ADVENTURES IN THE NATURE OF TRADE: THE QUEST FOR 'RELEVANCE' AND 'EXCELLENCE' IN CANADIAN SCIENCE

by

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ABSTRACT

The study addresses: (1) changes in Canada's science-policy climate over the past two decades; (2) impacts of such changes on the conduct and organization of academic science; and (3) public-interest implications of promoting, in public institutions, research 'relevant' to private sector needs. Working within the interdisciplinary traditions of science studies, the conceptual framework draws on the cross-cutting tensions at the intersection of public and private space, and basic and applied science. These tensions are articulated in two opposing models: 'open science' and 'overflowing networks'.

Canada's Networks of Centres of Excellence (NCE) program provides the study's empirical focus. Founded in 1988, the NCE program rests on dual goals of research *excellence* and commercial *relevance.* It promotes a national research capacity that 'floats across' existing provincial institutions. The first part of the study investigates the evolution of the NCE program against the background of Canadian science policy. The second part problematizes the notion of 'network' while investigating one of the NCEs in depth, examining the scientific, commercial, cultural, and spatial-structural *practices* that are the outcomes of policy. Examination of these practices reveals not only the cultural and commercial shifts sought by policy, but also unintended consequences such as regional clustering; élitism and exclusion; problems with social and fiscal accountability; tensions with host institutions; and goal displacement between *science* and *commerce.*

In relation to the overall problematic, the study constructs a new typology depicting network scientists as 'settlers', 'translators', or 'merchant scientists' according to their public/private, basic/applied orientation. The study then develops a set of broad conclusions about NCEs, especially those in the life sciences. (1) *Translational research*—at the nexus of public/private, basic/applied—is foundational for these networks. (2) As policy/practice hybrids, their spatial dynamics are highly enigmatic. (3) NCEs develop contradictory cultural norms. (4) Network effects resist standard assessments. (5) 'Public' and 'profit' seem to be problematic partners. (6) The recent historical focus of science policy has been myopic. The study expresses concerns for the public interest when commercial 'relevance' becomes an overarching goal of both science and policy. It concludes with a recommendation for open networks that would retain the flexibility of the network form, but would produce open rather than proprietary knowledge .

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Finally, embarking on the adventures of graduate studies at a relatively advanced age is less foolhardy when it is a *folie a deux*. Thanks to my partner in this madness: my husband and friend, Garnet Grosjean. We made it, kid!

DEDICATION

In gratitude for providing a climate of encouragement, and the intellectual and material resources that allow students to work outside disciplinary lines, I dedicate this dissertation to the Individual Interdisciplinary Studies Graduate Program (Rhodri Windsor-Liscombe) and Green College (Richard Ericson) at the University of British Columbia, and the Graduate Liberal Studies Program at Simon Fraser University (Hannah Gay, Len Berggren, and Steve Duguid.)

ACRONYMS AND ABBREVIATIONS USED

ACST	Prime Minister's Advisory Council on Science & Technology
ANT	Actor-Network Theory (aka Translation Sociology)
'big pharma'	Multinational pharmaceutical industry
CBDN	Canadian Bacterial Diseases Network
CGDN	Canadian Genetic Diseases Network
CHEO	Children's Hospital of Eastern Ontario
CIAR	Canadian Institute of Advanced Research
CIHR	Canadian Institutes of Health Research
CSA	Canadian Standards Association
HUGO	International Human Genome Organization
ILO	University-Industry Liaison Office (aka Technology Transfer Office; Commercialization Office
IP	Intellectual Property
IPR	Intellectual Property Rights
IRAP	Industrial Research Assistance Program (see NRC)
ISTC	Industry, Science, And Technology Canada ((now Industry Canada)
MOSST	Ministry of State for Science & Technology
MRC	Medical Research Council
NABST	National Advisory Board on Science & Technology
NCE	Networks of Centres of Excellence
NRC	National Research Council
NSERC	Natural Sciences & Engineering Research Council
NSF	National Science Foundation (US)
ORF	Ontario Research Foundation
OST	Observatoire des sciences et des technologies
PENCE	Protein Engineering Network of Centres of Excellence
PI	Principal Investigator
PRECARN	Pre-Competitive Advanced Research Network
PRI	Policy Research Institute
PUS	Public Understanding of Science
R&D	Research and development
'Sick Kids'	University of Toronto Hospital for Sick Children
SME	Small and Mid-Sized Enterprises
SPRU	Science Policy Research Unit (UK)
SSHRC	Social Sciences and Humanities Research Council
UBC	University of British Columbia
UQAM	Universite Quebec a Montreal
UT	University of Toronto

NB In quotes from interviews or documents, capitals in parentheses are my locator codes, not acronyms.

CHAPTER 1: INTRODUCTION

The creation of the Networks of Centres of Excellence (NCE) program, in 1988, was arguably the most dramatic change in Canadian science policy since the National Research Council was established in 1916. The NCE program can be understood as an attempt to create an interpenetrating system of public and private research within academic settings. The federal government sought to establish a university-based system of national research networks—'research institutes without walls'—that would target and develop commercial opportunities. The program is now a central element in the government's 'innovation agenda', where scientific excellence, commercial relevance, and public/private collaborations are recurrent themes. By the end of the 2001 fiscal year, a total of 29 networks had been funded in areas deemed strategically important to Canada's prosperity and international competitiveness (see Appendix A).

What makes the NCE effort an exemplar in Canadian policy history is the explicit attempt to turn the culture of academic science towards commercial application, and to manage research on private-sector rather than academic principles. Purposive tensions are 'designed in' to these networks in the form of commitments to *both* fundamental enquiry *and* exploitation of intellectual property; private-sector investment *and* public funding; academic ideals *and* commercial values. NCEs are institutionally ambiguous in that they occupy indeterminate public/private spaces, inside/outside the academy. As

such, they 'float above' universities—which fund a significant portion of their costs—with little accountability. This abundance of novelties would seem attractive to anyone interested in the shifting terrain of science, economics, and policy. Yet surprisingly, the program has largely escaped scholarly notice.[1]

My interest in NCEs began with an outsider's curiosity about the workings of academic science and the way it appears to be changing. In earlier graduate work,[2] I'd examined *science* as a master narrative of *modernity*, presenting 'the scientific worldview' as a metaphor for Enlightenment values of rationality, predictability, and order. In a world characterized by quite opposite values, I'd searched for a different metaphor: a 'postmodern science' that would more accurately reflect today's fragmentation and loss of certainty.

Revisiting that work, I find little in the way of critical reflection or recognition that *science* itself might require some unpacking. Despite much talk of the embedding of science in society, and the socially constructed nature of knowledge, my approach was deeply conservative. *Science* was treated as an institutional black box, governed by Mertonian ideals. The 'booming, buzzing, confusion' of actual scientific practices was nowhere to be found, and the structural and organizational contingencies that constrain and shape these practices were completely absent.

Moving on to interdisciplinary doctoral work in science studies and the political economy of science, I studied the way market forces and neoliberal public-sector reforms were affecting research funding and science policies. The conversion of public science into private (intellectual) property, and academic and state institutions into market players, was progressing rapidly and with relatively little

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[1] Clark (1998) is one exception, providing a comparative but atheoretical overview of various 'formal knowledge networks.' As well, as ongoing research program includes interest in certain NCEs, for example, Dalpé & Ippersiel (2000), Dalpé, et al. (2001).

[2] Atkinson-Grosjean (1996) 'Science in Postmodern Times', unpublished MA terminal project. SImon Fraser University

resistance. I found this curious because Canada's structure and values, and the heterogeneity of its federal and provincial political institutions, generally preclude radical change.

I could find few, if any, evidence-based studies of the phenomenon and no disinterested calculations of the social and financial costs and benefits involved. It seemed that the policy of 'privatizing' public science and its institutions was proceeding ideologically, rather than by rational calculation. Such policies were *assumed* to fuel innovation and maximize wealth creation, but that was a highly contestable assumption. Many economists were pointing to the relative inefficiency of proprietary approaches to public science.[3] Meanwhile, other critics[4] questioned a calculus that collapsed the social into the economic, and turned universities into 'knowledge factories'.

It was clear that these policies could fundamentally realign the public/private divide with potentially far-reaching consequences. The shift from 'public' to 'private' in Canadian university science was accelerating rapidly; intellectual property rights were becoming the hegemonic currency of the research economy. Gross revenues from royalties and license fees grew more than threefold between 1991 and 1997, while industrial research funding saw more than a fourfold increase (AUTM 1998).[5] Of the almost 400 spin-off companies created in Canadian universities since 1980, more than 62% had been formed since 1990, at an average rate of 23 per year (Statistics Canada 1999).

The 'free flow of ideas' into the public knowledge base tends to falter when *public* research becomes *privatized.* A review of various literatures indicated that researchers become reluctant to share

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[3] For example, Nelson and Romer, 1998:59; Nelson 1996; Mazzoleni & Nelson 1998; Rosenberg 1998, and see Chapter 2, following

[4] See the reference list for works by David Noble, Sheila Slaughter and colleagues, Janice Newson, and Claire Polster.

[5] AUTM is the US-based Association of University Technology Managers. Since the majority of Canada's major research universities participate in the AUTM survey, these are fairly reliable indicators of growth. Conversely, the majority of Canadian universities *do not* participate in the AUTM survey, suggesting that commercialization concentrates in the major institutions, as in the US. This is confirmed by the Statistics Canada survey (1999:17), which shows that the 12 most active universities account for 75% of invention reports and licenses, and two thirds of new patent applications. Of the remaining universities, medium-sized institutions account for the majority of activity. The number of universities that can *effectively* pursue commercialization activities and academy-industry partnerships thus appears limited.

information with colleagues; sponsored research contracts sprout clauses that restrict dissemination; public and private 'partners' squabble about the ownership of intellectual property; and universities develop policies governing disclosure of research with commercial potential. Such practices are rationalized on economic grounds: if science is to be harnessed in pursuit of competitive advantage, subscribing to free-flowing knowledge is deemed hazardous.

On the basis of the literature reviews and statistical evidence, I developed a hypothesis that some kind of radical break from past practices was underway. Academic science was turning away from disinterested enquiry and open sharing towards commercial interests and 'secret knowledge'. Academic forms of organization were being replaced by new and dynamic cross-sectoral networks. The hypothesis drew on the tension between research pursued for understanding and research pursued for use and on associated attitudes towards ownership and access, secrecy and openness. The argument was positioned within the shifting and historically contingent distinctions dividing 'public' from 'private' and 'basic' from 'applied'. My larger purpose was to question the impact of shifts in the organization and *ethos* of science on 'the public interest'. To whom is a privatized science accountable? I asked. What is gained and what is lost when longstanding institutional distinctions dissolve? These questions constituted the 'moral purpose' of the project.

A pilot study revealed flaws in the way the hypothesis had been formulated. To avoid the errors of my earlier work, I had adopted an empirical approach that would open up the black boxes marked *science* and *public interest.* Interviews quickly demonstrated that I was, nevertheless, focusing almost exclusively on structural forces. In the first place, the way my thesis was framed left no room for agency, yet the autonomy that individual scientists exercised over their work came through clearly at an early stage, as did the *choices* some had made to engage in 'academic capitalism'.[6] In the second

[6] The term was coined by Ed Hackett in a 1990 article and developed by Sheila Slaughter and Larry Leslie (1997) in their book 'Academic Capitalism: Politics, Policies, and the Entrepreneurial University'

place, it seemed to matter which *type* of science I was addressing. While network forms of organization were becoming a default requirement for funding, commercial interests were largely absent in whole areas of the natural sciences. It soon became apparent, therefore, that I should focus on changes in the *biomedical* sciences rather than, say, physics or chemistry.

Next, an examination of the historical record quickly dispelled notions of 'radical' or 'revolutionary' breaks. Before 'networks' there were 'invisible colleges', and the relations between science and commerce seemed anchored in a long evolutionary process. Comparing the end of the 19th century and the start of the 21st, for example, I perceived differences of degree rather than kind in academy-industry ties. Finally, as expected, I found many examples of federal steering of the research agenda, but few indications of *direct* interference by 'big business'. Thus the empirical realities of the data disciplined my opening assumptions, allowing a more 'grounded' approach to emerge. Adjusting for these new insights, the core assumptions seemed sound and the study could proceed.

Where this study fits, in theory

The study participates in the interdisciplinary tradition of enquiry known as science studies. Science studies is a broad church embracing many sects, including the three that inform this project: micro-studies of laboratory and organizational practices; the economics of science; and science policy studies.

Because the study incorporates a case study of the work that individual and institutional actors do to construct, extend, stabilize, and maintain complex networks and power relations, it is most at home in the 'Paris School' of science studies, where such networks have long been a topic of enquiry. Michel Callon, Bruno Latour, John Law, and others have worked to develop Actor-Network Theory (ANT), or 'the sociology of translation', for the past 30 years. But despite the powerful descriptive vocabulary it has accumulated, ANT carries little *explanatory* weight, as many, including the principals, have argued. A workshop at Keele University in 1997, and a subsequent book (Law & Hassard 1999), focused actor-network theorists on what 'comes after' ANT. Although this study's primary purpose is empirical, rather than theoretical, I hope it will in some way contribute to that debate.

One of ANT's weaknesses is 'explanation by incorporation'. Nick Lee & Steve Brown (1994) complained of ANT's 'colonial' expansion. Because *everything* is enrolled into the network, nothing remains outside. One of the results is that surrounding institutional structures are (under)explained, or explained away, as network *outcomes*. I find this unsatisfactory. Like Daniel Lee Kleinman (1991 & 1998), I believe that actor-networks are constrained and shaped in important ways by the institutional structures that provide their context. I see these structures as important already-existing features *external to the network*, rather than as the contingent outcomes that ANT depicts. Accounting

for the transition from 'micro-structure' to 'macro-structure' is an ongoing challenge in ANT and this study participates in that challenge. A related problem is that ANT adopts a deliberately agnostic stance towards the broader political, economic, and social implications of what it describes. In a theory that erases all boundaries between science and society, such agnosticism seems to me a contradiction, since it separates science from its consequences. I think an agnostic stance is a luxury ANT can no longer afford.

Thus, ANT needs to be 'stiffened' with several critical starches and this study may indicate just where those stiffeners can be most effectively applied. First, I point empirically to the myriad ways biomedical networks are bounded and closed by members, who thereby invent 'insides' and 'outsides'. Second, through the empirical evidence, I can challenge ANT with normative questions about the nature of public and private science and how the public interest can be served. Third, I develop a typology that classifies network scientists by their response to political-economic pressures. This new typology operationalizes the intersection of public/private, basic/applied divides in networks, and reinterprets ANT's notion of 'translation'.

Finally, in pursuit of this effort to give ANT an afterlife, I follow Michel Callon into the current controversy in economics of science and science policy, where 'open science' takes on the network model. By weighing the arguments against my empirical results, I hope not only to contribute something to that debate, but also to contribute to policy studies and inform future policy. In that regard, the study's *primary* contribution is empirical: collecting and systematizing data on the Networks of Centres of Excellence (NCE) program and the Canadian Genetic Diseases Network in the context of Canadian science policy.

Details of the Study

A review of the current science policy environment suggested that, for the reasons indicated earlier, the NCE program would reward attention. In fact, the research reported here constitutes the first full-length, academic analysis of this program. To extend the study beyond the structural level, I would conduct a detailed review of one of the networks, using ethnographic techniques. (Time and cost constraints precluded more than one in-depth case study.) A number of criteria were developed to guide selection, including research sector; position on the public/private continuum; longevity; density of linkages; amount of funding; and location. The best match with my selection criteria was the Canadian Genetic Diseases Network (CGDN).

CGDN was one of the first networks funded under the NCE program. Inaugurated in August 1990, it brought together medical genetics researchers across the country, under the leadership of Scientific Director Michael Hayden. By 2001, support received from the program totalled $50 M. Currently, some 50+ researchers belong to the network, together with 11 universities and hospitals and eight companies. The research program covers four integrated themes: gene identification; pathogenesis and functional genomics; genetic therapies; and genetics and health care. 'Core facilities' in major centres undertake work such as DNA sequencing, genotyping, and bioinformatics training.

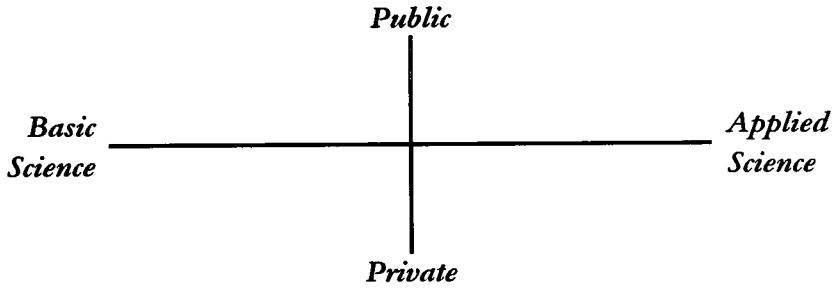
The network opened a 'child' organization—the Centre for Molecular Medicine and Therapeutics—in Vancouver in 1998. Merck Frosst, a founding industry partner, contributed $15M towards the Centre. The network's commercial prowess can be seen in the major intellectual property (IP) agreement it brokered between Schering Canada Inc. and the University of Toronto; at the time, the largest university IP agreement in Canadian history. The agreement was based on the 1995 discovery, by a network researcher, of two genes for early-onset Alzheimer's disease. Schering's

initial $9M funded a three-year research program in the development of drugs and technologies to treat and prevent Alzheimer's. Over the long-term, the agreement has a potential value of $34.5M, not including royalties.

In 1997, when the NCE program announced a 14-year 'cap' for all networks, CGDN learned its funding would 'sunset' in 2005. This policy change set in motion a major strategic shift. In 1998, the network incorporated itself as CGDN Inc. It adopted a corporate organizational form and an aggressively commercial focus. The goal was to maximize revenues from license agreements and equity holdings in order to replace the $4.5 M a year in federal funds that would cease in 2005. Many of the scientists interviewed expressed ambivalence at the direction in which the network was moving. On one hand, they knew action was needed if the network was to survive. On the other hand, they regretted the attendant loss of collegiality and openness that had marked earlier days.

If one imagines public/private and basic/applied as cross-cutting dimensions (see Figure 1 below), CGDN had, until this point, concerned itself predominantly with 'public science' and 'basic research'. Approximately 70% of NCE core funding supported fundamental, discovery-based research, with 20% going to early-stage development of technologies with commercial potential (the remaining 10% supports networking and administration). But the goal of sustaining the network beyond 2005 accelerated a downward shift to the 'private science' half of the matrix. A key question is how this policy shift affects the public's social and economic return on investment in CGDN and NCEs more generally.

Figure 1: Conceptual Matrix



The tension between the public and private faces of NCEs became increasingly apparent over the course of the study. Countervailing currents of confidentiality and openness ebbed and flowed around the project. Scientists spoke to me freely, for example, while gatekeepers erected formal blocks to access. The contradictions give an indication of the normative and ethical boundaries that are constantly negotiated in these networks.

At the federal level, the vast majority of people who designed and implemented the program in the mid-1980s had disciplinary roots in the sciences. Most held PhDs and were associated with the Research Councils. In interviews, their commitment to a scientific culture of openness prevailed. In contrast, 'career bureaucrats' remained guarded, refusing to provide key materials on the grounds of commercial and/or cabinet and/or third party confidentiality. Access was formally denied. The fact that the research was sponsored by one of the three federal funding councils (SSHRC) carried no weight.

The wording of the formal denial sidestepped an outright claim that NCE files were exempt from disclosure. But access would require implementation of the provisions of the Access to Information and Privacy (AIP) Acts and every individual document would have to be requested by name. Not only was this quite impossible without prior access to the files, the delays and costs would have been

unmanageable. The nature of the problem is demonstrated in the following extracts from correspondence (e-mail, January 21, 2000).

> Because of the sensitivity of the files in this case, we really have no option but to 'do it by the book'. This means that in order to gain access to documents in NCE files, we will have to ask you to submit formal Access to Information Act requests...Many documents within [NCE files] would have to be reviewed on a line-by-line basis to identify information subject to exemption. And in many instances a decision about the operation of a particular exemption could only be made in consultation with other federal institutions, with networks, and with any other parties affected by the disclosure.

Canada's information commissioner has criticized precisely this type of strategic use of the Access to Information Act by public servants. He speaks of 'the stubborn persistence of a culture of secrecy in Ottawa' (Reid 2000b) and complains of too-frequent recourse to claims of third-party 'commercial sensitivity' to avoid the release of documents (Reid 2001). When public information disappears behind a screen of privacy erected by public servants, questions are bound to be raised about accountability and the abuse of power.[7]

At the network level, the contrast between the exaggerated discretion of professional staff and the openness of network scientists was marked. And the balance of power between scientific officers and professional staff appeared to be highly delicate, given the goal of commercial sustainability by 2005. CGDN's scientific director, Michael Hayden, belongs to both cultures. His instincts were to be open but his position required him to attend to the concerns of staff.

Hayden was the first person I interviewed in the pilot stage. He was an enthusiastic participant and his support for the study never wavered. As my design of the study developed, Hayden assured me all involved would cooperate fully. But despite his endorsement, professional staff at the network's

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[7] This is not an isolated case. My experience confirms that of another doctoral researcher who attempted to explore a similar topic in Ottawa during the early 1990s. Claire Polster was seeking financial and statistical information on the proliferation of federal support programs for industry-relevant university science; some of the data required for her study were denied to her. Other data were not tracked, and what *was* tracked often proved inconsistent and unreliable (Polster 1993)

administrative centre, where I'd hoped to be based, initially refused access. Commercial sensitivity was the formal reason given: 'many of our interactions with industry involve the element of confidentiality and an "outsider" may impact those discussions negatively' (e-mail, March 31 1999).

Hayden suggested timing might be the problem; professional staff were simply 'too busy right now' but they *would* co-operate when the workload moderated. To accommodate the delay, I reordered the study and undertook the federal phase next, returning to the network several months later. At that time, Michael Hayden arranged for my participation in the annual scientific meeting and the International Human Genome conference. He also asked professional staff to arrange an 'internship' for me in the various facets of network governance and to facilitate my access to network documents.

Again, staff were slow to comply. A further ten weeks of refusals, negotiations, delays, reversals, and interventions were required before a compromise was reached and limited access granted. There would be no internship and access to materials was curtailed. Board and committee minutes and commercial files were denied to me. I was not allowed to photocopy or remove any of the materials provided, nor could I attend board, committee, or staff meetings. Only information that staff considered 'in the public domain, i.e. financial and annual reports...funding proposals and interim reports' (e-mail, May 17 2000) would be provided. By the time I began my fieldwork at the network's offices, 15 months had elapsed since my initial request for access.

Despite repeated requests, I was never allowed to consult board and committee minutes. Eventually I asked for a written rationale. In the response, elaborate framing sequesters aspects of this public entity as private. 'Management has received a legal opinion recommending against public disclosure of Board Minutes. The CGDN Board is a legal entity and as such, holds the right to maintain

confidentiality of its in-camera meetings. Management does not hold the right to disclose those proceedings' (e-mail, June 2000).

I was able to compensate for the lack of access to records by probing quite deeply in my interviews with private-sector and researcher board members. I also found evidence of board and committee discussions and decisions by triangulating against the materials prepared by the network for NCE site visits. In this way, I was able to form an adequate understanding of the key decisions over the years.

Data Collection & Analysis

The majority of data for the study was derived from in-depth interviews and participant-observation, supplemented by analysis of documents and financial and statistical reports. The preliminary phase of the study lasted from Fall 1998 through Spring 1999. I collected and analyzed documents, then interviewed CGDN officers in Vancouver and network researchers in Vancouver, Edmonton, and Calgary. At the same time, involvement in a separate study[8] of industry liaison offices (ILOs) in four universities (two in BC and two in Alberta) allowed me to solicit information on network commercialization practices and network-university interface issues from the 15 ILO officials I interviewed.

The next phase, extending from Fall 1999 through Winter 1999, focused on the federal level and the officials responsible for the NCE program. During a week-long fieldwork visit to Ottawa, a total of 19 individuals[9] involved in the program's initiation, development, and ongoing maintenance were identified and interviewed. Historical details of policy formation and program-building were sought,

[8] 'Academy-Industry Relations in North America,' Dr Donald Fisher, principal investigator, 1998-2001. Funded by SSHRC.

as well as the rationale behind certain 'design features', such as the twin criteria of scientific excellence and commercial relevance. At the same time, documents and reports spanning the NCE's history and pre-history were collected from the program directorate. These materials included annual reports; program evaluations; public relations materials; newsletters; and various committee reports. Particular attention was paid to the acquisition of program-wide information on partnership and intellectual property arrangements, company creation, and funding patterns.

The final phase of data collection encompassed the CGDN case study, which extended from Winter 1999 through Fall 2000, with follow-up visits to June 2001. In March 2000, I attended the annual scientific meeting in Vancouver, one of the network's key cultural events. The purpose was to present a paper introducing my study; conduct and solicit interviews; observe interactions; ask questions; and generally familiarize myself with the science and business of the network. Directly after, I represented CGDN as a volunteer media relations officer at the International Human Genome Project's annual conference, which the network was co-hosting. These meetings were invaluable introductions to network culture and science, and the vast 'industry' that molecular biology has become. In addition, over the course of the study, I made site visits to three research labs in Toronto and several to the Centre for Molecular Medicine and Therapeutics, in Vancouver, for interviews and observation. But the core of my fieldwork centred on the network's cramped administrative headquarters in the 'NCE Building', at the University of British Columbia. Here, for a period of eight weeks, I observed the workings of the network from a makeshift desk in the hallway.

Over the course of the study, I interviewed a selection[10] of board members and private sector partners and all current and former professional staff. In selecting which of the 50+ network researchers to interview, I focused on the 'founder population', i.e. the 16 scientists who remained

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[9] In the interim, the scope of the aforementioned SSHRC study had been extended to include NCEs, so this phase of my data collection process overlapped with that of the larger study. Data from 15 of the 19 interviews were shared.

active in the network, of the 21 who had signed the original 1988 proposal. Eleven of the 16 were interviewed. For balance, I also contacted two of the five founders who had left the network and three more-recent recruits, two from the start of Phase II (1994-5); another from the start of Phase III (1998-9). In total, the CGDN phase of data collection incorporated 40 formal interviews with 31 people.

Interviews were semi-structured, allowing scope for reflection and opinion. Informants were first asked to describe their recollections of the network-building process, then answered a series of questions about the science produced in the network; culture and relationships; commercialization practices; governance; and whether they had noted any problems or 'sticking points' over the years. The relative weight of these questions was adjusted to reflect the informant's role in the network. The majority of interviews were conducted in Toronto, Ottawa, and Vancouver—three of the network's four main nodes. I was unable to visit Montréal, the fourth major centre, but interviewed two researchers from McGill, one by telephone and another during his visit to Vancouver.

Throughout the study, I attempted to compensate for the 'single-case' focus by identifying and interviewing other knowledgeable individuals with interests in the NCE program. These included 'insiders' involved in networks other than CGDN, and 'outsiders' such as university technology managers and policy consultants. The purpose was to generate a cross-section of fact, opinion, and experience about NCEs from which shared patterns could emerge—patterns that would not be discernible in a single case.[11]

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[10] Access was controlled by staff; I was not permitted to contact board members and industry partners independently

[11] The technique, originally developed by Glaser and Strauss, is called 'maximum variation'. See Merriam (1998: 62) for a brief and useful description

In all, a total of 74 formal interviews[12] were conducted with 65 people in nine Canadian and two US cities (Figure 2 below). CGDN professional staff were interviewed twice, at the beginning and mid-point of the study, to check changing conditions and perceptions. Michael Hayden was interviewed three times: a wide-ranging discussion at the beginning of the study helped define my general focus; another at the mid-point dealt with the human genome program and the network's involvement in genomics; a third during fieldwork covered specific questions that had arisen and shifts I had noted. CGDN's current NCE program officer was interviewed twice; once, in Ottawa, in October 1999, and again in Vancouver during the annual scientific meeting in March 2000.

[12] Of these, 30 were shared with the previously mentioned SSHRC study.

Figure 2: Formal Interviews Conducted

	People	Interviews
Senior policy makers	4	4
NCE Directorate	7	7
NCE Program Officers	6	7
CGDN 'founder' researchers		
--Current	11	14
--Former	2	2
CGDN 'new' researchers	3	3
CGDN Professional staff	4	9
CGDN Private Sector	5	5
Non-NCE scientific networks	3	3
University administrators	2	2
University technology managers	15	15
Policy Consultants	3	3
Total People/Interviews	65	74

Initial analysis of the data began during fieldwork. Daily write-up of field notes helped me to reflect on what I was discovering and identify questions for subsequent follow up. After fieldwork, during the intensive analysis of the data, I continued with the practice of daily written reflection. These notes reminded me of where my thinking had been in relation to the study and suggested directions I might explore. They proved invaluable in helping me structure the eventual write-up.

The materials I had collected included financial and statistical reports. My background as a professional accountant allowed me to analyze financial and performance data using generally accepted accounting principles and conventions. Key ratios were calculated in an attempt to determine the program's economic costs and benefits, and comparative rates of public/private participation and reward. Such calculations are unable to account for social dimensions of the research questions, since social costs and benefits resist quantification. Nevertheless, these indicators can *suggest* the underlying social calculus and it is in this spirit they were sought.

The policy and program material was analysed and written up first. Several conference papers and articles were produced from these historical and interview data.[13] This process had the effect of 'stabilizing' a large part of the evidence. The 'macro' level of the program's composition and policy context could then be set aside in favour of a much finer-grained analysis of the network's micro-level practices. The material lent itself naturally to this bifurcation, leading me to question theoretical claims that actor-networks could not be bound within structural frames.

Next, network interviews were sorted into four broad categories: 'network scientists', 'professional staff', 'board and private sector', and 'other'. Provisional code-books were developed from iterative readings of the transcripts in each category, which were then coded and recoded using software tools of my own devising (rather than a commercial qualitative analysis program). Once I was satisfied the codings were consistent, each category was sorted by main code and sub-codes. Then all categories were combined in a single database and sorted. A numerical weight was assigned to the codes according to frequency across categories. The dominant codes became headings to which less-frequent codes were assigned on the basis of 'family resemblances'. These then provided a framework to guide the structure of the dissertation. In turn, these dominant codes were collapsed into broad interpretive themes, to aid theory-building.

Chapter Outline

In this chapter (1) I have provided a broad overview of motivation and methods. Chapter 2, extends the discussion of public/private, basic/applied, and the public interest. I focus on the fundamental tension between 'open science' and proprietary knowledge and set up the two conceptual models which guide the study. In the second part of the Chapter 2, I discuss some of the analytical tools

[13] For example, Atkinson-Grosjean, J. 1999c, 1999d, 1999e, 2000a, 2000b, Atkinson-Grosjean, et al. 2001, Fisher, et al. 2001, Atkinson-Grosjean 2002

that will be brought to bear, from actor-network theory and science studies more generally. Chapter 3 addresses the historical and structural factors contributing to the development of the NCE program.

The CGDN case study begins in Chapter 4 where I describe the network-building activities of this group of medical geneticists and the institutional identity they constructed. Chapter 5 demonstrates the way the network evolved a culture and sense of community, critically examines the rhetorical construction of the network's research program, and points to the authentic locations of 'network science'. These two chapters represent the 'public' face of the network; the following two chapters move to the 'private' side of network identity.

Chapter 6 describes the trajectory from 'public' to 'private' and 'basic' to 'applied' in terms of the network's development of intellectual property and construction of a commercial portfolio. Chapter 7 develops a typology of network researchers based on their alignment along the public/private, basic/applied dimensions. The last chapter summarizes the study and its findings, derives a number of conclusions and policy implications, and makes recommendations for future research.

In summary, what follows is an enquiry into the material and epistemic spaces of the NCE program in general, and the Canadian Genetic Diseases Network (CGDN) and its scientists in particular. Detailed descriptions of the social, cultural, and material mechanisms at work draw authenticity from the voices of federal public servants, network officers, private-sector partners, university administrators, and scientists themselves. I trace the trajectory of the NCE program and CGDN over time, attending to the ways federal policies are translated into specific research projects, practices, and institutional arrangements and recording how scientists embrace, resist, or ignore these initiatives. The purpose is to achieve a greater understanding of changes in the organization and motivation of academic science as well as the way these changes affect the public's manifold

interests in the science it funds. Close examination of how *Science* is planned and produced and the *Public Interest* is served in this one particular case will contribute to the development of science studies and policy research more generally.

Chapter 2: Conceptual And Analytical Tools

The conceptual framework of this study relies on the relationship between two sociological and epistemological distinctions: the public/private and basic/applied divides, and this chapter commences with a review of their relationship. The space where these dimensions intersect is particularly relevant to this study and I examine various attempts to describe it. Donald Stokes (1997), for example, calls the space 'Pasteur's Quadrant'. Others speak of 'strategic research', 'emergent science', or 'Jeffersonian science'.[14] I will introduce two models that present opposing interpretations of the relation between the divides: the *open science* model and the *overflow* or *network* model. The tension between these contrasting approaches to public and proprietary knowledge runs throughout this dissertation. In the last part of the chapter, I introduce the analytical tools I will use to understand the conduct and culture of science in networks.

[14] See, respectively, Godin (2000-3), Callon (in press), and Holton & Sonnert (1999)

I. Mapping the Divides

Public and Private

The public/private demarcation is one of the core sociological distinctions. Norberto Bobbio (1989) calls it one of the 'grand dichotomies' of western thought. Yet like other such dichotomies this one begins to collapse on closer examination, becoming not one but a number of related oppositions that nest one within the other like Russian dolls (Starr 1988). Is the stock market, for example, public or private? From one perspective it is a mass of individuals pursuing private interests; but from another, it is a public social and cultural aggregation. What do we mean by 'the private sector'? Usually, we mean private businesses, large and small. Yet many of the largest corporations are 'public companies', owned by millions of shareholders, some individual, some huge and institutional. But huge institutional investors are themselves often 'public', in that they represent the pensions and investments of millions of people.

What do we mean by 'the public sector'? Many publicly owned institutions and agencies are 'private' in the sense that they are exempt from direct, or even delegated, public control; for example, crown corporations; universities; even departments and bureaucracies of the state. What does it mean to speak of 'public' and 'private' life? For individuals in 'public life' we designate whole areas exempt from public scrutiny (private matters of conscience, conviction, family, and morality). But when these aspects of private life impinge on or attract the public interest, they enter the public domain and become 'public knowledge'.

Does my body belong to me? If so, I should be able to control what happens to my genetic material. But legal cases have been fought and won by researchers who have taken cell lines from unsuspecting patients and patented them for profit, rendering bodies 'public' by acts of

privatization. On discovering their colonization, patients fought not for the right to privacy but for the right to profit for and from themselves.[15]

What about ownership of the human genome? In the vast undertaking to map it, public researchers raced against a private company (Celera Genomics) which sought to patent and profit from 'the stuff of life'. Because results of the public effort were held in common in the public domain, Celera was able to use them to advance their own project. The controversy raised awareness of the role of patent law in privatizing public research. Patents make knowledge private by circumscribing ideas with property rights, so if a public university takes out a patent on a publicly funded discovery, is it 'privatizing' that knowledge? Or is it securing the ownership of that discovery for the public domain?

These questions without answers help to illustrate that public/private is a negotiated, discursive space rather than a fact of the world. But two core ideas help connect the many different meanings. These are, as Paul Starr, states, 'that public is to private as open is to closed, and that public is to private as the whole is to the part' (1988:2). In the first sense, public and private oppose each other along the dimension of accessibility. That is to say, the openness and transparency of public space, public life, and public disclosure contrast to the opaqueness and concealment of private space, private life, and personal communications. In the second sense, 'public' is synonymous with 'common', as in public opinion, public health, or the public interest; this sense has merged with the sense of 'official' or 'state'.

Thus, to Starr, 'public' can carry three contrasting meanings from which 'privatization' represents corresponding withdrawals. In the first sense, 'public' means open and visible, as in public life and

[15] The classic case is Moore vs Regents of the University of California, see Boyle (1996). John Moore sued researchers and their university for stealing his cell line (uniquely resistant to hairy-cell leukemia) for profit and without his consent. He lost the case.

social relations while 'private' means a withdrawal from sociability and the decline of public culture. In the second sense, we invoke the 'general public' or the public-at-large, to speak of public action and civic concerns in contrast to private concerns and the pursuit of self-interest. The third sense of 'public' is the domain of common (state or community) ownership, as opposed to appropriation by an individual or group. These senses of open, closed, and common will reappear throughout this study.

The *locus classicus* of the public/private distinction can be found in Greek and Roman thought. It represented the separation of the private household and its economy *(oikos)* from the sphere of collective public institutions--the *polis* or *res publica.* Collectively, heads of households constituted the 'body politic' or public realm (Arendt 1959:56). As Arendt explains, a physical space, a boundary or no-man's land, separated private households. The boundary demarcated one property from another, and marked off the household from the city. Arendt identifies the spatial significance of this boundary with that of the law. In the same way that the law harbored and protected the public domain that was political life, fences sheltered and protected the private property of households (Arendt 1959:57). Between the political (public) and intimate (private) domains, Arendt interposed a third space: that of the social.

By feudal times, public/private distinctions in property and affairs had developed a certain taxonomic and ideological slipperiness. The emerging concept of the corporation under Roman and Canon Law is a case in point. A corporation interpolates between the individual and the collective, the political and the economic; in a sense, it is both and neither public and private. After the church invoked the corporate form to sever itself from state control, the principle of incorporation spread into secular law, where it established the rudiments of a public sphere free of ecclesiastical control (Huff 1997). Thus, 'we find in the 12th and 13th centuries the widespread emergence of a vast array of legally autonomous [corporate] entities that were bestowed with a composite bundle of legal

rights and which presumed the legal authority of jurisdiction, that is, legitimate legal authority over a limited territory or domain' (Huff 1997:28). These newly incorporated (literally, embodied) entities included cities and towns, merchant guilds, charitable organizations, professional associations and universities (ibid.).

Subsequently, according to Huff, corporations contributed to the rise of the public sphere by facilitating the extension of trade in the high middle ages. The original trading companies were extensions of the private economy of the family, in that assets and investments entrusted to the company were commingled with family assets. The developing legal theory of the corporation made it possible to disentangle familial and business affairs, installing a distinction that converted what was previously private (*oikos*) into public (the market). Huff argues that corporate law made it possible to differentiate between individuals and the corporate body. The corporate collectivity was construed as a single, legal person. A distinction now existed between ownership and jurisdiction, especially concerning assets, liabilities, and debts. By providing for allegiance to the corporation rather than to individuals, the continuity of the enterprise was ensured. The historical development of these concepts , according to Huff, provided for the emergence of distinctive public and private spheres of action and interest. This separation laid the foundation for the emergence of modern science as a 'public' institution within 'public' universities by establishing a 'neutral space' of thought and action. As Huff explains,

> The medieval intellectual élite of Europe established an impersonal intellectual agenda whose ultimate purpose was to describe and explain the world in its entirety in terms of causal processes and mechanisms. This disinterested agenda was no longer a private, personal, or idiosyncratic preoccupation, but a publicly shared set of texts, questions, commentaries, and in some cases, centuries old expositions of unsolved physical and metaphysical questions that set the highest standards of intellectual enquiry...A disinterested agenda of naturalistic enquiry had been institutionalized... It thereby laid the foundation for the breakthrough to modern science (Huff 1997:33).

The science that emerged from the Renaissance took place in relatively small, interdependent communities of practice where scientific advance rested on the veracity of individuals. It depended on a culture of *honour,* epitomized by the position within the social order of the 17th century English gentleman-scientist (Shapin 1994). The production of scientific knowledge was, and remains, according to Shapin, a moral enterprise built on mutual trust. Personal trust is the 'great civility' and the currency of an 'economy of credibility' in the conduct of science.

> Within such small interdependent groups as the 'core-sets' of specialized scientific practices, the economy of credibility is likely to flow along channels of familiarity. The practitioners involved are likely to know each other very well and to need each others' findings in order to produce their own. Here...the pragmatic as well as the moral consequences of distrust and skepticism are likely to be high (Shapin 1995a:269)

Thus trust in the public institution of science rests on trust in the private morality of its individual practitioners. The 'public' nature of scientific knowledge rests on the collective construction of a collective good, under conditions requiring reliance on the work of others. Within this 'moral economy of truth' public and private, scientific and social, become inseparable.

Similarly, Habermas (1989) conceived the public sphere as a social space, first emerging in 17th century English coffee-houses and salons, where 'private' individuals came together to engage in rational-critical debate and thereby further the 'public' interest. Habermas distinguishes this 'authentic' public sphere from the 'public' realm of state interests. The authentic public sphere is a dimension of private life: 'a public of private people' who came together to further the 'common good'. In Habermasian terms, however, the common good and the public sphere itself are undifferentiated. As in classical Greece, where women and slaves were confined to the home, rational-critical discourse in the public domain was a white, male, bourgeois prerogative as was the

'scientific revolution' itself. The legacies remain, as will be seen in the empirical section of this study.[16]

Basic and Applied

With the onset of modern science in the 17th century, questions of public and private begin to map onto distinctions between basic science, applied science, and what lies between. These distinctions are part of an ancient argument that has its roots in the classical differentiation between *theoria* and *praxis* in early Greek thought (Godin 2000-3:3; Arendt 1959). The path of *theoria* travels from Plato, through Descartes and Newton. The path of *praxis* from Aristotle, through Montaigne and Bacon.

Toulmin (1990) shows how the rationalism of early modern science came to dominate the experiential and empiricist values of Renaissance humanism. For 16th century humanists, the central demand was that thought and conduct should be *reasonable* (rather than rational) tolerating social, cultural, and intellectual diversity. But after the Enlightenment, says Toulmin, ideas became decontextualized. Scientists began to conduct 'pure research'—a careful and systematic search for the abstract universal laws through which God governed nature (see also Latour, 1993 for a parallel discussion). A fundamental part of Francis Bacon's critique of institutionalized scholarship in the 16th and early 17th centuries was its ignorance of the concerns of industry and commerce, the crafts and trades. Consequently, an important part of his call for reformation involved bringing the two together so that in the reformed academy 'the sounds of industry' would be heard 'at every hand'.[17]

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[16] For an interesting discussion on historiographical approaches to the relation of public sphere and private life, see Dena Goodman's (1992). A definitive critique of the inadequacy of the liberal model of the public sphere descibed by Habermas is available in Fraser 1997

[17] Thanks to Stephen Straker for this point

According to Benoit Godin,[18] the word 'research', meaning thorough examination, emerged from French origins in the 16th century. The concept of 'pure research' was first used in the mid-17th century, to distinguish abstract theorizing from 'mixed research' dealing with concrete subjects (Kline 1995:196). It came into general use towards the end of the 19th century, as part of a contrast pair, the opposing element being industrial or 'applied' research. Thomas Henry Huxley (1880) had an aversion to the pure/applied distinction, stating

> I often wish this phrase 'applied science' had never been invented. For it suggests that there is a sort of scientific knowledge of direct practical use, which can be studied apart from another sort of scientific knowledge, which is of no practical utility, and which is termed 'pure science'. But there is no more complete fallacy than this. What people call applied science is nothing but the application of pure science to a particular class of problems (quoted in Kline 1995: 194)

Huxley was making a nice distinction, ignoring the fact that 'technology', particularly in industry, had its own distinct history and trajectory. Others recognized the linkages between 'pure' and 'applied' research, or disputed the proper place of each. As early as 1840 Prussian chemist, Justus Leibig, sought to establish a university program that would combine the search for pure knowledge with production training for students; he was strenuously opposed by faculty (Turner 1982). Lenoir (1998) describes a number of late 19th century German initiatives to link the demands of the pharmaceutical industry with the interests of academic science, first through consulting and contracting arrangements, then the establishment of independent institutes. Noble (1977) traces the connections between US academic engineers and industrial research problems from the early decades of the twentieth century. Veblen was complaining about too-close relations between universities and local industries as long ago as 1918. Well-documented debates[19] from the interwar years address the propriety of aligning academic and industrial research and patenting publicly-funded research. Conflicts of interest and commitment were not uncommon; there were disputes

[18] In this section, I draw quite extensively on Godin's series of working papers (2000, 2001, and ongoing) for the Observatoire des sciences et des les technologies, UQAM. His project constitutes a history of attempts to measure the impacts of scientific research.

about intellectual property ownership and concerns about on the proper role of the university. As we grapple with similar concerns today, the continuities argue against claims that a radical break in moral and organizational culture is in progress.

The terms 'pure' and 'applied' dominated the discourse until the 1930s, when 'fundamental' research came into occasional to avoid the moral connotations of 'pure' (Kline 1995:196). Subject-matter, e.g. theoretical or applied physics, defined what was pure or applied rather than the motivation of the researcher, as is the case today. The phrase 'basic science' was first coined by Julian Huxley (1934) (grandson and intellectual heir of T.H. Huxley) as part of a typology in which 'pure' and 'applied' each contained two categories: 'background' and 'basic' for the first; 'ad hoc' and 'development' for the second.

British socialists like Huxley, and his colleague John Desmond Bernal were inspired by the apparent success of 'planned' Soviet science. In The Social Function of Science (1939), Bernal advocated state steering of science through socioeconomic controls and goals. In contrast to this image of social engagement, Michael Polanyi and others who opposed 'Bernalism', founded the Society for Freedom in Science to defend the ideal of a 'pure science', unfettered by social constraints (Polanyi 1940; Sheehan 1993). According to Polanyi (1962:62) 'you can kill or mutilate the advance of science [but] you cannot shape it'; any practical benefits are incidental and unpredictable. The dialogue between Bernal and Polanyi on social direction and autonomy in science is the origin of our continuing debates about the relative allocation of resources to basic and applied research (David 1995).

The same debates were being engaged in the interwar period in the US, and the 'Polanyi' position dominated. At the time, academic science was controlled by 'a tacit oligarchy of eminent scientists

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[19] See, for example, in Weiner 1986 and 1989; Geiger 1988 & 1990; Noble 1977.

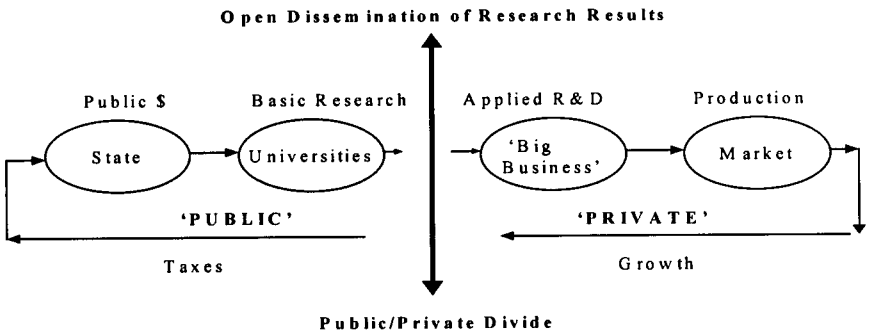
who shared a number of ideological convictions' (Geiger 1990: 19). Among these convictions, according to Geiger, were the beliefs that: (1) society should support basic science, because society benefited from its discoveries; (2) funding should be reserved for the 'best' scientists, because their productivity was established; (3) who the best scientists might be was a matter for the best scientists themselves to determine, and (4) government funding carried the taint of politics, so private support was preferable.

Robert K. Merton captured the Polanyi *zeitgeist* in The Normative Structure of Science (1942). Merton defined pure science by its characteristic methods and institutional structure, and also by the distinctive cultural values and mores that bound the behaviour of scientists. In combination, these clearly demarcated 'science' from 'technology'. The Bernal position on socioeconomic relevance was adopted by Harley Kilgore, a New Deal senator from West Virginia. Kilgore wanted publicly supported science to be politically and socially accountable. He suggested that the sole criterion for public funding should be 'manifest social utility' in the production of knowledge (David 1995). Vannevar Bush, an engineer and former president of MIT, who headed the wartime Office of Scientific Research and Development (OSRD), took the Polanyi and Merton side of the debate.

Bush (1945) politicized Merton and Polanyi's vision of a freestanding science governed by a system of binding universal norms that underpinned the moral authority on which it rested. He adopted Julian Huxley's term 'basic research' to describe what this autonomous university-based collective produced, and articulated a 'linear model of innovation' to link basic research to eventual socio-economic returns. The 'pipeline' is the dominant metaphor of the linear model. Fundamental discoveries are fed into one end of the pipe and move through various stages of development until they emerge onto the market at the far end of the pipe. The resultant growth fuels the economy and returns taxes, to maintain the cycle (see Figure 3 below). The linear model was a powerful argument for 'market failure' in that *basic science* was viewed as a public good, requiring public funding and the

open dissemination of research results. It was argued that government investment in basic research must be preserved, and science left to regulate itself, if the pipeline was to fuel the innovation process and produce wealth. These arguments were the foundation of the postwar 'social contract for science',[20] a contract secured by a promissory note on the eventual but completely unpredictable technological and social spin-offs of basic science.

Figure 3: The Linear Model of Research: WWII to mid 1970s



Basic research was 'performed without thought of practical ends' and with the sole purpose of contributing to 'the understanding of nature and its laws'. According to Bush, if basic research is contaminated by premature considerations of use it loses its creative edge. But if left alone, it provides the raw materials for innovation and becomes, at a distance, 'the pacemaker of technological progress' (1945:19). Thus, in the form of technology transfer, basic science generates social and economic returns on the state's investment—but only if scientists are allowed to pursue it, wherever it leads, without government controls. Government's role was simply to *support* university researchers with the resources they needed to produce knowledge.

[20] See David Guston's extensive work in science policy and the social contract, for example Guston (2000a); Guston and Kenniston (1994)

Scientists viewed Bush's 'Endless Frontier' as 'a charter for pure science' (Holton and Sonnert 1999:53). It enshrined the basic/applied dichotomy in US science policy, and entrenched the 'ideology of the autonomous researcher' (Godin 2000-1:9). Bush argued that 'the responsibility for the creation of scientific knowledge - and for most of its application - rests on that small body of men and women who understand the fundamental laws of nature and are skilled in the techniques of scientific research' (Bush 1945:7). Only peers could decide the value and merit of research. Consequently, 'there was no need for governments to worry about the evaluation and measurement of science and scientists, and to track the output of research' (Godin 2000-1:9).

Politicians and policymakers initially refused Bush's gambit. The National Science Foundation, for example, was not established until 1950, with far more restricted levels of authority and autonomy than Bush had anticipated. But in the late 1950s, in the aftermath of Sputnik, the linear explanation of the relation between basic science and application became compelling. Bush had argued that without significant investment at the source of the knowledge pipeline, no innovations would issue from the mouth, and the nation would fall behind its competitors. Sputnik seemed to demonstrate the truth of this claim. Fears of Soviet dominance of 'the space race' generated immediate revisions in the US federal research budget. The 'golden age' of state-sponsored research had arrived.

The Spaces in Between

Setting up a dichotomy between basic and applied dissolves deep connections between the search for solutions to practical and technical problems and the search for fundamental understanding. As Donald Stokes (1997; 1995) argues, and as the historical record suggests, basic research has never been divorced from application, and distinctions between research directed to useful ends, and research directed to the advancement of knowledge, are deeply misguided. Stokes suggests that a large proportion of university research is—and always has been—*both* useful *and* fundamental. He

suggests that the basic/applied dichotomy renders this significant segment of the research spectrum invisible, and that the linear model's one-way flow obscures the number of basic research questions arising from purely technological phenomena.

In furthering his claims, Stokes (1997) employs an illuminating typology. He classifies fundamental 'understanding-based' research as 'Bohr's Quadrant', and applied 'use-inspired' research as 'Edison's Quadrant'. Research that is *both* useful *and* fundamental resides in between, in 'Pasteur's Quadrant'.[21] Pasteur's research commitment, according to Stokes, was twofold: not only to understand the microbiological processes he discovered, but also to exert practical control over their effects in products, people, and animals (1997:71-2). 'The mature Pasteur never did a study that was not applied while he laid out a whole fresh branch of science [microbiology]'. (1995:5). In Stokes's view, it is this dual commitment to understanding and use that characterizes much of university research. 'Every one of the basic scientific disciplines has its modern form, in part, as the result of use-inspired basic research. We should no longer allow the post-war vision [of Bush] to conceal the importance of this fact' (Stokes 1995:6).

In further contrast to Bush's one-dimensional linear model, Stokes (1995:7-8) sees the rise in fundamental scientific understanding and the rise in technological know-how as two loosely coupled systems. Instead of the latter being dependent on the former, each progresses along largely independent trajectories, with no intervention from the other. But at times, Stokes argues, the mutual influences are profound and can go in either direction, with use-inspired basic research often cast in the linking role. At that point they conjoin in a 'seamless web'. While it is a commonplace that new technologies will be increasingly science-based, the under-appreciated concomitant, argues Stokes, is that 'more and more science will be technology-based' (1995:8).

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[21] A similar formulation, found in Holton and Sonnert, 1999, adopts 'Newtonian Science', 'Baconian Science' and 'Jeffersonian Science' as the ideal types. The latter emphasizes the role of state patronage in promoting scientific advance.

What goes unsaid but is nevertheless clear from the discussion is the relation of 'understanding' and 'use' to 'public' and 'private'. If Bohr is the former and Edison the latter, Pasteur occupies the shifting space *between* public and private. Clearly, as will be seen throughout this study, today's biomedical sciences epitomize these 'spaces in between'. In my empirical findings, physician-scientists describe much of what they do as *translational research,* a concept that fits the intermediate space between bench and bedside, laboratory and market. A second concept, transitional research feeds the findings of *translation* back into basic questions, as Stokes predicts. Policy instruments, such as the NCE program, that are geared to both scientific excellence and commercial relevance, address research in Pasteur's Quadrant. The implications of Stokes's insight are being explored by others.[22] The model is reproduced below.

Figure 4: Stokes's Quadrant Model of Scientific Research

Research inspired by:		*Considerations of Use?*	
		No	Yes
Quest for Fundamental Understanding?	Yes	Pure Basic Research (Bohr)	Use-inspired Basic Research (Pasteur)
	No	Research directed to particular phenomena *(Wissenschaft)*	Pure Applied Research (Edison)

Source: Stokes (1997)

In the following section, I summarize two opposing theoretical perspectives towards policies on university research, both of which can lay claim to the space between basic and applied, public and private. The 'open science' model, grounded in evolutionary economics, argues that commercial exploitation of proprietary knowledge by public universities undermines the pursuit of use-inspired basic research. The 'overflow' or 'network' model grounded in science studies, argues that the genie

[22] Stokes had a long and distinguished career in US science policy. He died of leukemia shortly after *Pasteur's Quadrant* went to press. Work has continued in Branscomb, et al. 1999; Nelson 1996; Nelson and Romer 1998; Holton and Sonnert 1999; Branscomb, Holton and Sonnert 2000; Sonnert and Brooks 2000).

is already out of the bottle, that institutional distinctions are largely irrelevant anyway, and that the resulting state of affairs (inter-sectoral fluxes, flows, and circulations) is largely beneficial.

II. 'Open Science' or 'Science that Overflows'?

In 1954 Jonas Salk, of the University of Pittsburgh, announced he had developed a vaccine for polio. In a television interview, he was asked why he had not taken out a patent on an invention clearly worth millions. Salk replied, 'How can you patent the sun?' (Zalewski 1997:51). Salk's point—that no one should own or profit from discoveries about the natural world—has been overtaken by events. Patents are now used routinely to translate university research into proprietary knowledge, as part of a systematic effort to turn universities towards the market by 'capitalizing' their own research (Etzkowitz, et al. 1998). This is where basic/applied and public/private dimensions overlap.

University intervention in the commercialization process is highly contested on both social and economic grounds. The first questions the social costs of commodifying universities and their knowledge, holding that these (public) institutions should remain outside the (private) system of market exchange.[23]. One argument is that while the costs of advancing basic knowledge are socialized—taxpayer supported—the benefits from its application are privatized, in the form of intellectual property rights (Noble 1997). Some make an ethical argument that when research is publicly funded, neither researchers nor their universities have moral rights to proprietary control over resulting products (for example see Goldman 1989). These are powerful debates and I only touch on them here. Note however, that the position of social critics is aligned, rather curiously, with the second line of contestation which advances the economic interests of industry.

This 'open science' model problematizes the new commercial role of universities and university researchers as *impediments* to industry and therefore to innovation and wealth-creation The focus on intellectual property rights creates tensions by redefining the role of universities. Once relatively open suppliers of ideas to industry, they become more closed and costly sources of information (Rappert and Webster 1997).

The Open Science Model

Articulated by Dasgupta & David (1994) as 'the new economics of science', the open science perspective advocates a return to 'no-strings attached' public funding of basic science; a recommitment to the open publication of results, and removal of expectations that universities should be involved in commercialization. Essentially, this model seeks to 'turn back the clock' to the linear understandings of the post-war Golden Age discussed above, when universities produced 'public' knowledge, industry exploited it, and an arm's-length relationship kept the two sectors at a healthy distance (see Figure 3).

Using Starr's formulation, discussed earlier, 'public' knowledge produced in universities is common property. In the classic formulations of Richard Nelson (1959) and Kenneth Arrow (1962), 'public goods' are the result of market failure in that knowledge is considered to be 'non-appropriable' and 'non-rival'. As summarized by Keith Pavitt (2000), 'the simple economics of basic scientific research' are such that basic research generates information that is costly to produce, but virtually costless to reproduce and re-use. It therefore has the properties of a public good and deserves public support. If business firms try to capture all the benefits of basic research for themselves, either through trade

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[23] For Canadian thinking on this issue, see for example Buchbinder 1993; Polster 1998; Newson 1998. For the US, see Sheila Slaughter and colleagues at the University of Arizona, for example Slaughter and Leslie 1997; Slaughter 1998; Slaughter and Rhoades 1990. Simon Marginson (1997) is a good source for Australia.

secrecy or property rights, knowledge remains under-explored or under-exploited. Thus state support for basic research can be justified on the grounds of economic efficiency (Nelson 1959).

Yet as Nelson (1998) and Nelson & Sampat (2001) have recently shown, *universities* are now patenting and licensing a 'non-trivial fraction' of what would previously have been placed in the public domain. When a university owns patents and licenses, transaction costs for industrial development are increased because companies must now pay for techniques and materials that were previously freely available. Industry's costs also increase when university researchers spin-off patented discoveries into their own companies, then license subsequent products to larger firms.[24]

Thus, again referring back to Starr, transaction costs reduce accessibility. Industry prefers, therefore, to maintain university research in the public domain. Nelson (1998:2) remarks that 'the large pharmaceutical companies, in particular, have begun to complain vociferously that since they and the public pay for this research through taxes given to the university, it is not fair for them to pay again for access'. As well, patents are said to restrict the diffusion of knowledge that promotes innovation. Traditional methods of knowledge diffusion from universities to industry—journal articles, meetings, conferences, and so on—are held to be more efficient (Cohen, et al. 1996). Since barriers to access decrease overall wealth, arguably it is more efficient for government to subsidize the production of fundamental knowledge and give it away 'for free' (Nelson and Romer 1998:59).

Florida & Cohen (1999:590) argue that although the role of the university in the knowledge economy is 'not yet clearly articulated, identified, or understood', inherent tensions beset their dual pursuit of *both* commercial alliances *and* the traditional 'quest for eminence'. A more balanced view of the university's new role in the economy is required, they say. Instead of positioning universities

[24] For an extended discussion on the economic costs and benefits of patents see Mazzoleni & Nelson (1998)

as engines of economic growth, a more nuanced perspective would reframe the university as 'an enabling infrastructure for technological and economic development'.[25]

In this vein, a recent empirical study of university patenting in the UK (Rappert and Webster 1997)[26] concludes that the construction of a 'regime of appropriation' in the academy, while effective in the short term, may in the medium to long term constrain the overall rate of return. The authors argue that university patenting and intellectual property rights can unintentionally compromise the commercial potential of research, and that in securing patents the university positions itself as a potential competitor to private-sector firms. Further, university patents may present an obstacle to future development if the patent coverage has been poorly framed or filed prematurely.

Starr's dimension of open/closed appears in disclosure restrictions associated with the securing of intellectual property rights; these may prevent research results from entering the public domain in a timely fashion. University commercialization activities can be perceived as impeding the cumulative advance of the research enterprise by increasing wasteful duplication of effort, and reducing the likelihood that current findings will contribute to future work (Nelson and Romer 1998). Disclosure restrictions are by far the most significant economic cost associated with university patenting and licensing (Cohen, et al. 1998; Blumenthal, et al. 1996). Restrictions in licenses are pervasive. A recent US study (Blumenthal 1997) found that 82% of companies surveyed require academic researchers to keep information confidential to allow for the filing of a patent application, while some 47% have agreements with universities that allow for even longer delays. Additionally, 30% reported that conflicts of interest had arisen with universities, and 34% had experienced intellectual property disputes with academic researchers. The study confirmed that participation by researchers in commercialization is associated with both delays in publication and refusal to share research results

[25] As will be seen later, CGDN has recently redefined its mission in precisely these terms.

[26] see also Packer & Webster 1996, 1995; Webster & Packer 1996a, 1996b, 1995

on request. Industry-supported and market-oriented academic researchers were more than three times as likely to delay publication as those who had no industry support.

Similarly, in a survey of technology managers and faculty at the 'top 100' R&D-performing universities in the US (Rahm 1994), 39% of managers had experienced situations where firms placed restrictions on the sharing of information between faculty. Also, 79% of managers and 53% of faculty reported that firms had asked for R&D results to be delayed or kept from publication. In addition to restricting the flow of knowledge, disclosure limitations also generate real and potential conflicts of interest that can damage public perception of the research enterprise.

Another issue receiving attention in the literature is the so-called 'patent-scope' problem. This refers to the practice of taking 'broad patents' on basic biomedical systems technologies, such as recombinant DNA or monoclonal antibodies. Especially problematic are rights claimed to 'whatever useful may come' from the patenting of DNA fragments. Critics (Nelson 1996; Nelson and Romer 1998) argue that the use of broad patents to commercialize 'public' scientific research, and the policies promoting that commercialization are unsupportable. Especially in the biomedical sciences, when discoveries are converted into proprietary products the amount of prior public investment required to bring them to fruition is not taken into account. Biotechnologies build on years of publicly-funded research in 'pure' molecular biology; they continue to draw on advances in 'public' science. As Nelson says, modern biotechnology is a canonical example of a field where science and technology, public and private are inextricably mixed (1996:141) Allowing those who placed the last brick on the wall—in patent terminology, the first to 'bring to practice'—to privatize the whole system seems not only unfair, but unjustifiable.

In more general terms, broad 'pioneer' patents appear to act as a disincentive to further development because of the likelihood of patent infringement, and the legal costs of defending such

infringement. The effect is analogous to an 'act of enclosure' over a wide area of the intellectual landscape. Nelson argues strongly that patent scope should be kept as tight as possible. To the response that broad patents are necessary to encourage inventors to innovate, Nelson points to technologies that have been developed without such protection; for example, semiconductors, transistors, and integrated circuits. He states unequivocally:

> We believe that the granting and enforcing of broad pioneer patents is a dangerous social policy. It can, and has, hurt in a number of ways...And there are many cases in which technical advance has been very rapid under a regime where intellectual property rights were weak or not stringently enforced. We think the latter regime is the better social bet (Nelson 1996:137).

In that it *underutilizes* scarce resources, the situation has been described as an 'anticommons' (Heller and Eisenberg 1998). Proliferating patents and licenses 'upstream' block each other, and impede researchers 'downstream'. Rather than stimulating innovation and diffusion, therefore, a tangle of fragmented and overlapping patent claims impedes the advance of knowledge. Researchers must obtain licenses and pay royalties to all who hold interests in the 'upstream' basic technologies (Nelson 1996). As a result, and paradoxically, an increase in intellectual property rights can lead to a decrease in useful products. In a 1998 report, the House Committee on Science in the United States' Congress acknowledged the 'chilling' effect of university patenting, stating that 'a review of intellectual property issues may be necessary to ensure that an acceptable balance is struck between stimulating the development of scientific research into marketable technologies and maintaining effective dissemination of research results'.

Rosenberg (1998) emphasizes the continuing economic importance of sustaining basic research, rather than directing it into specific and narrow commercial applications. He shows that the majority of R&D funding (80%) is spent on already-existing products; i.e. on improvement, not innovation. He cites telephones, transistors, lasers, and computers as examples of the essentially unpredictable

nature of the technological outcomes of basic research investments. Similarly, Nelson & Romer (1998) point out that basic academic research produces a multitude of new, publicly available ideas that everyone can share, thereby stimulating innovation. The enforcement of university intellectual property policies, they argue, chokes off this important source of innovation. They fear that 'instead of offering new and different opportunities for the Pasteurs of the university, policy makers may try to convert both the Bohrs and Pasteurs into Edisons' (1998:45). Modern-day Pasteurs must continue to find a place in the university, they say, if progress is to continue. 'If badly designed policies interfere with this interaction, they can do great harm'.

In summary, the conditions of knowledge production are such that the details of institutional and organizational differences between the public and private sectors 'really do matter' in the open science model. Paul David argues that the integrity of science and the scientific method depends on 'maintaining an ethos of *openness* and *cooperation* among researchers, supported by the presupposition that the *reliability* of scientific statements is a collective product requiring independent verification, and consequently conformity with some behavioural norms regarding the disclosure of their findings' (1995: 13). As noted earlier, these institutionalist economic arguments mirror those of social critics of university commercialization, indicating a developing consensus which may be significant for future policy.

But for another influential model, demarcations such as public/private and basic/applied are basically meaningless and intellectual property is just one of the many 'intermediaries' in a knowledge production system constituted by flows, circulations, and network linkages.

The Overflow Model

The opposing view to the open science model, (lacking an umbrella term; I will call it the *overflow* or *network* model) argues that changes to the knowledge production system over the last two decades are radical and irreversible, and constitute a productive force for the good. Callon (in press:3) states that the open science model defends 'Cold War institutions' that have now 'had their day'; they constitute obstacles to science's ability to contribute to economic development. Especially in the biosciences and information and communication technologies (ICTs), tight coupling and multiple linkages between state policy, university research and industry receptors, is the new norm. Public/private and basic/applied distinctions are beside the point here; what matters is the extent of the connections.

The model is process-based; its intellectual antecedents can be traced from Heraclitus through Alfred North Whitehead. What this model attempts to describe may be closer to the historical reality than the open science model, the 'purity' of which can be seen as an artefact of post-war affluence. As suggested earlier, there was a long tradition of cross-sectoral linkages in the interwar years and before. However, the shift in degree of cross-sectoral interactions today is a marked departure from earlier times.

Michel Callon[27] supports the argument that the state should invest in basic research, and he is concerned about the increasingly problematic confrontation between the logic of disclosure and free circulation of ideas and the logic of proprietary knowledge and secrecy. However, he rejects the economic foundations of the open science model.[28] Stories that invoke *market failure* to define science as a *public good* are wrong. 'The thesis of underinvestment in research [by the market] is becoming more and more difficult to support,' he says; 'public laboratories are one after another falling into private hands, either directly through takeovers and cooperative arrangements or

[27] See for example Callon 1994, 1997, 1998a, 1998b, in press

indirectly through incentives and research programs' (1994:401). Rather than defining the private domain in terms of withdrawals from the public domain, as Starr does, Callon inverts the question by pointing out that a lot of effort is required to make scientific knowledge public, whereas almost no work is required to keep it private.[29] To Callon, science has always been 'potentially privatizable'; to maintain it in the public domain requires intensive investments of energy by scientists and the state, and institutions like universities.

In Callon's formulation, which is anchored in actor-network theory, no a priori distinction separates public and private. Instead we have heterogeneous networks—*hybrid collectives*—some local, some extended, in which science is constructed and circulates. The more networks there are, the more scientific innovation flourishes. 'Science is a public good when it can make a new set of entities proliferate and reconfigure the existing states of the world. Private science is the science that firms up these worlds, makes them habitable. This is why public and private science are complementary: despite being distinct: each draws on the other' (1994:416).

Local networks are private in the sense of 'intimate', in that the space of circulation is limited. When network science overflows the local frame, the space of circulation opens up. At the same time, however, the magnitude of investments required is enormous and tends to generate long and complex chains of associations. As the network settles into place so the links and relations become standardized and 'heavy with norms'. This tends to produce what Callon calls 'irreversibility' and economists of an evolutionary persuasion call path-dependence and technological lock-in. In other words, the network becomes self-perpetuating and the space for the circulation of new ideas shrinks. It is at this point that intervention is needed and the hard work of keeping science public must take place. Strong, stabilized networks should receive no additional public support, says Callon. Instead,

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[28] The *economic* details of the arguments are beyond the scope of this paper, but are fully articulated in Dasgupta and David 1994; also David 1998a, 1998b, 2000, on the one hand, and Callon 1994, 1998a, 1998b, and in press on the other.

support should go to encouraging the emergence and proliferation of new networks. It is the *variety* of academic research that thwarts the tendency to lock-in. Established networks should be constrained by requirements to disclose the knowledge they produce and by limiting the duration of patent protection.[30]

But Callon (in press) admits that accounting for the 'dual movement' of scientific *exploration* and commercial *exploitation* is a difficult question, in that investments in established and profitable developments have to be encouraged at the same time as new, currently unprofitable, avenues of enquiry. In other words, without the incentives of 'open science', how do we ensure a continuing supply of basic research? To address this question, Callon directs our attention to fields such as biotechnology and ICTs that in his estimation successfully balance exploration and exploitation. These fields 'constitute veritable social laboratories in which new arrangements, devices, and rules of the game are tried and argued' (3). These areas rely as much on tacit (applied) as codified (basic) knowledge.[31] Callon argues that rules on prior disclosure make tacit knowledge easily appropriable as intellectual property while codified knowledge is not, because in the latter case disclosure is difficult to contain. The main problem with the open science model, according to Callon, is that 'it is not allowed to cross boundaries' (7). While good at describing existing institutions, it has 'nothing to say about the work that transforms scientific knowledge into commercial innovations' (7). In other words, it addresses only codified knowledge and assumes the same conditions also apply to tacit. Further, it assumes we can draw clean lines between these two forms of knowledge.

In contrast, Callon argues that biosciences and ICTs are 'emerging sciences' that are *both* autonomous *and* strongly connected to the market economy. 'Emerging sciences' seems to occupy a mid-point on the continuum between tacit/embodied and codified/consolidated. In other words,

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[29] For additional discussion on this point, see Cambrosio and Keating (1998)

[30] See Cambrosio & Keating & Keating (1998) for discussion of the way monoclonal antibodies moved from local to extended networks

they belong in Pasteur's Quadrant. Subsequent 'translations' align emergent networks and move them towards consolidation. The reverse is also the case. Consolidated networks can unravel and cede to emergent.

While more extensively theorized, Callon's model bears a close 'family resemblance' to two descriptive formulations that have been circulating in the science policy/science studies literatures since the early 1990s, when government cutbacks in research funding and enhanced expectations of commercial exploitation began to fundamentally rewrite the conduct of academic science.

MODE 2 AND TRIPLE HELIX

'Mode-2'[32] and 'Triple-Helix'[33] formulations emerged in the early 1990s to describe the changing conditions of knowledge production. The first argues that traditional ('Mode-1') ways of producing knowledge are being replaced by new ('Mode-2') configurations. Mode 2 knowledge is produced in contexts of application by new, transdisciplinary networks that operate along the periphery of the academy and extend beyond it. They combine heterogeneous skills and different types of expertise in flat rather than hierarchical forms, that shift and recombine as the problem-focus changes. Rather than being accountable to the community of science, they are accountable to the community at large. Quality control extends beyond traditional peer review structures to include the broader set of practitioners that populates these networks. Mode-1 may be considered analogous to 'Bohr's Quadrant'. The focus on 'useful' knowledge and the context of application in Mode 2 clearly suggests 'Edison's Quadrant'. In this typology, there seems to be no room for a 'Mode 3' or 'Pasteur's Quadrant'.

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[31] Collins (1982) provides a classic SSK analysis of tacit knowledge and scientific networks

[32] See Gibbons, et al. (1994) and Nowotny, et al. (2001) for the full exposition, and (Jacob 2000) for an excellent summary

[33] For the model's attributes see, for example, Etzkowitz, et al. (1998)

In a complementary fashion, the 'triple-helix' model posits the recursive interaction of academy, industry, and state institutions in pursuit of knowledge-based economic development and innovation. Triple-helix proponents argue that these institutional alliances signal a new 'democratic corporatist' form that creates a new 'quasi-public sphere...in between representative government and private interests (Etzkowitz 1997a:149-150). This new arena legitimates the state's involvement in an area that might otherwise be left to the 'invisible hand' of the market.

But Saul (1995) sees little difference between the new corporatism and the goals of old, fascist-era corporatism. These were 'to shift power directly to economic and social interest groups; push entrepreneurial initiative in areas normally reserved for public bodies; and obliterate the boundaries between public and private interest—that is, challenge the idea of the public interest. This sounds like the official program of most contemporary western governments' (87-8).

Integral to the triple-helix vision is an image of a new type of university--the *entrepreneurial university* (Etzkowitz, et al. 1998). In contrast to the 'passive' linear model, where knowledge was handed over to industry for exploitation, the entrepreneurial university capitalizes its own knowledge, thereby changing the dialectic between the university and society. The primary vehicles of change are public/private linkages and collaborations, and dedicated structures to capture, capitalize, and exploit intellectual property (Etzkowitz & Leydesdorff 1997; Etzkowitz, et al. 1998). Triple-helix proponents firmly locate these collaborations within the productive sector of the economy (Etzkowitz and Leydesdorff 1997). Again then, the empasis falls on 'Edison's Quadrant'.

Fuller (2000) warns against uncritical acceptance of the perceived dichotomy between new and traditional forms of knowledge organization, calling it 'the myth of the modes' (page xiii). Far from being new, says Fuller, the 'institutional dawn' of Mode-2 and triple-helix models can be found in 19th century Germany's large-scale academy/industry/state collaborations in physics and chemistry.

Positing radical breaks and new eras obscures the basic continuity in knowledge production and betrays a presentist understanding of history. Rip (2000) presents the new models as rhetorical ploys ('fashionable ideas') that name features always-already present. They favour descriptions of 'revolution' rather than 'evolution', because they are normatively loaded towards entrepreneurial activities and public/private partnerships. Nevertheless, a programmatic orientation towards the new formulations has been incorporated into the science policies of most OECD countries.[34]

Policy Regimes

In order to understand how these models are being operationalized, we need a broad framework that will encompass the way our two cross-cutting dimensions (public/private; basic/applied) play out at the policy and program level. Arie Rip's concept of 'policy regimes' fills that role.[35] Rip suggests that science policy regimes manage the mutually-dependent 'national research system': a landscape made up of interactions between research performers, funders, users, markets, and state 'incentive structures'. Policy regimes 'lock-in' to particular trajectories of institutionalization. In the 1950s and 1960s the linear model of innovation and the social contract for science dominated. In the 1970s and 1980s, a flurry of activity marked 'big science'. Today, we have the 'strategic science' regime that was initiated during the high-tide of neoliberalism in the late 1980s.

Neoliberal ideology advocated a comprehensive withdrawal of the state from the economy. Regardless of political complexion, governments 'all abandoned Keynesian policies and...pursued fiscal restraint, tax minimisation, deregulation, and marketization' (Marginson 1997:73). States began to divest themselves of public utilities, nationalized industries, national airlines, and controlling interests in strategic industries. Truly 'public goods'—that is, those with costs but no profit

[34] see Jacob & Hellstrom, 2000, for examples

potential—could safely be left with the state (Teeple 1995); everything else belonged in private hands.

At the same time, states adjusted their redistributive functions. Here too, the logic of the market prevailed. Citizens were to become self-regulating 'enterprises' and market themselves accordingly (Gordon 1991; Rose 1996). Translated into the public service, this reformist spirit became known as the 'enterprise' or 'entrepreneurial' model, more formally 'New Public Management' (NPM).[36] This new culture took as axiomatic market-like principles of cost-recovery, competitiveness, and entrepreneurship in the provision of public services (Power 1996). At the same time, accounting , auditing, and accountability measures normalized the new principles and entrenched them in the public service ethos. From 1980 on, then, public funding of academic science began to be contingent on these same principles, which continue to dominate policy mechanisms. 'Neoliberal science' is Strategic Science.

Strategic Science qualifies as the signature policy regime of neoliberalism across two dimensions: 'steering' (attempts by the state to impose an agenda) and 'aggregation' (institutionalized processes of agenda-building). Thus Strategic Science has developed 'more or less stabilized rules of how to proceed' towards the state's goals of wealth creation and sustainability. At the same time, an emergent new scientific establishment is 'promising to contribute to [those goals] and forging new alliances with policy makers and societal actors on this basis' (Rip 2001:4 ; see also van der Meulen and Rip 1996:346-7).

The Strategic Science regime typically combines concerns for *relevance* (applied research for the private sector) with demands for *excellence* (basic research to enrich the public knowledge base).

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[35] For the development of Rip's thinking over time, see Rip 1990, 1997, 2000, 2001; van der Meulen & Rip 1996

[36] See Hood 1991 and 1995 for a full accounting of NPM more generally; Savoie 1995 for its influence in Canada

These ideas were coming into the policy discourse at the time the Networks of Centres of Excellence program was conceptualized, in the mid-1980s. Rip (2000) speaks of 'fashions' in ideas and the 'abstract sponsorship' ideas exercise. Ideas matter. Their power lies in their performativity. They help to 'order the world', shaping agendas and outcomes (Goldstein and Keohane 1994). Modest ideas like *relevance* and *excellence,* and big ideas like New Public Management, Systems of Innovation, and the Knowledge-based Economy disseminate widely and become dominant. In describing this effect, I have used the term 'international ideas' (Atkinson-Grosjean 2002).

In science policy, 'international ideas' are a combination of principled and causal beliefs[37] held by dominant international 'knowledge élites' about the economic importance of scientific knowledge, and the best way to harness science to the economy. New ideas about science and the economy tend to circulate first in epistemic communities[38] of policy professionals in international organizations like OECD, the G7, and the World Bank. These expert communities then 'teach' the new ideas to member states,[39] creating convergence around particular regimes or models.[40] These organizations also supply the formal and informal structures through which policy frameworks are negotiated and ideas disseminated.

I suggest that the broad outlines of Strategic Science in Canada emerged as part of this general internationalizing movement. The material effects of international ideas can be seen in the reformulation of funding priorities; new infrastructures for the exploitation of intellectual property; and initiatives such as the Networks of Centres of Excellence program. 'Excellence' is one of the defining tropes of Strategic Science. It is not an innocent term.

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[37] See Goldstein and Keohane (1994) for a full explanation of worldviews and principled and causal beliefs

[38] In a later chapter I will be describing epistemic communities of scientists, but the term was first used in relation to the international policy community. See, for example, (Ruggie 1975; Haas 1992)

[39] Martha Finnemore's work is important here, see 1992 and 1993

[40] A dialectic is at work in that many of the policy professionals are seconded from member states. According to an informed observes, one finds a mutual shaping of policy, between and among the member countries, the Permanent Secretariat, and the expert communities

In its fixed sense, excellence simply means 'high quality'; this is unobjectionable. But in its relative sense excellence means 'superior' or 'better than the norm'. Used in this manner, performers of 'excellent' research stand in contrast to a much broader population of average or marginal[41] performers. In their critical review of the career of 'excellence' in UK science policy, Gallart & Salter (2001) point out that 'by its very nature excellence can only be achieved by a very limited number of researchers or research groups' (5). These authors fear a 'Matthew Effect' (Merton 1968) that will direct funding exclusively to researchers and research organisations with established records of excellence. Not only would this restrict diversity and capacity in the research system, it would cut off the important contribution of 'average' science in areas such as training the next generation of researchers, opening up new fields of inquiry, and offering a wider field of social choices about which new technologies get developed (Gallert & Salter 2001: 8).

Michel Callon has argued[42] that concentrating research funding on established scientists and institutions leads to less innovation than spreading funds across multiple sites. Nelson & Winter (1982) and Rip (1997) reinforce that variety in the system ensures possibilities for new entrants, who often sit on the margins of traditional disciplines. Similar concerns about exclusion and loss of diversity were expressed when Vannevar Bush was developing 'the doctrine of basic science'. As Bernard Cohen recalls,[43] there was a fear that setting up a National Science Foundation would institutionalize 'the monolithic pressures of scientific orthodoxy' and support 'only research of a recognized kind in established fields'.

In my later analysis of CGDN discourse, excellence will emerge as a dominant trope in the guise of a performative élitism, with themes of inclusion and exclusion. Actor-network theory (ANT), 'the

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[41] Gallart & Salter (2001) use the term 'mediocre' but do so polemically, to enhance the contrast

[42] See the upcoming chapter in Science Bought and Sold, Mirowski & Sent (eds) as well as Callon (1994)

[43] See (Stokes 1995); Cohen was responding to Stokes's presentation and responses are appended to the document. Cohen was disussing the Bowman Report, the foundation document for Bush's (1945) landmark: Science: The Endless Frontier

sociology of translation', provides a way of understanding these results. ANT helps me describe the way scientists and others in the Canadian Genetic Diseases Network continuously negotiate competing demands for excellence and relevance from the NCE program, while continuously inventing (translating) their network.

III. Translating Networks

The sociology of translation describes the politics of scientific organization and practices using a vocabulary of power, force, strategy, and negotiation (Pels 1997). As will be seen, these idioms are especially useful for analyzing the practical arrangements and power relations at work in the Canadian Genetic Diseases Network.

In ANT's precursor study, Laboratory Life, Latour and Woolgar (1979) crafted 'a political economy of truth' by weaving together the economics and politics of science. Their 'integrated economic model of the production of facts' explained scientific credibility in terms of the accumulation and maintenance of symbolic capital. At the same time, however, they portrayed political competence as central to scientific work, seeing little practical difference between 'politics' and 'truth' (Latour and Woolgar 1979:213, 237; Pels 1997:10). The emphasis on power was made more explicit in the work of Michel Callon, who coined the term *actor-network* and defined it as a theory of *translation.*

Using the analogy of a 'seamless web', ANT attempts to understand the *materiality* of the social and technical relations permeating heterogeneous materials. In ANT, distinguishing between 'facts' and 'artifacts' is neither useful nor relevant: all are actors in the network and all are treated symmetrically. Since materiality is a relational effect, it is provisional and susceptible to change. Boundaries are fluid not fixed; the emphasis is on connection, interdependence, mutuality, and flux (Bingham 1996). It is important, therefore, to stabilize actors and actants in order to maintain the tenuous stability of the

network, which can quickly dissociate without constant attention. Stabilized facts, practices, and artifacts—those under temporary control—are 'black-boxed.' For the moment at least, they are no longer questioned or considered controversial.

Power and agency are relational effects of networks 'acting at a distance' by 'remote control.' The achievement of action at a distance is exemplified by the concept of *centres of calculation* (Latour 1987) or *centres of translation* (Callon 1986), where the ability to control actors at the periphery translates into power at the centre. These key ANT ideas have found their way into post-Foucauldian theories of governmentality, illustrating not only ANT's conceptual fertility but also its location between dual 'repertoires of disenchantment': Nietzsche and Foucault on the one hand, and Marx and Bourdieu on the other (Pels 1997). Power flowing through networks accumulates in the hands of actors who are able to *enrol* the most *allies, translate* their interests, and act as their *spokesman* (Callon 1986; Callon & Latour 1981). Power and agency lie in this ability to intervene between forces and stabilize power relations. The most powerful actors—those who assume a network's leadership and become its *spokesperson*—are those who enrol the largest number of irreversibly linked allies (Pels 1997:11). Rather than being delegated by pre-existing groups to speak on their behalf, spokespersons actually *create* the groups they speak for, by the very act of assuming the role of spokesperson (Cambrosio, et al. 1990:214).

For example, Latour (1988) shows that Louis Pasteur 'the scientist', who made fundamental discoveries in microbiology and public health, is inseparable from Louis Pasteur 'the politician', who skillfully translated and mobilized legions of microbes, farmers, laboratories, and other allies to create new sources of social power and legitimacy. Pasteur became 'Pasteur', the authorized spokesman and exclusive interpreter for the heterogeneous multitudes he enrolled. For Latour, a sociology that concerned itself only with 'social facts' and 'social relations' would miss the most

interesting features of science as a political practice. The sociology of science needed to be redefined as the science of strong or weak *associations* (Latour 1988:40, see also 1986).

As ANT developed, the linkages between science and politics became firmly embedded, and the concept of networks so extensive, that *everything* was explained in network terms. *All* social relations, including power and organization, were treated as network effects (Law: 1992:379). In a seminal paper, Lee and Brown (1994) argued that ANT 'plays god' when it claims the ability to know the whole world through networks. They ascribed a 'Nietzschean world-view' to ANT: one which 'simultaneously secures the universal applicability of its political metaphorics, and stretches the notion of relational power...to cover everything' (778). In claiming the right to speak for all, said Lee & Brown, ANT risks becoming 'yet another ahistorical grand narrative'.

It was this paper that began the reflexive self-questioning that spawned a 1997 workshop on what 'comes after' ANT[44] as well as a subsequent book of the same name (Law and Hassard 1999), and a whole new literature to which Callon's analysis of 'overflowing' networks belongs. Despite the 'imperialist' tendencies Lee & Brown warn against, it is precisely ANT's 'totalizing tendency'—its ability to fully account for the workings of power in network relations—that makes it such an appropriate analytical tool for my case study of the Canadian Genetic Diseases Network. However, like micro-studies of science in general, ANT is less than helpful when it comes to accounting for the *structural* relations between CGDN with the NCE program.

[44] Actor-Network Theory and After, Keele University, 1997

Structural Issues

Relational approaches to the study of science ask 'how' questions about the micro-level of knowledge production (Knorr-Cetina & Mulkay 1983:6). The focus is on detailed, ethnographic description of local practices, or close historical study of specific episodes. The key is simply to 'follow the actors' (Latour 1987) at the actual site of their scientific work. Explanation emerges once description has been saturated, or pursued 'to the bitter end' (Murdoch 1995:731). With such a strong focus on the local, surrounding institutions tend to become epiphenomenal 'scale effects' of relational networks. The entire research system can be viewed as a contingent *outcome* of the 'powers of association' attached to networks. Causal accounts are abandoned. Social and normative 'why' questions disappear in the minutiae of mundane 'how' questions (Shapin 1995b). Political-economic issues vanish into the local politics of research.[45]

Like Winner (1993), Kleinman (1991; 1998), Fuller (1992), and others I find this not only unsatisfactory, but also methodologically unsound. To me, the micro-focus neglects important *already-existing* structural and institutional features that constrain individual and collective actors. One of the goals of this study is to encourage ANT towards something it has long sought to avoid: full engagement in the agency/structure debate and a more satisfactory accounting of formal institutions. ANT tends to fall into infinite regress when attempting to account for structural features. Keating & Cambrosio frame the problem as follows:

[45] Critics of the changing milieu of academic knowledge production view phenomena such as patenting and public/private research partnerships as evidence of the intrusion of global capital and market ideologies into academic institutions. But in practice-based approaches, as Knorr-Cetina (1995) admits, these wider concerns disappear.

> the fact that traditional sociological dichotomies (macro and micro, social and technical, nature and culture) are inappropriate tools for describing and analyzing scientific and medical practices ...has been a leitmotif of many recent contributions to the science studies field. Yet, once the ritual rhetorical ceremony of excommunicating the usual dichotomies has been performed, the question remains of [what] analytical frame...will allow us to move to...an appropriate account of, say, the development of biomedicine in the last half-century (2000:385)

I think we can meaningfully speak of 'structure'—and study its effects on the way science is organized and done—without reifying it. One way is to use the gerund form: 'structuring'. Another is Giddens' notion of structuration.[46] Law (1992) has proposed 'punctualization' to denote networks that 'run wide and deep—the seemingly macrosocial—[that] can be more-or-less, most of the time, taken for granted (Law 1992:385). Accepting that 'divide talk' is ultimately meaningless, and that continuity overrides all these distinctions, perhaps this is a good enough place to begin the structural (policy/program) level of my study. But in order to undertake the micro-level study of the Canadian Genetic Diseases Network, I first had to overcome another methodological problem associated with science studies.

'Studying up'

As Shapin points out, science studies is 'one of the few sociological specialities...that aims to interpret a culture far more powerful and prestigious than itself...[and]...few students come equipped with relevant competencies in the natural sciences' (1995b:293). He calls this the problem of 'studying up'.[47] By proposing to enter the social world of medical geneticists, without being an initiate, I had to deal with the issue of whether or not I could, or should, acquire linguistic competence in the field.

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[46] There are so many other differences between ANT and Giddens' theorizing that this does not seem practical, but the term itself is still suggestive

[47] For another perspective on 'studying up' see Bronwyn Parry's (1998) interesting account of her attempt to study 'élite networks' of senior executives in big pharma and biotechnology

Latour & Woolgar (1979:27-8) called Laboratory Life, their pioneering study of scientists-in-action, an 'anthropology' of science. The study was an ethnographic investigation, grounded in participant observation, of one specific group of scientists in one specific setting. Using anthropological means, they hoped to penetrate the 'closed-shop' status of science, and open up scientific claims, by breaking down the mystique of scientific objectivity. In order to understand the tribes they study, anthropologists usually attempt to acquire linguistic and cultural competence by immersing themselves in the field. In contrast, Latour & Woolgar made it a methodological principle to maintain their 'anthropological strangeness' in regard to their subject matter

Although conducting a field-based study, they made a point of maintaining critical distance. They decided an understanding of science was not a necessary prerequisite for understanding scientists' work. On the contrary, 'the dangers of *going native* outweigh[ed] the possible advantages of ease of access and rapid establishment of rapport with participants' (29). Thus Latour & Woolgar's stories of 'laboratory life' were accounts based on 'the experiences of an observer with some anthropological training, but largely ignorant of science' (30). In the land of science, they chose to be 'the stranger' (Simmel 1950), a mixture of presence and absence, proximity and distance (Shields 1992).

But to what extent can strangers, ignorant of the 'native language', expect to penetrate the meaning of activities they observe and document? Certainly strangers may be able to observe without bias but, on the other hand, they may utterly misinterpret what they observe. Alleged misinterpretations by science studies researchers have provided ammunition in the 'Science Wars'.[48] Physicist Alan Sokal argues that our case studies are often contaminated by 'extremes of subjectivism, relativism and social constructivism'.[49] Even science-studies scholar Steve Fuller admits that science studies

[48] This debate is well beyond the parameters of my study. For more information see, for example, Koertge 1998 ; Segerstrale 2000 and others

[49] The comment derives from my interview with Sokal for Atkinson-Grosjean, 1997:11-12

practitioners often appear to be 'carping from the sidelines' (ibid.) and argues that researchers should acquire at least a basic level of scientific literacy.

The solution, according to Harry Collins (2000), who has 'studied up' for decades, is to differentiate the types of competence required. Science studies researchers do not need 'procedural expertise', ability to do the science, but they must develop 'interactional expertise', ability to talk knowledgeably to experts in the field. I set out to gain interactional expertise by immersing myself in readings about medical genetics, and molecular biology, both prior to and during the study.

I relied mainly on journals like 'Science', 'Nature', and 'Nature Genetics'. While I found the 'empiricist repertoire' of the scientific sections of these journals almost impossible to penetrate, the 'news' and 'features' sections were couched in an informal 'contingent repertoire' and proved much more accessible.[50] I also tracked developments in the field by subscribing to electronic lists like Medscape's Molecular Medicine MedPulse and Science Week, as well as activist monitors like Genetic Crossroads and Loka Alert. As with any language, however, I found the best way to learn was to hear it spoken. I gained most of the interactional expertise I needed to complete the study by interviewing informants, participating in informal conversations, and paying close attention to the papers and posters at CGDN and HUGO scientific meetings.

IV. Summary

This study rests on the tension between two cross-cutting dimensions: public/private and basic/applied; it pays particular attention to the separating '/' . This '/' represents the overlapping interstitial spaces in which the 'open science' model and the 'overflow model' offer their competing

[50] See Gilbert and Mulkay, 1984, for a discourse analytic approach to science studies

explanations. The 'open science' model was the dominant policy regime of the postwar years. It enacted a social contract for science and a linear system of innovation that justified unfettered government funding for basic science. The 'overflow model' captures the *zeitgeist* of 'neoliberal science'. Examples include 'Mode-2' and 'triple-helix' formulations. In the Strategic Science policy regime, governments are more interested in funding research with direct application than in funding basic science. They deploy a dual rhetoric of research *excellence* and commercial *relevance*. Funding is contingent on cross-sectoral partnerships, market applications, and the formation of research networks. The sociology of translation—actor-network theory—offers ways to understand the complex interactions that take place in the network forms of scientific organization that emerge under this regime.

Chapter 3. Science Policy In Canada And the NCE Experiment

A review of Canadian science over the past century confirms the hypothesized fundamental continuity and absence of radical breaks. Continuity can be seen in longstanding R&D relationships between public- and private-sectors and in the federal government's historical commitment to the commercial relevance of publicly funded science. The broad periodizations and policy regimes discussed in the previous chapter, and the onset of Strategic Science, can be clearly discerned in the Canadian case.

In the field of policy studies, analysts account for three mutually interacting influences that shape and constrain the business of policy formation. Powerful ideas, powerful institutions, and powerful interests act as gatekeepers to the process of agenda-setting. These three 'structuring' influences can be seen at work in the historical development of Canadian science policy and public science institutions described in the first half of this chapter. The second half of the chapter focuses on the formulation and implementation of the Networks of Centres of Excellence program as an instrument of Strategic Science policy.

I. Historical Influences on Policy

Historian Donald Phillipson[51] suggests the Canadian state has had an abiding interest in the economic relevance of science and in promoting public- and private-sector interactions. He suggests three principal reasons why this might be the case. First, consistent with interest-based explanations, until quite recently 'everybody knew everyone else and everybody that mattered' at the senior levels of industrial, academic, and government science. For a century up to the 1960s, science in Canada was very much the enterprise of a small élite group of men from similar socioeconomic backgrounds[52] who held interlocking positions of power.[53] Their networks of influence went 'up' to the politicians, 'down' to the top Canadian talent in their own fields, and 'sideways' to senior scientists in other fields. This is illustrated in C.J. Mackenzie's response to a journalist on whether it was difficult to get government approval when the National Research Council established a nuclear research unit during the Second World War. Mackenzie, then President of NRC, replied,

> It was surprisingly easy. In those days the NRC reported to C.D. Howe [then Minister of Department of Trade and Commerce].... C.D. was a particular friend of mine.... We all went to C.D.'s office and discussed the idea with him. I remember he sat there and listened to the whole thing, then he turned to me and said: 'What do you think?' I told him I thought it was a sound idea, then he nodded a couple of times and said: 'Okay, let's go.' (B. Lee, 'The Atom Secrets,' Globe Magazine, October 28, 1961; cited in Porter, 1965:432)

For most of the country's history, policy making was personalist (Phillipson 2000). It operated on social capital rather than academic or scientific capital. Decisions were made on the basis of whom one knew. So the story of Canadian science policy is in large part the story of the people who made

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[51] The historical background presented in this chapter relies heavily on (Phillipson 1983) and (Phillipson 1991), but more especially on our personal correspondence. By virtue of his oral history projects in the 1970s and 1980s, Phillipson is an authority on the National Research Council and the evolution of Canadian science policy. He has communicated an enormous amount of background material to me in a series of letters over the period 1998-2001. His collegial willingness to share his scholarship has enriched my understanding and I acknowledge his contribution to this policy history, which in many cases draws directly on our correspondence. Parts of this chapter appeared in Atkinson-Grosjean, et al. 2001 and Atkinson-Grosjean 2002 (forthcoming)

[52] Most were Canadian-born of British extraction, middle-class in origin and Protestant

[53] See Porter 1965: 507-11. For the operation of the US 'power élite see Mills, 1956

it. The evolution of policy attitudes towards the respective roles of basic and applied science reflects the evolution in élite ways of thinking on the topic. Although the influence of élite interests has become more subtle in recent years, it remains a major factor: 'This is Canada. When these people speak others listen.'[54]

A second element identified by Phillipson relates to institutions. Boundaries between public and private in Canadian science are quite unstable and tend to evolve fairly quickly in institutional terms. Phillipson (1991, 2000) provides the example of the Ontario Research Foundation (ORF). Founded by the province in the Depression era as a rival to the federal National Research Council, ORF was transformed into a successful autonomous public industrial laboratory, a Crown agency, in the 1950s. Later, it was 'privatized' as a state-owned corporation. Subsequently, the shares were bought by a commercial company. Another example is the Canadian Standards Association (CSA). Founded in the early 1920s as a government-funded advisory committee of researchers and industrialists, it was incorporated as a company in 1940, with the approval of a government preoccupied with war research. CSA then moved its laboratories from Ottawa to Toronto. Here, it became a self-financing independent institution, and is still authorised to promulgate and enforce standards.

A third element is 'ideas-based'. Awareness of other national models—predominantly American and British—has always shaped what was implemented in Canada, whether in the early 20th century or the early 21st. In comparison to other advanced nations, we tend to feel we lag scientifically and this has always influenced the projects undertaken. 'The country is dogged by a national inferiority complex' (Phillipson 2000). As described more extensively earlier, the influence of policy 'fashions' from international forums like OECD and G7 can be clearly discerned in the formation of Canadian

[54] University administrator cited in Re$earch Money editorial; Henderson (2001).

policy. Canada's National Research Council, for example, founded 1916, was an example of convergence with similar bodies in Britain and the USA.

Taken together, the interests of powerful élites and the trade in international ideas tend to promote convergence around generalized policy regimes. However, the historical particularities of a nation's institutional and cultural legacies represent a countervailing force for divergence (Banting, et al. 1997). In other words, we put our own stamp on what we adopt. The Networks of Centres of Excellence program is an example. While the phrase 'centres of excellence' was appearing with increasing regularity in the international policy discourse at the time, networking centres of excellence together was a specific solution to the peculiarities of Canadian geography (sheer size and diversity) and 'soft federalism' (powerful provinces and the requirement to serve all regions equally).

Canada's constitutional arrangements represent a longstanding constraint on federal science policy. Universities fall under provincial jurisdiction putting them beyond direct federal reach.[55] Historically, federal control of research funding emerged as one of the few avenues for shaping the 'national' role of universities within the 'knowledge-production system'. But until at least the 1960s, universities were not major players in the research economy. The majority of public science—historically defined in terms of utility and industrial relevance— was conducted by the National Research Council (NRC).

Public Science in Canada

From inception in 1916, NRC's 'public' mission was to serve 'private' needs by directing its research towards 'the most practical and pressing problems indicated by industrial necessities.' The obligation

[55] The federal government funds university operations through transfer payments to the provinces but it has no direct influence on these institutions and receives little credit for its funding role.

to serve industry was literally graven in stone above the doors of the laboratories on Sussex Drive in Ottawa. Public science was defined not as the search for knowledge, but as the search for solutions.

As one of its first tasks, the NRC set out to gauge the state of industrial research in Canada. Survey results showed that only 37 of the 2,800 firms responding performed research on an ongoing basis and most of these employed only one researcher (Thistle, 1966: 29). There was little for NRC to coordinate, therefore, and a clear national need to develop a critical mass of researchers. This conclusion motivated the 1917 introduction of NRC-funded post-graduate scholarships in the sciences at selected universities (Thistle, 1966: 26, 127).

Shortly after, the idea of constructing institutes for industrial research on university campuses began to circulate. But this heresy was briskly disposed of when proponents discovered that university faculty were adamantly opposed to 'bargaining with manufacturers'.[56] Canadian universities modelled themselves on the humanistic traditions of Oxbridge, where the focus was scholarship and teaching. To undertake research was unusual; to undertake research for industry unthinkable. NRC's views were much the same, arguing that universities would subvert their role by conducting industrial research. NRC itself became increasingly drawn to fundamental enquiry, if only to retain its researchers.

Between 1916 and 1940, NRC's workforce expanded from one employee to 2,000; its annual budget from $91,600 to almost $7 million.[57] NRC's wartime expansion allowed Canada's academic scientists to work closely with British and American colleagues on the front lines of basic advances in knowledge of microwave techniques, jet engines, digital computers and nuclear power. They were intent on continuing this momentum into the postwar era but conducting research within Canadian universities was still a 'fringe' activity. For example, C.D. Howe's Office of Supply and

[56] The inquirey was conducted by Hume Cronyn's parliamentary sub-committee struck in April 1919. The 'bargaining' quote is attributed to Professor Lash Miller, University of Toronto, Cronyn Committee Proceedings, June 4, 1919, p. 99; cited in Lamontagne report, 1970: 31

Reconstruction began an annual inventory of university research in 1946 but abandoned the project in 1949. Scientists were 'faking the results, to conceal from university authorities how much they were diverting from teaching to spend on research' (Phillipson correspondence). Universities were preoccupied with educating returning war veterans and other undergraduates. Research was not a priority.

But by then, the linear model of innovation was beginning to circulate as an 'international idea'. In 1951 the Massey Commission[58] articulated the model's pipeline metaphor in noting the importance of fundamental research in priming the pump that eventually produces industrial products and applications. 'Without fundamental research,' said the commissioners, 'there can be no proper teaching of science, no scientific workers and no applied science' (175). In the commissioners' view, basic research was most properly housed in universities which should be adequately funded to conduct it. The Commissioners strenuously opposed the idea that publicly funded laboratories should undertake research for industry fearing that it would deaden the scientific imagination and stall the advancement of knowledge.

> applied research...cannot be expected to add in any way to the knowledge of scientific principles. Occasionally private donors offering research grants require that research projects be approved by them. University authorities generally agree with scientists that these gifts should be steadily refused. (Massey report, 1951: 177)

From 1952 on, when Dr. E.W.R. Steacie took the helm of NRC, support of basic research in universities became a key Canadian policy goal.[59] In line with the logic of the linear model, funding university research was seen as the best way for NRC to achieve its long-term mandate to serve industry. As Steacie said, 'it is absolutely impossible to have first-rate industrial research without

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[57] Lamontagne report, 1968-77, vol. 1: 61.

[58] The Royal Commission on National Development in the Arts, Letters and Sciences, 1949-51

[59] . Steacie left McGill University to become head of NRC's chemistry division in 1939. He was appointed NRC's vice-president in 1950 and president in 1952, holding the latter post until his death in 1962, at which time he was widely acknowledged the 'leader of Canadian science' (Babbit, 1965: 3).

first-rate university research' (1965: 159-160). As in the US, the 1957 'Sputnik shock' had a salutary effect on research funding, helping to cement the state's commitment to basic science. Federal expenditures devoted to R&D grew from an estimated $5 million in 1939 to over $200 million in 1959.[60]

But the policy climate began to change in the decade following the Massey Commission's report. A speculative paper submitted in 1957 by the [Gordon] Royal Commission on Canada's Economic Prospects envisioned the roles that science might assume in the distant future, setting the stage for more intense debate on the status of science in national progress and economic development. In 1962, having examined the federally funded research system, the Glassco Commission concluded that the system had failed. Glassco singled out the NRC for blame, arguing that its (vested) interests in basic 'public' research had been promoted at the expense of applied 'private' research.

> One of the original purposes of government in devoting money to research was to encourage and stimulate Canadian industry. From being a primary goal this has, over the years, been relegated to being little more than a minor distraction.... At present there is a wide-spread feeling that fundamental research is the only activity adequately recognized within the National Research Council. (Glassco report, 1963, vol. 4: 230, 271)

In short, Glassco famously concluded that NRC had 'turned away' from industry. According to a funding distribution in the late 1960s, 91% of the NRC budget was allocated to university research and its own laboratories (50% and 41% respectively) while only 9% was allocated to industrial support and information services (5% and 4% respectively) (Hayes, 1973: 38-39). Commenting on reactions from NRC's scientists and bureaucrats, OECD noted that 'many, no doubt, recognised that there were grounds for the criticism expressed by the Commission, but the majority protested against its recommendations' (OECD, 1969: 63).

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[60] Lamontagne report, 1968-77, vol. 1: 64

Following Glassco's recommendations, a Science Secretariat was established in 1964 and the Science Council of Canada began operations in 1966. Overall, however, the Glassco framework was fundamentally undermined by a report to Prime Minister Lester Pearson by C.J. Mackenzie, former NRC president, who advised against the substance of the findings. The personalist system protected its own. Nevertheless, the Glassco report established a policy climate more hospitable to the applied/private side of the matrix. A number of government initiatives intended to bring academic research closer to the needs of industry were designed in the 1960s.[61] By the end of the decade, the Glassco Committee's main criticisms were echoed in several other policy documents including the Science Council of Canada's 1968 report *Towards a National Science Policy for Canada* and an extensive survey of Canada's science and technology infrastructure by OECD examiners (1969).The OECD and Science Council reports substantially contributed to the decade-long deliberations of the Senate's Special Committee on Science Policy chaired by economist Maurice Lamontagne, 1968-77.

Lamontagne provided an exhaustive analysis of Canada's overall R&D system; the role and performance of federally funded science wherever it occurred; and the culture of science in Canada. At the core of the findings was an attack on the scientific élitism that had driven Canadian science policy since 1916 (Vol. 1: 268). Steacie's proud comment that Canada stands out among the nations by recognizing 'the fundamental fact that the control of a scientific organization must be in the hands of scientists' became an indictment (1965: 119, cited in Vol. 1: 269). Such freedom, the committee argued, 'cannot be justified as a general principle for the organization of scientific progress when the tremendous cost of research has to be met mainly by public funds and when the good and bad effects of science and technology on society are becoming so far-reaching' (Vol. 1:

[61] Among these, the Industrial Research Institute Program, established by the Department of Industry in 1966, provided grants to universities to establish institutes where they could work with industry and undertake contract research on their behalf. Legislative tools were also introduced; in 1967 government passed the Industrial Research and Development Incentives Act which was intended to foster academy-industry collaboration in research aimed at solving industrial problems. As well, in 1969 the NRC announced a grants program for universities that emphasized the promotion of industrial development through 'centres of excellence' aimed at fostering a regional balance of scientific and technological expertise. However, plans for this program were vague.

270-271). Steps needed to be taken to bridge the gap between science and industry and federal funding should affirm and reflect the priority of applied research (vol. 2: 521).

Lamontagne was enthusiastic about the whole business of *planification*--economic forecasting and planning--and its potential for fostering *innovation.* The latter word entered the Canadian policy discourse about halfway through the 'Lamontagne decade'. Seduced by this emerging 'international idea,' the committee also embraced 'the new quasi-economic discipline of science policy that went along with it' (Phillipson correspondence). Committee members and staff were thus 'naively enthusiastic about both (a) the notional completability of the Science Policy model...and (b) its political appeal to actual politicians' (Phillipson correspondence). In politics, extensive data is superfluous to the decision making process. Politicians do not wish to be confused by too many facts. As Cohen, et al. (1972) classically demonstrated, they operate from a 'garbage can model of rationality'. Consequently, despite the years of effort that went into it, the Lamontagne report, too, 'fell dead from the press', failing to find a place on the agenda of the Trudeau administration (Dufour & de la Mothe, 1993: 21, ft. 13).

The power of entrenched élites to resist unwanted change is formidable, but so is the power of new élites to advance change, once the correct tools are in hand. Many remained convinced that the role of public science was to foster industrial innovation and economic expansion and that NRC, with its focus on the advancement of knowledge, represented an impediment to that enterprise. As a crown corporation, however, NRC was beyond direct political and bureaucratic interference. The only way to control it was to systematically strip away its budgets and responsibilities and transfer them to another, more subordinate, agency.[62] In 1971, a Ministry of State for Science and Technology (MOSST) was created (as both Glassco and Lamontagne had recommended) replacing the existing Science Secretariat. In 1977, NRC's responsibility for supporting university research was devolved to

[62] This is what eventually happened to the Science Council of Canada, disbanded 1992 along with other autonomous agencies

a new agency, the Natural Sciences and Engineering Research Council of Canada (NSERC) which then fell under the administrative authority of MOSST. In 1978, MOSST also assumed authority over the Social Sciences and Humanities Research Council of Canada (SSHRC) after the Canada Council was reorganized. This restructuring gradually eroded the autonomy of all granting councils.

Science and technology policy edged gradually towards the top of the political agenda. The first G7 summit meeting, in 1982, revealed the fact that Canada had the lowest R&D investment in the G7.[63]. A Scientific Research Tax Credit was introduced to stimulate investment. It was a flawed instrument, open to abuse, and required a number of revisions to correct the deficiencies, but it marked a major policy innovation. As a result of the changes introduced then, Canada established--and still boasts--the most generous R&D investment and tax climate in the G7 nations.

The following year, as the Liberal Party came to the end of its long postwar mandate, several reports established the need to tie government support of public research to commercial relevance. In 1984, with the election of a Progressive Conservative government, the momentum towards a national science policy accelerated, and the neoliberal agenda came into play. After a period of intensive federal/provincial consultation, a national science and technology policy was formally signed in March 1987. Details of InnovAction: The Canadian Strategy for Science and Technology—a $1.5 billion 'package'—were announced the following month. MOSST would subsumed into a new 'superministry'--Industry, Science, and Technology Canada (ISTC)—a combination that clearly signalled the alignment of science and commerce. Legislation would provide $240 million for a new 'flagship' strategy: the Networks of Centres of Excellence (NCE) program.

II. Evolution of the NCE Program

The NCE program is an example of the way international ideas, existing institutions, and socioeconomic interests interact under a policy regime of Strategic Science.[64] The policy innovation was to bring ideological concerns for commercial *relevance* and research *excellence* together with the concept of distributed research *networks* to form *networks* of *centres of excellence.* Now that 'networks' are so associated with computer imagery, it is hard to remember it was not always the case. By way of policy studies and science studies, the network concept was just then becoming a 'fashionable idea' in its own right, as a way of thinking about the organization of science. This section presents an analysis of the evolution of the NCE program within the policy context outlined above. The data derive from examination of policy documents and interviews with key players involved in the program's formation. Although many of the sources interviewed for this part of the study belong to the scientific culture (most have at least one degree in the sciences and a background in government or university science) here they represent the science policy culture and the 'official' perspective. Most were associated with the federal government, either as past or present employees or policy advisors.

The decision to embark on the Networks of Centres of Excellence program was made in an ideological climate that promoted the outright privatization of public-sector functions. Where this was not possible or desirable, public-private partnerships were preferable to maintaining public-sector monopolies. Most new[65] initiatives in science and technology partnerships saw their beginnings at this time. According to Niosi (1995:34-35), Canada's provincial and federal governments launched over one hundred new intersectoral research partnerships during this period.

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[63] This remains a chronic problem. Only Italy has a lower R&D: GDP ratio. Finance Minister Martin has made increasing the ratio a key commitment for the 2001 to 2003 fiscal period

At the provincial level, Quebec's Programmme d'actions structurantes started in 1984-85 with forty networks of university and government laboratories. Ontario's eight Centres of Excellence were established in 1986. In 1987, Quebec pioneered the Centre d'initiative technologique de Montréal (CITEC) at McGill University. At the federal level, Industry, Science, and Technology Canada (ISTC, later Industry Canada) emphasized public-private partnerships and collaborations. Both the natural science and engineering and medical research councils (NSERC; MRC) actively supported collaborative targeted research. NSERC started to fund 'big science' networks in the early 1980s -- in the earth sciences (Lithoprobe) and integrated circuit design (Canadian Microelectonics Corporation). During 1987/88, the budget year prior to the establishment of the NCE, 15 percent of NSERC's total budget went to targeted research. (For further discussion see Friedman and Friedman, 1990, and Niosi, 2000).

In late 1987, delegates to the National Forum on Post-Secondary Education raised the idea of centres of excellence that would emphasize interdisciplinarity and involve networks of researchers representing several institutions across Canada (National Forum 1987). In 1988, the Science Council of Canada advised that prosperity depended on integrating the university with the marketplace (Science Council 1988). Reinforcing this theme, the National Advisory Board on Science and Technology (NABST) recommended that 'greater emphasis be given to funding generic pre-competitive research collaboration by university-industry in research consortia' (NABST 1988: 76) This complex of initiatives and recommendations helped provide a foundational platform for the January 1988 launch of the NCE program.

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[64] Some material in this section appeared in Atkinson-Grosjean (2002) and Fisher, Atkinson-Grosjean and House (2001)

[65] There were older initiatives. The Pulp and Paper Research Institute of Canada (Paprican), founded in 1925 at McGill University, represents perhaps Canada's most enduring example of a state-academy-industry alliance (C-HEF 1987: 45-6). Another enduring initiative is the NRC's Industrial Research Assistance Program (IRAP), launched in the 1960s, of which more will be said shortly.

Models for the NCE Program

The NCE program was designed as a hybrid of two influential models, one governmental and associated with industry, one non-governmental with no industrial affiliations. The first was NRC's Industrial Research Assistance Program (IRAP) established in 1962; the second the Canadian Institute for Advanced Research (CIAR) founded in 1981.

IRAP dates from when the NRC still ran along personalist ('old boys' network') lines. IRAP's prehistory was as the Technical Information Service founded by Mackenzie in C.D. Howe's Department of Reconstruction and Supply in 1945 and reenergized in 1962 by a retired air marshal named Ralph McBurney. TIS gave 'knowledge subsidies' to industry in the form of technical advice. The 1962 innovation added cash subsidies as well. IRAP would give grant funding to industry for private research, in the same way that universities received grants for public research. According to Phillipson, the idea of giving public money to private industry 'was such an extraordinary precedent that it took a year's preparation by the Advisory Panel on Scientific Policy and required Treasury Board and Cabinet approval' (Phillipson correspondence; see also Phillipson, 1983).

As well as having an innovative approach to industrial research, the IRAP program was organized as a solution to Canada's geographical challenges. Rather than hire technically trained civil servants to give hands-on advice to all sorts of different industries, in every region of the country, IRAP created a mechanism for borrowing them. Approximately two-thirds of IRAP's field agents were locals, co-opted from industries, universities, and professional associations in the region. They were paid by their own institutions which received salary support from IRAP to release them. According to a former IRAP director, these agents constituted a 'field army' (NRC 0101) who knew their regions, closely identified with their industrial clients, and enjoyed an enormous amount of autonomy from the Ottawa bureaucracy.

These Industrial Technology Advisors as they were called, were gateways in extended networks of resources and facilities. Through them, small and mid-sized enterprises (SMEs) had access to some 130 public and private research- and technology-based organizations that were partners in the field network. In the manner that John Law (1992) calls 'heterogeneous engineering', industry clients, their technical problems, technology advisors, provincial labs, federal labs, industry labs, engineering prototypes, and federal money were all linked together in long-chained networks dedicated to helping Canadian SMEs innovate.[66]

The networking model that began with IRAP was clearly focused on the technical needs of industry. In contrast, the Canadian Institute for Advanced Research (CIAR), launched some twenty years later (1981) by Dr. Fraser Mustard, a distinguished medical scientist, was a networking model concentrated exclusively on fundamental enquiry. Mustard and his associates promoted the idea of focusing the basic research effort in a limited number of fields where Canada had a strategic advantage and could make an original contribution. Certainly, elevating the overall pool of knowledge would benefit industry in the long-run, but no immediate applications would be forthcoming.

CIAR was conceived as an 'institute without walls,' a network that would link together outstanding researchers in institutions across Canada. According to those involved at the start, the idea came out of a dissatisfaction with existing arrangements and a realistic sense of the way knowledge works. To deal with complicated problems, some sort of institutional structure was needed that would override disciplinary and geographical barriers to the full exchange of knowledge. As well, the geographical constraints suggested that 'the simplest way to try to move fields was to opt for an institutional structure that invested in people rather than research' (OTHFM-2).

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[66] See Callon 1997 and 1998 for analysis of the market significance of these networks; these should be read in relation to Granovetter's (1985) notion of 'embeddness' in relation to economic action

CIAR raised funding from federal and provincial governments and from private donations but the funding was 'unencumbered and in no way strategic' (OTHPB). CIAR's mandate was the pursuit of fundamental knowledge for its own sake, without need for 'deliverables' or industry partnerships. Industry was viewed as 'a user of the knowledge generated, rather than a collaborative partner' (OTHFM:1). Funding was used to underwrite networking interactions and to buy-out researchers' time at their home universities so CIAR members could pursue research on fundamental questions. The only criterion was that, 'five years from now you're going to be reviewed by an international panel who will see if you have shifted the world community on how it views that question, in terms of its understanding' (OTHPB-12).

In 1986 Mustard became co-director of the committee that was designing the main features of Ontario's Centres of Excellence program, which was launched in June 1987. According to a senior civil servant, Mustard predicted that these new research centres would draw 'key researchers from across the country to Ontario's universities and Ontario's centres,' making it extremely difficult for universities in other provinces to retain the best researchers (NCE-DH:4) As a former NCE program officer put it, 'like a vortex all the best science would migrate to Ontario'(NCE-EI:4)

Earlier in the year Mustard and one of his associates in CIAR, Dr. Patricia Baird, had been drafted onto NABST. Not surprisingly, therefore, it was NABST that brought forward the idea of creating CIAR-like national networks in the fundamental sciences, to counter the Ontario initiative. The target would be fast-moving, high-profile, competitive fields that had technological implications in the relatively short-term. At that stage, direct links to industry were not part of the plan. The rationale was that effective strategic or applied research programs required a good fundamental research base.

The Minister and Deputy Minister of Industry, Science, and Technology Canada paid attention to the NABST recommendations. Clearly, the federal government needed something to balance the Ontario initiative. The idea of creating 'virtual' CIAR-type networks, rather than 'fixed' Ontario-type centres, was especially attractive 'because there just wasn't enough money to create dozens of new centres around the country' (civil servant, NCE-DH:4). The question regarding the relative merits of 'fixed' and 'distributed' centres originated in the postwar Kilgore/Bush debate regarding the creation of the National Science Foundation (see Chapter 2) to promote basic research; the issue was whether the NSF should follow a 'centre of excellence' model or one that favoured a more geographical distribution of funding.[67]

While interested in the network model, the Ministry was not convinced that a focus on excellence in basic research was the correct route. Government wanted to see far more in the way of *relevance*—technology transfer to industry. The outcome was a blend of IRAP and CIAR. Like the latter, NCEs would invest in people (researchers), rather than bricks and mortar (universities and hospitals), and would be free to undertake fundamental enquiry. But, like the former, they would partner with industry and concern themselves with industry needs.

As with IRAP and CIAR, network researchers would be paid by their own institutions but would build a strong sense of belonging to a larger national entity. But in contrast to both, NCEs would be parasitic on their hosts (Newson 1994). Universities and hospitals would receive no compensation for paying the salaries and benefits of network researchers, providing space and equipment, and covering laboratory overhead. NCE funds would flow to the researchers through separate 'network offices' which would have no duty of accountability to the university.[68] Because their reporting allegiance was to the NCE directorate in Ottawa, these new networks would 'float' above existing

[67] Thanks for this point go to my correspondent, Andrew Russell, of the University of Colorado-Boulder, who is studying the development of computer research in the US during the Cold War.

institutions (Clark 1998). They would provide the federal government with direct access to provincial university systems, overriding traditional autonomy (OTH-DR). The networks would create a *national* research capacity open to the needs of industry and the economy.

The compromise balancing 'relevance' and 'excellence' was the outcome of sustained bureaucratic struggles to capture control of the NCE initiative. The battle between the Ministry and the research councils was so fierce, it quickly became a case-study (Pullen 1990) for the federal civil service training institute.

Territorial Struggles and Program Design

Although the federal bureaucracy had been awash in rumours that a major reform of research funding was being planned, the prime minister's announcement in January 1988 came 'out of the blue and without any consultation' with the three granting councils responsible for university research (program officer, NCE-EI: 2). The research council presidents quickly forged an alliance to prevent the NCE initiative being implemented without their input. The president of NSERC assigned two staff members to observe how the Prime Minister's Office was handling the new program and instructed his staff to develop alternative plans (Pullen 1990). A senior NSERC administrator interviewed the consultant hired to develop the program and concluded that the objectives would be impossible to implement (too many criteria, often conflicting) (NCE-MB: 3-4). The councils discovered, as well, that public servants were to review the research applications, with final decisions made by the Ministry; no peer review would be built into the process.

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[68] While NCE funds flowed to the networks through university financial systems, the university was just an intermediary

This contravention of scientific norms became the councils' point of attack. They argued that peer-reviewed competitions were essential to the program's academic credibility. They insisted that the councils were the only bodies with the expertise to run such competitions and to administer the resulting research funding. Without their endorsement and involvement, they suggested, the NCE program would receive a chilly reception in the academic community. If the government wanted the program to succeed, the Ministry could not be allowed to control the initiative.

In May 1988, a compromise was struck. The peer review process would be deployed strategically. By cloaking the program in the 'objectivity' of peer review, it could be protected from political pressures. This separation could then be used to rhetorical advantage by the government. The Prime Minister's Office announced that the three research councils would run the NCE competition and distribute the funds, while ISTC would act as the program's secretariat. The research council presidents and the deputy minister of industry formed a steering committee, while ISTC retained overall control, albeit 'from a distance'.

As a senior civil servant noted, the Ministry 'holds the pen' when writing memoranda to cabinet or making submissions to the Treasury Board and is also 'closer to the centre' than the arm's-length granting councils (NCE-MAL: 18). Further, two of the three research councils (NSERC and SSHRC) fall within the Industry portfolio and the Minister of Industry's sphere of responsibility. Nevertheless, the three council presidents exercised considerable political leverage on the steering committee, because the Ministry had no experience with research management in universities. They were also able to influence the direction of intellectual inquiry, identifying as targets areas where they perceived a research gap.

As a result of the compromise, the policy objective was to reshape the culture of academic science around the dual goals noted earlier: *excellence* (fundamental research) and *relevance* (utility to industry).

An Advisory Committee (to which Fraser Mustard was appointed) was established in June 1988 to design and implement the program. The committee developed four selection criteria. The weighting assigned to each reflected the success of the research councils in capturing the initiative. Research excellence was weighted at 50 percent; a 'coherent, focused program of research' was deemed the most decisive feature (NCE 1988: 1). Relevance to industry was weighted at 20 percent, as was 'linkages and networking'. The remaining 10 percent covered administrative and management capability. In language reminiscent of Pasteur's Quadrant, an informant explains that

> [t]he strategy was to be pregnant -- we needed pure, long-term applied science that was somewhat guided by the needs of industry.... Everyone was grappling with the term 'pure, long-term applied science.' [It] was used to walk the fine line separating science and application (policy advisor; NCE-SS: 2-3)

The program attracted diverse support. On the one hand, it was sold to Cabinet as a regional economic development package. On the other hand, it was promoted to scientists as an élitist program for producing the best science. In fact, according to one interviewee, it was neither, but merely a means to pull together teams of the very best researchers who, by example, would pull the rest forward (policy advisor; NCE-SS: 1). The nomenclature of 'excellence' facilitated the process 'of capturing some of the best researchers in the country [and] recruiting them as champions for change within the system' (senior civil servant; NCE-DH: 8).

Yet the program was intended to reach beyond demarcations of excellence and relevance 'to bring in the whole concept of research management and cross-disciplinarity' (program officer; NCE-SM: 22). As suggested earlier, program design was much influenced by the Mode 1/Mode 2 theory of knowledge production developed by Michael Gibbons and colleagues in the late 1980s and 1990s. Gibbons served as a science policy advisor to Industry Canada during this period, and sat on the NCE selection committees. According to one informant, he was their acknowledged 'guru' (program officer; NCE-SM: 13). Michel Callon was also involved in the early design and

implementation of the program, as a member of the International Peer Review Committee. Thus the conceptual framework for the NCE program seems to have been a hybrid of Mode 2 and actor-network concepts.

Following the receipt of some 240 letters of intent, 158 formal applications were forwarded for assessment to an International Peer Review Committee in November 1988. Composed of first-ranked scientists, engineers and social scientists, mostly from the USA and Europe, this committee reported to the Advisory Committee in June 1989. As previously stipulated by the research councils, the report was made public. Public disclosure gave some assurance that the decisions were made in accordance with established scientific criteria and were not politically influenced.

Sixteen applications were deemed worthy of funding, nine in the 'must be funded' category and seven in the 'recommended for funding' second tier. The Advisory Committee endorsed all nine first-tier networks but, for reasons that remain unclear, would not support two of the second tier networks. One of these, on ageing, was the only social science proposal on the short list. After extensive lobbying by the councils, 'a decision came from above' to include the ageing network (policy advisor; NCE-SS). However, it would be funded by the research councils rather than the NCE. The poor showing of the social sciences was later attributed to selection criteria oriented toward engineering and the hard sciences rather than 'the broad perspective needed to make the participation of human scientists possible' (program officer; NCE-EI).

Because they reflected a compromise, the initial selection criteria failed to fully articulate the preferences of either the research councils or the Ministry. In practice, networking and industrial relevance hardly figured into the equation. And because companies made few cash commitments at the proposal stage, it was difficult to assess the extent of partnerships and linkages (program officer; NCE-MB: 6). Academics inexperienced in such matters found it difficult to demonstrate such

competencies. For similar reasons the applications were weak in defining proposed management structures. Furthermore, the reviewers themselves were not skilled in assessing this area (program officer; NCE-SM: 1-2). As a result, the reviewers

> could not bring themselves to say 'no' to the best science regardless of the other criteria. They could not displace top quality science with inferior science just because they had a better management structure or because they scored so high on practical application. The other three criteria were ephemeral, intangible, hard to measure or understand. [Reviewers] could not bring themselves to knock out top science on the basis of criteria they did not understand and could not operationalize (policy advisor; NCE-SS: 3)

In the end, the reviewers decided to 'gamble on the best [science] and...hope that [the rest] happens' (program officer; NCE-MB: 6)

Mobilizing Networks; Changing Attitudes

The NCE program introduced 'two radical and important' hypotheses according to Stuart Smith, chair of the International Peer Review and Implementation Committees. At a November 1989 briefing session for the winning networks, he told participants that the first hypothesis would test whether collaborative research could be done at a distance using telecommunications technologies. The second would test 'whether it was possible in the field of long-term and fundamental research to force researchers to think about the economic and social impact of their work, and more particularly about the channels by which the research results will be commercialized' (address reported in NCE program internal newsletter, Liaison 1 (1) January 1990).

The federal bureaucracy had no operational framework for the implementation of NCE policy. Ottawa and the networks made up and modified rules and expectations as the concepts evolved. One of the tasks of the program directorate, in the early years, was to convince scientists that their responsibilities extended beyond the standards of traditional funding programs, and beyond the

norms of academic science. Program staff realized that researchers initially viewed the program as just one more funding source for basic science. (See Chapter 4 for the way this attitude manifested at the network level). A policy advisor says 'the scientists didn't know what they were getting into. They just went into it for the money. Very clearly at the start, it was just another pot of money with some arbitrary rules that they would pretend to follow' (OTHDR: 24). It was necessary to convey the 'expectation that [they] were going to interact with industry and that there was going to be some kind of measurable outcome from that interaction' (senior civil servant: NCE-JW: 6).

For the networks, that first phase was all about inventing themselves, consolidating themselves, establishing relationships among researchers, host institutions, and industry partners. Industrial partnerships were slow in coming. 'There was a lot of courting in Phase I and not a lot of commitment' (senior civil servant: NCE-JW: 23). The first year, fiscal 1991, was only a partial year. Networks spent most of their time establishing the mechanics of administration—systems, committee structures, and so on. After that, only three full years remained before funding ended. At that point, no guarantees had been given that the program would be renewed. The program was experimental. As far as anyone knew, four years total was all they had.

That situation changed in December 1992 when the Mulroney (Conservative) government brought down its final budget.[69] In the same speech that abolished the Science Council of Canada and the Economic Council of Canada, Finance Minister Don Mazankowski[70] announced that the NCE program would be extended. A new competition would be held in targeted areas and existing networks would be able to compete for a second four-year phase of funding (fiscal years 1995-8). The decision was supported by a positive interim program evaluation carried out between July and December 1992. The evaluation reviewed the effectiveness of program and network management,

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[69] Mulroney announced his resignation in February 1993. He stayed on as caretaker until Kim Campbell won the leadership contest in June 1993. The party was routed by the Liberals at the polls in October 1993, losing all but two seats

the level of networking, and the nature and extent of industrial involvement. From the tenor of the announcement, it was clear that the latter was deemed less than satisfactory. In order to be renewed, networks would have to deliver much more in terms of commercial relevance and industry partnerships.

> From the beginning, the need for industry involvement and cooperation in the networks has been stressed. Given the need to strengthen this kind of industry collaboration with the research community, funding is being extended. This will ensure that the most successful of the existing networks continue to contribute to competitiveness. (1992 Budget Announcement)

A reduced budget of $197 million was allocated for the four-year period, 1995-98, with 25 percent set aside for developing the planned new networks. Modified selection criteria reflected the shift in emphasis from *excellence* to *relevance*, and precipitated the dilution of meaning mentioned earlier. Now five criteria, all equally weighted, had to exceed an established 'threshold of excellence':

- excellence of the research program : 20 percent (was 50 percent)
- training of highly qualified personnel : 20 percent (new)
- networking and industry partnerships : 20 percent (same as before)
- knowledge exchange and technology exploitation : 20 percent (new)
- network management : 20 percent (was 10 percent).

As a senior civil servant noted, ISTC had successfully 'reorient[ed] the program to something that they were more comfortable with.' (NCE-JW: 12) The new criteria reflected what they had wanted from the start: a program that fostered more industrially relevant research (senior civil servant; NCE-DH: 6). A rotation of research council presidents helped consolidate this position. The new leaders of the MRC and of NSERC were 'very much focused on developing university-industry

[70] Mazankowski's connection with NCEs lasted beyond his political career. In 2000, he became chair of CGDN's board of governors

linkages [and] on having academics work outside of their traditional environments for interaction' (senior civil servant; NCE-JW: 17).

The attitude of faculty was more ambivalent. The top-down decision to shift priorities represented 'a very serious concern for [some of] the researchers involved' (program officer; NCE-SM: 9) and considerable turnover among scientists occurred. Some found the program more appealing and enlisted; others 'knew this wasn't the place for them [and] got out' (senior civil servant; NCE-MAL: 9). Since Phase II, all networks have conducted more applied and less fundamental research. Reduced budgets for the renewed networks forced the scientists to 'focus much more on... lines of research that were likely to be of interest to industry'. The research still had basic components but was aligned 'to be of greater interest to the existing industrial environment' (senior civil servant; NCE-MAL: 10).

With the election of a Liberal government in October 1993, the emphasis on relevance became even more entrenched. By now 'neoliberal' principles had become a political orthodoxy as even centrist parties shifted to the right. Shortly after assuming office, the Liberals undertook a massive reorganization of ISTC. As if to confirm the subordination of science to the economy, the department now became simply Industry Canada. It assumed a much enlarged portfolio and a mandate to foster Canada's international competitiveness. The following year, 1994, a major science and technology program review was announced, together with the intention of moving towards a new, national science and technology strategy. Months of exhaustive consultation and review followed. After some considerable delay, the new national policy--Science and Technology for the New Century: A Federal Strategy --was finally announced in March 1996 (Industry Canada 1996).

The strategy adopted science and technology as a federal priority. Taking a 'National System of Innovation' (Nelson 1996) approach, it integrated academy, industry, and government research

under the rubric of job creation and economic growth. The focus was on the 'strategic investment' of resources for 'the maximum economic, social, and scientific returns' (Industry Canada 1996: 9). The principal means of achieving this was through the strategic use of public-private research arrangements between universities, industry, and other levels of government.

Both the Conservative and Liberal administrations had crafted a climate hospitable to commercial relevance by applying a multitude of mutually reinforcing policy instruments. Available data indicate their efforts were successful. Industrial support of university research appears to be advancing more rapidly in Canada than elsewhere. Table 1 shows that while the proportion of industry funding for university research has increased in all G7 countries from 1985 to 1996, Canada's share in 1996 is significantly higher than other G7 nations.

Table 1: Share of university research funded by industry (%) in 1996, 1990, and 1985

	1996	1990	1985
Canada	10.4	6.3	4.3
United States	5.8	4.7	3.8
Japan	2.4	2.3	1.5
France	3.3	4.9	1.9
Germany	7.9	7.8	5.9
Italy	4.7	2.4	1.5
United Kingdom	6.2	7.6	5.2

Source (OECD 1998:165)

By separating funding and performance sectors, Table 2 indicates that in 1996 Canadian universities performed a higher percentage of national R&D than other G7 countries, with the exception of Italy.

Table 2: Percentage of R&D Expenditures by Financing and Performing Sectors for the G7 Nations in 1996

	Financing Sector				Performing Sector		
	Domestic Business	Foreign Business	Gov't	Other. Internal	Business	Gov't	Univs
Canada	48.2	12.7	33.7	5.4	62.2	14.9	21.7
US	61.4	0.0	34.6	4.0	72.7	9.8	14.6
Japan	72.3	0.1	20.9	6.7	70.3	10.4	14.5
France	48.3	8.0	42.3	1.3	61.5	20.4	16.8
Germany	60.8	1.9	37.9	0.3	66.3	18.1	15.6
Italy	49.5	4.4	46.2	0.0	57.7	19.9	22.4
UK	48.0	14.3	33.3	4.3	65.5	14.5	18.8

Source (OECD 1998:166)

However, the Canadian business sector remains a low performer, suggesting Canada's industries continue to rely on publicly supported research rather than develop their own infrastructure.

Overall, the new strategy introduced in 1996 produced a reduction in federal funding support for science and technology, especially for the research councils. The NCE program was among the initiatives that would be cut. However, the networks came together, launched a public relations and lobbying campaign, and were successful in reversing the decision (for details see Chapter 5).

As a result, the NCE program was made permanent in the February 1997 budget, albeit with a 'sunset clause'. The purpose was to allow the program 'to continuously reinvent itself through a constant influx of new people and ideas' (senior civil servant; NCE-MAL: 11). The networks least likely to survive without government support would be culled, funding to those deemed to have 'graduated' from the program would be discontinued, and funding for all networks would be capped at a maximum of 14 years. For the surviving original networks therefore, Phase III would be the end of the line. Policymakers did not intend NCEs to become entrenched and institutionalized. They wanted researchers to be instilled, 'from the very beginning with a vision of life after NCE funding' (program officer; NCE-SM: 11-12).

But as I will demonstrate later, this sunset clause may have been a policy error. Especially for networks in the life sciences sector, where a 10 to 12-year gap can separate discovery and final-stage clinical trials, the timing seemed incomprehensible. The change created detrimental amounts of goal displacement among networks in this sector. Instead of focusing on advancing fundamental and translational research, networks facing sunset focused their attention on speculative financial projects in order to replace federal funding.

Part of the intention of the NCE initiative was precisely to generate this kind of cultural change in academic science. The program's biggest achievement, according to one interviewee, has been to establish 'a market orientation in academic researchers and a predisposition for collaborating with the private sector' (program officer; NCE-CA: 6). This included finding and developing receptor capacity in Canadian industry, securing venture capital, negotiating multiparty intellectual property agreements, and establishing an effective process whereby network technologies could be licensed to industrial partners. The numbers of patents filed and inventions disclosed increased significantly.[71] Sophisticated alliances with the financial sector allowed some of the networks to attain experiential knowledge of business and finance that often surpassed that of Directorate staff. They knew what was needed to run their own programs, and felt constrained by the pedestrian advice of NCE officials. Not surprisingly, the networks began to take on a 'life of their own' as they claimed increasing autonomy (program officer; NCE-SM: 11). As one senior civil servant put it:

[71] In some fields, however, patenting and licensing are not the normal routes for technology transfer; dissemination occurs instead through traditional routes, such as training and conference presentations.

> We started to see change where the people who were working in the program had a very strong concept of what it was that they were doing. It wasn't always exactly the same as our concept, but they began to drive the program in certain ways...We [government] still set the agenda, but the level of contribution is much higher from the networks now and I would say that many times now we are learning from them as opposed to them learning from us....we started to see a change from us really driving the program to them taking much more ownership for it and starting to push into new directions (senior civil servant; NCE-JW: 29-30)

The NCE Directorate became somewhat uneasy with the aggressive commercial ethos that developed in some of the networks. They sensed things had gone too far. In its review of one of the life science networks, for example, the Phase III Selection Committee suggested that the network's research program should be 'directed to goals that are appropriate in an academic setting' (NCE-SC 1997). In other words, the network 'should not try to compete in areas of research where major pharmaceutical companies are already investing enormous amounts of money and have a clear research lead and advantage' (NCE-SC 1997). But these Phase III funding proposals were prepared by networks facing the sunset of NCE support. They were required to show how they would handle the transition. It was almost inevitable that they would respond in commercially aggressive ways.

In recent years, many of the networks have formally incorporated to facilitate the management of their extensive research programs, intellectual property portfolios and partnerships. Incorporation was always Industry Canada's preference. They saw formal, legal structure as a means of eliminating the model of collegial governance that had guided academic decision making in the past. But the research councils resisted, preferring to leave the decision up to the individual networks. After initially adopting a 'wait and see' position, most have now incorporated. They have also created arms-length, for-profit corporations that use standard business tools such as mission statements and strategic plans.

A decision to incorporate raises some interesting conceptual issues. A network is a loose association of researchers, nodes, projects, and partners. It *is* the people and entities that make it up. But a

corporate body has legal powers of association and personhood. It exists *apart from* the people and entities that make it up. An incorporated (literally: *embodied*) network seems almost contradictory. Incorporation institutionalizes these 'virtual' entities, cloaking them in substantive legality. The increasing adoption of the corporate form signals the approaching funding sunset for 'mature' networks, and their desire to sustain themselves beyond this horizon.

Summary and Discussion

In Canada, as elsewhere, national policies promote the integration of public-sector research organizations into the economic mainstream: public science must move out of academic and government labs and into the marketplace. Policy goals include the commercialization of research results as proprietary products, and the adoption of new market-friendly institutional arrangements for the conduct of research. Policy tools like intellectual property rights and public/private research networks promote the development of closer academy-industry relations and facilitate what can loosely be called the privatization of the public knowledge base. Yet at the same time as promoting commercial *relevance* these policies also promote scientific *excellence*—a combination that may appear at first appear counterintuitive.

But Canada has a long tradition, stretching back into the 19th century, of state involvement in the promotion of programs that seek both.[72] The National Research Council was founded in 1916, largely to address the needs of industry for research that would advance innovation. At a time when universities were in the business of humanistic scholarship and teaching, rather than the advancement of scientific knowledge, NRC's establishment represented the institutionalization of federal attempts to advance 'useful' research. Over time, however, this intent was subverted as NRC

[72] The first federally supported science initiative was the Geological Survey of Canada, founded in 1841, which laid the basis for the mining industry. In the 1880s federal support of astronomy produced longitudinal maps used in building the railways. The creation of experimental farms patterned

became increasingly focused on conducting fundamental research and promoting the same in universities.

Beginning in the 1960s, attempts at policy reform proposed ways to 'correct' the orientation of federally funded research and scientific cultures and turn public research towards economic development goals. The scientific establishment successfully resisted these attempts until the 1980s, when the neoliberal turn in Canada's political culture established a Strategic Science regime that would harness public science to the needs of the economy. One of these initiatives established the Networks of Centre of Excellence program.

As a hybrid of the National Research Council's Industrial Research Advisor Program and Dr. Fraser Mustard's Canadian Institute of Advanced Research, the NCE program was dedicated to *both* scientific excellence *and* commercial relevance. Because of its novelty and dual commitments, the program was the subject of fierce jurisdictional struggles within the federal bureaucracy as the research funding councils and the ministry responsible for industrial and economic expansion fought for control. In the first phase of the program (early 1990s) the culture of the research councils dominated and scientific excellence was the primary selection criterion. In terms of my conceptual framework, this phase was concerned more with basic research performed under 'open science' conditions in public institutions, or 'Bohr's Quadrant'. In the second phase (mid 1990s), Industry Canada's concerns for commercial relevance came to the fore. As a result, some of the networks entered into market relations more aggressively than had been anticipated. In other words, these networks 'overflowed' in pursuit of applied research for private profit and moved into 'Edison's Quadrant'. After 1997, when the program became permanent, new networks were selected on more balanced criteria and relevance was redefined in social as well as economic terms. 'Pasteur's Quadrant' was the goal.

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after the USA's land grant movement produced innovations suited to a cold climate and large gains in agricultural productivity. Before the end of the

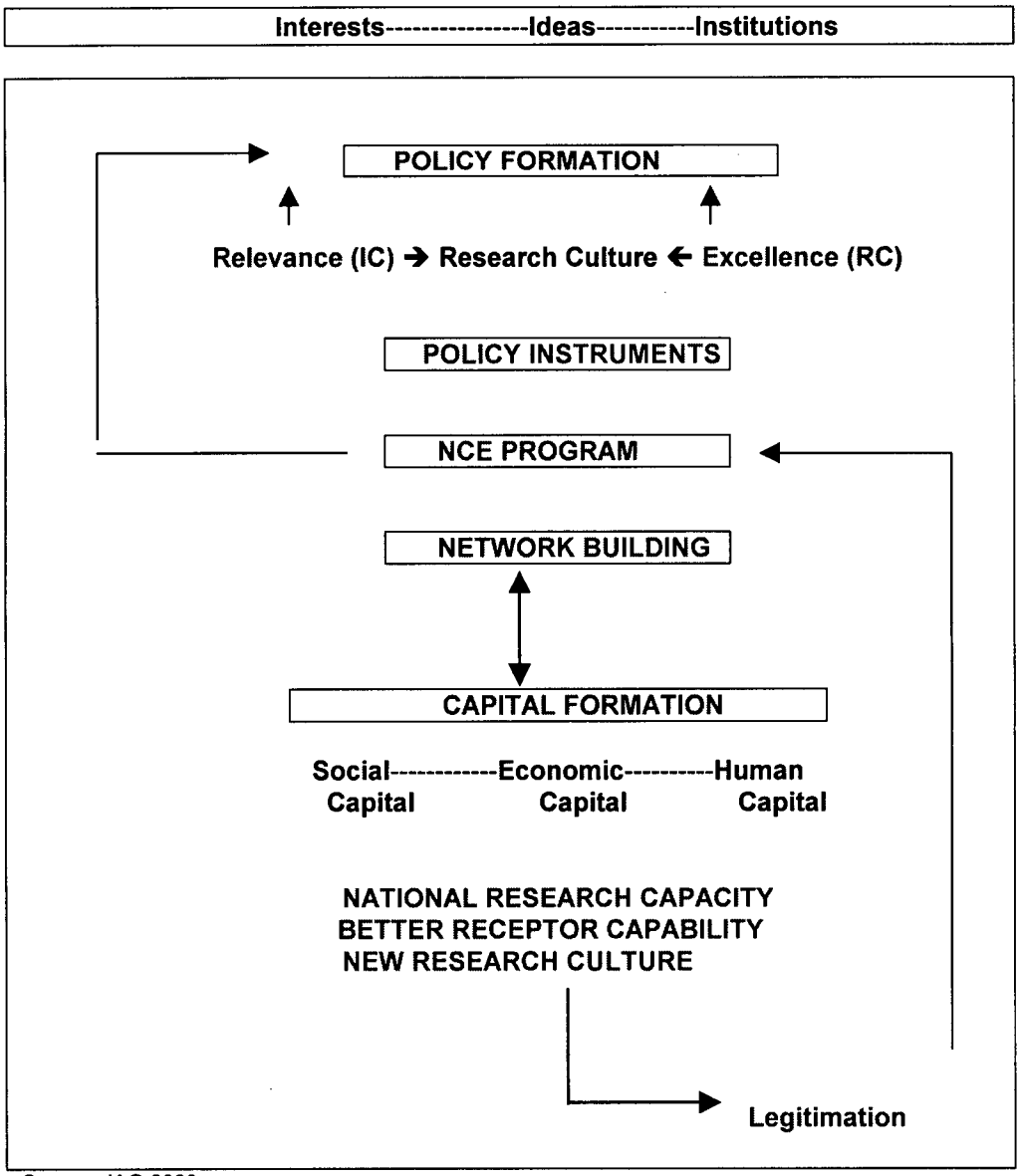
But this goal has been pursued throughout the program's history. Mechanisms have been sought that will couple creation of knowledge and traditional means of diffusion, such as journal articles, with 'translation' of knowledge and new means of diffusion such as technology transfer to industry partners. The two are rife with tension and ways have been sought to reconcile, for example, publication norms with the protection of intellectual property rights. Or, when considering who to recruit into a network, to reconcile traditional criteria of scientific merit with strategic judgements of a research program's commercial relevance. Control of these and other tensions is accomplished within the formal organizational and management structures the program requires networks to adopt.

Overall, the program sought to promote a broad shift in the research culture. Inter-institutional, inter-sectoral, cross-disciplinary, and multi-regional collaborations were favoured in the network selection process. Constructive relations with industry and cost-efficient, even revenue-generating, operations were to be pursued. The extent that these goals were achieved is an empirical question addressed in my case study of the Canadian Genetic Diseases Network.

From the material presented to this point, it is possible to develop a model of Canada's Strategic Science policy regime, and the way the NCE program relates to it (Figure 5). I suggest that this model, with modifications for local conditions, may be generalizable to other countries operating under a similar regime.

19th century, several federal government departments had established national laboratories for the exploitation of natural resources.

Figure 5: Model of Canada's Strategic Science Policy Regime in relation to the NCE program



Source: JAG 2000

The model shows the influence of powerful interests, ideas, and institutions at the agenda-setting stage of policy formation. Once an agenda for commercial *relevance* and scientific *excellence* is mobilized, competing state agencies (in this case: Industry Canada—IC, and the Research Councils—RC) place countervailing pressures on the research culture and attempt to influence the

development of policy instruments that will further their interests. The NCE program is such an instrument. The construction of 'networks of centres of excellence' is intended to promote the formation of human, social, and economic capital, leading to a new national capacity in research, improved receptor capability in industry, and a new research culture. These results would then legitimate such programs and encourage the development of other similar initiatives.

In the next section of the dissertation, I move from the abstractions of policy development to the materiality of the actual practices and relations policy instantiates.

Chapter 4: Configuring the Canadian Genetic Diseases Network

What is 'a network'? Often, we think of something flimsy or ephemeral, like a cobweb, that can easily tear and drift apart, just webs of relationships with nothing visible anchoring them in place.[73] But as translation sociology (ANT) has shown, that is not the case. Networks are anchored in the materiality of the actors that make them up: in the infrastructures actors inhabit; in the resources actors command; in the allies they enrol; and in the artifacts and instruments they employ (or, as is often the case, are employed by). As Callon puts it, networks are 'the very simple counterparts of the spatial and time persistence of actors: to translate is to exist' (in press, fn. 7). Thus actors 'come before' networks and actors 'make' networks; powerful actors make powerful networks. This chapter is about precisely that process.

What follows is the first part of my case study of the Canadian Genetic Diseases Network (CGDN). The chapter is divided into two sections. In the first I examine the way CGDN 'knitted the first few stitches of a web that still did not exist' (Callon in press, fn7) and how it secured itself to the material foundations of universities. My entry point is the individual leadership of the network's Scientific

[73] Used figuratively, the noun 'network' means 'an interconnected chain or system of immaterial things' (OED). Another usage is an 'interconnected group of people; an organization' (OED).

Director. The discussion is then expanded to take in his enrolment of a 'core-set' when setting up the network.[74] Flowing from that, chronologically, is a description of the network's genesis in 1988 and the recruitment of the founding researchers and professional staff. The section ends with a description of the management structure and the formation of an institutional identity, separate from the university. The second section comprises a critical analysis of problems that have emerged from the way the network has been configured. These have to do with issues of regional distribution, élitism and equity, social reflexivity, and public accountability.

I. Power of One

To succeed within the parameters stipulated by the NCE program, member networks seem to require a strong, even visionary scientific leader; someone who perceives the program as a means 'to animate their vision and execute it' (manager, PS-DS-23). According to ANT, the most powerful actors—those who assume a network's leadership and become its *spokesperson*—are those who enrol the largest number of allies. Spokespersons actually *create* the groups they speak for, by the very act of speaking (Cambrosio, et al. 1990:214).

Generative leadership of this type was common to all the networks created in Phase I, but particularly those in the life sciences. Strong leadership is consistent with the culture of molecular biology, where the laboratory leader focuses all the resources and recognition of the lab, and represents the entity as a whole to the lab's various communities. The leader functions 'as a symbol of the lab, as the lab's information interface, its 'provider', and as the one who plays the games of the field' (Knorr-Cetina 1999:254). In CGDN, that spokesperson was Scientific Director Michael Hayden. His vision, communicated in a January 1991 essay entitled 'Science and Dreams', was

[74] To use 'enrolment' and 'core-set' in the same sentence is to mix metaphors from two branches of science studies: ANT and SSK respectively. I will

> to create a functionally integrated but spatially dispersed intellectual consortium...to open new pathways for collaboration and networking while breaking down the old style, conventional, departmental and institutional barriers. This is not business as usual (CGDN SCAN-1: 2)

All interviewees agreed[75] that Hayden was the person most responsible for the network's initial success and that he remains its biggest influence. He conceived the network, envisioned its framework, and personally enrolled most of the researchers and staff. He is often characterized as 'a network in himself' (board member, B-MP-23) in that it is his contacts and force of personality that stamp the network's style as entrepreneurial and fast-moving. For a former NCE program officer 'Hayden is unrivaled as a scientific leader. He was the right person in the right place. He was certainly the most effective of the scientific leaders I observed' (NCE-PO-MAL). Hayden appears to command the loyalty and respect, even affection, of colleagues.

> He has actually made this one of the most, if not the most, successful networks out of all those centres of excellence that were set up. (Researcher, BR-9-10)

> It's very strongly led by Michael Hayden. He has maintained the leadership through the whole time. He's certainly done an excellent job. I think it's very much his baby. (Researcher, DC-7)

> Simply put, Michael Hayden is a wonderful, wonderful, network leader. He always has been, right from the beginning. He's a rare combination--a person that's guided by principle but tremendously goal oriented. He knows what he wants to accomplish and he is tenacious. He won't let go of an objective he believes in, and he believes in the network. (Senior executive, PS-DS-6)

Hayden's leadership style is characteristic of the traditional command-and-control ('Mode 1') model of academic science, in which senior scientists exercise almost total control of their eponymously named laboratories. This is the milieu in which the current generation of researchers was socialized. So it is not surprising that Hayden runs the network, in the words of a recent recruit, as 'a

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avoid engaging in the underlying theoretical disputes.

[75] In many cases the opinion was volunteered, rather than prompted

benevolent dictatorship', nor that everybody seems to accept autocracy as the natural order. As the recruit puts it,

> this is not a democracy; one cannot run a network like this like a democracy. Michael Hayden makes most of the decisions. He has the best background. He's the best choice. So it runs quite smoothly (MW-34-5).

An external observer notes that Hayden provides strong scientific leadership, but that his style is less collaborative and consultative than some. 'Hayden sets scientific directions by force of personality although he seems to do so without ruffling too many feathers. Not necessarily bad, but different from the other two networks I think' (HC, personal correspondence). One of the NCE program officers—all of whom are scientists themselves—explains it this way.

> It's not really a dictatorship. You have to understand the scientific community that you're dealing with...It's a highly educated population. A highly critical, opinionated population. We are trained to be very critical of each others' work. So when you're dealing with that sort of culture it requires very strong leadership. Others might equate it to dictatorship but it is not. You have to be able to stand strong against all of the criticism. And so the leaders have to be very strong. And very firm. Because it's not going to work otherwise. (NCE-PO-LD-7)

Hayden's way, explains a senior researcher, is to put his imprint on something and set the strategic direction, then hand it over to professional staff and move on to something else. 'He has the final word, but those people are now so indoctrinated that they run on their own. They don't need to go to him for everything. And it works' (BG-43). A veteran staff member agrees. Hayden makes the decisions and sets direction, she says, but, over the years, 'he backed off and let us do our own thing' (PS-CS-24). A senior science bureaucrat, who was the network's program officer for a number of years, notes that Hayden indeed did less hands-on management than most of the other leaders. 'But when he did intervene,' she says, 'he had vision and a pretty good schtick. He really got things done' (NCE-PO-MAL).

Hayden's willingness to allow the network's professional staff to manage network affairs was, in part, an artifact of the program's design. As described later in the chapter, a major novelty of the NCE program was that network research was conceived as managed research. Given the large amount of funding allocated to each network, and the complexity of linking so many institutions and researchers together, formal management structures were deemed essential. In effect, each network had *two* leaders. One was the scientific director. The other was a network manager who 'made bloody sure they knew what everybody was doing. And kept tabs on everything. Which is very unusual in a science program' (Policy analyst; NCE15-13).

As scientific director, Hayden coordinated and integrated all the research projects and programs. But the network's senior executive officer controlled the spending and monitored the researchers to ensure that all the network's non-scientific mandate points were being met. In accepting the position, says one of these senior staff members, he knew working alongside Hayden would be demanding, but felt confident enough to accept the challenge. 'I knew that I could work with him long enough to work it out. You just have to be strong. He backed me and I backed him, it worked both ways' (PS-DS-22).

Part of Hayden's success as a leader came from his strategic abilities. He knew how to mobilize resources, at the last minute, for the highest impact. For example, the face-to-face aspects of funding applications—expert panel visits, presentations to the NCE selection committee, and so on—were orchestrated to maximum effect. According to informants, every ally, every board member, every industry partner, every network scientist was invited to sit at the table. Everyone gave five-minute presentations on their research and/or role in the network, literally overwhelming panelists with information and enthusiasm for the science.

These funding reviews and site visits were highly polished performances. Everyone was well prepared. The whole effort was timed and scripted, without appearing slick. As the Managing Director describes it, 'everybody was there to back up that this organization was doing its stuff...you can't leave anything to chance, you have to cover all of the bases' (PS-DS-61). Hayden himself, however, relied on staff to set things up, rarely focusing until the very last minute. He caused more than a few anxious moments but people learned to have faith in his ability to deliver the goods. The following anecdote, by the NCE program officer responsible for the network in Phases I and II, provides an example of his eleventh-hour style.

> I never saw anybody like Michael for pulling things off at the last minute. I'd talk to him one day and he'd have to do something the next day and he would be totally disorganized. And I'd expect an utter disaster. And, then the next day I'd see him perform and he always seemed to pull the rabbit out of the hat. Yeah, the lights went on and Mike was there. He'd just put in a terrific performance and really inspire people in the network.
>
> The night before the selection committee meeting [for Phase II] there was a dinner for Michael Smith in recognition of the Nobel Prize. And Michael Hayden was at the dinner and I talked to him and he was really nervous about appearing before the selection committee the next day and all that went along with it. And I thought 'Oh God! He is unprepared. He is going to bomb,' you know?
>
> But when he came in the next day he did a really smart thing. He brought in JG, a private-sector partner, to say what was great about this network from industry's point of view. Hayden was the only person who did that. Everybody else brought in their scientific director and their management person. So his network was unique in that way. And that was exactly the dimension that the committee wanted to hear.
>
> A distinguished member of the selection committee...quite an influential guy.,said 'you know we can't *not* fund this guy. This guy shakes trees.' And I always remember that and it certainly is true. Michael really did have that impact. (NCE-PO-MAL)

Involving so many network members—scientists, board members, and industry partners—in the renewal effort was extremely innovative at the time. Not all networks took such an inclusive

approach. For example, a researcher from another life science network[76] reported having few companions when he attended a renewal panel. The leaders had invited only three or four scientists to present a synopsis of what was happening in that network. 'None of the other scientists was invited; it was only a handful of people' (FT-8). That network subsequently lost funding because, to this researcher, they had failed to engage their scientists in the process. Unlike Michael Hayden, that network's leadership 'essentially excluded all the scientists and then tried to move forward. But of course, they had nothing left. The scientists had abandoned ship' (FT-8). At CGDN, in contrast, 'every one of our scientists was at the review committee meetings. No one was missing unless their mother was dying. There were no excuses. You had to be there' (Manager, PS-DS-14).

Thus the essence of Hayden's scientific leadership was to involve others. Hayden extended that concept of involvement to the wider community. He calls this 'civic science'. When scientists accept public money, he says, they accept a responsibility to the communities that provide those funds. Science and scientists must not be cloistered; they must participate actively in society and be fully accountable. The obligation is not so much to the government, Hayden argues, but to the public at large. In return for the privilege of being funded to practice science, scientists must accept the responsibility of ensuring that that the community understands what they do. He says facilitating this understanding is as important as his work on human health. 'We have a responsibility to reach out to the people who support us...We are *guests* of the public. And so we have a responsibility to acknowledge that they are the source of what we're doing, and why we're doing it. ' (MH1-6-8)

Although 'civic science' sounds high-minded, it seems to have more to do with furthering public *funding* of science, than public *understanding* of science. To use the vocabulary of ANT, when scientists are astute about *enrolling* and *mobilizing* the public as *allies*; when they convey a convincing

[76] The researcher also belonged to CGDN, so was able to compare both networks

message, the public will pressure politicians to maintain or increase funding levels. The cuts to the basic research budget, in the mid-1990s, he says, occurred because scientists 'were not civic enough. And so people didn't place enough priority on it' (MH1-50A). Seeing what was happening to other programs, NCEs 'had to get out there and make sure [network] research was high up on the political agenda. Governments *do* respond to the people, particularly around election time' (MH1-50A).

The reference here is to 1996. As part of the deficit reduction program, the federal government had decided to discontinue NCEs. The winding-up process had begun; no more funding would be forthcoming. In response, the networks, led by CGDN, waged a national public relations campaign to save the program. As a senior network manager explains, 'it took about four months but we won. We won big...We convinced the government that this was a program that they couldn't afford to let die' (PS-DS-29).[77] In other words, through a process of *interessement* government had been persuaded to define their problem in such a way that the NCE program was the solution: the *obligatory passage point* for Strategic Science.

Since then, according to Hayden, network scientists have been 'tremendously civic'; in every part of the country, 'they are out there talking to the wider community' (MH2-26). Perhaps partly as a result of the mobilization of public sentiment in this way, scientific research recovered its place on the policy agenda. As the deficits turned into surpluses, former funding levels began to be restored, then equalled, then exceeded[78]. Research funding was back on the federal 'radar screen': a major priority item in the budget for four consecutive years (1998-2001). Powerful advocacy coalitions (Sabatier 1988) mobilized to lobby for NCEs. Program funding almost doubled between 1997 and 1999, from approximately $40 million a year at the end of Phase II, to $78 million a year in the 1999 budget.

Civic science can thus be seen as a rhetorical strategy that aligns scientists' self-interest with the public interest by enrolling the public as allies in the network. 'By doing it,' says Hayden, 'we ensure our future' (MH1-6). While mobilizing public support for science funding is a legitimate activity, some observers find something slightly 'slick' about the way Hayden packages it. One critic, a senior scientist and policy consultant, says, 'Mike Hayden is what I would call an *operator.* I do not mean this in a terribly critical way. It is just the sort of person that he is' (HC-1). Another senior scientist criticizes Hayden's ability to present genetics as the solution to a host of medical problems, thereby diverting attention from the complex 'web of causation' in disease of which genetics is but a minor part (OTH-B37).

Hayden has extended his personal network and entrenched his leadership role over the decade of the network's existence. Like 'Pasteur' (Latour 1988), 'Hayden' has become the authorized spokesperson for legions of molecules, machines, and tests; patients, doctors, and researchers; founder populations; government funders; disease foundations; and pharmaceutical interests. By interesting and enrolling powerful allies and mobilizing the rhetoric of medical genetics in the public arena, Hayden's science has become a political practice, a science of *associations,* what ANT calls 'politics by other means' (Latour 1988:40).

To understand 'Hayden' and 'CGDN' as consolidated complexes of linkages, it is helpful to map the beginnings of the network, before any taken-for-granted relationships were stabilized. In the early days, Hayden reached out to senior colleagues to help build the network. He was enrolling an élite nucleus of allies, a core set.

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[77] As discussed earlier, however, there was a sting in the tail of success. While the program itself was made permanent, individual networks would not be.

[78] For example, within 3 years of its founding in 1998, the Canadian Institutes for Health Research budget was twice that of the MRC it had replaced

Enrolling the Core-set

Harry Collins proposed the idea of a 'core-set' in relation to scientific controversies and their outcomes (1981, 1985). He used the term to describe the group of scientists involved in the resolution of any given technical controversy. Membership in the set does not depend on common institutional affiliations or seniority but only on a mutual interest in the outcome. A core-set thus can be understood as a web of interests and associations formed by people of disparate linkages and alliances. Because of its descriptive generality, the term has relevance outside controversy studies. Following Michael & Birke (1994), I combine it with ANT's concept of enrolment.

In January 1988, when Prime Minister Mulroney announced funding for something called the NCE program, Hayden, then a young Associate Professor at the University of British Columbia, immediately saw the potential for a genetics network. As a relatively junior researcher, however, he would need to enrol established members of the genetics community if a proposal was to succeed. He telephoned the two top medical geneticists in Canada: Charles Scriver (an expert on Tay-Sachs and PKU) at McGill University and Ron Worton (discoverer of the Duchenne Muscular Dystrophy gene) then at the University of Toronto's Hospital for Sick Children ('Sick Kids'). Hayden knew neither man personally—they had not worked together at all previously—but he knew their work and he knew their stature. He told them 'you know, we've got an opportunity here for a network in the genetic basis of human disease'. Worton had been thinking along similar lines himself and was willing to work on it with Hayden. Scriver was more circumspect. Hayden says 'I was really young back then and Charles was like the Father of Genetics. Why would he care? And why would he trust me enough to work with me on this?'

Scriver was an essential ally for several reasons beyond his scientific seniority. First, he had helped found a well-known program called the Quebec Network of Genetic Medicine, twenty years earlier,

in 1969. That network ran a screening program for newborns and a distributed system of centres providing diagnostic follow-up, genetic counseling, and treatment. The group had recently published an article in Science's first theme issue on how science could contribute to societal initiatives and concerns. Scriver suggests that within this context the network's name and structure attracted Hayden's interest. Second, Scriver had research projects funded under Quebec's Programme d'action structurantes. That provincial program, formed in the early 1982, appears to have been one of the prototypes of the federal NCE program, formed in 1988. Like NCEs, Action Structurantes projects had to be performed by a team of investigators. While industry partnerships were not required, they had to be multi-university and multi-disciplinary.

Scriver was coming to Vancouver the following week on a personal matter. Hayden arranged a meeting. The two researchers, separated in age by a generation, sat on the steps of Vancouver Art Gallery, in the chilly middle of February, going over the issues. Hayden summarized the federal announcement and pointed out the similarities with what Scriver had built in Quebec. He remembers talking about pulling together the 'best of the best' across the country, in the same way that Scriver had pulled together the 'best of the best' in Quebec. He talked about the millions of dollars being made available for research. Finally, he asked Scriver whether he would join in and Scriver agreed. Hayden calls it 'a pivotal conversation'. Scriver says of his recruitment,

> I think Michael recognized an interesting opportunity when he saw it, which has been his trademark all along. He was aware of what we had been doing in Quebec with bringing academic genetics to a societal interface, and he thought that would make an NCE proposal look good

All three had their own personal networks of colleagues and contacts and technical capacities, and these quickly combined and multiplied the way networks do, sparking from node to node. Hayden, Scriver and Worton were thus the embodied 'centres of excellence' from which the network originally sprang, and they continue to lead the network today. Senior members of the network

called them 'the triumvirate'. Beyond these three founders was the élite group of scientists they enrolled to craft the initial letter of intent and subsequent proposal. Worton recruited two people from 'Sick Kids'--Lap Chee Tsui and Rod McInnes, while Scriver brought in Roy Gravel and Emil Skamene from McGill. Together with Hayden, that made a core set of seven. This 'group of seven' met in a Toronto hotel room for a day and a half to brainstorm ideas. But that first session was followed by a long hiatus as they waited for the government to specify what was expected in the letters of intent. Ron Worton takes up the story.

> The next thing I remember is that I'd planned a three-week holiday for that summer and I'd just bought a cottage the fall before. So this was my first summer in my new cottage. I had never had a three-week holiday before. This was going to be my first lengthy vacation. And I'd been there about a week and a half and I got a call from Michael and he said he'd just heard that NSERC--the leaders of this program at the time—were doing a cross-Canada tour talking about the network model and how to apply and so on. The tour would be in Toronto the following week.
>
> That ended my three-week holiday. I went back to Toronto, listened to the presentation and took notes and called Michael and two weeks later I was with him in Vancouver. I guess we spent the best part of that summer putting together the letter of intent...and then...in the fall, it had to go very fast...We only had six weeks between notification of the success of the letter of intent and the requirement for the proposal.

The core-set identified and enrolled people in other universities and hospitals, expanding in multiples from the original group of seven, to fourteen, and then to twenty-one for the formal proposal. Roy Gravel remembers recruiting people into the program during the summer of 1988. 'I recall there was a meeting in Toronto, the Genetics Society or something of this sort, that was North America wide. It brought a lot of these people into the city. But that was very close to the deadline. We already had most of the people identified by that point'.

One of the most novel aspects of the NCE initiative, one that caught the attention of scientists, was that research was to be extended across Canada in lateral, east-west interactions. This was not the

traditional way Canadian science had been organized. Few national forums brought Canadian scientists together. Most connections and collaborations were north/south. Canadian scientists tended to meet each other, if at all, at conferences in the United States. As a result, apart from those recruited from the same institution, people came into the network as strangers, but with a new basis for interaction, which was the network itself. As one scientist explains,

> I didn't know who Michael Hayden was and I didn't know many of the scientists who subsequently became involved. It wasn't so much that people stayed on one side of the continent or another. It was just harder to find people throughout Canada. So this network idea became interesting very quickly, because we met new people doing collaterally related things. RG-5

The recruitment process was quite divisive, however, as will be discussed shortly. The rights and wrongs of who was, and was not, invited to join are still being debated. Four levels of investigator were specified in the proposal. Six of the original 'group of seven' were designated principal investigators (PIs)— individuals with 'established international reputations' in the field of molecular and/or human genetics. All men, three of the six PIs were based at Sick Kids; two were from McGill, while Hayden was the sole representative from the West. The seven scientists at the next level were designated research associates. These four women and three men (one from the original core-set) were individuals with 'established reputations' in human genetics, many of whom were shifting their research program to the molecular level. Of the seven, four were based at Sick Kids, one at McGill, while two represented the prairies. Hayden was still the sole representative of UBC, the headquarters institution.

A third level was called young investigators: All men, these three young Canadian scientists—one each from the universities of Ottawa, Montréal, and British Columbia—were said to have demonstrated 'outstanding creativity' in the early stages of their career. The significance of the final level—core facilities directors—was immediately understood by Hayden, but perhaps not by the

others. Directed by four men and one woman, the core facilities quickly became the key to the network's success. In fact, the core facilities came to define what it meant to do 'network science'—they were true 'collaboratories' (Finholt and Olson 1997; Wulf 1993). As will be explained later, core facilities had both cognitive (human) and material (non-human) elements. They were a combination of the directors' technical expertise and interventions, and the material equipment and instrumentation. Because Hayden realized the importance of these advanced technologies, directors of three of the five core facilities specified in the proposal were based at UBC.

In all, the 21 scientists listed as network members in the 1988 funding proposal represented eight universities and five associated hospitals and/or research institutes:[79] University of British Columbia (including the University Hospital and the Biotechnology Research Centre); University of Calgary; University of Toronto (including the Hospital for Sick Children); McGill University; University of Montréal (including Hôpital de Ste. Justine); University of Ottawa (including Children's Hospital of Eastern Ontario); Queen's University; and the University of Manitoba. Figure 6 below summarizes the investigators by level, their institutions and locations, as well as their research interests. (See also Figure 8, later, for comparison with Phase III).

[79] All the hospitals/institutes are associated with universities but some are more autonomous than others.

Figure 6: CGDN Investigators, listed in 1988 Proposal for Phase I of NCE Program

Name	Institution	City	Research interests
PRINCIPAL INVESTIGATORS			
Gravel[c]	HSC/UT	Toronto	inherited biochemical disorders including Tay Sachs
Hayden	UBC	Vancouver	late onset genetic disorders including Huntington
Scriver*	McGill	Montreal	physiological genetics and human genetic variation
Skamene	McGill	Montreal	genetic susceptibility to disease
Tsui*	HSC/UT	Toronto	cystic fibrosis and gene regulation
Worton*[a]	HSC/UT	Toronto	Duchenne Muscular Dystrophy and genome structure/function
RESEARCH ASSOCIATES			
Cox[b]	HSC/UT	Toronto	antitrypsin deficiency and human genetic variations
Field[e]	UC	Calgary	genetics of multifactorial disease including diabetes
Gallie	HSC/UT	Toronto	Retino Blastoma and other genetic malignancies
Greenberg[1]	UManitoba	Winnipeg	hypophosphatasia
McInnes	HSC/UT	Toronto	genetic diseases of the retina and inherited biochemical disorders
Morgan*	McGill	Montreal	complex phenotypes and population genetics
Robinson	HSC/UT	Toronto	lacticacidemias
YOUNG INVESTIGATORS			
Goodfellow[3]	UBC	Vancouver	multiple endocrine neoplasia
Korneluk	CHEO/UO	Ottawa	myotonic dystrophy
Mitchell	HSJ/UM	Montreal	inherited biochemical disorders
CORE FACILITIES DIRECTORS			
Aebersold[2]	UBC	Vancouver	Protein Analysis and Sequencing
Duncan[1]	Queens	Kingston	In situ gene mapping
Jirik[d]	UBC	Vancouver	Transgenic Mice and Gene Targeting
Lea[3]	UT	Toronto	Hybridoma technology
Lee[4]	UBC	Vancouver	Electron microscopy

1: Not renewed 1996

2: Resigned 1994

3: Resigned 1992

*: Also core facilities directors

(a) relocated to University of Ottawa, Childrens' Hospital of Eastern Ontario, 1996

(b) relocated to University of Alberta, Edmongton, 1996

(c) relocated to University of Calgary, 1999

(d) relocated to University of Calgary, 2000

(e) relocated to University of British Columbia, 2001

The last few days before the submission deadline for the proposal were especially intense. In the words of Ron Worton, it was 'an enormous effort'.

> I flew with my secretary to Vancouver for the last six days or so before the proposal was due, because it was too awkward to try to manage it from two cities. And this was the early days of computers, they were fairly crude at that time. Their memories were small. But Excel had just become available...So, we went out and bought that program a couple of days before I flew to Vancouver. My secretary was reading the Excel manual on the airplane so that when we got to Vancouver, she could do all the spreadsheet work to put the budgets together.

With everyone working around the clock the proposal was submitted on time, November 30 1988, under the title Genetic Basis for Human Disease: Innovations for Health Care (CGDN-FP 1988). It was one of some 158 formal proposals submitted in response to the original call. The leadership issue had been decided by then. Hayden would be Director and, by virtue of that fact, UBC would host the network's administrative offices. Worton and Scriver were listed as Co-Directors.

After the excitement subsided, everyone went back to their labs while the process worked its way through the bureaucracy. Given the intense activity of 1988, the hiatus was something of an anticlimax. It took almost a year before the successful networks were announced (see Chapter 4 for a description of activities at the federal level in the intervening months). Then, on October 26 1989, the 15 networks were notified of their awards. The genetics network would receive $17.5 million over four years.[80]. Asked why he thought the CGDN proposal succeeded, one of the founders responded

> The NCE review committees looked at our science, first. That's your ticket to get in. Once you've accomplished that, you also have to demonstrate that you have a different outlook within the network than in the basic science system. So, the balance I thought was good. We had the breadth of everything. RG-22

[80] Because of delays, the first phase was actually only a little over three calendar years, although it spanned four fiscal years. The fiscal year ends on March 31st.

For the new networks, the nine months following the announcement of Phase I awards—the gestation period from November 1989 through July 1990—were chaotic, as federal bureaucrats struggled to put administrative structures in place. The first tranche of funding was not advanced until August 1990, more than three years after the program was first announced as part of the April 1987 InnovAction strategy, and 30 months after the funding commitment was made in January 1988. The delays indicate the novelty of the program. Federal systems to implement and manage it had to be developed *de novo*. In the selection process, most of the attention had been paid to scientific excellence. In the implementation process, consideration had to be given to the other criteria: linkages and networking; relevance to future industrial competitiveness; and administrative and management capability. These non-scientific elements constituted a large part of the program's novelty. Taken together, they meant NCEs would function as 'research economies' with proper management and governance.

These elements would be covered by a 'memorandum of understanding' as it was then called, an internal agreement governing each network's formal 'powers of association'[81]--its management and governance structure, and its public- and private-sector partnerships. CGDN's first internal agreement was signed on July 4 1990. Two industry partners were signatories—MDS Health Group Limited and Merck Frosst Canada Inc—as well as the 13 institutional partners referred to earlier.[82]

Once the formal agreement was in place, funding was released and the network could seek staff to fulfil the non-scientific criteria. The dynamics of network formation came into play here too. The network's administrative manager was recruited from industry partner Merck Frosst's research planning division in Montréal. She set up the initial systems. Then Dr. David Shindler—a leading science policy advisor—was identified by one of the network researchers as a 'person of interest'.

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[81] Note that legal powers resided with the host universities.

He was recruited from Canada's science secretariat in London as the network's Managing Director. With the two key employees in place, the network's administrative centre was opened at UBC in September 1990.

II. Managing the Network

The history of the NCE program has been described as 'the evolution from free research to managed research to industrial participation' (policy advisor, NCE-MB: 13). The NCE directorate believed that management expertise and governance could 'make or break the networks' and was 'as important as the excellence [of the science]' (program officer; NCE-SD: 3). Management would be one of the key features that distinguished networks from academic science-as-usual. As stated earlier, the NCE program was conceived as large-scale managed research. For a former program officer, now a policy advisor, 'this was 'a major novelty [and] a shock to many; perhaps it [was] the first culture shock' (NCE-MB: 9). But an Industry Canada bureaucrat views NCEs as simply 'slightly more managed or administered' than is usually the case in academic science; managers simply looked after the paperwork, knocked on doors looking for partners, or otherwise freed researchers from tasks that diminished their productivity (NCE-DH: 14). The two interpretations: 'culture shock' and 'normal practice' reflect the cultural differences between the program's governing agencies: the research councils, on the one hand, and the Ministry, on the other.

Stipulations were put in place that all networks would have a board of directors, a scientific committee to organize the research program, and a management team. Network boards and committees were to be structured to bring the expertise of industrial partners to bear on research

[82] Where researchers worked in university hospitals, both the university and the hospital were named as network partners, making the network appear more extensive than it was. .

management. This industrial representation took time to achieve, however. In Phase I, CGDN's board was heavily weighted to academics, with UBC's Dean of Medicine as Chair.

The federal decision to restructure the selection criteria for the Phase II competition put the scientific and non-scientific mandates on a par. This decision reflected Industry Canada's concern that, in Phase I, too much emphasis had been placed on research *excellence* and not enough on industrial *relevance.* In other words, unless formal management structures were given equal status, it was far too easy for a network to allow researchers to do 'science-as-usual', that is, to follow serendipitous directions, and do 'more or less what they wished to do' (NCE program officer; CA: 10). The pressure for increased management was also a function of the increasing size of network research programs.

Management brought an overall vision, 'the strategic vision for the whole group, which was unusual in academia' (NCE Program Officer SD: 10). As CGDN interpreted the management mandate, 'some level of cohesion, some level of network identity, some level of management, some level of cooperation' was required (senior executive, PS-DS-24). In a distributed network, where people do not necessarily see each other, 'there has to be some [management] glue at the core; if there's no glue there it ain't going to work' (network administrator, PS-CS).

But CGDN scientists were not used to being monitored by managers. At least in Phase I, network funding looked to them like 'just another federal grant; just business as usual' (network administrator, PS-CS-9). It was the task of management to persuade them otherwise--that not only the standard of excellence but *all* of the program's mandate requirements had to be met. Managers made the baselines clear.

> If you fell down on any one of them, you were finished...We had to be pretty tough and it was hard. It was painful. We had to kick people out of the network when the work wasn't up to scratch. When they didn't maintain their science, or they weren't doing it the way we saw it had to be done (PS-DS-24).

This level of control over researchers was possible only because the most senior executives held PhDs. Both the original Managing Director and his successor belonged to the scientific culture and enjoyed peer status with network researchers. Their scientific credentials helped to establish their credibility when enforcing accountability. As members of the culture, they understood the competitive nature of scientific careers. While they reinforced high standards and the orientation to excellence, they also sought to encourage researchers to maintain their science and be acknowledged for it. 'It wasn't just about grants. But to be recognized by their peers for the good work that they were doing' (senior executive, PS-DS-17-18)

The maintenance of standards paid dividends. By adjusting to the program's changing demands, CGDN won a total of 14 years funding in all, the maximum allowable. The network was successful in each competition, being renewed for Phase II, in the mid 1990s, and again for Phase III. After Phase I, much of the hierarchical partitioning of researchers disappeared. In subsequent competitions, all the original associates were reclassified as Principal Investigators. Core facility directors were also listed as principals, reflecting the reality that most ran research programs as well as providing a service to other members. The category of 'junior researcher' disappeared. (Subsequently, promising young researchers were appointed as 'network scholars' on a fixed term.) Network documents[83] show 33 Principal Investigators at the start of Phase II, representing nine universities and four related hospitals/institutes. After the Phase III expansion, the network agreement details 50 Principal Investigatorss at 12 universities and eight related hospitals/institutes.

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[83] See (CGDN-FP 1993; CGDN-NA 1994; CGDN-FP 1997; CGDN-NA 1998). Often documents disagree. For example, the funding proposal will list more partner institutions than the network agreement. When there is a discrepancy, I take the network agreements to be the more reliable source, since these list only formal *signatories*. However, the fact of the matter often lies in between.

The hospitals and universities referred to above were rarely enthusiastic signatories to the network agreements. To them, a network was a problematic organizational entity. Given that Ottawa's original intent was to bypass university autonomy, it was little wonder that conflicts occurred between these reluctant 'hosts' and their unwanted 'guests', as the networks established their institutional identity. As Michael Hayden describes the relationship, 'universities didn't trust the networks. They saw us as a power grab. They saw too much power going to the networks away from the universities. And they didn't trust and didn't understand the process' (MH2-1).

Institutional Friction

Universities and hospitals that house network offices and researchers are called 'host institutions' but their hospitality is largely involuntary. The legal status of 'networks' is an important factor in understanding the host/network relationship. Under corporate law, collectives (e.g. societies or associations) hold certain 'powers of association' not available to members as individuals. Those powers are exercised through the association's officers, professional staff, and governance mechanisms. Legal powers of association, and legal personhood, require incorporation and CGDN did not incorporate until 1998. Until then, in legal terms, it did not exist.[84] As a CGDN manager says, 'these are very fragile organizations; they're built on practically nothing. There is very little holding them together except money' (PS-CS-66). Until 1998, then, CGDN was an 'ephemeral organization' (Lanzara 1983:88) existing only in the interstices of university accounting systems. Its status in relation to the university was highly ambiguous.

Commenting on the network's location on the periphery, Michael Hayden says 'we were federal but we weren't in the mainstream. It was strange' (MH). But from the margins, as 'federal agents', NCEs

[84] This is one reason Industry Canada, in early planning for the program, wanted to insist on incorporation. As described earlier, the Research Councils resisted

were able to mobilize significant *informal* powers of association. In the absence of formal identity, they bound CGDN together with a *willed* identity.

> When you are a network, when you're not incorporated, when you're undefined, when you're an instrument of the university, (the universities consider you their instrument even though you are not.) And when you're trying to do something *in between* everybody else, it's very difficult to establish an identity. And we worked hard to create an identity. (Senior Executive, PS-DS-64-65)

As the networks developed distinct identities two clear sources of friction with host institutions emerged. The first source of friction was the financial costs of hosting networks. Unlike the National Institutes of Health in the United States, Canada has never funded infrastructure costs[85] for medical research and only rarely allows researchers to charge their salaries to research grants. Whenever a new program was established, universities had to cover the additional costs. By any standard, NCE overheads were large and expensive for university budgets to absorb. In effect, these institutions supplied the incubation facilities in which networks could flourish, but received no compensation from the program, or recognition for their contribution. As well, it was a case of 'taxation without representation' since universities had no power to regulate the activities of networks, which were accountable only to Ottawa.

Overall public investment in the program from fiscal 1990 to fiscal 2000 exceeded $650 million (see Table 3). But this figure does not include university infrastructure or the salaries and benefits of university researchers. The NCE Directorate conservatively estimated the latter at approximately $100 million a year in 1996 (NCE Annual Report, 1996-97). Using the growth of the program since 1996 as a base for calculation, the annual salary figure has likely doubled to approximately $200 million a year in 2001. According to one federal informant, by absorbing these costs universities have contributed at least as much as the program itself over the years (NCE-SM: 19).

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[85] Effective July 2001, a white paper was circulating in Ottawa proposing to allocate a standard percentage of research funding to universities for infrastructure

Acknowledging the historical under-reporting of public support for the program, the Director of the NCE program estimated that 'the additional contributions from both the granting councils[86]... and the universities tends to almost triple the total amount' (JCG-11).

In contrast, the private-sector is credited with only $75 million, or approximately 10% of the program's 'official' $730 million cash budget. Even this figure may be overstated due to various reporting anomalies regarding cash contributions that will be discussed later. The same anomalies prevent any reliable estimate of 'in-kind' contributions from industry partners. Without full estimates of cost, it is hard to calculate the program's cost/benefit ratio.

Table 3: Total cash contributions to NCEs, 1990-2000, in C$M (excludes in-kind gifts and overhead support)

Agency	C$	%
NCE Grants	509.5	69.9%
Federal Agencies	27.3	3.7%
Administration/sundry	14.2	1.9%
Sub-total--Federal	**551.0**	**75.6%**
Provincial Agencies	45.8	6.3%
Sub-total--Government	**596.8**	**81.9%**
Universities (direct only)	8.5	1.2%
Other—hospitals and tax-exempt foundations	48.4	6.6%
Sub-total--public supported institutes	**653.7**	**89.7%**
Industry contributions	75.0	10.3%
Total Cash	**728.7**	**100.0%**

Source: compiled from NCE annual reports, 1990-2000

Perhaps understandably, universities resented their expensive and uncontrollable guests and did what they could assert their institutional authority. According to one CGDN informant, the initial reaction was 'the government has forced these damned networks on us...why should we even talk

[86] Comprising prior funding of fundamental research by the research councils, in the form of grants to network researchers for the basic element of

to these network guys? What do they bring to the table?' (senior researcher; RW-35). One way for universities to manage the intruders was through bureaucratic controls. As already stated, prior to incorporation CGDN had no legal capacity to hire employees, make contracts, or receive funds. In all such arrangements the host university acted as surrogate, as if the network were a minor child, incapable of forming intent. CGDN's researchers were employed by their individual hospitals and universities; network staff worked for UBC. When CGDN wanted to hire David Shindler as Managing Director in 1990, UBC refused. Michael Hayden recalls

> He was the guy we wanted [but] the only way to hire him was *not* to hire him but to get him to take a secondment from his current job. We would pay the Ministry of Foreign Affairs and they would pay him. We did that for five years. It took UBC that long to approve the appointment...[to] become more trusting of the networks (MH-2)

The universities wanted the networks brought under university control. As one network manager describes the situation, 'this was about power and greed. They wanted control of our budget. They wanted the ability to claim that the networks came under the universities, so that anything the networks accomplished, could be attributed to the universities. It's more money for them, it's more profile for them. It's a case of *the bigger our basket is, the more of a power base we have*' (PS-CS-31).

CGDN's principals resisted the administrative blocks imposed by the university. While acknowledging that university budgets were inadequate, they saw no reason to accept the blame and pointed to waste and inefficiencies that 'leaner' structures like networks avoid. 'Universities are under funded, but they are over-headed', says one founding member. 'There is too much infrastructure. To lay blame onto the networks for some aspect of it is unfortunate and misplaced' (RG-83). On the contrary, he suggests, universities should recognize the networks as assets.

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their network research.

A second cause of friction between host universities and networks relates to the management of intellectual property (IP) generated by network researchers. Both are involved in what Merges (1996) has described as a process of 'creeping propertization' as discoveries that would otherwise have remained in the public domain, are 'captured' (privatized) as intellectual property then exploited for profit. In this drive to propertize the products of science, NCEs and their host universities compete for profits. Each seeks to depict itself as the most legitimate agent and skilled representative in the drive to turn science towards the market.

Beyond the drive for profit lie several distinct irritants. First, the 'internal agreements'[87] that are supposed to govern intellectual property issues are universally described as 'ugly.' The program directorate is trying to set up a template to simplify these complex and unmanageable documents. In the meantime, the agreements are supposed to clarify relationships and IP ownership issues but they do not. This means that each commercialization deal must be treated on a 'one-off,' case by case basis.

Second, over time networks have become more aggressive about intellectual property. As I describe in some detail later, the networks had fairly limited interest during Phase I because program demands in this regard were modest. Phase II brought increased expectations on the part of the program and a matching response from the networks. Since Phase III, the networks have been looking to IP commercialization to carry them beyond sunset of NCE funding. As one university technology manager comments, 'the networks are really fighting for our intellectual property...the reality is that if they're going to be self-sustaining, they have to insert themselves into the process' (UA-SC-1). Another says, '[these] people are trying to protect their future at our expense' (UBC-CB-2).

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[87] The NCE Directorate requires such agreements. They govern all aspects of the relationships between a network and its university and industry partners

Finally, there is a sectoral disparity among the networks in their ability to deliver commercialization services, and in their approach to technology transfer. According to university technology managers, the information technologies and electronics networks tend to be 'fairly hands off and laissez faire', while the life sciences networks like CGDN tend to be proprietary and centralized. Because life science networks control their boundaries and members, they been able to make themselves 'obligatory passage points' (Callon 1986: 205) for IP protection in a way that university commercialization offices have not.[88] In networks, the processes of *interessement* and *translation* ensure that discoveries with commercial potential are disclosed to the network first.

Industry Liaison Offices (ILOs) in universities argue that NCEs duplicate existing technology transfer infrastructure and add little value in the process. In turn, the networks point out that historically universities had no incentive to pursue commercialization nor any particular interest in doing so. One of the driving forces behind the establishment of the NCE program, they say, was to 'leach out' technologies otherwise languishing in universities.

University technology managers argue that they carry most of the workload for the development of NCE technologies while receiving little credit. 'On any technologies that I've been dealing with NCEs, I would say I've done 80% of the stick handling' (UBC-CB-2). But to a CGDN board member (private sector) university ILOs 'appeared to be uniformly inept or nonexistent or both. The networks were much more competent' (B-MP-6).

In comparison to 'Johnny-come-lately' narrowly focused networks, ILOs depict themselves as deeply experienced and possessing a 'whole university' vision. In contrast, networks hold themselves out as fast-moving, sectoral specialists, moving strategically to secure IP. They depict ILOs as lumbering, bureaucracy-bound generalists, with no industry experience, trying to handle everything

[88] See Nelson & Sampat (2001) and Atkinson-Grosjean & Fisher (1999) for more thorough discussions of institutional constraints on ILOs

from astrophysics to zoology. According to a former CGDN commercial director, ILO staff just don't develop a good understanding of how industry thinks, so they don't really understand how to find market prospects. 'They mean well, and they try hard and they work hard. They often are extremely over-worked for what they get paid. But, you know, we were focused on our own field. And that meant we could specialize' (PS-MargM-13).

Heroic tales are told about the relative competence and ineptitude of the network and ILOs. These myths have entered the collective unconscious and seem to be part of the enculturation process. A classic example is CGDN's Alzheimer's Genes Legend, which was repeated to me, in various forms, by board members, researchers, and professional staff. The discovery of two genes for early-onset Alzheimer's disease was a big find. The university was not willing to move fast enough on protecting the technology so the network took the lead, realizing that 'if we didn't patent it--yesterday!--we'd lose it' (Network Manager; PS-CS-45). The legend describes how the heroic managing director got the genes patented within 48 hours, therefore protecting the technology for Canada. As recounted by the network's associate scientific director, the authorized version goes as follows:

> This was well into the NCE process, by now we're talking about Phase II and we're into about the winter of 1995. The researcher called me one day and said 'you know, we've got the Alzheimer's gene finally. I've gone to the university and they don't think that it's worth patenting. They don't think that it's worth anything. They don't want to follow-up on it. What should I do? Do you think the network would be interested in helping me to patent it?'
>
> So I called the network's managing director in Vancouver five minutes later and said 'you've got to call this guy and talk to him about the patenting. The university is going to be convinced that they need to be involved in the end, but would you take a lead role here and at least make sure that he doesn't go out and publish the stuff before it gets patented?'

> And the managing director said he would do something. That was like 5:00 in the afternoon. Ten o'clock the next morning, he phones me back. He is in Toronto, walking down University Avenue, talking to me on his cell phone. He'd flown in on the red-eye overnight, set-up a meeting with the researcher for that morning, and by mid-afternoon, they were well on their way to developing the patent position and talking about the whole strategy for exploiting this intellectual property. And of course, as soon as he got involved, the university realized that there really was something there that they should be involved in. And in the end, it worked out well for everybody. But, I think that was the first time I had seen the network really play a catalytic role in making something happen. (RW-37)

Ultimately, this initiative resulted in what was, at the time, the largest IP deal in Canadian university history, between Schering Canada Inc. and the University of Toronto in 1997. Schering's initial $9M funded a three-year research program in the development of drugs and technologies to treat and prevent Alzheimer's. Over the long-term, the agreement has a potential value of $34.5M, not including royalties.

Despite the sniping about the relative levels of commercial competence, network researchers work not in 'networks' but in universities and hospitals which pay their salaries, provide their lab space, and pay their overhead and operating costs. Resulting technologies are owned by the institutions. Their ownership of IP is 'cast in stone' and they are not about to cede their interests to the networks. Thus networks and universities *have* to work together or nobody benefits. In game theory terms, it is a classic prisoner's dilemma. Over time, both have made concessions and a truce of sorts has been worked out.

While the chapter so far has described how the network configured a structure and took on an institutional identity, the telling has failed to capture several critical areas. The report of CGDN's configuration is shot through with power relations and exclusionary criteria. These can best be understood as issues relating to the network's spatial-structural dynamics: the larger 'why' questions of regional distribution, élitism and equity, social reflexivity, and fiscal accountability.

III. Spatial-Structural Dynamics

Regional Distribution

As befits a federal program, success in fostering wide *national* distribution of networks and resources is a policy concern. However, the experience of CGDN shows this goal may not be realistic. When the program was being planned, the 'network' component appealed to politicians because it offset the élitism implied by 'excellence'. To a Canadian politician, élitism means geographical concentration. The program was sold to Cabinet 'as an economic development package—a regional economic package. But Cabinet was sold 'a bill of goods' (federal informant, NCE-SS-2).

Despite rhetorical claims of national scope, and significant expansion in Phase III, CGDN's main clusters are still at the three original institutions: Vancouver's University of British Columbia, the University of Toronto's Hospital for Sick Children, and McGill University in Montréal. An examination of research and core facility funding allocated to network PIs shows that these three institutions commanded more than 70% of the network's $33.5 M research budget in the period from 1991 to 2000 inclusive (see Table 4 below). Looking at the provincial distribution of network funding in the same period, PIs in British Columbia received 22%, those in Ontario got 43%, while researchers in Quebec received 27%. The remaining 8% was allocated across all other provinces.

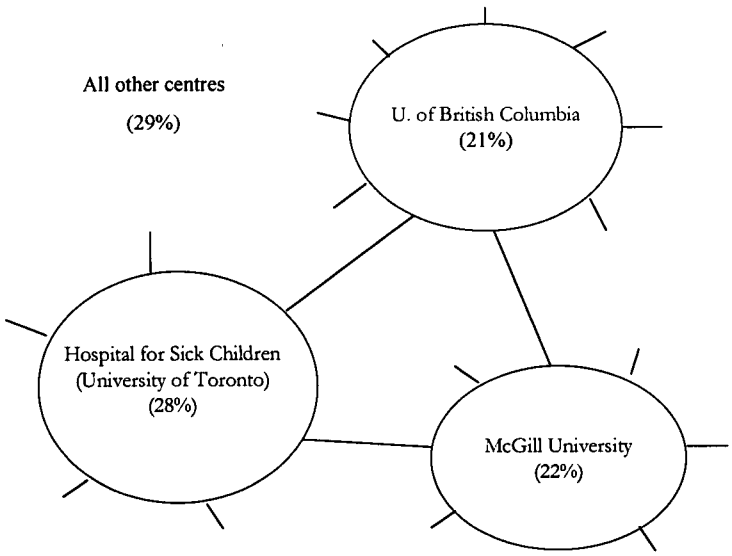
Table 4: Funding Allocations by Institution, 1991 to 2000

Totals	1991-00	%
University of British Columbia Vancouver, BC	7,076,592	21.1%
Hospital for Sick Children (University of Toronto), Toronto, ON	9,486,464	28.3%
McGill University Montréal, PQ	7,343,483	21.9%
All Others	9,590,292	28.6%
	33,496,831	**100.0%**

Source: Compiled from CGDN financial records

These figures indicate that the network is tri-nodal rather than widely distributed. In a sense, the 'network' metaphor is misleading; the dominant image is of 'spokes and hubs' (see Figure 6 below). A Matthew effect (Merton 1968) is at work, favouring those researchers and locations that are already well-established.

Figure 7: Tri-Nodal Distribution of Funding



Actor-network theory relates the density of linkages in particular areas to the activities of spokespersons and their success at *interessement* and enrolment. This is certainly the case. The tacit or embodied aspect—the 'spokesperson factor'--can be clearly seen when established researchers relocate to another university. New clusters begin to form around them, confirming the importance of face-to-face interactions. When Diane Cox relocated from Sick Kids to the University of Alberta in 1996, the university had no network members. Now three PIs are based in Edmonton as well as several associates. In the same year, Ron Worton moved from Sick Kids to Ottawa, where Bob Korneluk was the sole representative of the network. The University of Ottawa now represents a significant node and StemNet, the new NCE directed by Worton, will be headquartered there.

Finally, Leigh Field was for many years the solitary network researcher at the University of Calgary until Roy Gravel moved there from McGill in 1999, followed by Frank Jirik in 2000[89].

Other spatial and structural factors must be accounted for as well, for example proximity effects and institutional context. As Wolfe (2000) points out, economic geographers have long emphasized the significance of space and proximity ('territorialization') in creating the conditions under which resources and tacit forms of knowledge are generated and shared. The phenomenon of regional clustering among researchers, institutions, and firms is well recognized in the literature on industrial districts and regional systems of innovation.[90] As Murdoch (1995:743) notes, 'networks are differentially embedded in particular places and...different forms of organization evolve in different sociocultural contexts.' I suggest that something similar is occurring with CGDN. The combination of inertia and proximity means it is easier to build linkages with researchers in the same or nearby institutions than with those at a distance.

The institutional context is another key factor in facilitating clustering. Again, the Matthew effect is at work. One institution begets more. They layer together to create a regional system for the production and exploitation of knowledge. Amin & Thrift (1994) call this 'institutional thickness'. The network's Toronto node is a good example, with six hospitals and the main university campus within steps of each other. But an internal 'thickness' is also important. Kleinman (1998) has shown that laboratory practices are shaped by the university's formal structure and context. This context defines the 'rules of the game'; for example, how university resources are allocated and who can command them. Some institutions focus more power than others and can assign more resources to particular enterprises, providing a hospitable environment for network activities.

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[89] Field relocated to UBC in 2001

[90] For an authoritative analysis of the former see Lash and Urry (1994); for a Canadian perspective on the latter, see the articles in Holbrook and Wolfe (2000), also Wolfe (2000) in Rubenson & Schuetze (2000)

In summary, if CGDN is indicative, the NCE program supports the institutional *status quo* by directing resources to existing research 'centres' while 'peripheries' remain marginalized. However, the embodied nature of knowledge is such that if smaller universities can find the means to attract network researchers and their programs, these people become agents of change that attract others.

Élitism: Norms of Equity and Exclusion

The concept of centres and peripheries is closely linked to that of inclusion and exclusion. Both are cultural oppositions, linked to spatial notions of familiar and strange, presence and absence.[91] In this section, I examine the norms guiding enrolment to discover why some 'strangers' became present and included in the network while others remained absent and excluded.

To the first international peer review committee, who were 'unapologetically elitist', the term *excellence* meant that 'we should pull together world class teams of scientists: the very best people who, with support, could pull the rest forward' (NCE-SS-1). Roy Gravel recalls that excellence was defined as the top five percent of scientists in a field, worldwide. Gravel considered that an odd and arrogant statement, 'because science doesn't work that way. That wouldn't be the way you would identify the cream of Canadian science. And that wasn't a Canadian number...so it had no meaning' (RG-8). Nevertheless, given the 'excellence' requirement, the biggest challenge in putting the proposal together was choosing the people.

The core-set had to ensure program requirements (for example, geographic distribution) were satisfied, while covering the domains of science that interested them--human genetics, medical genetics; and key technologies. But the program's preference for Mode-2-type interdisciplinarity was

[91] An expanded analysis of these concepts can be found in Rob Shields'(1992) examination of Simmel's (1950) notion of 'the stranger'

largely ignored. Early in the planning, they decided 'that this would be a network of molecular geneticists. And so anybody who was doing cybergenetics, or biochemical genetics or any other type of genetics were automatically excluded in order to keep it focused' (RW-19). This network would operate almost entirely within traditional disciplinary bounds.

Beyond that, a degree of arbitrariness, capriciousness, surrounded debates about who the core-set did and did not want to work with. Perhaps this was inevitable given the need to select only a couple of dozen people from across the country. However, in designating a handful of people as superior scientists, 'excellent' enough to be in the network, they left an implication that those excluded were somehow inferior. The process left a legacy of ill-feeling. Lap Chee Tsui still regrets the élitist direction. 'In retrospect,' he says, 'I think we should have included everyone. The whole community is very small. and in the end about 75% became a part of the network. So there was a small number of people who did not get in. I just felt it wasn't really necessary to go through the agony when the numbers were so small' (LCT). As Ron Worton describes the process,

> The biggest challenge was not in determining who we should choose, but who we should not choose. We made that determination with difficulty, and somewhat arbitrarily. There were some pretty good scientists in the country that we excluded....For whatever reason. Maybe we felt their publication rate wasn't high enough, or they weren't well enough known, or we didn't like the way they did their science, so we excluded them. And in the early days I got phone calls from some of my friends who said 'I'm really angry that you guys did not include me in the network. Why did you not include me?' And when you're asked a question like that, it's almost impossible to answer. It's about standards and focus really. RW-20

The network is a kind of élite club, where membership is increased by invitation only. The inner circle—the priorities and planning committee—'sits around the table...and throws names on the table and discusses them' (RW-20). Often, names are put forward by other members, but even with those *bona fides* not all are selected to come in. Few outside the inner circle understand the selection process. Worton says merely that they try to identify people whose research looks really interesting and is complementary to the existing research program.

One member, a junior researcher back in 1988, thought the decision to include him in the network was circumstantial. He had trained at Sick Kids under Roy Gravel and was located in Ottawa, which gave the network an opportunity to add a node beyond the Vancouver, Montreal, Toronto triangle. He says, 'they tried to cover all possible aspects. Scientists in different parts of the country. Scientists that were young and scientists with a lot of experience. So when they went down the list, I guess I ended up [included]' (RK-2). He recalls that Mike Hayden used to joke that they needed at least one person in Ottawa to deliver the funding proposals.

For a similar reason—to get wider geographical representation—Hayden contacted a researcher at the University of Manitoba who would represent genetics researchers in the prairies. Another, at the University of Calgary, self-selected: 'I heard they were doing this and I wrote them a letter, I guess it was to Mike Hayden, and said I'd like to be part of it. And he said "well send me your CV" and I did and they invited me in' (LF-2). One person from the group at Toronto's Sick Kids remembers 'it was initially extremely exclusive. And then it widened out a little bit to include those people who had a particularly high ranking in MRC and I was one of those' (DC-9).

In a recent Nature opinion piece, a molecular biologist and a zoologist argued that the life sciences are in danger of losing their originality (Lawrence and Locke 1997). The authors perceived an homogenization of opinion, with fewer independent schools of scientists finding novel approaches to problem solving. Scientists are 'playing safe' by following established lines of inquiry, rather than taking intellectual risks. The authors believe this situation is perpetuated, in part, by the dominance of 'star' scientists at conferences and in the literature, and in the inherent conservatism of the peer review process. In other words, as argued in Chapter 4, by limiting selection to élite scientists, these networks tend to limit the variety that feeds more risky innovation-led research.

Another anomaly relates to gender. Women PIs say that the role of female scientists in the network has always been equivocal.[92] Only five of the original twenty-one members were women, and all the original PIs were men. Of the total research and core facility funding allocated in the period 1991 through 2000, women researchers received 11 percent rather than a proportional 24 percent. The two founder members who were not renewed, in 1996, were both women. As one of the five female founders points out, 'some very senior women scientists were not in the network, at all. They were not invited' (DC-38). The proportion of women has increased slightly over the years. In the Phase III proposal, submitted in 1997, eight were listed as members. In 2001, one of three new PIs was a woman as well as three of five new junior researchers called 'network scholars'.

All five women founders were interviewed and all made some reference to gender issues. Mostly, they saw the problem as systemic rather than specific to the network but expressed a degree of exasperation at the general lack of concern shown by the network's male core set. More than a frustration with gross numbers was the fact that women were not represented in the power positions. As one says

> It was very strongly male dominated. And we [women] have had little involvement in [running] the organization. I'm not even sure that [the men] notice, particularly. The women used to joke about it. But there's a problem that way in our field, in Canada, in general. There's a core of people who are very supportive of each other, in and out of the network. And it's very difficult because you're not a 'buddy' of the guys. I'm not suggesting it is a major complaint or anything, but it's simply a fact . I think it's better now for the younger investigators in the network...but the senior women are scarce. DC 38-40

The network made no serious effort to attract females, says another, 'even though there is a lower percentage of women in the network than is generally the case in human genetics in North America.' (LF-32). A recent report by the National Science Foundation tends to support this assertion: unlike in the physical sciences, about half the doctorates in biology are awarded to women. Even in the

[92] Knorr-Cetins (1999) found the same in her ethnography of a molecular biology lab.

1980s, one in three biology doctorates was awarded to a woman.[93] This researcher also finds it curious that all of the individuals dropped from the network have been women. One of those former PIs explains that it is simply much harder for a woman to succeed in medicine and science. 'The nature of the [science] system is that it's run by men. If women ran the system it would be very different. So there is no question there is a sexist component to it. It is just because men make the rules' (CG 15).

The women find the élitist 'invitation only' approach particularly troubling, and complain of a lack of transparency in the selection process. They can find no logical explanation for who is 'in' and who is 'out' of either gender. Names have been proposed, but to little effect. 'I don't exactly know what happened to those suggestions, but apparently they were looked at by the [leaders] who decided not to invite them' (LF-3). Excluding people placed a question mark over their career, especially as the network grew in academic prestige. 'People began to wonder...why didn't they invite me, you know? It's the coalition of top geneticists in Canada, well why haven't they invited me?' (woman manager, PS-CS-81). When the network was starting out, 'if you were left out, it didn't matter too much. But...the bigger the network got, the worse it was to be left out' (woman PI, DC50). Certainly several well-known Canadian geneticists have been excluded. One says 'it gave the impression [then], and probably still does today, of being a kind of an élitist club, and one in which I didn't belong' (CG-13).

With the exception of Lap-Chee Tsui visible minorities are also notable by their absence. The network's board, its scientific and professional leadership, and principal investigators are uniformly white. Whether or not this reflects the field of medical genetics as a whole, the homogeneity of race and gender perhaps indicates a profound social, if not scientific, conservatism at the heart of this

[93] reported in Chronicle of Higher Education, 23.02.01

network. This conservatism is also reflected in the absence of social reflexivity and public accountability.

Accountability as Social Reflexivity

According to Gibbons et al. (1994), one of the defining elements of new network forms of organization is their social reflexivity. Rather than being accountable to the community of science, these networks are accountable to the community at large. It is a pluralist framework, where the pushes and pulls of the agendas of relevant social actors condition the decisions and policies that emerge. Thus, argue Gibbons and colleagues, public interest groups, lawyers, social scientists, as well as natural scientists, have a voice in the governance of Mode 2 networks and, more controversially, in the composition of research teams. This broad representation is deemed essential because of the risks and issues inherent in contemporary science and technology. Similarly, Callon (1999) has noted the emergence of 'knowledge co-production' models in which patient groups establish themselves as 'partner associations' with research groups, and establish parity between lay and expert knowledges of the disease process.

Bruno Latour also emphasizes the social accountability and reflexivity of 'new' network formations. He argues that in a culture of 'open science', where autonomy is sacrosanct, there is no direct connection between scientific results and the larger societal context. But in the type of culture Callon describes as 'overflowing networks' there is a *new deal* with society—a type of collective experiment in which science and society are mutually entangled for mutual benefit. He concludes that 'scientists now have the choice of maintaining a 19th century ideal of science or elaborating—with all of us—an ideal of research better adjusted to the collective experiment on which we are all embarked' (1998:209).

Recently, Nowotny, Scott and Gibbons (2001: 258-9) extended the reflexive elements of their original Mode-2 formulation even further, arguing that scientific knowledge must be 'socially robust' as well as conventionally 'reliable'. Whereas reliable knowledge has traditionally been produced in cohesive and restricted scientific communities (Mode-1), social robustness depends on 'sprawling socio-scientific constituencies with open frontiers' (Mode-2). Socially robust knowledge is superior to reliable knowledge, they argue, first, because it has been tested and retested in contexts of application and, second, because it is the 'underdetermined' outcome of 'intensive (and continuous) interaction between results and their interpretation, people and environments, applications and implications' (258). The more open and 'comprehensive' the knowledge community, the more socially robust the knowledge produced. Further,

> public contestation, controversy and conflict...are not to be shunned on grounds of principle. Rather, they are a sign of a healthy body politic and part of the process of democratization...Space has to be made for what people want, what their needs are, and...even contradictory responses and claims (258)

To the extent that the NCE program was apparently seeking to create the type of networks envisioned by Gibbons,[94] Callon, and Latour, presumably with a broad understanding of public accountability, Michael Hayden's notion of civic science seems impoverished. As Irwin (2001) has shown, the construction of the scientific citizen is a far more complex process than Hayden suggests. For Hayden, the sub-text seems to be that the public (non-scientists) are useful when mobilized *en masse* but must otherwise be kept at arm's length, lest their ignorance and/or interests impede the research enterprise. This is a classic example of science/non-science boundary work (Gieryn 1995).

As Wynne (1999) points out, the lay public is often assumed to lack the 'epistemic capacity' required to judge science. One of the network's board members commented, for example, that 'the public is

[94] Again, note the advisory connection between the NCE program and Gibbons and, to a lesser extent, Callon

generally quite ignorant on the subject of genetics. I don't say that with any negative sort of connotations. It is just a fact of the matter. Why would they not be ignorant? It is a very complex science' (B-MM-14). Because of their ignorance of the science, it is assumed the public has nothing to contribute to the network, despite the ethical issues and broad social questions that accompany research in medical genetics.[95] At the same time, the network states 'no satisfactory policies will emerge if public concerns about genetics in health care are not addressed, and if those concerns are not fully and objectively researched' (website; July 2001).

Similar attitudes were found in a study of medical geneticists by Kerr and colleagues.[96] The study showed that these scientists view science as a 'gold standard' that clearly demarcates 'good and value free research from illogical or politically distorted opinion, which they paternally attribute to an undifferentiated lay public' (Glasner 2000:11). More troubling, in giving apparently objective assessments of risks associated with the new genetics, the experts in this study 'simultaneously disguis[ed] the extent of their own social location and vested interests' (ibid.). The demarcation of lay and expert knowledge and interests can be clearly discerned in the following remarks made by a senior network manager (a science PhD) in response to a question about the potential for appointing a lay member to the board.

> I mean what would a lay [board] member do? They would just ask us what *we* were doing. Well we can't explain that. We don't have time. So we try to pick intelligent members who at least understand the field a little bit. Public interest science is just politics. I want to tell you that right now! That's politics and I don't want politics in my network. If somebody has an agenda about organic foods or genetic engineering, I'm not interested. What I am interested in is: are we curing disease? Are we solving a social problem? We're just as capable of looking at the risks and balances as anybody else. But in the end, would you rather have a cure for Alzheimer's or not? Which is better? And people agree that, in the end, finding the cure for Alzheimer's is certainly a greater social good than being in favour or against clinical trials, or animal rights, or whatever.

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[95] For example, the goal of integrating genetic therapy into the health care system is to predict and prevent disease; predictive capacity requires population-wide genetic testing and stratification based on genetic variants, an issue that carries significant social 'baggage' in the form of eugenics.

[96] Kerr, et al. 1997 reported in Glasner (2000:11)

> The fact is that it would have been very disruptive to have grandstanding on the network board. Of any kind. The interests of the organization have to be paramount, not the individual agendas of board members. And if you have a board that has a bunch of people with individual agendas on it--public agendas, private agendas, political agendas--then you are going to have a dysfunctional board and a dysfunctional organization. You are going to lose that cohesiveness that is so important. You're not going to be able to function. Because they are going to block you and then you're not going to be able to carry out your program..
>
> So we had federal program officers on the board; we had foundations, we had industry, we had universities, we had *intelligent* people--medical people, physicians--that were thinking about all of these things. PS-DS-54-6

Apart from the evident paternalism, this network appears compelled to equate the public's legitimate interest in the conduct of the biosciences with anti-science or fringe activities. The reaction is exaggerated: if the public is given a voice, rationality will be lost; when scientific problems arise, we must 'trust the experts' to solve them. Brian Wynne (1999) calls this approach to problem-solving 'deterministic uncertainty', i.e. when problems caused by science are deemed reducible only by the application of *more* science.

Categories of 'lay' and 'expert' are intrinsically problematic and socially constructed.[97] Scientific discourses exert normative influence over the public domain and attempt to 'reshape the world in their image'. Wynne (1999) calls it 'a profoundly unaccountable and unreflexive process'. Recent work in the public understanding of science (PUS) shows that exclusionary discourse underpins much of the public's mistrust of scientific expertise. Barnes and Edge (1982:237) suggest that 'the tragedy of expertise' is its ultimate contingency.

In a high-trust, high-risk area like medical genetics, the absence of external voices within the network means the absence of fundamental questioning as to what might be an appropriate place for genetic approaches to illness. As one prominent critic points out, 'it's a major social hazard that

nobody is looking at those ethical, legal, and social questions within CGDN. Because there is an implicit assumption that all this will be good for us. And we need to ask: will it be?' (OTHB-21).

Langdon Winner (1993), speaking of the politics of technological change, has raised these questions in a wider context. The 'problem of élitism', according to Winner, is a question of the way powerful actors and groups skew the agenda 'in ways that favor some social interests while excluding others' (1993:370). The powerful define the rules of the game and the allocation of resources. Winner urges those who study the social aspects of science and technology to ask

> what about groups that have no voice but that, nevertheless, will be affected by the results of technological change? What of groups that have been suppressed or deliberately excluded? How does one account for potentially important choices that never surface as matters for debate and choice? (369)

Thus taking CGDN as an example, claims that networks are more publicly accountable appear insupportable. Although the idea of a 'new deal' between science and society is appealing, it is not apparent that it works. Far from expanding the public sphere, network arrangements can be viewed as contributing to its erosion.

In addition to deficiencies in public accountability, deficiencies in fiscal accountability also need to be examined.

Accountability as 'value for money'

The NCE program was conceived under a neoliberal agenda of public sector reform that was fuelled by a rhetoric of fiscal accountability. Results- or performance-based approaches tie funded science to key economic and social outcomes. It seems both responsible and logical to account to the public

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[97] For a sample of recent discussions see Epstein 1999; Haraway 1999; Irwin 2001; Yearley 2000

for the use of their funds. But accountability goes beyond *use* to *value.* Asking if money is 'well-spent' involves asking if it is *effectively* spent, and if it could be *more* effectively spent elsewhere. Put specifically, are programs delivering *value for money*[98]? Can they *demonstrate* cost-effectiveness?

The problem of ensuring that public programs remain accountable and return value for money can be understood as a 'principal-agent' problem of delegation and information asymmetry.[99] The state (principal) delegates provision of research that will fuel innovation to university scientists (agents) who are induced by incentives (research funding) to comply with the regime of Strategic Science. But especially in technical areas, agents always know more about delegated tasks than principals. This asymmetry of information makes it difficult for the state to reassure itself of the integrity and productivity of the scientists they are funding (Guston 2000b:33).

One solution would be to regulate research performance directly, but that means state control. The neoliberal preference is for refined forms of 'remote control' or steering that induce internalization of the state's expectations. Through these mechanisms of *governmentality* (Foucault 1978),[100] 'normalized' subjects come to control *themselves* according to previously established understandings of what constitutes 'the norm' (Hacking 1990).

Governmentality requires fidelity devices that will measure and induce compliance, and provide 'discursive validation' that agents are doing what principals expect them to. Largely, these devices are accounting tools: budgets, cost/benefit analyses, ratios and comparisons, statistics, financial and compliance audits.[101] Accounting tools are far from unproblematic. While appearing impartial, they

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[98] 'Value for money', or 'comprehensive' audits are fundamental to NPM (Power 1995) and have now been adopted –at least in principle—by all federal and provincial auditors general

[99] For a fuller elucidation see David Guston'srecent work, e.g. 1999, 2000a, 2000b

[100] The pos-Foucauldain governmentality literature is extensive, but see, for example, Burchell et al, 1991; Barry et al, 1996; Power 1995, Ericson & Haggery 1997

[101] For more on accounting's 'calculative practices' and 'rituals of verification' see Power 1995; Porter 1995; Miller 1994

selectively 'construct the world' from a complex web of social and economic considerations and negotiations. Through its surveillance and control capacities, and its ability to determine financial norms, accounting has the power to create a new 'factual' visibility and discipline performance (Hoskin & Macve 1993; Harris 1998:137). Embedded layers of accounting and accountability induce the required compliance. These types of reporting relationships govern relations between the NCE program (principal) and CGDN's administrators (agent); and between CGDN administrators (principal) and network scientists (agent). The network's head office thus acts as an intermediary that helps assure state-principals that scientist-agents are following the policy agenda.[102] It becomes a 'centre of calculation' (Latour 1987) for the accumulations of facts to send to Ottawa.

Following this logic, reported data gather 'positive modalities' and become harder to resist as they move away from their conditions of production (the lab) to the network office (the centre) and then to the program directorate in Ottawa ('centre of centres'). At each stage data are recombined and reinscribed. The NCE directorate seeks to control the network by specifying what 'makes up' the numbers (Hacking 1990). But network administrators reinterpret the directions in instructing scientists what information is to be supplied.

To illustrate, CGDN would report as *network* accomplishments almost everything their (university- and hospital-funded) researchers achieved, from scientific breakthroughs, to publications, external grant funding, and the raising of venture capital by researchers in network spin-offs. This over-reporting was so prevalent that many of the 'official' network statistics I consulted proved unreliable for the purposes of this study, because they failed to conform to the guidelines set down by the NCE Directorate. A serious example is that networks were supposed to report as 'cash contributions' from partners, only funding that flows directly through network accounts. In many

[102] In an international comparative study, Atkinson-Grosjean & Grosjean (2000) found that the proliferation of such intermediary agencies was a generalized feature of higher-education systems under neoliberalism.

cases, CGDN reported funding that went directly to network researchers. The network's legitimate interest in those funds was minimal, but because they flowed to members they were reported to Ottawa as contributions received by the network. Also, researchers were asked to report almost all their research activities as network activities, for the annual statistical report. As one complained,

> It seems sort of ridiculous, talking about all of these accomplishments, when in fact you know maybe 5% of them were funded by the network. And yet they want to hear about all [of them]. So every year I have the same argument, like: 'what do you want me to do? Write what my student MF did last year? Because that's all that you funded.' And she says 'oh no, put it all in.' And I say 'well, why should I?'
>
> And it's gotten, quite frankly, a little bit ridiculous, given the amount of money we get versus the accountability and justification. I mean what do you do? Write your whole program down and attribute it to the network? ...I mean I would say things jokingly like 'I think we should just spend all the money on...having great meetings in ski resorts. I'd get more out of it than you pretending to send me money and pay for my student.' RK-65-71

The directorate is not only aware of reporting anomalies but may have contributed to them. As shown earlier, for the program as a whole, additional public funding is under-reported while aggregated private sector contributions—both cash and in-kind--are over-reported. As early as 1938 the US National Resources Committee called such practices 'window dressing' (Godin 2000-3:16). Today, we more often label it 'spin'. The purpose is simply to make results look better than they are, to protect budgetary resources and allocations.

The NCE program's first full-time director was appointed January 2000. He says that the problem has been brought to his attention and agrees that, 'yes, maybe some better discipline should be followed...that's something that we will be looking at' (JCG-14). In September 2000, the Directorate instituted an audit requirement, meaning networks now have to submit externally audited annual reports. Since that directive, CGDN has restructured its administrative staff. Responsibility for financial and statistical reporting has been assigned to a new staff member with appropriate qualifications.

Summary Discussion

This complex chapter has attempted to capture the way CGDN forged an institutional identity and organizational structure under multiple constraints, including: demands for both scientific excellence and commercial relevance under managed conditions; resistance from local host institutions; the traditional structure of basic research and conservatism of researchers; and the sheer novelty of doing something that had never been done before. Now, after more than a decade has elapsed, CGDN's successes are clear. But, equally clearly, some have been achieved at the cost of consequences perhaps unintended by program architects.

The concentration of resources in CGDN creates a hegemony. The network *defines* the field of medical genetics in Canada. Non-members are 'othered'. Careers can be affected. Yet no objective criteria for membership exist. Instead, membership is an 'invitation only' affair, within the arbitrary remit of the same élite inner group of scientists that has controlled the network from the start. Power relations are asymmetrical; they concentrate in the most powerful actors and in the centre(s) they control. It is, quite literally, a self-reproducing 'old boys' network. Relatedly, network resources flow to the power centres rather than being distributed to scientists across the country. The consequence of exclusion and concentration is reduced diversity within the Canadian 'science system'. As a concomitant, there is no room in the network for 'lay' representations. The 'public interest' is constructed and defined in the abstract, within expert discourses that exclude authentic voices of interested publics.

That being the case, and in the spirit of 'value-for-money' accounting, we can ask about the extent of public investment in the network (as well as in the program more generally) and about the returns on that investment. In the ten years from 1990 to 1999, CGDN's six original Principal Investigators received between $1.4 million and $1.8 million each in network funding, while the 15 other founders

received on average between $800 thousand and $1 million. These are modest amounts, on an average annual basis, but it must be remembered that network funding is *incremental* funding. Network researchers also receive direct support from non-profit disease foundations, research councils, and industry contracts while their home institutions underwrite salary and direct costs.

By the time of federal exit, in 2005, CGDN will have received in excess of $60 million in direct NCE program funding. This figure does not include provincial and industry contributions, commercial revenues, or university subsidies to network researchers. It is impossible to tease out of this complex of funding sources what results are attributable to the network and what would have happened anyway. The same is true of the program as a whole where, as already shown, public investment exceeds $650 million. In other words, there is no reliable way to determine whether or not CGDN and the NCE program deliver direct 'value for money'.

But the problem with accountability frameworks is that they seek to capture and evaluate only those dimensions that can be quantified, objectified, and made accountable. Non-quantifiable and less tangible practices are literally *not taken into account.* At the same time, other elements assume new weight *because* they can be quantitatively evaluated: quantity (not quality) of research publications; numbers of patents held; dollar value of research contracts. In short, by focusing on readily *quantifiable* inputs and outputs we risk neglecting more complex social variables that resist measurement but are, nevertheless, valid outcomes. I am thinking, in particular, of the construction of intangibles such as 'network culture' and 'network science'. The next chapter examines the way the network forged a scientific culture and community and a scientific legacy.

Chapter 5: Culture And Science

Forms and practices of scientific culture and community[103] were in place well before Robert Boyle convened an 'invisible college' in Oxford and London in the mid-17th century. In the mid-1960s, Derek Price (1963) borrowed and extended Boyle's metaphor, reminding us that small, informal collectives of closely interacting scientists are the principal means of scientific advance. Subsequently, Diana Crane (1972) defined an 'invisible college' as an informal interpersonal network based on shared scientific interests, rather than geographic proximity. As Philip Agre (1999) points out, 'so-called invisible colleges are in many ways more visible to the researchers than the physical campuses where they organize their places of work'.

The distributed and informal nature of scientific interaction is also captured in the term 'communities of practice' which describes self-organizing, self-selecting groups of colleagues whose members are informally bound together by their shared expertise (Lave & Wenger 1991). Note the family resemblance with the scientific 'thought-collectives' identified by Ludwik Fleck (1979). These communities, characterized by intellectual interaction and the mutual exchange of ideas, constitute

[103] This section draws in part on Fisher, et al (2001)

the 'carriers' of a field's knowledge and culture. Similarly, Knorr-Cetina (1999) speaks of the very different 'epistemic cultures' of molecular biology and high-energy physics.

Together with actor-network theory these concepts, drawn from the wider field of science studies, will help us understand the development of a distinctive culture and community in the Canadian Genetics Diseases Network (Part I of the chapter), and the nature of what might be termed 'network science' (Part II).

I: 'A Nation of Colleagues'

> The cooperation and collegiality have just been incredible. It's created a nation of colleagues that is totally unbelievable. (Michael Hayden, Scientific Director. MH2-21)

At the end of its first year of operation, CGDN listed among its achievements the development of 'an ethos and common understanding of what it means to be in a network' (CGDN-AR 1991: 8). The use of the term *ethos* indicates an interesting ambivalence. It draws around the network the cloak of Mertonian ideals relating to the normative structure of science. But at the same time it invokes the new ideal of 'network science' with its emergent (counter-) norms such as patents and industry partnerships. The rhetorical purpose of the claim was to persuade NCE bureaucrats that CGDN took the program's non-scientific requirements seriously. Another claim about network ethos can be found two years later, in the proposal for the second phase of funding: 'we have created a *nationwide department* of human molecular genetics' (CGDN-FP 1993, emphasis original). The subtext here is recognition of Ottawa's intent to change the overall research culture in Canada, network by network, by overriding university boundaries and autonomy.

Even if we are to take the idea of a 'network ethos' seriously, the claims were premature to say the least. Ethos can be understood as a cultural achievement, and the development of culture takes time.

As well, an interesting question can be posed about whether culture can be *induced* by the imposition of a network model, or the provision of funding. But in examining CGDN's history, we can see that very gradually, and taking on a different tenor in each of the three funding phases, a distinctive ethos or *esprit de corps* (CGDN-FP 2001) did, in fact, emerge. CGDN's 'induced' epistemic community anchored itself in the production of a discursive space of face-to-face interactions that promoted trust and reduced competition.

Inducing Solidarity

Although socialized in 'invisible colleges', network researchers were confused about, and initially resisted, the whole concept of 'mandatory networking'. No real agreement suggested what that might be, or how it might be accomplished. The network's professional staff had to invent virtual and face-to-face ways of meeting program requirements. They had to grapple with the complexity of somehow linking together a dozen institutions, two dozen principal investigators, as well as post-doctoral fellows and graduate students. And the reporting requirements meant that networks couldn't just *say* they were doing networking; they had to *prove* they were doing it to the NCE directorate. So ways had to be devised of enticing scientists to comply.

The method they implemented was to make principal investigators' funding conditional on participation in network activities. Subsequently, it was hoped, PIs would realize the manifold benefits of voluntary participation. Almost all network researchers interviewed commented on this creative relationship between network funding and network-building. For example,

> Although the other aspects of the network have been much more important, you wouldn't have pulled the people together without the bait of the funding. We would have said, 'I haven't got time to just go and talk with these people.' But you'll go and talk when you know that if you don't, you won't get your funding. And then you find it is really worth while having talked to them and it is really fun. BG-17

> The biggest value of the network is not the funds that they give us, but the networking opportunities and the collegiality and so on. Although, I have to say that if we didn't have funding for our labs in addition, we'd probably say, 'Oh, I'm so busy, I don't think I'll go to the annual meeting. I don't really need to be there.' Whereas, if we're funded by the network, we have an obligation to be there. RW-17

The network funding was not a significant proportion of a network researcher's total budget. Only a small component of their research program would come into the network. Usually the component that would profit best from the collaborative opportunities. Other aspects stayed outside. Even in the early phases, when the network was less extensive, the funding allocated to researchers probably never amounted to much more, on average, than 15% or 20% of their research budget. This would have been enough to perhaps support a senior technician or post-doctoral fellow. Put another way, 'out of perhaps 15 to 20 projects in my lab, maybe two or three were covered by network funding, the rest were covered by other kinds of funding' (RW-16). But a moral obligation was attached to the network funding. It got people 'to buy-in to the network concept and become part of it' (FJ-21). It helped to overcome the resistance to leaving the lab for yet another meeting. And it was this face-to-face aspect that quickly became far more important than the virtual aspects of networking. The latter soon became taken-for-granted, an enabling technology[104] to further the personal relationships and community of practice that was being forged. As a policy advisor explains,

> The network mechanism...forced people to get together face to face, because of the funding provided...Face-to-face meeting is really important, especially early on. You need a lot of personal interaction to make that networking work. And after that you can do it by e-mail and telephone and fax and all the rest of it, but in the beginning you really have to have the face-to-face communication. ARA-DR-49

The face-to-face community that became the Canadian Genetic Diseases Network began to take shape in 1991, at the first network meeting.

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[104] Another enabling technology is the conference call. Board and committees frequently 'meet' by telephone

Face to Face Community

As the main forum for interactions and exchange, the network's early scientific meetings laid the foundations of network culture and community. Unanimous about the cultural importance of these meetings, scientists considered them one of the main benefits of belonging to the network. The first meeting, held at Whistler, BC, in May 1991, set the format for those that followed. Because of the NCE requirement to dedicate 10% of the budget to networking, full costs of attendance were covered for Principal Investigators and Core Facility Directors. These individuals could, in addition, nominate three members of their teams for full subsidy. For example, students and fellows funded by the network or working on network projects could attend cost-free. In rare cases, a technical support member of the group could be included if their contribution was deemed to constitute fundamental research. In molecular biology, where rewards usually go to lab leaders (Knorr-Cetina 1999), subsidizing conference travel for junior researchers was so unusual as to be unique.

Each participant was expected to present and discuss their results, either through a poster (students, fellows) or an overview lecture (PIs). As a result, delegates to the Whistler meeting faced a busy three-day schedule of scientific sessions, workshops, and discussion periods. Approximately 100 participants attended from across Canada including board members, external collaborators, and industry partners, as well as network researchers and special guests. Concurrent workshops debated, among other issues, the topics of 'Industrial Relationships' and 'Search for the Gene'.

This routine may seem much the same as *any* scientific meeting or conference. Scientists get together and give papers as matter of course. But there are significant differences. First, as one of the researchers explains, 'a network provides you with access to a completely different and much broader group of people than you would ordinarily associate with at meetings' (FT-3) Normally, scientific meetings are segregated by narrow research interest. In contrast, network meetings are

broad, covering the field of genetics in Canada. Second, from the start, the norm was 'full disclosure'. The meetings were intended to encourage in-depth discussion of interesting, early-stage research results, often prior to journal publication. Sensitivity to priority, if nothing else, would have precluded this level of frankness in a 'normal' scientific meeting.

At the same time, however, even in these first meetings, a countervailing force emphasized confidentiality. Unless you were a network principal—that is a researcher or partner (industrial or institutional) listed in the network's Internal Agreement—you were required to sign a confidentiality agreement. Intellectual property rights had to be preserved in order to fulfill the network's commercial mandate. So those 'full and frank' discussions had to take place behind closed doors; participants were advised that discussing results in a closed forum of colleagues did not constitute disclosure for patent purposes.[105] Even so, researchers were cautioned to apply 'normal discretion in disclosure of scientific data' (CGDN-ASM 1991). In practice, however, it soon became clear that 'normal discretion' was not required.

> It is totally different than going to a meeting where you have to be careful what you say because someone will rush off and do your experiment and publish it before you get to it. BG-20

> In the network, you're not in competition. And so you can confide and get some valuable feedback from these people, right? It's nice to get up there and maybe brag a bit about the stuff that you've got before it's published. It isn't like you feel 'I can't say anything because Frank in Vancouver's gonna scoop me' (RK-65)

> It's one of the strengths of this network that we're all in this together. It's difficult out there. The more that you can discuss things, in confidence, the better. You have to be confident that the person you talk to is not going to spill the beans. The trust relationships and the reliance on individual integrity is very important. PS-SH1-20

[105] Debatable but not tested

The third factor that marked these meetings as different was the social cohesion they engendered. Despite all the scientific *gravitas*, the social aspects remain particularly vivid for most people. Asked what she recalls about the first meeting, one of the founders, a distinguished scientist, says, 'we went skiing up on the glacier all together. It was great' (BG-31). The second and third meetings, in May 1992 and June 1993 respectively, were held at the Far Hills Inn, Val Morin, PQ. Again, her recollection of the Quebec meetings is that 'we had afternoons off. We went hiking...We did plays--skits and things. And we had fun' (BG-31). Few more effective ways can be found to build trust and loyalty--and the foundations of future collaborations--than to play together and build personal relationships.

> When you know somebody personally, because you've met them at these network meetings, then you are much more liable to approach them, to work with them. It increases the potential for collaboration. LF-42

> For me the network has meant a lot of relationships with people that I wouldn't have met otherwise, so I have a whole circle of friends now that I wouldn't have had. That's just on a personal level. FJ-37

> I have a strong sense of belonging to the network. What I do is defined within my grant applications. How I feel is defined in my interactions with the network. RG-38

So the network community was about openness and sharing, on the one hand, and building a sense of solidarity and belonging, on the other. Through the annual scientific meetings, everyone in the network knew something about what the rest were doing and that facilitated a climate for collaboration.

A Climate for Collaboration

Network scientists became familiar with each other's research from hearing presentations on work-in-progress. This annual 'overhearing' enabled synergies to happen. As one researcher explains,

'going to the network meeting, it's a very easy, fast way of getting a survey of who's doing excellent research in Canada in our field. And that....saves a heck of a lot of time for us all' MW-30.

Perhaps listening to somebody talking about a particular gene, a researcher will realize that they have a piece of the same puzzle. Or perhaps they need to find someone with particular skills, to help them with a project. In either case, they can make contact, confident that their overture will not be rejected. In other words, to borrow a felicitous phrase from one PI, the network acts 'kind of like a blanket purchase order on collaboration' (BG-19).

> The whole game is sitting open on the table and then you can reach in any direction. Anyone who gets a call from another person within the network has a sense of obligation to talk and participate and collaborate... It is like asking your brother or sister for something as opposed to someone with whom you don't really have the same relationship. They can't say 'sorry, I'm too busy.' Or 'sorry, you're competing with me.' BG-19

> I know those people well. I've met them many times at network meetings. I've heard them talk. And if there was anything I needed or wanted, I certainly wouldn't hesitate to pick up the phone and expect that I would get a very positive response. DC47

The fostering of trust and reciprocity on this scale was a unique experience for network scientists, who were more used to a culture of competition than one of co-operation. Reducing competition and enhancing the ability of network scientists to work together constituted an advantage for the entire collective. It should be noted, however, that the absence of competition was in part an artifact of the selection process. Researchers were chosen for the complementarity of their programs. No two teams were working on exactly the same thing. So in the network, as one PI says, 'we're not in competition, because we're doing different things. We're tied together with the common interest, but we are distinct' (MW-42).

Being collegial also included working for the common good, and trusting community decisions. Through the years of meetings and network-building, a process of sedimentation took place. CGDN began to settle into the shape it had claimed at the start--a community of colleagues, with a shared ethos and a common understanding of what it means to be in a network. One researcher comments that, 'as a group of geneticists we really got to know each other much better than would have happened otherwise' (LF-13.) Another says that the network created value through 'personal contact, personal motivation, driving the science' (RK-61). Over time, members began to identify themselves as *network researchers.* Almost by accident, they agreed, government had 'got it right' and produced a capacity to do 'national science'. As Hayden comments, 'it's quite unusual to be led from Ottawa. But this was real leadership' (MH2-2). For Tsui ,'whether by design or by accident, the federal government somehow had the foresight to create these kind of networks. [Now] we are leading the world' (LCT-23). The beneficial effects of this foresight on the conduct of science was noted.

> Because of the networks, across Canada we are doing science in a manner that I don't think could possibly have happened before...A very large piece of the scientific community is [now] involved in promoting collaboration--inter-university and interdisciplinary, not just geographic. That is a very positive thing. RG-78

> The network is like a national lab without the consequences—the bureaucracy, the 9 to 5 mentality. Here, it's academic, competitive, but then we get together and we figure we're all part of this same process. RK-64

> We created research groups that would not have existed otherwise, that spanned the country. Or involved different components of the country where we might not otherwise have encountered each other. These are cross-country collaborative interactions. RG-28

However, the network did not evolve quite the way the program's architects envisioned. They had anticipated large-scale, cross-country collaborations. For whatever reason—institutional logistics,

egos, distance—that did not happen. And, despite mutual goodwill, the number of researchers who built one-to-one, bench level collaborations was less than the potential would suggest.

> We don't interact on a project by project basis as much as was hoped we would. I think we fail a little bit there, just because there is too much to do and no time BG-12

> There are some collaborative projects within the network. But, it's not as heavily networked as it could be, I think. FJ-40

> I have not been one of the ones who has interacted...perhaps as much as some other people. Because I don't really have a collaborative project with anybody in the network...it's not because I'm not interested, it simply hasn't been beneficial DC46

Still, by creating the intellectual and collegial infrastructure described above, the network allowed individuals to formulate different questions and approach their science differently. So even in the absence of hands-on collaborations, researchers benefited from their interactions in the network. Says one network researcher, 'I don't think we would have done that project in quite the same way if it wasn't for the network' (BR-4). Another confirms that

> we have changed in the way we ask questions and, therefore, the questions that we answer and what we publish. I know that for me--the kind of science I was doing, the directions I was taking--it's very, very clear that I do things differently than I would have done before RG-80

But because each phase of funding added new researchers, institutions, and industry partners, the capacity for collaboration and the nature of the network community was not static. The orientation changed over time.

Phase Transitions

When the network was renewed for Phase II, with its enhanced emphasis on commercial results, it meant more industry partners[106] and more emphasis on commercial potential at the annual meetings. Yet the overall ethos stayed much the same. Largely, this was because the core-set remained unchanged and because the expansion had been relatively modest, from 21 to 33 researchers, and from 11 to 13 institutions. So the growth was easy to absorb. That was not the case in the transition from Phase II to Phase III. With the expansion to 50 researchers in more than 20 institutions, intimacy was almost impossible. Almost all founders felt the culture changed radically at that point and that something important was lost. As one comments, 'in the early days I knew everybody and now I don't. That happens when a group gets big enough. It means that we're now more of a conglomerate than a bunch of guys working together' (RG-24). A comparison between Phase I and Phase III follows in Figure 8, showing growth in numbers of investigators and institutional partners.

[106] Details of industry partnerships appear in Chapter 7

Figure 8: Growth in Partner Institutions and Principal Investigators Comparing Phase I to Phase III

	Phase III	Phase I
PRINCIPAL INVESTIGATORS	50	21
UNIVERSITY PARTNERS		
--Alberta	✓	✖
--Calgary	✓	✓
--Laval	✓	✖
--Manitoba	✓	✓
--McGill	✓	✓
--McMaster	✓	✖
--Montréa	✓	✓
--Ottawa	✓	✓
--Queens	✖	✓
--Toronto	✓	✓
--UBC	✓	✓
--UVic	✓	✖
TOTAL UNIVERSITIES	11	8
HOSPITALS & INSTITUTE PARTNERS		
--Biotechnology Res. Centre, UBC	✖	✓
--Children's & Women's Hlth Cntre UBC	✓	✖
--Children's Hosp. East. Ontario, Ottawa	✓	✓
--Hôpital Ste-Justine, Montreal	✓	✓
--Hôpital Saint François d'Assise, Laval	✓	✖
--London Health Sciences Centre	✓	✖
--Mount Sinai Hospital, Toronto	✓	✖
--Hospital for Sick Children, Toronto	✓	✓
--Montréal Children's Hospital	✓	✖
--Montréal General Hospital	✓	✖
--Ottawa Hospital Research Institute	✓	✖
--Robarts Research Institute, London	✓	✖
--University Hospital, Vancouver	✖	✓
TOTAL HOSPITALS & INSTITUTES	11	5

Earlier, I discussed how the élite recruitment criteria that were applied in the first two phases caused a fair amount of debate. Many were uncomfortable with the emphasis on exclusivity. However, the wisdom of this approach was that it produced a strong and cohesive culture. As a result, when the approach was reversed in Phase III, it tended to undermine what had been built to that point. One of the founders had spoken strongly in the past about including all qualified scientists. But when that eventually happened, he found the effects disturbing.

> We had such stringent criteria in the beginning and then, in order to get the Phase III funding, we had to open it up again. Wide open. That was a most difficult decision for me. I was not very happy about opening the thing wide because it was so indiscriminate. Some people were recruited just for their name. They didn't really have any interest in the community . They are part of the network and as yet I still haven't seen any contribution from these people. LCT-13

Because so many people and institutions were now members, maintaining the same level of familiarity was impossible. The mechanisms of interaction that worked so well in a relatively small group stalled when numbers grew. People were disappointed that they could no longer get to know each other in the same way. Fear was expressed that a more corporate, commercially oriented style of doing things would undermine collegiality. Even the tenor of the scientific meetings—the great binding mechanism of the past—was affected.

> The meetings haven't been great. All scientific talk; no play. This year's meeting was held in the middle of Vancouver, in a small hotel, where there was nothing that you could do together for fun. And it was tied to another huge conference. So everyone had been away from home too long, and were too tired to play together. BG-30

> It is immediately obvious when you go to a network meeting, that this is not...the style that we have been used to. These are meetings where the commercial aspect of what we work on is stressed. That's probably the biggest thing. And then the scientific content comes second. MW-6

Not only were the meetings different, the sense of commitment was different. When researchers were recruited for Phase I and Phase II, it was for the long term. Renewal of funding was not guaranteed, of course; competitions were fierce and anxiety on that score was high. But no one sensed a finite horizon. In those early years, funding could be lost in only two ways: either the whole NCE experiment would be cancelled, in which case all the networks were in the same situation; or a network would not be renewed because its proposal would be judged inferior to others, and that was the luck of the draw. No third contingency, no sunset provision, appeared until Phase III. It came as a complete shock and a bitter irony that when the program was made permanent, in 1997, removing fears of overall cancellation, it was at the cost of continuity for individual networks. Thus

researchers recruited for Phase III came in knowing that, at best, they would be with this group for a maximum of seven years. Together with the sheer numbers of new recruits, the sense of finitude limited 'buy-in'. In fact, by this point, several scientists were members of two or even three networks.

So the relationships, and the willingness to trust, were not there in the same way. This was manifestly the case in attitudes to the annual scientific meetings. In the past, attendance had been mandatory, not discretionary. But to many of the Phase III recruits it was 'just another meeting'; they did not bother to attend. As one of the managers complains, 'the minimum that we ask is that you come to the annual scientific meeting. The old groups from Phase I and II are always there...[but]... there is a much weaker understanding [among the new recruits] of why they need to be there. Some of them from the new group just didn't come' (PS-CS-80).

The funding bait was so diluted, because of the number of researchers, that it no longer offered sufficient inducement. As well, many of the associations written in to the Phase III proposal were strategic. The purpose was to *simulate* dynamic expansion; actual connections were tenuous at best and in some cases divisive. For example, principal investiagtors had been recruited from Mount Sinai Hospital in Toronto but historical disagreements marred relations between this team and their neighbours across the street at Sick Kids. The most recent concerned the administration of funds for Genome Canada, the new umbrella body for genome research.

> Genome Canada is very much the legacy of CGDN. And we [Sick Kids] worked very very hard to get the government to do that. And I think it is just a crying shame that we at this institution, the place where most of the genetic diseases work is done, are not being given the job of making sure the money goes to the right places. It is going to go to Mount Sinai. It has been diverted. There is a lot of political stuff that goes on. If Mount Sinai is going to use the money for genetic disease, that would be great. But it sounds like it is going to be diverted to doing all kinds of rubbish that has got nothing to do with genetic disease. BR-53-7

In a climate of tenuous connections and actual rivalry, the authority to compel attendance was lacking. As a result, enculturation into the network was minimal. At this late stage of the network's development, the best way to describe it may be as an 'imagined community.' Benedict Anderson (1983:1-7) coined this term in developing a theory of nationality and 'nation-ness' but it provokes some interesting thoughts when applied to this network as it presently stands. Anderson proposes to define nationalism as *an imagined political community [that is] imagined as both inherently limited and sovereign* (5-6). It is *imagined* because most members will never meet their fellows 'yet in the minds of each lives the image of their communion' (6). Anderson suggests that *all* communities are imagined, once they exceed the possibilities of face-to-face contact achievable in primordial villages. What distinguishes communities is not their reality, he says, but their *style* of being imagined. It is imagined as *limited* because it has finite, though elastic, boundaries beyond which lie other nations. It is imagined as *sovereign* because nations dream of being free. And it is imagined as a *community* because it is conceived as a deep, horizontal comradeship. I suggest these attributes are applicable to the imagined community that is the Canadian Genetic Diseases Network today.

In this section I have explored the idea of the network as a community and a culture. In the next, I investigate the type of science produced by this community.

II: Network Science?

Grounded in laboratory practices and commercial motivations, molecular biology is an example of a 'practical science'. Divisions between the creation of knowledge (theory) and its applications (practice) are largely rejected. Meaning collapses into application, and truth value collapses into use and exchange values.[107] The focus is converting lab results into profitable new therapies. In this

[107] The phrase 'practical science' was R.G. Collingwood's and these points were made by Evelyn Fox Keller in a lecture at St John's College, UBC, March 2000. For a political economic perspective see Mackenzie, Keating and Cambrosio (1990)

section, I will review what happens when individual research programs in molecular biology (medical genetics) are brought together under the banner of 'network science'.

Science is normally conducted in a highly competitive environment; individual labs are pitted against each other in races for resources and priority (Merton 1957). At the same time, *within* a laboratory and under the direction of its leader, people co-operate, share resources and ideas, and publish together. In a sense, CGDN extended the boundaries of 'the laboratory' to include everyone (and every*thing*) in the network.[108] All members of the network were considered colleagues; all had access to the network's technologies. In the long run, this 'extended lab', proved 'more important to the scientific enterprise that a lot of the rest of what CGDN does, because this is where the new ideas and approaches that power everything else will be generated' (Expert Panel Report; CGDN-EP 1997:15).

The ethos of trust and cooperation allowed network researchers to reduce competition. They helped each other with scientific problems, reviewed each others' papers, exchanged students, and advised each other at all levels. These tangible and intangible aspects of belonging made the network a coherent and cohesive entity. It provided an organizational structure, albeit loose, that contributed to the production of first-class science. But whether this science could be described as a distinctive form of 'network science' is an open question.

In my initial reading for this study, I found in network and program documents descriptions of a clearly defined network research program, divided into projects and themes, with teams of researchers working together under the direction of project leaders. I imagined the discussions at the start of each phase, about what 'we' were going to do next. I imagined scientists working together, according to plan, to discover genes and therapies. On closer examination, as I will explain, the

reality of network science proved elusive. Network science was not where I expected to find it, in the 'network' research program. But it was very much in evidence elsewhere: in the services provided to members by core facilities and their directors. In order to approach these questions, I first needed to develop an understanding of the Medical Genetics field.

Medical Genetics: An Overview

The science of CGDN is medical genetics, the field that studies the relationship between human genetic variation and diseases. Genetic disorders are classified into one of three types: single gene disorders, chromosome disorders and multifactorial disorders (Prater and Newlands 1999). Single gene defects are caused by mutant genes, usually a single critical error in the genetic code. More than 4000 single gene disorders have been described. Chromosome disorders are due to an excess or deficiency in the number of genes contained within an entire chromosome. The most common example is Down Syndrome (Trisomy 21), which is an extra normal copy of chromosome 21.

Multifactorial inheritance is responsible for a wide range of disorders, believed due to multiple genetic mutations. Some cancers, coronary artery disease and diabetes mellitus are included in this group. A mutation is defined as any permanent change in the nucleotide sequence of DNA. Mutations may occur in somatic or germline cells, but only germline mutations are inherited. Somatic mutations, however, are responsible for many medical problems. For this reason cancer and coronary artery disease are often considered 'genetic' diseases (Prater and Newlands 1999).

The practical goal of medical geneticists is to understand the basis for mutations and to use that information to design new therapies for gene-related disorders. The field contains numerous, rapidly

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[108] Latour (1988) describes a similar effect in the Pasteurization of France.

advancing areas of interest, such as chromosomal analysis; cytogenetics; biochemical genetics; clinical genetics; population genetics; genetic epidemiology; developmental genetics; immunogenetics; genetic counselling; and foetal genetics. Michael Hayden's research program in Huntington's disease is one example of the type of cross-overs that occur. Hayden's team has identified a marker used in genetic testing for Huntington's disease. As well as researching the genetic basis of the disease and testing for it in patients, they are also involved pre-natal testing, and in studying the psychological consequences of genetic testing on patients.

The history of medical genetics and the history of the gene are intertwined (Childs 1999). Keller (1995) traces an arc through three periods. The early 20th century was dominated by a very powerful discourse of gene action. But the gene itself remained a statistical entity; a black-boxed construct. In general, medical science paid little attention. Interest increased when the physical basis of heredity was established, but mainly among those who studied rare anomalies (Childs 1999). But little progress was made until 1953 when James Watson and Francis Crick described the molecular basis of DNA.

The mid 20th century was the era of early molecular biology, which seemed to provide answers to questions about the nature of the gene and gene action—the 'genetic program'. At this point, according to Childs, medical genetics began in earnest, following the functional definition of one gene-one enzyme. In the 1960s, the development of the structural definition of the gene meant that inborn errors of metabolism could be described in terms of protein differences. The comparative youth of the field can be illustrated by network scientist Charles Scriver., who learned biochemical genetics in its infancy. When Scriver joined the McGill faculty in 1961, he was the first biochemical geneticist in Canada, meaning that he was 'the first one formally trained to do that type of thing and be taken onboard as a person who would do biochemical genetics' (CGDN-CS).

In the late 20th century, the molecular definition of the gene led to a technological explosion that moved genetic and molecular analysis beyond rare single-gene disorders to complex, multifactorial diseases. The tools of molecular genetics underwent revolutionary changes. They include the identification and use of restriction enzymes, cloning for recombinant DNA, vectors, probes, polymerase chain reaction, DNA sequence analysis and protein analysis. The availability of these tools, and the promise of genetics, led to the foundation of the Human Genome Project in the early 1980s. As the project neared completion, molecular biology again changed radically as fields like proteomics and functional genomics came to the fore.

The 'new genetics' is revolutionizing medical genetics. It raises the prospect of altering the genome to *prevent* disease rather than *treat* disease. Virtually all disease progresses as a combination of environment and genetics ('nature versus nurture'). Medical geneticists believe 'nature' plays the most significant role and act to intervene. Many believe this prospect raises the spectre of biological determinism and a new eugenics.[109] For others, the new genetics ignores the significance of 'nurture', i.e. the socio-economic determinants of health and disease.[110] While these debates and issues are compelling, except where they impact directly on CGDN they lie beyond the scope of this study. The next section examines issues of space and scale in the molecular sciences and relates these to CGDN.

Space and Scale

In her comparison of high-energy physics and molecular biology, Karin Knorr-Cetina (1999) describes the latter as small-scale 'benchwork science' geared to 'treatment and intervention'. By definition molecular biology manipulates small objects in small labs . This modest scale was

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[109] Richard Lewontin is an authoritative source, see 1991 & 1999

illustrated on one of my site visits to a network researcher in Toronto. The team was just setting up a new laboratory in a university annex. The lab, quite literally, came in two cardboard boxes. One contained a powerful PC, pre-loaded with genetic analysis software. The other contained slides, reagents, and biological materials. We laughed about franchising 'Lab-in-a-Box', or 'Lab-to-Go'. Of course, the physical infrastructure of the laboratory is provided by the university but the space and benches are generic. Beyond unpacking the boxes, nothing special is required.

Gieryn (1999a & b) has commented on the standardization of space in these labs and the architectural boundary work they embody. I noted similar effects in my site visits to different locations. The organization of space is predictable. For example, the labs at the Centre for Molecular Medicine and Therapeutics are laid out in such a way that the upper and lower floors are virtually identical. A common room/kitchen is located on each floor, at one end of the hallway. This area is the social focus, with a lot of coming and going. Signs on cupboard doors advertise meetings, seminars, and social events. Groups of grad students and post-docs chat over coffee and microwaved food at the common table. Overheard conversations: 'I had to sacrifice my first mouse last night'; 'I just found a mouse up my sleeve; its tail was sticking out. I thought I'd lost it'. (The mouse core facility was located at CMMT at this time).

The labs are situated around the circumference of each floor, while the heavy and/or shared equipment is in the centre. Each lab appears to have two working benches in a bay and a computer desk. The building's architectural boundary work discloses no 'public face', not even a functioning reception area. All exterior doors are locked and electronically controlled. None is identified as the main entrance to the building. The most likely candidate carries a sign advising visitors, in no uncertain terms, that they are at 'the wrong place'. 'This is not the hospital', it says. Those who

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[110] In Canada, note the work of Patricia Baird e.g. 2000 and Clyde Hertzman e.g. 1999. Both are members of CIAR

persist must use the intercom to ask someone to come and physically admit them. Indifference to (or fear of?) public intrusion was a spatial feature of the all the network facilities I visited. The sites of knowledge production were not 'open'.

These sites, molecular biology labs, house 'biological machines' for the genetic engineering of knowledge. Knorr-Cetina calls these machines 'prolific small-scale factories' for the mass-production of cell-lines, bacteria, viral vectors, and purified mice, like those the grad students were discussing. These were 'knock-out mice', used in the study of oncogenes (cancer), that the network supplies from its mouse core facility. ('We put genes into the mice and then send them off to the investigators' [Mouse Core Facility Director, FJ-16]). Mouse models ('animal helpers') are research tools. Geneticists engineer them by 'knocking out' particular genes to try to cause cancer. The mice are bred to be exactly the same; a blastocyst injection into the ovum changes the organism. These mice are not 'natural'; they are constructed in the laboratory. Bruno Latour (1987) talks about the 'purification' of wild nature that takes place in a lab. '

In her comparison of the cultures of high-energy physics (HEP) and molecular biology, Knorr-Cetina (1999) notes that experimentation in HEP involves large and very expensive experimental devices and hundreds of scientists. These huge investments demand a long-term communitarian orientation to the management of spaces and technologies. Thus 'big science' like HEP is largely a collective enterprise. Publications list hundreds of authors in alphabetical order; discourse is open and free-flowing along 'confidence pathways' that link people together; a variety of spokespersons represent the work. Knorr-Cetina calls this a 'post-traditional communitarian structure'.

In contrast, molecular biology's 'lab in a box' has no dominating technical apparatus that would focus a community. Instead, says Knorr-Cetina, individual scientists occupy separate spatial and epistemic lifeworlds. In contrast to HEP, molecular biology is highly individualistic: witness the

tradition of naming labs after the leader (the Hayden lab; the Worton lab). As described in Chapter 5, leaders speak for and represent the lab as a whole. They are the focal point for public and scientific recognition. They appear in the media, give papers at conferences, accept the awards, while those who actually do the work often go unrecognized. Glasner & Rothman (1999; 2000) show that the most prominent and authoritative 'experts' are those who are furthest from bench research. A dual system is at work. Teams of post-doctoral fellows, graduate students, technicians, and junior faculty do the actual hands-on science under the direction of project leaders, while the lab director attracts the resources and plans the research program. One of the network's core set, Lap Chee Tsui, is chair of CGDN's Scientific Advisory Board and head of the International Human Genome Organization. He says,

> I'm still in the lab in terms of interactions but not day to day, not hands on anymore. I have to rely on people telling me what is going on. Of course I miss it. But it would be very difficult to go back. Because now I design experiments so complicated I need people to help me out. LCT-26

Given the dominance of laboratory leaders, and the fragmentation of molecular biology, CGDN's achievements in fashioning 'something like' a communitarian network culture, and 'something like' network science, are worthy of comment. Unable or unwilling to overcome embedded epistemic norms, they were able nevertheless to scale-up until the network approximated 'big science'.

Scaling Up

Until quite recently, molecular biology in Canada was a competitive and fragmented world where solitary researchers, in small laboratories, conducted small-scale experiments. Interactions were limited, if nothing else because of the time and costs involved. As one of CGDN's investigators recalls,

> You might see your research colleagues at meetings or even make special trips to go to their lab and discuss research in common. And you might even send some grad students around or a technician to learn a procedure or something. But that was a relatively small number of interactions that each lab would have with another lab...There was [no] money there. You could [not] justify saying well, I would like to go over and see so-and-so do this, [and] take it out of your operating expenses (Researcher, AD-17).

But as the research issues became more complex, it was increasingly clear that molecular biology could no longer operate effectively at a small scale and remain competitive internationally. By the time Michael Hayden reached out to colleagues across Canada, in 1988, it was already unlikely that a medical geneticist, working alone, would find both the gene and subsequently the cure for a genetic disease. A more likely scenario for that type of advance was the kind of 'heterogeneous engineering' (Law 1992) that combined medical geneticists and other molecular biologists with viral agents, tissues, genetic physicists, pharmaceutical chemists, gene sequencing technologies, 'purified' mice, and bioinformatics. Like high-energy physics, biology was becoming 'big science'. Lap Chee Tsui, gave a clear description of the differences.

> The way we do science is definitely different now than it was say 15 years ago. Back then it was all very small experiments. And of course things were very primitive too. Medical research has definitely changed- - its scope, the way it approaches things, the knowledge required to run or operate it. It is no longer just a solitary person dreaming up some experiment. It definitely requires quite a lot of help from other people. And if not from other people, from computers and the internet. Before, the literature and meetings were the only things we had. You got all your connections that way. Now the scope has just broadened so much.

> To undertake a biological question, you need engineers and statisticians to come in. A single person can't operate effectively in biology any more. I don't know how to put it. Compare biology to physics. In physics these days, although a few are still doing investigator-driven research in small laboratories, seeking answers to a few very specific questions, the bulk of the experiments are done by big groups, large-scale networks using central facilities. I think biology is moving towards that model. LCT-21-2

Through the NCE program, Canadian biologists were able to aspire to the benefits of big science. NCEs helped the Canadian life sciences earn respect and remain internationally competitive in medical genetics, protein engineering, bacterial diseases, neuroscience, respiratory diseases and other

biological areas. As one of CGDN's founders comments, 'the network has been very good for the field of medical genetics in Canada. It has strengthened the discipline. People regard Canada as being a good place to do genetics' (BR-52). Another network researcher compares his experience in CGDN with his experience in the UK.

> In England, I [belonged to] a large collection of scientists working on a similar topic. The group is so big it's like a force of nature. In that that type of institute you are immersed in science in a way which we can't do in Canada. We don't have the resources. We can't allocate that much money to do focused research of that type. *But that's what we're doing here in the network. We're doing focused research*...The network allows us to bring together a critical mass of people who think about medical genetics problems, from different perspectives. And I think that's a real strength. MW-39

But to begin with, beyond the fact that everyone was doing something to do with human genetics, this 'critical mass of people' was *not* focused. It took time to develop an understanding of what it meant to have a *network* research program and to weave together the projects of individual researchers in some way that made sense.

A Network Research Program?

When the founding researchers were recruited in 1988, they were asked to write up a 'wish list' of projects they would choose to undertake were funding available. Brian Robinson recalls that when Ron Worton visited his lab to invite him to join the network, 'he said, well, have you got projects that you are not doing now but you would like to propose? And I said, oh yes. There are always lots of those' (BR-1). The *desiderata* of individual researchers were then creatively combined to constitute the *network's* research program in the funding proposal.

To reinforce the point: the 'network research program' was an imaginary, rhetorically constructed from *individual* research programs for the purposes of obtaining funding. What was proposed was

simply a continuation and expansion of ongoing individual studies, with some of the expansion being due to network funding. The overall scientific objective of this composite was to study the molecular basis of genetic disease and the genetic basis for susceptibility to common diseases. The major goal, at that point, was to clone the genes responsible for selected genetic disorders. This would evolve in later phases, but in 1988 geneticists were still preoccupied with 'gene-hunting'.

Little changed once the network was operational. Early NCE assessments criticized the emphasis on the individual researcher: 'for the most part, [the science] seems to be too much PI-driven and not enough project-driven' (CGDN-EP 1992: 4). Over the years, however, the network became more astute at shading annual reports and statistical materials to convey the *impression* of integrated research projects and active lab-to-lab collaborations, despite the relative paucity of both. 'We always said we had research projects, because that's what we were supposed to have, but we didn't really. We had people working on different diseases...So it was pretty hard for us, at the end of the day, just to describe what our projects were' (Manager; PS-CS-74).

In the original proposal, individual projects were loosely grouped under seven themed headings: (1) identification of disease genes based on chromosome location, for example cystic fibrosis; Huntington disease; myotonic dystrophy; Wilson disease; (2) mutation and functional analysis in Duchenne muscular dystrophy, retinoblastoma and retinitis pigmentosa; (3) genetics and biochemistry of inborn errors of metabolism, for example in Tay-Sachs and Sandhoff disease; (4) analysis of genetic factors predisposing to common diseases in mice and humans, using recombinant congenic strains in mouse models of human disease, and amplified sequence polymorphisms; (5) the structure of human genetic variation, such as thassalemia in French Canadians, and Tay-Sachs in French Canadians and Ashkenazi Jews; (6). construction of chromosome specific cDNA maps for specific tissues including retinal cDNA isolation and mapping and linkage analysis in diseases

affecting the retina; (7) core technology facilities--the nine technologies offered in Phase I are listed in the next section.

At the end of Phase I, this research program was assessed by an expert panel, based on self-reports submitted by the network and a 2-day site visit by the panel to the network's head office at the end of September 1993.[111] Descriptions of 'themes', 'projects', and 'teams' were accepted at face value as part of an integrated program. The panel recommended trimming some projects, focusing others on more competitive fields of research, and regrouping physicians and scientists into smaller numbers of highly competitive teams (CGDN-EP 1993: 11). But overall, in their estimation, the network had achieved 'outstanding progress'. If there were an international standard in genetic research, they said, CGDN 'might well be on top of such an international comparison' (CGDN-EP 1993:8). The panel submitted a favourable report to the NCE Directorate on October 25 1993. In part, that report read

> The Site Visit Committee noted the outstanding role played by scientists in this network on the international level with respect to the cloning of disease genes and investigating their functions...The Committee was also impressed by the collegiality and networking established among the investigators of the network and noted the importance of the establishment of the core facilities as a catalyst in this process. The Site Visit Committee, therefore, enthusiastically recommends that the network continue (CGDN-EP 1993: Cover letter)

On October 28 1993, three days after the expert panel had submitted its favourable report, CGDN tendered its proposal for Phase II of the NCE program. While building on what went before, the research program was restructured to accommodate the research interests of new recruits. The research emphasis would now switch to common multigene disorders like Alzheimer's and breast cancer instead of the rare single-gene disorders that had been the focus of Phase I. According to Ron Worton, this was a pragmatic decision made because 'if we don't get into the complex diseases, the reviewers are going to wonder why and they're not going to give us funding for Phase II' (RW-

25). Even more pragmatic was the fact that these were *profitable* diseases. As another researcher comments, 'the big pharmaceutical companies are interested in these big polygenic diseases...the diabetes, the inflammatory bowel disease, the sort of things that tens of thousands of people suffer from. Because that is where they are going to make their money' (BR-43).

The eight themes for the Phase II research program were: (1) identification of disease-causing genes; (2) genes and phenotypes; (3) dynamic mutations (novel causes of human genetic disease); (4) genetic analysis of complex traits (mouse models of human disease); (5) genetic epidemiology and population genetics; (6) therapeutic interventions for genetic diseases (new theme); (7) applications of molecular genetics to health care (new theme); (8) core facilities. The two new themes emerged from the new emphasis on *relevance* in program criteria, that weighted translation of findings into practice equally with excellence of fundamental research. Theme 6 was a move into gene-based therapeutics and clinical trials; theme 7 into commercial diagnostics.

By the end of Phase II, the network had adopted in its reporting a language of 'key discoveries', 'breakthroughs', and 'commercial impacts'. They maintained metrics on all, claiming 170 discoveries overall in Phase II, of which 100 were related to common, multigene disorders. Twenty 'key discoveries' were highlighted, including the isolation of the first two Alzheimer familial disease genes by a researcher at the University of Toronto in 1996.

The discoverer was new to the network that year, recruited when he was close to the breakthrough after working on the project for a number of years. Even though the discoverer was a new member who allocated only 10% of his time to the network, CGDN was able to claim credit because he was a member at the time the genes were cloned. Another of the new Phase II researchers identified

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[111] Note that site visits assess all aspects of a network's mandate. In addition to the scientific program, its commercialization activities, partnerships and linkages, management, and training activities are also reviewed.

breast and ovarian cancer mutations in the genes BRAC1 and BRAC2. These too were claimed as 'network discoveries'. On the other hand, it was one of the original PIs—a 1988 'young researcher' who had spent almost his entire career with the network—who discovered a family of proteins that inhibit cell death. This breakthrough was quickly patented and spun-out into a company (see Chapter 7).

In the new theme concerned with therapeutic interventions (#6), researchers had not yet translated findings into applications; rather they had 'created tools for gene-based therapeutics, setting the stage for therapeutic advances in Phase 3' (CGDN-FP 1997a: 11). Progress had been made in biological problems in hematology that had been barriers to the use of gene therapy for blood diseases, and in the use of herpes simplex virus (HSV) as a vector for gene delivery. The second new theme, Genetics in Health Care (#7) demonstrated much more translational progress. For example, headway had been made towards the identification of a direct genetic marker for osteoporosis risk, based on estrogen receptor variants, and of predisposing genes for risk of coronary artery disease (atherosclerosis). In addition, one of the researchers developed a novel technology for rapid, accurate, and cost-effective DNA sequencing of mutations, that was quickly adopted by the Human Genome Project. Also, key advances had occurred in the mutation analysis of the gene for Retinoblastoma (Rb), a devastating childhood cancer of the eye. Because each Rb mutation was revealed as virtually unique, efficient methods for mutation analysis were required. This need was translated by the researcher into mutation diagnostic reagents and kits for cost-efficient diagnosis and cascade testing in families. The investigator comments that, without the network,

> we might never have developed the RB test the way we have. We would have failed, like every other lab in North America, to practically help patients, because the test would have been too expensive, and too difficult. [Without the network] I don't know where I could have got funding to do that research. BG-44

The network submitted its progress report on Phase II , together with an application for Phase III funding, on April 29 1997. In February 1997, the NCE program had been made permanent, but individual networks—including CGDN—had been 'sunsetted'. At that point, the Phase III funding proposal had been in preparation for almost a year. In less that two months, it had to be reoriented towards sustainability beyond the exit of NCE funding. The research program was collapsed into the four elements with the most potential for commercial exploitation[112]: (1) identification of disease causing genes; (2) pathogenesis and functional genomics; (3) genetic therapies; and (4) genetics and health care. A 2-day site visit was arranged for late June. Subsequently, the conclusion of the panel was that funding should continue for the maximum, allowable period: until March 31 2005, subject to mid-term review in 2001. They cited the increasing number of multiple-authored papers across projects as an indicator that 'the group now shows much more evidence of working together as a team', and concluded that the network's evolution had been nothing short of 'remarkable, in that it has not only achieved its stated goals in fulfilling the mandate established for NCEs, but in almost all cases has surpassed them' (CGDN-EP 1997:15, 17).

Weaving together individual strands to give the appearance of coherence, such that reviewers were convinced the network had 'achieved and surpassed' the stated objectives, was a considerable rhetorical achievement. But whether the credit belonged to the network or the individual researchers is an open question. It remains unclear how much of the *network's* research program would have been achieved in its absence or how to calculate the incremental value the organization added to existing individual research programs.

Recognizing these ambiguities, CGDN has recently revised its organizational purpose. Until early 2001, the mission was 'to research the diagnosis and treatment of genetic diseases and to help move

[112] see Chapter 6 for detailed discussion of the network's commercial activities in all three phases

the resulting discoveries into the health care system' (CGDN-AR 1999:1). It now defines itself, more accurately, as an 'enabling organization' and a 'catalyst' for research (CGDN-FP 2001: CD1).

But in one aspect of its research program—the provision of Core Facilities—little doubt existed about the network's contribution. Core Facilities are the advanced technologies and technological expertise that helped network investigators speed research progress and 'breakthroughs'. They were the 'enabling technologies' on which the network's research program rested. The Core Facilities are what legitimate the network's claims, and justify the notion of 'network science'.

Core Facilities: 'Where all the spokes converge'

> The core facilities are a kind of network legacy, I think. They are really the axle where a lot of the spokes converge. FJ-62

The network's core facilities simulated the technological support infrastructure of 'big science'. Easy access to powerful and expensive technologies allowed relatively small labs to undertake ambitious projects and compete internationally. In a priority race to identify genes, where every additional day matters, and where specialized technologies may not be available at a researcher's home university, they enabled resources to be dedicated to a particular project in order to move it ahead rapidly. The network would fund core facilities when it could balance demand and supply; that is to say, when demand for a novel and/or sophisticated 'leading edge' technology could be matched to a principal investigator, ready to act in the capacity of director, and willing to offer that technology to other members of the network.

As discussed earlier, in defining an NCE the network metaphor itself is less than helpful; the more accurate image is of 'spokes' and 'hubs'. This was the case with core facilities. Network researchers across Canada (spokes) drew on core facilities and expertise (hubs). The hubs supplied the network's

material and intellectual infrastructure. Rather than researcher to researcher, collaboration was between researchers and core facility directors--the network's 'master collaborators'.

The nine Core Facilities and directors available in Phase I are listed in the Figure 9 below. At McGill, Ken Morgan built databases for analysing population genetics and Charles Scriver maintained a longstanding cell bank holding about 2100 cell strains. Alessandra Duncan at Queens provided radioactive detection of short probes. At UBC, Rudi Aebersold supplied Protein Analysis and developed improved sequencing reagents and protocols; Frank Jirik and Jamey Marth started to create transgenic and knockout strains of mice, while Greg Lee focused on production of monoclonal antibodies.[113] At Sick Kids, the first facility for sequencing small fragments of DNA was set up in Lap Chee Tsui's lab and was heavily utilized from the start. Ron Worton provided somatic cell mapping, to map cells to specific chromosomes. Peter Lea supplied electron microscopy at the University of Toronto.

Figure 9: CGDN's Core Facilities, End of Year One, Phase I (1990-1)

Facility	Director(s)	Institutions
Computing and genotyping	Morgan	McGill
Cell Bank	Scriver	McGill
In Situ Chromosome Hybridization	Duncan	Queen's
Protein Analysis	Aebersold	UBC
Transgenic and knockout mice	Jirik and Marth	UBC
Hybridoma	Lee	UBC
Electron Microscopy	Lea	UToronto
DNA Sequencing	Tsui	UT/HSC
Somatic Cell Mapping	Worton	UT/HSC

Source: CGDN-AR 1991; CGDN-EP 1993; CGDN-FP 1988

The status of the core facilities developed at the end of Phase II and the beginning of Phase III are shown in the Figure 10 below. By this point three DNA Sequencing facilities were supported. A new large-scale sequencing site at UBC, a small fragments core at UVic, plus the original in Toronto. By this time, Francois Oulette, based at CMMT, was offering training in computational biology

[113] for the importance of monoclonal antibodies as a research tool see Mackenzie, Keating and Cambrosio 1990 and Cambrosio and Keating 1998

(bioinformatics) so researchers could develop skills needed to access the new genomic databases being produced by the human genome project. At McGill, Emil Skamene screened recombinant congenic strains of mice to idenify genes controlling complex traits. Jeremy Squire, at the Ontario Cancer Institute used FISH techniques to map genes and cDNA to chromosomal regions of human and mouse genomes. Mount Sinai's Joseph Culotti isolated mutated *c. elegans* gene homologues of human disease genes. Two new facilties for the provision of genetically modified mice, at McMaster and Mt Sinai, eased the load on Frank Jirik's existing facility at UBC. A new immunoprobes facility was established by John Wilkins, at the University of Manitoba, to develop reagents for cell and molecular biology experimentation. At Laval, Réjean Drouin analyzed the physical state of DNA *in vivo* for information on DNA-protein interactions. Three researchers at the University of Toronto's Banting & Best Institute established a facility to isolate and identify interacting proteins. At Sick Kids, Joanna Rommens identified transcribed sequences in genomic DNA in aid of gene discovery projects.

Figure 10: CGDN's Core Facilities, End of Phase II, beginning of Phase III (1998)

Facility	Director(s)	Institutions
Bioinformatics training	Oulette	UBC/CMMT
Complex Traits Analysis	Skamene	McGill/MGH
Fluorescent In Situ Hybridization	Squire	UT/OCI
DNA Sequencing	Hayden	UBC/CMMT
	Scherer	UT/HSC
	Koop	UVic
Genome alteration in *C.elegans*	Culotti	UT/Mt Sinai
Genome alteration in mice	Rudnicki	McMaster
	Jirik	UBC/CMMT
	Nagy & Rossant	UT/Mt Sinai
Core computing and genotyping	Hudson & Morgan	McGill/MGH
Immunoprobes	Wilkins	U.Manitoba
In vivo DNA analysis	Drouin	Laval/SFA
Protein-protein interactions	Friesen, Greenblatt, Pawson	UT/B&B
Transcribed sequence detection	Rommens	UT/HSC

Source: CGDN-AR 1999; CGDN-EP 1997; CGDN-FP 1997

By the time of the Phase III mid-term review (May 2001) the network had instituted a major shift in emphasis. As described earlier, the network's new mission was to be a catalyst for research advances in the wake of the sequencing of the human genome. 'We are now in the post-genomics age. Many genes involved with pathology have been cloned. The focus now shifts to the proteome and pathogenic mechanisms' (CGDN-FP 2001). The core facilities were rationalized into four clusters: (1) Core Technology Platforms DNA sequence analysis; bioinformatics;[114] (2) Gene Technologies: in vivo DNA analysis; genotyping; transcribed sequence detection; (3) Protein Technologies: immunoprobes; proteomics; (4) Genome Alteration: c.elegans; mouse. As before, the highest demand was for DNA sequence analysis. A partial cost-recovery program shifted some of the burden for facilities maintenance from the network to the users, reflecting the Phase III focus on sustainability.

[114] See (Keating and Cambrosio 2000 on the significance of platform technologies

All participants interviewed agreed that the core facilities, and the skills of their directors, represented one of the network's key legacies.

> **Researcher:** The core facilities were a real catalyst for promoting interactions. We did a lot of cross-country running about among different labs, but a lot of them centred around core facility usage. RG-29

> **Researcher:** I think a key feature of the network has been the [core] facilities, especially the sequencing facility. There is no way I could have got that sequencing done without the resources of the network (DC-12)

> **Researcher:** For me, the high point of the network has been the core facilities. That's been my favorite component of the network. It's been fantastic. RG-89

> **Core Facility Director:** If you want something immediately there is immediate cooperation. When we know that someone is getting close to a gene, and they need this kind of help, we put the secondary requests aside and emphasize this competitive project. LCT-11

> **NCE Selection Committee**: The committee attributed the success of this network to an exemplary collegial exchange of knowledge and its reliance on and extensive sharing of resources, such as the core facilities. Genetic research, especially human genetics, is extremely costly to perform. The committee considered that the sharing of core facilities alone represents a significant benefit from the investment (NCE-SC 1997: 11)

The added value was in setting up an infrastructure for undertaking the technical work that no single researcher could afford to set up independently, in their own labs, but needed to use sporadically. Gene mapping was an example. When the original core facility was set up a backlog of demand quickly accumulated. As the director states, 'if somebody wanted something mapped, they just sent it to me, and it was a given that I was going to do it...If they hadn't been part of the network, they would have had to organize for just one little probe to be mapped with somebody else' (AD-21). As technologies like this became more and more central to research progress, and demand for them increased, universities and hospitals started to acquire their own capacity. At that point, network resources were redirected to other technologies not yet generally available. Core facilities would also be terminated if they were not used enough. For example, as can be seen in the two Figures above,

seven of the ten Phase I facilities had been replaced by the first year of Phase III (1998/9), when CGDN offered 11 core technologies in 15 locations. In between, other core facilities had been started and abandoned.

Between 1991 and 2000, some $8M--approximately 20% of the network's total program funding--was dedicated to core facilities. The system appears to have been a cost effective way of sharing resources. Some researchers argued that *all* the network's resources should be directed into such facilities rather than into the relatively inconsequential amounts of funding allocated to each researcher. One researcher says, '*I* always thought that the majority of [network] activity should go into the maintenance and development of core facilities, to encourage collaboration' (RK-10). Another researcher, the director of a core facility, allocated most of his own network funding towards its support: 'Most of the money I get through the network we've thrown on the core facility--two people and about 600 mice...and various equipment and instruments' (FJ-16).

But core facilities were more than just sharing expensive equipment and biological materials; they also represented the pooling and sharing of expertise. They were an efficient way to leverage the productivity of researchers and ongoing research. Rather than duplicating facilities at different sites, resources were concentrated at one site *and in one person.* As a network researcher explains, 'it is the expertise of the people that is core, rather than the machines' (BG-39). In fact, it is the *combination* of people and machines that counts. 'The core resource is one thing and the experience of the director...and the people who work there, is another (LCT-10). The combining of machines and their directors in this way constituted what Latour (1987) calls a human/non-human hybrid and Pickering (1993:373) describes as a human-machine interface. Such 'cyborgs' can find answers far more expeditiously than any 'regular' scientist or technician would. The issue is familiarity and the way constant practice refines skills. 'I don't want my technician to have to learn a whole technique

to do 10 samples. That is a waste of everyone's time and money, quite apart from the machine' (BG-39).

As technical and scientific experts, core facility directors operated the 'mangle of practice' (Pickering 1993) at the intersection of the network's material culture and moral economy. The material culture of a science is its 'tools of the trade': the machinery and methods of knowledge production, its instruments and experimental practices. The moral economy is the social rules and customs that regulate access to the material culture, establish authority over research agendas, and allocate credit. As Robert Kohler points out 'tools and methods only become productive when they are part of a social system for socializing recruits, identifying doable and productive problems, mobilizing resources, and spreading the word of achievements' (1998:243). The interesting question, according to Kohler, is how material culture and moral economy operate together to make research productive. Pickering (1993:374-5) argues that the mechanism is the 'mangling together' of human agency and performative material devices in 'a dialectic of 'resistance and accommodation'.

With this in mind, the following combination of factors in relation to core facilities might be considered salient. (1) The researcher's requirement to have results processed, say genes to be sequenced. (2) The budgetary resources required to mobilize machines and/or technical staff to do the processing. (3) The power of these machines and technicians to produce inscriptions and standardizations from the data supplied. And (4) the technical and scientific expertise of the core facility director, who manipulates the technologies, even when they resist, to process the experiments. When we relate machines, money, molecules, and magi in this way we are able to perceive modest, 'local' actor-networks of human and non-human elements, that become nodes in the larger actor-network that is CGDN. From their location at the nexus of science and technology,

knowledge and expertise, core facilities represent as much a form of artisanal or craft 'know-how', as fundamental 'know-that'.[115]

Earlier, I referred to core facility directors as 'master collaborators'. This was because, by virtue of their position at a hub, they were aware of and participated in the majority of research projects, and could suggest potentially fruitful interactions between researchers who may have been unaware of each other's work. As one director describes, 'in the early days...I was among a small number of people who were actually connected to most other people in the network...Virtually everybody had been storing up a bunch of stuff that they wanted mapped...I interacted with a lot of people' (AD-8). But, more than that, directors could actually steer the direction of a project and the research agenda.

> By virtue of running a core facility, I know a lot of things that are going on, like new projects and stuff. And I have had input ability to actually participate and to help steer some of the research. A researcher will come to me and say that they want to do something and I say, well, maybe it wouldn't be good to do it that way, it's better if you do it this way. You see what I mean? I can actually play a role in determining the projects. If you're in a core facility well then, everybody is coming to you and saying 'I want to do this, what do you think?' And so you have a chance for having input there. FJ-39-40

In terms of the communal life of science, Kohler (1998:249) argues that three elements 'seem especially central to its moral economy'. These are *access* to the material culture; *equity* in assigning credit for achievements; and *authority* in setting research agendas and deciding what is actually worth doing. Under this definition, which encompasses rules of mutual obligation, I would argue that the central role of core facilities directors makes them responsible for a substantial portion of the network's moral economy.

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[115] For interesting historical treatments of artisanal knowledge see Eamon (1985) and Jackson (2000)

Conclusion

This chapter has presented two contradictory impressions of CGDN. On one hand is a sense of the chimerical: an 'imagined' community with an 'imaginary' research program; now you see it, now you don't. On the other hand is a sense of real durability: established relationships founded on mutual trust and anchored in significant technologies. Is the black box empty or full?

We can approach an answer to that question by looking at the shift between Phases. The addition of new actors into an existing network is always destabilizing. New actors come with their own networks, all with goals of their own. Stability requires the disconnection of alternative associations such that the network becomes the only point of passage. A process of mutual shaping must take place to incorporate the new into the existing actor-network. That integration was successful in the shift from Phase I to Phase II. But in Phase III, the enrolment of new allies (researchers and institutions) seems to have taken place without enough attention to *interessement.* The latter is where network-builders lock-in potential allies by gaining their commitment to a set of goals and a course of action. *Enrolment* without *interessement* creates a fragile network that readily fragments. The Phase III expansion was overwhelmingly strategic, thus translations were incomplete and the voice of the *spokesperson* no longer spoke for all. When there is 'interpretive flexibility' (Bijker 1994), the system's stability becomes precarious: black boxes open; points of passage are ignored; and ambivalence becomes pervasive.

What then to make of *strong* associations that only seem to strengthen with time? Perhaps we can think of networks within networks; layers of associations like tree rings, showing different stages of expansion. The older layers are the most dense; compacted; difficult to *dis*associate. The newer layers are more porous; they can be peeled apart, and peeled away. As well, it is clear that materiality makes networks durable and that more-durable materials tend to produce relatively more-stable networks.

Ideas and talk are ephemeral; to persist they need to be embodied in inanimate materials like machines, books and buildings (Law 1992). The core facilities thus 'anchor' the network in complex and costly technological tools and in the embodied knowledge of the scientists and technicians that operate them. As Law points out, however, durability itself is a relational effect.

Chapter 6: From Science To Commerce

> Truth and understanding are not such wares as to be monopolized and traded in by tickets and statutes and standards. We must not think to make a staple commodity of all the knowledge in the land, to mark and license it like our broadcloth and our woolpacks.
> John Milton. Areopagitica. (1644)

NCEs were funded with the idea that they would, among other benefits, generate products and technologies for profit. Although 'excellence of the research' was the dominant criterion in Phase I selection, and remained a background condition, commercialization and partnerships with the private sector were key to the core mandate. With the sunset of NCE funding looming, CGDN focused on constructing a portfolio of licensing deals and spin-off companies that would provide a stream of future revenues. *All* alternative sources of income were investigated.

In this chapter, I draw on the metaphor of the 'pipeline' that links the lab and the market. According to a recent description the process of traversing 'the pipe' is 'arduous, passionate, rich in ritual, and steeped in conflict and controversy'.[116] I begin by discussing the nature of the pipe and CGDN's position in relation to it. I then review changes in CGDN's connections with its industry partners.

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[116] A network of Canadian social scientists has recently begun a SSHRC-funded study (Financing the Pipe) that explores the moral basis of profit when disease is defined as a market opportunity (what I earlier called 'profitable diseases'). Although there are as yet no results or publications from the study, the funding application (supplied to me by the principals and available on the web page http://www.pipe.ucalgary.ca/) contains powerful and evocative descriptive language

Next, I map the two major strategic shifts in the network's evolution 'from science to commerce': first, in the mid-1990s, bringing some coherence to the commercial portfolio; second, in the late-1990s, with a focus on network sustainability. In relation to the latter, two new initiatives are discussed that ratchet networking to a higher level, by bringing the life-science NCEs together, to jointly finance, 'bundle', and market the technologies in a combined pipeline.

I. Understanding the Pipe

The pipeline metaphor originates in the linear understanding of innovation that underpinned the postwar social contract for science. Even proponents of the 'open science' model on whichMost now view the linear model as an unrealistic depiction of the public/private, basic/applied relationship, especially in 'forefront' sciences like information technology and molecular biology which 'overflow' attempts to contain them. Yet the pipeline metaphor survived the collapse of the linear model; it remains ubiquitous in the 'pharmaceutical talk' of molecular biologists, as well as in the policy discourse. As Godin (2000-3:7, fn.31) argues it is, in fact, 'the spontaneous philosophy of scientists' and has been used in public discourse since the end of the 19th century.

Certainly, 'the pipe' accurately represents the realities of commercial development in the life sciences. In this sector, the pipeline is the 10-12 year evolutionary pathway between the discoverer's laboratory bench and the packaged, brand-name drug or testing kit on the pharmacist's shelves. Once a candidate gene or pathway is discovered in the lab, patents are secured.[117] The patents are then licensed out to biotechnology companies (sometimes the researcher's own 'start-up') which raise venture capital on the basis of the intellectual property then 'add-value' to the discovery. After scaling up and early trials are successful, smaller biotechnology companies often merge in order to

[117] For an interesting discussion of this process, and the inherent tensions, see Mackenzie, et al (1990)

'bundle' their candidate technologies and advance them further. Eventually, a partnership will be entered into with a pharmaceutical company large enough to command sufficient resources to navigate the late-stage clinical trials and regulatory approval process.[118]

The length and complexity of the pipe made the NCE program's expectations of commercial prospects unrealistic. Government had a poor understanding of how long it takes to move 'raw science' out into the market. The federal attitude 'was short termist and linear, very linear. We will do some research, we will have a result and we will make a product and we will sell it. That type of approach' (Policy advisor, ARA-DR-38). As a senior CGDN scientist comments about anxieties on this score

> We were really very scared that it would be impossible to get renewed if they expected us to produce a line of products and a group of connections in five years...It's taken us into the third term to begin to produce what they thought we were supposed to do from the outset. Which was to create the links with the private sector, to produce the spin-off companies, to generate patents and products. And I think that's just about the right timeframe. Ten to twelve years is the realistic timeframe (CS-7-11)

There is no shortage of good ideas; good ideas are plentiful. But it takes a great deal of time, money[119] and effort to steer a discovery from the front-end of the pipe, through myriad competing ideas, to commercial success at the far end of the pipe. 'Ideas are cheap' (CGDN-PS-RW) but most do not survive. 'For every hundred academics that spot something they think is commercially interesting,' says the network's CEO, 'only one will actually get it together to carry it through to the marketplace. The other 99 ideas just languish. They never happen' (PS-RW1-3). This attrition rate was one reason behind concern at government's expectations. Even the pharmaceutical industry was disturbed at federal misunderstandings of the way 'the pipe' worked. As one of CGDN's industry partners stated in the network's first annual report

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[118] This description draws on the 'financing the pipe' materials referred to earlier.

> It is important to realize... what the time frame is likely to be for the emergence of product candidates, especially in the pharmaceutical area. It is important that this [network] research be government funded, *and that renewal of funding not depend on the commercialization of products in academic research centres.* This is the best way to assure that academic research stays at the cutting edge in each field, and generates the unexpected discoveries that can be pursued and developed in strong industrial research centres (Michael Gresser, Merck-Frosst Director of Chemistry in CGDN-AR 1991, emphasis added)

This is an ardent defence of the division of labour in the linear 'open science' model: government funds science; science publishes results; industry takes up and develops results. Under the 'overflowing' model in the Strategic Science regime, the state hopes universities and research networks will become 'profit centres' by patenting and commercializing their own discoveries. This interferes with the traditional division of labour and increases transaction costs for industry (Rappert & Webster 1997).

Because of the risks and costs involved in commercialization, network and university technology managers hedge their exposure by maintaining portfolios of discoveries 'in the pipe', each at a different stage of translation and financing (which are intimately related.) In CGDN, recent activities have focused on the far end of the pipe, as the strategic plan moves from translation to speculation; that is, from early-stage scaling up of research results, to speculation in finance and investment vehicles and venture capital funds.

In the next part I examine the role CGDN's industry partners play in this process of moving network discoveries along the pipe. After that, I examine the network's trajectory along the pipe 'from science to commerce'.

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[119] Conventionally estimated, with little supporting evidence, at around $500 M to take a new drug through clinical trials and the regulatory approval process

Industry Partnerships

Industry partnerships are not as extensive as may be thought from a cursory perusal of program or network documents. Many alliances are listed but most involve minimal commitment and funding. Willingness to sign on to the formal network agreement is an indicator of who is, and is not, a 'real' industry partner. In CGDN's case, only two private-sector partners signed the first formal network agreement: MDS Health Group Limited and Merck Frosst Canada Inc., and only Merck Frosst made a funding commitment--$70,000 a year for three years to provide research fellowships. A third 'industrial partner' signatory—BR Centre Limited—was actually UBC's Biotechnology Research Centre, where three of the network's researchers worked. (This body was also listed as an institutional member.) Calling the Centre an industrial partner was a fiction that helped gloss over the fact that little attention had been paid to the NCE mandate for industrial linkages. Two of the scientific leaders simply imported their longstanding relationships with Merck Frosst and MDS respectively, into the network. As one of the founders comments,

> We didn't know what industry partnerships meant. We needed partners and the government kept saying the partners must contribute in a direct fashion. But, obviously there had to be a desire on the part of industry to participate and some means for them to feel that this is worth their time and effort. They weren't going to join us to make charitable contributions. There was also the concept of in-kind [contributions], which was, in those days, very primitive. We didn't know what in-kind really meant. So this was an extremely difficult thing for us to cope with...nobody knew what the rules should be and nobody knew what the government was looking for. RG-10

The Phase I funding proposal also listed Pharmacia (Canada) Inc., Squibb Canada Inc., and an entity called EuGENE Scientific Inc. as potential partners (CGDN-FP: 1988-S4). The majority of discussion in the section of the proposal on 'Potential for New Products and Processes for Commercial Exploitation' relates to EuGENE. The company was to be the network's research and development corporation, a public/private joint venture between the Hospital for Sick Children at the University of Toronto and MDS Health Ventures Inc. Half-a-dozen pages were given over to

EuGENE's prospects, products, capitalization, and profile in the network. According to the proposal,

> Eugene Scientific is a new company which is determining its goals as a direct consequence of the proposed establishment of this network...It is primarily because of the proposed involvement of network investigators that MDS laboratories has agreed in principle to make an investment in the order of $2 to $2.5M to this company (CGDN-FP 1988: 3.7H)

> Scientists who are part of the network will participate as scientific advisors to EuGENE for development of their gene probes for diagnostic tests [and] diagnostic kits...The scientists in the Network see the establishment of EuGENE as vital to the proper exploitation of their gene probes (CGDN-FP 1988: 3.6G.1)

However, EuGENE proved to be a chimera. Between proposal and legal agreement, the company changed its name, then more-or-less disappeared, apparently despite investments from NRC-IRAP and the MDS investment fund. By the following year no further trace of the company could be found. The Industry Liaison Office at the Hospital for Sick Children believes it ceased operations in 1991 or 1992 (personal communication).

Another phantom company haunted the proposal for Phase II funding, submitted in 1993 (CGDN-FP 1993). The industrial linkages section of that proposal was structured around a spin-off called "NGI" (Network Genetics Inc) that had been formed to commercialize network research. The language of justifation on diagnostics and therapeutics was similar to that used for EuGENE.

> In an effort to create Canadian receptor capacity for CGDN's intellectual property, the network has taken the bold step of launching a new venture [NCE Genetics Inc. or NGI] the first Canadian company focused on genetic diagnostics and therapeutics. This is part of a long-term strategy by the network to capture value in Canada and enhance Canadian commercial contributions in this area (CGDN-FP 1993:1.2)

> The ultimate competitive edge for this company is based on its special relationship with network researchers [which] represents an invaluable source of commercial and market intelligence which will assist in ensuring the development of new IP...The new network venture will begin its commercial activity within the next months and start the process of technology transfer (CGDN-FP 1993:1.8-9)

According to the Phase II proposal, RGI had hired a scientific director. Its financial and business plans would be ready by the end of the year; and the proposal confidently predicted the company would be operational in early 1994. But, as with EuGENE, after the renewal award, further references to NGI ceased.

Subsequently, according to network documents, CGDN researchers developed working contacts with some 21 authentic companies or corporate divisions, during Phase II, of which three were network spin-offs (see Figure 11 below). As can be seen, of the $10.2 M generated from these contracts and contacts, more than half ($5.6 M) came from two 'big pharmas': Merck Frosst ($2.4 M) and Schering Canada ($3 M). Most of the Merck contribution relates to their support for the new Centre for Molecular Medicine and Therapeutics at UBC, while Schering's investment is for the presenilin genes project (Alzheimer disease).

Figure 11: Industry Relationships, Phase II

Company	Cash Inv (C$K)	Principal Investigators	Project
Amgen	130	Dick	Stem cell technology
Apotex	132	Gallie	Retinoblastoma protein
ApoptoGen (spin-off)	1,194	Korneluk, MacKenzie	Apoptosis/ cancer
BioChem Pharma/ Gene Chem	328	Skameme, Gros, Rouleau	BCG therapy, bladder cancer, congenic mice
Connaught	130	Morgan, Skameme	TB/BCG genotyping
Glaxo-Wellcome	25	Hayden	Huntington's disease treatments
IBEX Technologies Inc	70	Scriver	PK treatments
ID Biomedical	15	Jirik	Genetic testing technology
INEX Pharmaceuticals	287	Cullis, Worton, Tsui, Dick	Liposome carrier therapy
Leo Laboratories	50	Rousseau	Psoriasis
MDS-SCIEX	370	Dovichi	DNA sequencing technology
Merck Frosst	793	Triggs-Raine, Jirik	Yeast 2 hybrid/ tyrosine phosphatases
Merck Frosst/CMMT	2,448	Hayden, Jirik, Hieter	CMMT
Merck, Sharpe, Dohme	151	MacLennan	Phospholamban interactions
Millenium	48	Gros	Cloning LPS locus
Myriad Genetics	60	Rommens	Breast cancer
NeuroVir (spin-off)	405	Tufaro	Neurological/ HSV gene therapy
Rhone Poulenc-Rorer	446	Hayden	Lipoprotein lipase therapy
Schering Canada	3,000	Hyslop	Presenilin genes/ Alzheimer
Visible Genetics	30	Gallie, MacLennan	Retinoblastoma/ malignant hyperthermia
Xenon BioResearch (spin-off)	80	Hayden	Gene identification in unique populations

Source: CGDN-FP 1997a: 20

The nature of these relationships, and the degree to which they were attributable to network facilitation, is not clear from the documentation. The network classifies them as 'industry collaborations' for reporting purposes but also refers to them as 'sponsored research' (CGDN-FP 1997a: 20). Apart from the Merck relationship, the majority of these linkages appear to be arrangements whereby network researchers are funded to further develop patented technologies licensed by the company. Where the relationship is with a spin-off company, the amounts reported parallel the funds raised in the investment community to advance the patented technologies.

Despite what we might expect from program and network discourse, little evidence exists of *bench-level* collaborations between academy and industry researchers, working together to advance technologies along the pipe. A CGDN private-sector board member confirms that, to the best of his knowledge, 'there are no network/private sector collaborations in the same sense as there are network/public sector collaborations based on the relationships amongst the scientists' (B-MP-14). The main factor inhibiting bench collaborations is that industry labs are largely concerned with product development while researchers are in the business of knowledge creation. Industry rarely involves itself in collaborative basic or even translational research. With the possible exception of Hayden's work on Huntington Disease with Merck-Frosst, network researchers cite no examples where they have worked directly with researchers in industry. According to a policy analyst, this is the case for the NCE program in general.

> Side by side bench collaborations are few and far between. I can't think of an example off hand. I think that it's rare. Collaboration is [defined] much more in terms of planning and monitoring the research and dealing with disclosures and IP issues and training and so forth. I can't off hand think of an example where 2 people actually sat side by side at the bench and did things. ARA-DR-76

As mentioned earlier, large pharmaceutical companies tend to wait until small biotechnology start-ups and spin-offs have completed early-stage proof-of-concept and development work, then they buy the company. Their unwillingness to collaborate at more basic levels of the pipeline causes a degree of resentment among researchers.

> If you are looking for a disease gene, forget it. Nobody is going to support you in terms of a company, a commercial business. You want to isolate genes for diabetes? They say 'good luck'. But if you already *have* a gene, then, yeah, they are very interested. But the support doesn't come until you have a gene. You have to have a result. LCT-7

> The big ones, the Pfizer's and the Glaxo's of this world, they haven't been anywhere near the network. Despite lots and lots of overtures to try and get them to show some interest...They are not interested in big collaborative projects with basic scientists at all. They want to do clinical trials and they want to do basic research in their own facilities where nobody can see what they are doing and they get all the patents. They don't want to be involved with basic researchers in universities...BR 35-41

> They will come and pick stuff up. If they see you doing something interesting that they like, they will come and try and pick it off you. But they don't want to work with you on it....If you look at the stuff that they *are* funding, a lot of it is clinical trials. So what they are basically doing is they are getting the government to help them do their clinical trials. I mean they are laughing all the way to the bank. I am very cynical about this. I have been at it a long time and I have watched this stuff and I have tried to talk to them about doing some basic stuff and they don't want to do it. BR 35-41

Even Michael Hayden, an indefatigable booster of industry and a close collaborator with Merck-Frosst, admits that support from big pharma is weak. 'I think industry has a legitimate right to serve their shareholders,' he says, 'but at the same time they should think about how they can invest...more in fundamental research. That's my only real criticism...that not enough has gone into basic research, too much has gone into marketing and that doesn't really help us, doesn't fund students, doesn't fund post-docs...We have to push them in that direction' (MH1-48)

Realizing that hands-on partnerships with big pharma were unlikely to happen, and that they had to meet the technology transfer mandate of the program, the network needed a commercialization strategy. After failing to develop one in-house (see remarks above re Eugene Inc and NGI Inc) the network turned to the private sector and hired-in the talent it needed to move its intellectual property into and along the front end of the pipe.

II. Traversing the Pipe

Because scientific excellence was the dominant criterion in Phase I, no one in Ottawa or the networks paid much attention, at first, to how the other mandate points would be implemented. No

one really knew what was required or what the ground rules were. For example, it wasn't until 1995 that the position on holding equity in spin-off companies was clarified. Prior to that, opportunities had been lost because holding equity was not an academic norm. 'Equity was almost like a dirty word in those days. We didn't know if the network could hold equity' (MH2-12). For example, the incorporation of Visible Genetics Inc, now a multimillion dollar public company, was assisted by the network but no shares were received in return. Network managers often cite VGI as a lost opportunity and have always listed it as a network spin-off. However, the PI most associated with the company suggests the network's involvement was minimal.

Attitudes began to change in Phase II, when the weighting of the selection criteria changed, putting commercial development and industry partnerships on a par with research excellence. In order to be renewed for Phase II, networks had to adopt a much more aggressive stance on commercialization and industry linkages. As a policy advisor describes the change:

> The networks just said just stand out of our way, step back, we are coming through. And this is especially the case in the medical networks. They had huge amounts of money on the line and they said, just stand out of the way. We don't want to hear about policy or programs. Forget about the ILO's, they don't know what they are doing. Universities, step out of it. It's ours. We need to sign a deal...tomorrow and it is going to be for hundreds of millions. Just get out of the way. So it really took on a life of its own. ARA-DR-57

At this time, CGDN changed its research direction to one that was potentially much more profitable. In Phase I, the focus was on simple, single-gene diseases, such as Cystic Fibrosis and Tay-Sachs, which are potentially lethal but also relatively rare. These diseases are of little interest to pharmaceutical companies because not enough of the population is affected by them for drug development to be commercially viable. On the other hand, complex, multi-gene disease—like cancer, heart disease, and diabetes—are much more common. They affect a large percentage of the population and are therefore potentially much more profitable. While the idea of turning away from

pure curiosity-driven research towards 'profitable diseases' made some researchers uncomfortable, the network's Phase II funding proposal presented a research program that, while still directed towards basic understanding of the phenomena, took a much more hands-on approach to commercial applications and partnerships. This was the first 'commercial turn'.

The First Commercial Turn

Arie Rip speaks of 'promise requirement cycles' and the pressures for credibility that result when 'promises...become an accepted means of exchange between scientists and sponsors' (1997: 635/10). A lot of promises were made in the Phase II funding proposal about translating network discoveries into commercial applications. When the network was renewed, those promises had to be met.

Principals describe a sense of controlled panic as they struggled, and failed, to come to grips with implications. Recognizing their limits, they went outside the academy and recruited the network's first commercial director. That individual came in with an impressive background in the biopharmaceutical industry, a one-year contract (subsequently extended to a second year) and a large compensation package.

> They paid me a lot more than would be typical for that kind of position. Plus I had a bonus. A big bonus. So at the time, my salary was comparable to a VP in a pharmaceutical company in Canada. In fact they paid me more than the Managing Director. They just decided that I was what they needed. PS-MargM-19

Her mandate was to try to organize the network's scattered portfolio of projects into something that made business sense. She found a group that knew they needed to 'do' commercialization but no idea how to go about it. 'There was zero experience,' she says, 'I mean zero. When I arrived, they

were making a company by pushing all sort of things together that didn't go together. It just didn't make sense. None of them really knew how to get into it' (PS-MargM-2).

They were desperate for someone to bring commercial order to the chaos. A collective sense of relief took hold as the new director took over. She visited all the network nodes and PIs, working to understand their projects, calculating what could be bundled together to create spin-off companies and what might be better as pharmaceutical company collaborations. She asked for, and got, a pool of money—about half-a-million dollars—to invest in projects that needed a little more time in gestation before anything commercial could be attempted. This became the strategic fund, which is still operational, under which scientists can apply for $50 - $75K to advance the commercial viability of their projects. By March 2001, the strategic fund had advanced some $800,000 to network investigators seeking to find commercial relevance in their research (CGDN-FP 2001).

The network gave the new director a free hand in terms of how to use her resources and time and what to focus on. She became the entrepreneur-in-residence, teaching scientists about 'what makes a company, what makes a product, and how you put those things together' (PS-MargM). After talking to all the scientists, she identified a few, solid commercial opportunities. 'It took me about three to six months to figure out where to point the business focus,' she says, 'and then I ended up with, I think, about six different initiatives' (PS-MargM-3). Her strategy was to look for commercially interesting projects that could be realized quickly. From that first top-down assessment emerged two new companies (Neurovir Inc and Apoptogen Inc), the Alzheimer's project (Schering Canada), and a handful of patents. Basically, she elevated the network's commercial ambitions to the biopharmaceutical standard and taught researchers to look behind the science for profitable opportunities.

> The problem is that the scientists see exciting science as opposed to seeing a product....I was looking for products, not for science. To some extent I couldn't care less about science. So I was asking a different set of questions. I was adding a commercial rigour that wasn't there before. PS-MargM-40

To validate her recommendations, she was able to turn to the network's private-sector partners and board members for support. As already stated, the network's private-sector partnerships were not extensive. Even today, linkages are predominantly with small start-ups and local biotechnology companies. But if the quantity of private-sector partners was low, their quality and commitment was high. They participated in the annual meetings. They reviewed network research. And they provided input to the commercial director and validated what she was doing. 'It meant I wasn't alone. It gave me a sounding board and support from [governors] on the pharma side. If it had just been the scientists, they wouldn't have trusted my judgement' (PS-MargM-45).

Less than two years after arriving, the commercial director moved on, but not away. In September 1996, she become Chief Operating Officer of Neurovir, one of the two new network spin-offs she had helped launch. Now, in retrospect, she feels she was lucky to arrive at the network when she did. At that point, the commercial opportunities that had been absent in Phase I were beginning to emerge and she was able to capture them. But when she left little of similar magnitude was lined up in 'the pipe'. 'I didn't see a whole lot more to go capture, which is one of the reasons I was ready to move on. It's hard to maintain a constant pipeline. The question is how do you keep the pipeline going with big opportunities? Because the little ones are just not going to amount to companies' (PS-MargM-50). A senior member of the professional staff confirms this impression. 'In those days, there was not a lot in this network to keep a commercial person fully involved, fully engaged. I think the direction that we're taking now, belies that to some extent. But back then, when she looked back down the pipe, she didn't see anything else coming up' (PS-CS-68).

After the commercial director left, the managing director took on the commercial role and another staff member was recruited to help spread the administrative load. The network was able to consolidate its resources for a few months. Then, in the February 1997 federal budget, came the welcome news that the NCE program had been made permanent—the network had played a lead role in rallying the forces lobbying for its continuation—and the announcement of funding and criteria for the Phase III competition. At the same time, however, the government released a bombshell that few had predicted—a sunset provision. Networks would receive no more than 14 years funding in total. After that they were on their own. Thus it wasn't until Phase III, when the 14 year cap was announced, that science-for-profit became a survival priority for networks. This was the second commercial turn.

The Second Commercial Turn

None of the researchers, managers, or private-sector partners I spoke to approved of the federal exit strategy. All felt it was fundamentally misguided given the length of time needed to develop commercial viability in the life sciences sector, and given the lack of receptor capacity and venture capital in Canada. Basically, the consensus was that bureaucrats simply did not understand the way science works. For example, as one senior policy advisor and NCE board member explained:

> Some of the people in the NCE [Directorate] thought that [at sunset] what the NCEs were supporting, the fields of research, were then 'finished' because they would have put all their ideas into applied research. Which is a pile of BS because fields that are important keep moving, and you've got to stay with them if you're going to stay hot. The people doing policy don't have any experience [of the way science really works, which is really what the issue is. [A person like me] becomes a broker or an irritant, [to tell them how science works.] OTH-FM-12

That said, the NCE Directorate's exit strategy *was* implemented and the network set about dealing with it. In the words of the scientific director, 'we've taken our destiny into our own hands. We

don't really trust the federal government, or the provincial government, or our universities to secure our future. We're now saying we've got to do it ourselves' (MH2-24). The sunset provision meant that the Phase III proposal, covering 1998 to 2005, had to lay the foundation for sustainability beyond federal funding.

The strategic plan for the Phase III proposal had been in preparation since the Fall of 1996. It now had to be modified in light of the policy change. In assessing which existing projects and PIs should be continued or abandoned, therefore, and which new PIs should be recruited, attributes of personal entrepreneurship and commercial potential came to the fore. Researchers were assessed on their ability to initiate and innovate; the numbers of IP disclosures, patents, and licenses they had been involved in; their industrial collaborations, industrial consulting and commercial advising; the amount of industrial funding they had raised; and the new diagnostic and therapeutic products or services they were developing. For any particular project in commercial development, hard questions were asked. Had partners or sponsors actually committed funding? What was the stage of development and funding ratio? And, how coherent was the completion and exploitation strategy? (CGDN-EP 1997). As one of the managers commented, 'we really had to get serious about the business of making money...If the money wasn't there, we couldn't do science. So we had to hustle' (PS-CS-21).

The net was cast wide in an attempt to enrol commercially inclined researchers. If the proposal was approved, the number of PIs would almost double as new centres and programs were added. While network members endorsed the expansion, many did so reluctantly. As discussed in Chapter 6, the changes that were entrained in Phase III fundamentally altered the culture of the network. Until that point, a relative balance had been maintained between traditional academic norms and new commercial values. Now, commerce came to the fore and collegiality suffered. Many of the founding PIs found the changes distasteful and distanced themselves. As one puts it, 'the emphasis

on profit as what we have to do, makes me feel uncomfortable...I regret the intrusion of profit and its more ugly form, greed, upon academic culture. I consider that a loss' (CS-17). Another is upset that network funding to PIs is now so contingent on market values: 'Basically the network is now in the mode where...they are particularly looking for stuff which is commercial. It has got to have a commercial application, or else!' (BR-27).

The revised proposal was submitted May 1997. In September 1997, the network received notification that it would be renewed for Phase III. A series of changes followed quickly. The founding Managing Director resigned effective February 1998. The network was subsequently incorporated as CGDN Inc., commencing the first fiscal year of Phase III as a not-for-profit corporate body. And, to complete the transition, a new Chief Executive Officer was appointed with a mandate to make the network self-supporting by 2005.

Recruited on the basis of his past experience in forming companies, and taking them to market, the CEO arrived on site in June 1998. From the start, his approach was strongly oriented to profit, and the rapid achievement of his mandate objectives. His first change was to the organizational structure: the 'ephemeral informality' discussed in the previous chapter. The flat, collegial arrangements that had been in place for network management were replaced with a hierarchical corporate framework, one that reflected industry standards rather than those of the academy. His own title–Chief Executive—is one indication. As one of the professional staff commented, a few months into the CEO's appointment,

> He is not a network man; he's not an NCE man. He's not going to have those collegial kind of relationships because his mandate's different, and because where he's come from is different. And his style is different. He's much more of a lone player. PS-CS-51

If the network was to aggressively commercialize its technology and interface with the private sector, the CEO believed, it needed a corporate facade. It should be recognizable by industry as a formal entity. Incorporation had been considered in the past but rejected. Now, the CEO wanted the network to have the practical ability to sign contracts and hire its own employees. He wanted an organization that was transparent to the finance and investment sector, and an organization that was disciplined by its structure.

> Eastern philosophies will say that with structure comes freedom. And I believe that. So to me, it's a natural extension for people, after a while, to realize that having a corporate structure will simplify a lot of relationships. Incorporation in no way restricts, in fact I would argue it enhances the ability of the members to interact more freely, in a true network sense, because you're not always wondering what exactly the framework is that you're dealing with. PS-RW1-44

That in place, he recruited as his commercial director a young lawyer-scientist with a background in putting together biotechnology deals. A three-pronged strategy to ensure sustainability was then developed. It was approved by network members at a strategic planning meeting in June 1999, one year into the CEO's term. At the same meeting, the network's scientists agreed to give up 10% of the current research budget, if required, to enhance strategic activities.

The first innovation was an aggressive emphasis on commercial development. Finding likely prospects was the task of the new commercial director. Scientists had to be motivated to recognize the commercial potential of their discoveries if the pipeline was to be fed on a continuous basis. Says the commercial director, 'Probably 90% of the best of the best of the best scientists in the country belong to this network. If *they* can't come up with technologies that can feed the pipeline then I don't know who can' (PS-HC-17). In earlier phases, the tendency had been to attempt to build companies around genes. That hadn't worked particularly well. The legacy was a clutch of small companies, built around single technologies, only a couple which could stand on their own. The new

goal was to look for platform technologies with broad applications, and scientists who would stay the course.

> It has to provide a solution to a gap in the market. It has to have more than just the potential of one product. If it is a service it has to be a service that is lacking or missing in the industry. You have got to have a scientist that is going to be entrepreneurial and stick with you along the way. You have got to be committed. And the business plan has to make sense. There are lots of things but the primary things are the technology and the commitment of the scientist. PS-HC-12

The stance was proactive. Instead of waiting for scientists to call, the approach was to 'do rounds', visiting every lab at least once a year. That helped the commercial director understand what was in development and allowed her to suggest a commercial spin where appropriate. Another strategy was to solicit for commercial prospects at the annual scientific meeting. 'Me just being there and being in their face is an important reminder to the scientists that there is a commercial aspect to their work' (PS-HC-12).

Once the commercial prospects were identified, the CEO built partnerships with the finance sector to exploit them. As a pharmaceutical industry partner notes, 'he's a broker between the scientists and the money folks. It is something that he does quite easily' (B-MP-26). He moved to ratchet up the commercial profile of the network, by forming a for-profit company. This would allow greater scope in terms of generating revenue opportunities and allowing the network to partner with venture capital firms, in order to receive a larger share of the profits from intellectual property commercialization. In the past the network had only been a facilitator; now it would learn how to be a full partner in financing the commercialization process.

But this could not be done without the co-operation of the other institutional partners, the universities and hospitals who owned the intellectual property. In general, the CEO was unimpressed with the commercial abilities of public institutions. The public system is not by

definition an entrepreneurial system, he says. 'It was set-up for the mail to be delivered, for civil and criminal justice to be administered. It's not based on innovation, inventiveness, entrepreneurialism. It is to provide some basic infrastructure to allow society to do the day to day things' (PS-RW1-36). As a result, he argues, asking the public system to act entrepreneurially is misguided.

According to the CEO, it is the willingness of entrepreneurs and venture capitalists not only to pledge risk capital but also to manage the complexity of translating publicly funded technologies to the marketplace, that justifies transferring these technologies from the public to the private domain and allowing the venture capitalists to realize high returns. How else will university technologies be translated into therapies and find their way to patients, he asks? He entertains no scruples about privatizing public knowledge.

> For people who think there's too much commercialization, I'd suggest that (a) they have never been involved in commercialization, and have no idea of the difficulty in managing the hundreds of steps that all have to be successfully implemented in order to get that commercialization. And (b) if you were to strip away all the commercialization of scientific findings in the last 50 years, we'd still be suffering from plagues. Sometimes you commercialize a piece of physics which later turns out to allow laser operations on cataracts. . So, how can you arbitrarily say you're not going to commercialize technology? (PS-RW1-1)

In the real world, entrepreneurs do not draw boxes between 'public' and 'private', between 'science' and 'technology', he says, and neither should society. 'The public funding of scientists can lead to cures for tuberculosis, polio, and other diseases. But we also need a recognition that the process requires an entrepreneurial component to result in cures or therapies' (PS-RW1-37).

The need to fill the pipe with public discoveries is far greater in a knowledge-based economy than it was in a resource-based economy, he says. 'Where before you had prospectors that went around the North, picking up rocks, and looking for gold, you've now got prospectors going around universities, picking up ideas, and looking for knowledge' (PS-RW1-12). Because the network has a

track record of having done excellent science and of having put some of that science into the pipe, the CEO felt he could go to these prospectors—venture capital companies—and forge partnerships where universities had failed. Indeed, this would be essential if the network was to survive beyond federal funding. So shortly after his appointment, he visited all the partner institutions in the network.

> We sat down with all the universities and medical institutions. We explained that all of the money we derive for research purposes flows out to researchers at their institutions. So in 2005, when government funding ends, if the network disappears, their researchers are going to see a negative impact. Then we explained what we're going to do to ensure that the network continues...And once they understood what we were doing and why we were doing it, they were very supportive. PS-RW1-7

The second component of the CEO's sustainability strategy was a plan for building a $20 million endowment fund through a federally registered charitable foundation. 'We have a noble cause,' he says. 'We are seeking to find cures for a variety of gene-based diseases that inflict a lot of suffering and cost on society. So we can take that and we can build a Foundation around that' (PS-RW1-5). A now-retired private-sector board member was asked to head up the effort and the Foundation appointed its own executive director. They used as a model a similar entity established by Neuroscience, a network that was not renewed for Phase III, and that had established various funds and entities in an attempt to ensure its continuation.

While willing to invest energy in the project, the board member in charge of the new Foundation had doubts that he could raise enough funding to help ensure the network's survival beyond NCE sunset. 'We don't have the same pizzazz in fundraising as the Children's Hospital or the Cancer Society or the Alzheimer's Society,' he comments. 'We touch all of those diseases and many more, but somehow genetic research is not something that you can put your hands around' (B-MM-3). As of March 2001, the foundation had a 'paper value' of $6.2 M, of which $1.3 M was cash, with the balance represented by an estimate of the value of equity in CGDN start-ups (CGDN-FP 2001: 22).

With the decline of stock market interest in the life sciences sector, the values are likely inflated. Even if achieved, a $20 million endowment would produce a maximum of $1.2 million in annual revenues at best. That represents only 25% of the $4.5 million in annual federal funding that will be lost.

So something else was needed to generate ongoing revenues and this was the third, most challenging, and controversial leg of the sustainability strategy. Traditionally the network had enhanced its ability to earn an interest in intellectual property by helping researchers develop spin-off companies, and by finding partners for the commercialization of network intellectual property. Now, through its for-profit company, the network would venture directly into the world of finance and investment. Not only that, it would do so in partnership with the other two 1989 life-science networks, which were also facing funding sunset. The CEO's innovation was to bring together the boards of the three original life-science networks (CGDN, CBDN, PENCE) and get them to agree to form their own venture capital fund—Excella Ventures—and seek investors jointly.

An investment fund, he argued, would provide the kind of profits that would assist all three towards sustainability. The vision was of a self-funding entity, searching for profitable therapies for the same disease state from three different entry points—genetics, protein engineering, and bacteriology. In this way, says the CEO, the possibility existed of generating a critical mass of intellectual property—the kind of critical mass that would eventually grab the attention of big investors and big pharma.

The logistics were as follows: Each of the networks has an incorporated for-profit company. The three network companies are equal shareholders in a federally incorporated entity, Excella Ventures Inc. Excella Ventures, under CGDN's leadership, manages the development of the Excella Life Sciences Equities Fund. In the first instance, the goal was to raise $60 million which would be invested in second-stage financing for 15 companies over three years. This was the niche identified

for the fund, since early-stage financing—the first $500K to $1 million—was fairly readily available in Canada.

> It's very difficult to generate that next round of money, the second stage financing of $5 to $10 million. That's the market that we feel we can add significant value in. The reluctance to invest in that second round is because people know that for every hundred companies that get seed financing, 10, at max, are going to survive. And so, how do you pick the ten that have a hope of surviving through the next round? Most people do not have the sophisticated knowledge base to do that. We, at the networks, do. I mean if we can't evaluate the potential of [these] technologies, who can? There likely isn't anybody else in the world that can do it as good a job as we can, because if we don't have the world leader in the field, somebody in our networks will know who the world leader is. PS-RW2-12-16

Although comprehensive and ambitious in scope, the CEO's 3-tier strategy met scepticism and resistance both within the network community and externally.

Resistance

Several of the network's more senior researchers actively disliked the corporate façade. As one says, 'that positive feature of being able to meet with other people and be part of a scientific society, it's just not the same when it's a corporation' (DC 41). The new emphasis on profit was viewed with suspicion. In the words of one long-term member, 'it isn't what the scientists want. It isn't how we live or think or want to see things go forward' (BG-33). Another states, 'I never dreamed that we would be creating a foundation. To this day, I'm not sure that I agree with it. Or with the creation of a venture capital group. It never occurred to me that we would be sponsoring the creation of a venture capital group. And with the other networks too' (RW-47).

Some researchers felt that the new goals had little to do with providing health for Canadians or advancing the research frontier and everything to do with stock market speculation and commercial

'bubbles'. The resistance was not to commercialization as such, but to the *type* of commercial activity, which was beyond the comfort zone of many.

> They were looking to fund the 5 most fresh, great, fantastic ideas as strategic business opportunities. You know, big and fancy and harebrained and virtual with little potential for contributing to health. That is where they were going to put all the [network's] money. And they were turning down translational research[120] opportunities that would be of immediate and obvious impact! A simple little thing that actually turns a profit, that actually does a job and delivers health care, they didn't want to bother with.
>
> I got really mad because of it. To me the network's responsibility was *exactly* to do something practical and useful and now. And not go off into funding harebrained, high-risk stuff. To me, the best way to ensure [sustainability] is the safe way. You have to have some safe, solid things that are going to be solid, real companies. Old fashioned ones that do something and pay dividends. Not ones that just get sold on the stock market and make money in a phony way. That is really scary and that is not how we should build the strength of our network. BG-22-26

Some informed external observers detected a certain amount of hubris in the network's plans, especially in regard to Excella. As one private-sector funder comments, 'the networks are in danger of falling into the trap of forcing the scientists to raise money for its own sake, as opposed to using money to create something of lasting value' (PS-DS-35). One of the problems, according to the same observer, is that the networks are trying to do everything at once. 'They are trying to become commercialization vehicles, as well as science vehicles, as well as management vehicles, and that's doing too much. You can't do all of that' (PS-DS-43). In his opinion, while networks have strengths in identifying technologies that might be commercialized and in managing the arrangements to get them commercialized, 'they are not going to be able to do it all, nor are they going to be able to retain all the value. And if they think that by starting their own little captive fund they are going create a lot of value, when today the pharmaceutical companies don't even have enough money to do it, that seems a bit ridiculous' (PS-DS-43).

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[120] This concept of translational research is important and I will be returning to it.

In summary, pressure to become self-supporting changed the operational strategies of CGDN. Many in the network were uncomfortable with the new direction. 'People have been worried about commercialization for a long time but I think we are starting to see some of the dark side of that come out. It is very clear that there is a down side to it' (ARA-DR-21). Others worried that the focus on commercialization would crowd out fundamental research. Nevertheless, the CEO was convinced of his path to sustainability and enjoyed the support of his board and scientific director. The latter described the strategy as creating 'a legacy that is totally independent of government' (MH2-24). As will be seen next, however, the desire for 'total independence' did not stand up to scrutiny.

III. Third Turn?

In 2000, PENCE, a more commercially aggressive network than CGDN, analysed the income it could expect to generate from spin-off companies, based on existing performance, and concluded that sustainability was not achievable by 2005, if at all (PENCE 2000). 'There's just no way these companies have the financial capacity to fund the fundamental research, because they are not big enough. It's as straightforward as that' (PENCE informant, OTH-FM-9). Without fundamental research, the networks would have difficulty retaining their integrity as independent entities. Increasing focus on commercial goals was already distorting the accumulation of knowledge on which the whole edifice rested. 'Eventually the program became so focused on private sector involvement that, in a way, we were simply doing applied and strategic research with and for companies. And we were basically undermining our fundamental research base' (PENCE informant, OTH-FM-8). This same conclusion was reached almost six decades earlier, in the Bush report: 'applied research invariably drives out pure. The moral is clear: it is pure research which deserves and requires special protection' (Bush 1945:83).

As a solution, the Excella concept received a generally negative response from universities and ILOs, as well as the finance community (Henderson 2001). Even CGDN's two partner networks were lukewarm. As one representative states, 'the [Excella] fund is sort of a daydream...but it won't work. Investors aren't going to put their money into something which hasn't been proved out' (OTH-FM-10). So although Excella continued[121] the CEO's focus on speculation and short-term profit started to look *passé.* As a group, NCEs initiated planning on two broader alliances: broader in terms of the number of networks involved, and broader in terms of the underlying approach to knowledge development.

The first was a task force of board chairs, working to develop scenarios under which government could continue to provide core research and training funding to successful networks beyond 14 years, rather than have them shut down.[122] As of June 2001, no decision had been made, but the NCE program director appeared receptive and willing to rethink the issue.

> I'm looking at the networks as an investment that we make. The Canadian government invests in these organizations. So let's look at it from the point of view of an investor. Like an investor we spend a lot of time looking and selecting, we want to be careful where we put our money. Once we've invested then we work *with* the networks and that's the role that I want us to play. To make them grow, make them successful, help them. JCG-1

Certainly, an argument can be made that if it made sense to fund NCEs in the first place, as part of the strategic policy regime (see Chapter 2), then it makes sense to continue funding them. It would be unreasonable to expect desirable policy outcomes to continue once the state withdraws. 'Scientists pick up the resources and run with them, and only in exceptional cases will they continue with newly initiated research lines after the funding stops' (Rip 1997:635/10).

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[121] Excella made its first $500K investment in December 2000, in Vancouver-based Neuromed Inc (CGDN-FP 2001)

[122] The program set up a 'Research Management Fund' (RMF) to provide transition assistance up to $500,000 over one or two years to sustain networking activities for NCEs at the end of their funding window.

The second broad alliance was initiated by Dr Fraser Mustard, head of The Founders' Network, chair of PENCE, founder of CIAR, founder of PRECARN, architect of the Ontario Centres of Excellence, and prime mover of the NCE concept. It shows his characteristic flair for bold policy measures. Although still in the very early planning stages, this initiative could prove to be an authentic reconciliation of public and private interests in the Canadian biosciences.

'Funding Galileo'

> How in the hell would you do a costs/benefits equation on Galileo? It's basically stupid to try to justify the NCE program economically. (Fraser Mustard, OTH-FM-19)

As described above, the chances of any one network providing for itself from commercial revenues was highly unlikely. Even to attempt such a thing would be to seriously compromise the research endeavour. In Mustard's estimation, it was tantamount to expecting Galileo to make a profit. He believed the commercial approach of the NCE program was misconceived from the start. It had not formed part of his initial conceptualization.

> We had proposed national networks in the *fundamental* sciences. At that stage, it wasn't conceived to be directly linked to industry. ...But each time [the bureaucrats] kept making it tougher and tougher in terms of those commercialization requirements, because I guess that's how they were marketing it with the powers that be in the public service...And. as we found out with PENCE, eventually the program became so focused on private sector involvement that, in a way, you were simply doing applied and strategic research with and for companies. And you were basically undermining your fundamental research base. And if you don't have a good fundamental research base you really don't develop any effective strategic or applied research programs (CIAR-FM-6-8)

Even in the United States, Mustard argues, industry does not fund the biosciences; that mandate belongs to the National Institutes of Health. Inevitably, he says, once a program becomes dependent on business-sector financing, it is steered towards an applied science mode, and 'probably ends up being driven by huge pharmaceutical interests, which does not really give you the base for your

fundamental research' (OTH-FM-17). Without intervention, suggests Mustard, this is the fate that awaits the three original life science NCEs after 2005, and the others as they graduate from the program. Basically, he says, 'although we've moved on macroeconomic policy for innovation we've been pretty brain-dead about the microeconomic issues' (OTH-FM-9).

What was needed was both a division and concentration of labour. In the first instance, NCEs and industry should each do what they do best: basic research and development respectively. A similar conclusion was recently reached in Sweden, where the 'Research 2000' plan recommended that universities withdraw from the role of making *direct* contributions to industry. The position is essentially grounded in evolutionary economics and the 'open science' model (see Chapter 2). In the second instance, instead of each network (and each university, for that matter) maintaining their own commercialization directorates, the tasks should be centralized. It made more sense, economically, to *jointly* develop and manage an intellectual property portfolio, expand relations with venture capitalists and potential licensees and, importantly, support proof of concept work. More beneficial still, it would facilitate the 'bundling' of technologies. Technology managers interviewed, whether in universities or NCEs, all mentioned the 'one-product; one-company' phenomenon and acknowledged that bundling would solve the proliferation of unsustainable start-ups, but they did not know how to achieve that solution.

Excella was a start but Mustard sensed that it was situated too far down the pipe. As well, the profit orientation would get in the way of what he wanted to achieve, which was *pre-competitive* funding for proof-of-concept work. 'It is one thing to build a network of talent and produce ideas,' he says, 'but how do you know the ideas will work on a larger scale? Venture capital won't fund that, because there is no profit in it' (OTH-FM). What was needed was something *between* basic research and commercial development, a co-operative (rather than for-profit) body, that would fund translation

and scaling up. Mustard drew for inspiration on the PRECARN model he had helped establish in 1988.

PRECARN is the Pre-Competitive Advanced Research Network, an industry-led, Ottawa based consortium of 39 companies. PRECARN companies work together to develop receptor capacity for advanced basic research in robotics and artificial intelligence. The fundamental science is drawn from IRIS (Institute for Research in Intelligent Systems), by far the largest and most complex NCE, for which PRECARN acts as host institution. (IRIS is one of only two NCEs that are not hosted by a university or hospital). In the same way that PRECARN provides a pre-competitive platform for IRIS, Mustard proposes that CNBioNet (Canadian Network for Biotechnology Commercialization) will provide a pre-competitive platform for the life sciences NCEs. The draft proposal suggests that without CNBioNet, or a similar entity, the world-class research expertise developed in NCEs will continue to be licensed to foreign companies. Canada will lose the benefit of its investment in the program (this is a tacit recognition of the failure of the 'Benefit to Canada' clause in NCE agreements).

CNBioNet's backers are proposing a government/industry partnership. Mustard considers government funding essential to offset the risk aspect of life-science research. Lobbying by the initiative's powerful supporters has caught the attention of Ottawa and the corporate sector. The proposal has been presented to a meeting of NCE boards of directors, receiving a mixed reception; ILOs are also dubious (Henderson 2001:7). As a centralized vehicle for commercialization, CNBioNet represents yet another threat to their single-institution focus.

Mustard is not actively seeking funding until in-principle approval is received. But once that is in hand, he anticipates that $15 M—half from government, half from industry—will be enough to launch the start-up phase of the project. The purpose of the exercise is to take a more rational, long-

term approach to the issue of NCE sustainability and to renew the vision of the program's architects: to build both globally competitive *fundamental* research networks, and the receptor capacity to exploit their results.

Conclusion

In this chapter, I described the changes CGDN has initiated in its trajectory from Phase I through Phase III in response to program demands for commercial relevance. But more than any other factor, it was the federal policy decision to limit NCE funding to 14 years that shifted the attention of CGDN officers from 'science' to 'commerce'. Ambitions progressed from relatively small-scale licensing and start-up activities near the front end of the pipe, to large-scale speculative ventures much further along, involving investment funds and 'high finance'. This latter move upset the delicate balance in this network between public and private interests, and basic and applied science.

When I first conceived this study, I thought of 'privatization' in terms of the transfer of assets from one sector to another. The boundaries seemed fairly clear: 'public' knowledge on the one hand; large corporate entities on the other. The image of cartoon capitalists might have come to mind: portly moguls clutching bags of money while pillaging the commons. But the privatization that takes place in NCEs—and in universities too, for that matter—is a far more complicated affair. In the majority of cases, the transfer from 'science' to 'commerce' is achieved internally. A component of the public transforms itself into a component of the private to commercially exploit a discovery. A patent transforms some piece of public knowledge into a private commodity. A public research organization disappears behind a corporate façade. Solutions to disease become a locus of profit. Beyond this 'Russian doll' nesting of public and private (Starr 1988), we see the triumph of neoliberal attempts to 'marketize' the public sector. There are no cartoon capitalists here. If capital

is the enemy, the enemy is within. The move has caused some disquiet in policy circles. The NCE program's architect has intervened to protect the basic science component of these networks. Success in this endeavour would return NCEs from the 'overflow' model to the 'open science' model.

The next chapter explores the tensions between science and commerce at the human level. Without the individual researcher, NCEs would have no technologies to exploit. Much depends on the way the *scientists* locate their loyalties to the different institutions in which they are enmeshed: their hospitals and universities, the network, and 'science' itself. I follow the researchers as they undertake, or choose not to undertake, 'adventures in the nature of trade'.

Chapter 7: Adventures In The Nature Of Trade

The organization of this chapter draws on an archaic and useful term with roots in taxation law. *Adventures in the nature of trade* are 'in-between' kinds of enterprise, involving schemes for profiting from a type of activity that is not part of a taxpayer's regular business. An example might be the purchase of a painting by someone who is not an art dealer, with the intention of eventual resale at a profit. I argue that the academic life sciences, today, are just such an adventure, somewhere between public and private enterprise, somewhere between the discovery of knowledge and its application. The commercialization of molecular biology was originally, and still remains, an activity that academic scientists engage in almost accidentally, 'on the side' of their other university activities.

As discussed throughout this dissertation, participation in commercial activities comes at the price of an ambivalent acceptance that scientific ideas are 'intellectual property' that may not be disclosed until 'protected'. As Rip (2000) has discussed,[123] scientists in commercially significant fields are cousins to the Renaissance 'professors of secrets' who used their artisanal knowledge to collect and develop 'recipes' for sale in the marketplace or to sponsors. 'They had to advertise themselves and their knowledge in order to create some visibility. However, at the same time they had to keep their

[123] Relying on Eamon 1985; see also Jackson 2000

secrets in order to maintain a competitive advantage over other such 'professors'" (35-6). Present-day professors, socialized on the open-science model, are faced with a similar dilemma and respond in two ways: they either embrace change, or reluctantly accommodate it. The latter attitude is particularly apparent among 'the old élite, the spokespersons for established science' (ibid). As reported in this chapter, I found precisely these responses among CGDN scientists towards 'adventures in the nature of trade'. As a beginning, I examine researchers' perceptions of their relationship to their 'home' institutions.

I. Localizing Cosmopolitans

One way of understanding the situatedness of individual researchers is by retrieving the sociological concept of locals and cosmopolitans. Merton (1957) applied the term to community leaders; Gouldner (1957) adapted it for professionals in organizations, and Glaser (1963) extended it to scientists in industry. The local/cosmopolitan tension is between location in physical space (where you work; who you work for) and conceptual space (profession-wide expert knowledge and codes of conduct). Academic scientists are generally thought of as cosmopolitans, whose allegiances are to the 'Republic of Science' rather than their institution. 'A cosmopolitan orientation...manifests in their working for professional goals and the approval of colleagues throughout their professional world, in focusing on a professional career, and in a concomitant lack of loyalty to and effort for the organization' (Glaser 1963). Industry scientists are seen as locals, whose allegiances are to their company.

Both are ideal types and Glaser argues that scientists are a blend of the two. The relative emphasis on one or the other depends more on the motivation and direction of their work effort rather than on any intrinsic qualities of the researcher or the organization. Thus an industry scientist working on

basic research would be cosmopolitan in orientation while an academic scientist working on commercial applications would be local. This seems intuitively correct. Every scientist I interviewed was a principled 'cosmopolitan' but, as I describe in detail later, all maintained a portfolio of research projects, some of which were more 'local' than others. Kornhauser (1962) also recognized a 'mixed type', loyal to science but interested in 'facilitating the utilization of technical results'.

Initially, I did not understand these distinctions. Before starting my research, I thought of scientists as 'working for' the universities and hospitals that paid their salaries. Part of the problem, it seemed to me, was that networks were not compensating these institutions for the time spent by researchers on network affairs. In other words, I mistakenly thought of network scientists as 'locals' with displaced loyalties. In interviews it quickly became clear that scientists perceive their situation quite differently.

Academic scientists 'work for' no one but themselves. This is the embodiment of what we earlier referred to, in Chapter 2, as the 'ideology of the autonomous researcher' (Godin 2000-3). They are 'strongly independent' (FJ-33) and prize their autonomy. (loss of autonomy is one of the major problems scientists perceive with commercialization, see next section). Rather than employees, academic scientists are 'franchise owners' (RG-45), funding their research through grant sponsorship and conducting it under the auspices of the university. Thus the university is simply 'a place to be a scientist' (RG-45); its role is to provide facilities and institutional infrastructure. The university has never exerted moral suasion to pursue a particular research direction (RG-63) or exercised domain control over scientific activities (RG-53). That is not within the purview of the institution, which recognizes that cosmopolitans control their own work. However, researchers recognize a reciprocal obligation to teach classes, supervise students, and give other types of service to the university in return for their salaries and infrastructure support.

For medical researchers, the university is already at one remove. Most medical scientists are located in hospital research institutes and health science facilities that only nominally fall under university control. Hospital institutes have taken over a lot of basic research, or have grown where the university had no capacity to grow. The relationship between 'practical' hospital research and 'pure' university science is full of tension and boundary work.

> The medical genetics department at the university wasn't doing any stuff that was actually relevant to any diseases [and] the biochemistry department was doing very, very basic stuff on protein folding. They are getting money from the medical research council. But it is nothing that can be directly applied (BR-61).

> Of all of the peer-reviewed dollars for basic [medical] research, I don't know what fraction is in the university's medical sciences building, [rather than the hospitals]. But it is not the majority. I would guess it might be 20% (BG-10)

The network was an additional layer that rode on top of these existing complexities. It can be thought of as a national subset of 'cosmopolitan science'. Hence scientific 'franchise owners' saw no conflict in becoming involved in network activities. *Nothing changed for them.* 'In my career as a scientist, which went on for 15 years before the networks existed, I don't see any difference whatsoever, at all, *I mean at all*, in what I do in terms of the relationship to the university, versus the relationship to the network (RG-46). Scientists continued conducting their research as before, with research council funding and university infrastructure support. But they got 'far more than infrastructure' from the networks, which provided 'an atmosphere, an ability to move into this new world that the university had never provided' (RG-64).

While recognizing the continued relevance of the local/cosmopolitan distinction, I argue that the new propensity of researchers to undertake 'adventures in the nature of trade' tends to turn it on its head. I propose a new three-part typology that goes some way towards recognizing the way research now 'overflows' earlier definitions.

II. Towards a new typology

Some network scientists have become seasoned adventurers in trade. Under the local-cosmopolitan typology these researchers would be classed as 'locals' because of their industry affiliations. But 'locals' seems too parochial a term for participants in the 'global knowledge economy'. Instead, I call them *merchant scientists.* These are the researchers who incorporate companies ('start-ups' or 'spin-offs') to license the intellectual property emanating from their academic labs. They are intimately involved at all stages of 'the pipe'—raising venture capital, proving out the concept, scaling up for clinical trials, and promoting the resulting products. For much of the time they straddle the public/private divide with panache but somewhat uneasily, dancing backwards and forwards, skirting conflicts of interest and commitment, and not-quite-successfully differentiating between their academic and commercial roles. Like merchant banking, 'merchant science' is not open to everyone. Merchant scientists need the scientific capital and entrepreneurial flair to successfully negotiate the very different social worlds of bench science and commercial enterprise, the academy and the market. The pressures are considerable and straddling this boundary does not appeal to many researchers.

Most network scientists make only token forays across the public/private divide, just enough to meet their network membership requirements. These are Rip's 'reluctant accommodators'. They have little appetite for commercial adventures; the frontiers they pursue are in the laboratory, searching for new intellectual territory to add to the public sector research base. They are the 'cosmopolitans' we spoke of earlier. Polanyi (1962) called the republic of science 'a society of explorers': a system rooted in tradition that at the same time cultivates radical progress. Both 'cosmopolitans' and 'explorers' carry connotations of 'wide-ranging' and that is certainly the case, in the intellectual sense, for the network scientists I studied. But in comparison with merchants, these scientists seem more anchored in place. I call them 'settlers'. Settlers are uncomfortable with

increasing demands for commercial exploitation. Their work is grounded in a different economy—that of free intellectual enquiry uninhibited by commercial pressures.

The space between merchants and settlers is occupied by *translation.* The term is ubiquitous in the discourse of medical researchers and research funders. It describes the work that moves 'between the bench and the bedside'; i.e. that turns basic discoveries into therapeutic techniques and products. The same move translates science from the laboratory to the market: the process that translates science into therapeutic uses also translates it to profit. Of course, *translation* is also a key concept in actor-network theory but as used by *practitioners* it takes on a sense not yet theorized in the literature.[124] I believe this is theoretically important and will return to it in the next chapter. The class 'merchant scientists' contains the other two classes: merchants are always translators too and they hold on to their settler status as long as possible. Settlers can also be translators but not merchants. Merges (1996) suggests that scientists will 'role differentiate' in their dealings with each other according to whether the other is acting in a commercial or academic role, and that seems to be the case here. Translation is 'neutral territory'.

Translation from settler science to merchant science is what the network's professional staff and commercial advisors attempt to make some sense of, maximize, and manage. With merchants, a company needs to be incorporated and capitalized and equity shares agreed, before the intellectual property can be transferred. With settlers, the search commences for an external licensee with deep pockets who will actually develop the technology settlers have discovered, and bring it to the bedside.

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[124] In a footnote, Keating & Cambrosio (2000) call it a 'current buzzword'. This is the only reference I could find outside the medical research literature. Benoit Godin, who specializes in definitions of science, was unaware of the term (personal correspondence). Correspondence with Alberto Cambrosio revealed that he and Peter Keating will explore the ubiquity of the term in biomedical research in an upcoming book on Biomedical Platforms, currently in preparation

In the rest of this chapter, I consider the tensions and contradictions inherent in these different orientations, and thereby illuminate the way that public and private, basic and applied, map onto each other during a process of cultural change. In contrast to previous chapters, the focus shifts from the network to the individual researchers: those who are, and are not, engaged in adventures in the nature of trade and the boundary work this entails. In the last part of the chapter, I suggest a framework that links settlers, translators, and merchants, respectively, with intrinsic, instrumental, and market values.

Settler Science: 'Excursions into the land of ignorance'

For settler scientists, raising venture capital and floating biotechnology stocks are of little interest. Commercializing discoveries and creating new companies are not for them. Theirs are the traditional values of academic science. They are focused on career research programs which are publicly funded, and curiosity-based. Rather than venturing out into the commercial world, in pursuit of profit, settler scientists undertake adventures in place. They mine knowledge, making what one distinguished network member calls 'excursions into the land of ignorance'.

> I am a career academic scientist in the medical field. I've always worked out of the university and its frame of reference and its culture. And I've always been funded by [the state]...That's the only thing I know. I was *allowed* to be a scientist. I was allowed to make excursions into the land of ignorance, to try to bring back knowledge that would benefit patients. I did develop technologies but I didn't patent any of them. We just never did that then. We just got on with the job of science. I was allowed to do these things, I enjoyed doing them and that was sufficient for me. And somebody paid me to enjoy that. What could be better? My paymasters were the peer review people and my department chairman, not the market. The paymasters now are the shareholders. And the venture capitalists. And that's why I have had trouble with the network in my own personal relation to it. The emphasis on profit as what we have to do, makes me feel uncomfortable CS-13-17

The vast majority of scientists in the network are settlers. Very few are involved in commercial activities in anything other than a peripheral manner. As a result, a relatively low level of interest

exists in the commercial mandate. This became quite apparent to me during my participant-observation at the network's annual scientific meeting, in Spring 2000. I observed the CEO playing the role of cheerleader, encouraging members to *start* thinking about the potential commercial applications of their research. For a ten-year old network, with a mandate to commercialize, his exhortations struck me as telling. Few of the researchers at the meeting seemed to be in a commercial 'space'. The CEO might have been suggesting something novel. At the two-hour 'Concurrent Networking Session', four tables were set up. Session leaders dispensed information on bioinformatics, DNA sequencing, model organisms (knockout mice), and strategic funding for commercialization. While the first three tables were extremely busy for the whole two hours, no one sat down to talk to the commercial director about the strategic funds the network makes available to move discoveries towards the market. *No one.* I kept checking back and noted that, eventually, the commercial director abandoned the table.

At a subsequent interview, the CEO acknowledged that 'it's a challenge getting them to even *start* thinking about commercial activities or to see translating their science as anything other than a necessary evil. They enjoy the science. And the science allows them to go off in any tangent that they want. The commercial world doesn't' (PS-RW2-54). Also, he explains, many network scientists were socialized at a time when the thought of being an entrepreneur was foreign. 'Whoever thought about starting a company back then?' (PS-RW2-53). So his preferred approach is to help people commercialize if they are interested, but not to insist.

> Doing anything commercial is difficult enough with everybody pushing in the same direction with the same amount of energy. To try and push something where people aren't driven in the same way, it's just not worth the pain and anguish. So my approach is very simple. If the PI wants to do something, we're there. We can give him [sic] the tools, we can work with him [sic], whatever. But we only have limited resources so we'd rather work just with the ones that have the interest. PS-RW2-57

According to the CEO, only five or six network PIs are 'first tier' entrepreneurs—those who would be willing to take leave from their academic research to devote themselves full-time to company creation and commercial development. About the same number are open to and interested in commercialization but 'second tier in the sense that I don't believe any of them has seriously contemplated taking even a 50% leave from their research' (PS-RW2-58). A third tier is willing to be involved with industry to the extent that they will sit on scientific advisory boards. But at most, 10 to 15 percent of the total complement of PIs is involved in commercial activities. This proportion is actually higher than in the first two phases, since the Phase III expansion, by design, brought in some commercially-oriented newcomers.

One settler ascribes the reluctance to conservatism. 'People are worried that it's not going to work out if they give-up their academic career and go into industry. They are leery; they don't feel all that confident. After years and years of writing grants you get to know the system. To now go out into the business world. It's a very strong break to make' (FJ-11). Another fears that the nature of research may change under pressures of commercialization. In a comment reminiscent of arguments in Bush (1945), she states:

> You may actually discover something very important and very applicable to something practical, but you can't predict how it's going to go. And so if you are always trying to orient your research to the practical end product I think you are restricting the types of research that will be done. And, if there is a large emphasis on this then the nature of research will change LF-23

For other settlers, it is the time commitment required for commercialization that is at the root of much of their resistance. The participation of the PI is essential in any move to capture the commercial value of a discovery. They are the knowledge producers and they embody the knowledge they have discovered. Even in relatively simple arrangements, like filing for patents and

licensing them out, the PI must be involved. And that involvement takes away time from the laboratory.

> Sometimes, when they take it to the lawyer, I almost dread them coming back and saying 'oh, we should patent this.' Because I realize how much time that's going to take. If they are good lawyers, they can come up with most of the writing and stuff like that. But...it still comes back to you doing the detailed work. FJ-49

Many of the settlers view commercial activities as a subsidiary requirement of network membership; a chore that gets marked on their annual 'report cards': 'my report card was always stars for science and good for networking and zero for commercialization' (LF-33). They say they feel guilty about not doing more but their own research programs come first—'I really have not had any time leftover to pursue commercial interests. It's something I would do if time allowed' (DC32). Those settlers that participate on the commercial periphery, for example by licensing out their discoveries to others, do so to underwrite their discovery-based research programs.

> We're not doing this [patenting] out of personal gain. I don't think any of us have any illusions on that score. With monies being so tight right now, if I do make anything, it would just go rolling back into the lab. MW-13

> Every step that it goes, they [royalties] feed back into my research. DC-20

Another source of the settlers' resistance to commercialization is the loss of freedom that accompanies ventures outside the academy. Academic researchers are highly autonomous. The academic environment allows them almost complete domain control over their scientific activities. As mentioned earlier, they consider themselves 'franchise owners' who choose to locate their franchise at the university (RG-45). Their independence allows them the ability to follow serendipitous directions. It is one of the 'fun factors' in doing science: the fun of 'discovering stuff...seeing something working and saying wow, no one's ever seen this before' (FJ-51). Because of this autonomy, says one researcher,

> I can wake up tomorrow morning and say, 'wow, I'm going to try to get a grant on juvenile diabetes. I've got a crazy idea but it might work and if I get the funding, we're actually going to try to do it.' And that freedom is just the greatest thing. FJ-56 [

For someone used to this degree of freedom, the constraints of trying to commercialize a discovery are severe. Suddenly, you are accountable to strange new constituencies—investors and venture capitalists; pharmaceutical companies. 'People are breathing down your neck' (FJ-57); you have to meet 'milestones' in order to release the next tranche of funding. It isn't a very attractive proposition for scientists who have been grant-funded for their entire career.

> To have a bunch of investors tell you 'well you haven't met your milestone' is not quite right. If you've got an academic lab and you're also in some company and the board of directors is saying, 'well you said you were going to have this done by now, what's going on here?' And then you've got to go and yell at your scientists to get them going. Well, you know, that kind of pressure, to me is not worth it. Some people like doing all that stuff. But I would find it just too stressful. FJ-57

Some settlers who initially went adventuring became disenchanted and retreated to the academic environment. As one describes his experience,

> In the end, it wasn't what I wanted to be doing. I spent six months looking at it very very seriously. [But] I didn't like what was happening, I didn't like how it was happening. [And] the funding that I had expected to get from the venture capital group hadn't come through. In the meantime an opportunity came along for me to move to the University of Ottawa and develop a new research group there and they offered me $23 million to set-up the new facility, so I went. RW-40.

Another whose research is a long way from the market end of the pipe tried to forge an alliance but failed because short-term profit potential was lacking.

> The company had attempted to get venture funding for a product that would diagnose dyslexia genes and there wasn't enough commercial interest because it is not viewed as a severe enough disorder. And who would be asking for genetic testing? It would probably be something through say, the school system, and the company couldn't envision that happening in the near future because school systems are not at that level of sophistication yet. Maybe 20 or 40 years down the road hopefully they will be, but sort of in the immediate future the company could not envision it. LF-36

Clearly, even if not particularly interested on their own behalf, settlers have largely overcome their discomfort with commercial activities. Albeit with ambivalence, they have come to accept the utility of combining a commercial and collegial ethos within the network.

> We're lab scientists. We're no more used to the idea of commercializing something than a social scientist is. We have increasingly more likelihood that we will become involved in a commercial aspect. But we're not trained for that. And many of us view it with a lot of concern because we know some of the horror stories. But on the other hand, we know that we're probably going to have to go that route. MW-26

Settlers endorse the network's commercial mandate and will assist with the protection of intellectual property, deploying a means-ends rationalization to justify the privatization of publicly funded research. 'As long as it ultimately benefits society, this is an acceptable relationship in my view' (CS-24). They will even move towards translational research themselves. As one says, 'in the old days if you made a discovery that might potentially have commercial applications, it would have ended there. Now, you have the possibility of moving forward with it and the network is there to help us through that' (RG-21). So now, they will take out patents and participate in simple licensing deals, as long as they can turn over their discovery to someone else to develop. As one explains, 'I have been trying to look after this [orphan drug] project by bringing in people who know about where to find funds in the private sector better than I do. They've become involved in this and this is where the network has been very helpful' (CS-22).

At the same time, they have also come to accept the restrictions on disclosure and dissemination that accompany intellectual property protection. As one explains 'I think all of us are now more aware not to disclose prior to patenting' (MW-13). Another says 'remembering to protect the intellectual property before we publish, that's not so much a strain now as it was ten years ago. We're getting used to that' (RW-38). Still, the secrecy required by participation in commercial activities remains a source of disquiet for many settlers. Confidentiality requirements are onerous. In

this type of field, simply mentioning the name of a principal investigator, is tantamount to telling a competitor what has been discovered, because everyone knows what everyone else is working on. So that has altered the culture of open sharing. The problem became particularly acute in Phase III.

> It was much more fun before. Now, going to a network meeting, it is just not the same. There are people saying 'I cannot talk about this.' Or busy answering their cell phones. So instead of being a dedicated time to talk about science, people are side-tracked.. That is the thing that I find different, that I regret. I regret the drive towards commercialization. LCT-24

Commercial attitudes and constraints on disclosure are antithetical to the open science model under which settlers were socialized. Academic researchers have not learned how to extract information from each other with skill under commercial constraints. But as another settler points out, the constraints can be justified. 'I don't think it would be fair for a company to take all the work...that we put into [a discovery], and then make money on it. Without patenting and licensing, anybody and everybody could use it and earn money commercially on our efforts. That doesn't seem fair' (DC-18).

The irony here is that, until recently, the whole point of public funding for academic science was that 'anybody and everybody' could then use it. Under the post-war social contract for science, government support helped to generate knowledge which was made freely available to the productive sector. As discussed previously, under the linear model the resulting innovations would fuel growth and jobs and return taxes to the state to fuel the pipe again. Until quite recently, therefore, scientists' rewards came in the form of research support and peer recognition, rather than in the form of profits from proprietary knowledge. As shown in Chapter 2, 'open science' economists argue that in making knowledge proprietary at such an early stage of the pipe, public sector researchers choke off innovation and wealth creation.

Whether or not that is the case, settler scientists accept that the moral economy of science is changing and have learned to co-operate with the new norms. The researcher quoted above on fairness says that she is far from enthusiastic about the concept of intellectual property, but cannot see a way to withdraw. 'When everyone else is patenting and licensing their discoveries, one cannot say, oh well, everybody else in the world is doing it, but I'm not going to' (DC14). For another researcher, 'it's like standing on the edge of the deep-end of the pool wondering whether you'll sink or swim. They [commercial staff] give us the shove and Whoosh! There you are, deep in the water, and they say 'great, good luck" (RG-37).

So open science and proprietary science occupy a tilted continuum, with settler scientists at one end inching towards merchant scientists at the other. Justifying this 'creeping propertization' is a rhetoric of *translation*—translation from the lab to the market, creating wealth, and from the bench to the bedside, creating health. In justificatory discourse, the two are often conflated. The argument is that the second requires the first: that the production of beneficial technologies requires the motivation of profit. Whether or not that is the case, the desire to translate their results—to apply them to achieve socioeconomic impacts—adds a new dimension of utility to scientists' research programs, achieving one of the goals of the NCE program and situating the work clearly within Stokes's 'Pasteur's Quadrant'. However, the scientists themselves, merchants and settlers alike, clearly differentiate between research and translation, discovery and development, using classic boundary-work strategies to make a sharp demarcation.

Translational Research: 'I wouldn't call it science'

> The stuff that we do in a commercial setting is really technology development. I wouldn't call it science. You know what I mean?' (FT-20)

Translational research is the space between the 'R' and the 'D' in R&D. It involves building a better drug or procedure around a basic discovery, then doing clinical and preclinical studies to test it out. Settlers tend to look down on it and undertake boundary work to differentiate. In the words of one, 'if you're not doing [basic] research, you're like a mechanic that repairs cars. You may just work on Mercedes Benz and it's highly specialized work, but you're still just repairing cars (FJ-36). In other words, basic science is 'real' science; translational science is not. All scientists interviewed made this boundary-work distinction, even those who were themselves involved in translational work. As one explains, 'after they have discovered the gene, and they have figured out what the protein is, translating it is really boring; it's not [as] exciting [as] basic science' (BG-4). Another describes his company's translational work in the same deprecating tone,

> We develop leads, and do structure/activity relationships and do chemistry, and build new compounds, and then test them back and see which one gets better and better. That's going to be an exciting drug but as basic science it's not all that interesting. It's not what's published in the basic journals. It's not how they find out how this gene works, and what pathway, and what it interacts with, and so on. RK-29

The valuing of basic research above translation and application is deeply embedded in scientific culture. One of the goals of the NCE program was to change these norms and, to a large degree, that has been achieved. Within the network, much of the stigma has been lifted from translational research largely by making it legitimate for scientists to maintain multiple roles. In other words, researchers came to understand that they could do basic research *and* translation; it was not an either/or proposition and both aspects could benefit from the interaction.

The program goal of promoting translational research was not understood at first, especially in the first phase. At the time, researchers viewed the NCE program simply as another funding source for basic research. According to the scientific director,

> it took a while for everybody to understand the *place* of the NCEs. The place of the NCEs is *not* fundamental research necessarily. There may be some, but it's really the *translation* of research into products and services for economic benefit for Canada. But that wasn't appreciated early on. Nobody really understood. And nobody trusted it (MH2-10).

Back then, many researchers 'hated the idea that their work would be applied' (ARA-DR-17). But gradually, through a combination of resource steering and peer example, attitudes began to change.

According to one observer, a personal aspect of the cultural change had not been predicted. Translation meant researchers could go home and share their work with their families. They could present their work in context, in a way their children could understand. 'Before, they were studying something nobody had ever heard of and their kids couldn't care less. [Now] they could talk to people and people would think [they were] doing relevant stuff. And they discovered that was kind of nice. It was nice to be valued for something other than just the science' (ARA-DR-19). So although translational research was not as exciting as basic research, over time researchers came to accept that it was a valuable and even scientifically interesting enterprise.

A core of network scientists had always been involved in translation. About one-third of network investigators are physician-scientists. One believes that the rigours of maintaining a clinical practice, as well as a research lab., produces people who are more entrepreneurial or opportunistic than average (FJ-34). Whether or not that is the case, creating therapies was an honourable goal long before profit became a consideration. A long tradition marks supply and service through various small enterprises. For example,

> I sort of run my own diagnostic company on the quiet. It is not a formal company... The money comes back into a fund within the hospital. After I have paid for the technical help and the tissue culture, I use the excess to do research. This is all service oriented. I don't do this to make money. I do it because there is a demand for it. If I didn't do it there would be a big hole which the commercial companies would not fill. They can't make money at it. It is an orphan service. BR 16-20

> About twenty years ago...it was absolutely impossible to buy any of that enzyme...[needed to treat a rare disease] because nobody could afford it.. So what did we do? We used recombinant DNA techniques. We took a yeast version of this enzyme, we put it into a vector and put the vector into e-coli bacteria . And then, in a little room on a back street in Montreal, we had the potential to produce a world supply of this enzyme. CS-18

But outside these traditional enterprises translation was little respected and had always been difficult to fund. It was not what the research councils understood. That was why network support was so important. 'MRC thought translational science was pretty boring stuff. And all the agencies thought that was pretty dull. And so it might never have happened without the network push' (BG-5). Until quite recently, universities and hospitals provided little support for translation. In some universities and hospitals that is still the case.[125] Outside the boundaries of the network, translational researchers still have to fight basic scientists for resources and respect. As one explains her situation,

> We needed a way to...put respectability on translational research. Before, the basic scientists would say, well, 'what are you doing that for? We shouldn't be doing that in this hospital.' That is what they still say at [my hospital]. 'We should only be doing basic science. Shouldn't drug companies do that [testing]? Shouldn't industry do that?' BG-7

> They gave us the worst space that nobody will otherwise habitate because it is dangerous and ugly and has no windows. But that is the only space good enough for people like us. So we accepted it because we otherwise were going to have to quit and we had no way to ever continue doing this work again. This was what [my hospital] thinks of translational research. BG-63

Arguably, it was the scientific director's attraction for translation that motivated him to found the network. Commenting on his early career choices, he says 'I always thought I would do research but

[125] The role of the ILO is one deciding factor; see Atkinson-Grosjean & Fisher 1999

it always translated into patient care and the patient. So I was always a researcher who was not far removed from issues related to improvement of patient well-being. I was pretty much based in the area of translational research, right from the beginning' (MH1-5). Another senior researcher with a similar clinical background states: 'to me, the network's whole purpose was to actually take the research, diagnostics, whatever, and apply it to make health better. And so translational science has always been my focus. My interpretation of the network was that it should actually make health better' (BG-3-5). For physician-researchers, the logical extension of their work is creating a product or service that will impact human health.

> We want answers. What we're trying to answer right now...is whether inhibition of a certain pathway has prospects for therapy. Well, believe me, the quicker I get that answer, the more I'll feel I'm fulfilling my responsibility. And if we can do it one day earlier, we should do it one day earlier. Industry loves that.; they say that's great. MH1-23

As the last sentence above suggests, in the knowledge-based economy translating research into products that impact human health also translates into the potential for significant personal and corporate wealth. As the scientific director puts it, 'we're becoming more entrepreneurial, more capitalist' (MH2-23). It becomes increasingly difficult to tease apart the two different types of translation. The profit potential in bioscience 'hits' (when big pharma and/or the stock market invests in a discovery) is huge. Researchers who pursue translation can become seduced by the economic value of their work. In terms of recognizing the normative changes underway, Shapin (1995b:309) suggests we need 'to produce a post-Mertonian picture of the moral economies of science.' Especially where spin-off companies are involved, the potential for conflicts and controversies becomes marked.

Merchant Science: Worlds in Transition

Merchant scientists straddle the boundaries between research and development, science and commerce, public and private sectors. In a manner some observers find disturbing, they move confidently between their academic responsibilities and laboratories, on the one hand, and their spin-off companies, contract research, and clinical trials, on the other. No travel time is involved in this progression, since all these activities are usually housed in the same premises, creating potential and perceived conflicts of interest.

Kohler (1998: 243) defines the moral economy of a scientific discipline as a 'workplace culture' in which tacit rules of mutual obligation guide community life. The boundaries of propriety between the moral economy of medical genetics and academic obligations and the market economy of biopharmaceuticals and venture capital have to be managed. The process is often described in the language of 'worlds'. As one settler comments, 'they have both worlds going at once. I don't know how they do it...I would find it more than I would want to do' (FJ-59). The network's CEO says that these scientists 'can walk in both worlds; it's not a problem for them'(PS-RW2-54). And the scientific director—himself a merchant scientist—says 'I don't think I'd ever want to be wholly bound in either world, to be honest. I'm quite happy having the ambiguity between both. It makes life complex and intricate. But it's really interesting' (MH1-43A).

In sociological theory, the language of worlds is both cognitive and social. Mary Douglas (1986: 12-14) talks about the 'thought worlds' that are structured around an inner élite who determine what can be counted as valid questions and set the limits of enquiry. This concept seems similar to Knorr-Cetina's (1999) notion of epistemic cultures and the 'styles of scientific reasoning' elaborated by Ian

Hacking (1983).[126] The 'worlds' of academic and commercial science have such different thought styles that it would not be surprising to find cognitive dissonance in the scientists that move between them. However, if rather than 'thought worlds' we think of 'social worlds' the transitions seem less problematic, since the latter analyses emphasize *interaction* between domains.[127]

Unlike 'real' entrepreneurs in the world of the market economy, merchant scientists undertake no personal financial risk; sinecures drawn from their academic world buffer them from the volatility of the biotech sector. And unlike 'real' medical geneticists in the world of the academy, their commercial activities buffer them from departmental responsibilities. Enjoying 'the best of both worlds', they thereby attract peer resentment. According to one merchant scientist, 'the perception of peers is the biggest problem. The people in your department often don't fully understand it and are generally pissed off. It's serious and never-ending' (FT-25).

Part of the resentment stems from the fact that the merchants are working in their own company for their own benefit. It is they, rather than their academic community, that will realize the rewards. Another aspect is that the merchant scientist is not available to work in the department. 'Someone else has to teach for you; someone else has to be on committees' (FT-25). Finally, the disparity in compensation is an issue. Given the relatively low pay in Canadian universities, 'you're talking about people getting paid in companies 4 to 5 times the salary they'd be making at the university. So you add these facts together and that builds some pretty serious resentment' (FT-25).

Given the differing benefits of each world, the desire to retain both is understandable. 'I want to do the science,' says one, 'but I'm also very interested in making sure that the business side works properly' (RK-51). The decision to occupy both worlds at once is strategic; it provides the scientist

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[126] Based on Styles of Scientific Reasoning, by A.C. Crombie

[127] See, for example, Becker 1982; Strauss 1982; Fujimura 1987, 1992; Kaghan 1998

with choice, legitimacy and protective coloration and marries scientific credibility with commercial expertise. Ultimately a decision for one world or the other will have to be made, but that decision is deferred as long as possible. Says one, 'I'm still hiding under the guise of being an academic scientist because it suits my style' (RK-48). Another admits that he is 'something in between. Even though I run a company, I consider myself an academic scientist when I'm in academia. There's no other way to do it' (FT-17). A third says, 'I prefer to do what I do within the university, for now. But who knows? ...I take it one year at a time' (MH1-52).

One of these merchant scientists is reaching the end of a year-long leave of absence. Having worked exclusively for his company and taken it international, he is currently piloting it through a merger with a larger German public company. Soon he will have to decide which world he is going to inhabit permanently. He has been offered an opportunity to stay at the merged company so whether or not he returns to his academic post is still moot. 'We'll have to see. I'm living a week at a time here, I'm not worried' (FT-44). It will be a wrenching decision, because he is truly committed to both. His international reputation depends on his research world; the commercial milieu is new—'the bulk of my work, the work I've become known for in the last few years, was completely independent of any commercial activities or considerations' (FT-19). At the moment, he is running what he describes as 'parallel lives':

> One has to do with cancer and bone tumours and we publish in regular journals like Nature and Proceedings of the National Academy of Science. That's a very academic pursuit. And then there's the stuff I'm doing with the company, which is completely commercial. I mean it's clinical development, you have to be scientists to move it forward, but I don't consider it to be anything other than a commercial activity. The two lives are completely parallel; they're not connected.FT-21

It seems to be the case that *this* scientist's academic and commercial lives *are* 'parallel and unconnected'. His leave of absence has defused any friction between his corporate and academic roles. But that is not the norm for other merchant scientists in the network. He is the only one to

have made this move. Others stay in place, in their university labs, and that generates the perception of conflicts of interest and commitment.

CONFLICTS OF INTEREST AND COMMITMENT

Merchant scientists occupy multiple and often contradictory roles. They are university faculty, hospital clinicians, network members, company founders, contract researchers, inventors, teachers, supervisors of graduate students, directors of research institutes or centres. Often, they are incubating their own companies and running clinical trials for big pharmaceutical companies out of the same academic labs where they conduct their basic research projects. Not surprisingly, some of these individuals have difficulty keeping track of who they are representing from moment to moment. And that can create problems. One university technology manager ruefully referred to his ILO as 'the office of conflict of interest' (DJ).

When scientists who are the principals of private companies remain in place in their university labs the perception of conflict is difficult to avoid. The core issue is reconciling their dual interests in publicly-funded research and private profit. Put simply, how can commercial development of a discovery be separated from the basic taxpayer-funded research from which it arose? Often, the perception is that conflicts of interest are out of control in the biosciences, which one network board member describes as 'a cross between research and making money' (B-MM-26). The appropriate use of public money is a valid question when private interests are involved. But setting aside the larger ethical questions for the moment, I want to examine control of conflicts of interest within the network. CGDN has to minimize the potential for conflicts while recognizing that they are endemic to merchant science. From the network's perspective, the challenge is to ensure that no company or individual is leveraging network funds for personal profit. The perception of role conflict is at the root of the problem.

Commercialization is part of the network mandate. What that has come to mean in practice is that commercial work is done in the university. Companies have been formed that work partly in academic labs and partly outside. As a result, it is difficult for colleagues to determine which interests a merchant scientist is representing at any one time: whether the university's, the network's, or their own company's. The tensions are profound and cannot be ignored. As one policy expert states, 'You have got people who stand to make big money personally through these networks and it has to colour their thinking' (ARA-DR-62). A board member from the private sector agrees. 'There *are* conflicts. They do exist and they are real. And they can be dealt with by people being straight up about which hat they are wearing' (B-MP-34). The network's commercial director says 'there is a conflict in anything that you do. You just need to manage it. And if you don't manage it, then that is when it becomes a problem' (PS-HC-25).

For the network, the essence of the problem is the start-up companies. Because they are products of network activity, these companies tend to become industrial partners of the network. The network scientists who founded the company will be involved in the first-generation product development taking place within it, using venture capital raised for that purpose. But at the same time, they remain as a network scientists and the network continues to fund the academic element of their work. This may or may not be related to what happens in the company. If it is related, it will tend to be fairly speculative, second-generation product development. As the network's manager of scientific affairs sees it, 'it is appropriate for the network to continue to fund the [second generation] type of activity, because that is how you get the new improved mouse-trap' (PS-SH1-34). When the mouse-trap is successful, it gets moved into the company as well.

From the network's perspective, the opportunity for real conflict of interest arises if the company or its scientists use their relationship to leverage money out of the network.[128] Once technology has been transferred into a company, therefore, an arm's length arrangement should provide full disclosure of the interest. Once the interest is disclosed, that particular scientist is not allowed to participate in decisions pertaining to any award of money to the researcher *qua* academic researcher. 'Generally speaking,' says the manager of scientific affairs, 'you try to avoid circumstances where somebody has to walk away from something, because the interest is too close. That doesn't mean people from outside see it that way, but that's how we deal with it' (PS-SH1-38). As well, no funding is permitted to flow directly into the company although fairly modest amounts of money continue to be put into the hands of the company's network scientists. The research funded in the university lab eventually benefits those companies, of course, but that is seen to be the whole point of the exercise, and in everybody's interest, 'because the overall goal is to make sure that these companies survive and start making profits' (PS-SH1-43).

Although the network's professional staff appear confident that the situation is under control, a number of the senior scientists—the settlers—are less than comfortable about the blurring of relationships.

> I've seen cases where company people are working side by side with the researchers, where the academic research team are studying the same thing as the company people. And, I think that's pretty tricky and should be [stopped]. FJ-13

> I am concerned about the fact that when people become involved as principal scientists in companies, they maintain their academic lab. It is quite feasible to do both and to do both well and to keep them separate. But I think a problem can easily develop when you have that company and you're then involved with contracting work back to your own lab. I think it's dangerous. RW-41

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[128] In one of the life-science networks, for example, (not CGDN) an officer 'was alleged to be running a lot of the network purchasing through his own companies...the specifics weren't as important as the clear indication that there were starting to be conflict of interest situations arising' (evaluator/analyst, ARA-DR-43).

One of the major concerns is that graduate students will be exploited as the principal investigators move into more pointedly commercial activities. Graduate students are vulnerable. They have to get their degree and they have to publish. This puts the senior scientist in a difficult position if the student has been involved in some commercially sensitive activity which constrains their ability to publish in a timely fashion. This is widely recognized as unacceptable and clauses in network internal agreements limit the potential delays. Merchant scientists argue that such a situation is unlikely to arise and see concerns as unwarranted.

> So far there has been no real problem for any of them having the excuse that they can't publish because the company's keeping a lid on it. In fact, they had better look to themselves as the reason why it's not published, not the company...If you find a compound...it won't be--and rightfully so--put in the literature until it's clear that everything around it is solidified and understood. And no students are going to be *wanting* to work on that, let alone [actually] be working on it. So it's a straw man. RK-37

But if graduate students *do* conduct their thesis research on company-funded projects, what happens if the company's economic or commercial goals change, and they redirect or cancel the project? I was given actual examples of this, for example

> You end up shifting resources away from one thing to another in a lab. And a graduate student's doing his work and he finds that the resources around him are diminished because it's gone to another project that has major commercial value. That's where there's conflict, resentment, bitterness. Those things happen. MH1-34

> [When the company decided not to proceed] the graduate student was deeply threatened... If it had been on an MRC project, what we had achieved would have guaranteed renewal and the funding would have continued. [But] when you're dependent on the private sector, their responsibility is to their shareholders. CS-21

Even post-doctoral fellows can be vulnerable to exploitation.

> I wouldn't advocate putting students into any kind of commercial setting. With post-docs I'm willing to be more flexible as long as they know in advance what they're getting into. And they don't always. Sometimes they come as an academic post-doctoral fellow to work on a project for Dr. So and So. And when they're six months into the project they realize that 'hey this project really is one of the prime goals of Dr So-and-So's company and he is going to make a bundle of money from this if I figure it out.' RW-45

One merchant scientist perceives no conflict. 'It is just fine to have a company start up and grow within the lab. It is a good way to bring money into the science. It is a good way to get money from other sources. You are producing products. And it ties things together' (BG-102). Another is aggressive in defence of his ability to keep each interest separate, and differentiate between his academic and merchant science. A third acknowledges the problem and describes how he deals with it.

> I'm not sitting there trying to exploit my graduate students, getting them to work on company business, because it's very different science. In a company, you want to make a [patent] claim that *this* compound is tested in all these ways and does the following things. That's not a thesis...but you'd do that in a company. So I'm not making these students do company stuff. RK-36

> There is the potential for conflicts and what you try to do is put into place [agreements] that certain things will happen when a conflict arises. In other words, if I discover something on the way to work and I happen to be going to the company and my university lab on the same day, the lab always gets credit for it. The university always gets the patent, even if the company is interested in the idea. We never try to split hairs and say 'oh, I thought of that when I was at the company'. It just doesn't happen. We always throw the university the intellectual property. FT-23

As well as conflicts of interest, commercially oriented network scientists may experience conflicts of commitment, especially when they are partnered with powerful pharmaceutical companies. 'When industry calls me to do something,' says the network's scientific director, Michael Hayden, 'and I've got a graduate student to meet. What am I going to do?' (MH1-33). Hayden has a longstanding relationship with Merck Frosst, which funds the network's 'spin-off' institution, the Centre for

Molecular Medicine and Therapeutics (CMMT). As one of the NCE program's senior bureaucrats explains,

> Merck always wanted Mike Hayden. They couldn't recruit him but they bought him in other ways. What Merck got eventually was more than Mike Hayden; they got the network. Or a good part of the network. But that's really the whole point of the program. And the partnership has to be mutually beneficial (NCE-PO-MAL-7 & 48)

Hayden freely admits that Merck tries to influence him. The truth is, he says, 'I want them to be happy. My future depends on them. My future and *our* [the network's] future' (MH1-37). Also, because the pharmaceutical industry has its own political agenda, Merck draws on his influence whenever an issue is on the table. Hayden tries not to get involved but usually ends up doing what they want, if he agrees with them on the issue. And most times, he says, 'I do agree with industry. I see them as worthy partners. They've got a lot to add. So it doesn't help us to not fulfill some of their requests, if they are science-driven' (MH1-19).

That phrase—'science-driven'—is the key to resolving conflicts, according to Hayden. The agreement with Merck, for example, gives him total discretion over the direction of research. 'I choose what we work on. In the contract, it doesn't say what we're gonna do. It says that Dr. H, in consultation with his colleagues, shall decide what research will be undertaken' (MH1-36). Conflicts occur, he says, when relationships are 'dollar-driven', because corporate cash can weaken research ethics. When relationships are science-driven, and supported by strong and clear agreements—for example, limiting publication delays to 60 days—conflicts are under control. But he acknowledges that much depends on the strength and integrity of the individual and their ability to withstand temptation. That temptation, he says, is rooted in Canada's historical underfunding of basic science.

> If you're not able to compete, and not able to get sufficient funding for research, you end up taking money from industry because you're desperate. And you end up taking *any* money from industry...because you're helpless and weak and you haven't been able to raise money elsewhere. And so you end up doing things that you really shouldn't do. And that's primarily a reflection of the bankruptcy of our funding for research in the country. MH1-41

As discussed earlier, to minimize both the appearance and potential for conflicts, the 'right' thing to do is move a start-up company out of the lab as soon as possible, and for the scientist involved to take a leave of absence from the university. This puts some physical distance between the merchant scientist's 'public' and 'private' worlds. Most universities have set up research parks and incubator facilities for this purpose. But that still doesn't solve the problem. The basic issue, as one informed observer comments, is the perception that

> it is a contradiction in terms for a scientist to be an entrepreneur. One is trying to make an industrial product, a market product, and the other is trying to do good science. It reminds me of the saying—you can't serve God and Mammon. There may be some instances where the two are congruent and you're lucky. Particularly in the non-health areas, where people can decide for themselves whether a new and different gidget is in everybody's best interest. But in health product areas, it's much more difficult. Because your motivation is to push that product and the public can't assess whether or not it is useful and good. There's a knowledge gap. And so the chance for the public interest not being served by the union and marriage of scientific pursuit and entrepreneurial pursuit is greater—there's a problem. CIAR-PB-34

INCORPORATING MERCHANT SCIENCE

With the network as an equity shareholder, CGDN's merchant scientists were actively involved in nine spin-off companies to June 2001, six of which were launched in Phase III. Of the three prior companies, Apotogen, founded by Bob Korneluk and Alex Mackenzie of Ottawa in 1995, to exploit their discovery of apoptosis inhibitors, has recently merged with another company to form Aegera Therapeutics Inc., and a subsidiary, Aegera Oncology Inc. As yet, the company has no products in clinical trials but current capitalization is approximately $28 M. NeuroVir Inc, launched by Michael Hayden and Frank Tufaro in 1996, in partnership with Max Cynader of the Neuroscience network,

is exploiting herpes as a viral vector, as well as a 'basket' of technologies they bought as a portfolio. They have a product at stage 2 clinical trials and are currently merging with a larger European company. According to an inside informant, the company is now worth 'slightly more than $100 M US'.

Xenon Genetics Inc, established by Michael Hayden in 1997, exploits genes associated with lipid disorders. The company signed a deal with Werner Lambert ('big pharma') in May 2000 potentially worth $87 M. Originally capitalized at $13.2 M, a 'mezzanine financing' deal was completed in May 2001, valued at US$47.6 M. As of November 2000, Xenon had taken over RGS Genomics Inc, a Phase III spin-off founded by three network researchers from McGill.

The newer companies, as yet, are still establishing themselves. Brenda Gallie, of Sick Kids, set up Solutions By Sequence Inc in 2000, in partnership with a former economics professor from Simon Fraser University, to undertake retinoblastoma testing. These partners have modest aspirations in terms of financing and profit; the primary goal is to service the at-risk population. Frank Jirik, now of Calgary, and Chris Ong of UBC set up Genexyn Pharmaceuticals Inc in 2000 to pursue gene- and protein trapping. Signalgene (1999) is capitalized at around $50 M and has negotiated a $1.2 M research contract to extend Francois Rousseau's work at Laval on osteoporosis and psoriasis. Ellipsis (1999) has ambitions as a gene identification company and EcoGenix Inc. has just raised its first $750,000. Certainly, since Phase III, and the appointment of the new CEO, increasing emphasis has been placed on company creation.

Perhaps relatedly, between the time of my fieldwork (1999-2000) and a follow-up visit 12 months later, CGDN's Board had become increasingly concerned about perceptions of conflict of interest, in part reflecting increasing concerns in the NCE Directorate. As required, the NCE's conflict of interest framework had been adopted as part of CGDN's Phase III network agreement, signed May

1998. That framework charged the Board of Directors with the responsibility of 'managing conflict of interest, and determining and implementing the appropriate course of action' (CGDN-NA 1998: 37). Conflicts had always been managed on an *ad hoc* basis. With the mid-term review pending in May 2001, the Board was warned by the network's new Commercial Director[129] to expect questions from the review panel about how the conflict of interest framework was being implemented. They were advised that unless a process was put into place, the review panel might identify a deficiency. Accordingly, just before the review, the Board appointed a *pro bono* Conflict of Interest Officer (a lawyer and former hospital board chair) to advise on potential conflicts. They also directed staff to prepare a list of the direct and indirect financial interests and positions of influence of each individual in the network, including scientists, board members, and professional staff.

Discussion

Others have noted the phenomenon I call 'merchant science'. Lynne Zucker and Michael Darby, (1996, 1997) coined the term 'star scientists' to describe the pioneer molecular biologists who established the biotechnology industry in the US. They are classified as 'stars' not only for their commercial acumen but also for their academic productivity. These are élite bioscientists: 'extraordinarily creative, innovative, and productive individuals' with the 'vision and genius [to] consciously change the boundaries of what is possible' (Zucker and Darby 1997:503). What this conveys is a 'great man' theory of history: an important new industry arose from the efforts of a handful of geniuses; contributions of lesser mortals pass without comment. Henry Etzkowitz (e.g. 1989, 2000) takes a similar triumphalist tone in speaking of 'entrepreneurial scientists'. A key criticism of ANT is that it, too, studies the big and the powerful—the 'heroes'—to the exclusion of

[129] Recruited from the federal government. This person was, until recently, CGDN's program officer at the NCE directorate

others.[130] Heroes are studied because they have a greater influence on the shaping of networks; their successes and failures are larger scale (Latour 1992).

Sheila Slaughter and Larry Leslie (1997) apply the label 'academic capitalists' to star entrepreneurs and seek, as I do, to situate them within wider political, economic, and institutional contexts. In a comparative four-country study, they investigated how faculty in public universities in four countries reconcile their often-conflicting positions at the intersection of academic and market, public and private, economies. Like the merchant scientists in my study, Slaughter & Leslie's academic capitalists did not perceive the two economies and sectors to be at cross-purposes. Research oriented to the market was seen to have social utility, so to subsidize it from public resources was uncontroversial. Merchant scientists/academic capitalists thus do not simply turn *away* from public goals and *towards* the market, they elide the two, and define market values as contributing to the advancement of science and the public interest (179). The academic economy is redefined in terms of the market economy, and public becomes a subset of private.

Where my study goes beyond others is in (1) locating merchant scientists in relation to settler scientists; (2) pointing to the key role of 'translation'; and (3) considering critically the different values attached to each of these categories. The latter task lies well beyond the bounds of actor-network theory, which has no interest in such matters. I need to look elsewhere for a vocabulary.

Philosophers have long distinguished between different types of values.[131] C. I. Lewis for example, identified five different kinds: utilitarian; instrumental; inherent; intrinsic; and contributory.[132] A more basic distinction collapses the first two together as *instrumental values*, encompassing use for a purpose and as a means to an end. The other three are classified as *intrinsic values*, including

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[130] For example, see Susan Leigh Star (1991)

[131] The following two pghs appeared in Grosjean, et al. 2000

aesthetics; things that are good in their own right; and things that are good because they are parts of a whole. Geoff Bowe's rough definition serves: 'things which have instrumental value are good because they can be used to obtain something else. Things which have intrinsic value are good for their own sake, and as intrinsically valuable, they are not exchangeable for something else' (1999:1).

Bowe suggests that a third type: *market values* should be added to this typology. Market values are tied to the emergence of classical economics, which defined them as natural and providential, such that faith in the 'invisible hand'—letting the market decide—was justifiable. Market values determine the price to be attached to instrumental values but intrinsic values cannot be priced. At least in their pure form, they cannot be partitioned into priceable components. But once we *do* partition something that is intrinsically valuable, the market can assign instrumental worth. Bowe says that 'the more we commit ourselves to an ideology that sees value only in instrumental parts, the further we lose sight of the intrinsic value of wholes' (1999:4).

I suggest that these concepts provide a useful heuristic for thinking about the conduct of science. Basic science has intrinsic value; applied science is instrumental, while the life sciences are increasingly characterized by an openness to market values. In the typology of network scientists developed above, 'settlers' seem to be guided by the intrinsic value of science *for its own sake*, 'translators' appear to understand the instrumental value of their research in terms of human health, and 'merchant scientists' seem to be driven by the market value, or price, of what is translated.

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[132] cited in Bowe, 1999: fn.1

I. Argument and General Findings

I began this study with a hypothesis that academic science was turning away from *disinterested* enquiry and open sharing of results towards commercially *interested* enquiry and 'secret knowledge.' The study has supported this hypothesis, but with interesting qualifications. I made an assumption that the primary carriers of this change were new network forms of organization that crossed sectors and institutions and competed with traditional academic structures. Network forms of organization indeed proved ubiquitous but traditional structures seem to be embedded within them. The hypothesis drew on the tension between research pursued for understanding and research pursued for use and on associated attitudes towards ownership and access, secrecy and openness. The study suggests that the first distinction is artificial; the historical record shows that academic science is dedicated to *both* use *and* understanding. However, the boundaries between ownership and access, secrecy and openness, seem to be assiduously policed.

The argument was positioned within the shifting and historically contingent distinctions dividing 'public' from 'private' and 'basic' from 'applied'. My larger purpose was to question the impact of

shifts in the organization and *ethos* of science on 'the public interest'. Results suggest that there are indeed valid concerns about the status of the public interest under the current policy regime, where the emphasis is on converting university research into marketable technologies as quickly as possible.

The 'public interest' concerns may be summarized as follows: First, while policy measures may contribute to national prosperity in the short term, over time they could prove problematic. But because market ideologies are hegemonic, no institutions stand outside of the system of market relations to protect the public's long-term interests. Science, academy, industry, and the state are all aligned along the same short-term economic axis.

Second, policy's economic calculus redirects funding towards commercially relevant research. Because academic science is funding-dependent, resources directed to discovery-based (non-commercial) research may be depleted. Yet this is the research that feeds the public knowledge base. Unless replenished, it could wither away, leaving little for future researchers to build on or draw from. Third, the costs of building that knowledge base were socialized by decades of public support. But now, as the investment begins to pay off, the benefits are being privatized. In other words, under the current regime, policy taxpayers pay twice—first to fund the initial research, then to buy it back in the form of proprietary knowledge. Fourth, the understanding of science as a public good is an important element of the culture of academic research. If science is redefined as proprietary, scientists' self-interest may come to predominate with resulting negative effects. Finally, to a large extent the transfer of public assets and institutions to the private sector is irreversible, yet it is proceeding in the absence of public scrutiny and informed consent. Without broad debate, the process may be viewed as lacking in legitimacy.

In summary: the current science policy regime attempts a fundamental realignment of the public/private divide with possibly far-reaching consequences. Yet the process seems largely *ad hoc,* and meaningful analyses of societal costs and benefits are conspicuously absent. Given the potential significance of the problem, it seems important to enquire into the dimensions of the impact on the public interest. My evidence suggests, however, that it would be a mistake to *overstate* the extent of privatization in these networks or the threat to the public interest that represents.

II Case Study: Conclusions and Implications

Translational research is foundational

The concept of *Translational Research* may be theoretically significant. The results of my study suggest neither 'basic' nor 'applied' accurately captures the empirical reality of much work in biomedical research, or the longstanding 'third space' between the bench and the bedside, the laboratory and the clinic, where this occurs. Translational research seems to fit within what Stokes describes as 'Pasteur's Quadrant', where research is dedicated to both understanding and use. Others reserve the space for 'strategic science', 'emergent science', or 'Jeffersonian science', all of which carry slightly different meanings.

Linking the various explanations of the third space are two common elements: the goal of advancing the public interest or common good, and the blurring of public and private interests in pursuit of that goal. In other words, research in the third space in some way advances the aims of society and the state. It is fed by both *science* and *technology*, not by a linear flow that constitutes it as a way-station between basic research and application. In the Cold War years, for example, this sector provided a home for 'mission-oriented' defence research in the US, largely conducted by private-sector

contractors. Agricultural research into new crop strains belongs here, as does the search for new therapies in the biomedical sciences. In many senses, Vannevar Bush's 'linear' model wrote this third space out of history, but it never really went away. A major finding of my study is the importance of this productive zone at the intersection of former divides. Translation in not a linear process.

Although translational research is an accepted part of clinical practice and drives many funding decisions in the health sciences, there seem to be few settled definitions. The National Institute of Environmental Health Science in the US says that 'translational research is the conversion of findings from basic, clinical or epidemiological...research into information, resources, or tools that can be applied...to improve public health outcomes'. A report from a 1998 meeting of the Breast Cancer Funders Network noted that while 'many of the participants were interested in finding ways to facilitate translational research...it was immediately obvious was that there was no consensus on meaning'. A recent workshop at the Mount Sinai School of Medicine refined an interesting distinction between 'transitional' research and translation. *Transitional* research *translates* discoveries in basic science into clinical applications, then uses the resulting clinical observations to generate basic research foci, in an iterative loop. *Translational* research, on the other hand, focuses 'on the integration of activities from bench to bedside.' The three elements necessary for translational medicine are: 'disease-based programs; access to animal models and proximity to relevant groups of patients, and ease of communications among basic scientists and clinicians.' Dissemination of information across disciplines is fostered by intermediaries such as physician-scientists and graduate students.

To my knowledge the role of translational research, in the sense used here, has not yet been explored in the science studies or science policy literatures. Yet the importance of the mediating activity called 'translation' is well-known. It is a key plank in the platform of actor-network theory where it represents the way powerful actors enrol allies by 'translating' interests. The way the

physician-scientists in my study use the term recalls Callon's sense of 'linkage' but situates the work of translation more as a 'boundary object' (Star & Griesemer 1989) or 'articulation work' (Fujimura 1987). Although translation is the subject of much boundary-work that differentiates it from 'real' research, the NCE program is making it an increasingly 'respectable' activity for scientists to undertake.

But complicating any straightforward definition of the term, my data show *two* distinct but related meanings: translation-to-practice and translation-to-profit. 'Settlers' are more comfortable with the first; 'merchants' are driven by the second. Whereas translation-to-practice is an easy boundary-object to get behind, translation-to-profit is the source of conflicts and resentment. It seems, therefore, that the problem is not the *activity* of translation but the *motivation* of the researcher. The slippage between the two meanings requires further study, as does the whole concept of translational research and the space it occupies. As well, in practical terms, translational research seems to 'fall down the cracks' between health policy and science policy. 'Translation-to-practice' is clearly a significant cultural component of medical research, yet it is not accounted for statistically (Godin, personal communication). If it is not counted, it does not count. With the health sciences becoming increasingly significant in economic terms, it is important to measure and understand the activity of translational research and its extent, as well as the market mechanisms that deliver translated therapies to the bedside.

Spatial dynamics are enigmatic

During my presentation of results and analysis, I detailed the way CGDN's configuration was shot through with power relations and exclusionary criteria. I explained these as issues relating to the network's spatial and structural dynamics: the 'clustering' of regional distribution in three main nodes, caused by the 'spoke and hub' configuration; issues of élitism and equity and the way

exclusionary criteria were used to homogenize network membership; the worrying absence of social reflexivity and lay representation in a network dealing with the social and ethical risks of medical genetics; and the cavalier attitude towards fiscal accountability. The full arguments, articulated in earlier chapters, carry a number of policy implications relating to the structuring of future programs. Ways must be found to overcome the 'Matthew effect' and to maintain variety and heterogeneity. Maximum diversity is necessary for a healthy research system.

An initial spatial-structural problem with the analysis concerned my terminological expectations. By this I mean that because NCEs were *called* networks, I expected to find the weblike patterns and dynamic spontaneity described in network theories.[133] But CGDN resisted this characterization. Many attributes identified in the literature *were* present, as is clear from the analysis. Yet, in many ways, 'the network' did not behave the way 'networks' are *supposed* to behave and did not look the way networks are *supposed* to look (the aforementioned 'spoke and hub' pattern is one example.)

Two reasons can be suggested for this recalcitrance. First, the label 'network' was an appropriation not a description. By this I mean that NCEs were described as networks *before* the fact. The federal government wanted networks so, by an act of naming, networks were mandated into existence. But they were empty spaces awaiting time. 'Networks' are historical achievements. We recognize them *after* the fact. We map past connections, record relations, and describe what we find as 'a network'. Until late in the analysis, I accepted the label at face value as a descriptor rather than as a *desiderata.*

Second, theoretical networks are dynamic *open* structures that spread across space and time. Permeability is a defining element;. networks are unbounded. But because NCEs did not develop 'naturally', their morphology is different. Unlike the heterarchical, transdisciplinary networks

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[133] For example, actor-network theory and Mode 2, or the spatial interpretations of Scott Lash and John Urry (Lash and Urry 1994; Urry 1998) and Manuel Castells (1996).

described in Mode-2 formulations, for example, CGDN has more in common with the Mode-1 'command and control' monodisciplinary model of academic science. Also, while open in its internal dealings, CGDN is a closed interactional order. Admission is 'by invitation only'. The network is bounded.[134] Later networks, however, particularly those funded from Phase III on, do seem to carry more of the attributes of 'networks'. They are more interdisciplinary, for example, and work more in the context of application.

A key element may be the factor of *incorporation.* Before incorporation, networks were genuinely 'ephemeral organizations', acting simply as facilitating agencies for their members; they were not 'a body' (corporeal-corporate). When they took on the corporate form they took on the desire to have their own assets, funds, and future. It was no longer enough to be an 'enabling technology' for others. Philosophically, a corporate structure made a huge difference—one that Industry Canada had wanted from the start. As ephemeral organizations they were dependent on the university. In a way, they could be considered an extreme form of traditional 'sheltered' faculty enterprise. But once incorporated they became separately institutionalized, with a separate trajectory entirely. They were no longer the sum of their parts.

Finally, much of the boundary work and territorial politics between NCEs and host institutions concerns who is the more competent at research management. But it is 'a narcissism of minor differences' since networks could not exist unless sustained by universities, hospitals, and research council funding.

[134] Callon (1998:250-5) seems to have abandoned the notion of networks as infinitely open. Retrieving the work of Goffman, he has recently described the closure of interactional space in networks as 'framing'.

Cultural norms are contradictory

Another analytical problem emerging from this study is the abundance of cultural contradictions. In large part these contradictions relate to the tensions between scientific and bureaucratic rationality, collegiality and commerce, that lie at the heart of the NCE 'system'. In a sense, networks are 'of two minds' because of the dual commitment to scientific excellence and managed research. The oscillation between scientific and bureaucratic rationalities shows clearly in the network's funding proposals, which are carefully crafted to satisfy both traditional disciplinary criteria *and* to demonstrate performance against secular standards like managerial quality and commercial relevance.

Traditional scientific norms--communality, universalism, disinterestedness, and organized scepticism--are honoured *within* the network, where high-trust, high-familiarity climates promote openness and sharing. But they are discouraged by administrators in *external* relations, where secrecy and distrust more often prevail. Knowledge is treated as a communal resource within the network but is alienated outside, where it is 'protected' by administrators as intellectual property. Merchant scientists embody the tensions, expressing allegiance to traditional norms of academic science at the same time as practising the counter-norms of commercial science. Translational research is itself a counter-norm, since it serves the interests of commerce and therapeutic communities rather than being 'disinterested' in the Mertonian sense. Translational researchers, settlers as well as merchants, profit financially from their work, as does the network itself.

In terms of Starr's open/closed dimension we find Russian dolls in effect, opposing the open/public academic values of scientists with the closed/private corporate norms of professional staff, who reflect the bureaucratic rationality of the *program's* gatekeepers. The culture of administrative secrecy prevails over a political rhetoric of openness and transparency. in government, and cultural norms of

openness and transparency in science. At the level of this study, for example, network scientists encouraged the project, were eager to talk to me, and willing to share information with a fellow-researcher. Administrators, on the other hand, in both Ottawa and the network, did everything possible to discourage my enquiries. Requests for information were refused in the name of corporate confidentiality, even though private-sector partners make no such demands, and maintain a fair amount of distance from day-to-day affairs.

Overall, the key policy issue is accountability. Lack of openness and full disclosure results in misinformation, and impedes adequate evaluation of the effects of policy. At both the network and program levels, obfuscation, obstruction, and manipulation of information restricts broad and informed discussion.

Network effects resist assessment

In assessing the effects of the NCE program, we lack what might be called a 'compelling counterfactual' (Mowery, et al. 1999:280). That is to say, given the trends operating in university finances and research after 1988, we do not know what *would* have happened in the parallel universe where no NCE program exists. My research indicates that some $650 million in public funds were recorded as invested in the NCE program between 1990 and 2000. However, insiders suggest that three times that amount, or $1.95 billion, is closer to the actual figure. The NCE program's reporting requirements were so casually policed, until recently, that despite this substantial public investment it is impossible to determine with any degree of confidence what the incremental returns actually are. In other words, we have no way of calculating which of the claimed benefits are actually attributable to the program and which would have occurred anyway, with the passage of time and given the context and conditions. We have no idea of the opportunity costs of allocating almost $2 billion in public funding to one area rather than another. That is to say, we do not know what would have been generated in the *absence* of the NCE program, so we have no way of calculating the value *added* by the program.

For example, we do not know whether the network system has produced more discoveries, patents, and publications than would otherwise have been the case. ('Probably not,' admits one of CGDN's founding scientists). However, my data suggest that the *quality* of research may be higher within the networks, and that the *culture of collaboration* is a key legacy. It is clear that CGDN has had a salutary effect on the way medical genetics is conducted in Canada Further, in CGDN at least, the material and intellectual resources represented by the core facilities constitute much of the 'added value' of the NCE program.

'Public' and 'profit' are problematic

Determining the legitimacy of public funding allocations is problematic because the NCE program has historically emphasized the need to commercialize results. Scientists are financially supported to start up private companies to act as receptors for discoveries that were themselves publicly funded. Further, the private start-ups are generally incubated within public universities. Thus new, high-risk companies are triple-subsidized with public money. The justification used is that these types of subsidies are the only way to build receptor capacity. But an opposing argument can be made that this is an inappropriate use of public funds and that market mechanisms, not government subsidies, are the best way to ensure a viable biotechnology sector.

The focus on start-ups makes conflict of interest a serious concern within the network community. Interviewees worry about protecting the graduate students of merchant scientists, about the relation between research funding and company equity, and about the ethics of profiting personally from public funds. Conflicts of interest at the management level have also occurred in the past, with professional staff demanding and receiving a percentage of equity in spin-offs. Potential conflicts are present when board members are also network partners, when network members appropriate institutional resources, and when commercial results are exaggerated and performance over-reported to justify continuation of public funding.

A number of other contradictions pervade the privatization of public science. First, public institutions (universities, hospitals, NCEs) are the agents of privatization. Second, the state drives the process in order to assist capital. But, third, capital dislikes the process, often complaining that the patenting and licensing activities of public institutions are *impediments* to wealth creation. Fourth, public institutions view privatization as a means of supplementing public funding. Finally, privatization often results in publicly funded investments leaving Canada for development.

Finally, the threat of *removal* of public funding by way of the 14-year cap may be a policy error. Network scientists were impressed that government 'got it right' with the NCE program, accompanied by a high level of buy in. But they are uniformly opposed to the 14-year cap. They feel that the cut-off is arbitrary and ignores the length of the life sciences pipeline Sunset and future sustainability have been 'top of the mind' issues in CGDN for the whole of Phase III. Instead of providing a fertile climate for research and translation, energies have been focused on profit and survival. Goal displacement and culture shock have been profound. The 'fun factor' is important for many scientists. The focus on profit interferes with the serious fun of doing science and belonging to the network. A majority of respondents see the effort to replace program funding from private sources as misplaced, believing that sustainability is doubtful without federal support. If the trajectory of CGDN could be reduced to a cipher, it would read like this: Phase I = 'Foundation'; Phase II = 'Translation'; Phase III = 'Speculation'.

Policy's focus is myopic

Finally, I want to reiterate the danger of focusing policy on scientific *excellence* and commercial *relevance* to the exclusion of other criteria. First, relevance is social as well as economic, but the latter interpretation has been the predominant concern of the NCE program. At least within the biomedical networks, NCE funding underwrites the drive to *commodification*—the production of products and processes that can be sold for profit. Until recently, relevance was seen as synonymous with commodification. Even now, when the program's senior bureaucrats are beginning to factor improved population health into their economic equations, the contribution of network research to that goal is still seen in commodity terms—what one informant calls 'the search for a better pill.' But if we return to our counterfactual universe, what if the $2 billion allocated to the NCEs had been spent on programs to change people's behaviour, thereby reducing risk factors? Or on redistributing

income and services to alleviate the effects of poverty and low social status? These social solutions have no commercial value but they are equally 'relevant', and research consistently shows their effectiveness as determinants of human health. By focusing policy on saleable products, like drugs and genetic tests, we may be missing simple and cost-effective measures that can improve overall well-being. Specifically, in terms of CGDN, claims of broad relevance may be obscuring the point that genetic factors are only a limited component of the overall 'web of causation' in complex diseases.

Second, a focus on 'excellence' may be less productive than nurturing 'hybrid vigour'. My data indicate that for much of CGDN's history, 'excellence' was interpreted in the form of exclusionary admission criteria that limited the variety of research and researchers recruited. While these limitations produced organizational coherence and a strong, core identity, they also constrained vitality by limiting the cross-fertilization that can occur when borders are more open. Hybrid vigour is further constrained when a single-minded focus on commodities and commercial relevance limits discussion and the sharing of results with 'outsiders'. Vigour clearly cannot be derived from the few 'strong ties' within an organization; it requires a wide and redundant diversity of 'weak ties' with multiple outsiders (Granovetter 1973).

III. Suggestions for Future Research

This study indicates a need for further research in a number of areas related to the pursuit of relevance in public science. First, further research is necessary to determine if the findings of this study can be replicated in other life sciences networks. Findings derived from a single network covering a single discipline may be unique to that particular context. Data collected from scientists, administrators, and board members in other life-science networks could provide evidence to validate, refute, or refine the findings and interpretations reported here. Further, comparisons with networks in other sectors would help distinguish the effects particular to the network form, from those attributable to the disciplinary culture.

Second, further study is required to investigate *translational research.* In the first case, a longitudinal study designed to investigate the trajectory of a discovery from the bench to the bedside, and from the lab to the market would be especially useful, as would assessment of the effects of *transitional research* on fundamental enquiry. In the second case, study is needed to develop a means of measuring translational activities and including them in national research and development statistics. Canada has the lowest R&D:GDP ratio in the G7, with the exception of Italy. It could be that the inclusion of translational activities would improve national performance.

Third, we need a detailed study of *merchant science* to map the transition from the university to the market. First, we need to follow what happens to scientist/founders as their companies scale up; anecdotal evidence says that, beyond a certain stage, venture capitalists dilute founding shares to insignificance. Second, it would be useful to know what proportion of merchant scientists stay with their companies and resign their university positions as their companies grow. Also, we need to find a way to accurately measure the cost/benefit ratio of merchant science, taking into account

opportunity costs as well as direct and indirect costs; benefits flowing back to the university, the network, and the state, in the form of dividends and taxation; and social returns and related cost savings in the form of new therapies and better health for Canadians. This would assist in identifying the appropriate level of overall public investment in merchant science.

Fourth, research is needed to explore the long-term impact of new institutional forms, like NCEs, on existing research capacity in universities and hospitals. Some express a fear that the university is being 'hollowed out' as research increasingly moves to public/private institutes along the periphery. What are the constitutional implications of redirecting funding from core university budgets, controlled by the provinces, to research organizations effectively controlled by the federal government?

Fifth, as discussed above, the results of this study indicate that through the use of selective admission criteria and regulation of access the NCE is an intentionally élite program. Further research is needed to investigate the differential benefits accorded to scientists within the program, in comparison to those outside. Because scientists in NCEs have access to more resources than other university faculty, and because certain advantages accrue to scientists in these programs, there is a need to investigate if NCEs are creating a scientific aristocracy at the expense of other researchers.

Finally, from the start, NCEs have been unsuccessful in integrating the social sciences into the program.[135] Only four of the 29 networks funded since 1990 have been in the social sciences. As recently as July 2001, two of the three remaining social science networks were cancelled. Research is needed to determine why the social sciences are not better represented in this élite program, both as

[135] Continuing a tradition begun in (Bush 1945) which also 'forgot' the social sciences

integrated elements in other NCEs, and as 'stand-alone' networks . An important question relates to the returns in social reflexivity that could be added by the social sciences.

IV. Summary

In summary, networks of centres of excellence draw and defend their various epistemic boundaries in a number of interesting ways. In one sense, the boundaries are bound-less. 'We are a nation of scientists. A community of scientists. We are everywhere and nowhere', says Michael Hayden. In another sense, the boundaries are narrowly commercial and closely defended, policed by legally binding undertakings of confidentiality & non-disclosure.

But as I argued earlier, it is somewhat misguided to posit a prior pristine status for 'open science' in its natural state. As I have asserted throughout this study, the public/private & basic/applied distinctions have always been fuzzy categories, constructed in action. The framing and funding of research agendas, and the interpretation and application of scientific results, are contingent and negotiated political achievements, conditioned by the interplay of power relations, market forces, social dynamics, and discursive strategies.

Still, with networks becoming the default institutional structures in which public science is performed, and with public scientists undertaking ever more adventures in the nature of trade, accountability is a valid concern. As in the Human Genome Project, we might want to reassess and reassert our expectations of what is properly open and properly closed, whether in public or private science.

This study of NCEs has not allowed a definitive determination of whether the 'open science' model or the 'overflow/network' model is the better policy choice, since NCEs are bureaucratic

constructions that miss the essence of both. Overall, however, the evidence suggests that a hybrid of the two would be optimal. *Open Networks* would combine the best features of both. *Open networks* would provide a structurally flexible form consistent with the overflow model. But the basic and translational knowledge produced in these networks would not be proprietary, consistent with the open science model. Practically, that would mean handing-off further, pre-competitive development to an arm's length, non-profit entity, as discussed earlier in the dissertation.

APPENDIX A: NETWORKS FUNDED

Figure 12: NCE Program: Funded Networks 1989—2005, sorted by date of first funding

	Sector	Acronym	From	Headquarters Host
Funded Networks	(note)			
Automobile of the 21st Century	**NRI**	**Auto21**	2001	University of Windsor
Canadian Language & Literacy Research Network	**SS**	**CLLRNet**	2001	University of Western Ontario
Canadian Water Network	**NRI**	**CWN**	2001	University of Waterloo
Stem Cell Genomics & Therapeutics Network	**HBT**	**STEMNet**	2001	Ottawa Hospital
Canadian Aquaculture Network	**NRI**	**Aquanet**	2000	Memorial University
Canadian Stroke Network	**HBT**	**CSN**	2000	University of Ottawa
Vaccines of Cancer and Chronic Viral Diseases Network	**HBT**	**CANVAC**	2000	Université de Montréal
Canadian Institute for Photonic Innovations	**ETI**	**CIPI**	1999	U. Laval/ U. York
Canadian Arthritis Network	**HBT**	**CAN**	1998	University of Toronto
Geomatics for Informed Decisions	**ETI**	**GEOID**	1998	Laval University
Math of I.T. and Complex Systems	**ETI**	**MITACS**	1998	University of Toronto
Health Evidence Application Linkage Net[1]	**ETI**	**HEALNet**	1995	McMaster University
Intelligent Sensing for Innovative Structures	**NRI**	**ISIS**	1995	University of Manitoba
Sustainable Forest Management Network	**NRI**	**SFM**	1995	University of Alberta
TeleLearning Research Network[1]	**ETI**	**TL-RN**	1995	Simon Fraser University
Canadian Bacterial Diseases Network	**HBT**	**CBDN**	1989	University of Calgary
Canadian Genetic Diseases Network	**HBT**	**CGDN**	1989	U. British Columbia
Canadian Institute for Telecom. Research	**ETI**	**CITR**	1989	McGill University
Institute for Robotics and Intelligent Systems	**ETI**	**IRIS**	1989	PRECARN Inc.
Mechanical and Chemi-Mech Wood-Pulps	**NRI**	**Wood-Pulps**	1989	PAPRICAN
Microelectronic Devices, Circuits, Systems	**ETI**	**Micronet**	1989	University of Toronto
Protein Engineering Network	**HBT**	**PENCE**	1989	University of Alberta
Non-Renewed Networks				
Concrete Canada	**NRI**	**Concrete**	1989-98	University of Sherbrooke
NeuroScience Network	**HBT**	**Neuroscience**	1989-98	Montreal General Hospital
Respiratory Health Network	**HBT**	**Inspiraplex**	1989-98	Montreal Chest Hospital
Canadian Aging Research Network	**SS**	**CARNET**	1989-94	Concordia University
Canadian Network for Space Research	**NRI**	**CNSR**	1989-94	U of Western Ontario
Insect Biotech Canada	**HBT**	**IBC**	1989-94	Queens University
Molecular and Interfacial Dynamics C of E	**HBT**	**CEMAID**	1989-94	University of Guelph
Ocean Production Enhancement Network	**NRI**	**OPEN**	1989-94	Dalhousie University

Source: Compiled from NCE program documentation

(1) funding expires March 31 2002; not renewed

Note: This is not an 'official' classification. I assigned each network to one of four broad sectors: NRI = Natural Resources and Infrastructure; HBT = Health & Biotechnologies; ETI = Electronics, Telecommunications, Information; SS = Social Sciences

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