolin's study are an excellent informant for my own thesis as he describes in some detail the clash of modes experienced by the actors within the networks of the hospital. Particularly he talks about the impact of a strategy termed 'clinical leadership', a discourse used by the managers of the hospital to reconstitute clinicians as managers (Doolin, 2003).

Doolin stresses the importance of the various discourses available in the wider public service and western health systems sector in the change experienced by the hospital. In particular the 'clinical leadership' model promoted at Doolin's site was located in a wider discursive framework that was present throughout public health sectors in the west, and as Doolin notes:

These discourses acted as a strategic resource for the change management group, in that the group were engaged in 'sense-giving' activities, attempting to present their own construction of the change process... The discourses were available to them in the broader societal context, and they drew upon them in their daily construction of organizational reality without any necessary subjective intentionality (2003, p. 761-762)

So in this sense the change agents were promoting the narrative of clinical leadership without any particular manipulative intent, it was just that the wider discourses available to them supported an organisational reality that was successfully based on the tenets of clinical leadership.

The attempted success of the clinical leadership mode of ordering the hospital was achieved partly because of its promulgation through various materially heterogeneous actors; Doolin describes its embodiment in texts, people and particularly technology as the information technology (the 'casemix' system) associated with the mode embodied key aspects of clinical leadership – for instance budgetary management was built into the casemix system, and subsequently became a skill now required of the clinicians (Doolin, 2003).

As we have seen in Law and Davies work various modes co-exist in any organisational situation, and clearly this was also the case in Doolin's case study. Like the clash between professional scientists and new public management, much has been written on the cultural clash between clinicians and administrators, so it is unsurprising that the "introduction of clinician management involved some mediation between two professional cultures that historically have been constituted as different" (Doolin, 2002, p. 380). Some of the clinicians in Doolin's study took to the new mode of clinical leadership, reconstituting their professional identities to incorporate the tenets of clinical leadership; though this was done primarily with traditional clinical values as motivation (Doolin, 2002). However the resistance was also very significant as many senior clinicians felt that the managerial mode of ordering represented by the clinical leadership was "a betrayal of their professional medical identity" (2002, p. 382). Clearly there was a significant conflict of ideologies at the hospital; one set that was constituted around the

expert professional knowledges of the clinicians and one constituted around the managerial discourse of clinical leadership, as Doolin describes:

Hospitals are institutions with their own inherited ideological appeal and complex power relations constituted around various expert knowledges... These different discourses and knowledges interact with those of management and enterprise, with potentially complex results (2002, p. 381)

These can be seen as competing modes of ordering the hospital – and as such are very useful for considering the complexities of the Allan Wilson Centre, as scientists and clinicians have much in common as professional expert groups.

2.3. *Conclusion: Research Problem*

Each of the three main research studies I have presented in final section of this literature chapter provides a useful addition to constructing the analytical framework I used to conduct my thesis. Law's work is directly relevant in that the subject of his study, the Daresbury Laboratory, was experiencing the pressures of new public management reforms and the conflicts this had with more traditional vocational modes of ordering science. Davies's research on the NHU shows how the history of the development of an organisation can shed light on its various modes of ordering and Doolin's study, in a New Zealand context, shows the distinct clash of modes between professional expertise in a clinical sense and new public management reforms in the public service.

The actors within the Allan Wilson Centre are not being directly confronted by the privatisation of the public service, but the Centre's existence is directly related to the impact of western public policy, informed by new public management discourses, on New Zealand Government policy. This new mode of ordering science is similar to Doolin's new mode of ordering hospitals (clinical leadership) and to that experienced by both Davies and Law, hence the approaches utilised by Law, Davies and Doolin have heavily influenced my own research approach.

My research problem is relatively simple, in that I am interested in whether the institutionalisation of the Allan Wilson Centre through the government mechanism of the

Centres of Research Excellence fund has made a difference to the way they order science. The difficulty resides in how I can make sense of the change, that is – *how* has institutionalising the Allan Wilson Centre made a difference? This is problematic to explain, and is why I have chosen to use an actor-network approach to try and make sense of what is ‘really’ going on. Actor-network theory helps us to explain how things are ordered as compared with other analysis that privileges the voice of the actors or the arrangements of funds etc. By contrast, actor-network theory attempts to see each of these as ‘networks of actors’ that privilege particular formations of routines over others.

What is clear from the literature is that there are multiple registers operating within any particular mode of ordering an organisation, for instance the material register – where modes, like Law’s administration, are embodied in equipment, and also the account register – where human actors recount narratives about their organisational lives, performing and reinforcing different modes at different times to serve different purposes.

The following methodology chapter discusses the actor-network approach in detail, and summarises with the various modes of ordering the Allan Wilson Centre and the main registers through which these can be seen to be operating that I have identified through the process of the thesis.

3. Methodology

The purpose of this chapter is to explain the research methodology and research methods I have used to collect and analyse the data for this research project. The chapter is ordered along the following lines; in the first section I discuss actor-network theory, including a consideration of the seminal works in this field and an overview of the key concepts. In the second section I discuss the specific analytical approach I intend to use, following John Law's 'modes of ordering' approach discussed in the previous chapter. In the third section I briefly outline the case study approach, and how this has been utilised by actor-network scholars. The fourth section considers the various sources of data I have utilised within my thesis; the fifth and sixth sections present the ethical issues and general limitations of my methodological approach.

My overall research methodology is primarily qualitative in nature and fits within the social studies of science (SSS) tradition. Kuhn in *'The Structure of Scientific Revolutions'* (1970) described the social nature of the world of positivist physics and in doing so brought to attention the many criticisms of positivist science; this new understanding of science paved the way for sociologists of science to ethnographically study the nature of scientific knowledge production (Liebrucks, 2001). Through highly influential works such as those of Latour and Woolgar (1979) and Knorr-Cetina (1981) the understanding of scientific knowledge as social construction has become commonly accepted in sociology. Latour and Woolgar's *'Laboratory Life'* (1979) in particular was a seminal work in the development of a sociological research tradition known as 'Actor-Network Theory' (Law, 1992). I use a specific form of actor-network theory in this study to frame, collect and analyse the data gathered on the Allan Wilson Centre. The subject of my research is one single case of the Centres of Research Excellence initiative and as such I have employed a case study approach to conduct the research within the actor-network genre.

3.1. Actor-Network Theory

Actor-Network Theory (ANT), rather than being a coherent body of literature, is described by Davies as a “critical dialogue between a variety of positions” (2000, p. 541). Within these positions Davies includes the sociology of science knowledge, ethnomethodology and organisational analysis. Rather than taking an static ‘entity’ view of a particular system (like an institution) actor-network approaches consider why certain network interactions within the system are more successful at producing order than others, and how various actors (human and non-human) play a part in these network mechanisms, and also importantly why and how current order can break-down and be replaced by new order.

3.1.1. Beginnings of ANT – Latour and Callon

Any discussion of ANT must first start with a consideration of the works of Bruno Latour and Michel Callon as they are commonly described as the pioneers of actor-network theory. The works by these authors laid the ground work for the actor-network theory now in common use in a huge variety of disciplinary areas; one of these is *Laboratory Life*, mentioned above. This piece was published in 1979 by Bruno Latour and Steve Woolgar, and its main contribution is encapsulated by its sub-title “The Social Construction of Scientific Facts”. Latour and Woolgar tracked the construction in a laboratory situation of a scientific ‘fact’: TRF(H), a rare hormone that releases the compound thyrotropin from the pituitary gland. They concluded that this compound was socially constructed as it was unable to be observed except through the bioassay which ‘proved’ its existence, and this bioassay itself was simply an assemblage of previous social constructions; thus proving that the theory of TRF(H) could in fact only be ‘proved’ by standing on the shoulders of previous social constructions. Although this claim has been met with a certain amount of disbelief from scientists, the more general tracking of the process of construction that occurs in a laboratory environment is now a more commonly accepted phenomenon.

Latour and Woolgar’s story laid the groundwork for further studies along these lines. Michel Callon’s (1986) study of the scallops of St Brieuc Bay took the ideas promulgated

by Latour and Woolgar and developed the notion of the 'sociology of translation', which later became synonymous with actor-network theory. Although a more detailed consideration of Callon's work is explored later in this chapter, the basic premise outlined a four stage process whereby an actor(s) constructed a situation that promoted a particular way of conceptualising a situation. In his example he talks about three scientists positioning other important actors (human and non-human) in and around St Brieuc Bay to promote a certain view of the social and natural world of the scallops. Callon's concluding remarks outline why his sociology of translation is a useful way of analysing social and natural processes:

"Understanding what sociologists generally call power relationships means describing the way in which actors are defined, associated and simultaneously obliged to remain faithful to their alliances. The repertoire of translation is not only designed to give a symmetrical and tolerant description of a complex process which constantly mixes together a variety of social and natural entities. It also permits an explanation of how a few obtain the right to express and to represent the many silent actors of the social and natural worlds they have mobilized" (1986, p. 224)

The above is just a brief summary of Latour and Callon's seminal work; more is included in the sections below. There are several important aspects common to research undertaken in the actor-network genre; the following sub-sections introduce these, particularly the notions of: Heterogeneity; processes vs. stases; translation, immutable mobiles & de-centering and; empirical agnosticism & free association. These notions are necessary to understand the appropriateness of using an actor-network approach as the theoretical position for my case study of the Allan Wilson Centre. The final sub-section also discusses some of the key criticisms of actor-network theory, as alluded to above. The particular actor-network based model I am using to analyse the data originates from John Law's 'modes of ordering', this is described in more detail in the section immediately following the discussion of actor-network theory below.

3.1.2. Heterogeneity

The notion of heterogeneity is very strong in ANT – the concept itself is actually relatively simple; things we can view as homogeneous 'entities', such as 'an

organisation', are in fact constituted by many heterogeneous relations between various actors. This is the actor-network. The implication for our understanding of any entity (from a single computer through to entire societies) is not then of a stable homogeneous structure but of a heterogeneous pattern of relations. In this way ANT suggests "that society, organisations, agents and machines are all effects generated in patterned networks of diverse (not simply human) materials" (Law, 1992, p.2).

The diverse patterned networks often look like "single point things", Law (1992) gives the example of the British Government, but the same can be said of a particular university for instance. Often we refer to the university as one unified, generally stable, entity which is in fact a series of changing heterogeneous networks of relations between numerous actors. The order of a university, under actor-network theory, is a complex performance of the power relations in a particular moment, which produce the institution.

3.1.3. Processes not Stases

Heterogeneity suggests that we need to study, not the noun-ed 'single-point' entity of an organisation but the verb-ed processes through which that organisation is performed. This is a very different way to conceptualise the object/subject of study in organisational studies, and has been contrasted to both modern and postmodern approaches to organisational analysis (Lee & Hassard, 1999). By re-conceptualising the object/subject of study as a process rather than a static thing, an ANT approach demands a consideration of the struggles inherent within the process of performing something (such as an institution); key to this concept is the notion of translation.

3.1.4. Translation, Immutable Mobiles and De-centering

Translation refers to the possibility that one thing may stand for another, where an actor (for instance an institution) in fact represents something else – a network. The process of translation is the process that studies in the ANT genre follow during their research. This requires the researcher to put aside the traditional understanding of the object/subject of study (as institution, society or individual for instance) and instead, as Davies describes "view these established tenets as the precarious achievements of potentially reversible patterns of association, or networks." (2000, p. 541).

We often view something that is a process as a static entity because of what Latour entitles 'immutable mobiles'. We label entities (such as 'The CoRE fund') and by doing so these "networks are often converted into inscriptions or devices" (Tatnall & Gliding, 1999, p. 958). Many commonly understood 'entities' are reified and given a 'black box' status, a salient example of this can be seen in documents – we may see them as entities, but viewed through the lens of actor-network theory they are just processes that have won the struggle against their resistors to emerge as order (Law, 1992). For instance in New Zealand the Treaty of Waitangi is often viewed as a reified entity, but can also be viewed as a process that won a struggle against other processes to establish a treatise between indigenous and colonial peoples. The thing about these immutable mobiles is that become immutable (literally – 'not able to change') *and* mobile (literally – 'able to move freely around') so as Latour states you have created "objects which have the properties of being *mobile* but also *immutable*, *presentable*, *readable* and *combinable* with one another" (1986, p. 26, emphasis in original).

If the immutable mobile is the result, the process of translation is the process of winning the struggle to create order within a network. A particular actor within a network acts to reinforce a particular position, and they perform this position in all of their interactions. This is most easily understood from a human within institution perspective when one actor (be that individual or group etc...) performs a particular 'mode of ordering' their institution's arrangements (these may be for example as a not-for-profit or as a profit making enterprise) this (these) actors will position other actors (human and non-human) in the network to reinforce the order their chosen mode represents. The more successful they are the more 'real' the institutional order becomes, this is the process of translation. Michel Callon tells of four stages of translation as the promoting actors employ various strategies to reinforce their position. The first is the *problematizing* of the institution from the perspective of their mode; this enables the actor to become indispensable to the cause. The second is by utilising the devices of *interessment*, where the actor positions other actors to create interest in their own mode, this may also involve interrupting the interessment devices of competing modes. The result of successful interessment is *enrolment*, where other actors enrol in the promoted mode. The final stage involves

ensuring the enrolled actors are representative of the masses, if so, thereby *mobilizing* the masses to continuously reinforce the order (Callon, 1986). As Hardy, Phillips and Clegg (2001) note:

“These strategies help to create convergence by locking actors into the network. The more fixed or stable it appears, the more ‘real’ and durable it becomes, and the less controversy and ambiguity are evident... The aim, then, is to put relations between actors into ‘black boxes’ where they become a matter of indifference – scientific ‘facts’, technical artefacts, modes of thought, habits, forces, objects” (2001, p. 538).

The section above is posited from the position of a politically active human actor – mainly because this is the most easily understood position. It is however essential to note that actors are both human and non-human. A text, for example, may be a powerful reinforcing actor in a network, as Law describes:

“Networks are composed not only of people, but also of machines, animals, texts, money, architectures -- any material that you care to mention. So the argument is that the stuff of the social isn't simply human. It is all these other materials too. Indeed, the argument is that we wouldn't have a society at all if it weren't for the heterogeneity of the networks of the social.” (1992, p. 2-3)

Clearly the active involvement of human and non-human actors within a network removes the requirement for the process of translation to be a conscious, political power struggle. It may be, but it also may not – in fact it is probably both. The Treaty of Waitangi, used as an example above, for instance is an inanimate, but powerful, non-conscious actor (it could be argued that it is in fact a network – now reified as ‘single-point’ actor) within the Government sector in New Zealand.

One of the main concepts discussed in the above section is the proposal that, often, when we refer to an entity we are reifying what is actually a network. An important part of ANT is the de-centering of these reified entities to expose their network composition, as Davies notes: “Actor network theory is about decentring. It develops a strand of social theory that destabilises the subject in explanations of social organisation.” (2000, p. 541).

3.1.5. Empirical Agnosticism and Free Association

Actor-network theory is purposefully agnostic towards the empirical sources that inform the research. This means that particular sources are not given priority over others; often in organisational studies human data sources (through interviews, observation, questionnaires etc...) are given preferential treatment over other sources (documents, observation of non-human actors etc...) as if the data sourced is more representative of 'truth'. By contrast ANT approaches demand analytical impartiality of "all the actors involved in the project under consideration, whether they be human or non-human" (Tatnall & Gliding, 1999, p.958)

Also important is a principal of free association; as Tatnall & Gliding describe: "The principle of free association requires the elimination and abandonment of all a priori distinctions between the technological or natural, and the social" (1999, p. 958). Under the principal of free association it is essential not to make a priori assumptions about the object/subject of the study. This includes not assuming that there is something fundamentally different about macro level things and micro level things. (Law, 1992) "It suggests, in effect, that we should analyse the great in exactly the same way that we would anyone else." (Law, 1992, p. 1)

In sum Lee & Hassard describe Actor-Network theory as being ontologically relativist "in that it allows that the world may be organized in many different ways", but by contrast empirically realist "in that it finds no insurmountable difficulty in producing descriptions of organizational processes" (1999, 392). I think this is a fitting summary of the actor-network model.

There are several aspects of ANT that make it a suitable theoretical position from which to conduct my research. Research institutions are complex entities, and an actor-network perspective allows a flexible analysis of the processes that perform these entities. Calás & Smircich (1999) describe ANT as providing a "very good way of telling stories" (p. 663) about organisational life that we otherwise take for granted. They go on to describe the value of ANT for organisational studies:

“If nothing else, ANT, with its focus on irreductionism and relationality, rather than facts and essences, may become a very useful exercise to counter conventional ‘theoretical tales’ in organization studies. More immediately, as organizational studies face contemporary technologies in a reconfiguration of the time/space of organizations, as ‘the Web’ and ‘virtuality’ become part of our everyday mode of existence, and as our interactions with machines incrementally define our life experiences, ANT provides ways to navigate and represent these (dis)locations while displacing more conventional ‘organizational’ thinking” (p. 664)

To me the importance is the displacement of “conventional organizational thinking”. Many research approaches take a critical, but modern approach to studying complex entities like research centres – this simplifies the inherent uncertainty and complexity within the institution and views it as a mostly stable, structural entity comprised of human actors attempting to reach some legitimate institutional unity (Cooper & Burrell, 1988). I view this as an un-empowering theoretical position which has been many times critiqued. Perhaps the most frustrating aspect of the modernist position in organisational analysis is the emphasis on the human actor as the informant of choice, as Cooper & Burrell (1988) note in their seminal article: “For critical modernism, the thinking subject is the human individual or, more precisely, a network of interacting individuals who, through the commonsense of ordinary discourse, can reach a ‘universal consensus’ of human experience” (p. 97). Actor-network theory, as an approach to organisational analysis, takes issue with this modernist perspective and instead uses a range of human *and* non-human informants to analyse the subject, using a reflexive approach, and does not try to force a unified world-view but instead considers the subjectivities inherent within the research site.

Any research methodology has limitations; the limitations often ascribed to research within the actor-network genre are considered in the following sub-section.

3.1.6. Criticisms of Actor-Network Theory

Several criticisms of the actor-network approach have been emerged since its formation and growth in the 1980s and early 1990s following the publication of *Laboratory Life* by Latour & Woolgar in 1979. Latour (1999) separates the major criticisms into two categories: “demiurgy on one side; ‘death of Man’ on the other” (p. 16). I will consider

each of these and then summarise the specific limitations these criticisms lay upon my methodology.

By 'death of Man' Latour is referring to the human, non-human equivalence promoted by actor-network theory. That is, that non-human actors can and do attain delegated power in the networks of the social; this concept was compellingly shown by Callon in his study of scientific research conducted on scallops (1986), and now commonly features prominently in most studies in the actor-network genre. Davies, for instance, emphasises the importance of this "alternative way of attributing agency to non-human actors" in geography and uses the concept to consider, persuasively, the agency of non-human film archives in the Natural History Unit of the BBC (1999, p. 51). Despite its popularity there have been some criticisms of the ascribing of agency to non-human actors. Collins & Yearley, in particular, have strongly criticized this attribution as constituting "a backward step, leading us to embrace once more the very priority of technological, rule-bound description, adopted from scientists and technologists, that we once learned to ignore" (1992, p. 322); they go on to say that this backward step has emerged because of a fundamental misconception, within actor-network theory, that "takes humans out of their pivotal role" (p. 322). Actor-network theory's consistent response to this criticism has been that things are not either natural or social but are in fact a combination of these – the dichotomy is false, and unnecessary – so human actors and non-human actors should be treated equally, without alternating language systems, when studying networks (Latour & Callon, 1992; Latour, 1999).

By 'demiurgy' Latour draws a link to the concept of 'demiurge' – in philosophy this can be represented as a supernatural force that created the world, but also as the originator of evil. Typical of the Latourian style he is referring to criticisms of actor-network theory as being impossibly trying to reconcile the four modern predicaments, that is people criticize actor-network theory for trying to be a theory of the social; a theory of psychology; a theory of politics and a theory of theology, all simultaneously. But as Latour says ANT is not a theory in a modern sense but rather "is a theory of the space or fluids circulating in a non-modern situation" (1999, p. 22) or, slightly more accessibly,

ANT is not trying to replace other forms of sociological enquiry, but to extend them, in a non-modern direction.

Related to these criticisms is the non-political bias that is often directed at studies in the actor-network genre (Davies, 2000). This however is an unfair criticism, as Law identifies, actor-network theory is primarily about the power struggles within society (1992); furthermore actor-network theory actually provides a way to study the mechanism of organisation that is the power struggles. It does however, not do this from an *a priori* political position – perhaps this is the criticism.

In reviewing the literature that discusses these criticisms it seems somewhat clear that they are primarily related to the ontologically relativist but empirically realist position of actor-network theory, this position results in a theoretical basis that resists definition, and fails “to forge its own internal and external boundaries” (Lee & Hassard, 1999, p. 392). A main consequent of this ‘failure’ is that ANT becomes incommensurable with the order that exists in sociologically academe – it doesn’t easily fit within any established paradigm and is therefore tentatively positioned. To me this criticism is a positive feature of the theoretical position – because I agree that the world we study is ontologically relative whilst being equally empirically realist!

The following sub-section introduces the analytical model I use within my thesis: ‘Modes of ordering’, which is firmly based in the actor-network theoretical genre.

3.2. *An ANT Analytical Model: Modes of Ordering*

John Law’s analytical model of ‘modes of ordering’ was developed during his ethnographic study of the Daresbury SERC Laboratory in 1990. As an analytical model ‘modes of ordering’ is based on a premise succinctly put by Cooper and Burrell, and quoted by Law (1994b, p. 248): “It becomes a question of analyzing, let us say, the production of organization rather than the organization of production” (1988, p. 106). Law, by studying the working practices of the Daresbury Laboratory was able to identify four ‘modes’ that are embodied in and performed by the various actors that perform the Daresbury Laboratory.

A key aspect of modes of ordering emphasised by Law is the blend of complementary and/or competing modes that make up a network. Law suggests that “there are plural and incomplete attempts at ordering” (Doolin, 2003, p. 756) in any network setting and for something to emerge as a perceived ‘stable’ mode it must overcome the other orders that resist its emergence. For instance, capitalism as a mode of ordering capital must constantly overcome the resistance put forward by other competing modes of order such as socialism.

The various blend of modes discussed above “are *mixtures* of orderings embodied in us, performed by us. And our surroundings too” (Law, 1994a, p. 126). Picking up on the actor-network notions discussed earlier in this section Law is clearly drawing an important link to the human/non-human *non-divide* emphasised in the ANT literature. Various modes of ordering an entity such as a research laboratory are embodied in and performed by the human *and* non-human actors alike. It is tempting to focus on the human actors and particularly what they say – their stories; but as Davies notes: “Modes of ordering are thus more than mere stories; they actively organise relations and generate materials, including the role of non-human actors within networks” (Davies, 2000, p. 542).

Bill Doolin used Law’s concept of modes of ordering to conduct a narrative study of a New Zealand public hospital undergoing organisational change in the face of ‘new public management’ managerialism being implemented to create efficiencies in the hospital. Doolin, mirroring somewhat those concepts discussed regarding actor-network theory generally, describes the key characteristics of Law’s modes of ordering as being strategic; discursive; performed; materially heterogeneous and incomplete (2003). Strategic in the sense that a mode does perform, to some extent, an intentional structure; discursive in that actors simultaneously *draw on* discourse as a sense-making mechanism and *use* discursive structures as a sense-giving mechanism. Performed in that a particular mode is replicated and strengthened through its embodiment *and* its performance by those actors embodied, materially heterogeneous in that it is bound up in the social and the material and incomplete in that in the end orderings tend to fail – reflecting “the precariousness and fragility of organization and organizing processes” (Doolin, 2003, p. 758).

In the opening actor-network section above I suggested that using an analytical approach based on the actor-network, such as Law's 'modes of ordering', allows a consideration not only of current orders (in the moment represented by the thesis text) but also how these orders have achieved stability relative to their cohort and how they maintain their dominance, as Doolin describes: "What is of interest is how such relative durability is achieved – how it is that entities are performed into relations that are stabilized for long enough to generate their effects" (Doolin, 2003, p. 757), and likewise Davies: "If networks are pools of order where relationships have achieved stability through space and time, it makes sense to ask how is this order created and through what strategies is it maintained" (Davies, 2000, p. 541). This then is the 'what' of 'what shall I study?' – I use the 'mode of ordering' analytical model to investigate the mixture of orderings that represent the Allan Wilson Centre, but also importantly I attempt to track how these have formed during the five years since its establishment and to identify the strategies by which they are maintained.

Gail Davies (cited above) conducted a study of the BBC's Natural History Unit (NHU) (1999; 2000) using an actor-network approach informed by Law's modes of ordering. Although ostensibly Davies work is positioned within human geography her study is directly relevant to my own and as such I use it as an exemplar of how to conduct the analysis of the data derived from the various sources described later in this document. Davies makes use of a variety of data sources to inform her thesis and particularly remains faithful to the key ANT concepts of not assuming a priori orderings or granting higher status to data sourced directly from the human actors within the Natural History Unit. In describing her position Davies quotes Latour (1999, p. 51):

The fact that we do not know in advance what the world is made up of is not a reason for refusing to make a start, because other storytellers seem to know and are constantly defining the actors that surround them – what they want, what causes them, and the ways in which they can be weakened or linked together. These storytellers attribute causes, date events, endow entities with quality, classify actors. The analyst does not need to know more than they; (s)he has only to begin at a point, by recording what each actors says of the other... The only task of the analyst is to follow the transformations that the actors convened in the stories are undergoing (Latour, 1988, p. 10)

Davies utilised several different strategies to gather data on the BBC's NHU – including library research, observing working practices and conducting interviews, the research process allowed Davies to follow “the flows of ideas, expertise and film around the Unit as researchers, producers, managers, camera operators, technologies and animals attempted to impose their order on others” (1999, p. 52).

Davies concluding statement in the 1999 ‘Area’ paper reads: “Through using ANT, it is possible to explore what is marginalized, as well as incorporated, in the new networks of the electronic zoo” (1999, p. 56). Several points exemplified in this summary sentence inform my own data analysis; the ‘new networks of the electronic zoo’ are a useful metaphor for the new networks established by the institutionalisation of the Allan Wilson Centre through the CoRE fund. Like the changing zoo depicted in Davies research, this research management structural change takes a traditional phenomenon (the university and university academic) and forms a new network structure – the ‘Research Centre’. Therefore it is important, in my analysis, to consider what is marginalised as well as what is incorporated in the new networks of the Allan Wilson Centre.

Davies also emphasises the ‘dual nature’ of the modes of ordering the BBC's NHU, she describes them as “both narratives of institutional history and devices of organisation, legitimation and authority” (2000, p. 540). This is a key point which I also use in my own analysis – the modes of ordering identified in the Allan Wilson Centre as well as being devices of organisation are, in part, narratives of the history of *several* institutions, including the Centre itself – but also the other institutions involved, including universities and Crown Research Institutes.

Developing the historical context further Davies makes another key point describing the reliance of each mode of ordering on preceding forms of ordering “all modes of ordering depend on previous, building on network associations already in place” (2000, p. 550). The importance of this issue is specifically about the importance of the historical context that impacts current network orderings. As Law comments “I think these histories tell us much more about current ordering than they do about the past. For, one way or another, the past is related to the present: it *justifies* the present” (1994a, p. 52-53, emphasis in

original). This gives an impetus for a serious consideration of the histories of the Allan Wilson Centre – and the histories of the institutions that contribute to it.

Many of the strategies used by Law at Daresbury and more recently Davies at the BBC NHU and Doolin at a New Zealand Public Hospital involved extensive ethnographic survey. Clearly I am heavily restricted by the scope of my research (limited to a Masters thesis) in spite of this restriction I have attempted to follow the research strategies employed by these authors to collect my data and conduct the analysis. The following sections discuss the case study approach I utilised and the data sources used and the method and reasoning applied to the data capture.

3.3. The Case Study Approach

The methods of actor network theory focus upon reconstructing interactions and positionalities, demanding an engagement with the living spaces of social life. The case study is the commonest exemplar of actor network theory. These include research on the production of knowledge within laboratories and research bodies; government institutions and economic organisations (Davies, 2000, p. 542)

As Davies notes above, studies in the actor-network genre most often use a case study approach as this approach allows significant ‘engagement’ with the networks of interest. I concur with Stake when he identifies case study research as not a methodological choice in itself; but is instead a choice of what to study (Stake, 2000), ‘how’ the case is studied can qualitative or quantitative, interpretive or discursive, positivist or postmodern. As Yin (1994) identifies; the case study as a ‘form of inquiry’ “investigates a contemporary phenomenon within its real-life context” and is most practicable “when the boundaries between phenomenon and context are not clearly evident” (p. 13). The driving question behind the use of this approach is: “What can be learned from the single case?” (Stake, 2000, p. 436).

The phenomenon of the ‘inter-institutional and interdisciplinary research centre’ is undoubtedly an interesting one. There has been much written, in many different contexts, with many different approaches and from many different perspectives, about these Centres. For example, some studies have chosen to investigate certain aspects of a

Centre's formation; Myers (1993) for instance chose to study, using a textual analysis, the process of applying for government funding for a UK interdisciplinary research centre. In complete contrast Garrett-Jones et al (2005) used a type of risk model to study the perceptions of scientists across a range of Australian Co-operative Research Centres. Some studies have specifically used a case study approach. Contractor and Ehrlich (1993) for instance utilised the case of a \$50 million dollar interdisciplinary research centre at a U.S. Midwestern university to study organisational birth. Each of these differing research approaches can, and has, offered different things to an understanding of inter-institutional and interdisciplinary research centres.

Stake provides a useful heuristic categorisation of case study approaches; he identifies three types of case study: An intrinsic case study; an instrumental case study and a collective case study. An intrinsic case study is undertaken if the researcher wants a better understanding of one particular case, in particular the researcher has "an intrinsic interest in, for example, this particular child, clinic, conference or curriculum" (2000, p. 437). An instrumental case study is used to provide insight to a particular issue; with the issue being of primary importance; a collective case study is an extended version of an instrumental case study – extended to several related cases.

It would be fair to say that my research project most neatly associates with Stake's 'intrinsic case study' category. I have a particular interest in the 'particularities' of the Allan Wilson Centre; from its institutional and disciplinary arrangements through to its scientific pursuits and research successes. I am also particularly interested in the impact of its institutionalisation, partly as a result of the impact of managerialism on the New Zealand government's research management policies. The choice of this case thus allows the simultaneous pursuit of both intrinsic interests – it is my choice of what to study, my methodology is actor-network theory.

As Davies noted above the methods of actor-network theory involve the retrospective reconstruction of networks (2000), that is by looking at the histories of the networks and specifically "back to points prior to the stabilisation of networks, following the

transformations of actors and entities in their construction” (p. 542). This is the worth of the combination of the case study approach with an actor-network methodology.

3.4. Data

Actor-network theory and modes of ordering analysis does not privilege one data source over another – instead it considers a range of sources – anything that can shed light on the various modes of ordering competing for exposure in the institutional performance. To this extent I have used the following data sources to varying extents and for varying purposes: A quantitative citation analysis of the Allan Wilson Centre’s publication profile, conducted by myself; a collection of print media articles, from varying sources, that discuss the Centre, either during its formation or since; the transcripts from seven semi-structured interviews conducted with various members of the Centre; and my own auto-ethnographic accounts of the pre and post Centre forms. Each of these data sources is discussed in more detail in the following sections.

3.4.1. Quantitative Citation Analysis

My application of citation analysis is limited to a study of the citations held in the ISI Web of Science database attributed to various members of the Allan Wilson Centre. I achieved this analysis by using the search string: ‘*Allan Wilson Ctr Mol Ecol & Evolut*’ in the ‘address’ field of the ISI Web of Science basic search screen. This search string is commonly used by the members of the Centre to identify their institutional involvement; though not in all circumstances. For the purposes of this thesis it does however allow enough information to demonstrate the publication profile of the Centre members over the five years since it was formed.

This type of quantitative research can be liberally termed ‘scientometric’, as Van Raan notes: “Scientometric research is devoted to quantitative studies of science and technology” (1997, p. 205); though it appears that other terms such as bibliometrics and/or informetrics can also be used somewhat interchangeably with scientometrics (Verbeek, Debackere, Luwel & Zimmermann, 2002).

Studies in the Actor-network genre have used types of scientometrics for many years to inform their studies (see Callon, Law & Rip (1986) for instance). There are two main types of citation analysis that have been utilised in this vein – the co-citation and co-word techniques, both used to try and map scientific fields of enquiry (Verbeek et al, 2002). I do not attempt to replicate either of these established citation analysis techniques for two reasons; firstly it is not a critical part of the mode of ordering analytical approach and secondly it is definitely beyond the scope of a Masters thesis.

The citation technique I use is a more mathematically simple approach, I do not claim to represent any statistical significance with my quantitative analysis – it is used as another informant to try and understand the modes of ordering the Allan Wilson Centre; it is, if you like, performing the role of ‘single point’ actor within my thesis, representing a part of the Allan Wilson Centre actor-network. I have incorporated this research, where appropriate, throughout the data, analysis and conclusion sections of this thesis.

3.4.2. *Print Media Articles*

Actor-network theory tells us that the power inherent in the structural stability of an institution such as the New Zealand Government is achieved through the dominance of the strongest alliances within its various heterogeneous parts. Some ‘institutions’ seem stronger than others – science (or technoscience) is one of those ‘institutions’. Latour’s argument for the particular power that science holds is consequent of “its ability to build strong alliances out of heterogeneous components, to enrol allies *in all areas of society* by translating their interests and imposing severe costs for resistance, thus establishing ever more powerful actor networks” (Brown, 1992, p. 70-71, my emphasis). The Allan Wilson Centre is an entity of science and as such draws on those powerful established societal actor-networks to order its operations. As such, an investigation of wider society’s modes of ordering the Allan Wilson Centre is a useful and important element for this study. There are various mechanisms for approaching this type of research, such as conducting focus groups with various members of society, but this is beyond the scope of a Masters thesis. Instead a print media review was conducted of articles that mention the Allan Wilson Centre.

Although limited in many ways a review of print media articles was deemed as being partly representative of wider societal views: “News draws upon, reframes and recirculates tensions within society in order to construct images that have resonance with the wider audience” (Loto, Hodgetts, Chamberlain, Nikora, Karapu & Barnett, 2006, p. 104). The key aspect of the above statement is the two-way interaction between news media and society – in that news media “draws upon” and “recirculates” societal constructs – including constructs such as the dominance of technoscience as an actor-network (Latour, 1987).

Three main sources of media publication were targeted for this part of the research, broadsheet newspaper articles from the major New Zealand newspapers; publications from Massey University (the host of the Allan Wilson Centre), and; government press releases. These three groups reflect and represent different parts of society and as such an analysis of the publications from these various sources is illuminating for establishing modes of ordering the Allan Wilson Centre.

The specific analytic mechanism I utilise to review the print media articles is based around the rhetorical devices used to represent the Allan Wilson Centre construct within the articles. As Watson describes “rhetoric is all about using language to persuade” (1995, p. 806), and particularly using this persuasion opportunity to establish not only credibility of content – but also worthiness (Watson, 1995). I think Watson encapsulates the usefulness of utilising this approach – to me it is important to analyse the rhetoric devices used with an understanding of the ‘conditioned position’ within which the journalists, science writers and press office staff constructing the articles from the three different sources find themselves. Each uses rhetoric to persuade the reader of the credibility of content of their article, but also, specifically in the case of the Massey University and Government press releases, they use rhetorical devices to persuade the reader of the worthiness of the Government policy that created the Allan Wilson Centre in the first place. This observation can be explained in general terms by the political affiliation of the article author; government press release writers and journalists employed by Massey University will clearly aim to promote the virtues of the Allan Wilson Centre. This does not render their comments less any ‘real’ than an ‘unbiased’

newspaper journalist but it does highlight that “the rhetorical strategies and practices of journalists are always conditioned, whether consciously or unconsciously, by the people the writer wishes to address” (Kuronen, Tienari & Vaara, 2005, p. 250). The ‘people’ in this statement are not just the unaffiliated readers of the article; they are also addressing their employer and the Allan Wilson Centre and affiliates.

Given the above context I closely examined the print media articles that mention the Allan Wilson Centre for the various rhetorical devices employed in sense-making (making sense of the Allan Wilson Centre entity) and sense-giving (disseminating this sense to the reader of the article). I identified the articles by searching the Newztext and Newzindex databases and the Massey News archive with the search phrase “Allan Wilson Centre” within the date range January 2002 – March 2006 inclusive; these databases include government press releases.

The results of the print media articles analysis was a set of rhetorical categorisations that are used by society (represented through the media) to order the Allan Wilson Centre – these devices were used in conjunction with the other data discussed in this section during the final analysis to determine the various modes of ordering the Centre, and as such are referenced, where appropriate, in the remainder of the thesis.

3.4.3. Interviewing

I utilised the interview as one data construction tool for this research project. Following from my commitment to social constructionism, and particularly actor-network theory, as this project’s theoretical position I hold that the social world that I conducted the interviews within, as a student researcher, was co-constructed by the interplay between the various actors that operated within that particular society. As such any research conclusions I draw from the interview data, or any other data indeed should be considered a modest attempt to co-construct (with the participants) some order out of what are selections of heterogeneous positions created within a common local context (The Allan Wilson Centre).

This ‘view’ on the interviews begs the question of what theoretical and analytical devices I can utilise to make sense of the data. Alvesson (2003) describes three ‘views’ of the

research interview in organisational studies: Neopositivism represents a traditional position where the interview is used as a tool for discovering the truth about the 'reality' of the organisational context, and in particular the interviewer attempts to be objective and unbiased; Romanticism by contrast endeavours to establish a rapport with the interviewee and discover their 'inner world' in an attempt to find out about the "experienced social reality" (pg. 16) of the interviewee; Localism is a less common view on the interview that considers the social context of the interview, the localist asserts that "people are not reporting external events but producing situated accounts, drawing upon cultural resources in order to produce morally adequate accounts" (pg. 17).

Given the options above, I would describe my view on the interviews as most closely matching the localist perspective – but I agree with Alvesson that it is not this simple, interviews are "complex social situations" (pg. 18) and can be conceptualised in ways other than the two dominant metaphors provided by neopositivists (the interview as an instrument) or romanticists (the interview as a human encounter). The localist position provides some other alternative metaphors to the neopositivists and romanticist positions but Alvesson extends and replaces these by suggesting eight alternative metaphors each based on "a key feature of an interview and a central problem (challenge) that the interviewee must "solve" or relate to" (pg. 18). I utilise some of these metaphors here to clarify, given the reflexive nature of my mode of enquiry, the context within which the interviews were conducted.

1. The Interview Context: Local Accomplishment

Alvesson describes this situation as being important because the social interaction that makes up the fabric of the interview is heavily impacted by its situational context (2003). I have identified two aspects of the interview context that have impacted the accounts produced:

- a) Firstly demographic aspects of the interview situation and particularly some interviewees' prior knowledge of me and my context impacted the interview content. My partner and I have both had positive science related academic and professional associations with some of the participants. These prior relationships will have altered

the accounts given during the interviews – probably allowing more open discussion and certainly with less suspicion about my intentions.

- b) Secondly six of the seven interviews involved two or more people, all from the Centre; this is likely to have profoundly altered the performance of these interviews. This approach was selected because, as Hyde, Howlett, Brady & Drennan (2005) suggest, the natural group interview has the potential to capture some of the numerous benefits of ethnographic research – particularly they note the potential to capture “the enactment of the kinds of things that normally occur in a cultural setting, such as joking, arguing and so forth” (p. 2591). Actor-network approaches routinely use ethnographic research studies to gain as much rich data about the actor-network as possible, but because my project is limited by the Masters level thesis a full ethnographic approach was not possible. Group interviews can however yield some of the benefits of ethnographic research.

2. The Interview as Identity-work

This metaphor sees the interview as an opportunity for the interviewees and indeed for me, as interviewer, to invoke, perform and reinforce the various identities we have at our disposal (Alvesson, 2003). In particular I was interviewing the participants as members of the Allan Wilson Centre – but for many of the participants this is only one aspect of their professional and academic identities. In a number of situations this was very evident as people would expressly position a comment from a certain identity – this was mostly evident during discussions about funding – where a ‘host institution’ identity and a ‘Centre identity’ would often be working at odds. The interview data has been analysed with this identity conflict in mind.

3. The Interview as a site for Political Action and as an Arena for Construction.

Alvesson actually separates this into two distinct metaphors but acknowledges that it can be difficult to distinguish them from each other and the other metaphors (2003). In my view it proved more useful to combine these; certainly for this case the use of the interview as a tool for politicking and as an arena for the participants to use their

language to find voice for frustrations were inextricably intertwined. The interviewees appeared to ‘use’ the interview to describe their frustrations with the New Zealand science system, specifically the tertiary teaching and research funding models. This lean towards the political spokesperson role did not appear to be a thoughtless ‘dig’ at the system, but rather a purposeful attempt to have a voice, it became clear that the scientists involved struggled to find opportunities to be heard and it seems that this was interpreted as one. This metaphor certainly does not cease with the participants – as interviewer (and part-time lounge-chair political activist) I found myself sharing and performing their concerns. To me this has two main implications for the analysis of the interview data. Firstly I now have a requirement to reflect that voice – it is an important part of the actor-network in play within the Centre. Secondly I must attempt to contextualise the transcriptions to identify when this type of activity was occurring.

4. The Interview as a Play of the Powers of Discourse

This metaphor picks up on the poststructuralist interpretation of the power of language systems and the various available competing discourses that structure particular contexts for any person; as Alvesson describes “available discourses position the person in the world in a particular way prior to the individual's having any sense of choice” (2003, p. 23). An interviewee therefore is, in many ways, simply a conduit for a range of competing discourses that are emerging through the language used in the interview. This metaphor is fundamental to my research situation as several wider discourses were heard through the interviews – including those commonly associated with scientists, for instance – the ‘blue-skies’ vs. applied science funding discourse; and the unease with managerialism discourse amongst others; a consideration of the power of discourse when analysing the interview transcripts is therefore of prime importance.

3.4.4. *Autoethnographic Accounts*

Research associated with the actor-network approach has used ethnography as a key methodological position since its beginnings in the 1970s. One of the reasons for this was noted above – ethnography provides very contextually rich data about the actor-networks of interest. John Law’s book *Organizing Modernity* (1994a) is a quintessential exemplar

of organizational ethnography in the actor-network genre, and an essential part of this book is the autoethnographic accounts that Law uses to supplement the more classic ethnographic accounts of the networks of the Daresbury Laboratory. Speaking of Law's work Suchman describes the organizational ethnography as consisting of "a kind of second-order accounting made up of the juxtaposition and alignment of organisation members' stories with those drawn from, and accountable to, the ethnographers' own" (2000, p. 314). So clearly the ethnographer's stories (of their experience of the research) are important in organisational ethnography as is their subjectivity. Autoethnography, then, is an extension of more traditional ethnographic research approaches (Bochner & Ellis, 2002) using the ethnographer's experience and relationships as a source of research data, as Gergen & Gergen note:

Autoethnography represents a significant expansion in both ethnographic form and relational potential. In using oneself as an ethnographic exemplar, the researcher is freed from the traditional conventions of writing. One's unique voicing – complete with colloquialisms, reverberations from multiple relationships, and emotional expressiveness – is honored (2002, p. 14)

The usefulness of autoethnography for my thesis is that it allows me to use my own experiences of the Centre as legitimate data from which to construct an understanding of the modes of ordering the Centre. Without an explicit use of autoethnographic techniques I would be falsely trying and failing, but claiming to succeed, in achieving 'objectivity' distinct from my subjective position as a researcher. I have known some of the researchers within the Allan Wilson Centre since 1996 when I began my Biochemistry degree at Massey University, and these experiences count towards how the Centre is constructed, as such autoethnography forms a small, but critical part to the methodology of this research project.

An example of an autoethnographic vignette is included here, I wrote this before embarking on the other empirical data gathering exercises to capture what I thought about the Centre:

My involvement with the AWC began before its inception... in fact it began when I first entered science tower D at Massey University in 1996. I undertook a Science degree in Biochemistry and as part of this qualification interacted with some of the people and institutional structures that have

played a part in the current form of the AWC. Even more so my wife, Anna, was closely involved with the structures, and particularly with David Penny (current co-Director of the AWC and Professor of Theoretical Biology at Massey University). David was Anna's BSc (hons) supervisor – and he was an impressive intellectual creature for me as a student (and partner of Anna); specifically he was (is) kind, very professionally understanding and encouraging, and inspiring. In spite of his occasional vagueness he was (is) an intellectually abundant supervisor and prolific scientist – we even referred to him as a modern day Darwin.

Excerpts such as this will be included where deemed useful during the remainder of the thesis. These will include both accounts written prior to gathering data and accounts written during the research process. In addition, as my analysis is informed by my subjectivity, auto-ethnography extends to more than just the accounts I include but is also present within the general scholarship.

3.5. *Ethical Considerations*

There are two main ethical considerations for this research thesis; these are at the level of the individual involved in the interviews and at the level of the institutions involved, being the Allan Wilson Centre specifically and those other institutions involved directly or indirectly with the Centre.

It was agreed with the Allan Wilson Centre that I would work towards ensuring group and institution identity confidentiality by not directly referring to the names, positions and disciplinary or institutional affiliations of the participants. Thus, although the Allan Wilson Centre is named in the thesis I endeavour to maintain individual confidentiality but not referring to any informant's location, discipline, and/or institutional affiliations.

3.6. *Limitations of my Research Methodology*

In this research thesis I combine four main data gathering methods to identify the various modes of ordering the Allan Wilson Centre; because of the limited scope of a Masters thesis I have had to limit the amount of data that I can utilise. Specifically, given additional scope I would have utilised more ethnographic methods such as observation

and shadowing to consider the working practices of the Centre. This would have resulted in a better understanding of how the Centre operates.

My prior relationship with some of the interview participants may have impacted on their answers during the interviews – my honest perception is that this would have resulted in a less ‘manufactured’ answer, in the sense that many of the interview participants would trust my intentions – but I am very aware of carefully limiting my own *a priori* assumptions. I have identified this potential limitation in an earlier section of this chapter and will attempt to identify its impact during the analysis and conclusions if necessary.

The quantitative section of my research methods described earlier identifies that only a sample of the publication profile of the Allan Wilson Centre is utilised to inform the research. This is a limitation, but given the scope of the thesis is unavoidable at this stage. It is fair however to utilise the quantitative analysis that I have done because it is representative of a portion of the publication profile.

3.7. Conclusion

This chapter has presented the research methodology used to conduct this thesis. A case study approach was used because firstly, as Stake (2000) notes, it is my choice of what to study and secondly is an often used method in actor-network theory. My analytical model is based on John Law’s modes of ordering approach (1994a), and follows Davies exemplar of her study of the NHU within the BBC (1999, 2000) and Doolin’s study of the New Zealand public hospital. Following these studies I use a range of data sources, four to be precise: Quantitative citation analysis; press media article review; participant interviews, and; autoethnographic accounts. These data sources enable me to identify the various modes of ordering the Allan Wilson Centre.

Actor-network theory has been used to study institutions of science since its first inception; I see this project as an extension of this – an application of actor-network methodology to a New Zealand science institution. The concept of ‘modes of ordering’ is a satisfying ANT based analytical model to apply to my research – to me it encapsulates the ontologically relativist but empirical realist nuances of an actor-network approach. It

allows me to analyse and describe the ‘realness’ of the Allan Wilson Centre without forcing an *a priori* conception of that reality. The Centre is what it is – I am simply employing a modest sociology (Law, 1994a) to try and follow the actors as they interact and form the networks that make up the Allan Wilson Centre.

Following an actor-network approach the next three chapters introduce three modes of ordering the Allan Wilson Centre for Molecular Ecology and Evolution (I refer to this often as ‘the Centre’). These modes constitute different ways of ordering the Centre, but like Law, I claim only a “modest sociology” (1994a, p. 32), certainly I cannot say that these are the *only* three modes of ordering the Centre, in fact by identifying these three in particular I am by necessity deleting other ways of ordering. I understand how Law must have felt when making this comment:

Sometimes this process is disorienting, sometimes it is exciting, and sometimes it is nerve-racking and painful. Indeed, sometimes the process of trying to order is so unsuccessful that it is simply miserable (1994a, p. 18)

So during this process it was important for me to constantly consider my own impact on the process of ordering. In this sense I have this comment to make:

I have spent the last year thinking about the AWC almost every day; I have talked to the members, read their research, experienced their brilliance and bumped into them on campus. I can’t help but feel like I want to help them in any way I can – they are really good, caring and inspiring people.

Having said these initial qualifying remarks I have identified three distinct and competitive modes of ordering present within the actor-networks of the Allan Wilson Centre. I have labelled these: Doing science, encouraging science and managing science. These three modes interact, intersect and collide in a range of ways, but common to all is the science, this seems to always be at the forefront of the Centre, thus is also at the forefront of the modes of ordering the Centre.

Each of these modes is explored in more detail in the following three chapters; chapter four on doing science, chapter five on encouraging science and chapter six on managing science – more details on their interrelationships, intersections and conflicts are discussed in chapter seven, entitled: Conclusion: Intersecting Modes.

4. Doing Science

Doing science is the core business of the Allan Wilson Centre; it revolves around the purity of scientific pursuit, around the mechanisms used to channel resources into science, and it is about collaborating between institutions and disciplines in order to generate publications. I see similarities to the vocation mode of ordering the Daresbury Laboratory in Law's ethnography; particularly Law recognised that vocation is the core business of Daresbury, the Laboratory he notes "works in the first instance by performing the vocational mode of ordering" (1994a, p. 168).

It is fair to say that the Allan Wilson Centre is probably more focused on doing science than it is on encouraging or managing science and this is reflected in the science focus of all three modes of ordering the Centre. When encouraging and managing the Centre is still fully immersed in many aspects of doing science, if you like, it is the most 'sciency' of the three modes! This chapter comprises three main sections; doing excellent science, doing Allan Wilson's science and doing serendipitous science. Each of these represents a strand of the Centre's focus on doing science.

4.1. *Doing Excellent Science*

I chose to present this doing excellent science section first as, generally, it answers my research question by demonstrating in quantitative terms exactly how much the institutionalisation of the Centre appears to have changed things.

One of the main actor-networks at play in the Centre is what I refer to as the 'publication machine'. This is not a machine in the mechanical sense but rather represents the powerful actor of the 'top journal', in the case of the Centre these are mainly journals such as: Nature; Science; Proceedings of the National Academy of Science (PNAS); Systematic Biology as well as many others. The pull of these journals mechanises the processes of the human actors in the Centre, so much so that one member now will *only* publish in journals with a certain (high) rating. The power of the scientific journal as an actor-network can not be underestimated; it maintains significant control – partly because it is a highly salient measuring device for the quality of scientific enterprise; in that it is

easily identifiable, measurable and comparable. Additionally its saliency makes it a useful device for demonstrating the quality of Research Outputs, which is important under the Government’s Performance Based Research (PBRF) scheme; this is also an important factor. But it seems that the largest draw card for the ‘top journals’ is the readership; it is the main mechanism used by the Centre members to communicate their research results to the wider international peer group.

The rhetoric of the media and society in general performs the Centre and its members as being excellent scientists doing excellent work of an international standard. This is being constantly reinforced by the name of the fund under which they were formed – as a Centre of Research Excellence. In fact it is difficult to dispute the excellence of the researchers within the Allan Wilson Centre; their collective publication profile is outstanding. They publish prolifically at the top of their field and are furthering the established reputation of New Zealand Biodiversity research. I make these claims with the backing of empirical quantitative evidence; my scientometric analysis (for methodological details see chapter three) of the ISI publication profile of the Centre paints a picture of research success. To me the best evidence of this is by looking at the average impact rating of the journals they are publishing their work in. The following graph, figure 2, shows how this has changed between 2002 and 2006:

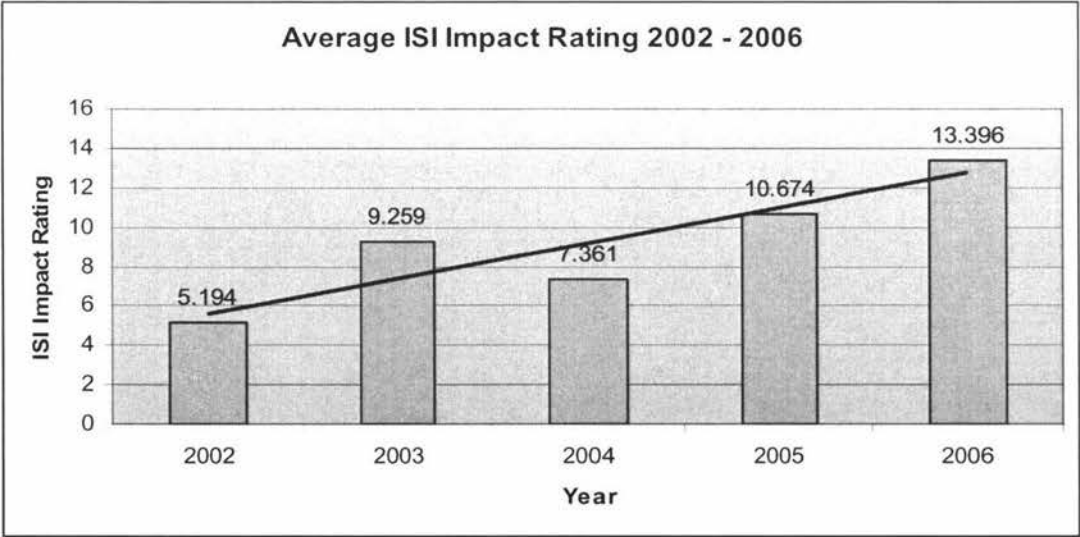


Figure 2: Average ISI Impact Rating 2002 - 2006

Clearly this is a very significant change between 2002 and 2006, although it does vary, overall the trend shows a steep increase. ISI journal impact ratings are a useful means by which to assess the 'quality' of a particular journal. Journals have ratings between 0 and about 50, with the top ten all having ratings over 29. However the majority of journals having ratings below 5 (95%), so for the Centre to have an *average* rating of 5.194 in 2002 still meant that, on average, their research was published in the top 5% of journals, but in 2006 with 13.396 they are now in the top 1%, I find this compelling evidence of their excellence. This shows that the Centre, in their view and in the view of the wider society, is doing excellent science, because doing excellent science is equated with doing science that is published in the top journals, the excellent journals – the ones with high ratings.

The above seems to show rather convincingly that excellent science in the Allan Wilson Centre is often equated with the activities that will probably result with publication in the top journals. When I talked with the members about these activities, two main factors seemed to emerge that together would often result in publications in the top journals; the first was doing visionary science, and the second was doing interdisciplinary science. Many of the top journals are multidisciplinary; they do not publish a particular disciplinary field but publish research that is considered to be the very best. The journal *Science*, which has a 2005 ISI impact rating of 30.927, for instance claims to:

Publish the very best in scientific research, news, and opinion. Whether you're concerned with AIDS, SARS, genomic medicine, Mars, or global warming (*Sciencemag*, 2006, para 3).

So the Centre members realise that interdisciplinary science has a better chance of being published in the excellent journals. However I do think that this driver towards excellence is secondary; primarily it is the visionary aspect of doing science that seems to drive the Centre's research success.

4.1.1. *Doing Visionary Science*

In the Allan Wilson Centre doing excellent science means doing visionary science, and it seems that the institutionalisation of the Centre under the CoRE fund has allowed the researchers to conduct science with more vision. During an interview one member told a

story about an inhibited New Zealand science community; inhibited by a traditional funding system that mainly promotes ‘attainable’ research projects for New Zealand scientists. The member contrasted this with the research scope that the environment created by the Allan Wilson Centre provides and encourages:

At the research level the Allan Wilson Centre allows us to attack harder problems, New Zealand scientists... feel dependent and they have this inferiority complex: ‘we can’t really do that in New Zealand’... We *can* tackle really major problems in New Zealand and make a difference – I feel that this is one of the hardest things we have to face... the funding agencies say ‘here is a nice little problem that you can solve’, but anybody can solve that – lets find the harder one that *only* we can solve... the range of problems that is just over the horizon

This comment clearly shows how important the Centre’s research environment really is; the Centre performs itself as a promoter of unconstrained research. This member for instance describes how the research leadership in the Centre is highly supportive and encourages this ‘lack of constraint’:

There is a lack of constraint as well, you get certain personality types probably, some people, you have an idea and they say ‘hmm... we have these limits here and we have to do this’ but [colleague] never says that, [colleague] says ‘how can we make that work?’

Constraint limits vision; they experience this concern when managing the science relationships with the CRIs. Their science colleagues in the CRIs are performed as being scientifically constrained by the financial model imported from the private sector and therefore being limited in their ability to exercise vision. But the above story also tells of a more general concern with being from New Zealand, and therefore being constrained by the size of the New Zealand scientific market. This constraint sits in complete juxtaposition with the Centre’s accepted idea of what constitutes ‘true’ visionary science. For instance, one of the Centre’s directors, Mike Hendy, is quoted in the press media below describing some research that meets with the Centre’s understanding of the work of a true scientist:

The research is that of a true scientist of great curiosity and a desire to further investigate established theory and understanding (Gray, 2004, para. 5).

Clearly this type of scientist is not constrained; by contrast their ‘curiosity’ and ‘desire’ inform their scientific vision. The second element of doing excellent science is doing interdisciplinary science, which is also characterised by shunning constraint; this is explored in the following sub-section.

4.1.2. Doing Interdisciplinary Science

When I began working on this thesis project I thought that the notion of inter/cross/trans/multi-disciplinarity would be one of the major concepts I would encounter, but in fact I don’t think the Centre really orders along disciplinary boundaries. Certainly not in a political way, it just doesn’t seem to be that big a deal. The way that different disciplines are ordered in the Centre is not so much hierarchical, as seen in other studies (see Jakobsen et al, 2005 for instance), but rather pragmatic, in that different disciplinary backgrounds are performed as suiting different working roles in the Centre. A particular set of roles do appear to have emerged for Mathematicians and Biologists in the new networks, but this does not inspire conflict, but rather is inspired by an acknowledgement that there are labour efficiencies to be gained through the application of specialisation:

There is a disparity in the work that people do... but there is no particular clash... It kind of has to happen that way; it would be terribly inefficient to do it any other way...

So this story talks about the synergies that are constructed between the major disciplines of Mathematics and Biology, and this is a story that has existed since prior to the formation of the Centre through the CoRE fund – in fact when the funding model was announced the synergy between Mathematics and Biology signified a competitive advantage to those organising the Centre’s bid:

When the CoRE initiative came along there was an obvious synergy there of Mathematics and Biology to build a CoRE around

In this way interdisciplinarity and particularly the collaboration between Mathematics and Biology is *how* the Centre does interdisciplinary science. The change that the institutionalisation of the Centre, through the CoRE fund, has provided is through the increased access that different members have to each other and unconstrained nature of

collaboration between members; the synergy is in the new networks as it allows Biologists and Mathematicians to unify where before they would not have had the chance. This unification assists the Centre to be able to do excellent science (as measured by the journals they can publish in) by doing interdisciplinary science, for instance this comment describes how the collaboration between a Mathematician and Biologist allowed unification that resulted in a high rating publication:

Finding a data set that had those properties would be nigh on impossible and [colleague] had a different set of skills that allowed us to unify

More generally, I found the comment below illuminating, as it describes specifically the ‘intangibles’ of collaborative science when one scientist, gets free, ready and willing (i.e. unconstrained) access to other disciplinary knowledge and skills bases:

It has also given me the opportunity, when we meet, to run our ideas past them, so it gives me a way of getting an idea of other issues of science that I am not familiar with... Those are the intangibles of collaborative science... but not a collaboration of the formal getting together and doing research

It does seem that there exists an interesting difference between the Centre’s experience of interdisciplinary collaboration and that experienced within other organisations of science. Specifically I am referring to Jakobsen et al’s findings discussed in chapter two. Jakobsen et al found that “insecurity regarding career implications of working across disciplines as opposed to staying within your discipline was listed as a barrier for individuals in both projects” (2005, p. 23). I kind of expected to find this kind of issue within the Centre – but it just doesn’t seem to exist. Some references were made to the issues that the Mathematicians experienced within their discipline as being associated with Biologists can be perceived as being pseudo-Maths, but these comments were few and far between and made with humour rather than anxiety. I think this is because of the association with the scientific *excellence* of the Centre; in that they are ‘doing excellent biological mathematics’.

4.2. *Doing Allan Wilson's Science*

The name “Allan Wilson” is inextricably linked to scientific excellence in the minds of many New Zealand scientists and other people interested in the science arena, and this makes Allan Wilson a highly important actor in the networks of the Centre. A good example of the power of Allan Wilson as an actor in the science industry can be seen in these comments from Steve Maharey, the current Minister for Research Science and Technology made at an award event in July 2005:

Of course it is easy to talk about high-level strategies and dollars, but in the end it comes back to people doing excellent work. It's Ernest Rutherford, Alan MacDiarmid, Beatrice Tinsley, Maurice Wilkins and Allan Wilson who inspire us and the next generation of students (Massey News, 2005, October 10, para. 14-15)

As I have stated earlier The Allan Wilson Centre is a relatively small research Centre – comprising only ten primary investigators; this seems to make it easier for them to unite under the identity of Allan Wilson. Their affiliation with Allan Wilson is actually quite profound; during an interview several members and I were discussing the scientific impact of the work of Allan Wilson on the development of science over the last twenty years or so. One member offered the following observation in relation to the work of the Centre:

In fact, all of the activities seem to be interests of Allan Wilson

All of the Centre members I talked to seem to be very proud of their association with Allan Wilson – and take their responsibilities as stewards of excellence very seriously. They *visibly* and *proudly* perform the Centre as being fundamentally united through the identity of Allan Wilson.

There are several elements of this strand of doing science that make it a unique and interesting mechanism of ordering the activities of the Centre. One of these is the early realisation by the Centre of the importance of having some sort of united identity under which to do science. I am sure this was heavily influenced by the CoRE proposals requiring that new Centre's must be able to demonstrate “the ability for the centre to

develop a separate identity from its host” (RSNZb, 2006, p. 9) and so even prior to formation the primary investigators were providing a united identity:

Once the short list had been come up with Massey flew us all to Palmerston North, and we met the assessment panel, that was really good, and I think one of the reasons we got funded... because we were all there... we even wore suits and ties I think!

Upon funding the members continued to embody and perform the united identity of the Centre when doing science, they recognised the benefits of performing a united Centre for attracting international recognition and important conferences to New Zealand – and united under the name Allan Wilson. The instant recognition amongst the international science community of the Allan Wilson Centre because of the work that was done by Allan Wilson on evolutionary science is considered to be a very important factor in its international success, and when joined with the subsequent work that the members of the Centre have done also on evolutionary science (one member told me how “the reviewers have always said: ‘New Zealand bats above its weight internationally in this field’) the result is a reinforcing loop, as this member describes:

The fact that now there’s this Centre reinforces that, it has Allan Wilson’s name associated with that, where previously people may not have known that Allan Wilson was a kiwi... it reinforces the whole thing

Although the Centre has clearly developed a significant and distinct identity, they would not describe it as an institutional identity. In fact during one interview I unwittingly referred to the Allan Wilson Centre as ‘an institution’; one member reacted strongly with a humorous but slightly scary rebuke along the lines of ‘we are NOT an institution, but more of an entity’ (from memory). The potential of developing an ‘institutional’ identity reminiscent of a university sits uneasily with many of the Centre members. However, this is completely contrasted with the willing and active development of a specific Allan Wilson Centre entity identity. For those members who had come from a traditional university institution environment (the majority of the Centre’s members), the differences with regard to establishing a united identity where specifically around the problems that do exist with this process in a university institution, but were *not* experienced in the Centre. As this member describes:

There is a sense of identity in the Allan Wilson Centre, which you don't have in [the member's host institute] sometimes in institutions there might be in-bickering... we have made a conscious effort not to have any barriers so there is nothing to breakdown... there is a lot more collegiality in the Centre

The emphasis from the member above was on the increased amount of collegiality experienced in the Centre and the conscious attempts to prevent barriers to collegiality from emerging creating 'in-bickering'. So there is an explicit comfortableness with being affiliated to the Centre, and this is substantially different to what members had experienced within their host institutions. Additionally a generational change was noted; the participants suggested early career scientists whose first professional experiences are in the Centre environment felt an affiliation to the Centre and specifically not to any traditional academic institution such as a University – even if their funding was through a university:

Most of the junior people see themselves primarily in the Centre, not in an institute

I think that the backlash against the concept of 'institution' is a reaction to the problems and isolation that people can often feel when encased within a large organisation as it is difficult to subscribe to a rewarding shared organisational identity in a large organisation. The Allan Wilson Centre is currently still quite small, and this seems to be highly regarded as it allows the Centre's identity to emerge as definitively distinct from the universities that provide the investigators. The Centre is also geographically dispersed which can make it difficult for an organisational identity to develop because, as Davenport and Daellenbach note: "building identity in a virtual organization is probably similar to building an identity in a network, in that they will have issues building identity and achieving legitimacy that are distinct from those that concern individual organizations" (2006, p. 5). These issues seem to be resolved with particular success in the Allan Wilson Centre, as they have an actor-network around which to unite that transcends any particular institution – Allan Wilson himself.

4.3. Doing Serendipitous Science

This strand of doing science within the Allan Wilson Centre combines certain key elements that emerge when the members talk of the Centre and the difference that it has created in their research lives. This section has three subsections; the first ‘manufacturing serendipity’ discusses the specific working practices of the Centre that allow discovery and particularly the clash experienced between encouraging serendipity and trying to manage science. The second subsection expands on the notion of specific serendipitous discovery to consider the more general freedom that the Centre environment promotes, and the impact that this has on research opportunities and careers. The third subsection discusses a counterpoint to the serendipitous view; this is a little heard but present voice emphasising the managing science mechanism of directing research done by the Centre.

4.3.1. Manufacturing Serendipity

Although it seems like an oxymoron the name for this mode of ordering the Allan Wilson Centre has been carefully chosen. The concept of serendipity, which I mean as ‘discovering something by accident while investigating something quite different’ is taken directly from a member who used the term during an interview in an almost apologetic manner, but it seems this concept is important generally within the Allan Wilson Centre. I have prefixed the term with ‘manufacturing’ because the emphasis is not on the completely unexpected discoveries associated with the slightly ‘un-scientific’ overtones of the commonly understood meaning of serendipity but more on the importance of manufacturing or performing, if you like, an environment where serendipitous events can emerge. I think an interesting mode conflict revolves around how the Centre actually does serendipitous science against how they try to manage serendipitous science. This can be seen in this comment made by a member during an interview:

It is almost serendipitous, but it isn't serendipitous if the right sort of culture is there to make people look at things broadly

On the ‘unmanaged’ side of the serendipity coin the emphasis is placed on the importance of creating an environment where serendipitous events can occur, because of an

acceptance that this helps to create important moments of discovery. A good example of Centre working practices that embody this position can be seen when the members recount stories of the annual entire Centre meetings; these meetings seem to act as a facilitator towards manufacturing serendipitous relationships, as this member notes, they promote a 'frenetic sort of activity' that 'creates the environment' for serendipitous discovery:

[The Allan Wilson Centre has a] frenetic sort of activity... chaotic... which is important for creativity... you cannot allocate time for creativity... it just doesn't work like that, a lot of significant breakthroughs come from spontaneous meetings and interactions among scientists. The thing is to create the environment where ideas can be explored and developed, and this is what New Zealand should be doing

This view is relatively common within the Centre, with several of the members describing the Centre as a 'catalyst' for creating serendipitous moments. It is interesting to note that there seems to be uncertainty around whether the Centre's additional money allows an increased level of *already established* activity or conversely whether the additional money facilitates the creation of *new collaborations*. The following comment was made to me during an interview, the member and I were discussing the point raised above, and they compared some current collaborations they were involved in:

I have collaborated with [colleague] and I have collaborated with [colleague], but that is a direct result of the money [AWC funding]... but some of the collaborations are not, like the collaborations with [colleague] are not, except as a result of the money that allowed the meetings to happen

This comment seemed to me to be emphasising the unexpected collaboration with the final colleague that just occurred because of the Centre meetings. This member and their colleague had never met and just stumbled across each other whilst interacting in the context within which the Centre exists. As a result of the publication success of these 'unmanaged' collaborations there is a strong feeling that, where possible, the co-location of members of the Centre was enormously beneficial, as it allowed natural interactions to occur that wouldn't have happened in the traditional university environment. In particular

the co-location of the people from different disciplinary areas was actively pursued; this comment explains how the co-location process worked at Palmerston North:

Although we struggled for space... the opportunity for that interaction was one of the best things... they can hang out together on a day to day basis and just turn to the person next to you and say 'can you help me with this' because it is outside your area... You get them communicating about one thing and it doesn't preclude them discussing other things and opportunities, you break down those social barriers and then all sorts of things become possible because of good communication

Clearly the benefits felt were not just around the new interactions, but also more generally around the increased communication resulting from a more social environment, and because they got the opportunity to talk to other like minded people – which they constantly reminded me – shouldn't be underrated.

Serendipity and science is not a new thing, Shapin, when reviewing a book on science and serendipity makes the point that scientists used the term "to say something about their practice" (2004, p. 374). He goes to quote Pasteur's famous adage "Chance favors the prepared mind" (p. 375). Pasteur was making the point that serendipitous discovery can best occur when the scientist is well prepared and ready to identify the chance event and, more importantly, is skilled at exploiting the opportunity. An example of this is given contemporarily by Davisson, talking about discovery in genetics research and the role of serendipity in identifying mouse mutagenesis. She says "Serendipity awareness should be included in the standard training for all animal care technicians because they are in the "front lines" for finding new, potentially valuable mutations" (2005, p. 344), and also identifies the importance of scientific skill for exploitation of serendipitous discoveries. Clearly this concept is relatively accepted in the scientific community and the members of the Allan Wilson Centre fully utilise its ramifications.

One of the specific tales of serendipity told is about scientific data, and the exploitation of data to its full capacity. I found this particularly interesting as Biology is often described as data rich but theory poor (Krimsky, 1991). That is, it is easy to collect Biological data, but not that easy to fully analyse it given a theoretical framework. The Centre members describe an environment where Biological data could be used more fully, because of the

serendipitous nature of the interactions between the scientists within the Allan Wilson Centre. The members were quite frustrated that there was a lot of information in the data that they didn't get the opportunity to use in a traditional university environment, partly because of funding issues (discussed further in the research freedom section below) but also because they didn't have the access to specific different skill sets and discipline knowledge. The Centre environment by contrast provides more opportunity to exploit the data:

I think the thing about the Centre is that it enables you to see ways that you can essentially mine what you have to the utmost... In a group that has people with different skills it opens your mind to consider alternatives that wouldn't have occurred to you previously

In sum, the general story told of the facilitation of environments that can result in serendipitous discovery:

You cannot force collaborations and interactions to happen; a lot of it is just fortuitous interaction of ideas and people that like to work together

4.3.2. *Research Freedom*

In the above section I made reference to Davisson's perception that all front line scientific officers be trained in "serendipity awareness" (2005), Davisson does not say exactly how she thinks this should be done, but her point is clear. This element of the 'doing serendipitous science' concept could be read as the Allan Wilson Centre's unknowing answer to Davisson's challenge. It concerns the research freedom that association with the Centre allows the researchers.

In New Zealand's contemporary research science environment, prudent financial management is one of the main accepted limitations of research freedom. This limitation, which has been generated mainly by the new public management ideologies implemented by New Zealand Government policies of the 1980s and 1990s, features heavily in the actor-networks of the Centre. One of the main rhetorical devices utilised by the media in reference to the CoRE fund was the additional research that this type of funding would allow. For instance the Dominion Post in June 2002 made this statement in relation to the CoRE fund announcement:

Kiwi scientists will soon be able to do DNA spot checks on people - in a scene straight out of the science fiction film *Gattaca*. This and other technology is now possible in New Zealand with \$60 million of new science funding announced last week for five emerging centres of research excellence. (Dominion Post, 2002, July 24)

This type of research speculation is fairly common in the media sources I analysed, and many references are made to the 'new science funding' of the CoRE fund. In support of this the Centre members contrast the longer term funding provided by the CoRE fund with the financial limitations of other sources of funding, such as Marsden grants. In chapter six we will see that when the Centre is 'administering science' the Marsden fund is performed as being a 'good' fund, but in the context of doing science and when viewed through the lens of 'doing serendipitous science' it embodies aspects of research limitation, for instance this member talks of freedom and the Marsden fund:

The freedom is really good, Marsden is good, but the downside of Marsden is that you have to say what you are going to do before hand... some of this [the research and its directions] is so unpredictable... and I think that the freedom that the Centre provides... is really crucial

This couldn't be clearer; research freedom is facilitated by the CoRE funding model, and this freedom allows the researchers to follow up on serendipitous events that occur during the process of science – something they were generally unable to do previously. I think it is this change that encourages "serendipity awareness" training of the students and early career researchers, because they can actually practice serendipity as a working technique. Members, for instance, speak of being given the encouragement to do research that is more risky – in that it is less likely to result in any publication, but, if it works, is more likely to result with publication in a top journal (as discussed earlier in this chapter). For instance the following member made this comment during an interview – we were discussing the personal impact that the Centre had on their career:

For me it is really good – I get this [stable funding] every year, like a safety net... it gives me the encouragement and incentive to do some more ambitious research, that are perhaps a bit more speculative, you can't guarantee that there will be a publication in the end

Several members were quite specific about the personal benefits of being part of the Centre. During interviews they highlighted the importance of the Centre's validation of

their research excellence and their host institution's acknowledgement of this, this member, for example, describes the benefits for them personally:

Since the AWC I think I have been promoted twice... and being a member of the Centre was a significant part of that... it was a validation of research quality, I suppose

Many of the members noted the rapid acceleration of the pace of research that has occurred since the formation of the Centre. It seems that this research freedom, together with the ability to interact easily and regularly with colleagues in the Centre, are the two main elements that provide an environment that allows excellent research to be done at this accelerated rate. A particular element of this emphasises the importance of having the research freedom to use financial resources to manufacture serendipitous moments. This was performed as enormously different from what was experienced in traditional university institutions; in particular the experience of students was often discussed during the interviews. In the Allan Wilson Centre students and early career researchers have easy access to funds to travel and meet with other members of the Centre, the members seem to work hard to make sure that there are not barriers to this. The concern was that if they do not put money into facilitating the interactions they won't allow the environment for serendipitous discoveries to emerge *and* they won't be training the students and early career researchers to be able to identify them if they occur. Examples of serendipity at play were common during the interviews, even though I struggled at times to understand the science I could very easily understand the importance placed on the role that financial research freedom played in serendipitous discovery. The following two comments come from two sides of an interaction that resulted in a serendipitous discovery:

Very difficult for that to have happened without the Allan Wilson Centre money, that just put us in the same place, and again, it came out of having a chat at the lunch break

Serendipity... unpredictability... the money allows you to follow those up

These two groups both agree that they would have struggled to make the connection that resulted in publication in a top journal, and that the Centre environment facilitated that interaction to that end.

Financial research freedom also resulted in having long-term staffing support; this member specifically identifies the difficulties they would have had securing funding to have a colleague employed:

For me personally has benefited me incredibly; having long term funding, having long term staffing, I couldn't do the work that I do without [colleague], and we wouldn't have [colleague] probably as solidly... I would have been fighting to get funding to keep [colleague] going...

It isn't just freedom to do their own science that is different about the Centre funding, but also the un-allocated money that exists so that researchers can offer places to PhD students and post-doctoral fellows to do their science. This was emphasised by several of the members, and contrasted strongly with previous problems in their host institutions. They spoke of being approached at conferences by truly excellent student and early career researchers and not being able to offer them money to come to their host institution – but now with Centre funding they have this ability:

We have the ability to say yes straightaway to someone very good... if you don't have the background funding... they will have another good offer

Further, now some members actively recruit at international conferences, building on the reputation of the Centre by bringing excellent people into the fold. During the telling of these stories there was a palpable sense of pride that they could do this and that once in New Zealand the recruited students or early career researchers would generally be able to secure external (other Government) funding to do their research. The difference with the Centre funding was the “ability to say yes straightaway”.

Dr John Hay, current head of the CRI Environmental Science Research (ESR), is quoted in 2002 as saying in relation to applying for research funding “In New Zealand, if you miss out, it's your salary and your mortgage. Everything goes down the gurgler” (Collins, 2002, para. 18). This is a relatively common perception amongst the science community in New Zealand, but it seems that the longevity and freedom associated with the CoRE fund has changed the perceptions of some of the members of the Allan Wilson Centre, scientific serendipity is alive and thriving in the Centre.

4.3.3. *Doing Directed Science*

The dominant story performed when members are doing science demonstrates the Centre as embodying a very different research entity from the traditional university environment. As we have seen from the above it tells of a freedom and flexibility to pursue ideas and research leads in a gloriously unregulated fashion – for the good of serendipitous discovery. I think the dominance of this position has emerged because the members are somewhat concerned that trying to actively manage serendipitous discovery may remove or reduce the main ingredient: serendipity.

However this short section discusses a rare but present quiet dissenting voice – one that speaks of concerns over the *direction* of the research conducted by the Centre. This story gives the impression that the Centre may have fallen short in directing research towards the kind of questions posed in the original CoRE application. It points the finger at the serendipitous ‘free’ structure of the Centre and describes it as not being conducive to developing *directed* research. However the story is somewhat clarified, it also tells of an environment that *is* conducive to letting people go and do excellent science, but just not excellent directed science.

This strand seems to be quietly critiquing the Centre for prioritising the doing of science over the managing of science; and the dominance of the doing science mode can be seen here in all of its power – as even when using managerial concepts to criticise doing science they still refer to the importance of allowing people freedom to do the kind of excellent science talked about in the first section of this chapter.

In the introduction to this chapter I made the point that I believed that the Allan Wilson Centre is more focused on doing science than it is on managing or encouraging science, and that doing science is the most ‘sciency’ of the three modes – what I meant was that by doing excellent science, doing Allan Wilson’s science and doing serendipitous science the focus is clearly and completely on the process of science – this should not be underestimated, the doing science ordering mode resonates within the actor-networks of the Centre. The following chapter discusses the how the Centre encourages science.

5. Encouraging Science

The three ordering modes I present in this thesis are inextricably intertwined and exist in this ordering regime only in tentative way, as Law noted, modes of ordering are precarious things that exist only because of their privileging over other, different ways of ordering (1992). You could argue that in the Centre encouraging science is probably the most tentative of the three. Doing science and managing science seem to be 'natural' partners in many organisations, possibly because conflict and cooperation between ordering modes of this type have resulted from the impact of new public management 'managerial' ideologies on extant professional modes. This kind of thing was described by Law (1994a) in the Daresbury laboratory, by Davies (2000) in the NHU of the BBC and by Doolin (2003) in the New Zealand public hospital. However I do not think that this precludes the existence of other things, representing other important modes of ordering an organisation; as such, in this chapter I present the mode encouraging science. Encouraging science is about students, emerging researchers and the public; it is about communicating science and promoting science, about repaying the debt to society for funding by being an institute of quaternary learning, about excellence in education and scientific profile.

This chapter is separated into four main sections; profiling science is about the use of the Centre's profile to encourage science; the stewards of science is about the role the Centre plays in the encouragement of fundamental research in New Zealand; encouraging students is about the importance of education in science, and; reaching society is about the various mechanisms used to communicate with society.

Before continuing I want to make a clarifying remark about this chapter; that is 'encouraging science' does not really sit easily with me. Although it does seem to be the most 'appropriate' way of saying some things, some vitally important things, about how the Centre encourages science, I feel uncomfortable about privileging this over other ways of saying these same things. However, when I tried to represent them in other ways I felt more uncomfortable, so I thought I would present them in the least uncomfortable way, after all it is only a temporary and tentative way of ordering my thesis.

5.1. Profiling Science

In 2005 the Tertiary Education Commission (TEC) conducted a mid-term review of the Allan Wilson Centre. As part of this review they asked the Centre what its future challenges were, one of the responses was “Increasing the Centre’s international profile (to gain more overseas funding)” (TEC, 2005, p. 17). The TEC followed this up by recommending that over the remainder of their tenure (another 3 years – with the potential for continuing funding) the Centre should “consider ways that it might raise the overall profile of the Centre, nationally and internationally” (p. 18). To me this signifies that the Centre is intensely aware of the ability of their profile to encourage science and the importance of using their profile to ensure continuing funding. As a consequent their national and international profile is paramount in the minds of the Centre members, this was demonstrated during interviews when they almost uniformly expressed the importance of marketing the Centre to national and international audiences:

We do have a marketing plan behind ourselves to try and make ourselves known to both a national and international audience

They do this in numerous ways, including by having templates for presentations and publications and by branding Allan Wilson Centre on clothing and small gift items to give away at events and by Centre members using the AWC name on publications. It is also clear that they believe that they have had significant success at establishing a distinctive profile, for instance in their 2006 CoRE application abstract they make the following claim:

Since its inception in 2002, the Allan Wilson Centre for Molecular Ecology and Evolution (AWC) has established a unique identity, derived from a strong international and national reputation. It is now recognised internationally in the field of ecology and evolution as an entity in and of itself, distinct from and beyond the reputations of the various partner institutions (RSNZ, 2006a, para. 1)

Media rhetoric appears to provide support for the Centre’s position above, and to me this signifies the extent of their success at establishing a profile; though in part I think the media’s use of the profile is as a result of their desire for a commonality from which to hang stories about scientific exploits. In other words I think it is easier for the media to

pick up stories about science when they have the established profile of the Centre to help hook the reader into the story.

One of the main mechanisms in the academic world for communicating research results and networking is the conference. In the field of evolutionary studies there are several important international conferences, two of these are Molecular Biology and Evolution (MBE); and Evolution. The Allan Wilson Centre hosted MBE in 2005 and will be hosting Evolution in 2007. Hosting these conferences is seen as being an excellent way to grow the profile of the Centre and in doing so to encourage science in New Zealand. During interviews members presented a common consensus viewpoint on how the Centre had managed to attract the hosting rights for these prestigious international conferences. They consistently agreed that no scientist or single institution in New Zealand could have attracted the major conferences on their own, take the following comments:

What the Centre has done with respect to these conferences is brought together a focal point, these conferences would not be held if it was just a diffused network of researchers, you always need a driver, a name behind

Individuals would try to get a big international meeting held, affiliated, hosted by their University, but I don't think they actually would be successful. I don't think they would have the same impetus. I think there is a certain amount of: 'we want recognition to', of the Centre, and hosting these things is really good for that

I think also that by bringing these conferences to New Zealand the Centre reinforces the 'success' of their international profile in two ways; firstly by showing to the rest of the international science community the draw power the group has, and secondly by bringing the international credibility associated with that 'draw power' to play on a national scale. It seems that the Centre believes that it has more international profile than national profile, this they would like to rectify and they perceive that the conferences go some way to bridging that divide because they associate international credibility to the Centre's national profile.

When it comes to encouraging science the national profile of the Centre seems to be more of a concern to the members than their international profile. To me this is quite justified

because I can see why they have had less success establishing a well known national profile independent of their partner institutions. The five universities involved in the Centre in 2002 play a powerful role in the actor-networks of the Allan Wilson Centre, particularly on a national scale. The universities are well known and embedded in society as institutes of scientific research, as well as this the CoRE model still has the investigators in the Centre being employed by their host institution, not the CoRE itself. This reinforces the lower status of the Centre as its own identity competes for significance in the actor-networks of the research community. The general funding system does not assist this process as the investigators can only run grant applications through their host institutions, not through the Centre (as it is currently not recognised as an independent research institute for funding purposes). All of this sits uneasily with the members as, probably as a result of their international successes, the members have a palpable sense of the Allan Wilson Centre in its own right, and seem very proud of their involvement and its achievements. Particularly there is a distinctive sense of involvement of something other than a traditional research institution:

I think the Allan Wilson Centre has really become a brand now... it is great that it is hosted by Massey, but it is really much bigger than Massey – it is national thing

We are the Allan Wilson Centre, we happen to be hosted by Massey University, but that is coincidental

As we would expect, it appears that some things have changed, at least to a certain degree since the formation of the Centre in 2002. Although the overwhelming performance reinforces the Centre as a united identity with its own distinctive profile there is a shadowy sense of exclusion from some elements within the Centre, and this seems to have been more prominent in the early years, and particularly from those hosted by institutions that may have been less favourably inclined towards the perception of a Massey University based Centre. However this is a dull shadow compared to the general brightness of the standard 'we are the Centre' narrative, it was inconsistent and difficult to pin down and, interestingly, was best expressed by a Massey University based member who suggested that members in other locations may feel excluded and under resourced

because of the numbers located in Palmerston North. In contrast to this other a non Palmerston North based participant made the following comment:

People like [colleague] have said 'there is a room put aside for people from the AWC' which is great because you don't feel like you are imposing, it is part of the Centre – you can go and spend a week there, it is fine

Although this was not actually in reference to Palmerston North the general feeling was that strategies were in place to have people feel like they were part of something bigger; in recognition of the need to have an encouraging common feeling reinforcing the Centre profile amongst the group. In an ironic sideline I think the Centre profile was reinforced for some of the members who are situated at institutions other than Massey by the non-interest shown by their host institutions, as this member describes:

Once it [the Allan Wilson Centre funding] was awarded I was kind of underwhelmed at the response. People kept on going round saying how terrible it was that [other institutions] didn't get any CoREs... they didn't get to host any, but there are people involved

This kind of institutional reaction seems to have strengthened the members association to the Centre profile, and weakened their attachment to their host institution.

5.2. *The Stewards of Science*

When the Centre is performing the doing science mode of ordering, the discourses of the Centre talk about the scientific excellence that emerges from the synergistic interaction of the combined skills and knowledge of the various actors during scientific collaboration. Within the encouraging science mode the stewards of science is all about using the synergistic qualities of the Centre as a steward for promoting science and the Centre's impact as a catalyst for encouraging molecular evolutionary science internationally and in New Zealand.

The stories told about the management of the Centre's finances talk about the Centre operating under an efficient democratic model, where each primary investigator is allocated around about the same amount of money, with a small and accepted distinction between wet lab and dry lab scientists ('wet lab' being scientists who work in a

laboratory environment – most often biologists and ecologists; ‘dry lab’ being mainly theoretical scientists – usually mathematicians and computer scientists). Centre members are proud of the success of this model, which they believe is quite unique amongst research institutes, and they allocate its success to a principle of scientific stewardship:

It is closer to this model of giving key people money and saying ‘spend this as you see fit’, bearing mind that you answer to us and we expect you to produce

We said, ok here’s this amount of money that we have, we’ll split it amongst the investigators and go off and do good science, go do your work, you know

There is a very tangible sense of the Centre, to use a member’s language, as a “broker of good science”. They talk specifically about the Centre’s financial stewardship as playing its part in the New Zealand Government’s science funding framework:

What you are doing is trusting the CoRE to be the broker of good science... between the Government and the lower level scientist

The feeling is that by funding fundamental or ‘blue skies’ research through the CoRE initiative the money is getting to the right place, and that prior it was not. This is extrapolated to a fuller and more general encouraging science level, as the rhetoric within the media tells regularly of the importance and excellence of the fundamental science conducted by the Centre, you can feel the trust that the media rhetorically places at the hands of the scientists. Simon Collins for instance wrote an article for the New Zealand Herald in May 2002 (May 4th: ‘An Endangered Species’) criticising the science funding model that has been applied in New Zealand, and particularly the issues associated with a lack of fundamental research funding in taxonomy. The article tells of the declining expertise across the board in New Zealand science and the student and early career brain drain, all as a result of a lack of science funding, but sees the CoRE fund as the best potential reversal of this trend, this is an excerpt:

But there are some signs of progress. The most concrete change so far is the \$60.6 million which the Government is putting into five new "centres of research excellence". One is the Allan Wilson Centre for Molecular Ecology and Evolution, based at Massey University and including researchers at Auckland, Victoria, Canterbury and Otago. "Within our core programme we will

have projects in molecular biology. It's also going to involve studies on plant taxonomy and plant physiology and similar developments in animal biology," says Peter Lockhart, one of the centre's principal researchers. (para 31-32)

Collins's views are not isolated – the general media rhetoric revolves around the CoREs as stewards of excellent fundamental science. Another example of this can be located conjointly when the Centre is 'doing science'. When they are doing science the publication machine is in full flight, this was discussed in the previous chapter and is also an important aspect of encouraging science – the stewards of science concept is heavily reinforced by the recognition of publication success in peer networks and in the media. As I have discussed earlier, the media rhetoric powerfully reinforces the excellence of the Centre's science, for instance these excerpts come from the Evening Standard and Massey News respectively:

The research, headed by former Palmerston North scientist David Lambert, is featured in the latest issue of the internationally renowned science journal Nature, released today. (Hurley, 2003)

The research by the team in New Zealand is the cover story in the latest edition of a prestigious American based science journal, PNAS, appeared in the latest issues of National Geographic and is making headlines world wide. (Massey News, 2005, November 18)

The terminology leaves no doubt about the perceived skill of the Centre's scientists, phrases such as 'internationally renowned' and 'prestigious' clearly embody the excellence of the research conducted by the Centre and perform the scientists within as quality stewards of science; if the research appears in National Geographic then the money must be well spent.

Stories are also told of the Centre as a catalyst for the development of Molecular Biology in New Zealand generally but also specifically within certain institutions. I have used the term catalyst as it captures the charismatic aspects of the Centre as a champion of biodiversity; it seems to act as a catalyst to allow an environment to emerge where things happen. It is worth noting at this point how the Centre as an institutional structure is clearly performed as a significant point in the actor-networks of the *international* biodiversity discourse; although a side point, this certainly helps to explain its success as a CoRE within New Zealand. Back at a local level, several members of one institution

talked specifically about their on-going quest to promote molecular biology within their host institution. Their participation in the Allan Wilson Centre was seen as being a direct catalyst to building a bridge between the established traditional biological disciplines and the emerging molecular biological disciplines.

By backing excellence the Allan Wilson Centre has been a catalyst by fast forwarding collaborative relationships that may have emerged anyway, but has enabled a significant expansion of these and an expansion and progression of the careers of those involved. As an example of this one member identified a specific collaboration that has emerged between their group and another Centre member and talked about their belief that this involvement, facilitated directly through the Centre, had significantly contributed to achieving a better research result:

This is the first paper that I have had in a long time that has just sailed in. I sent it in, and got reviewer comments back in two weeks saying this is good... their [Centre colleagues] contribution turned it into a paper that I think is much better, and would never have happened without the Centre

They go on to talk about other examples of the same collaborative success and then conclude with:

And they are just the papers I can think of that got into good journals

One of the key mechanisms used to explain how the Centre encourages science by being good stewards is encapsulated in their use of the term 'synergy'. Synergy as a concept arose from general systems theory and refers, in that context, to the different behaviour of a whole system that cannot be predicted by the behaviour of its parts as separate entities. This is suitably relevant to the Allan Wilson Centre and the other CoREs as they can be seen as a reformation of several established systems (groups of researchers) into a new system through the application of new funding processes. This view of the Centre is one that is almost commonly performed by the Centre, some members, during interviews, were very specific in the use of the terminology 'more than the sum of its parts', such as in these evaluative comments:

The Allan Wilson Centre combined a group of ten people with diverse interests and allows them to feed off each other and create an environment that is more than the sum of its parts

However other members are more general in their comments, but still indicate the importance of the newly combined group. This, for instance, tells a story about the many invitations the Centre receives, as a group, to participate in or host events or reverent visiting fellows, and specifically identifies that this would not have happened without the Centre grouping:

Whereas as individuals we might never have seen similar invitations – we wouldn't have been on their radar. We are, as a group, recognisable as a brand name and we have an observed performance. We have success in that international sphere... That relates to the 'business thing' about having identity, and perception and being known

The description used in the above comment "we wouldn't have been on their radar" is, in my view, a particularly salient use of imagery – you can imagine a series of groups of or individual researchers as systems invisible to these 'inviters' – but the combined system (i.e. the Allan Wilson Centre) suddenly, mysteriously, appears on the 'inviters' radar screen along with the other potential invitees – it is not that a group of dots on the radar converged to form a larger dot – but as if the group suddenly appeared to be something that is more than the sum of its parts. Also interesting in the comment above is the uneasy undertones concerning this 'business thing' which is most often associated with managing science, and particularly its importance when encouraging science.

From the above, clearly synergy remains an important concept to the Centre, and further evidence for this can be seen in the words they chose to include in their 2006 CoRE fund application; the abstract contains the following sentence regarding the Centre's success in the first five years:

This hallmark of its success is a consequence of a cooperative enterprise amongst New Zealand biologists and mathematicians in which the whole has become more than the sum of its constituent parts (RSNZ, 2006a, para. 1)

I think this association with the concept of synergy is reinforcing their own belief that they are good stewards of science, and that the public money they utilise to do their

science is well spent on them as a group rather than on them individually, as together they constitute more than their sum of their parts.

5.3. *Encouraging Students*

The importance of students and post-doctoral fellows in the actor-networks of the Centre cannot be overstated; they embody the past, the present and the future of the Centre. Together with the skilled technicians they are its front-line laboratory and computer workers – conducting a significant proportion of the hands-on science – all under the gaze of the investigators. The relationship between the investigators and the students and post-doctoral fellows is not like a relationship between a worker and supervisor in a profit-centric business. It is performed as a partnership, where mentoring and assistance go hand in hand with research freedom and the ‘lack of constraint’ present when doing science. In this way the Centre is committed to encouraging science by encouraging students. Several mechanisms they use to encourage students are discussed in the paragraphs below; as are the concerns they have for students of science in New Zealand.

The Centre embodies excellence in education as well as excellence in scientific practice; they perform themselves as an institute of quaternary education, one step removed from New Zealand’s undergraduate tertiary sector, as this member notes:

We see the mentoring of these young people as critical... to some extent we are talking about quaternary education institution... post-graduate, post-doctorate... in a sense we see ourselves in that niche

The concept of ‘niche’ in this quote is slightly misleading; they have found themselves in this position because of the undergraduate science education system in New Zealand. They do not see themselves as one step removed from international tertiary science generally but specifically they recognise that New Zealand science students straight out of their undergraduate programmes struggle to participate in the Centre’s science, they went to great lengths to stress that the problem was not the quality of the students but the educational opportunities provided by the system. Students from Germany and the United States did not have the same difficulties due, primarily, to their expanded and more generally interdisciplinary undergraduate programmes.

It is interesting to note that this seemed to be an issue also for the Finnish equivalents of the CoREs. This may signify a serious issue in this type of funding of excellence in research; take for instance the following passage from the 2004 International Evaluation of the Academy of Finland, conducted by a panel chaired by Michael Gibbons:

Centres are also contributing to the establishment of what are perceived to be a privileged tier of researchers, especially in the universities where some dual funding is allocated for them. The researchers in the centres may also be full time employed and not required to do any teaching, which again furthers the development of a two tier system and can undermine teaching at this level (2004, p. 30)

The members of the Allan Wilson Centre seem to be very aware of this concern and consequently treat the encouragement of education and students as extremely important. Most important is the encouragement of students who are scientifically open-minded and who can think along interdisciplinary lines. Certainly the extant state in the New Zealand tertiary sector is characterised by Centre scientists as breeding mono-disciplined students and scientists; take this quote for instance describing the Centre's alternative perspective:

We are trying to keep the students mind open, I see a lot of this stuff is closing it off. This is the trajectory of 'I'm a Biochemist'; 'I'm a Taxonomist'; 'I'm an Ecologist', we are closing off their minds, and I think one of the important things is trying to keep their minds open

This also seems to indicate that many of the scientists in the Centre appear to be performing their scientific identities as interdisciplinary, preferring not to be characterised as 'mono-disciplined' (Max-Neef, 2005) and specifically struggling to locate themselves within any discipline boundary, which they might characterise anyway as arbitrary indicators of social relations rather than useful differentiators of knowledge (Klein, 1996). This creates issues for encouraging New Zealand science by encouraging New Zealand students as most New Zealand students are 'disciplined' early in their academic careers and do not easily develop the interdisciplinary views shared by many Centre scientists, which appear to be necessary to 'make it' as post-graduate students.

Encouraging students is not just about encouraging New Zealand students; the Centre also appears to encourage science by encouraging international linkages, both by attracting international students and early career researchers to come and study in the

Centre but also by forging links with international science institutions which become important collaborators. I think that this is an 'encouraging science' strategy that the institutionalisation of the Centre has significantly changed. At the beginning of the first CoRE funding cycle a Centre member is quoted in the press as saying:

It would also allow the centre to build ties with institutions around the world, give scientists more time to spend on research and equipment and pay salaries to graduates. The money would also stop graduates from having to go abroad to do research, and could even help entice many back to New Zealand (Eames, 2002, para. 11-12).

I find this quite enlightening, as it emphasises not only the importance of international linkages, but on attracting and retaining New Zealand early career researchers. This is seen as vital to encouraging science in New Zealand. There is an important link here within encouraging science between the 'AWC' profile and encouraging students' activities. In doing science I discussed one of the changes experienced by the Centre members after the CoRE funding was allocated, this was the new found freedom to offer money to attract excellent emerging researchers to join the Centre, this extends in a similar way to market the Centre at international conferences to attract the best and brightest student and early career researchers with substantial success. Linked to this is the publication profile of the Centre – as a marketing strategy to retain and attract the best students and early career researchers, and also because of the new government PBRF funding regime, students will want to conduct research in an institution where they become prolific high quality publishers of science, this can be seen in the media rhetoric:

Prof Hendy said the funding would also allow the centre to retain some of the bright, young New Zealand scientists trained in bioinformatics and biomathematics who would otherwise move overseas (The Press, 2002, June 20, para 1.).

5.4. *Reaching Society*

Reaching society is performed an important part of encouraging science in the Allan Wilson Centre. Reasonably, many of the Centre members seem to see general society as representing the main source of their direct funding, more than the government agencies that actually provide the funds. This makes reaching society important to ensure the

continuance of funding for their type of science as well as the more general desire to popularise research findings.

Although I think that the Centre had tendencies towards this type of ordering anyway, it is reasonable to think that the selection criteria provided by the government agencies for selecting CoREs acted and continues to act as a powerful actor in the network, ordering the Centre to 'reach society' in a fairly specific way. The 2006 Government selection criteria for assessing CoRE applications explicitly identifies the importance of reaching society; they make several references to a 'wider community', which seems to refer to the general society but also some specific community groups such as 'end-users'. There are two main parts to the selection criteria which I think have played a significant role in the actor-networks of the Centre, the first is contained within the "Access or Human Capital Development" objective (there are four government objectives used to assess CoRE applications), this identifies the importance of linking the Centre's research programme "to the skill needs of the relevant end-user community" (RSNZ, 2006b, p. 7). The second is contained in the "Relevance: Contribution to New Zealand's future development" objective, one of the selection criteria states "Transfer of knowledge to end-user, communities and other researchers", and the 'indicators' of this criteria include:

The involvement of end-users and the wider community in the planning, implementation and uptake of the research programme; the plans for promoting the Centre's activities to the wider community, including where appropriate, for commercial gain; the strength of institutional connections with stakeholders in industry, community, and iwi, and; the impact on Māori and Pasifika from the research (RSNZ, 2006b, p. 8)

On the basis of the above and other selection criteria all existing CoREs were required to reapply for a second six year round of funding in 2006. Addressing the parts of the selection criteria quoted above the Allan Wilson Centre's abstract of their 2006 application made the following comment with regard to reaching society: "Outreach and implementation of our research is an AWC priority, together with the development of educational resources" (RSNZ, 2006, para 2). In fact the Centre planned to allocate about 3 – 4% of their total funding on 'reaching society' activities in their round two budgets, this indicates a significant commitment to these activities.

Reaching society is not a new thing in the Allan Wilson Centre. Many of the members talked about society outreach initiatives and educational programmes that they had been involved with prior to joining the Centre, in many instances their involvement with Centre had simply allowed a significant expansion of this kind of activity, but in some cases it allowed new activity to be launched. The media rhetoric surrounding this mode of ordering the Centre spoke of some of these activities, examples include the Centre's participation in the Royal Society of New Zealand's Science, Mathematics and Technology Teacher Fellowship programme, whereby secondary teachers spend time working on science projects with Centre scientists; the organisation of the DNA 50 public lecture series in 2003 celebrating 50 years since the discovery of the structure of DNA; the writing of popular books and the delivery of public education programmes on New Zealand native animals, and; the participation by Centre members in lectures on various evolutionary topics in the public domain, including for a church group in Palmerston North. The general rhetoric in the media revolves around the Centre as a communicator and promoter of science.

Within these interactions with society described above it is interesting to consider the lens that the Centre uses to perform its image to society, when doing this I found it useful to think about Gail Davies findings at the Natural History Unit of the BBC and particularly the relationship between the 'amateur naturalist' and 'television manager' ordering modes (2000). In my interpretation both of these, often conflicting, modes demand an aspect of 'reaching society', the manager specifically wants to ensure efficient production leading to high levels of viewing, and the 'managerial' artefacts of the NHU embodied this desire – things like viewer reporting systems, efficiency processes and the like. The amateur naturalist also aims to reach society – but through the accurate dissemination of interesting science; the modes clashed over the methods used to get the information from nature to the screen. Davies identifies this conflict as a concern on behalf of the amateur naturalist over the *accuracy* and *integrity* of the science being communicated and a concern on behalf of the manager with the pressures of managerial efficiency. This clash can be seen in this following amateur naturalist comment, quoted by Davies: "You do come across situations where you say 'BBC' to scientists and they say sod off, because somebody has trampled on them" (2000, p. 548). Clearly the actions

caused by concerns of managerial efficiency have at some stage 'trampled on' the accuracy and integrity of the science. Bill Doolin found a similar clash in the major New Zealand public hospital, though in this case it was the quality of clinical care experienced by patients that was thought to be suffering when patients were viewed as 'clients' through the lens of new public management (2003). I think in Doolin's case all parties were still trying 'reach society', it is just that different modes position 'society' in different, and competing ways.

When encouraging science the Allan Wilson Centre is most closely associated with the NHU's amateur naturalist mode and the traditional clinician mode in the public hospital, in that their interest is in the bringing of the professional product (their science) to the public with accuracy and integrity, and this is the lens through which they portray the Centre to society. Most recently this can be seen in how they are ordering arrangements for the Evolution 2007 conference, including partially funding a documentary on the life and science of Allan Wilson, in the hope of promoting the importance of evolutionary science to the next generation of New Zealand students and to society generally.

Although the general voice in the Centre speaks of the importance of reaching society to encourage science, the difficulties with bridging the gap between the Centre's scientific exploits and public understanding are also present as a force in the networks of encouraging science. This seems to be consistent with the general discourse present in scientific society of the 'burden of communication', a neat example of this can be seen in the words of Dr James Watson, former president of the Royal Society of New Zealand: "Often scientific work seems obscure and irrelevant to the public because they are tackling a very small piece of a very large puzzle" (RSNZ, 2004, para. 6). Some Centre members were concerned, not over society's ability to understand the science, but rather simply whether the public would be interested in the happenings of the Centre. During an interview one member commented on the absence of Centre authored propaganda in the media over the hosting of the major international conference Molecular Biology and Evolution in 2005 – another member commented 'but you have to wonder how much the general public really wants to know?' (from memory).

I think the above shows a rather important point in encouraging science generally; that is the divide the Centre members feel between themselves and those they aim to encourage. What is different, I think, is that now they want to bridge that divide and are driven to do so, through the application of managing science techniques and doing science achievements.

This chapter has presented encouraging science; the following final chapter presents managing science, the final of the modes of ordering the Allan Wilson Centre.

6. Managing Science

The managing science mode of ordering the Allan Wilson Centre is all about the mechanisms used by the Centre to manage their science – specifically the administrative and entrepreneurial activities of the Centre. These include: Dealing with government agencies; organising and hosting conferences; negotiating contracts between various groups; building the Centre profile; marketing, and; managing budgets. This performance of the Centre draws heavily on discourses associated with private managerialism and new public management.

6.1. *In the Beginning...*

The ‘business’ of research has been part of the ordering schema for the Centre since prior to its formation in 2002. The original selection criteria published by the Government for potential Centres had a section entitled ‘Governance & Management’ this had specific items that had to be covered by Centre of Research Excellence applications including business planning, proposed management and governance structures and financial systems. Also included was reference to the potential for a Centre to become self-funding in time by attracting funding external to that provided by government. In the latest (2006) version of this document the specific phrase reads: “The prospects for the Centre to develop into a viable entity in the long term will be evaluated by the proposal showing a clear and probable path for its future development” (RSNZ, 2006, p. 9). This terminology is highly suggestive of what many scientists would refer to as ‘business’ speak – phrases such as ‘viable entity’ and ‘probable path for its future’ leave little room for doubt that Government policy is concerned with the management of the science. In support of this the New Zealand Government press release announcing the formation of the first five Centres of Research Excellence in March 2002 specified the importance of these entities as providing research to “expand our knowledge base in key areas to boost economic, environmental and social development” (New Zealand Government, 2002, para. 4). From the beginning, managing science was significant in the actor-networks of the Allan Wilson Centre.

6.2. *The Long Arm of New Public Management*

Science policy and funding in New Zealand has become increasingly more economic since the reforms of the 1980s and 1990s (Leitch & Davenport, 2005). These changes came as a result of the more general permeation of 'New Public Management' ideology into the New Zealand public service (Doolin, 2002). New public management is "characterized by the introduction of private-sector management practice and discourse into public services" (Doolin, 2002, p. 371). This permeation has impacted on the way science organisations in New Zealand are constructed and performed. Simpson for instance notes the significant changes that have occurred in the New Zealand science sector and particularly the change from a centralised discipline based Government research institution (the DSIR) into vertically integrated entities serving a specific sector (the CRIs) in 1992 (Simpson, 2004). The universities were also significantly impacted by the permeation of new public management into the Government's science policy agenda. University research funding prior to 1990 was allocated based on institutional block funding (Carter & Bollinger, 1997), but since then contestable funds have become normal with scientists in universities and the CRIs now able to apply to the public good research fund; the Marsden fund and other research funds as appropriate. This new system is managed by matching proposals to 'output classes' deemed strategically important in the Government's science policy arena (Carter & Bollinger, 1997).

Clearly the tenets of new public management have profoundly impacted the nature of scientific research in New Zealand. The managing science mode of ordering the Allan Wilson Centre sits within the Centre's struggles as an actor in the post-reform Government funded science industry; the Centre is performed as a sometimes passive and sometimes active actor in the world of competing managerial structures.

I found it useful to conceptualise managing science as having two associated and complementary strands; the first, administering science, is around the specific working practices used to prioritise science over administration or vice versa and the conflict this creates within the Centre. The second strand, enterprising science, borrows the word enterprise from John Law mainly because the embodiment of enterprising science with

the Centre reminded me of the heroic nature of enterprise within the Daresbury Laboratory (Law, 1994a; 1994b), and particularly the challenges of conducting the enterprise of science within the wider managerial environment of general new public management and government funded science.

6.2.1. *Administering Science*

The concept of administering the Centre creates conflict for many of the members. This conflict is specifically around the prioritisation of the Centre's administrative functions over its scientific functions or vice versa. There is a general frustration, but also a recognition, of the need to be involved in what is described often as the 'administrative burden' of running the Centre. This was embodied and performed regularly during my research, and appears in many forms.

A classic example of the administrative scientific conflict was shown when one Centre member recounted their involvement in the organisation of a major conference, they clearly supported the efforts (in fact they heralded the efforts) but felt that the involvement took an enormous amount of time away from their primary goal of research. The conflict is between the scientific benefits of hosting conferences, such as the attracting international visitors to develop linkages around the world and the Centre's own scientific research profile, and the administrative time and cost that goes hand in hand with hosting international conferences.

Generally the members do not seem surprised by the administrative issues associated with hosting conferences, and tend to put forward a combined positive face, highlighting the substantial benefits. In the news media, a reflection of the Centre's outward facing profile, conferences are only discussed in highly positive tones, for instance: "the success of a phylogenetic conference in Whitianga last month, which attracted biologists and ecologists from around the world, already showed work by New Zealand scientists was considered world class" (New Zealand Herald, 2002, March 7, para 12), I think that this is primarily because the administration associated with a Centre hosting conferences is not new thing, but they are concerned that the public realise the scientific benefits and not just the administrative burden.

The Centre makes a concerted effort to keep the formality of the administrative requirements away from the researchers whenever possible; the role of the 'administration' of the Centre is broad, and is focused on efficiency. The efficient running of the Centre seems to be judged by the amount of administration that is not conducted by the researchers, they try to only get involved in administrative activity when absolutely necessary.

Centralised administrative activity includes the facilitation of the contracts between the researchers in different institutes and the active coordination of funding applications across the Centre. Applying for research grants is the core business of any New Zealand scientist, and the formation of the Centre is sometimes seen as an opportunity for the administrative burden associated with the application for research grant funding to be lessened for the researchers. However, there is a palpable sense of frustration that they cannot act as a 'complete Centre' because of the difficulties between the institutions that make up the Centre (though most agree that this has dramatically improved since 2002). Specifically they were frustrated that they cannot apply for funding through the auspices of the Centre, for example this exchange is between two members in an interview:

p1: My University does not want me to run a Marsden grant through anything other than my university

p2: Which is definitely something we need to sort out – it kind of makes this idea of a multi-institution Centre a bit of a mockery

p1: It does really

The traditional administrative university system is often contrasted with the 'lithe' administrative system associated with the Centre. A good example is the attitude towards the Marsden fund mentioned above, administered by the Royal Society of New Zealand. Generally, when managing science, this is performed as a 'good' fund, as this member describes:

They attract excellence, and once they decide to fund you they don't bog you down in administration

It is 'good' because it is basically reasonably flexible and not administratively onerous on the researchers' time, they are left to get on with the project. The administrative problems that do exist are more associated with having to 'run' the grant application through the administrative system of the university. The Centre, to the members, embodies the antithesis of a bureaucracy and is performed as such. For instance, during an interview, this member describes the change they have observed between a traditional research institution, such as a university, and the Centre:

The bureaucracy has virtually gone... the culture within an institution like [member's host institution] and the culture with the Centre... one is the opposite of the other... we are trying to breakdown the hierarchy... When students want to apply for travel funds – it is difficult within a university, there is only x amount of dollars... from our perspective we want the students to go away, the money is there, they know that they can go out and share their science – it is far more relaxed here

The imagery is very strong in the terminology used by this member; 'one is the opposite of the other' and 'far more relaxed here', it is clear that they perform the Centre as significantly different from its constituent universities.

John Law describes the 'administration' mode of ordering the Daresbury Laboratory as creative rule following, specifically he says "in this ordering narrative *proper administration is creative*" (1994b, p. 257, emphasis in original). What he is saying is that the administrative judgments do not need to be mundane, only that the policing of the resultant administrative processes are performed as needing to be carefully followed. I see a great deal of sense in this identification, and it provides a very useful position from which to consider certain administrative exploits of the Centre. For instance the decentralised funding allocation model used within the Centre differs from other research institutions, including other CoREs, in that the money stays very much with the primary investigators; they have a substantial amount of control over where the money goes. This, to me, is creative administration – although it increases the administrative burden on the researchers it allows the dollars to be very close to the lab benches and computer processors.

The feeling within the Centre is that money should be made to be as close to the science as possible, and that this administrative judgment will ensure that funds are spent to facilitate those things that create excellent research. This does not just include reagents, equipment and consumables it also includes significant travel expenses, though there is also a clear concern over how this is viewed. I think the Centre generally is concerned that people might think they should be spending less on what are considered (non-science) administrative items. But they, generally, have a clear recognition that saving money through administrative savings can be disadvantageous, as this member notes:

We spend less than 10% of our budget on non-science issues – but saving money through efficiency comes at a cost – there is a lot of evidence of the damage that penny pinching can do

The above discussion shows two distinct facets of administering science. One seems to represent a 'natural' disdain for administration and the other seems to represent a certain satisfaction in the 'creative administering of the science' that enables more money to get to the research. These appear to be simultaneously in conflict and in harmony, and so it stands to reason that most of the disagreements within the Centre are attributed to administrative issues. This was evidenced during an interview when one member made this comment:

Tensions between investigators is not so much scientific issues, but more ways of doing things, Centre processes

And several members discussed how frustrated they were with the institutional game playing that they perceived played a major part in the formation of the Centre. The concern was not around the negotiation process (which were perceived as necessary) but specifically the costs associated with reaching a resolution, because that money could have gone to science. This theme remains with the current function of the Centre, as this member describes:

There was a lot of legal and administrative bluster; and there is still a fair bit of admin involved... When we get together we generally spend 95% of time on administrative matters and 5% on scientific matters

In conclusion, administering science in the Allan Wilson Centre is part creative bureaucracy, but more so actively disliked necessary evil. Overall the Centre members do not like to perform the administrative role because they are scientists!:

When the Allan Wilson Centre is humming, it's because everybody is talking about science, when we start to get bogged down, it's when it starts to be nit-picking about budgetary issues

6.2.2. *Enterprising Science*

In September 2002, a few months after the formation of the first five Centres of Research Excellence, including the Allan Wilson Centre, then Minister for Research Science and Technology, Pete Hodgson, made the following comments with regard to innovation and enterprise:

Innovation is most likely to flourish where scientists and entrepreneurs mingle to the point where the distinctions are lost. Where academics flick in and out of commerce, or for that matter the policy arena. Where clever people look back on a career so eclectic and varied that describing it to a stranger becomes a burden. Where venture capitalists, technologists, marketers, designers and lawmakers live and work amongst each other (Hodgson, 2002, 'speech notes: exploring innovation')

The comment above reflects the policy climate within which the Centre was making sense of how they should manage their science. As I have already said in the introductory comments to this chapter the managing science mode of ordering the Centre talks of the struggles the Centre has within the 'new' public management in post-reform New Zealand. These concepts include those things specified in the Minister's comments above – such as entrepreneurship, venture capital, marketing and design. Cohen, McAuley and Duberley summarise that language used to describe scientific endeavour is changing from "Metaphors of "discovery", "forging frontiers", and "working at the cutting edge"" to ""wealth creating," and "life enhancing," "Competitive," "market oriented," and "entrepreneurial"" (2001, p. 145). The section discusses how the Centre makes sense of these changes in metaphor in the scientific world.

To me enterprising science is represented by some very specific struggles within the Centre, particularly these were around how the entrepreneurial or enterprising activities

should be incorporated into the general functioning of the Centre. These struggles include: Cultural issues experienced when collaborating with scientists in the Crown Research Institutes (CRIs); anxiety when considering the Government's apparent focus on applied science and commercialisation of science rather than doing science for science sake; frustration and acceptance around the limitations of contestability and competition in funding models, and; the perils and privileges of adopting a Centre identity separate from a university identity. The enterprising science of the Centre is about how these struggles are resolved to the benefit of the science, and about the stories told by Centre members to reinforce their adopted method of managing science.

Working with scientists in the CRIs provides an interesting juxtaposition for the Centre members. It is clear that the participants generally hold that the CRI and university collaboration framework is the framework within which most of the excellent international research is done, and this is supported in the literature. The Co-operative Research Centres (CRCs) in Australia for instance were established to bridge the gap between pure research conducted in the universities and applied research conducted in the non-university sector (Turpin, 1997) enabling the formation of research Centres that, since, have been heralded as successful, particularly in the commercialisation and technology transfer arena (Grigg, Johnston & Milsom, 2003).

However the Centre struggles to work formally with the CRIs, in this context, managing science does not come easily because of the difference in the financial mechanisms between the Allan Wilson Centre model and the generalised CRI model. The CRIs (formally) have very different expectations from the Centre; as one member describes they don't have the luxuries that scientists in universities enjoy, they have to charge out their time at inhibitory rates. A formal collaboration was given as an example, this required Centre members and CRI scientists working together – and the costs associated with the CRI involvement was seen as inappropriate:

Their people [a CRI] are charged out at \$1000 per day... there was some considerable concern particularly from the collaborators as to how much money would actually go to [the project]

The relationship concerns with the CRIs extend beyond the problems with just managing Centre and CRI collaborations, there is a significant thread running through this mode regarding the type, scope and direction of research undertaken under the auspices of the CRIs. The Centre's management model is performed as distinctly different from the CRIs model – particularly 'enterprising science' in the Centre seems to be characterised by the ability to vision, performed as an almost heroic activity bound up in the freedom associated with the Centre's ordering mechanisms. We might understand 'excellent science' as a mode of ordering a particular set of practices held together to represent, on the one hand, excellently conducted science and on the other hand a set of practices that speak to 'visionary science'.

The Centre members were glowing of the standard of science conducted in the CRIs; they are celebratory of their fellow CRI scientists, in that the science conducted within the CRIs is seen as being of an excellent standard, the concern (in fact the term horrified was used by one member) was the lack of scientific vision allowed by the management model implemented within the CRIs.

The Centre has demonstrated itself as being a pioneer of evolutionary science on an international scale; and they distinguish their research philosophically from the kind done primarily by the CRIs. The CRIs are driven by user demand and consequently are performed by the Centre as having scope for very limited scientific vision; the argument is that there must be two-way interaction between theoretical research (blue-skies) and practical research (applied). The blame for this short-sighted scientific vision is placed squarely on the shoulders of the policy climate that contributed to the development of the CRIs as commercially driven entities and specifically *not* on the shoulders of the scientists employed within. In fact the Centre members tell of the frustrations of the CRI scientists at the commercial model and emphasise the efforts of these scientists, generally informally, to collaborate in spite of the inhibitory environment. This is the enterprising solution; the scientists (often colleagues and friends from previous positions) collaborate with the Centre members in their spare time and holidays. The upshot is that when managing science collaborations with the CRIs; formally collaborating does not promote

excellent science, but informally collaborating does promote excellence. This is extremely frustrating for the Centre members, as one member describes:

You could very easily see that these institutions could gain significantly by formally recognising these collaborations

In spite of the above there are grave concerns over perceived changes in perspective some scientists within the CRIs are undergoing. The concern is around the 'indoctrination' of these, previously liberal, scientists into a business way of thinking. One interview group recounted a story of needing to urgently write a piece for a conference and expressed consternation when a CRI scientist commented along the lines of 'that will be about 0.2FTE of my time – I am not sure if I can find that over the next 8-12 weeks'. They tell of a university scientist thinking in a very different way, as in 'I wonder if I can get that done after dinner tonight?' They conclude with:

So there is a different thought process going on, a 'management' way of thinking

In sum there is a clear clash between the management way and the science way, I think that this is a conflict primarily because of the perception amongst the members that things done in a management way are assumed by the 'Other' to be a better way of getting results. However one member offered a story which demonstrates how they believe the science way is just as, or more valid. They did this anecdotally by referring to the results of some research that determined how profitable research was, this concluded with two things: The first was the relationship between the researchers and the marketers, and second was that the more basic research a company had done the better the return. In other words, you get bigger gains from solving the harder problems – thus supporting the science way over or alongside the management way.

The concern with the management way discussed above was that 'harder problems' wouldn't be attempted if the management way is prioritised, and this concern can be seen by considering two main facets of the reform and post-reform research funding system in New Zealand: Competition and contestability; both being key tenets of the management way. These two concepts featured heavily in the managing science mode of ordering the Centre. In particular the universities are performed as being the quintessential

embodiment of tertiary competition, the general voice in the Centre talks about the perils of this embodiment for the New Zealand science sector, and particularly the failure of a competitive model in the tertiary research sector. As this interview participant describes:

Ever since the science reforms of the early 90s, there's been a recognition that new problems have emerged because of the competitive model... Universities that once were very cooperative have been driven into competition by the policies of the 80s and 90s; this has prevented research groupings across universities and across institutions from developing

The theme expressed in the above statement is a common one in the narrative of managing science, as one interview group discussed, the 'Darwinian' model (as in the survival of the fittest researchers/research groups/universities) didn't pan out in New Zealand. Since the early 1990s each major university has been independently developing competing specialties – repeated many times over in different disciplines, resulting in a watering down of research skills and knowledge. The interview group perceived that the actual impact was that competition becomes focused on competition for attracting students, not necessarily competition for conducting the most excellent research. This, as one member described, 'EFTs grab', has bred serious competition between universities that has been translated into competition between research groups in all universities. The Centre model, they believe, changes that, so much so that:

If you go to [university] they think, 'here is someone from [other university], they think competition' if we go to [university] they think 'here is someone from the Centre, they think co-operation'

The Centre's perspective is that competition between institutions significantly reduces the level of cross-institutional collaboration, collaboration that is necessary for conducting truly excellent research of an international standard. A lack of collaboration between research institutions, it seems, is also considered a problem by the Government, the Tertiary Education Advisory Commission in their November 2001 report 'Shaping the Funding Framework' suggested that a CoRE model must include three things:

Significant collaboration and linkages between research providers (including TEPs and Crown Research Institutes) and industry, or other research users, an emphasis on leveraging (with public

funding related to the level of contributions from other sources), and a focus upon the nation's strategic goals (Tertiary Education Advisory Commission, 2001c, p. 104)

And more recently a cabinet paper (July 2006) from the office of the Minister for Education makes the following comment in relation to the importance of funding an additional two CoREs:

Further centres would provide enhanced opportunities for inter-institutional and intra-institutional linkages (consistent with the government's goals for collaborative networks in the research community) (Office of The Minister for Tertiary Education, 2006, p. 6)

Consequently competition, although a key strategy attached to the new public management reforms is now seen as a barrier to conducting excellent research of an international standard in the New Zealand science research sector.

Like competition, contestability is a component of new public management. Prior to the reforms, funding was allocated in bulk to institutional groups, now, much of this funding is provided through contestable means. Contest requires that standards be laid down against which applications are judged, in the science sector this is now the normal mechanism for allocating funding and it has been this way since the early 1990s (Leitch & Davenport, 2005). Although in some ways contestability is seen by members of the Centre as a hurdle to get over rather than a good mechanism by which to fund science, there is a seeming 'acceptance' that contestability is the right way – just not the practical way to fund science, take the following exchange between three people in an interview:

p1: There was a lot of discussion about whether we should split it up or make it contestable, I think we made the right decision

p2: We were in a contestable situation to start with, so we get the funds and then create another contestable situation... that's nuts!

p3: But the two tier contestable thing, I have some sympathy with that

p1: theoretically it would be good but not with the realities of personalities and politics and everything else, and that's science!

The Centre members are partly supportive of contestability, as they see the sense in having funding standards by which to assess potential projects but clearly there is

something else at play to do with the 'personalities and politics' of science; something else which prevents them having a second tier of contestability through which they distribute the Centre funds. There is a tension here between what is perceived as the 'right' way to distribute Government funding and what is perceived as the most socially acceptable method of collaboration between the Centre members; many of the people in the Centre I talked to commented on the fairness of the Centre's funds distribution method, to me it seemed as if the method was perceived by most of the members as not only fairer than contestable alternatives but importantly, more respectful of collegiality.

An aspect of the managing science mode performs the Centre as an entity that is highly distinct from the universities within which its members originate. As I discussed above to the members the Centre embodies everything that a traditional university is not – and this caused difficulty when negotiating agreements between the Centre and its university partners. This difficulty is attributed, by most members, to a mind-set difference between the traditional university model and the 'new' Centre model, as one member describes:

When we are negotiating agreements with [the universities], from [the universities] perspective it is difficult for them to recognize that we are cross-institution, they struggle to see that... It has been a mindset change for all of the institutions

Although frustrating the Centre members perform this difference in conceptualisation as something expected and try to bridge the conceptual gap by annually talking with the hierarchy of their partner institutions; the aim to break down the barrier by being very open with information:

What we as a Centre have done is to try and make this more of a partnership

Along with this recognition that they are different from the traditional structures there is a voice that talks of the Centre as being in 'danger' of becoming like a traditional research institution. Growth is performed as *both* enterprising *and* dangerous, particularly concerns exist over becoming larger and the geographical separation of the Centre resulting in a split and 'hurting' of the interdisciplinary nature of their science.

This concern is probably quite genuine and necessary. Up until this stage I have not talked a great deal about the geographical and spatial issues within the Centre. Primarily this is because the localities seem to work in relative symmetry and harmony, but also because I was limited to the extent I could ethnographically investigate the issue. I did however visit and talk to representatives from all of the institutions involved in the Centre. The Centre has annual three-day symposiums where all members come together in Palmerston North to discuss both administrative and scientific matters. Outside of these full Centre meetings the contact between localities seems primarily to be via email, phone conversations or specific project related travel. This contact apparatus appears to work well for the Centre, and allows the members to conduct their host-institution teaching and collegial responsibilities. The main two points of difference between the localities are related to discipline and institution affiliation.

The whole point for establishing the CoREs was to allocate funds to existing networks of *research excellence*, so it stands to reason that prior to its formation the actor-networks that comprise the current Centre and its five year history were already in existence, this certainly shows through in the disciplinary affiliation of the different localities. However because of the location cross-fertilisation of students and early career researchers these differences are probably lessening. Also the internationalisation of science and the Centre makes the geographical separations within New Zealand seem a little trivial.

The institutional affiliation with the members host institution is still quite strong, though the degree differs depending on locality and career 'closeness' to the Centre's directors. However there is also a significant voice describing the problems the members have with their host institutions. One member for instance talked of the concern the head of their institute had of their involvement because of the potential to create an environment that clearly separated researchers into 'class A' and 'class B' scientists. Along PBRF lines the concern was that the Centre would recruit and cultivate people with the potential to become 'class A' researchers and that those that 'remained' in the original institution would be isolated as 'second class citizens'. The feeling was that this would harbour resentment and be divisive.

6.2.3. *Managing Equipment*

A notable tension exists when managing equipment within in the managing science mode of ordering the Centre. The original CoRE brief called for a separation of operating and capital expenditure, to this end the Centre proposed to purchase equipment to the value of just over five million dollars during their initial formation; amongst capital purchases was a supercomputer ('Helix') and two DNA sequencers. These purchases were heralded in the media, this exert is from computerworld:

Developed by the Allan Wilson Centre for Molecular Ecology and Evolution at Albany, Helix is reputedly 200 times faster than a common desktop. When completed late last year, Helix was judged among the world's top 500 supercomputers, with a total of 67GB of RAM and 2.7TB of hard disk, making it twice as fast as its nearest New Zealand rival (Computerworld, 2003)

Although this supercomputer cost only about 5% of the Centre's initial capital expenditure there is a certain amount of angst over its under-use by Centre members. It is, and has been, fully utilised since its commission, but only about 7% of the time by Allan Wilson Centre scientists. They charge for its usage by other scientists, so it is self-funding; but there was a sense that had they the opportunity they would have chosen to spend the money on the Centre's operating expenses, however as this member noted during an interview:

We were in the position of having this money to spend on capital purchases or nothing at all, so we did

By contrast the DNA sequencing units are called a 'national resource' as they account for more than half of DNA sequencing conducted in NZ.

It is very easy to be critical in retrospect in a field that is rapidly changing; it could have been helix that was a huge success and the sequencers were not, but this is not the issue, the issue is the tenet of managerialism – capital expenditure vs. operating expenditure – and the senselessness of 'having' to spend money in line with budgetary requirements. This does not sit easily with the science way of the Centre discussed above.

6.3. *In Sum: Managing Science*

This chapter has discussed how the Centre members perform the management of their science. I think it is fair to say that their relationship with the discourses of new public management create a certain amount of anxiety. This member volunteered this statement in an interview for instance:

We get slammed that our Management is not up to scratch, I mean right from the very start that was something they said was a weakness

However I have struggled to find any official or unofficial statement in support of this view – I think perhaps it is an assumption that, because they are outstanding scientists, they will struggle as managers; certainly this appears to be what the discourse tells us we should think. This point emphasises one of the key concerns within managing science; the conflict that exists between managing the science and doing the science, and how the actors in the Centre act to prioritise one over the other.

This chapter has presented managing science, and concludes my results and discussion. The following final chapter presents my conclusions – where I aim to re-centre the Centre and discuss the implications of intersections between the modes.

7. Conclusion: Intersecting Modes

As I said in the opening chapters, my research problem is relatively simple: I am interested in whether the institutionalisation of the Allan Wilson Centre through the government mechanism of the Centres of Research Excellence fund has made a difference to the way they order science. The difficulty resided in how I could make sense of any changes and specifically *how* institutionalising the Allan Wilson Centre made a difference, if at all? In the previous three chapters I introduced and discussed three interrelated modes of ordering the Allan Wilson Centre; doing science, encouraging science and managing science. An understanding of these modes as ordering mechanisms has allowed me, in least in part, to identify what is ‘really’ going on – in essence to identify what has ‘really’ changed – if anything. This chapter summarises my conclusions about what has changed for the Allan Wilson Centre as a result of the CoRE funding.

Throughout the process of this thesis I have argued with myself about how to represent the ordering arrangements of the Centre, and have settled on doing, encouraging and managing science as three distinct but related modes around which to order the things I wanted to say; this arrangement is by necessity rather arbitrary, but in my view that does not limit its usefulness. In this conclusions chapter, in contrast to the previous three, I specifically aim to re-integrate my experience of the Centre; this is why I have called it ‘intersecting modes’, because I feel that this phrase most faithfully describes the Centre I experienced. Each of the main sections within this chapter discusses an area where I believe things have ‘really’ changed for the Centre members, and often I present these as modal intersections. Within each of these sections I also discuss the implications of my conclusions and specifically how they might be useful in two main arenas; firstly for government when they consider the development of science policy, the funding of science and the strategies and structures they put in place to implement policy, and; secondly for scientists and managers in science organisations, as they catch a glimpse of how another entity of scientific research orders its operations. The final section presents some of my research shortcomings and directions for further research.

7.1. Allan Wilson's Identity

My first conclusion revolves around the resources available to the Centre members to construct their professional identities. In the chapter on doing science I talked at length about how the Centre constructs its identity around the actor Allan Wilson, but, although I think it particularly resonates when they are doing science, Allan Wilson is a strong and prevalent actor in all of the registers operating in the Centre.

When doing science, Allan Wilson as an actor in the Centre's networks embodies the purity and excellence of the Centre's scientific pursuit – the aim for excellence, of reaching and exceeding international research goals, of attracting recognition, reward and funding. When encouraging science, Allan Wilson as an actor in the Centre's networks inspires the members to be the best theory teachers and laboratory trainers of New Zealand evolutionary students; to encourage the kind of fundamental molecular evolutionary science pioneered by Allan Wilson himself. Yet the identity and branding afforded by the title 'The Allan Wilson Centre' embodies the managing science mode of ordering – they perform a managed entity of science, with a governance board, budgets and organisational profile all united under the 'Allan Wilson' brand.

The implications of this conclusion may not seem particularly earth-shattering, but I think there is something to be learned from this experience. Here are my own autoethnographic thoughts on Allan Wilson and the implications of this conclusion:

Earlier this year when I first talked to my supervisors about this identity idea one of them commented that the Centre seemed to be metaphorically 'bringing Allan Wilson' back to New Zealand. Prior to this I had only really thought about the idea that they were trying to link in to Allan Wilson's international profile, and I still think that this is the case – but this other angle has implications for my conclusions. That is – it is important work, re-establishing Allan Wilson in the minds of New Zealanders, it is reaching society, and encouraging students.

In 2006 the Centre has supported the development of a documentary about the life and scientific impact of Allan Wilson. This will be shown for the first time during the international Evolution conference to be hosted In New Zealand by the Centre in 2007. In relation to this their website includes the following words: "We want to create a smart,

contemporary, enduring digital resource that younger generations of biologists can learn from and that will be linked to ongoing research in molecular evolution at laboratories around the world” (Allan Wilson Centre for Molecular Ecology and Evolution, n.d.c, para. 5). To me this signifies the importance of doing and encouraging science for the Centre and this is clearly a significant change from the pre-CoRE form, where a production of this kind would be unlikely to find the funding and support to go ahead. In this way the implications for those interested in the promotion and encouragement of excellent science are rather important: Carefully choose an identity with which to associate around, as the Allan Wilson Centre proves that it is a powerful unifying feature.

7.2. Valuing the Management of Science

This conclusion is about the relationship between the managing science mode of ordering the Centre and both the doing science and encouraging science modes. Although there are distinct clashes between these modes, overall I can see a growing valuing of the use of ‘management’ concepts to order the Centre. The first part, encouraging management, talks about the intersection between managing science and encouraging science, the second part, clashing cultures, talks about the issues inherent between managing science and doing science.

7.2.1. Encouragement Management

There is symbiosis between managing science and encouraging science, particularly in the sense that there is an accepted use of managerial techniques to encourage and promote the scientific exploits and achievements of the Centre members. They talk of branding and marketing and produce artefacts of identity, not to establish profile in order to generate profit, but to establish scientific and institutional profile. Although the aims are quite different from a business, the techniques are systematically utilised. This is performed as important for both; within managing science it allows promotion for funding allocations and power in negotiations; within encouraging science it is needed to encourage international visitors and to win the rights to host international conferences.

The main of implication of this conclusion is for the managers of the other existing CoREs and indeed science managers generally, in that here they can see another research operation fits normally within the standard discourses of marketing.

7.2.2. Clashing Cultures

Similarly to Law's ethnography in the Daresbury Laboratory I can see a distinct clashing between what is represented by these two modes of ordering the Centre: Managing science and doing science. This is not a new thing, many have written of the clash of cultures between the 'business world' and the 'science world' (Cohen, McAuley & Duberley, 2001; Doolin, 2003; Leitch & Davenport, 2005). It is interesting to see however that in the post-reform days in New Zealand that the tensions appear to be accepted, at least to some degree. It seems to me that the Allan Wilson Centre is, at least in some ways, a boundary object (Star & Griesemer, 1989); spanning the boundary between a traditional university research environment and a commercial government laboratory environment. The managerial mode of ordering performs the 'business' of the Centre, embodied in marketing plans, government performance reviews and operating agreements between parties. Yet simultaneously the science of the Centre is put on a pedestal, and is in effect, partitioned from the 'gritty business dealings'. As you might expect, given the purpose of the Centre, the primacy is placed on the science, in almost all activities the purity of the science endeavours are prioritised over the business aspects – Law did not experience this as strongly in the Daresbury Laboratory, rather the scientific impact of business decisions weighed heavily on the shoulders of the scientist and CEO Andrew Goldsmith (1994a). This stands to reason however given the focus and funding of the Allan Wilson Centre, though the tension between conducting excellent research and attaining revenue streams from private sources does not remain unnoticed, it is just that the chase for scientific excellence in the purity mode is so strongly supported by many influential actors in the network – including the Government funding agencies – that it is privileged over the managerial requirement to attain private funding. It is so strongly privileged that the Centre is confident in not actively looking for private external funding, particularly if doing so would prejudice the purity of the scientific pursuit.

Having said the above, the pervasion of managerialism into science purity is certainly not absent within the Centre. It can be seen, emerging through the embodiment of actors that represent the 'business way'. For example there is concern over the fact that the Helix supercomputer is underutilised by the Centre – a concern is alleviated somewhat by its application in a market economy. Although the Centre might only use about 7% of the Helix it is used commercially by others. Similarly the DNA sequencers are described proudly in many contexts as providing a fantastic service to New Zealand science, fantastic, in part, because the service is *faster* and *cheaper* than other services, in other words – it is more *efficient*. In this way, like the NHU in Davies study (2000), the impact of the changing discourses of history, and particularly the long arm of new public management, is felt in the actor-networks of the Allan Wilson Centre.

I think the implications of this cultural clash are important for both government policy makers and funding agencies and for science managers. Much of the influence of new public management ideology on the science sector has revolved around concepts such as 'return on investment' and 'self-funding' (Leitch & Davenport, 2005). For blue skies research this has always sat uneasily, and this is reinforced by my experience of the Allan Wilson Centre. The lesson for policy makers is that maybe new public management concepts are not the best way to measure the success of fundamental research. And for science managers – it is quite simple; the example from the Centre indicates that if you are excellent enough people will not ask you to demonstrate a financial return on investment. To me this indicates that privileging of science that still pervades our society, regardless of what 'business' ideology we import.

7.3. *Polarisation: To Do or To Encourage?*

This conclusion discusses an interesting juxtaposition that seems to exist within the actor-networks of the Centre. Mostly doing science and encouraging science seem to exist as sides to the same 'science purity' coin, in that they generally sit well together. However, this is not always so black and white. I use the term polarisation specifically in this title as it encapsulates the nature of the stress between the doing mode and the encouraging mode. This exists, I think, in two main spaces: The first is in a stress felt between factions

in the Centre – some believe that the networks might be just business as almost usual, others see it as synergistic. The second is a more fundamental juxtaposition, highlighting a growing gap between the growing excellence of the Centre’s scientific endeavours and the shrinking skill-set of New Zealand graduate students. Each of these is explored in more detail below.

7.3.1. Synergy vs. Business as ‘Almost’ Usual

The doing science mode of ordering the Centre talks in part of simple collaboration expansion; that is, the Centre’s formation could be explained by accelerated ‘business as ‘almost’ usual’ behaviour. Thus by building on the extant working practices of the primary investigators prior to the formation of the Centre one can extrapolate that the success of the Centre can be, at least partially, attributed to this factor. This is contrasted by encouraging science which talks of the exploits of the Centre in synergistic terms – that is the combined group is more than the some of its parts and that the successes simply could not have been achieved without the development and environment provided by the Centre.

My analysis is that the Centre is most definitely more than the sum of its parts, and the performance of this encouraging science concept seems to becoming more ‘normal’ as the Centre’s accolades build. From an implication perspective I think it is fair to say that the more institutional and disciplinary barriers that are removed between excellent scientists the better, and the CoRE fund does appear to change the nature of the landscape, at least for the members of the Allan Wilson Centre.

7.3.2. Quaternary Education

John Law in *Organizing Modernity* often hints at what he perceived as an interconnection between vision and vocation in the Daresbury laboratory, here he expressly makes the link: “Here’s a possibility: vision connects with scientific vocation; if you are already caught up in scientific vocation, then you are caught up in vision too” (1994a, p. 117). When I think about the Centre’s efforts to encourage science I think about Law’s vision and in particular the importance of charisma, of charismatic people and of charismatic science. As I have said before doing science in the Centre is all about science purity, and

vocational aspects of being a scientist, and in this I can see to what Law was referring when he linked vision and vocation, like the members of the Daresbury laboratory in 1990 in 2006 the Allan Wilson Centre members, when encouraging science, also embody doing science; they are caught up in both modes in a reinforcing simultaneous loop. This makes the conflicts that do exist between these modes the more confusing and frustrating for the Centre. Given this, in this section I want to discuss the most troubling conflict between encouraging science and doing science; fundamentally this conflict arises when the Centre tries to encourage science by encouraging *New Zealand* students.

The calibre of the science conducted when the Centre is doing science has been proven, this calibre, in my opinion, is excellent due to the quality *and* velocity of the science produced when they are doing science. They produce excellent science at a fast and growing rate, and have reached and superseded international standards quickly. Accompanying their publication record is a developing set of knowledge and skills that enable their production of science, and likewise this is growing at a substantial rate. These skill sets and knowledges are embodied in the actors operating in the Centre's networks; one of the main actors is the pool of graduate students and early career researchers available to produce the science. In New Zealand, in contrast to the rate of change in the Centre's research production, the 'market' for graduates is not developing to meet the needs of the Centre. New Zealand graduates rarely have the skills or knowledge to enable them to easily transit into the Centre, and consequently many graduates come from overseas. In this way New Zealand students may soon require some sort of bridging quaternary education before being able to join the Centre.

The implications for this conclusion, in my view, are profound and double-edged. On the negative side the gap between the knowledge and skills required of graduates entering the Centre and the knowledge and skills held by available New Zealand graduates is widening, resulting in less and less New Zealand graduate students being able to bridge it. This means that the system must change, to prevent a situation occurring where New Zealand students must do primary, secondary, tertiary *and quaternary* education before entering the international post-graduate community. On the positive side, clearly the

Allan Wilson Centre is a resounding success, certainly as judged by international science standards.

7.4. *Changing Discourses of Science*

This conclusion reflects on the changes in available discourses that have been experienced by the scientists and policy makers involved in the establishment and functioning of the Centre. Prior research has considered the changing discourses in the science sector (Leitch & Davenport, 2005; Simpson, 2004) and these changes can be seen playing out in the histories of the Centre, hence this section looks at the advent of the CoRE fund, and using an actor-network analysis considers how it has come to be.

The CoRE model is an inherited model, based on the Finnish and other international examples and utilising “conventional organizational thinking” (Calas & Smircich, 1999, p.664) this seems like a sensible thing, I mean – why re-invent the wheel, or even change it slightly, if we can utilise a working example with little need for ‘local’ change? But thinking in a conventional way is only one way of analysing the rise of the CoRE fund in New Zealand government policy; another way is using the sociology of actor-network theory. We often view something that is a process as a static entity because of what Latour entitles ‘immutable mobiles’ (1986). I think that the entity ‘The CoRE fund’ can be viewed in this way as an immutable mobile, as it seems to have taken on its own life as a device of government (Tatnall & Gliding, 1999) – I argue that it has been reified and given ‘black box’ status. The thing about these immutable mobiles is that become immutable (literally – ‘not able to change’) *and* mobile (literally – ‘able to move freely around’) so as Latour states you have created “objects which have the properties of being *mobile* but also *immutable*, *presentable*, *readable* and *combinable* with one another” (1986, p. 26, emphasis in original). So I think that the concept of the ‘CoRE’ landed in the policy arena of the New Zealand government as an immutable mobile, transported from other western governments, and was combined, through a process of translation, into our policy framework. If the immutable mobile is the result, the process of translation is the process of winning the struggle to create order within a network. A particular actor within a network acts to reinforce a particular position, and they perform

this position in all of their interactions. The more successful they are the more 'real' the institutional order becomes, this is the process of translation. Michel Callon tells of four stages of translation as the promoting actors employ various strategies to reinforce their position: *Problematization*; *interessment*; *enrolment*, and; *mobilization*.

The news media texts espouse the success of the CoRE fund, and so do the researchers in the Centre, and the Government press releases (as we would expect!) and so did (do) I. It has been successful hasn't it? Everyone says it has – so it must be, or is this simply a testament to the power of the 'CoRE' mode of ordering research management, as an immutable mobile – black boxed and reified into an unquestionable 'truth'.

If we follow the line above you can argue that the Government actor-network managed to successfully *problematise* the things that would support the establishment of a 'CoRE fund', such as issues with inter-institutional and cross-disciplinary collaboration in New Zealand universities and government laboratories, as many other western governments had already done. For instance the following statement describes the government's vision for the CoRE fund:

Establishes and promotes excellent, collaborative, strategically focused research; creates significant knowledge transfer; provides opportunities for the creation and diffusion of knowledge that are not available through existing funds; and encourages tertiary education institutions to develop relationships and linkages with other research organisations, enterprises and communities that they serve (RSNZ, 2006a, p. 1)

Then, through Callon's process of *interessment* the Government worked to block other potential competing modes of ordering that would promote or support collaboration between universities, government laboratories and disciplines. This can be seen in the selective advice taken from the Tertiary Education Advisory Commission (TEAC), who suggested that *two types* of CoRE's be funded, whereas only one was. Third, it does seem that the Government has managed to *enrol* a range of influential actor-networks to support their project (certainly the first round of CoRE's involved many influential New Zealand scientific actor-networks) and now also acts to continuously *mobilize* supporting actors (like student researchers who are researching the 'success' of the programme) to reinforce the mode.

When I was thinking about and writing this conclusion I was constantly concerned with the seeming political manoeuvring of the authorities that imported the CoRE scheme. The saving factor that allowed me to not recoil from the ramifications of my analysis is the fact that the Government in this story is in fact also only an actor-network in itself bowing under the strain of other international forces of *mobilization* – New Zealand did, after all, follow the Finnish CoRE model – because of its apparent success.

I think the implications of this conclusion resides in the fact that it is important for us to consider how ‘imported’ government policy comes to rest in New Zealand, often with little local adaptation. It isn’t that this (the ‘CoRE fund’) is not necessarily a beneficial thing for New Zealand, but rather it is about questioning the motivating factors. However following the empirical realism of actor-network theory (Lee & Hassard, 1999) I am comforted, because the Allan Wilson Centre has achieved a range of very impressive feats, ranging from a substantial increase in publications, to launching internationally successful student alumni, to international conference hosting and many others – it incorporates, to be as sure as I can, an impressive bunch of scientists!

7.5. *The Power of Excellence*

This conclusion discusses the power inherent within the pursuit of excellence operating within the actor-networks of the Centre. In the ‘doing science’ chapter I provided some quantitative evidence of the excellence of the Centre’s science, as measured by ISI journal impact ratings. This is interesting in itself, but what is more interesting is *how* they have achieved a change from an average of just over 5 to almost 13.5 in five years. In that chapter I suggested that doing excellent science consisted of doing visionary science and doing interdisciplinary science, and that there is a ‘lack of constraint’ link between these two mechanisms for the pursuit of excellence. This answers part of the how puzzle, but in order to get the full picture and understand the implications of the power of excellence we need to link with the encouraging science mode and consider the concept of scientific stewardship.

In encouraging science I introduced the notion of the Centre as a broker of excellent science. This was explicitly around the funding model they had democratically chosen to

distribute the available CoRE funds. They chose an even distribution system rather than a contestable fund as other CoREs have done – I think this is because they view the contestable schema as constraining, in that a scientist in applying for contestable funds must ‘fit’ into certain disciplinary, capacity and potentially institutional moulds. Certainly this is how other funds are performed, even ‘good’ ones like the Marsden fund.

To me this conclusion has significant implications for policy makers and science managers alike. Firstly, for policy makers, it might call into question the rationale behind the application of contestable funds; that rationale being that contestability processes result in choosing excellent awardees. The lesson to be learned from the Centre’s experience is that backing excellence can be done by approaching excellent people and allowing them to broker good science. Secondly for science managers, the discourses of new public management will generally suggest following a second or third tier contestable model, the Centre by contrast did not choose this path – and yet they still demonstrate profound publication success; thus proving that there are other ways to order science.

Thus ends my conclusions. The above five sections cover a broad range of issues of relevance to government science policy and funding, the nature and society of science, the Centre’s of Research Excellence fund and specifically the Allan Wilson Centre for Molecular Ecology and Evolution. My conclusions are based on a fairly limited exploration of the networks of the Centre and how these might ‘fit’ within the world in which they operate, and to reiterate I only claim a modest sociology – others would interpret the same results in other ways. I do however claim to have faithfully represented the issues as I see them, I hope they prove useful. To finish I want to talk briefly about my general research shortcomings and the directions for future research following this framework.

7.6. Research Shortcomings & Directions for Future Research

This section combines a short look at what I perceive are my main research shortcomings in tandem with a presentation of some directions for future research. With regard to research shortcomings I am only aiming to point out a couple of things that I believe I

have overlooked during the production of this thesis. I am specifically not going to talk about most of the issues with actor-network theory as this was covered, where necessary, in chapter three; I will however talk about one, by following aspects of Ben Fine's criticisms of ANT studies of capitalism (2005). However, before following this line, I want to talk about location and identity.

It is fair to say that I have overlooked the locality issues inherent within geographically separated institutions, Davies talks about this at some length as she examines the networks of the BBC's Natural History Unit (1999; 2000) and I am sure that some Centre members will read my analysis and conclusions and struggle to identify with some points. However I will only partially apologise for any oversight as I have always held that this is only my account of the Centre, viewed through the lens of my subjectivity – hence my claim of employing only a modest sociology (Law, 1994a). Having said this I felt hints of location issues as the members attempted to run an entity that is geographically dispersed, taking this line I would like to pursue this issue in the future, perhaps by doing more investigation on the specific spatial and geographic separations, taking into account Davenport and Daellenbach's (2006) ongoing investigations of the MacDiarmid institute (another CoRE) and research generally into organisations that struggle with geographic separation.

In a similar way to the above I believe that I have only just scraped the surface of some of the identity issues emerging in my study. Of particular interest to me is the way the scientists in the Centre construct their identities when undertaking interdisciplinary and inter-institutional projects; this might form the basis for further research.

Fine's criticisms of actor-network theory, mentioned above, revolve mainly around Callon's analysis of economics using an actor-network approach. I sympathise with certain aspects of Fine's criticism as it rings true with some of the concerns I experienced whilst researching the Allan Wilson Centre. Fine is highly critical of Callon's approach, and the concern that rang true with me was the issue with heterogeneity, key to ANT studies, and particularly the ANT assertion that it is pointless abstracting something and assigning it agency out of a context. Fine argues, conversely, that homogeneity does

emerge within certain circumstances, such as money in political economy, and that therefore there does exist “a case for developing an abstract understanding of money in general prior to addressing how it adopts different forms, functions and meanings according to context and practice” (2005, p. 105). Although I have not explored this in any depth I think that a similar argument could be made for the homogeneity and therefore characterisation, outside of an institutional setting, of ‘fundamental research’ in science institutions; as it appears to hold true across time, location and philosophy. I do not think that this issue is in any way of concern to my study, my theoretical and analytical project remains intact – but it does pave the way for further studies of the nature of scientific research. Actor-network theory remains a useful approach for studying the nature and society of institutions such as the Allan Wilson Centre for Molecular Ecology and Evolution.

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