Working in the Lab:

Social Organization of Research and Training in Biomedical Research Labs in Canada and its Relationship to Research Funding

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Canada

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Abstract

There are significant indications that a transformation has occurred in the organization of research and post-graduate training in the biomedical sciences in Canada over the last few decades. A typical academic lab in the 1960s was small: a professor, maybe a technician, and perhaps a graduate student or two. However, many labs now have twenty or more members, most of which are graduate students and post-doctoral researchers. Changes in social organization of academic labs during this period and the reasons they occurred have not been systematically studied. Nor has the relationship between the social organization of research and research funding, despite the dependence of most academic scientists in North America on external funding. In my dissertation, findings showed that the social organization of biomedical labs in leading universities in Canada has been transformed over the last few decades. Data gathered from more than 70 in-depth work history interviews done in the context of an ethnographic study of biomedical research labs at two leading Canadian research universities in 2002-2003 suggests that there have been changes in the social organization of work (e.g. division of labour, recruitment, structure of organization and occupation) in the biomedical sciences in these universities since the 1960s , and that these changes, including the emergence of larger labs, were due primarily to effects resulting from an increase in competition for federal grants in the 1980s, after institutional accommodation of external funding made biomedical faculty dependent on them. The main argument is that the major influence has been dynamic federal research funding and its institutional accommodation. Specifically, dependence of an academic career on maintaining competitive federal funding led scientists to change their work and organizing practices; applying for multiple grants, and recruiting graduate students and postdocs instead of technicians as in the past, which led to a transformation in both the organization of research and training. Current dependence of biomedical scientists supported by standard federal grants (as most are) on the work of trainees in a competitive funding environment is associated with the incorporation of trainees into the production of faculty research and publication with several institutionalized practices, using a change in the reward system in science. The career strategy necessary, obtaining multiple grants, also means that individual labs of successful biomedical scientists grow in size. As they do, scientists tend to change their practices, such that the social organization of research and post-graduate training in large and small labs typically differs considerably.

Résumé

De nombreux indicateurs laissent conclure qu'une transformation s'est produite dans l'organisation de la recherche et de la formation au niveau des études supérieures en sciences biomédicales au Canada depuis quelques décennies. Aux années 1960, le laboratoire universitaire typique était petit et accueillait normalement un professeur, un ou deux étudiants en études supérieures et peut-être un technicien. De nos jours par contre, les laboratoires accueillent jusqu'à vingt personnes, dont la majorité est composée d'étudiants de 2e ou de 3e cycle ou en études postdoctorales. Les changements dans l'organisation sociale des laboratoires universitaires ainsi que les raisons derrière ces changements n'ont jamais fait l'objet d'étude systématique. Il n'existe pas, non plus, d'études sur la relation entre l'organisation sociale et le financement de la recherche, malgré le fait que les chercheurs universitaires en Amérique du Nord dépendent de plus en plus du financement externe. Les constats de cette thèse doctorale démontrent la transformation de l'organisation sociale des sciences biomédicales au Canada sous l'influence avant tout du financement fédéral des travaux de recherche. Les données recueillies à partir de plus de 70 entrevues sur l'expérience de travail et effectuées dans le contexte d'une étude ethnographique sur les laboratoires de recherche biomédicale de deux grandes universités de recherche au Canada en 2002-2003 semblent indiquer qu'une transformation s'est produite dans l'organisation sociale de la recherche et de la formation dans ces deux universités depuis les années 1960 et que ces transformations, y compris l'arrivée de plus grands laboratoires, découlent avant tout du fait de la concurrence accrue pour obtenir les subventions fédérales depuis les années 1980 quand les établissements universitaires ont démontré une plus grande acceptation du financement externe créant ainsi une plus grande dépendances des chercheurs en sciences biomédicales. Le principal argument de cette thèse s'appuie sur la dépendance qui existe entre la préservation d'une carrière universitaire et l'obtention de financement fédéral; cette dépendance a obligé les scientifiques à changer leurs pratiques organisationnelles, à postuler plusieurs subventions en même temps, et à recruter des étudiants en études supérieures ou postdoctorales au lieu de techniciens, comme dans le passé. Ces changements ont abouti à la transformation de l'organisation de la recherche et de la formation; le recours aux étudiants en formation implique une plus grande intégration de ces derniers dans la production des recherches à l'aide de diverses pratiques institutionnelles et d'un changement du système de récompense dans les recherches scientifiques. Cette stratégie nécessairement axée sur la carrière et l'obtention de multiples subventions se traduit par l'agrandissement des laboratoires des meilleurs chercheurs en sciences biomédicales et par le changement de pratiques organisationnelles en opposition avec les plus petits laboratoires dont l'organisation est tout à fait différente.

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Chapter 1

Introduction

Investments in science have dramatically increased in North America since World War II, with the largest increases over the last three decades in the biomedical sciences. Postwar funding initially favoured the physical sciences, but there has been a massive shift of funding from the physical to the biological sciences over the last few decades (Mirowski and Sent, 2002). Although research spending by the private sector has increased, the majority of funding for science in universities in Canada is provided by the state.[1] Over most of the last three decades, changes in science and economic policy have meant that increases have primarily been increases in research funding, not faculty.[2] In Canada, the availability of funds for sponsored research in universities increased tenfold between 1980-1 and 2003-4, from 495 million to 5 billion.[3] Total funding for biomedical research from the federal granting agency alone has grown from 80 million in 1980-1 to 758 million in 2005-6.[4]

The hallmark of the model of academic science developed in the U.S. is the integration of basic research and the advanced training of new scientists (Clark 1993). However, considerable change in the structure of research in the life sciences has paralleled these increases in research funding over the last few decades. Thirty or forty years ago, in Canada as in the U.S., "a typical academic research laboratory ...included a professor, perhaps a technician and sometimes a graduate student. Today, many life sciences laboratories include 20 or more people, most of whom are in the process of training to become independent scientists." (National Research Council, 1998: 13). In other words, academic research labs now often have large research groups, and these are typically composed mainly of graduate students and post-doctoral researchers (post-docs).

Associated with these changes in the structure of research has been a major transformation of the structure of professional training in science in North America over the last 30 years, in terms of its structure, aggregate size and likely outcome. First, PhDs have become longer on average (Gumport, 1993a, National Research Council, 1998). Second, in many fields, including the biological and physical sciences, professional training has expanded to include post doctoral experience (Gumport, 1993a). In some disciplines, according to the Association of American Universities, the post-doctorate has unofficially become the "new de facto terminal degree." It found that "over 80% of biochemistry and physics departments surveyed would not even consider hiring someone without post-doctoral experience for a tenure-track position (1998: 12-13). In other words, professional training is now a process involving an additional stage.

There are many indications that this transformation in the organization of academic research and training in the life sciences is related to increases and changes in the structure of research funding. In the life sciences, increases in research funding in the U.S. have been associated with large increases in the total population of graduate students and postdocs. According to the National Research Council, a 42% increase in the number of PhD graduates between 1987 and 1996 in the US was "fuelled almost entirely by the increased availability of federal and institutional support for research assistants" (1998: 4-5). In addition, most PhD graduates seeking positions as scientists now go on to hold post-doctoral appointments, most of which in the U.S. are also funded through work on the sponsored research grants of faculty. The majority of the 36,619 postdocs in U.S.

universities in 1998 were in the life sciences (National Academies of Sciences, Committee on Science, Engineering and Public Policy, 2000: 7-8).[5] The population is so large relative to the number of faculty positions that it is no longer possible to assume that the likely outcome of professional training will be a position as an academic or independent scientist (National Research Council, 1998).

In advanced industrialized economies, knowledge is now seen as the basis for economic growth (Drucker, 1993; Gibbons et al. 1994). Universities have generally been recognised as the primary source of new knowledge and innovation (Slaughter and Leslie, 1997, Etzkowitz, 2002). Gibbons et al. (1994) argue that the mode of production of knowledge has changed in the post-industrial economy, the single investigator disciplinary model shifting to production in multi-disciplinary research teams. Scholars concerned with change in organization of the life sciences have primarily been looking for effects due to commercialization of research and involvement of faculty with industry. Several researchers have studied the changes in faculty activities in response to the increasing commercialization of research (e.g. Owen-Smith and Powell, 2001; Slaughter and Leslie, 1997; Slaughter and Rhoades, 1996; Slaughter et al., 2002; Kleinman, 1998), where the traditional idea of the university scientist is being replaced by the idea of a "scientist-entrepreneur who balances university responsibilities with corporate activities" (Owen-Smith and Powell, 2001, 111), or the activities and organization of the university in response to this change (Etzkowitz, 1999; Kodama and Branscomb, 1999). Although Slaughter and Leslie (1997) have argued that the increasing commercial value of research will mean significant changes in research funding patterns and laboratory life, these issues (and their relationship) have largely not been studied.[6]

The purpose of this thesis is to extend the recent ethnographic study of the social organization of research in the biomedical sciences by examining the relationship between the social organization of research work and training, how it has changed over the last few decades, and relationship of both of these to research funding, in the case of leading Canadian research universities. Ethnographic studies have been important for developing new theory in the social studies of science. Grounding concepts and conclusions in detailed empirical studies is particularly critical in the social sciences during periods of major institutional and infrastructural change (Barley and Kunda, 2001, Shinn 2002) as has been occurring in Canada in the biomedical sciences.

Sociologists have recently begun to do ethnographic studies which do examine the social organization of work in biomedical labs (e.g. Knorr-Cetina, 1999; Owen-Smith, 2001). However, these studies examine labs as production sites, largely ignoring the other process taking place in these laboratories, the training of scientists, and the implications of the fact that most of the workers in these enlarged research groups are not independent scientists, but graduate students and post-docs. In addition, these studies have proceeded without recognition that the local structure of research has *changed* since the 1960s and 1970s. In fact, there seems to be an implicit assumption by some researchers that the structure of research groups in academic laboratories has not changed much over the last century. Etzkowitz (2002) argues that research groups have always been structured as " 'quasi-firms' (faculty member and graduate students)."

Recently, Callon (2002) noted that despite the lab studies of the 1980s (Latour and Woolgar, 1979; Knorr 1981; Lynch 1985), "knowledge of the different forms and profiles of laboratories has made little progress." Labs have traditionally been differentiated in two ways: fundamental basic research and applied research. There was an assumption that basic research was done in academic labs and that applied research was done in industrial settings. In addition, within particular fields, labs have been thought to function similarly. Knorr-Cetina (1999) concluded that the social organization of knowledge production in science is different in high energy physics and molecular biology because the *objects* of research differ. However, organization in labs appears to be more diverse. Laredo and Mustar (2000) identified seven major "activity profiles" of research laboratories which cross both institutional and disciplinary barriers, in one region of France.

Scholars have recently argued that science studies must move beyond the local context of science to examine how the institutional context affects local settings in science (e. g. Knorr-Cetina 1999; Kleinman 1998; Breslau, 2002). The most powerful influences on academic science in the US since the early 1980s are assumed to be increased involvement of faculty with industry and the commercialization of research.[7] There have been studies examining changes in faculty work and student experiences with the increasing commercialization of research in the life sciences and involvement of faculty with industry (e.g. Slaughter et al., 2002; Owen-Smith and Powell, 2001; Kleinman, 1998, 2003). Mirowski and Van Horn (2005) however have recently argued that the major change in the organization of science due to commercialization of research may be occurring primarily in industry, not academia.

Despite the dependence of most academic scientists in North America on external funding, the effects of funding arrangements on the social organization of research and training have not been systematically studied. [8] Science studies scholars in the US have examined the effect of funding on the growth of scientific knowledge in different subfields and specialities, [9] and more recently, the effects of military funding on the content of research.[10] However, there is a growing recognition by researchers in several areas of social science that funding and/or specific national contexts are important for understanding the social organization of research (Clark 1993, 1995; Traweek 1988; Fujimura, 1988; Mirowski and Sent, 2002; Hackett 1987, 1990; Mangematin and Robin 2003; Owen-Smith et al. 2002; Shinn, 2002; Slaughter and Leslie, 1997; Fuller, 2002). More generally, there is also a growing recognition among sociologists studying organizations that "the state is not an external player; it is caught in this process of inventing and consolidating new types of practices and relations" (Callon 2002 citing Block 1994; see Fligstein 1990, 2001). As Cozzens (1986: 10) has argued, "the problem is not that we ignore science policy entirely, but rather that we do not take it systematically into account....or take the role of government agencies in scientific development as problematic in and of itself."

The study focuses on two major questions. First, how has the organization of research and training in the biomedical labs changed over the last few decades, and how is this related to external funding? Second, how are the organization of research and training in the biomedical sciences currently related? No studies have systematically examined these questions.

Since little is known about these relationships, I want to generate detailed empirical data that are embedded in the local context of particular laboratories, within the wider historical context of research funding in Canada, using several different methods. The study uses several ethnographic tools, the most important of these being the work history interview (Li, 1985). The core of the study involves over 70 in-depth work history interviews with graduate students, postdocs, lab managers, technicians and professors in three large basic research labs in the biomedical sciences in two leading Canadian research universities. No other study has used work histories to examine the organization of work in science. Participant observation was also done in each of these labs, and documents relevant to the organization of work in these labs (publications, grants, etc.) were collected and examined. Interviews were also conducted with older and/or recently retired faculty members in the biomedical sciences at these same two universities which focused on their own graduate studies, how they did research in their labs in the past, how students and staff were involved, how they were funded and how this may have changed over the years. Annual reports of the main funding agency, the Medical Research Council (MRC, later the Canadian Institutes of Health Research (CIHR)) were also examined, and key findings from the interviews were later corroborated with statistics.

The first aim of the analysis is to identify key categories of change in social organization of biomedical labs over the last few decades, processes and mechanisms associated with these changes, and their connection to institutional influences, in the case of leading Canadian research universities. The second aim is to develop an understanding of the current relationship between carrying out research in university-based biomedical research laboratories and the post-graduate training of scientists, how this relationship has changed since the 1960s, and how it is related to external influences, primarily research funding.

The findings suggest a theory of transformation in the organization of research and training in the biomedical sciences in Canada since the 1960s, as well as contribute to an understanding of how the organization of post-graduate training is currently related to local organization of research in the biomedical sciences. The implications of these findings for current social organization of work in university laboratories in the biomedical sciences in Canada are examined.

The study therefore addresses significant gaps in the literatures of sociology of science, social studies of science, sociology of education and the sociology of work and occupations; lack of research on the organization of training in science, the relationship between the organization of research and training in science, the effects of the broader institutional context on the social organization of laboratories, and more generally, lack of research on recent historical change in the local organization of research and training in science. The findings make an empirical and theoretical contribution to the social studies of science through development of an understanding of how changes the local organization of training and training in a specific field are related to the broader institutional context, particularly that associated with research funding, which will also be of interest to those studying work and organizations, higher education, and science policy.

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[1] 67.2% of total sponsored research income to Canadian universities came from government sources in 2002 (Statistics Canada data in Canadian Association of University Teachers (CAUT) Almanac of Post-Secondary Education in Canada. 2004, p. 40)

[2] Between 1981 and 1998, the number of full-time faculty in Canadian universities increased only slightly, from 31,099 to 33, 665 (Association of Universities and Colleges of Canada (2002) using data from Statistics Canada).

[3] Current dollars (1980-1 figure- Association of Universities and Colleges of Canada (2002) using data from Statistics Canada, and 2003-4 figure - Canadian Association of University Teachers, CAUT Almanac of Post Secondary Education 2006, using data from Statistics Canada and CAUBO)

[4] Current dollars. MRC/ CIHR expenditures in Grants and Awards. Data from the Canadian Institutes of Health Research (CIHR) database, special request.

[5] Data on the numbers of postdocs in Canada are not collected.

[6] For an exception see Slaughter et al. (2002) "The Traffic in Graduate Students : Graduate Students as Tokens of Exchange between Academe and Industry"

[7] Hackett (1990) has suggested more generally that the organizational culture of academic science is changing due to its resource and cultural relationship with society.

[8] However, Hackett (1987) has suggested some specific effects that competition for funding was having on the social organization of the biomedical sciences in universities in the U.S. in the 1980s.

[9] See papers in Social Studies of Science 16 /1 (February 1986) edited by Susan Cozzens.

[10] See Special Issue - Social Studies of Science, Vol. 33/5 (October 2003).

Chapter 2

Literature review

Surprisingly, none of the major empirical traditions in the social studies of science have explicitly examined the social organization of work in academic research labs (i.e. the division of labour, staffing, and the size and structure of organization). The sociological study of science took a radical new direction in the 1970s, when it began to do ethnographic research on the production of scientific knowledge. Unlike Mertonian sociologists, these researchers went into labs to do observations of scientific work. However, researchers did not explicitly examine the social organization of work in these labs. Nor did they examine the processes involved in the training of new scientists, the other main product of academic science (Delamont et al. 2000; Kaiser 2005).[1] Changes in social organization of academic science over the last three decades and the reasons they occurred have not been systematically explored.

There have only been two ethnographic studies examining the culture of scientific disciplines. However, these studies have important implications for the study of science since they show that these cultures differ in different disciplines, and also that they differ in the same disciplines in different countries. Until recently, the only study which had examined the culture of a scientific discipline was anthropologist Sharon Traweek's (1988) ethnographic study of high-energy physics in the U.S. and Japan in the 1970s. This study showed that there were major differences in disciplinary culture in the two different countries at the time, including in the reproduction of scientists. Even Knorr-Cetina's recent ethnographic study *Epistemic Cultures*, which examines and compares the cultures of high energy physics and molecular biology, did not examine the practices involved in professional training of scientists. However, it is an important contribution to the field since it begins detailed comparative research on cultures of research production in different disciplines in science, *which includes examination of the social organization of local research.* Detailed empirical evidence from her comparative study of high-energy physics at CERN in Geneva and in molecular biology labs in Germany reveals distinct cultures of research in these disciplines, strongly supporting her argument that the culture and social organization of research in different fields differs and need to be studied separately. Knorr-Cetina argues that scientific disciplines have different cultures, because the objects of research in different fields differ.[2] This study, however, did not explicitly examine the relationship of funding to organization, or the processes involved in the professional training of scientists (I suggest that the differences in organization of research between disciplines are not just "epistemic," but are related to funding, both its size and structure). However, the detailed evidence on the social organization of research in the biological sciences from this study, as well as from recent studies by several researchers in the sociology of education studying the socialization of doctoral students in science (Delamont et al. 1997; Delamont et al. 2000; Parry et al, 1997; Delamont and Atkinson, 2001; Clark, 1993; Gumport 1993a, 1993b, 2000) certainly suggest some of the ways in which organization of research *may* be related to professional training. However, these studies did not explicitly examine this issue.

In the sociology of education, few studies of processes involved in graduate education exist. Although a large number of studies of undergraduates have been done, studies at higher degree levels are much less common. Most existing research on graduate students is quantitative, and has been done by education researchers, U.S. organizations interested in graduate education, or government science agencies, not by sociologists.[3] The sociology of education as a discipline, at least in North America and the U.K., has been concentrated primarily on schooling, where schooling is seen as compulsory schooling [4] (Delamont et al. 2000: 4).

In order to fill this important gap in the literature, several sociologists of education have begun to study the socialization of doctoral students (Delamont et al. 1997; Delamont et al. 2000; Parry et al, 1997; Delamont and Atkinson, 2001; Clark, 1993; Gumport 1993a, 1993b, 2000). A group of British sociologists examining doctoral education in the UK see the doctoral period in the sciences as a key phase in the transmission of knowledge (Delamont et al. 1997; Delamont et al. 2000; Parry et al, 1997; Delamont and Atkinson, 2001). What this socialization perspective misses is the involvement of the student as a *worker* in *faculty* research. The role of the student as worker in a production context is invisible in these studies. Further, these studies include the assumption that students will become professional scientists (see Delamont and Atkinson, 2001), and do not recognise that the Ph.D in science results in several different major outcomes. As will be argued in more detail later, an educational or socialization framework is not adequate to understand the processes involved in professional training of scientists in Canada and the US due to student involvement as paid research assistants in faculty research.

Lack of research on processes in graduate education has been recognized as part of the larger neglect of universities in the institutional literature, despite their central importance in modern societies politically, economically and culturally (Kalleberg 2000).

Ethnographic studies in universities are rare.[5] Despite the fact that the university is considered the home of professional science, few studies of universities have been done in the field of science studies (Kalleberg 2000). Studies of the complex bundles of tasks in universities, such as basic research and graduate teaching are particularly lacking (Kalleberg, 2000).

Modernization theorists have argued that Western societies are becoming knowledge societies. However, these theorists tend to see knowledge "as an intellectual or technological product"and have been primarily concerned with the transformative effect of knowledge *on society* (Knorr-Cetina 1999:6). The cultures of knowledge producing organizations themselves have largely not been examined (Knorr-Cetina, 1999). Surprisingly, this is even true of science organizations, despite the fact that science has been studied actively by sociologists of science for over half a century.

These gaps in the sociological literature on science can be understood as rooted in the debates surrounding scientific knowledge and whether it should be an object of sociological investigation. The standard sociological view of science has been based on the mainstream view in the philosophy of science that "scientific knowledge is based on a direct representation of the physical world" (Mulkay, 1979: 21, 60). The main implication of this view has been that "sociology should be concerned, not with the actual cognitive content of science, not with certified knowledge as such, but with the *social conditions which make possible the attainment of objective knowledge*" (DeGre, cited in Mulkay, 1979: 21, emphasis mine). Merton developed a sociology of science, which, based on the sociology of knowledge, exempted the knowledge of the natural sciences and mathematics from sociological investigation. Instead, he sought to outline the normative conditions which make possible the attainment of an objective knowledge. Mertonian sociology of science did not do empirical research in specific research contexts to investigate the actual social processes involved in the production of scientific knowledge.[6] The traditional view was that individuals produce knowledge (Shapin 1995: 300).

In the 1970s, the new sociology of science made a radical break from the previous Mertonian approach. It questioned the exemption of natural scientific knowledge from sociological analysis, after Kuhn and developments in the philosophy of science in the 1950s and 1960s cast doubt on the claim of scientific realists that scientific knowledge is a direct representation of nature.[7] The sociology of scientific knowledge (SSK) extended the questions of the traditional sociology of knowledge to the natural sciences, and extended *empirical* sociological investigation of science to scientific knowledge (Knorr-Cetina and Mulkay, 1983:14). The technical contents of science would be examined as "situated processes of knowledge production and not exclusively as methodological and epistemological concerns," rejecting the study of science abstracted from specific research contexts in philosophy and sociology (Lynch and Woolgar 1990: 3–4). SSK instead argued that scientific knowledge was " constitutively social", and supported this claim using socio-historical case studies to show that social influences affected the production of theory and observations in the physical sciences, mathematics, and statistics. It rejected the old sociology of science approach, the traditional division between the social and the technical, where social explanations for the production of scientific knowledge were given only in cases of error or where findings were found to be fraudulent (Shapin, 1995: 300). The group at Edinburgh had a more traditionally macro social approach, investigating the relationship between sociological variables, primarily the "interests" of particular social groups, and content of knowledge they produced (Pickering, 1992; see Barnes, 1977; Bloor, 1976). The micro-social approach exemplified by Collins (1992[1985]) examined scientific controversies and detailed how knowledge produced was the outcome of negotiations (Pickering, 1992).

Several new micro-social approaches were applied to science studies as the field moved past the concern with the production of theory to investigate what scientists “really do” in the lab. One of these approaches, laboratory studies, concentrated on the laboratory as the local site of the production of knowledge. These studies eventually shifted the focus from the production of knowledge to actual scientific practice (Pickering, 1992).

Ethnographic studies have been important for developing theory in the social studies of science. The theoretical development associated with laboratory studies was constructionism. In contrast with both philosophers and early SSK, instead of seeing the study of scientific knowledge as a study of the relationship between theory and observations, constructivists view “the products of science first and foremost as the result of process of ...fabrication” (Knorr-Cetina and Mulkay, 1983: 118). In this perspective “the study of scientific knowledge is primarily seen to involve an investigation of how *scientific objects* are produced in the laboratory” (118, emphasis mine). The constructivist approach sees the products of science, including fact and theory, as “ ‘occasioned’ by the circumstances of their production ... fabricated and negotiated by particular agents at a particular time and place” (Knorr-Cetina and Mulkay, 1983: 124). Adoption of a micro-social approach to empirical study of science in the laboratory is associated with a concern with the detail of how things were done through local, mundane embodied and physically situated activity, without necessarily a commitment to any existing theory of scientific knowledge[8] (Knorr-Cetina and Mulkay, 1983;7; Pickering, 1992: 7, Shapin, 1995). Ethnographic studies of the "situated processes" in laboratories were not extended until recently to the social organization of research (e.g. Knorr-Cetina, 1999; Owen-Smith, 2001) and have never explicitly examined the training of scientists that took place in those contexts.

My study will extend the recent ethnographic study of the social organization of research in the biomedical sciences by examining the organization of research and its relationship to professional training, and the relationship of both to research funding. More generally, the implications of these findings for the current social organization of work in the biomedical sciences will be examined (i.e. division of labour, recruitment, structure of occupation, structure of organization).

Graduate students in science are not just students, they are workers in the production of sponsored faculty research. In the model of science operating in North America, research in universities depends primarily on the labour of scientists in training, graduate students and postdocs. During the period of growth in science in the US after World War II, and the expansion in the U.S. federal support for university research and graduate education, the need for research assistants by faculty who were engaged in sponsored university research became a major determinant of the size and kinds of graduate programs (Ben- David, 1977). Science began to use technicians, as in engineering and medicine, but unlike in engineering and medicine these technicians were "often in the guise of students" (Glaser, 1964), especially in academic science. In an era where the number of positions for scientists was expanding, this was not problematic.

However, although the period of growth in faculty positions has ended, increases in the availability of funds for sponsored research have continued. The biomedical sciences have seen their biggest growth in research activity in this new environment. Changes in the structure of professional research in the life sciences have occurred; the structure of life sciences research is now built around large groups of graduate students and post-docs (National Research Council, 1998: 4). Most doctoral students in the US are now funded indirectly through work as research assistants on sponsored faculty research projects, while the proportion directly funded through fellowships has decreased (Natural Sciences and Engineering Research Council, 2000; Gumport 1993a). Under these conditions, there is a concern that faculty may be using some students primarily to carry out narrow technical tasks in research projects that contribute little to training (Clark, 1993).

A major transformation in the structure, length and likely outcome of professional training in the biological sciences has paralleled changes in the structure of research. The median length of time needed to complete the doctorate in the life sciences in the U.S. has increased substantially from 6.0 to 8.0 years between 1970 and 1995 (National Research Council, 1998: 25). In addition, another stage has been added to professional training. A postdoctoral appointment is now considered a "virtual prerequisite" for access to positions as independent researchers in the life sciences, physics, chemistry and many other fields. In the life sciences, the terms tend to be the longest, at least 3 years, and five years is common (Committee on Science, Engineering and Public Policy, 2000: 11). The result is that obtaining access to a position as an independent scientist is usually not possible for most students until they are 35-40 years old (National Research Council, 1998). In the last few years, frustration among postdocs in Canada, US and the UK about supporting the research enterprise for longer and longer at low rates of pay with few benefits and little certainty of finding a permanent faculty position is evident in articles and letters to the editor in the news sections of the top scientific journals *Science* and *Nature.* The National Research Council in the US has warned of a "crisis in expectations" for students and postdocs that could threaten the research enterprise as a whole (National Research Council, 1998).[9]

Relevant studies

Detailed empirical research is needed to reveal the processes involved in the organization of research in the contemporary research context and its relationship to professional training. To date, there have been no studies which examine this relationship. Evidence on the social organization of research in molecular biology from Knorr-Cetina's (1999) study, and from recent studies by several researchers in the sociology of education studying the socialization of doctoral students (discussed in the next section) indicates the central role of the supervisor or principal investigator in the direction of research in these laboratories, and of some of the ways that local production of research may be related to professional training. This evidence and its implications for the research problem in this study are reviewed below.

Social organization of doctoral education

A group of sociologists of education in Britain have been studying the differences between social organization of doctoral education in the natural and social sciences in the UK (Delamont et al. 1997; Perry et al., 1997). Their primary focus is on how socialization in the natural sciences takes place in the context of group or team based research. The research group is described as a supervisor or research director with a team of doctoral students and postdoctoral researchers working in the supervisor's topic area on related topics (Delamont et al. 1997).

The supervisors in the research groups they studied were responsible primarily for the research direction of the group, finding funding for the research, and integrating the student into the research of the lab. However the supervisor was not usually involved in guiding or supervising the members of his group on a day-to-day basis. Instead, day-to-day assistance of the PhD students, in terms of practical integration of the student into the work of the lab, and help with mundane problems were primarily the responsibility of postdocs and advanced graduate students (Delamont et al. 1997: 538).

Another key finding of these studies is that in the research group context, the supervisor picked the topics of the Ph.D. research, not the Ph.D. students themselves. The doctoral students they interviewed were not responsible for identifying their topics or the structure of their study. This had been the task of the supervisor, who assumed full responsibility for identifying projects, attracting necessary funding, and assigning them to new students. The students' accounts of their work in the laboratory sciences were expressed in terms of the problems and projects being determined for them. Delamont and

Atkinson (2001) characterize this as construction of Ph.D. projects by the supervisor. Students considered it the responsibility of the supervisor to set up manageable projects which will yield results in the period of time allotted for doctoral study. The doctoral students interviewed also described their commitment to their research topics as something derived from external sources, and as something they grew to be interested in. Delamont and Atkinson (2001) argue that in the natural sciences, student academic identities, loyalties and commitments are the result of ascribed positions in the research group.[10]

The other major finding is that successful doctoral students saw their mastery of tacit craft skills - becoming "good at the bench" - "as their real achievement, and their most important long-standing gain" (Delamont and Atkinson, 2001: 103). They suggest the development of this tacit, indeterminate craft knowledge, acquired through processes of enculturation – by oral culture, by means of trial and error, and through practical example – is the main outcome of doctoral research, despite the fact that this process is not written up in the thesis.

This group is focussed on the doctoral period in the sciences as a key phase in knowledge production and transmission. Their findings show that the skills, equipment and topics were passed down from advanced members of the group, postdocs and advanced doctoral students, to newer graduate students (Delamont et al. 1997). They argue that a key feature of this organization is that it allows a continuity of practice in science which is an important contributor to the stability of scientific work and knowledge (cf. Hacking, 1992).

In the US, Gumport has been using ethnographic research to compare professional socialization of academics in different disciplines , as well as in elite and non-elite institutions (1993a, 1993b, 2000). Unlike the group in the UK, her findings contextualize these processes in the current socio- historical period, and show how disciplinary differences and institutional differences in socialization are related to differential resources. Her study of doctoral training in institutions with unequal status and financial resources (elite and non-elite) showed that professional socialization in same discipline in these different local contexts results in stratified expectations and identities. In physics, for instance, those at the elite university internalized an expectation that they would be research managers (laboratory leaders), while those in the non-elite department learned to be scientific technicians and workers. Although she draws her conclusions in terms of differences in socialization, her evidence actually begins to describe how student involvement in faculty research may be related to differential student outcomes.

Most suggestive to me were some of her ethnographic findings *within* the elite department of physics (Gumport, 2000). Research training was found to be occurring on a primarily instrumental rather an intellectual agenda, with research skills developed and performed for current faculty projects. On the basis of her findings, she argues that differentiation with respect to sponsorship of students was based on performance in the context of faculty research, with the strongest performers becoming the most heavily sponsored (Gumport, 2000). These findings clearly suggest that understanding processes involved in the professional training of scientists in the current research context will necessarily involve study of student involvement in faculty research. They also suggest the inadequacy of any theoretical framework that ignores the role of the student as a worker.

Social organization of the laboratory

Considerable evidence exists to suggest that the experiences of graduate students in science are not primarily determined by educational or professional socialization concerns. In the following description of the typical activities of the members of a biological sciences laboratory, it is evident that the academic laboratory is viewed by scientists themselves *primarily as a production context.* The National Research Council outlines the typical activities of the principal investigator in the biological sciences:

> A principal investigator builds a research group by defining the scientific questions to be addressed, specifying the methods to be used, obtaining the necessary funding, finding suitable research environment, and attracting the research personnel usually a mixture of students, technicians, and postdoctoral fellows. The day-to-day jobs of the principal investigator include those of a *research manager*: making decisions about expenditures and personnel matters, evaluating data, planning the next experiments or observations, providing training for less experienced personnel, and directing the whole enterprise towards the completion of research manuscripts for publication. Ancillary tasks include the writing of grant proposals and such research related articles as reviews of the literature,

> critiques of work of other principal investigators, and the committee work associated with the host institution. Many principal investigators must also teach and administer activities distinct from their own research projects. (P.18, emphasis mine)

Much less detail is provided on the activities of the other members of the lab, but it is evident that the students and postdocs in the lab are seen primarily as research personnel or workers, not as students :

> The *research personnel* in the group usually work on more specific tasks that pertain to the construction of research tools or the acquisition and analysis of data. Group size usually ranges from a few *workers* to around 20; some exceptional research groups are much larger. (P. 18, emphasis mine)

Social organization of research production

Viewed solely from the socialization perspective, therefore, important aspects of student involvement in laboratories in the biological sciences are not visible.

Knorr-Cetina (1999) examined the social organization of the laboratory in molecular biology as a production context. The main themes in social organization she identifies will probably have relevance for lab-based biological sciences in general.

Knorr-Cetina found that the most striking structural characteristics of the molecular biology laboratories she studied was their "dual organization in terms of two levels;" the laboratory level, associated with the laboratory leader, and the level of the individual projects associated with single researchers in the laboratory (p.216, 224). Echoing the NRC description of the typical laboratory in the biological sciences, she observes that the laboratory leader's role in the molecular biology labs studied was fundamentally different from that of the individual researchers in the lab. Laboratory leaders "do not usually continue to do much bench work" (p. 223). Instead, it was the laboratory leader's role to determine the direction of research to be done in the lab, and to secure the resources needed in order to carry it out, and to allocate each scientist his or her "own" project (p. 217). While the laboratory leader was identified with the lab itself, individual researchers in the lab were identified with individual projects.

In these labs, the lab leader saw the laboratory as a production unit comprised of techniques and materials, capable of several lines of research.

> From the perspective of the laboratory leader, scientists are seen in terms of how they stock a *repertoire of carefully selected technical expertise*, which supplemented by *stores of materials* – of cell lines, mice strains, bacteriophages, restriction enzymes, and so on – *constitutes the lab.* (P. 224-5, emphasis mine)

Techniques in the molecular biology labs she studied are closely associated with individual scientists. She argues that the technical expertise of scientists is comprised of skills that are based in “object-centered relationships.”(p. 218). These object-centered relationships, developed within arrangements involving the scientist, materials, instruments and bench space are “the instantiation of individual expertise” (p. 217 -18). Due to their intimate connection to particular objects

> *techniques* in molecular biology and similar sciences travel not just through laboratory protocols, but through “packages” of arrangements that incorporate *scientists and material objects* and need to be recreated in local contexts. (P. 220, emphasis mine)

These object-oriented structures, she contends are also the basis of “management by content”:

> The idea of management by content can be captured by two principles: management that maintains participants' proximity to objects or to the substance of scientific work; and management that substitutes, where possible, object-oriented structures for social authority structures....the link between persons and objects creates a situation where objects cannot be decided upon or acted upon without "'their" [scientists]. (P.171 – 72)

What creates identity for individual scientists in these laboratories is project allocation, techniques associated with particular material objects, and authorship conventions (p. 220).

The processes involved in the professional training of scientists, occurring in the same environments, were not explicitly examined in this study. Knorr-Cetina uses terms like 'scientist' and 'short term researcher' for the members of the labs she studied, describing the laboratory as a setting in which scientists "complete a stage of their career, limited for most of them to two to four years."(p. 225). But since many of these scientists are doctoral students and postdocs, this is not then a stage of their career but more accurately a stage of their professional training. This is emphasized in her later description of qualifications necessary to obtain a permanent position: "at the time of this research, two consecutive "post-docs" (periods of approximately two years) in the same or different labs were often necessary before a scientist could hope to get an assistant professorship or more permanent research position" (p.227). In addition to the "dual organization" of these labs on two levels identified by Knorr-Cetina, therefore, is the organization of the lab to produce two products - research and new scientists. In other words, the lab is both a production context, and a reproduction context. If we look at her findings on the social organization of production in these labs, keeping in mind that the members of the lab are students and postdocs, it begins to illuminate how the professional training of scientists is related to local production of research.

Three aspects of the social organization of production identified by Knorr-Cetina in these labs have important implications for the training of scientists. These are recruitment on technique, responsibility of lab members for the lab as a facility (service work), and the distribution of risk involved with research.

Recruitment on technique

One of the main concerns of the lab leaders Knorr-Cetina studied was being able to maintain technical "continuity" in their labs (p. 227). In recruiting new members to the lab, she found that emphasis was placed not on " 'knowledge' or 'theory', but on technique" (p. 227). Several means were used: "training up" students to work in the lab, recruiting postdocs, or more rarely, hiring into permanent positions when students and postdocs were not available (p. 228). Graduate student recruitment was based on a six week observation period where the student worked with a researcher, after which a decision was made on whether to offer an opportunity to do a masters degree in the lab. Postdocs were recruited based on possession of "techniques" which were needed in the lab, as this laboratory leader outlines:

> ... when I look at the applications, one of the problems I have to fight is that many people who apply are, with respect to their training, not on the same level as the ones who just finished their Ph.D. here. And those, of course, are out.... One has to get people *well versed in a number of techniques*.... (P. 228-9)

What does this aspect of the social organization of the production of research tell us about the processes involved in the training of scientists? If we remember that the completion of postdoc appointments is now a required part of professional training as a scientist, we can see that, at least here, the entrance to last stage of professional training is based heavily on possession of technical expertise needed to carry out research in that lab. Advancing to the final stage of professional training in the biological sciences may then be based on production needs for technical competence – not proposals for research or other intellectual considerations. If completing a post-doc is now required, and post-docs are recruited based on techniques, it is possible that Delamont and Atkinson's (2001) finding in the UK that doctoral students considered their development of craft skills, not the thesis, their most important achievement can be understood as related to the requirements for entrance to the next stage of their professional training. If so, this shows one of the ways in which the expansion of professional training to include another stage in the current research context may have changed the meaning of the Ph.D. Further, if sponsorship is based on performance in faculty research, as has been suggested by Gumport (2000) for physics, we should expect students will be sponsored to the postdoc stage based on technical performance in their advisors research, and recruited to specific post doctoral positions by other faculty on the basis of their specific repertoire of techniques.

Service work

Another aspect of the organization of research in molecular biology labs revealed by Knorr-Cetina's study is that the laboratory is more than the sum of its individual projects. In addition to their responsibility for their "own" research projects, "each researcher carries a load of service functions to help maintain the lab as a facility" (p. 226). Service work in the molecular biology labs examined was of three main types:

products obtained by one researcher which were also needed by others, exchange of information between laboratory members, and, training of students (p.234). Service work was performed either for other members of the lab, or for other labs through arrangements made by the lab leader.

Knorr-Cetina argues that service work in the laboratory "appears to be construed within a logic of exchange" (p. 236). However, since equivalents as are difficult to determine, it was a source of tension and conflict in the lab (p. 234, 238). Service provision also creates tension because much of it is invisible to the outside world. Senior members of these labs resented responsibilities for training students because they considered their purpose to produce research. Even when service work is supposed to be visible in the authorship credits in a paper, this was not guaranteed. According to authorship conventions in molecular biology

> the first name author contributed the most in terms of performing the actual inquiry, the ones in the middle have provided specific ingredients, and the last-named author is the laboratory leader who supplies the resources, guides the direction of research, and contributes ideas. (P.167)

However, individuals making "specific ingredients" were not necessarily credited in the authorship credits of paper. According to one postdoc who was asked about whether the providers of protein extracts or cDNA made in his lab would be named as authors in papers involving their use:

> That's a problem. You don't know that. No... You are a member of the lab, that's a service you provided, you have to give it to everybody. You can't say anything against this argument. (P.234).

In the laboratories studied, despite authorship conventions, final decisions about who will be first-ranked author on a paper were made by the laboratory leader. Quarrels over who should be the first ranked author can involve disagreements over what constitutes service work *after the work is done*. In the instance she describes, these definitions are externally derived:

> Then there was this competence quarrel, who should be first, second, third author. And actually sometime ago there had been a decision that I would be first ranked on this paper. This is now overthrown, [the laboratory leader] doesn't do it, now someone else gets to be [first author]. And then I said I am not going to continue like that... And then everybody said, you need your own project, because they hadn't seen these transgenic works as my own project, only as service [to the lab]. (P.226)

We can see from a production perspective therefore another process that was not visible from the socialization perspective. Service work is done by members of the lab that is not directly required for their own research projects. Some of it may be connected to "their" material objects (e.g particular blood cells, specific protein molecules), and some to functions necessary to maintain the lab as a facility but which are not necessarily directly connected to their objects, such as training students. If we view the lab as a training context in addition to being a production context, and remember that some of the researchers are students and postdocs, unequal distribution of service work could slow the progress of some trainees relative to others through their degrees in the same laboratory, or limit their access to authorship credits in publications necessary for obtaining permanent positions. It is not clear how the service work is distributed.

Risk

Another important aspect of the production context in these labs with implications for the training of scientists is the distribution of risk associated with individual projects in the lab. The lab leader in the molecular biology labs studied by Knorr-Cetina "saw the laboratory as the distribution of lines of research, each of which carried varying risks and varying chances of failure and success" (p.230). Risky projects potentially benefited the lab, but individual researchers often paid a price for the risk taking: "... some participants end up not producing interesting results, having to switch research topics, taking too much time or all of these." (p.230). Projects involving the highest risk were assigned to doctoral students:

> Doctoral students, in this view, were better risk takers for many reasons. Compared with postdocs and senior researchers, they are still under less pressure to publish quickly, copiously, and in good journals.... Also, doctoral students were considered to be more willing to take risks -- out of

> a sheer lack of knowledge about the kinds of trouble they would encounter, and perhaps out of greater confidence in a laboratory leader who tends to be enthusiastic about risky research.... Doctoral students are more suited for risky projects, because even if the results are few and nonrevolutionary, a dissertation may often be wrested from them. Laboratory leaders tried to ensure students complete their PhDs, but they may not be able to do much for an unsuccessful postdoc, for whom only publications count. (P.230-1)

Evidence of this kind of distribution of risk in sponsored research between the two stages of professional training is also found in a recent report on university bioscience in the US (Freeman et al. 2001). Principal investigators, in order to compete successfully for funding, made proposals with a high probability of success to federal granting councils, which were often extensions of previous work.[11] In turn, they

> ...often assigned postdocs to these projects, since post doctorate careers depended on successful completion. The PI's did more innovative and risky research "on the side" or with other money, and assigned or encouraged graduate students to undertake such projects (since they had more time to recover from failure) (P. 17).

According to a principal investigator (PI) interviewed, "the market for new faculty had changed from one where offers were based on promise to one where they were based on accomplishment" (p.7).

In these laboratories, the production context may interact with the training context by allocating the projects with the highest risk to doctoral students. In a production context, the student not only does not choose his project, but does not choose his level of risk. How is this risk distributed *among* doctoral students? Although a thesis may be written for a project producing few results, Knorr-Cetina emphasizes that "when research is organized in terms of individually attributed projects, research failures are individual failures" (p. 231). What consequences do failed projects have for doctoral students? The identification of this particular risk distribution process in biology labs suggests another way that development of professional training of scientists into a two stage process in a production context may have changed the Ph.D.

The changing nature of work in the post-industrial economy

According to Barley (1996), despite tremendous interest in organizational transformation in organizational studies, "researchers have paid almost no attention to how these organizational developments might either reflect or affect the changing nature of work." When work is discussed, it is discussed using terms like flexibility and complexity, but detailed investigations of "what people do and how they do it are rare" (p.405). Analysis of organizational change in research science has largely proceeded without an empirical examination of how the *nature of work* in this field has been changing.

Technicians: transformation and caretaking at an empirical interface

Barley argues that a new ideal type of occupation exists in the post-industrial or knowledge-based economy, that of the technician. Based on extensive ethnographic work, he found that the fundamental characteristic of technicians work is that they work at "an empirical interface: a point at which a production system met the vagaries of the material world" (p.418). He concludes that the core of their work lies in creating linkages between the material world and the world of representations.

Linking the material and the representational, he argues, involves two processes, transformation and caretaking. Technicians used sophisticated instruments, techniques and bodies of knowledge to *transform* material entities into signs and symbols. Science technicians, for example, used instruments and protocols to produce data and charts from physical and biological material. In addition to generating signs and symbols, technicians were also responsible for *taking care* of the material entities such as machines, organisms and other physical systems from which they generated data.

While the fundamental characteristics of technicians work was similar across all occupations they studied, they found that technicians were situated in one of two ways in the local division of labour, either as "buffers" or "brokers." Science technicians were of this first type. They produced data that became the "input" for the work of professional scientists. In doing so they stood between the scientists and the material world:

> ... these technicians did much more than produce data. They buffered the professionals who used the data from the very empirical phenomena over which the latter were reported to have mastery. For instance, because science technicians operated lab equipment and conducted experiments, it was they, rather than the scientists, who presided over a labs' encounters with the physical world. Thus, the scientists did not have to concern themselves with the practical uncertainties of empiricism. (P.420)

Barley's other main finding was that contextual knowledge, not formal knowledge, is most critical for technicians to work effectively at the empirical interface. This knowledge involved more than experience: "...by experience technicians did not simply mean years of practice. Instead, they meant a situated, rather than a principled knowledge of materials, technologies, and techniques" (p.425). In addition, the contextual knowledge was distributed among technicians.

Barley argues that the finding that contextual knowledge is most important for technicians challenges the traditional notions that the technician is a junior professional whose knowledge is a subset" of the professional's knowledge (p.424). Sociologists have argued that technicians operate within an established professions field of knowledge, where this profession is acknowledged to control the entire knowledge system of the technician, both theoretical and practical (see Keefe and Potosky, 1997: 54-55). Instead, he found that technicians often possess substantial contextual knowledge that professionals did not have, and that this created dependence of the professionals on technicians.

Implications of existing evidence for the social organization of work in university laboratories

A picture of the organization of research in laboratories in the biomedical sciences and how it is related to professional training begins to emerge from existing studies (and evidence from my own pilot interviews).

Existing studies and initial evidence suggests that similar to the shift in high energy physics several decades ago, university labs in the biomedical sciences now have a more organizational structure of work, as opposed to the traditional occupational organization of work in science, where a master-apprentice relationship exists between the scientist and graduate students. From the review of some of the key findings of existing studies, professional training of scientists appears to be a secondary function in a production context. Recruitment of trainees and staff to these labs is based on the need to acquire or train for techniques for current lines of research. Although studies of biomedical laboratories did not discuss sponsorship of doctoral students and postdocs (to postdocs and to permanent positions), findings of existing ethnographic studies of physics have described sponsorship as the responsibility of the supervisor (Traweek, 1988), and found that those most heavily sponsored were the best performers in the context of production of faculty research (Gumport, 2000).

In all existing studies, the biomedical labs studied have multiple lines of research, which involve use of different techniques and have different levels of risk associated with them. Technique has emerged in the existing studies as a major factor in the organization of research and its relationship to professional training, in that it is important in recruitment, training, division of labour, the identity and prominence of individuals in the lab, and may be tied to sponsorship to postdocs.

Evidence from existing research also indicates the expansion of professional training to include the postdoctoral phase, in a production context, has changed the Ph.D., as well as the bases on which independent scientists are now hired. From the production perspective, the Ph.D. is at least sometimes used to do risky faculty projects. From the professional training perspective, it may be used to develop technical expertise, necessary for sponsorship to the next stage of postgraduate work, the post doctorate. The postdoctoral period, from the production perspective, is at least sometimes used to do more conservative research. From the training perspective, the post-doctoral period seems to be used to build a body of publications of successful research on which to be hired.

There are some parallels between Barley's ideal typical characterization of technicians work and what recent studies have found that characterizes the work of graduate students and postdocs in university laboratories with large research teams in the biological sciences. Technicians, graduate students, and post-docs in the biomedical sciences all work primarily at the empirical interface (at the bench) transforming material into data and charts. For all of these groups, contextual knowledge or craft skills have been found to be the most important. Delamont and Atkinson (2001) found that students considered the tacit, craft knowledge they acquired during their Ph.D.'s their most important achievement. Knorr-Cetina (1999) found that postdocs in the molecular biology she studied were hired on the basis of their repertoire of specific techniques learned during their Ph.D. Several studies have found that scientists do not work at the bench, and are not responsible for the day-to-day training of students. Instead, this was found to be the responsibility of postdocs and advanced graduate students (Knorr-Cetina, 1999; Delamont and Atkinson, 1997) or technicians. Barley noted that in the molecular biology labs he studied that graduate students and postdocs "learned empirical procedures largely from technicians" (Barley 1996; 430). These findings suggest that the traditional characterization of the students' relationship with their supervisor cannot be characterized as master and apprentice, since the skills acquired are not being learned from the supervisor, but from others who work at the bench. The relationship of the student with the supervisor instead seems to involve primarily the supervisors' allocation and funding of their projects, direction and planning of their research.

When research in universities is performed in large teams in the biomedical sciences, it is suggested that not only has scientists' work that has changed, with the scientist becoming an entrepreneurial manager, but the work of the students and postdocs as well. In university laboratories, where there are typically few technicians, it is suggested that the work of scientists-in-training may be fundamentally that Barley has described for technicians, in the broad sense that it is they, not the principal investigator, that work at the bench, on projects selected and planned by the principal investigator. Latour and Woolgar (1979) described the transformation of material into representations ("inscriptions") as the work of *scientists*. It is suggested that a *general* technical division of labour may have occurred in the biological sciences where work at the empirical interface involving technologies and materials has been split from work of designing and planning research. More generally, this may mean that the structure of work in science is changing, shifting from the traditional occupational structure of work towards an organizational structure of work, where someone designs and plans the work while others carry it out, using materials, space and technologies they do not control.[12]

[1] Callon (1995) described four models for the dynamics of science, 1) science as rational knowledge, 2) competition, 3) sociocultural model and 4) extended translation. He observed that the sociocultural model is "paradoxically, only moderately interested in questions of organization and institutional forms. This observation applies as much to the internal organization of scientific activity as to its relations with the socio-political environment." It emphasizes instead the role of learning, "stressing the importance of skills transmission and training." Similarly, Model 4 has little to say about organizational forms.

[2] She argues that this diversity of "epistemic cultures" reveals the fragmentation of contemporary science and challenges the argument for the unity of science (Knorr-Cetina 1999:3).

[3] Education literature on graduate studies is primarily concerned with many of the same issues as the literature on undergraduate education (e.g. attrition and persistence). Literature by associations or government is survey research, statistical reporting, guidelines for best practices or, reports of committee findings prepared in order to make policy recommendations. Some literature also exists on the recommended role of the supervisor and supervisory experiences.

[4] Education became a focus of sociological research in North America only after World War II. The focus was on education as promoting integration, consensus, and on the role of education as promoting inter- and intra-generational social mobility. This research was largely quantitative and did not examine the social processes producing the effects measured. It was much later, in the 1970s, that ethnographic studies of schools were carried out (Burawoy, 1991: 20).

[5] According to Marcus (1983), this is part of a larger scarcity of studies of the internal cultures of elites.

[6] Mertonian sociology of science views science as a social institution which is seen as having a goal, production of knowledge, and norms that govern behaviour required to achieve this goal. The normative principles were outlined by Merton as universalism, communism, disinterestedness, and organized scepticism (Merton [1942] 1973).

[7] Although the Duhem-Quine thesis of underdetermination of scientific theories by evidence and the thesis of the theory-ladeness of observation did not necessarily mean that social factors must be relevant in explaining why scientists choose particular theories, it did suggest the possibility that social factors could be involved, and that production of scientific knowledge need not be exempt from investigation by social scientists (Knorr-Cetina and Mulkay, 1983: 3-5).

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[8] One of the main claims of SSK, which is considered radical given the traditional view that individuals produce scientific knowledge, was the scientific knowledge was not the product or the possession of individuals, but instead was produced, held and changed collectively (Shapin, 1995:300). SSK characterized scientific knowledge as a conceptual net (Barnes 1983) based on the earlier concept developed by philosopher Mary Hesse, where learning knowledge of particular culture to involves interacting with a competent member of the culture in a particular environment (p.22). According to Pickering (1992) SSK saw the extension of the conceptual net to new situations as a process of modeling, where extensions can be made in the an infinite number of directions (p.4); the achievement of closure - the direction in which extensions would take place - was seen as something involving interests of certain groups, based on instrumental considerations. However, the image of practice that can be derived from SSK, he argues, is "thin" is one of an "open-ended process of modeling structured by interest" where scientific knowledge is not the direct representation of nature, as is held by realists, but rather " knowledge relative to a particular culture, with this relativity specified through sociological concept of interest" (p.5).

[9] See "The World of Postdocs" *Science* 285 for a collection of articles on the current situation and issues for post-docs.

[10] Delamont and Atkinson (2001: 94-5). They contrast the modes of socialization in the social and natural sciences as positional and personal modes of socialization, terms derived from Bernstein's characterization of modes of socialization in families. They argue that in science, as in positional families, social roles are primarily ascribed, and identity is determined in relation to a closed set of roles and relationships. In contrast, in social science, as in a personal family, socialization is based on achieved identities (Delamont et al. 2000).

[11] While working in a lab as a technician for a university professor, I was told the. proposals for research grants are often based on work which has already been completed.

[12] See Watson (1987) for the differences between occupational structure of work and organizational structure of work.

Chapter 3

Methodology

I. Methodology

This thesis used a multi-sited ethnographic case study design to study the relationship between organization of research and training in biomedical labs, as well as how it has changed over the last few decades, and its relationship to external influences, primarily research funding, in the case of leading Canadian universities.

My approach is consistent with Burawoy's approach to the ethnographic case study (1991, 1998, 2000, 2003). The thesis examines the connection between internal processes involved in the organization of research and training in labs, and external influences. My focus is on how the internal relations in the site, the lab, are influenced by connections to external forces, in historical context. In other words, I'm looking at how sites are produced and transformed in connection with dynamic external forces (see Gille and O'Riain, 2002).

A case study design was used since little is known about the processes involved in the local organization of research and the professional training of scientists, and I wanted to generate detailed empirical data that are embedded in the local context of particular laboratories, and the wider context of research funding in Canada. The case study is a method of empirical inquiry particularly suited to the investigation of "a contemporary phenomenon within its real-life context, especially when the boundaries between the phenomenon and context are not clearly evident."[1]

Although case studies are assumed to lack generalizability, several social scientists have argued that while they are not statistically generalizable (generalizable to populations), they are generalizable to theory (see Snow and Anderson, 1991; Yin, 1994, Burawoy, 1991). The case study can be used to generate claims about "societal significance" rather than "statistical significance" (Burawoy 1991: 281), "where the former refers to the development of ideas of theoretical and practical import and the latter refers to the finding that an association of two variables is not the result of random variation" (Fitzgerald, 2006: 15). Similarly, Glaser and Strauss (1967), Strauss and Corbin (1998), Denzin (1989) argue for an empirically grounded, naturalistic generalization which can either generate or clarify theory. Yin (1994) argues that the theoretical generalizability of case studies benefits from development of a prior theoretical framework to guide data collection and analysis.

II. Key concepts

The approach used in this study is theoretically innovative because graduate students will be conceptualized both as both workers *and* students. Although recent studies of graduate students in science by sociologists of education concentrate on the socialization of doctoral *students*, evidence from other studies of biology labs as production contexts (Knorr-Cetina, 1999) and reports by scientists describing the situation in labs in the biological sciences (National Research Council 1998), show the students are primarily viewed as workers involved in carrying out faculty research. My approach is also innovative because the laboratory will be studied not just as a production context as in the few existing studies of social organization of labs in the biomedical field, but as a professional training context. It is argued that since most of the researchers in the labs are graduate students and postdocs, the organization of research and professional training are intricately linked.

Further, professional training in science in this study is conceptualized as involving both a Ph.D. and postdoctoral work, based on evidence that obtaining a position as an independent scientist is not usually possible without postdoctoral experience, as well as other evidence that principal investigators distribute projects with the assumption that professional training as a scientist will include post-doctoral appointments (Knorr-Cetina, 1999; Freeman et al., 2001)

Another basic methodological strategy used in this study borrows from one of the main strategies employed earlier by those doing laboratory studies to study the production of knowledge. Laboratory studies used ethnographic techniques to study "unfinished knowledge"- processes involved in the production of knowledge before it is accepted as scientific fact (Knorr-Cetina, 1995: 140). In this study, I use ethnographic techniques to examine the work experiences of "unfinished scientists" - in order to study the current social organization of work in the lab and how it is related to the professional training of scientists.

III. Research questions

The study focuses on two major research questions: 1) how has the social organization of research and training in biomedical labs changed over the last few decades, and how are these changes related to external influences, particularly research funding? 2) How are the organization of research and training in biomedical labs currently related? These two questions are addressed with more specific questions; 1) How was research and training in labs organized in the past (e.g. composition of labs, division of labour)? 2) How did this change over time? 3) What were the processes and mechanisms involved? 4) How are students and postdocs involved in research in their laboratories? 5) What do these findings tell us about the relationship between professional training and the organization of research? 6) What do these findings tell us about how research funding is related to the local organization of research? 7) What are the broader implications of these findings for the current social organization of work (i.e division of labour, recruitment, structure of occupation, structure of organization) in research in the biomedical sciences?

IV. Methods

The study uses several ethnographic tools, the most important of these being the work history interview (Li, 1985). The core of the study involves 78 work history interviews done in the context of three large basic research labs in the biomedical sciences in two leading Canadian research universities. Work history interviews were conducted with 70 graduate students, postdocs, lab managers, technicians and professors in the biomedical sciences in these universities between October 2002 and November 2003. The interviews, which ranged between 2-5 hours in length, generated detailed data on the organization of research and training in specific laboratories. No other study has used work histories to study the organization of work in science. Participant observation was also done in each of these three labs, and documents relevant to the organization of work in these labs (publications, grants, etc.) were collected and examined.

Detailed empirical research is needed to reveal the processes involved in the organization of research in the contemporary research context and its relationship to professional training. To date, there have been no other studies which examine this relationship. In-depth work history interviews with students about their work and experiences in graduate studies generated rich data on the organization of research and training in specific local sites. No other study has used work histories of trainees to study organization of work in science. Examining detailed student work histories in the context of their own particular laboratories allowed me to investigate how student involvement in faculty research is related to professional training. Further, examining these work histories allowed examination of the organizing activities of the supervisor, both in terms of organizing the production of research in the laboratory, as well as the training of scientists, and how they may be connected.

The work history interview also gave students a focus to move through their experiences over the years spent in their laboratories, around which they were free to elaborate on other aspects of their experiences both formal and informal, inside and outside the laboratory. Lab members were interviewed about their work histories beginning with their first involvement with doing research, and therefore their work histories often covered their experiences in two or three labs. All students and postdocss were asked about several key issues identified as potentially important for the organization of research and training in the review of existing literature and in my pilot interviews. How did the student come to be a graduate student? Why in this particular lab (recruitment)? What was the composition of this lab (how many students/postdocs/technicians/junior faculty, men /women) ? How did it change over time? What did the student do in the lab? Did the student have a particular project? How did they come to have that project? What was involved ? How did it change over time? How was the supervisor involved in their research? How were other people involved in their work/projects (training, provision of materials, advice, technical assistance)? How was the student funded over the course of their time in the lab? What expectations were associated with this funding? What else did the student do while in that laboratory (service work, weekly meetings, attending conferences, publishing papers, Friday beer night, etc.)? What were the students expectations, difficulties, successes, etc? Will/did the student complete a thesis? Did the student publish? What was involved? What will be/was the outcome (terminal Ph.D, post doctorate, leaving grad school)? How did this come about?

Data on biomedical research labs have not been systematically collected in Canada.[2] In order to begin to explore how the organization of research had changed in biomedical labs in these universities since the 1960s, I did exploratory work history interviews with eight older and/or recently retired faculty members in the biomedical sciences at these same two universities. Professors were asked about their own graduate studies, how they did research in their labs in the past, how students and staff were involved, and how this may have changed over the years. All of the older and/or retired professors interviewed became faculty in the biomedical sciences at one of these two universities in the 1960s or 1970s, and six of them also had done graduate studies and/or postdoctoral work in these same universities starting in the 1960s or early 1970s. The research experiences of all of these scientists therefore covered at least three decades in these universities and more generally in the Canadian research system. They had (or had had in the past) small or medium sized basic research labs during their careers (6 and 2 of the scientists respectively). Three of the older scientists interviewed had active research labs at the time of the interview and the other five were recently retired (four still had offices and/or labs in their departments). Five were men and three were women. The interviews were carried out with the scientists in their offices, in one session lasting between 1.5 and 3.5 hours, tape recorded with permission, transcribed and analyzed.

The interviews in the study were carried out with the understanding that confidentiality and anonymity would be protected. Some of the key findings from the interviews were later corroborated by quantitative data from the federal granting agency. Annual reports of the main funding agency, MRC (later CIHR) were examined, and key findings from the interviews were later corroborated with statistics.

Data analysis was ongoing throughout the research phase. I was looking for themes supported by several sources. Although each laboratory is unique in some respects, it is also an example in a broader class of organizations (labs), which exist in the same but changing national funding context. The goal was to develop a set of generalizations from the themes and processes identified in the data, which were compared with existing theories in order to generate (or modify) theory.

V. Research sites

All three of the large labs studied were headed by faculty members in basic research departments of the medical faculties of leading Canadian universities, who had a relatively large research group working at the forefront of biomedical research. Both universities are important centres of biomedical research in Canada, and are training relatively large numbers of graduate students and postdocs. All universities in Canada are publicly funded institutions. As is typical for academic research labs in the biomedical sciences in Canada, these labs receive and have received the majority of their research funding through funding agencies of the state.

VI. Research design and analysis

The three large labs were initially selected because their funding profiles differed; one of the labs was funded by primarily with standard operating grants, as are most labs in the biomedical sciences in Canadian universities, and the other two labs also had new types of funding from the state for genomics research and infrastructure, in addition to standard grants. Initially, I had been planning to examine and compare the organization of these labs, and compare it to the organization in labs in the past. However, I did the interviews with five of the older and retired professors before starting the lab studies. These interviews were exploratory work histories, where I asked about their own graduate studies, how they organized research in their labs at first, but in addition, followed their histories through the last few decades. I began to uncover a process by which the organization of research and training had changed in the case of the biomedical sciences in Canada, and how it involved research funding.

These work histories provided such rich detail about processes that instead of just interviewing the lab members of the three large labs, as planned, about their work histories in their current labs, I did all of the formal interviews with lab members as work histories of *their whole research experience,* which for many had been in more than one lab, and in some cases, as many as three. Through this technique I was able to access trainees' experiences in many other labs, small and large. In the analysis, through the accounts of investigators and trainees about their work experiences, I was able to compare key aspects of the current organization in large and small labs, with those in labs in the past, and relate them to the process of change that had occurred, which eventually led to theory of transformation in the organization of research and training due to dependence on standard federal grants. Since this analysis was clearly interesting and needed to be done prior to a thorough analysis of the difference between labs with standard and new different types of federal funding, the comparison of the three large labs themselves was not done as part of the analysis in this study. In other words, my empirical contact with the field modified the analytical focus, but it did not change the core elements of my research questions, that is, how research and training were related, how this relationship had changed, and how both of these issues were related to research funding.

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[1] Yin (1994: 13). Yin argues that a case study is a distinct form of empirical inquiry, which differs from histories, surveys and experiments in the extent of the investigators control over and access to actual events. Histories are the preferred method when there is no access or control, and are used to study non-contemporary events. In an experiment,

the investigator has control over actual events and separates the phenomena from its context. In a survey, ability to deal with context is extremely limited (1994, 10-13).

[2] With the exception of a survey of research personnel in labs conducted in 1965-6 by the Medical Research Council (1966a).

Chapter 4

Social organization of biomedical research labs: socio-historical dynamics and the influence of research funding

The purpose of this chapter is to extend the recent ethnographic study of the social organization of research in the biomedical sciences by examining how the social organization of research work has changed over the last few decades, and relationship of these changes to the structure of research funding, in the case of leading Canadian research universities. The aim of the analysis is to identify key categories of change in social organization of biomedical labs in these universities over the last few decades, processes and mechanisms associated with these changes, and their connection to institutional influences.[1] The analysis draws primarily on the subset of interviews done with older and retired professors, but is informed by all of the work history interviews as well as the participant observation done in the large labs in these universities. Some of the key findings from the interviews were later corroborated by quantitative data from the federal granting agency. Although the numbers of older and retired professors interviewed was not large, the interviews generated rich data on how social organization of research in labs in these universities had changed over time, and represent the beginnings of a dynamic analysis of the social organization of work in biomedical labs in these universities. [2]

The structure of external funding in the late 1960s and 1970s and its consequences for the social organization of work

In the mid 1960s, most biomedical research labs in universities in Canada were relatively small. A survey of biomedical labs in 1965-6 found that 75% of the investigators in basic science labs had four or fewer members in addition to the professor.[3] Only 8 labs had more than 10 members. Many labs in these leading universities are now considerably larger.

Evidence found in the study suggests that the small size of most labs in the 1960s and 1970s was related to the structure of external funding. Although many professors in the biomedical sciences today have more than one grant, it was discovered that professors in the late 1960s and 1970s typically had one operating grant, and this grant was from the Medical Research Council. [4] It actually wasn't part of the culture to have *more* than one grant. A retired professor who started as a graduate student at one of these universities in the mid-1960s emphasized that her graduate supervisor's situation was very unusual because he had two grants, not one.[5] Instead, professors kept applying to renew the grant they had, and renewals were described as relatively assured if they had been publishing at the rate of one or two papers a year. A professor who became a faculty member in 1978 describes the situation in the late seventies and early eighties:

> Now for the early years the funding situation was nice. It was...you had a grant, usually one grant. ...I had my grant in the MRC. I had been sort of funded uninterrupted from the MRC from 1980 till now. ...And then your

> renewal would come up and if, you know, you had been productive in the last few years, the renewals were almost automatic....It was kind of almost a given then. Once you were in this business, as long as you did your bit and you published a few papers and all that, you would be automatically renewed ...And it went on for a while. ...And then, I mean at that point in fact it was a no, no to write two grants. You had one grant, that was enough. [6]

Since the norm (then as now) was that a professor paid all the technical staff and trainees in his or her lab from the grant (except trainees who had studentships and fellowships), most academic labs in the 1960s and 1970s were relatively small. Another older professor still actively doing research explains:

> Actually one of the grants. ...I've had since '79. It's been renewed every time. So, that was the original grant, and it's still there. But it was my only grant for many years. I didn't have other funding or other grants. So, you really basically operate on one student and one technician, two students and one technician and that's it, 'cause that's all it would support. [7]

In Canada, the primary source of funding for biomedical scientists over the last few decades been the federal granting agency, the Medical Research Council of Canada (MRC). This agency was created in 1960 to fund biomedical research in Canadian universities.[8] The program has been extramural, supporting research "chiefly through an extensive program of grants-in-aid of investigations proposed and carried out by the members of the staff of Canadian universities and their affiliated hospitals and institutes."[9] Operating grants, won by investigators in a peer-reviewed process, were the primary form of funding available to faculty from federal sources. These grants have a short duration as well as specific regulations and allowable uses of grant funds, the basic structure of which has not changed much since the 1960s. In the last few years, a few academic scientists in the biomedical sciences have had access to new types of state funding, such as very large grants for projects and infrastructure associated with genomics research from other agencies at the provincial and federal level. The analysis in this thesis however deals with the relationship between social organization in academic labs and what has been for decades was the available form of federal funding for research in the biomedical sciences, the standard operating grant.

Increased competition in the federal granting system in the 1980s and its consequences

In the mid-eighties, the structure of research funding at the MRC changed, resulting in an increase in the competition for research grants.[10] According to the professors I interviewed, although it was difficult for a professor to get his or her first operating grant, in the years prior to the mid-eighties, the renewal rate for grants was around 80%. After this, the renewal rate reportedly dropped to from 80% to around 50%. The data for success rates in MRC grant competitions corroborates these findings, showing that the success rate for new grants was 30% in 1985 and the grant renewal rate was 80%, but dropped to 70% for the years 1986-9, then gradually to 50% by 1995, and it has remained around 50% until the present.[11] Instead of having their grant renewals relatively assured if they were publishing, there was now a significant chance that professors might lose their grant.

This change in the renewal rates was seen as a fundamental shift in funding policy, from a system that funded the research of academic scientists, to one that funded the best research. Prior to this, as a retired professor who began as a faculty member in the mid-sixties told me:

> ...in our minds, there was no such thing as rejection of the grant proposal ....Unless you were a dud, if you were not productive at all...at the time, I think that the thinking was as long as there were good people doing research ... we have to support them.[12]

As another retired professor, who was also hired in the sixties, explained:

> In the earlier time, the funding was that if you are productive, you would definitely get a grant....the philosophy of funding has changed, because you do not fund the research because its good, you fund the research because its better than someone else's. ...[if] there are a hundred applications ...[totalling] ten million dollars, and [there is] only five million dollars, they are going to decide only to fund 50 of the applicants and not all 100. So the competitiveness in getting grants is different....[13]

Losing their external grants had very serious consequences for professors. In the years where receiving external funding had been relatively assured if for faculty members in these leading universities, it had also become the norm that they had a technician in the lab, and perhaps a student or two, and that these lab members were paid from your grant.[14] Losing the grant meant losing the ability to continue to pay lab members and buy research materials. In addition, in the 1980s, research materials were becoming more expensive. A professor who set up a lab in 1979 describes the impact molecular biology had on the cost of biomedical research:

> And just running the lab [then] was inexpensive. I mean, you know, with the advent of molecular biology and all that, the costs of running labs, it's just unbelievable compared to what they were then, then, you know, you could run it on a shoestring. First grants might have been thirty odd thousand. Now [even] if you have a hundred plus, you can't really make it.[15]

When faculty members lost their grant funding, these universities usually did not or could not take over funding the research, including funding their students, on more than a short temporary basis. Since renewals were no longer relatively assured once you had a grant, the ability of biomedical professors to maintain their research programs and research groups was not a given either:

> P: And then things got really tight. ... people were not getting renewed...good people, you know, it was shocking.
>
> A: And if that was their only grant...
>
> P: That's it. I mean people were being terminated... I mean given a terminal grant. ...Okay, so you had to be hustling...sometimes you had to close down and start again. You know, if you were tenured. [16]

Losing one's grant also had serious implications for status as a faculty member. In the years where external funding to professors in these leading universities was reasonably assured, the findings suggest that their medical faculties had incorporated external funding into their institutional decision making. By the late seventies, having research funding from the MRC was reportedly being used as one of the main criteria for receiving tenure. An older professor explained what happened if a professor lost his or her grants before tenure:

> They won't get tenure.. ...So, basically you didn't get tenure. Why would you get it if you didn't have a grant? is the attitude that was....Basically, you need to have CIHR funding and/or salary support to get tenure in this faculty. ...And if you don't have it, you won't get it. ...And it has nothing to do with your research or administration, you need CIHR funding. [17]

If already tenured professors lost their grant funding, they might lose their students and staff. They were expected to find a new source of research funding, whether it be a new grant from the MRC, disease foundation or from industry:

> A: But if you were tenured, then what happened?
>
> P: Well if you were tenured, you would still have pressure to go and find money somewhere...an independent...I mean if you were tenured and if you were non-publishing, especially in one of these institutes where that's your main raison d'etre. You're not really fulfilling a teaching role. You're not...you know, you're here to do research. And so that was a very hard time....Now, you know, the comforting part of working in this institute, some other institutes is that there was internal money, so they would provide bridge funding.
>
> A: Between the grants.
>
> P: Yeah, to tide you over. I mean you have to apply, it wasn't just an automatic.... But in some cases when these things would happen, they would have to let go their staff, and students could have been in trouble too. [18]

The hospital research institutes, with their affiliation to hospitals and their foundations were in a better position to help investigators who had lost their grants maintain their research program and research groups. Increased competition in the MRC granting system also led investigators to look for money from industry, often from pharmaceutical companies. Prior to this, having 'drug money' was frowned upon by the academy. When the federal system became more competitive, having this extra source of revenue was seen much more positively.

However, if a professor did not find new research funding within a few years of losing his grants, he or she would possibly never get grants again. As one professor explained:

> ...Sometimes they can do collaborative projects, so sometimes they can participate in projects that are funded through someone else but they still can maintain some kind of research presence....Sometimes they go on sabbatical.... That doesn't usually help... It's kind of used to acquire new expertise but its very difficult. It's extremely hard to get back into the system. It's extremely hard to get new grants after you haven't had them for a few years. ...It's almost impossible...it's extremely competitive. So unless you have a wonderful expertise in something, and you have some kind of current research ... [whispering] there's no way. [19]

If you were a tenured professor, and you could not find another source of research funding, even though you would not lose your faculty position, you might lose your status as an investigator. If a professor could not re-establish an externally funded research program, the university or institute sometimes even withdrew his or her lab space. Without research funding, students and staff or lab space, a tenured professor in the biomedical sciences would maintain a faculty position, but not an identity as someone who directed research and graduate students.

Emergence of larger labs: how changes in the system of external funding led to differentiation in the size of labs

The findings also suggested that more competition in the granting system at the MRC was an important factor leading to greater differentiation in the size of biomedical research labs in Canada. According to older professors, the serious consequences associated with losing their existing grant led investigators to submit multiple new grant applications, in hopes that they would have at least some funding (the submission of multiple applications by investigators seems likely to have further increased the competitiveness of the process). Although data for the numbers of applications are not available before 1985, the Medical Research Council reported that "application pressure increased significantly in all MRC grant programs in 1984-5." [20] The data in later years do show that a dramatic drop in success rates and continuously low renewal rates are associated with increased numbers of new applications. Between 1986 and 1994, an average of 1048 new applications were submitted each year. After a sharp drop in success rates for both the new grant applications and renewal applications between 1993 and 1994 (from 65% to 31% for renewals, and 25% to 15% for new grants), the number of new applications jumped dramatically from 952 to 1681 between 1994 and 1995. Renewal rates have hovered around the 50% mark since 1995, and during this period the number of new applications submitted has risen sharply: 1681 new applications were submitted by investigators in 1995, and by 2005, this number had risen to 2837. [21]

The unintended consequence of applying for more grants to be sure that you would have one was that if you got more than one, you were then *obligated* to do more research than in the past, which involved hiring more people. The findings suggest that many small labs initially got bigger inadvertently, through actions taken to make sure that they survived at all. An older professor describes the process by which the uncertainty associated with greater competition for external funding in the 1980s led to growth in the size of some labs:

> P: .... at the time I applied ...the chances of getting renewals in general was 80% and probably your first renewal was somewhere in between 'cause it was more precarious, but, you still probably had a good chance. So, you didn't have to apply for multiple grants because you were pretty sure you were going to keep the one you already had.
>
> A: You can see that with some of the older professors. You can see they had a grant, from...
>
> P: Forever. ...Then, as it became more precarious, you were encouraged to apply for more different grants because if you didn't get it, you were sunk. ...And at one time you had to wait a year before you could re-apply.... So then you had no money for a year. So, the natural thing is to apply for different sources of money, different grants, so then you had have to do these projects..... If you get them you have to have more students, or more postdocs, or more... So it becomes a vicious circle. Because you are not sure about the first source, you apply for more, then you have to do more,

and then you need more people, and you then you have to support them, so you ultimately feel that you need to support all these people so you keep applying for grants....So that's why labs expand. [22]

Key changes in work of faculty members

The uncertainty associated with more competition in the granting system resulted in significant changes in the content of the work of investigators. First, since losing grants was now a significant possibility, and, had such serious consequences given the norms and institutional accommodations that had developed with extensive availability of external funding, there were many indications that preparing grant applications and grant renewals had become a priority for investigators:

> P:...you know, the early years, I mean it was great. You got your grant and you didn't think about it anymore. And you spent your time doing your research, writing papers, giving lectures, traveling, and whatever. Then you got this tough time where you had to think mostly about looking for money. And it was...I thought that...I mean that was the worst part. You had to really focus on money.
>
> ... That's your priority....You have to have the grants, and there are deadlines they have. Whereas, sending off papers there aren't really deadlines...nobody's standing over you....
>
> A: Right. You have to do it but ....Nobody's saying the 15th...
>
> P: You have no real deadline. Whereas the grant, it's coming up September

> 15, you don't make it you're out, you know, like that.... And those deadlines are always looming, looming. [23]

In a competitive system, more grant applications had to be written than would be funded. Older professors reported that in the 1960s and 1970s, grant renewal applications were much "more like progress reports," very short, and for most professors, only for one grant. After the system became competitive, professors spent much more time writing grants. In effect, professors who maintained externally funded research programs became scientific entrepreneur-managers (Slaughter and Leslie, 1997), since in a very competitive funding environment, obtaining the financial resources to do research was very important, very difficult and took a great deal of time.

> It's still...the major change I would say from early to now is the amount...the significant greater amount of time that an investigator has to spend looking for money.... Writing grants is generally time-consuming and you know, and then there's the down side. You write a grant, you spend a hell of a lot of time, you don't get it, then re-vamp it. But it's a big work. And instead of writing your papers... [24]

Increased competition in the granting system also resulted in greater differentiation in the work among faculty members. The work of tenured professors who had lost their funding might differ quite a bit from that of funded investigators. When they lost their grants, tenured professors were often expected to assume larger teaching and administration roles in their departments. However, particularly as the size of labs of faculty successful in the grant competitions became relatively large in some cases, widening differences in individual faculty workload may have created tensions within the departments. There were indications that the roles of those who lost their funding sometimes became awkward and undefined, and of frustration on the part of their colleagues who had externally funded research programs. When asked what a tenured professor who had lost his or her research funding did in that department, one active investigator gave this somewhat surprising response:

> P: In theory or in practice?
>
> A: Both.
>
> P: OK, in theory then, the hope is that those kind of people will pick up on teaching and administration and contribute in those areas more. So they may have a small research component but they would do much more teaching and administration. ...the people that lose the grants are usually the ones that aren't good teachers, and are pretty lousy administrators 'cause they didn't get things done.... So in practice they pretty much are useless.
>
> A: So what happens to them?
>
> P: Without any names, they're tenured professors.
>
> A: Yes, but what do they do?
>
> P: Not much. It's very hard to define...It's very, very frustrating (laughing) Because other people have many grants and many students and

> do a lot of teaching.....And they see these people, and they don't know what they are doing....If you can pick up something else. Then your still functional....and you're appreciated....It's the times when you don't have those other things. That .... A little frustrating. [25]

Internal grant review: institutional response to a more competitive granting system

The changes in the granting system at the MRC were also found to have led to changes in some cases at the institutional level resulting in additional grant-related work for investigators. Losing funding could result in an investigator having to dissolve his or her research group letting experienced staff and students go, which was very disruptive to research and training. Even if he or she did win new grants, loss of trained staff and students experienced in the particular research and technical environment of that lab was a major setback. Because research institutes in hospitals often had access to some funds associated with the hospital or institute itself, they were often better able than university departments to provide temporary internal funding to these faculty members while they looked for new external funding. However, the expenses associated with this were considerable, and some research institutes had begun a process of internal grant review to try to make sure that grants submitted to the MRC from that institute were as competitive as possible. This led to even more grant-related work for investigators, since they had to participate in a formal review process reviewing the grant proposals of others. Groups of investigators were formally organized to improve the quality of the grants submitted to the MRC. An investigator at a research institute where this internal review process was instituted about ten years before explains how participation in this process was necessary if you going to be able to get temporary research funding from the institute:

> P: ... we have a process here, now more institutions are having this process whereby these grants are being reviewed even before they go out to MRC or CIHR.... It's mandatory here in this institute and I have a feeling it's now becoming mandatory... in most institutes. It's a good thing. The only problem with that is you have to be ready...Before your deadline. And that's the big hassle...I hated it when it first started but in the final analysis it's a good thing because you got three different reviewers from around here reading it... But that's been on now for quite a few years.... Ten years maybe.... you get three people, three reviewers, hopefully that have some expertise in the area that you are talking about, we even get reviewers from outside... it's a meeting, the grant is sent around to three people, a meeting is pending. You sit for two hours with the investigator and...
>
> A: Give comments or...
>
> P: Tell them what's wrong and then they have to revise it... And then you give the report to the institute. One of the reasons they decided to do this was that, because then, if it doesn't make it ..let's say you're a tenured professor...if it doesn't make it on the outside, you have this operation [lab] that you have to let everybody go. You know...it's a disaster...then you have to lose all trained people. How do you begin to build your lab again? So, the institute didn't like to have to have that, so they would give bridge

> funding. But then the bridge funding thing was getting out of hand. You go losing your money and the institute, it's costing a fortune to keep all these failing labs going. So they decided to streamline the process..., if you don't go through this internal grant review you will not be eligible for any bridge funding. [26]

Changes in the social organization of work in biomedical labs with more competitive grants

Given the relatively assured funding of faculty research in these leading universities in the seventies, the findings suggest that by the late seventies, the lab had become an organization, typically with a small research group (students and technicians) paid from one grant, with a distinct division of labour. The investigators did not normally do experimental work themselves at the bench. Although they trained students and directed the work of technicians, students and technicians did the experimental work, as one professor told me: "...in general, the [older] professors you meet have not done an experiment with their own hands for decades." [27] Currently, students and postdocs currently are often directly involved in doing thesis projects that are part of the professors grant funded projects. Unlike in the social sciences, where PhD students usually do courses and comprehensive exams before developing their own projects and beginning research, students in biomedical sciences begin full time work in the lab when they begin graduate studies, taking time out to attend courses and seminars. They typically have their initial projects assigned to them, rather designing the projects themselves. [28] From their research, the PhD student is expected to obtain results from which papers can be published in order to graduate. [29]

The papers are usually written by both the student and the professor, and both receive authorship credits. A paper for the student, therefore, is also a paper for the investigator. In other words, the students in the biomedical sciences are not just working towards the goal of graduating themselves as in the humanities and social sciences, but are contributing to the output of an organization. The work needed to achieve the main goal of the PhD student, to graduate, was integrated with the necessity that investigators publish papers and maintain their grant funding.

With increased competition for funding, given the insistence of their institutions that investigators have funding, the lab became an organization that could disappear. In order to be able to maintain a lab in this environment, an investigator had to compete successfully for grants on an ongoing basis. Since the main criteria in the competition for renewal of standard grants was productivity, investigators needed to optimize output, in terms of published papers, from grants.[30] In this very competitive situation, an acceptable level of publishing could no longer be defined:

> ... it's like being a small business man, you've got to be able to manage, and how can you optimize the output, which is going to determine whether you get your next round of grants. [31]

Principal investigators responded to the need for increased productivity by changing the social organization of work in their labs, especially in terms of division of labour and recruitment. Since the professor was now busier competing for resources, but simultaneously needed to increase the productivity of his or her grants, he or she became more dependent on the work of the lab members than before. In fact, the success and/or survival of the lab as an organization and the professor as an investigator became dependent on the lab members work.

The importance of the structure of standard grants and the allowable uses of grant funds

The way the investigators organized to be more productive after competition for the federal grants increased in the 1980s brings to light something that may have only been implicit in the earlier years. The form of social organization of work in the lab, both before and after the change in competitiveness, was shaped by the regulations for the allowable uses of funds from the operating grant.

While the competition for MRC grants increased in the 1980s, the structure of the standard operating grant remained very similar. The grant funds from the MRC could be used for research materials, small equipment and research assistants. The annual report of the MRC for 1984-5 outlines specifically what grant funds could be used for:

> Operating grants support research activities by individuals working alone or in collaboration with others. These grants can be used to employ assistants or trainees, to purchase materials, supplies, and items of equipment costing less than $10,000, to buy and maintain laboratory

> animals, and support limited travel costs. (p. 14).

However, as the 1979-80 annual report suggests, in the earlier years, research assistants on grants almost always included technicians, and graduate students or postdocs only if the project was appropriate:

> Many investigators responsible for the direction of approved projects may also have significant responsibilities for teaching or the care of patients. They must therefore have the help of technicians ...MRC grants provide for their salaries. If the project is seen to provide a good environment for research training, funds may also be provided for the salary of a graduate student or more advanced trainee. [32]

After the competition for federal grants increased in the 1980s, principal investigators began to favour lab workers with trainee status (graduate students and postdocs) over technicians, for several reasons. The student, in order to reach his or her own goal of graduating, is often highly motivated. In contrast, motivation has been a problem with technicians in science (Barley and Orr, 1997). Technicians did research too, often under the direction of the investigator, but the technician position in the academic lab was often more a support to the professor and the other lab members. As a professor explained, the student is committed to the success of the research in a way that the technician is not:

> A tech does, is a service job. Often a tech doesn't have a vested interest...in that research.... in a sense their survival depends on it, because if you don't get your funding, the tech doesn't get the position....but that doesn't really weigh a lot, in the sense that, "if I don't work for you, I'll work for someone else"....A graduate student has a life-death vested interest, I'm being sarcastic....overstating it, but , they have a kind of commitment that you can get from no other employee.[33]

The difference that student status can make to motivation is clearly illustrated in the comments of a student who had decided to finish with her Masters degree in a lab, instead of re-classifying to a PhD program, because she found being a student was associated with too much pressure. She was convinced to stay on as a technician in the same lab because she had skills related to a new technique needed on a then current project. She explained to me that she was willing to work in the lab as a technician since it would be the same work without the pressure to get results. "If it doesn't work, it doesn't work, I don't have to worry about it." [34]

Not only did the student have more of a vested interest in the research than the technician, but the norm was that students also prepare the initial drafts of papers based on their experimental work, while technicians did not. Due to the intensified work for investigators resulting from the increased competition for funding, this difference in the work performed by students and technicians became important. Several older investigators explained to me that they found they had a lot less time for writing papers: "...you're writing [grants] all the time, you're writing a lot. And I find that what happens is you get behind in writing your papers which is what I love to do best."[35] Another investigator outlined how the work of technicians resulted in slower production of papers than that of students:

> P: Well, I wrote papers when it was the technician who had who had done the research ...But that's difficult, those papers took a long time to write. I'm not very ...you know, you have too many other emergencies, there's too much teaching, too much administration, too much this. So...those tended to sit around for awhile longer than would have been optimal.

But preference for the student in this new funding environment was not just related to their greater commitment to and involvement in the research process. Students were also cheaper. Students and technicians both typically worked in the lab full-time, all year round, and both students and technicians were paid from the investigators grants. Granting agency stipulations specified however that students were paid a stipend that was about half of the salary amount paid to a technician. In addition, a student or post doc could often get a studentship or fellowship which meant that the investigator did not have to pay that trainee from his or her grant. The technician became more of a luxury under these conditions:

> Well, and I'm looking to have more productivity from my grant... And it depends on the tech, but if the tech is doing routine lab [work], which you know, that's nice, it keeps the student free of those jobs, doesn't

> have to worry about stocking medias, running down and getting things, maintaining cell lines, and somebody has cells for you…
>
> ...we used to say, and that's something that's changed, that originally, in our institute, that a tech was a, what was the phraseology, was an honoured position, a respected position, they would be paid fairly, and they would get respect.... And that's of course where the techs disappeared, the honoured position, was that as the money got tight, you couldn't afford a tech. I can get two graduate students for the price of a tech.... And that was said, that was said. [36]

Many technicians were reportedly released from labs in the years following introduction of a more competitive granting system, and that when labs expanded in the 1980s and 1990s, they grew primarily through the recruitment of graduate students or postdocs, not technicians. [37] Trainees took over much of the service work formerly done by technicians.

The data suggest that these cost vs. productivity considerations affected the recruitment of all types of lab members. A professor with a small lab that had not been able to attract well-trained postdocs expressed this same cost vs. productivity concern about postdocs compared to graduate students, emphasizing the dependence of the investigator on the lab members in the process of production of research output from the lab. That the current division of labour in the lab involves the postdocs (or research associates [38]) planning experiments and drafting papers as well as carrying out the experiments is evident in the frustration of this investigator with the violation of this expectation:

> P: We've had postdocs ....but the ones we've had haven't been as good as our graduate students....They've come without the same basic training. Even the research associates ...they just don't cut it....So, then why waste the time? You're in fact putting yourself behind. So its better to train somebody, and get them, and have them be good, than to have a postdoc that's costing you more, that in fact has a shorter time to produce something, because a post doc doesn't last as long as graduate studies, and that doesn't produce. Because we train our graduate students not just to do, but to think and some of these guys come in and they can do but they can't think. And that's pretty crucial for us....if you want a lot of data, if you give them the experiments and get them to do it, they will do it.
>
> A: Right, and they'll produce a paper.
>
> P: And they'll produce ...and they'll produce stuff. But you'll have to write the paper for them.
>
> A: They will write something.
>
> P: They will write something, but it won't be publishable.
>
> A: And that's where the part comes in that's very costly for you...as the person that has to do it...
>
> P: Because I have to do the work (laughing) I've never thrown back a paper at a student, and said 'Hey, you haven't even got the basic

> components, what are you asking me to do? Why can't you do it?'. ...'
> Take the journal, take the format, put it into paper format.' I've never done
> that to a student, but I have done it to research associates or postdocs. [39]

With the growth of labs and the necessity that the professor spend more time writing grants, the structure of training changed in the larger labs as the investigator did less direct supervision of students. The senior PhD students or postdocs in large labs were often informally given the responsibility for the technical training and daily supervision of the newer students in the lab. An older professor who lost his grant in the early 1990s explains:

> In an ideal situation when you have a number of maturing students in the lab, the senior graduate students will then instruct the junior graduate students. The technician would look after them all and manage, provide supplies and do projects for me as well. When you get down to the smaller lab, more of the direct work devolves to me, I have to show them how to do things. [40]

But while basing your research program on graduate students could potentially result in higher productivity, basing your program on the labour of students, at least in the initial stages, might jeopardize or even end an investigator's program of research altogether. The same professor who lost his grant (above) in the early 1990s, got another grant, only to lose it too when he lost his group:

> I recruited three students in successive years....all three students decided to stop at the Masters level. Which is an enormous inhibition to the development of the program ...You end up spending most of the time training them, the pay back is usually in the PhD days when they become autonomous workers, so as a result I didn't have enough research productivity and publication to warrant continuation of the grant. [41]

Summary and Discussion

By the 1960s, a system of extensive external funding from the federal government for the research of biomedical faculty in Canadian universities existed. One federal agency has historically provided research funding to the staff of universities and their associated hospitals and institutes, primarily through one type of grant, the operating grant, which has specific regulations and allowable expenses. Biomedical scientists therefore did not have pluralism in federal funding. [42]

The findings suggest that by the late 1970s, tenured faculty usually had one grant from the main source of external funding in this area, the Medical Research Council. Although these grants were initially won on a competitive basis, they were renewed on a much less competitive basis. Once a professor in these universities had a grant, he or she was reasonably assured of continued funding of his research through grant renewals, as long as he or she was publishing at acceptable rate of one or two papers a year.

Under these conditions, the academic lab had typically become an organization, the investigator with a small group of lab members paid from the grant, including the students. A division of labour existed where the professor usually relied on lab members to do the experimental and technical work in the lab.

Changes in the structure of funding from the MRC in the 1980s resulted in a shift to a fully competitive granting system, which led to significant changes in the social organization of work in biomedical labs at these universities. The findings further suggest that the influence of the changes in external funding on the social organization of work was due to the dependence of these investigators on funding from this source. By the late 1970s, funding for research was being provided primarily through operating grants from the MRC to individual faculty members. Other than external operating grants, there were typically no regular funds available to professors from the university to fund their research. In the 1980s, more competition was introduced into the federal granting system with a reduced renewal rate for existing grants. Professors began to lose their funding, and the university did not step in to take over funding of these labs on more than a temporary basis. Since in a fully competitive system continued funding was not assured, these changes in the granting system meant the survival of a lab as an organization was threatened.

Institutional accommodation of these funds had a second aspect. By the late seventies, the findings suggest that medical faculties in these universities had also incorporated the extensive availability of the external funding for research into their institutional decision making for granting tenure. In effect, the findings suggest that tenure decisions were largely based on the funding decisions of the peer review committees of the main federal granting agency. Further, since these universities did not provide more than some temporary funding to a professor who lost his external funding, peer review committees also in effect had some influence through their granting decisions about which tenured faculty would be able to continue to direct research and train graduate students. If faculty members could not re-establish external funding, the university might also withdraw their lab space. In other words, it was now possible that a tenured professor could lose his status as an investigator through the decisions of the peer review committees of the granting agencies. In this fully competitive granting environment, a tenured professor had job security, but no security in his status as an investigator, someone who directed research and trained graduate students.

The data indicate that increase in the competition for federal grants, beginning in the 1980s, on which most of professors were dependent, resulted in significant changes to the social organization of work. First, the content of the work of funded faculty members changed significantly. Obtaining grants necessarily became a priority, and took much more time, particularly since investigators began to make multiple applications in order to have more chances to receive funding. The division of labour existing between the investigator and lab members was reinforced, because the investigator, busier with grant writing, relied much more heavily on lab members to do the experimental work and assist in writing papers. Second, as faculty sought to maintain their status as investigators and the survival of their labs through continued funding in a more competitive system, they needed to maximise productivity from their grants in terms of papers. In response, they changed their recruitment patterns and the division of labour in their labs.

The specific changes investigators made were shaped mainly by cost vs. productivity considerations, made within the constraints and opportunities associated with the regulations for the allowable uses of standard grant funds. Federal grants specified (then as now) the types of lab worker that could be hired, as well as the cost of types of allowable worker. They allowed investigators to hire only graduate students, postdocs and technicians, but stipulated that graduate students be paid annual stipends which were approximately half the rate of technicians salaries and postdocs annual stipends. Given that the goals of graduate students and postdocs meant they had a vested interest in the research, were more motivated, drafted papers in addition to doing experimental work, and could potentially get scholarships from various agencies to cover their stipends, investigators now favoured recruitment of trainees (graduate students and postdocs) over technicians. [43]

Increased competition in the granting system also seems to have resulted in greater differentiation in the size of labs, since some investigators were more successful than others in winning multiple grants, and lab size grew as they hired more lab members to do the research. The existence of larger, often more productive labs also made building a larger lab a necessary goal in order to better ensure continued ability to compete for grants successfully. When labs funded by standard grants expanded, they typically grew through the recruitment of more students or postdocs, not technicians. Although the exploratory interviews in this study did not address this issue, an interesting topic for future study will be to investigate the relationship between the developments associated with the advent of molecular biology in the 1980s (standardized technologies, such as pre-made reagents and probes, protocols, automation, etc. [44]), and the development of larger academic labs in this same period.

Differences between the social organization of labs in these leading universities and that of labs in other countries illustrate the importance of the interplay between the structure of research funding and its institutional accommodation for the social organization of work in research labs, and further emphasizes the importance of the structure of standard operating grants and their regulations in influencing the social organization of work in biomedical labs in leading universities in Canada.

Unlike most biomedical research labs in these universities, Mangematin and Robin (2003) describe research laboratories in the life sciences in France as having both senior and junior members. Most junior member are PhD students, who make up approximately 30% of the skilled labor in these labs. Similarly, Shinn (1988) described the CNRS physics lab in France he studied in the early eighties as having senior and junior scientists (which included students), as well as a director. An explanation of this variation in social organization of labs can be found in how the configuration of the French research and training system (Clark 1995) differs from that in Canada in terms of resource dependence. In Canada, government funded research in the biomedical sciences has been assigned to investigators in universities and their affiliated hospitals and institutes, and is supported primarily by standard grants which allow the investigator to pay only graduate students, postdocs and technicians. These grant funds cannot be used to pay senior scientists (those with PhDs or postdoctoral experience who are not trainees). In France, in contrast, the central place in government-funded research is not assigned to universities as it is in Canada in the biomedical sciences, but to a massive government research sector.[45] Laboratories are situated on universities but are not under university control. Research training in science in France has largely depended on relations between these government laboratories and universities: to gain access to training, students must compete for national grants controlled by individual professor- researchers and full time

CNRS researchers in these government labs (Clark 1995, 9).[46] Most researchers and faculty members in France are civil servants, and research laboratories are only allowed to hire people on grant money for less than one year, with the exception of PhD students who can be in the lab for three years (Mangematin and Robin 2003). The research laboratory in France therefore is a government laboratory with full time permanent senior researchers (and other permanent employees) as well as doctoral students on national grants.

This difference points out an aspect of academic biomedical labs in Canada which was largely implicit in the findings. In Canada, most biomedical labs are funded from short term operating grants which can be used to pay only trainees and technicians, and therefore have only temporary employees. The only permanent employee in the lab is the investigator.[47] Since Shinn (1988) and Mangematin and Robin (2003) have both argued that junior/senior, temporary/permanent researchers of labs make different contributions to research, it will important for future research to investigate how this aspect of social organization has affected research in Canada.

In the US, as in Canada, the largest source of funding for academic scientists in the biomedical sciences is the federal granting agency, the National Institutes of Health (NIH) , which similarly provides funding to investigators primarily in the form of competitive research project grant, known as an R01 (Stephan and Levin , 2002: 419-20). Hackett (1987, 1990) found evidence, similar to that found in this study, that academic biomedical scientists who lost their external funding in the 1980s might be denied tenure, or if tenured, lose their lab groups, suggesting that there have also been similar institutional accommodations of external funding in the US by the 1980s, at least at some universities. The presence in US universities of a phenomenon not apparent in Canadian universities, a considerable number of investigators in the biomedical sciences without tenure track positions, whose salaries are in large part paid from their research grants ('soft money positions,' Stephan and Levin, 2002: 427; also see Hackett, 1987, 1990), points to the importance of the specific structure of federal standard grants in influencing social organization of research. In Canada, recipients of federal grants in the biomedical sciences must hold salaried academic positions in their institutions, and grant funds cannot be used to pay salaries of the investigators who hold them in part or in full. The existence of non-tenure track investigators in the biomedical sciences in US universities whose positions (as well as their labs) are dependent on external funding shows how differences in eligibility of applicants and allowable use of funds can create variation in social organization of academic research in this field, even where investigators are similarly dependent on short term competitive grants.

Conclusion

The findings reported in this chapter suggest that considerable changes in the social organization of biomedical labs in leading universities in Canada have taken place since the 1970s, and suggest key processes and mechanisms by which those changes have occurred, which in turn suggest the processes of institutional influence.

Evidence found in this study suggests that there have been significant changes in the social organization of work in academic labs in the biomedical sciences in leading Canadian research universities since the 1970s, and that these changes, including the emergence of larger labs, were primarily due to effects resulting from modifications in the system of research funding, after institutional accommodations of extensive external funding made biomedical faculty dependent on it. The key change was an increase in competition for operating grants from the federal agency in the 1980s; a shift from a situation where grant renewals were reasonably assured after obtaining an initial grant competitively, to a situation where grant renewals were also very competitive. The influence of increased competition for external funding on the social organization of work in the biomedical labs in these leading universities was due in large part to the dependence of most faculty in the biomedical sciences on external funding from one source, and the serious consequences for faculty in these universities associated with losing external funding.

An analysis of the socio-historical dynamics, as opposed to a static analysis, begins to outline the long term effects of federal research funding on the social organization of work in the biomedical sciences in these leading universities, as well as the some of the particularities of the organization of Canadian research and training system in the biomedical sciences. The findings suggest that with an extensive system of federal funding for the biomedical sciences, by the late 1970s, professors in these leading universities had become dependent on external funding in order to do research and maintain their lab groups, get tenure, and maintain their identities as research scientists, due to its internal accommodation by their medical faculties. The particular structure of available funding had led to the academic lab becoming an organization with a few members led by a funded investigator. When this funding became more competitive, this dependence resulted in the possibility that academic labs could disappear.

Continuing research, however, is central to the identity of a research scientist (Sismondo, 2004). With the existence of their labs, tenure, and the maintenance of an identity as a research scientist made dependent on external funding, the career of biomedical faculty members in these universities had a level of resource dependency that drove structural change. [48] When federal grants became more competitive, the findings suggest that biomedical scientists responded with two new practices, in order to have a better chance to maintain external funding on which their careers now depended. These were 1) applying for multiple grants, and 2) trying to maximize the productivity of those grants by rearranging the social organization of work in their labs within the regulations and allowable expenses of those grants, by shifting to hiring graduate students and postdocs instead of technicians. These conditions seem to have been responsible, therefore, for creating a system of biomedical research labs largely dependent on the temporary labour of trainees, and for creating larger labs.

The comparison of the social organization of biomedical labs in these universities in Canada with those in other countries is another reminder that the social organization of research in any given field is not ahistorical or universal (cf. Traweek 1988). The analysis also historicizes and specifies the cycle of credibility argument. Latour and Woolgar (1979: 200-201) presented a formal model of the credibility cycle, where scientists publish papers, making them more credible so that they will get grants, the grants allow them to buy equipment and pay lab members, which in turn allow them to publish more papers, to become more credible, and so on. Evidence from these leading universities in Canada actually suggests that a granting system must be fully competitive and scientists allowed to hold more than one grant at a time in order for this system to produce differential accumulation of credibility between scientists. Although they presented this cycle as a general feature of science, findings here suggest that it describes the situation in a particular period in U.S. science.

The findings of this chapter showed that biomedical scientists responded to an increase in competition for federal grants beginning in the 1980s with two new practices; applying for multiple grants in order to have a better chance to stay funded, and recruiting trainees instead of technicians in order to try to maximize productivity. The next two chapters examine how these now institutionalized practices of investigators affect the current organization of research and training in the biomedical sciences in these leading universities.

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[1] The ability of ethnography to show process in rich detail and mechanisms which link independent and dependent variables is a recognized strength of the method (Fitzgerald, 2006:12)

[2] However, as an exploratory analysis, the findings are suggestive, not conclusive, and it has several important limitations. The small number of interviews with the older and/retired scientists does not allow distinctions to be made on the basis of particular biomedical discipline, or the characteristics of the investigator (gender, ethnicity, etc). Second, in order to locate older faculty that had been doing research in these universities since the 1960s, several referrals were obtained from older professors in the biomedical sciences that I knew. This proved to be important for access (all but two referrals contacted agreed to be interviewed), but is a potential source of bias. Finally, since the faculty interviewed had been doing research in these universities since the 1960s or early 1970s, they were all "survivors," in the sense that they either still had research labs or had stayed in the research system until their recent retirement, therefore the perspective of those who left faculty positions earlier is not represented.

[3] Medical Research Council (1966a). In 1965-6, 330 of 438 investigators in the basic sciences had four or fewer members in their labs (Table 25). The basic sciences were defined as departments of anatomy, biochemistry, physiology, pharmacology, biophysics, genetics, and medical research (p. 33).

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[4] Lists of operating grants published in the annual reports of the MRC in the 1960s and 1970s confirm that most professors with a grant from the federal granting agency had only one.

[5] Interview with older professor, July 17, 2002, by author. In addition to a grant from the federal granting agency, he also had a grant from the Arthritis Society in the U.S.

[6] Interview with an older professor, September 19, 2002, by author.

[7] Interview with an older professor, September 17, 2002, by author.

[8] In 2000, this agency was absorbed into the newly created Canadian Institutes of Health Research (CIHR). Prior to 1960, medical research was funded by a division of the National Research Council (NRC). Funding and coordination of medical research did not become an official government responsibility in Canada until 1938. During its first twenty years, the NRC, created in 1916 to advise the government and promote industrial research, did not have responsibility for medical research, although it did support some specific projects, such as the study of tuberculosis (Li, 2003: 149). For more details on the history of private and public funding for biomedical research in Canada, see Li (2003).

[9] In the 1960s, research funding from the MRC was structured as follows: "Operating grants, which may be awarded on an annual or a term basis, are designed to assist in defraying the normal operating costs of research; major equipment grants provide for the purchase of units of special research equipment costing $5000 or more; travel grants may on occasion be awarded to investigators in the medical sciences to enable them to visit other laboratories for purposes of furthering their own research programs and, under special circumstances, to attend scientific conferences. The Council also provides a general research grant to the Dean of each of the fourteen Canadian medical schools to be used at his discretion for the development of Medical Research in his university." The MRC also provided personnel support for research through competitive fellowships to postdocs and competitive salary awards to investigators, and beginning in 1968-9, to graduate students (Medical Research Council, 1966b:2)

[10] According to the Medical Research Council (1984: 7), the federal budget of March 1983 did not provide sufficient base funding to cover the funding requirements of MRC programs. According to the MRC, government policy to fight inflation (the 6 and 5 program) allowed for an increase of only 6% to compensate for inflation, but the actual cost of conducting research increased 11.7%. The council " was forced to reduce the funding available for all programs." The report gives data only on the new grants however: only 92 were allotted in March 1983, compared to 265 in 1982, resulting in a funding rate in 1983 of only 12.6% of new grant applications being judged worthy of support. The Council received supplementary funding of $14 million in June 1983, and used this to restore the levels of funding for new grant applications in 1983 to " a more

appropriate" 40% of those which were judged by peer review to be worthy of support, and to address a large backlog of requests for major equipment purchases. In addition, at least one of the provincial agencies decreased their funding to research around the same time.

[11] Data for the success rates in MRC/CIHR grant competitions was obtained by special request from CIHR. The first year that data are available from the CIHR database is 1985. While the funding rate data seem to support the findings, a full fledged analysis of trends in funding rates and numbers of applications (see the next section) would necessarily become a separate paper, since further research would be required to find out if there have been changes in criteria and definitions over the years (e.g in the definition of what constitutes a new application or a renewal) and the data interpreted carefully.

[12] Interview with a retired professor, August 15, 2002, by author.

[13] Interview with a retired professor on September 18, 2002, by author.

[14] While most students probably would not go to a lab without receiving funding from the professor, in recent years many biomedical departments in these universities require that professors be able to fund a student with annual stipends at specified minimum levels before they can accept the student into the lab.

[15] Interview with older professor on September 19, 2002, by author.

[16] Interview with older professor, September 19, 2002, by author.

[17] Interview with older professor, September 17, 2002, by author.

[18] Interview with older professor, September 19, 2002, by author.

[19] Interview with older professor, September 17, 2002, by author.

[20] Medical Research Council of Canada. (1985: 7).

[21] Data for the numbers of new and renewal applications in MRC/CIHR grant competitions was obtained by special request from CIHR. The first year that data are available from the CIHR database is 1985.

[22] Interview with older professor, September 17, 2002, by author.

[23] Interview with older professor, September 19, 2002, by author.

[24] Interview with older professor, September 19, 2002, by author.

[25] Interview with older professor, September 17, 2002, by author.

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[26] Interview with older professor, September 19, 2002, by author.

[27] Interview with retired professor, September 18, 2002, by author.

[28] This is expected now, students are often very confused when they are not given a project when they enter the lab.

[29] Current PhD students told me that they thought having two or three first authored papers would be enough to graduate. This is a norm, not an official written requirement.

[30] In the 1960s and 1970s, not only were one or two papers a year acceptable, but also as a retired professor explained to me, "a paper was a paper." (Retired professor, Nov. 3, 2003) Another explained, " ...at one time, just simple numbers of papers would be criteria for funding." (Retired professor, August 15, 2002). As time passed, papers in high impact journals began to count more.

[31] Interview with retired professor, November 3, 2003, by author.

[32] Medical Research Council of Canada (1980).

[33] Interview with retired professor, November 3, 2003, by author.

[34] Interview with a Master's student, 2002.

[35] Interview with older professor, September 19, 2002, by author.

[36] Interview with retired professor, November 3, 2003, by author.

[37] Historically, and typically today in these universities, because most labs are funded by operating grants to the investigator, the labs are comprised of graduate students, postdocs and technicians since this is the type of labour that the grant can be used to pay. In the large labs I studied, however, there were some senior scientists. This was related however to the fact that these labs, exceptionally, had some funding other than standard operating grants from Canadian sources. In the large lab I studied funded solely from standard grants, for instance, there were two senior scientists, a visiting professor from France, and a senior scientist paid from a U.S. grant (the National Institutes of Health grant allowed the investigator to pay a research associate (post PhD, non-trainee scientist). In the other two large labs I studied, new forms of grants from the state, available to a small number of scientists doing genomics research, allowed the investigators to pay associate scientists, research associates, and lab managers.

[38] Post-PhD, non-trainee member of a biomedical lab.

[39] Interview with older professor, September 17, 2002, by author.

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[40] Interview with older professor, October 3, 2002, by author.

[41] Interview with older professor, October 3, 2002, by author.

[42] Gillmor (1986: 13) reported, for example, that one theme of the interviews he did with ionospheric scientists in US universities was "pluralism of funding." These physical scientists had a wide range of experiences with many agencies, including several federal agencies, which allowed them to characterize the funding "styles" of the various federal agencies involved. Among these agencies, only the National Science Foundation (NSF) funding , which was in the form of competitive, peer reviewed short term grants, seems to be similar to that available from the Medical Research Council of Canada after its funding became more competitive.

[43] It is beyond the scope of this study to investigate the more policy-related question of whether these changes did increase productivity in the biomedical sciences. However, overall scientific productivity (as measured by total number of publications) did increase dramatically in Canada in the period after 1980. In 1980, the number of publications from the university sector (i.e. containing at least one university address) was 11, 838 and by 1996 this number had increased to 21, 330 (Godin and Gingras, 1999).

[44] See Fujimura (1988) ; Keating, Limoges and Cambrosio (1999); Jordan and Lynch (1998), for example.

[45] In France , the government funded research sector includes the national laboratories such as CNRS (Centre National de la Recherche Scientifique), INRA (Institut National de la Recherche Agronomique), INSERM (Institut National de la Sante de la Recherche Medicale) (Mangematin and Robin 2003: 408)

[46] My interviews with postdocs who had done their PhDs in France and discussions with a visiting French professor suggested that doctoral students in France are not paid from research grants but instead from separate grants for the support of students controlled by the lab director.

[47] Stephan and Levin (2002: 427) note that this is a characteristic of biomedical labs in US universities.

[48] Use of the resource dependency framework for academic science was discussed by Hackett (2001). Hackett (1990) has argued that recent change in the organizational culture of academic science is partially resource dependent. I am arguing, on the basis of the findings in this paper, that the academic lab in the biomedical sciences in these leading universities in Canada, as a unit, is primarily dependent on a specific resource, external funding.

Chapter 5

Dependence of biomedical scientists on competitive grant support and the incorporation of graduate students and postdocs into the production of research

The last chapter showed that biomedical scientists responded to an increase in competition for federal grants in the 1980s, given the institutional accommodation of external funding by their universities, which made their careers dependent on it, with two new practices; applying for multiple grants in order to have a better chance to stay funded, and recruiting primarily trainees instead of technicians in order to try to maximize productivity. Most professors in the biomedical sciences in Canada are still dependent on competitive federal operating grants, and these practices have become institutionalized. This chapter examines the current organization of research and training in the biomedical sciences now that most investigators are dependent on graduate students and postdocs.

The hallmark of academic science since its beginnings in the 19^{th} century is the integration of basic research and the training of scientists (Clark 1993). Through the practices of professors and the experiences of trainees, the findings show that a particular pattern of involvement of students and postdocs in research currently exists, where graduate students' and postdocs' projects have become part of the production of the professor's research and publication, such that his or her scientific productivity becomes dependent on them. This pattern differs from that found in the humanities and social sciences in these universities.

The analysis shows how this pattern is related to external influences, primarily those associated with competitive federal grant funding and its institutional accommodation by these universities. Under the imperatives associated with competitive grant support, the dependence of investigators on trainees is associated with their incorporation into the production of research using several institutionalized practices. The data also suggest that these practices differ from standard practices in the 1960s and 1970s, and that these practices have changed the nature of both the organization of research and of training.

The main argument is that the investigators' strategy of obtaining multiple grants and their dependence on trainees has resulted in delegation of the experimental work on projects to trainees, and that this practice is coupled with a change in the use of the reward system in science. Not only is the trainee, unlike the technician of the past, given scientific credit in published papers, but his or her goals as a trainee have been aligned with those of the investigator through the informal but institutionalized integration of scientific credit into the structure of training. Other practices associated with the dependence of investigators on the work of trainees are also described and their implications for trainees discussed.

Finally, I argue that recent changes in cutting edge research as well as in the criteria for competition for standard grants may be resulting in other new practices as investigators make adjustments to try to get research done within the now institutionalized delegation-credit-evaluation arrangement with trainees.

A particular pattern of involvement of trainees in research

Unlike in the social sciences, where graduate students usually do courses and/or comprehensive exams before developing their own projects and beginning research, graduate students in biomedical sciences in these leading universities begin full time work in their supervisor's lab when they begin graduate studies, taking time out to attend courses and seminars. Graduate students and postdocs also typically have their initial projects assigned to them, rather designing the projects themselves. From their research, they are expected to obtain results from which papers can be published in the scientific literature. These papers are usually written by the student or postdoc and the professor, and both receive authorship credits. Although it wasn't a written requirement, the expectation is that PhD students in the biomedical sciences will publish at least two papers in order to graduate. The lengths of their degrees vary, but are typically 5-8 years long. Those who want to obtain positions as assistant professors expect to have to obtain postdoctoral experience in the lab of another investigator, typically three to five years, and to have to have a good record of publication during this period in order to be considered for a position as an academic scientist.

The current pattern differs not only from that typical in the social sciences in these universities, but there were suggestions in the data that many aspects of this pattern were not standard in the 1960s and 1970s in the biomedical sciences in these universities.

In next section, I outline in more detail the particular pattern of involvement of graduate students and postdocs in research found in these leading universities, and how it relates to the dependence of investigators on competitive federal funding.

I. Incorporation of graduate students and postdocs into the production of research and publication: Three institutionalized practices

A. Centrality of the project in the organization of research and why students and postdocs do not typically design their own

A prospective graduate student approaching a professor about doing graduate studies in his or her lab will usually find he or she is offered a project, or a choice of two or three projects. A postdoc describes how he became a graduate student in the lab where he did his Masters degree. A professor in the department where he had just finished his B.Sc. told him that a new professor was looking for graduate students. He had not yet applied to graduate school, but went to talk to her about the possibility of doing a Masters degree in her lab:

> I: Yeah I had an interview, I told her what I want to do. I told her and she told me what she wants to do and I told her I'm very interested. And she needed somebody...because I was interested she offered me the job....She was studying Tay Sachs disease. Which was a human disease. And basically, which is what I wanted to do, I wanted to study a human disease. A hereditary one. And she [said] that we identified the mutation in the [X] gene and they just need somebody to characterize it...so I'll be the guy to do it.... Very defined project. [1]

Although he expected to be able to choose his own project, it quickly became evident that h e was being offered a place in the lab as a graduate student on the basis of his interest in doing a specific project that had already been designed.

Some investigators, especially those with large labs, offer the student a choice of projects, but in a small lab, this isn't always possible. As a recently retired professor who had had a small lab told me of his interviews with prospective students:

> … some people will say I have two or three projects, this one, this one and this one, we'll talk about each one of them, which one do you think you're interested in. I think I did less that, I just said, we more said, this is kind of what we're interested in, are you interested in it? [2]

In the past, however, as is typical currently in the social sciences in these universities, designing your own project was still often considered part of being a PhD student. One of the older profs I interviewed had been a technician for a year in her PhD lab before deciding to do a PhD in the early 1970s. She emphasizes that she initially developed her own project, and that this was a key difference between technicians and graduate students:

> P : …I worked as a technician for a year.
>
> A: … And then how did things progress from there?
>
> P: I decided I could do a PhD. Why should I work for somebody?
>
> (laughing)

So I did a PhD, in his lab actually.

A: ....Did you have a particular project when you started the degree? Or were you continuing ...

P: ...When I worked in his lab I did all kinds of different things, different projects, which ever projects he wanted done. When I started as a PhD student, I chose a project. And I worked on that for a few months, and things didn't work, so I shifted, but it, none of it was what I did as a technician. It was a different project.

A: How did you come to have that project?

P: Well, the first one we chose, based on what I thought was interesting, questions in the literature basically, and it wasn't anything his lab did, which was probably a mistake, in retrospect, but he let me choose it (laughing). Said OK, go ahead. And then, a few months later, we figured out that it wasn't going to go anywhere too quickly. So I shifted closer to a project that he was interested in... one aspect of it. And the way that it was constructed at the time didn't work very well either. So I ended up actually going to a [scientific] meeting and meeting people that were doing things that were better and, so we shifted the project together to something else. It was closer to what the lab was interested in the second time. [3]

She also emphasizes how her experience differs from the norm today, and how the difference was related to the fact the lab was not funded by standard grants:

P: We had a lot of independence, I guess is what I'm saying.

A: Yes, because many students now definitely are offered a choice

P: Yes.

A: from specific projects that are available.

P: The lab was funded by a drug company.

A: OK, in it's entirety?

P: ...yes, at the time, it got [standard] grants later. It was a grant from the drug company. So it was the [research unit of drug company X], ...they paid our salaries, they paid research, they paid a lot of things. They paid me. But I wasn't working on any of their drugs or any questions that were related to their - we were totally free. We didn't have to meet specific gains, or research projects funded by a granting agency.

A: OK.

P: So maybe there was more freedom.

A: I see. So the lab was funded, but not specific projects.

P: It was a global area I was working in, not a specific project. [4]

Her PhD lab was funded for six years by this drug company grant, a three year grant, which was renewed once. Unlike a grant from the federal granting agency, the funding was not for a specific project, nor did it not require any specified research output. She relates her freedom to choose her own project to this difference. She later became a professor in this same department in the late 1970s, and said that after competitive standard grants to professors became the primary source of funding, investigators felt pressure to get the projects done outlined in their grant. She also associated this type of support with the end of a rotation system in her department where new graduate students spent a few months in each of several labs before choosing a supervisor:

> P: ...the funding situation had changed, and ...everybody felt they had to do what was in the grant, and ...they had to pay the people that were doing it, and that was that. [5]

B. Dependence on Competitive Grants and the Delegation of Projects

The standard federal operating grant application in the biomedical sciences requires a scientist to propose a project, and submit a budget. In 2002-3, the grant typically covered a three year period,[6] and allowed the investigator to purchase materials, small equipment and pay research assistants. In the 1960s and 1970s, when professors typically had one federal grant, the grant provided funding for research assistants primarily to accommodate other non- research responsibilities that investigators have in university or hospital settings (such as teaching or caring for patients). Because, unlike in the social sciences, biomedical scientists' careers are now dependent on maintaining external funding in a competitive environment, investigators do not have a choice about whether to apply for more than one grant. Since grants are won for doing projects, investigators with multiple grants must have research assistants just to get the multiple projects done. That is, they do not have the option of carrying out the research on all the projects themselves.

Federal operating grants allow investigators to hire technicians, graduate students and postdocs as research assistants, but as outlined in the last chapter, once grant renewals became more competitive in the 1980s, the stipulation by the granting agency that graduate students were to be paid at a rate that was half of that paid to technicians was one of the factors that led investigators to shift to hiring primarily graduate students instead of technicians. The investigator can support graduate students and postdocs from his or her grant if their projects are related to the grant.

The investigator is required to specify how many graduate students, postdocs and technicians are going to be paid from a grant when he makes the application.[7] When an investigator gets a grant, therefore, he or she is typically actively looking for students or postdocs to do the projects on the grant. This is why the trainee typically does not design his or her own project, but is delegated one.

The findings show that the project is still central to both organization of research and training in the biomedical sciences. Dependence of investigators on competitive grant support, however, which necessitates applying for multiple grants, also necessitates delegating the experimental work on those projects to others. *Under competitive standard grant support, therefore, projects are now typically designed by faculty, and delegated to graduate students and postdocs.* In other words, although the trainee still does a project, he or she no longer designs it. This is the first aspect of the incorporation of graduate students and postdocs work into the production of research in the biomedical sciences.

C. Institutionalized extension of credit to graduate students and postdocs in published research

If a graduate student or postdoc produces results on his or her project that the investigator wants to publish, a paper will be written by the trainee, the investigator, or both, but in any case, both will get scientific credit in the form of authorship on the paper.

In the past, when technicians were doing the bench work on their projects, the technicians were not given authorship credit in the papers published. [8] Now that trainees work on sponsored research projects, however, an extension of credit to them in the form of co-authorships has been institutionalized.

There is a specific distribution system for scientific credit on papers published. On a typical research paper, there are at least two authors. The first author is usually the student or postdoc whose project the paper is based on, and who did most of the experimental work in the paper, and the last author is the investigator whose lab the first author is a member of. Middle authors are typically other students and postdocs in the lab who contributed technical assistance, reagents, or data. In the situation where there is a collaborating lab, the name of the student(s) or postdoc(s) in the collaborating lab who contributed technical assistance, reagents or data will go second (or third or fourth, etc) and the name of the investigator in the collaborating lab will typically appear second last.[9]

Credit, however, is not just extended to trainees *if* they produce publishable results. Both the graduate student and postdoc are now expected to publish papers from their results in order to achieve their own goals as trainees.

D. Scientific credit has been integrated into the structure of advance training

Just as student and postdoc work has been informally incorporated into production of research through delegation of grant projects and sharing of scientific credit in published work, scientific credit has informally been integrated into the structure of professional training. The student expects to graduate, and the postdoc to advance to a position as an independent scientist on the basis of a record of publication.

Although it wasn't an official requirement, PhD students in the biomedical sciences expect to have to publish at least two papers in order to graduate. A soon-to-be PhD graduate in one of large labs I studied explains:

> A: So how was it decided by you or someone else that you would finish the degree at the point that you did?
>
> S: You have to have publications. And that's why I'm getting .... (our professor) will never prevent you from leaving, but if you don't have enough for a thesis he's going to tell you.
>
> S: What's considered enough? Two, three papers?
>
> I: Oh, yeah, for a PhD thesis? Two papers is good enough. [10]

As an investigator in another one of the large labs I studied clarified, the expectation is actually that the student produces enough "publishable work" for two papers. The typical format of the dissertation is a "manuscript-based" thesis, primarily a collection of published papers (and/or accepted, submitted, yet-to-be-submitted papers), linked together, with an introduction, literature review and conclusion.

The expectation that PhD students publish papers from their research in order to graduate is not only different from what typically happens in arts and the social sciences in these universities, the development of this as the standard in the biomedical sciences appears to be new, a change that has occurred since the early 1960s. In the early 1960s, as a retired professor who graduated in 1960 from one of these universities explained, doctoral research was written up as a traditional thesis. In his case, for example, there was no expectation that he publish papers or the dissertation in order to graduate, and none of his dissertation research ever was published. (In addition, although he was funded by his supervisor, he had chosen and designed his dissertation research project, and decided how much research he would do. His PhD research took about a year and a half).

The expectation that obtaining a position as an independent scientist will depend on having a good record of publication as a postdoc is also new; in the early 1960s, even doing a postdoc was optional. As another retired professor who had done a postdoc in the early 1960s at one of these universities explained, doing a postdoc at that time was about acquiring experience in a new research area before taking up an academic position. A postdoctoral fellowship was therefore done in a different lab than the PhD and was typically for a period of a year. These days, a postdoc is still done in a different lab than the PhD. However, it is no longer considered optional for those seeking a position as an assistant professor (or other independent scientist), and the postdoctoral period is typically much longer than a year. The federal postdoctoral fellowships, for instance, are currently three year awards, and many trainees expect to have to do a second postdoc before getting an academic position. However, since postdocs are also expected to have a good publication record, it could actually be quite risky to do a postdoc in a lab that involved working in a new research area, since postdocs who are not able to publish assume that their chances for an academic career are probably over.

Thus, there has been a shift from the earlier practice of submitting a traditional thesis in the early 1960s to the current institutionalized practice of publishing with the supervisor. This transition appears to have involved a period of time where these practices co-existed. An older professor who graduated from one of these universities in the late 1970s describes her "manuscript-based" thesis as an accepted form, but not the norm:

> P: Yes, it was papers.
>
> A: Papers. How many papers?
>
> P: Three papers that were published and one that wasn't. That I didn't submit because it wasn't finished enough to do it. But it was a chapter in the thesis. So basically four.
>
> A: And that kind of thesis was normal at the time, or...
>
> P: It was accepted in the department. So people had already done it, it wasn't the standard, necessarily, but it was definitely an accepted protocol.[11]

E. Dependence on Competitive Grants: Trainees and Co-authorships

Biomedical faculty in these universities became dependent on federal grants due to the institutional accommodation of external funding, as outlined in the last chapter. Standard federal grants have a short duration (typically three years in 2003), and allow for renewals. Grant renewals allow the investigator to continue research in a particular area, if he or she can show "progress." In practice, "progress" is measured using productivity related to the grant, in terms of published papers.

Since investigators acquire multiple grants in order to stay funded, they in turn have to delegate the experimental work. By allowing expenses for research assistants, federal grants have always provided for a division of labour on the projects. However, under very competitive conditions, investigators delegate the projects to trainees in order to try to maximize productivity. As a retired professor explained:

> I: Because our department and our institute had a reasonable reputation, we got good applicants, and so, we had excellent students. That's an unbeatable combination. Whereas techs, ...because so much of the job is "I want you to do this for me, I want you to do that for me. You know, they can't say well, "That's a stupid thing to do, I don't want to do it, I think we should be doing this," well, they may ....but they don't usually, they're not hired to do that, it's not part of the job description. Whereas a graduate student will say that. So I guess, unless you have very labour intensive type of projects, it comes down to, if you want to get return for

> your research dollar.... You can't, you can't be entirely Machiavellian about it, because you have a responsibility to the student, you can't just bring them on because they're cheap and they, they work hard....I mean they have to have a career, you have to give them a return that's worth all their hard work, in terms of training and support... [12]

Since both the student or postdoc and the investigator receive authorship credits on a paper from the lab, a paper for the trainee is also a paper for the investigator. In other words, graduate students and postdocs in the biomedical sciences are not just expected to publish, but are expected to co-publish with the investigator. The above professor continued:

> I: ...I would say our MRC, our grant support, ours are all MRC's as you know....graduate students I would say, for many of us in our institute anyway, they were the lifeblood of our lab...The group, graduate students came, young, enthusiastic, questioning, hard working, they knew they had a goal to reach to get papers published to get MScs and PhD's and get on with their life. And, about the time they got sick of you, you had a whole new influx of bright, young, enthusiastic people. You couldn't ask for a better situation. [13]

Another older professor who, like the retired professor above, had not had many postdocs, explains how the production of papers in biomedical labs is governed by pressures associated with grant renewals and doctoral degrees:

> P: I want them to graduate.... And they want to graduate. So it works at, you know, there's a little more pressure to get them out. ...It's also related to grants. So if you have to renew your grant in 3 years, you are going to try to publish all those papers in 2 ½ years. Before the renewal comes up. .... So its student pressure in terms of graduation and its granting pressure in terms of being able to apply for renewals. [14]

In contrast, when graduate students began to publish papers and submit manuscript-based theses in the 1960s and 1970s, the papers were not necessarily co-authored by their supervisor as they are now. As the retired professor above who graduated in the early 1960s told me, he had not published during his PhD, but at the encouragement of his postdoctoral supervisor at one of these universities, he did publish a paper based on his dissertation research as a paper during his postdoc. He published the paper, however, as a sole author.

Activities in the lab, therefore, under standard competitive granting support, are governed by two primary imperatives: the investigator needs to publish papers to compete for external funding, which under institutional accommodation of external funding is not a choice, and trainees are expected to co-publish papers in order to graduate and obtain positions as independent scientists. The main goals of the PhD student and of the postdoc, as trainees, are now aligned with the necessity that investigators publish papers and maintain their grant funding. This is the third aspect of the incorporation of graduate students and postdocs into the production of research.

Evidence in the data that several different practices existed and co-existed in the 1960s and 1970s (some graduate students were delegated projects and some were not, some students published and others did not, and even when they did publish they did not necessarily do so with their supervisor) suggests that while the current institutionalized practices of delegation of projects to trainees, sharing credit and co-publication with the supervisor were adopted by some investigators and trainees before federal grants became competitive, they may only have become institutionalized when investigators became dependent on the work of trainees for their own productivity.

II. Further Institutional Accommodation of External Funding

A. Acceptance of graduate students by individual investigators is directly linked to their grant possession

In the biomedical sciences, the norm is that a professor's graduate research assistants must also be his or her own graduate students. Unlike in the social sciences, graduate students are not paid as research assistants by faculty other than their supervisor. It has long been the practice in the biomedical sciences in these universities for a professor to provide funding to his graduate students from his grant. Given the widespread availability of funding, students often expect to be funded by their supervisor.

However, biomedical departments in these leading universities now also typically require that professors fund (or be able to fund) their graduate students from their grants in order to be able to accept them into their lab. In addition, they must be able to fund the student at or above a department-specified minimum level. One of the older professors I interviewed who was also chair of the graduate committee in her department describes the policy for funding graduate students:

> P: The departmental policy is that the minimum is whatever the [provincial agency] minimum is. So, a few people are paid that. However, most people are paid off of CIHR grants, and their minimum is $17,000 … 17,000, you cannot pay less. [15]

In 2006-7, the minimum annual stipend that can be paid to a graduate student from a CIHR grant is $17, 850. (In practice, this was usually the amount paid to Masters students, PhD students I interviewed were usually paid a few thousand more).

This kind of departmental policy is relatively new. The oldest one I came across had reportedly been in place for about ten years at the time of my research. These departmental policies mean that professors in these universities cannot accept graduate students into their labs unless they have enough external funding to fund them. In other words, even a professor who has external funding is limited in the number of students he or she can accept. On the other hand, there are almost no unfunded graduate students in the biomedical sciences.

This also means that a student cannot choose any supervisor. In the current environment, a student usually only has the choice of working with investigators who have a position on a grant available at that time. In most departments, therefore, graduate students have in effect become a category of paid worker, paid by those scientists able to pay them for work on delegated projects, at low but specified levels. As an illustration of the parallels, one person recounted to me that he needed a job after graduating with his B.Sc. and went to speak with a specific investigator about a job as a technician. He was convinced to do a Master's degree instead however, since although it was going to pay less, it was the "same work", and he would get a degree. [16]

B. Admissions processes involve the individual investigator

Unlike in the social sciences, graduate students must find a supervisor before beginning their programs. One of the large labs I studied was in a department where the students had to apply to the department before interviewing with investigators in a matching process organized by the department with professors who had funding for a student. However, because many departments require students to find a supervisor willing to supervise them (which now means have funding for them) on their own in order to complete the admissions process, many students actually find a supervisor before applying to the department.

In this case, the individual investigator effectively often decides in many cases who will be admitted to the department. Students contact investigators, and if the investigator has funding and is interested, he or she usually asks for their transcripts and letters to make sure that they meet the requirements for admission to the department. If the investigator is still interested, he or she invites them for an interview. Although some departments had lists of professors "accepting students," (i.e. who had grant funding for a student) students looking for a supervisor often didn't know which professors had funding for a student and which did not.

Students do apply to departments first, without finding a supervisor. But in order to complete the admissions process and be able to begin graduate studies, they still must find a supervisor. The experience of a postdoc from one of the large labs I studied had finding his PhD lab at one of these universities illustrates that the student does not choose a supervisor, but "applies" to various investigators for positions. After he had been accepted to a biomedical department at one of these universities, he came to visit fifteen labs, but then had to wait to hear from the investigators. Since he needed an agreement with a supervisor to complete the admissions process, he accepted the first offer he got. Later that same summer, before he started his PhD, the investigator in the lab that he really wanted to go to offered him a position. At the time, he felt that he couldn't change his initial decision (but says he thinks now that he could have done this). [17]

III. Dependence of the investigator on trainees in a competitive grant system

A. Recruitment of trainees by individual investigators

Potential graduate students often approach professors themselves about doing graduate studies in their labs. However, these days, when grants are won by professors, for specific projects, and there are three years to renewal, the process of students and postdocs presenting themselves at the lab as potential trainees is not always fast enough. Projects are defined, and positions on the grants need to be filled so the work can be carried out before the renewal comes up.

This dependence of investigators on trainees is associated with recruitment of trainees by individual investigators. The findings showed that many graduate students were actively recruited by investigators or through referrals to investigators by other professors, former students, and their own lab members. The undergraduates or technicians who have done projects in their labs, other labs in the department or other labs familiar to them or have technical experience in industry are often particularly sought after. This is because, as a retired professor told me, the investigator will be reasonably assured that this person is 'good at the bench' – can do technical work, is not sloppy, and can work with others. These are important to the student's potential productivity, as well as the overall productivity of the lab, but the student's grade point average and letters of recommendation, and even an interview will not necessarily give much information about these qualities.

However, the need for trainees sometimes meant, especially for new investigators, that relying on students to approach the lab and informal recruitment might not be enough. Some investigators post advertisements for graduate students and postdocs around biomedical departments. Websites have also emerged where investigators looking for graduate students, postdocs and technicians "post" the availability of these spots. An examination of even a few of the listings on a Canadian website, in existence since 1997, illustrates clearly that investigators are recruiting for positions on specific projects, and that students, like technicians and postdocs, essentially apply first to the professor, not the department, and they often start immediately:

1. Position Title :

A graduate student position is available at … to study erythroid differentiation

Recruiter Information :

Dr. A

Project Description:

A graduate student position is available at ….to study the regulation of erythroid regulation. Molecular, mouse in vivo and genechip array approaches are used to determine the roles of regulators of red blood cell maturation in transcription and signaling in mouse and humans.

Available: immediately

Qualifications:

Graduate student candidate should have a B.Sc or M.Sc., a GPA of at least 3.5 and a strong interest in working with cell culture and/or mouse models. Previous laboratory experience (e.g. summer or honours research project) in molecular biology would be an asset.

International applicants acceptable? Yes

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2. Position Title :

Graduate student to characterize novel cardiac anti-apoptotic genes

Recruiter Information :

Dr. B

Project Description:

The project involves studying novel anti-apoptotic sequences that we isolated from a human heart cDNA library by screening for suppressors of mouse Bax mediated cell death in yeast. We will overexpress and knock down the expression of the genes from the anti-apoptotic sequences and determine if they can protect cultured cardiac and skeletal muscle cells from different apoptotic stimuli. We also want to study the structure and regulation of some of the new anti-apoptotic genes since many of them are orphan genes that are in the GenBank database but their function has yet to be determined.

Available: Immediately

Qualifications:

BSc. Must meet requirements for acceptance into [graduate dept X at University Y]. Experience with tissue culture an asset but it is not a requirement.

International applicants acceptable? Yes

---

3. Position Title :

PhD candidates (Winter 2007 term)

Recruiter Information :

Dr. C

Project Description:

Various exciting projects are available in the lab to study structural and functional aspects of extracellular matrix components involved in genetic disorders. The projects focus on components of the microfibril/elastic fiber system including fibrillins. A broad spectrum of methods will be involved including recombinant protein production, mammalian cell culture, protein chemistry, immunological methods, and proteomics approaches. For further information, please see lab website.

Available: Immediately

Deadline: October 15, 2006

Qualifications:

Minimum cGPA of 3.3. The applicant must have enthusiasm for cutting edge science and hard work. International applicants cannot be accepted since the application deadline for the Winter 2007 term starting in January 2007 is expired.

International applicants acceptable? No [18]

As can be seen from the advertisements, the criteria for their acceptance into labs, are not solely academic. Many investigators are looking for students with previous experience working in a lab. My interviews with trainees showed that research experience is not necessarily part of an undergraduate program, but can be gained through electing to do an honours project in the senior year of a bachelor's degree, or through summer experience in a lab. It can also be gained by working as a technician prior to going to graduate school.

B. Masters period is the training period in a lab

Unlike the social sciences, where graduate students can often receive a considerable amount of training in methods in the courses they take, in the biomedical sciences they are trained in the individual investigator's lab. In Canada, unlike in the US, beginning graduate students in science enroll in Master's degrees. In the biomedical sciences, many re-classify to PhD programs after about a year and a half, instead of completing a Master's thesis, and spend several more years in the same lab as PhD students. Because the Master's period is the training period, and professors are dependent on the productivity of their students, they are not really interested in recruiting graduate students who only want to do a Master's degree. In their initial interviews, graduate students told me that professors asked them if they had written the admission tests for medical school (MCAT), and whether they intended to do a PhD. One of the postdocs I interviewed told me that when he began as a graduate student he intended only to do a Master's. However, soon after he got to the lab, the supervisor began trying to coax him to do a PhD. When he did re-classify to a PhD after more than a year in the lab, his supervisor further emphasized that there " was no turning back," he would have to complete a PhD in order to get a degree, since a Master's would not be awarded if a student decided to leave graduate studies after re-classification to a PhD. [19]

C. Full-time Work

Graduate students in the biomedical sciences begin research work as soon as they begin graduate studies. They work full time in the lab, taking time out during the day to attend courses and seminars at first. To a new Master's student who had been in the lab a year and a half but who didn't have any prior lab experience, this had come as a surprise:

> I: I knew it was a lot of... I knew in advance it was a lot of work. I didn't know that it was that much.
>
> A: Ah, it's more than...
>
> I: It's a full time job...
>
> A: You're in the lab all day, do you go home and work too?
>
> I: No, well personally I made myself kind of a promise, I said well, do work at work...I had enough of my day so.... they end, they end fairly late....I go in on weekends...I always get that little sense of guilt taking Friday or Saturday off because you have something else to do.
>
> A: Okay...you're normally in the lab on Saturdays?
>
> I: Sometimes I spend the day there, but most of the time I'm there for maybe an hour...I think the major thing my parents notice is that I come home very late and I'm very tired. So usually I come home, eat and go to bed....but I don't, they complain that I don't have a lot of interaction with them, but I'm just too spent. It takes me an hour to get to the lab...It's two hours of travel every day... it makes very big days. [20]

Many students and postdocs I interviewed actually started working full time in their labs as soon as they were available after being accepted by the investigator, paid for several months before they officially became graduate students or postdocs. As the following online ad for a graduate student at one of these universities makes clear, this is often expected by the investigator. Although the PhD program in the relevant department starts in September, the qualifications section of the posting makes it clear that that the investigator is only interested in a student who can start working in the lab in months earlier:

Position Title :

PhD candidate

Recruiter Information :

Dr. X

Project Description:

Various exciting projects are available in the lab to study structural and functional aspects of extracellular matrix components involved in human genetic disorders. The projects focus on components of the microfibril/elastic fiber system including fibrillins. A broad spectrum of methods will be involved including recombinant protein production, mammalian cell culture, protein chemistry, immunological methods, and proteomics approaches. For further information, please see lab website.

Available: September 2007

Qualifications:

Excellent cGPA (minimum 3.3). The applicant must have enthusiasm for cutting edge science and hard work. Only candidates will be considered who are available for an interview in person, and who are willing to do a summer project of 2-3 months before the actual PhD program starts on September 1, 2007.

International applicants acceptable? Yes [21]

The annual stipends to students from standard grants or fellowships, are received as pay every two weeks through the institution. Trainees reported that they worked in the lab full-time on a year round basis, with the exception of a two to four week vacation each year.[22] As one student told me, "you're only hurting yourself" if you take more time off. But the dependence of investigators on the productivity of trainees led some investigators to pressure students for regular hours, and even for regular evening and weekend hours.

D. The student as a cost

The intersection of provincial requirements, established in several provinces in the 1970s, that universities must charge international students differential tuition fees (higher fees for international students than for Canadian students when the student is in the residency period) with the departmental policies has made international graduate students in the biomedical sciences in these universities rare. An investigator outlines the policy in her department:

> P: And what we have specified is that, if you accept an international student...you must pay the differential in the fees. So the supervisor has to pay the differential. ...So the [Canadian] students pay their fees and we pay the extra above that, for the other students.
>
> A: OK.
>
> P: Which makes it expensive to accept foreign students. ...It's a major restriction.
>
> A: It's a major restriction. Because the fees are...
>
> P: There's two disadvantages. One, you don't have the student to interview, and second, you have to pay the differentials. They have to be good (laughing).... Money is important! [23]

Although education is usually seen as an investment, dependence of the investigator on students and being required to pay them in the biomedical sciences has turned the student, at least from the point of view of the investigator, into a cost. The professor supported by competitive grants, who must think of cost vs. productivity of lab members, may have no reason to believe that an international student will be more productive than a Canadian student. Students that cost the investigator more were unlikely to be accepted. The process by which the international student from Europe below came to do a PhD in a large lab I studied illustrates how these cost vs. productivity concerns of the professor were allayed in her case, making her recruitment to the lab much less of a cost (and a risk) to the supervisor:

A: ...So how did you get to grad school here? ...you decided I guess to leave the country?

I: My thesis professor, my boss in [my country] did a sabbatical in [this] lab.

A: ... Oh I see. This man has a lot of people who do their sabbatical in his lab.

I: Yeah, he is very big in the world. And for me, I arrived, I came... first for four months the year before I started my PhD.

A: For sort of like a...

I: I was finishing up my thesis work. And we had some work to do with my boss since he was spending a year here, it was difficult to work together...So he asked me to come.

A: Okay.

I: And I really enjoyed it. I enjoyed the lab, I ask [the investigator in this lab] what do you think if I come, and he said yeah, no problem. But you have to get a fellowship (laughing). He never...it's very rare he accepts people right away without a fellowship.

A: Okay. So you had to...you applied to your own country for ...?

I: Yeah, fellowship to come in.

A: And were those government fellowships or?

I: They were all sorts, [the first one was] connected to my university. They want the people that work in the university to be trained. It's a training fellowship.

A: Training fellowship from the university but you can take it somewhere else?

I: Yes.

A: Okay....And that fellowship, how many years was the fellowship for? The whole time?

I: It was for almost three years.

A: Okay.

I: And .... you get it for one year, then it gets renewed if you're lucky. Then I applied for another fellowship. I got paid by [the investigator in this lab] for two years, I guess, two or three years. Then I got another fellowship for one year and then it was renewed for another year. ...I got, I got three different fellowships.

A: And that second fellowship was from where, [this university] or?

I: Never, never because I am not Canadian. None... all my fellowships were from [my country], there were three. [24]

Nevertheless, there were several graduate students in the large labs I studied who had initially approached labs as international students, in the country because their spouse was already here, who ended up being graduate students, even without fellowships from their home country. They too allayed the concerns of the investigator about cost. They applied to become Canadian permanent residents, which meant that they could be paid from grants without paying international fees, as well as possibly receive Canadian scholarships.

E. Scholarships and Fellowships in a Competitive Granting Environment

Although the norm is graduate students are funded throughout their degrees, in practice the professor doesn't usually fund the student for the entire length of the degree or postdoc. Most graduate students in the large labs I studied had held fellowships for part of their graduate studies, and most postdocs came to these labs with external fellowships to fund a two or three year period. Typically, graduate students came to the lab without fellowships, and then applied for them during that first year. Below a new graduate student describes his funding situation in his first year. His description also illustrates the rather vague understanding many students have of the regulations and conditions that affect their funding:

> A: How were you funded? You told me that he…in the summer he said he would pay you…. did you discuss amounts at first?
>
> I: No, there's a basic minimum amount, I think it's by law they have to give us…
>
> A: By law?
>
> I: I don't know, there's some kind of ruling where like they say that graduate students must receive a minimum of this amount of money per year….So wherever the money comes from, we get paid that minimum

amount. So in the summer I got paid only by him.

A: Okay.

I: Then September comes you write fellowship applications. I won something from [the university, a] fellowship that covers me for a year. So it pays two thirds of my salary. And the third that's left is still his payment.

A: Okay, so he tops up to...

I: Well the salary, from getting all paid from the lab, or getting paid partly by the fellowship plus the lab is...you get the same amount...you keep the same amount.

A: .... he tops it up to that amount. Which I understand is about 17 [000] right?

I: I don't...I think it's 15 or 16 [000], I'm not sure. [25]

So the investigator is responsible for funding them but in practice they didn't usually have to fund their students fully throughout their degrees. Graduate students are often funded through a mixture of funding from fellowships and supervisors grants. This source of funding was actively sought after particularly by new investigators, as is evident in the qualifications section of a posting for a graduate student position by an assistant professor. The posting, made in June, was available immediately, but was intended for a graduate student who would be able to win scholarship funding later:

Qualifications:

Positions for HIGHLY motivated graduate students are available. ONLY M.Sc. and Ph.D students with outstanding track record are encouraged to apply since they will be expected to apply for external funding. Individuals joining our lab will receive a complete training in molecular cell signalling and will have the privilege to gain expertise in mass spectrometry, proteomics and molecular biology. [26]

Since the institutional accommodation of external funding that means graduate students can only be accepted by professors who have funding, and in a competitive grant system funded professors delegate the grant- supported projects, this means that scholarships will be held by students and postdocs of funded professors who will be assigned parts of faculty research. Students applying for a fellowship in the fall after beginning work on a project with the professor, are applying with that project. However, the practice of assigning the project to the student is institutionalized now, so even if the student begins with their own scholarship, they are often still doing projects assigned to them by the investigator. Postdocs coming to the large labs I studied with a project proposed on a fellowship were usually asked to do another project when they got there. In other words, scholarships to students and postdocs in this environment have in many cases have in effect become a subsidy to the research programs of funded professors.

F. Competition for Trainees

Because biomedical scientists are dependent on trainees in order to do research, there is competition for them between labs and departments. One of the labs I studied was in a department housed in a research institute which was part of a hospital. They didn't have an undergraduate program in this department, which is an important source of recruitment of graduate students. In order to attract students, one of the retired professors explained that the director of the institute regularly went around to other universities to raise the visibility of the institute in order to help with recruitment of trainees to its labs. In order to compete with other departments and universities, this department had also established minimum stipend levels which the investigators had to pay graduate students which exceeded the amounts that CIHR required investigators to pay graduate students from its grants. In order to help attract and retain top students with their own funding, graduate students in this department who won major graduate student scholarships (such as those from CIHR and NSERC), were also awarded an additional 3000 dollars a year. There is a very similar department in an adjacent hospital. Even though these two departments are competing for students, since they are very similar and in such close proximity, they had reportedly agreed to keep the amounts they pay to graduate students the same, so they did not have to compete with each other for graduate students on stipend amounts.

IV. Involvement of trainees in the production of research and publication and its effects on training

A. Cutting edge research, grants, and risky, difficult projects

While graduate students are expected to produce publishable work, they are typically assigned their projects, so do not choose (and sometimes do not know) the level of risk and difficulty involved. As a postdoc told me of her PhD project:

> A: When you came and [the investigator] discussed what you might do, did she give you a choice of projects, or…
>
> I: I went in to her telling her what I wanted, but I also said I'm a [medical specialist in the area of X], so she said "You know what, this one's perfect for you because this enzyme is expressed in … [X] and in a developmentally regulated fashion, such that she suspected there would be a predominant [X] phenotype." So I jumped on that right away, I wasn't even interested in the other project. So I said "Sure, I'll take that on."
>
> A: OK.
>
> I: Not having a clue how difficult that project was going to be, and the potential for disaster, which just dumb blind luck prevented, I think, in retrospect. [27]

No one can be sure that any project will work. But changes in the demands of the federal granting agency over time have contributed to investigators trying to do risky projects. In the past, as a retired professor explained to me, "A paper was a paper, as years went by high impact papers counted more." That is, papers in top journals, as defined by average number of citations per article for that journal, now counted more in the grant competitions. As an investigator who had been on grant panels of the federal granting agency explained, they are now asked to rate "quality, not the numbers" of papers when it comes to assessing the publication record of investigators in grant applications. An investigator who wants to try to publish in top journals will have to attempt risky research, since, in the words of a postdoc, the top journals publish research that is "not only novel but surprising."

A former PhD student in one of the large labs I studied, for instance, was assigned a project by her supervisor where she was looking for a gene that her supervisor thought should exist. She was successful in finding it, but it was difficult project involving a lot of risk, something she describes as a common characteristic of graduate students projects in that lab:

> A: ...When you started in that lab, how did you start, did you have a particular project right from the start?
>
> I: Yes, I did.
>
> A: And how did you get that project, basically?
>
> I: It was let's say pure luck and somebody had to do it. It was a difficult project. Usually grad students, and they are given the worst projects

(laughing)

A: I'm coming to understand that.

I: Because they have lots of time. [28]

She later returns to this issue when we discuss the project of a then-current postdoc in the lab:

I: No, but this is the kind...he is a postdoc. This is the kind of project for a postdoc. He...more or less expect something, try to prove. So this is kind of shorter project. For me...

A: When you started...

I: When as a grad student, it's much more in the air.

A: Much more ...

I: Maybe there is a dream. "Why don't you try to find it? " (laughing) [29]

PhD students "have more time" because the normative length of the PhD was currently much longer than a typical postdoctoral fellowship or appointment. Doctoral students typically spent 5 -8 years in the lab, while postdoctoral fellowships or appointments, on the other hand, are currently usually shorter, lasting for two or three years. In addition, in order to be considered for a position, a postdoc expects that he or she must have a good record of publication in that shorter period. So risky projects for postdocs were often avoided or rejected. [30]

B. Getting a project to work

Students often described the first part of their graduate studies as trying to get something to "work." According to many students and postdocs, since negative results are thought to be a lot less likely to be published, negative results are not usually written up and submitted to journals.[31] In practice, therefore, in order to publish papers, students do not just do a project and publish the results, they do projects until one "works." Students often have to attempt more than one project before something "works." Some students had spent as much as 2 or 3 years on projects before getting them to work or having to abandon them.[32]

Because a lot of cutting edge biomedical research is now done *in vivo* (e.g. using live cell cultures and live animals), students and postdocs often must first develop cell cultures or create specific animals so that experiments can be done. [33] One of the postdocs I interviewed began his graduate studies with a project that did not work, so he switched to a new project when he began his PhD in that same lab. The project involved working with cardiac myocytes (heart muscle cells). Myocytes do not divide, so a cell line could not be obtained from outside the lab.[34] Instead, in order to do his experiments, he had to isolate the myocytes himself from rat hearts:

> A: When you're starting your PhD, you actually switch to a new project....What do you start doing with that new project?
>
> I: Well, the first thing again was getting a good cell culture.

A: These are …primary cardiac myocytes.

I: Yeah.

A: So you do that.

I: Yeah. Well, that was, that took a while. (laughs)

A: Yes, how long did that take actually…

I: Oh boy. To get it good, was about two years

A: Ohh.

I: before it actually. Yeah.

A: Right. OK.

I : So I was doing preliminary experiments on cells that kind of looked half decent. But… I was trying to refine the isolation. And it took about - it took quite a while. [35]

He explained they first tried to isolate them from embryonic rat hearts, but realized at a later point that they would have to use neonates (newborns) in order to ensure they had enough material from each isolation to do experiments. But trying to isolate the muscle cells from the other types of cells in the heart took a lot of time:

A: So how far are you into your PhD, when do you have it …working, so that then you're doing [experiments] and not focusing on refining that process?

I: Ah, that was like quite a few years in, when everything was working well…so that was '94 -'96, figuring out how to get good cells…. And then

'96-2000 doing the experiments and the papers.

A: Right, but using the isolation process, you're not refining it any more.

I: Not refining it, it just works (laughs)

A: OK. [36]

It becomes evident in the next excerpt from his interview how working *in vivo* further contributed to the length of his degree:

A: But because it's a primary [cell culture], does that mean that it's only good for a while, or?

I: It means you have to isolate them every week

A: (long pause) Oh... So we're not establishing a stable cell line or anything here…

I: Primary cells. Isolated every week….Very few, that you get as well. Like a maximal perfect yield would be 40 million cells, which is 40 little wee dishes. And that's when everything went really, really well.

A: OK.

I: That's, that meant - that's I think one of the reasons that my PhD took so long, because I didn't have a cell line, right. In order to repeat the experiments, you have to have new cells every week.

A: And how long did this process take every week? And it's you who did it?

I: Yeah.

A: OK.

I: It's like a full day, to get like, to do the isolation, and then you had to wait a couple of days for the cells to be prime for doing the actual experiment.

A: OK, and then you would do experiments every week…

I: Well, either a different experiment or a repeat of one that you'd already done that had to be repeated. [37]

Because the cells can only be used for a week once they are isolated, in order to do the experiments, they had to be isolated each week, from live rats. Although students do not usually do the regular care, feeding and breeding of the experimental animals, it *is* the individual trainees who usually do the technical work with the animals necessary for their experiments.

Since the student's thesis work has been integrated into the production of faculty research and publication, the length of time required to finish the degree is affected by the demands of cutting edge research and the granting process, as mediated by the supervisor. It can also be affected by the demands of the journals themselves.

C. Demands of Journals

Although the student has to publish, if the investigator wants to publish in a top journal, such as *Science* or a *Nature* journal, not only are projects more risky, much more work will probably have to go in to the individual paper than one that is published in a less prestigious journal. A student in one of my large labs, who eventually published his first paper in one of the *Nature* journals, had spent six years in his PhD before his first paper came was published. After some time working on his project, new work in the literature meant he had to take a whole new direction on the project. This involved using an animal model (Drosophila - fruit fly), which was difficult, especially since it was not used by anyone else in his lab. He said that the project was very stressful, since not only was it long and difficult, there was always a risk that it wouldn't work. After a rejection of the first paper they wrote by a top journal, he did much more experimental work. A new version of the paper was submitted to a second top journal, and was accepted, providing the lab did further experimental work, which took the student a good part of another year:

> A: So the early work that you did, it seems you didn't publish it, you waited until you did this extra stuff that you did on flies.
>
> I: Absolutely.
>
> A: Why did you do that?
>
> I: Because it's what made this paper interesting.
>
> A: Well, it seems it looked quite interesting...how did it get decided that you wouldn't publish...how did this go?
>
> I: Oh it was published. The biochemistry work was published in there too. What I ended up doing was that you have a protein and okay, I studied biochemical properties. I discovered interesting things on it. But then around 1996 or yeah some time in 1996, 1998 people started looking

in Drosophila at … cell growth…cell growth is what a cell does when it doesn't divide… . Well what genes control that? It wasn't known. And it turns out that the gene [for his protein] is important for regulating cell growth. At least one of the pathways….Cell growth is even more basic than cell division. Because if the cell cannot grow it doesn't divide… So that's why it's so important. And that's probably one of the reasons why my paper got into such a great journal. Because it's - I basically show that this protein synthesis inhibitor that I worked on is able to block cell growth ...as we thought it would do, right. And I showed it. In a live animal.

A: In a live animal. Okay. But okay there's all this pressure to get papers out….how did the decision process go? You had finished some stuff, probably you could have published it. Or not?

I: No, we tried to get it published in - what's it called - [the journal] *Science*, but the data that wasn't very convincing at the time. So I went back and I did more experiments. But doing fly work isn't something you do rapidly. It takes a long time. So I had to generate all these reagents and I did a lot of fly work. I was exclusively doing that for the longest time. So then we submitted it to *Nature Cell Biology* and it came back and they said we would be interested in looking at a new version of this once you make like changes to your manuscript. Which I did.

A: Did those require extra experiments?

I: Oh damn, yeah. Oh lots of experiments. It was really a lot of work.

A: But they told you what they would really need in order to...

I: Yes. The reviewer suggested anything that they would like to see and I did also that.

A: So you kind of knew at that point that it would go in if you got this other stuff working. But that other stuff [took] how long, like a year, two years?

I: ...like I really had a lot of work in this paper. Some of it was not even shown in the manuscript. So there's...like [a fellow student] was telling me one day. He says "It's like you have two papers in one paper," and it's quite true. Because like this paper is two chapters of my thesis and they're not short chapters. So...

A: No, but maybe if you hadn't done it that way it wouldn't be in *Nature Cell Biology,* right?

I: No, probably not...Yes, yes. I'm happy though that I have this, but it was very stressful.

A: Because it was many years that you weren't having any publications.

I: Absolutely.

A: And how many years was it from when they said okay we'll publish it, if you do these extra things, which were obviously going to take a long time...

I: It was less than a year but it was many months. It was at least six months. [38]

Although he might have been able to publish a paper based on his work earlier, in a less prestigious journal, these decisions were made by the investigator, who wanted to publish in a top journal. This student published another paper two years later, and was able to graduate after eight years.

D. Supervisor controls the publication process

Although the graduate student and postdoc must publish, it is the supervisor who usually chooses the project. In addition, the supervisor controls the process of publication from the lab, often deciding what results will be published, and/or when and to what journals the papers will be sent.

Students usually do not understand, certainly at first, how involvement in production of published research will affect them. A Master's student in one of my large labs explains how she now could see how the investigator's goal of publishing in top journals affected the students, given that they had to publish, but their investigator controls the publication process:

> I :...if you want to publish in journals that aren't quite as high as journals that [this investigator] wants to publish in, you can probably get papers out a lot faster....Of course it's always nice to have a paper from a really good journal, but if it takes you two extra years, is it worth it to you as a student? I mean - it could be really frustrating when you have all this data and you could publish if you went to this journal that was step below what

> your boss wants to publish [in] but you're going to be there for two extra years because he refuses to publish until you.... because he won't publish in any journals below that. [39]

A postdoc in one of the large labs I studied saw the indeterminate length of the PhD as one of the biggest frustrations with graduate studies. The following excerpts from his interview show the relationship of publishing his research to the length of time spent in the PhD:

> I: I think sometimes the...I guess grad studies, in our field, like there's no guarantee as to when you will finish. ... There's no guarantee. Like from the beginning, there's no defined end. You go to, let's say medical school, you're done in four years.
>
> A: When you say there's no guarantee, you mean there's no guarantee when you will finish...
>
> I: Yeah. You don't know how much time you're going to be there. If you even think you're going to be there four years, you're there five years. I wanted to be there three and a half/four years. I ended up being there five and a half....
>
> A: And this was because...
>
> I: I wanted to finish my work. [40]

Another part of his interview reveals that finishing his work involved getting his papers published before he left the lab. It also reveals another aspect of the supervisor's control of the publication process: it is the supervisor who will decide who the first author on a paper will be:

> A:...And how is it decided that you would finish your PhD when you finished? What were your expectations or her expectations of what you needed to get a PhD?
>
> I: Well papers, a couple of papers, like one, at least something published. And, something pending. The committee agreed that I was a good candidate to finish, so she wasn't here anyway, there were so many conditions involved.
>
> A: But basically, you know, maybe one [paper] out and others on the way and then it's okay? But before that you wouldn't try to leave the lab?
>
> I: Like...I was never going to try to leave the lab without this paper.
>
> A: Okay.
>
> I: There's just no way, otherwise I knew she wouldn't.... I put my heart and soul in, it's like, there's no way I'm going to let her take that away from me.
>
> A: Actually it came out just before you defended, ... so it wouldn't have been published by the time that you were [submitting your thesis]...
>
> I: Well I knew, I knew it was...It was going to be accepted. ...I submitted it, my thesis in March...But the paper was submitted in December...The

> first review came back. And I, the review said that it needed to be shorter. So the way they worded it was that if you submit a shorter version of it, we'll accept it.[41]

I was initially confused about why some students were worried about losing first-author credit on their papers if they left the lab before the papers were published. Students really didn't seem to trust that they would necessarily get first-author credit on a paper if they left the lab before seeing the paper through to acceptance by the journal. There seemed to be two main reasons. First, if more experimental work had to be done after the paper was submitted, if you were the only one who could do it, you had to be there to do it, or the paper might not get published at all. A postdoc from Europe doing a second postdoc in one of the large labs I studied, for example, had left his first postdoc in Europe without having finished the work for the paper he was working on. He had agreed to start this second postdoc at a certain time, but still had more experimental work to do in the first lab. Since the materials and equipment necessary to do this work did not exist in the new lab (not to mention that the investigator here wanted him to work on new projects in this lab), it was not clear that he would be able to finish the work for the paper. This was very stressful since he did not have a publication from the new lab yet, and he was more than a year into his second postdoc. [42]

Similarly, one of the PhD students I interviewed had arranged to go to a postdoc at a top U.S. university. The investigator in the new lab insisted that he begin in a specific month. When he left his PhD lab, however, he still had several papers unfinished. When he came back several months later to defend his PhD thesis, he also went back into his

PhD lab at his former supervisor's request for two weeks to do the bench work necessary to complete one of these papers. Although his supervisor wanted to submit the paper to a top journal, he didn't. Top journals, as illustrated earlier, often ask for more experimental work, and he didn't want to have to come back to the lab again. [43]

Second, if the student or postdoc left the lab before the paper was published, although it might still be published, he or she might lose first-authorship. Later, I found this same idea stated explicitly as a norm in a handout circulated at an intellectual property workshop for graduate students at one of these universities in 2005. This could happen because the investigator gave the first-authorship to someone else who later did more experimental work on this paper after you left the lab, or because another student or postdoc who had also done work necessary to publish the paper "needed" the first-authorship more. Postdocs expect to need first-authored publications to be considered for positions, PhD students expect to need at least two first-authored publications to graduate, and it is the investigator who ultimately controls the distribution of authorship credit.

E. Trainees usually have their own projects, but the projects are ultimately the supervisor's

Although the investigator usually delegates a project to the trainee, he or she does not *give* the project to the student or postdoc. Student and postdocs may lose first-authorship if they leave the lab because the supervisor controls the publication process, and, as was implicit in the examples above, because the project itself stays with the lab. As a PhD student told me of his lab, recounting how a postdoc tried to take samples from the lab when he was returning to his own country, "it's understood that materials stay in the lab." [44] Although the student works on a project, the project is ultimately the supervisor's project.

Projects are often passed from a trainee exiting the lab to a trainee entering the lab. A postdoc in one of the large labs I studied was assigned a project in her PhD lab when she began as a graduate student, involving a particular enzyme. The project involved a biochemistry component, as well as working on creation of a "knockout" mouse. The creation of a knockout mouse is risky and difficult. She actually inherited this project from an outgoing postdoc who had already been working on it for three years, without success:

> I: Oh it was hilarious. I was assigned to a project. I had no idea what I was doing. It was really quite funny. And the reason, she was, I was, working on a knockout [mouse].... I just jumped in. It was so funny. ' Cause I did [enzyme B]...
>
> A: I can see that in your papers....So she had a knockout all ready for that?
>
> I: She had a postdoc who had been working on the knockout for three years and Nicole was disheartened in leaving because she didn't think she had it [the knockout mouse]. To make a long story short, she actually did have heterozygote animals, didn't realize it. With the help of Greg, who was [the investigator's] chief tech - I took the project over with Greg. And we managed to actually breed [these mice] to have our first homozygotes born in November of [the same year]. We didn't really know what we had,

but we were able to get the homozygotes by November, so I was very lucky, in retrospect that, uh, I had something to work on.

A: So they were in the lab…

I: So they were in the lab, and Nicole, basically the design was very poor….Finally, when I started we went back through the books, Greg and I, 'cause Greg was a tech and he hadn't had - so between the two of us, going back through the blots and everything we realized that we had a contaminating band. So we looked at all the mice we had genotyped, and realized that we had some where there had been a slight shift, so we thought OK, what's to lose, let's breed those together….and sure enough those were the mice that gave us our knockouts…. So we had actually, but that was only because a new person took over. So you know, you retrospectively go back through everything. [45]

Although this part of her project had gone well, the biochemistry side of her project had not advanced as far. That part of the project was given to an incoming student when this student was finishing her degree.

When she and the technician had success with the knockout mouse, and published a paper in a top journal, they were made "co-first authors." This is indicated by placing asterisks above each of the first two authors names on the paper, and indicates that they are both considered to have made an equal contribution. It allows both trainees to count the paper as a first-authorship for the purposes of completing a thesis or obtaining a position. However, the former postdoc who had done all of the initial work was made a co-first author too. The investigator had apparently made it clear from the beginning that if they succeeded in getting the knockout mouse, the postdoc too would be given equal credit as a first author on the paper:

> A: So she left...
>
> I: She left as soon as I came.
>
> A: So she didn't feel highly successful.
>
> I: Well, she did [in the end] because [the investigator] was very fair. The bottom line was that the first paper that came out of that, Nicole was on it as a co-first author. Because she'd put so many years on it.
>
> A: OK.
>
> I: So when we actually did realize that we had [it], we emailed Nicole, and we characterized it, so Greg, myself and Nicole are co-first authors on that *Nature Genetics* paper, which was a huge paper.
>
> Y: Yes, I see that was very early on, well, two years after you started in the lab.
>
> I: ....It took us a couple of years to characterize and publish. But Nicole, she's - Greg, Nicole and I are co-first authors on that. So [the investigator] was very fair recognizing Nicole's contribution. And she made that clear from the word go, which I really respected her for. [46]

Because the project usually stays with the lab, and the assignment of first authorship rests with the supervisor, a graduate student or postdoc who leaves an unfinished project without publishing may not get credit. The use of "co-first authorships" seemed to be a way for investigators to address trainees needs for first-authorships in order to complete a thesis or obtain a position in the delegation-credit-evaluation arrangement with trainees developed earlier, when projects were typically not as long and/or as difficult, and did not involve as many people.

F. Dependence on the trainee, length of degree, and the ability to pay students

Even though projects can be passed to new trainees, investigators have a vested interest in trainees finishing what they are working on before they leave the lab. Since the professor is under considerable pressure to manage his or her funding resources to publish papers, and is dependent on the trainees, he or she has a considerable interest in keeping the advanced and successful student or postdoc in the lab when the trainee has unfinished work that is not yet published.

Although students expect that they must publish at least two first-authored papers published in order to graduate with a PhD, having two papers doesn't mean that a student will graduate. Doctoral fellowships in Canada usually run out before the student graduates, and at that point the supervisor often takes over funding the student. The dependence of the supervisor on the work of the students and postdocs can lead to pressure from the investigator to extend the length of the PhD, which he or she can do by funding them, as the length of the PhD is not fixed. Far from being urged to finish up their PhDs, investigators sometimes pay their students more towards the end of their degrees to keep them in the lab.

One of the postdocs I interviewed, for example, had published four papers in the first four years in his PhD and so was ready to graduate. However, he stayed an extra year in his PhD to finish other work he had already started. He explained that he could have gone on to do a postdoc at that point, but agreed to stay since the professor was willing to raise his stipend to $30,000 in that extra year, approximately the same amount he would have been able to earn in a postdoc. This year did not actually cost his supervisor $30,000. Since the student still had a year left in his $15,000 graduate fellowship, the supervisor only had to pay him an extra $15,000. Two additional papers resulted from the work done during his extra year in the lab. [47]

Similarly, another student who graduated from the same lab as the student with the *Nature Cell Biology* paper had many, many more papers at graduation, but also stayed eight years in the PhD program. A Master's student in that lab told me that she had come to understand that the lab was an "eight to ten year" lab, while the investigator's lab downstairs was a "five year lab" (that investigator reportedly did not believe the PhD should be longer).[48] This suggests that despite the norm of two papers, in practice, the supervisor has some influence on the length of degrees for students in their lab. The pressure applied to trainees demonstrates not only the dependence of the investigator on the trainees, but the dual status of graduate students and post-docs. Under a system of competitive standard grant support, the production of research is dependent on their labour. Graduate students and postdocs have become essential workers in the production of research process, in addition to being trainees.

Since scientific credit is integrated with the structure of training and PhD students expect to be judged on their record of publication, they too often want to publish as much as possible. However, as the next example suggests, this motivation may be connected to the intention to become an academic scientist. One postdoc I interviewed related to me that after finishing enough papers for a PhD, she wanted to graduate and do an industrial postdoc. Her supervisor resisted because she still had a year of funding left on a five-year studentship, and had still had work that could be done during that period. She found out from the company where she had applied to do the industrial postdoc that her PhD supervisor was not sending her letter of reference. Although her supervisor eventually did send the letter, and she got the postdoc, the incident was very stressful for her, and may have caused her to reflect more deeply on the social relations in the lab. This was the only trainee I interviewed who explicitly described the research papers produced in the lab as the supervisor's, saying "they're not our papers, they're his." [49]

Although some people had changed their minds while in graduate school, the majority of graduate students I interviewed began with the goal of becoming a professor. Only two graduate students indicated that they began graduate studies with the explicit goal of going into industry after graduation. A PhD student in one of the large labs I studied had started a biotech company during his PhD studies, which he ran part-time. Although this company was the student's primary concern, he intended to finish the PhD. [50] This is because the PhD seems to have informally become a necessary professional qualification for scientists and managers in the biotech and pharmaceutical industries. A new graduate student in one of the labs I studied had started graduate studies after many years working as a technician in a biotech company because there was no way for her to move up to a position as a manager or a scientist in industry if she didn't have a PhD. [51] Similarly, the manager of a genomics core facility who had a Master's degree told me that he was often looked at askance by people he met at trade fairs etc. because he didn't have a PhD.[52] It remains to be seen whether those who become students so they can obtain a necessary professional qualification for industry are motivated to produce as many papers (staying in the lab for extra years, etc.) as those who intend to become academic scientists.

G. Taking a project from the lab: generation of possible competitors

Because trainees now work on the projects of their supervisors, this also usually meant that they could not take their projects from the lab. In other words, the student or postdoc is usually not allowed to continue research on their project in the next lab they go to, or to start out as an assistant professor with that project. The fact that the trainee does not own the project helps explain the at-first confusing statement of a successful graduating student in one of my large labs. He had accepted a postdoc already, in a completely different research area, which he hoped would give him skills that would be useful in industry. Of a career as a professor, he said: " I don't see what I would do research on. I like everything and nothing." [53]

Delamont and Atkinson (2001) have argued that the doctoral student in the sciences is primarily gaining skills during the PhD. The additional idea that the supervisor does not give the project to the trainee enhances the understanding of this process. Unlike the social sciences where the graduate student often builds a research identity as they design and carry out their own project, graduate students in the biomedical sciences become proficient in particular techniques and perhaps working with certain animal models. Although the trainee has a project, it is the supervisor's project, and he or she retains control of the materials and equipment necessary to do the work, as well as the data.

Not being able to take a project from the lab is not ideal for the trainee trying to become an academic scientist. One postdoc I interviewed already had a position as an investigator, so I wondered why she decided to do a postdoc. She told me that she had gone to the lab of a new investigator for her PhD, but hadn't realized until she had been in that lab for awhile both the importance of continued research, and that an investigator in the early career stages might not allow or could not really afford to let trainees take projects from the lab. She had come to do a postdoc in one of the labs I was studying in order to start her own new projects:

> A: Why did you decide to do a postdoc? Or that's the next, if you want to be a PI that's the next [step]
>
> I: No, I mean a lot of people would say you have to do that, it's expected. I needed...so I came out of [her PhD supervisor's lab] with very sound molecular biology and protein biochemistry skills.
>
> A: Right.
>
> I: But, being a new PI, there was nothing she could let me take away with me, for my own lab. So I have three potential projects that I knew I could develop for myself.
>
> A: OK.
>
> I: I knew I had to come to a big lab, where I could come in and say this is

what I want to do, and I want to take one of these with me. And I needed someone to who would say "Oh sure."

A: OK

I: And so, somebody like [the investigator in this lab]. I also wanted to learn a new technique. I need some high through-put [microarray], because that's where things are moving....So I specifically came here to do that.

A: So one of your projects is high through-put?

I: All three were microarray screens .... I will be taking these with me and [the investigator] had no problem with that. [54]

When I asked if those projects were related to projects she had done in her PhD lab, her answer begins to reveals the complex relations between research, training and the career stage of the investigator in a competitive grant system:

A: ... So she won't let you take anything from the lab. When you developed these projects, though, are they based on things that you did in her lab?

I: These projects? No. Completely dissociated.

A: Completely new.

I: I left [molecule x] field completely. And I did that because I want to stay friends with her.

A: OK.

I: And I think that the only way, honestly, is if we are not even in the same field, 'cause she would, I think she would get angry, um, she would not be easy going if I was to say pick up a [molecule x] and come into some competition with her later on. And I don't, I just - she's too valuable a resource and too, too important to have any sort of negative...

A: How is she a valuable resource?

I: She'll be able to read grants for me, she's already told me she'll read my grants.

A: OK.

I: And her grants have ranked No. 1 at the CIHR for like, three years in a row. The woman writes phenomenally well...And her science is spectacular and the combination is very unique. ...So she told me when I left, "Oh, I'm happy to read everything for you." I know I can always go back there for intellectual input, you know, she's told me that I can go and get help... as much as I want, I have tremendous support there and I'm not going to do anything to jeopardize that.

A: So you changed lines of research basically, with these three projects...

I: And [the investigator here], when I came in, I said this is what I want to do and he will tell you "you have to do something to take with you." His philosophy is very different. He doesn't want any of this stuff.

A: Right.

I: You know, he expects that the papers, he'll be the senior author, right, 'cause I'm a postdoc in his lab and that of course makes complete sense. [55]

Now that investigators delegate projects to the trainees, the trainees have their own projects, but they are working on the investigators' grant funded projects. If they were to take that project with them, they would be taking the investigator's project, and further, may later become competitors with the supervisor for grants and for publication priority. Her experience also further illustrates the way distribution of credit works in the current research environment. Although she has her own funding and is bringing her own project to the lab, authorship credit for the investigator on any papers published is understood. Just as graduate students and postdocs accept projects that the investigator delegates expect to receive in return the first-authorship credit they need to achieve their goals, an investigator with enough resources may be able to accept a postdoc bringing a project to the lab, but expects to receive credit on any papers in return.

Discussion

The findings show that dependence of most biomedical scientists on trainees is associated with the incorporation of the graduate student and postdoc into the production of faculty research, using several institutionalized practices. The data suggest that these practices differ from those in the early 1960s, and were not standard even in the 1970s. While the project is still central to both the organization of research and of advanced training, the findings show that projects are now typically designed by faculty and assigned to graduate students and postdocs. In other words, while the trainees' research is organized around a project, they no longer design that project. Second, unlike technicians in the past, who usually did not receive authorship credits on papers, scientific credit is extended to trainees in papers published. Finally, scientific credit is informally integrated into the structure of training. Instead of completing a traditional thesis, PhD students expect to have to publish at least two first-authored papers from the results of their research to graduate. The doctoral thesis in the biomedical sciences is now usually based on the content of papers published (or accepted, submitted or to-be-submitted papers) on which the student is the first author. Similarly, postdocs expect to obtain positions as independent scientists on the basis a good record of publication.

The incorporation of the trainee into the production of research via assignment of projects, integration into the credit system and integration of the credit system into the structure of training can be seen as aligning the goals of the trainee with those of the investigator under the imperatives associated with his or her dependence on competitive standard grants. Although further research would be necessary to document the earlier situation and the process of change, since there are suggestions in the data that these practices differ form those in the 1960s and 1970s, it is likely that these practices did not become institutionalized until after renewals for federal grants became more competitive.

The finding of a delegation-credit-evaluation system for the incorporation of trainees into the production of research is consistent with and extends the application of principal-agent theory in analyzing the relationship between science and government. David Guston (2000) argues that principal-agent theory is an important tool for analyzing the relationship between the government and science:

> … principal-agent theory applied to science policy means that the government is the principal who requests the agent - science - to perform

> tasks because the principal is not capable of performing them directly. The agent performs the task, out of self-interest, but with some of the benefits accruing to the principal as well. (P. 15)

In other words, the government funding of academic science through competitive grant funding can be seen as delegation of projects to scientists, which the scientist performs out of self-interest, with some benefits to the government (e.g. development of the health care system, growth of the economy). The argument of this chapter is that when investigators are dependent on competitive federal grants, the necessity that they obtain multiple grants and therefore multiple projects means that they are "not capable of performing them directly," and a *second delegation* occurs. Multiple projects must be delegated, and under the allowances for expenses on standard operating grants, they are delegated to trainees. The extension of credit to the trainee while at the same time requiring the trainee publish papers to reach their own goals of graduating or obtaining a position can be seen, in turn, as an informal requirement which motivates the trainee to produce publishable results out of self- interest, but with benefits which also accrue to the investigator, since the authorship credits are shared.

It seems likely that the expanded use of scientific credit in the biomedical sciences was initiated by investigators themselves, as an adjustment to the imperatives of competitive grants, which under competitive conditions necessitated delegation of projects to trainees. In any case, the granting agencies obviously accepted these multi-authored papers as evidence of an investigator's productivity on his or her grant applications. These co-authored papers have also become the basis of the PhD thesis, changing the traditional content of the dissertation. The student's research project is often actually chosen and designed by the investigator, and the content of the dissertation is based on a series of papers co-written with the investigator, the thesis is original research, but it is based on a project which is not conceived of and designed by the student, and where major portions of the text have often been co-written by the supervisor. The university accepts this co-authored work as a dissertation. More research is necessary to explore the process by which the expanded use of credit by investigators was accepted by the granting agencies and the university.

Although the granting agencies and the university may have accepted multi-authored papers, the prevalence of multi-authored papers has been problematic for the editors of biomedical journals. The emergence of scientific authorship was associated with an individual author who both received credit and was responsible for the scientific claims (Biagioli 1998); multi-authorship problematizes the attribution of responsibility for the claims made in the paper. The International Committee of Medical Journal Editors (ICMJE) has attempted to deal with this problem by establishing guidelines for authorship which Biagioli (1998: 6) argues attempt to re-establish the priority of the individual by limiting authorship to those who can each take full responsibility for the work. The following are the guidelines outlined in 1997:

> All persons designated as authors should qualify for authorship. The order of authorship should be a joint decision of the coauthors. Each author should have participated sufficiently in the work to take public responsibility for the content. Authorship credit should only be based on

> substantial contributions to (1) conception and design, or analysis and interpretation of data; (2) drafting the article or revising it critically for important intellectual content; and on (3) final approval of the version to be published. Conditions 1, 2 and 3 must all be met. Participation solely in the acquisition of funding or the collection of data does not justify authorship. General supervision of the research group is also not sufficient for authorship. Any part of an article critical to its main conclusions must be the responsibility of at least one author. Editors may ask authors to describe what each contributed; this information may be published. (ICMJE 1997: 982)

The guidelines requested that others contributing to the research "who do not met these criteria should be listed, with their permission, under acknowledgements, or in an appendix." (ICMJE 1997: 982). Biagioli (1998) argues that this request does not seem to recognize the role credit plays in the evaluation process in professional science:

> ... such a reform of authorship would work only if accompanied by a serious re-education not only of researchers, but also of those who evaluate them for jobs, promotions, and funding (P.11)

As the findings in this chapter show, the integration of credit into the evaluation system has been extended beyond its use in evaluation for jobs, promotions and funding, into the structure of training. Graduate students too now expect to be evaluated for graduation on the basis of a record of publication with their supervisors.

Despite increasing prominence of authorship studies in both academic and public discourse, there has been very little examination of scientific authorship by scholars (Biagioli, 1998). Although the implications of multi-authorship with respect to responsibility have begun to been examined, scholars have not systematically studied the question of *why* multi-authorship in the biomedical sciences became widespread.

For decades, the multi-authored or co-authored publication has been seen as an indication of collaborative activity (Katz and Martin, 1997: 2), and as consequence, there is generally a consensus that the increase in the occurrence of multiple authorships is an indicator of an increase in collaboration (Smith 1958; Clarke, 1967; de Beaver and Rosen 1978, 1979a and 1979b; Heffner, 1981). Multi-authorship seems to have begun, as commentators as early as Caplow and Reece (1958) suggest, with individual scientists co-authoring papers. Journal editors in the biomedical sciences "began to notice this tendency in the 1970s and usually interpreted it as resulting from the need to pool together different skills and specialized knowledge within increasingly large and collaborative research projects" (Biagioli 1998: 6).

On the basis of my findings, I suggest, however, that the dependence of investigators on competitive grants may have changed the primary reason for the occurrence of multiple authorships in the biomedical sciences, at least in Canada, from collaboration among scientists, to a division of labour with trainees in their own labs. The practices of delegating the projects to trainees and extending credit to them means that almost all of the research papers published in the biomedical scientists in these universities will have at least two authors from the same lab, the trainee and the investigator. Only *some* papers from *some* labs have, in addition, collaborators from other labs. The current near-universal multi-authorship of research papers in the biomedical sciences from these universities, therefore, is arguably best understood not as the result of collaboration between scientists in different labs, although this is common, but as the result of a division of labour with trainees (necessitated by having multiple grants and a need to have competitive productivity), with which unlike technicians in the past, authorship credit is shared.

The sheer tenacity of the assumption that multiple authorships represent collaboration with other scientists becomes evident in this excerpt from Katz and Martin (1997):

> In general, collaboration between peers (i.e., scientists of similar standing) is more likely than collaboration between individuals of unequal rank but this is by no means always the case. In this connection, Hagstrom made a curious observation about the relationship between teachers and students – namely, that in some teachers' minds students do not count as collaborators. During his interviews, he asked scientists of co-authored papers if the work was carried out in collaboration with others. A number of scientists replied 'no' although most or all of their papers had been jointly written with students. This may have been a reflection of the social distance between the teacher and student combined with an implicit belief that 'true' collaboration must involve a partnership of equals. (5)

Another possible interpretation is that multiple authorships do not necessarily represent collaboration. The suggestion that scientists themselves do not consider graduate students as collaborators has been missed by those studying co-authorships. [56] (This is interesting given that the relationship between the scientist and the graduate student was traditionally seen as a master-apprentice relationship.)

Based on the findings of this study, it is suggested that the relationship between investigators and trainees is not best characterized as collaboration, despite the fact that they appear together as authors on research papers. The further institutional accommodation of external funding by these universities in recent years, where departments in these universities require investigators to ensure students will be paid at a certain level when they accept the student, means that beginning graduate students can often only do their graduate studies if an investigator who has a position on a grant-funded project agrees to fund that student. This, combined with the fact that trainees are typically assigned a project by the investigator, whose own career success is dependent on the productivity of those trainees could arguably be seen as having turned the trainee into type of paid worker who receives "on-the-job" training. However, this study suggests that the relationship between investigators and trainees is one of dependence of investigators on trainees as a result of a necessary division of labour on grant funded projects, *where trainees are compensated with both annual stipends and scientific credit.*

The categorization of graduate students and postdocs as employees is actively resisted by the granting agencies and the universities, despite that fact that the granting agency often provides the funding for them to work on grant funded projects, the departments require that the investigator be able to pay the graduate students, and both the department and the granting agency have regulations about the amount that graduate students must be paid. However, the argument that they have become at least a new type of paid worker in biomedical research sciences is amply supported by numerous practices of investigators, such as taking in graduate students and postdocs only when they have funding available, advertising for positions, recruitment of trainees on experience, recruitment of trainees on cost (international students rare), expecting the trainee to begin work immediately after being accepted, expecting full time work on a year round basis, extending the length of the PhD or postdoc with funding from their grants, as well as topping up the stipends or fellowships of graduate students as an incentive to stay longer in labs. Perhaps the most suggestive indicator is that, although the trainee works on a project in the lab, the project does not typically become their project in the sense they can take it with them when they leave the lab.

The increasing complexity and risk associated with cutting edge biomedical research, as well as changes in the grant system which reward investigators for engaging in cutting edge research, seems to be resulting in further changes to the organization of research and training. Despite the fact that scientists supported by standard grants cannot hire permanent employees (as in countries like France, see Chapter 4), given the informal requirement that the trainee publish, the ability to assign projects and control the publishing process, and to pay trainees from research grants in the context of variable length of PhDs and postdocs, the head of a laboratory in these leading Canadian universities often has the resources with which to do long, difficult and/or risky projects. In the system of delegation of projects and integration of credit into the structure of training, as the investigator attempts to do risky and difficult projects, the trainee bears more risk, which can lead to longer degrees and postdocs as the projects take longer to do or have to be abandoned for new projects.

Because current trainees are not just paid workers, but compensated with both funding and credit, these types of projects seem to be leading to further adjustments to use of the reward system in the biomedical sciences by investigators. The appearance of "co-first authorships" suggests, for instance, that the investigator may have to increasingly manage distribution of credit in their labs, as projects are worked on by more people in the lab than in the past (as a result of their complexity, and/or because the projects that do not work are passed to new trainees or because journals demands more experimental work be done on papers), in order to ensure that trainees who work on increasingly long and difficult projects still get the credit they need to graduate or compete for positions in the institutionalized delegation-credit-evaluation system that developed under standard grants.

Conclusion

The structure of the standard grant from the federal granting agency in the biomedical sciences in Canada has always provided for a division of labour on grant funded research. As argued in the last chapter, in the past, this division of labour was between professors and technicians. Under competitive conditions, given the structure of allowable expenses on standard federal grants (and many others), the division of labour in most labs is now between professors and trainees.

The findings show that dependence of most investigators on trainees is associated incorporation of the graduate student and postdoc into the production of faculty research. This is accomplished through several institutionalized practices. The main argument is that the investigator's strategy of obtaining multiple grants and their dependence on trainees in the system of competitive federal grant support results in delegation of the grant funded projects to trainees, and that this practice is coupled with a change in the use of the reward system in science. Not only is the trainee, unlike the technician in the past, given scientific credit in published papers, but his or her goals as a trainee have been aligned with those of the investigator through the informal but institutionalized integration of scientific credit into the structure of training.

In the next chapter (Chapter 6), we further examine how the institutionalized strategy for investigators supported by standard grants, obtaining multiple grants, leads to change in the organization of research and training as the lab group of the individual investigator successful in getting multiple grants grows, and more generally therefore to differentiation in the organization of research and training between biomedical labs.

[1] Interview with postdoc, December 12, 2002
[2] Interview with retired professor, Nov. 5, 2003
[3] Interview with older professor, Sept. 17, 2002
[4] Interview with older professor, Sept. 17, 2002
[5] Interview with older professor, Sept. 17, 2002
[6] The January 2007 funding decisions on standard operating grant applications made in the fall of 2006 to CIHR suggest that five year awards are now more common.
[7] The budget module of the 2006 version of the CIHR operating grant application appears in Appendix 1.
[8] See Shapin, 1989 for a discussion of the invisibility of technicians in Robert Boyle's lab and in general.
[9] The convention differs in different fields of science. In chemistry, for instance, the lab director (investigator) is usually the first author, not the last author as in the biomedical sciences.
[10] Interview with doctoral student, Dec. 6, 2002
[11] Interview with older professor, Sept. 17, 2002
[12] Interview with retired professor, Nov. 5, 2003
[13] Interview with retired professor, Nov. 5, 2003
[14] Interview with older professor, Sept. 17, 2002
[15] Interview with older professor, Sept. 17, 2002
[16] Interview with former Masters student, July, 2003
[17] Interview with postdoc, Dec. 12, 2002
[18] Experimental Medicine Job Listing, 2007
[19] Interview with postdoc, Sept. 26, 2003
[20] Interview with Masters student, Oct.23, 2002
[21] Experimental Medicine Job Listing, 2007
[22] This schedule, which required taking courses while working full time in the lab, was rough for foreign students who were not familiar with the language and the testing style of the courses they had to take. In one dept, where the students had to take 4 courses, an international student told me she had a lot of trouble with these courses. She explained that the language was a real barrier, since although she knew the words in her own language for chemicals and molecules they were so different in English she often could not understand what was being said. The courses that these graduate students take are often undergraduate lecture courses and foreign students were often unfamiliar with the written testing and the multiple choice format of these tests. These things together contributed to the student above failing a particular course many times. Her supervisor, she explained, was very understanding once it became evident that this was a problem, and gave her time away from the lab to study. Another foreign student, who was now a postdoc in one of these large labs explained that her Canadian PhD supervisor, who was cross appointed to several departments, told her that she had to become a student in a different department than she had intended to. Her English was not good, and the more prestigious department required PhD students to take several undergraduate courses

which would be hard to pass with limited English skills. Instead, she enrolled in a medical department which had no undergraduate program, where she could take graduate seminar courses with only 3-5 students.

[23] Interview with older professor, Sept. 17, 2002

[24] Interview with former PhD student, Oct.16, 2002

[25] Interview with former Masters student, Oct.23, 2002

[26] Experimental Medicine Job Listing, 2007

[27] Interview with postdoc, Sept. 23, 2003

[28] Interview with former PhD student, Oct.16, 2002

[29] Interview with former PhD student, Oct.16, 2002

[30] In contrast, in France for instance, PhDs are a maximum of three years. Doctoral students in the sciences are funded by national training grants which the lab directors assign to students, but which last a maximum of three years (and temporary workers, including PhD students cannot be funded by the lab for more than a year, see Mangematin and Robin, 2003).

[31] In the biomedical literature, this is referred to as "publication bias." At least one study of editorial decisions did not find a statistically significant difference between publication rates of submitted manuscripts with positive and negative results (Olson et al. 2002), however, if negative results are not usually submitted in the first place, a significant bias would exist.

[32] One postdoc told me that she did have some negative results in her doctoral research that her supervisor felt should be published, but now a year into her postdoc, she had not found time to write that paper yet.

[33] [Note about *in vivo* work].

[34] [Note about cell lines]

[35] Interview with postdoc, Sept. 22, 2003

[36] Interview with postdoc, Sept. 22, 2003

[37] Interview with postdoc, Sept. 22, 2003

[38] Interview with doctoral student, Dec. 6, 2002

[39] Interview with Master's student, Oct. 24, 2002

[40] Interview with postdoc, Dec. 12, 2002

[41] Interview with postdoc, Dec. 12, 2003

[42] Interview with postdoc, Oct. 31, 2002

[43] Interview with doctoral student, July 23, 2002

[44] Interview with doctoral student, July 23, 2002

[45] Interview with postdoc, Sept. 18, 2003

[46] Interview with postdoc, Sept. 18, 2003

[47] Interview with postdoc, Sept. 26, 2003

[48] Interview with Master's student, Nov.1, 2002

[49] Interview with postdoc, May 10, 2002

[50] Interview with doctoral student, Oct. 25, 2003

[51] Interview with doctoral student, Sept. 15, 2003

[52] Interview with lab manager, Nov. 6, 2003. In addition, he had to explain that the Master's degree that he had was an "earned" Master's, since in the U.S., graduate students

in the biomedical sciences start as PhD studnets and are awarded a Master's as a consolation prize if the supervisor does not think they should proceed further in the PhD.

[53] Interview with doctoral student, Dec. 6, 2002

[54] Interview with postdoc, Sept. 18, 2003

[55] Interview with postdoc, Sept. 18, 2003

[56] Since Hagstrom (1965) was writing in the mid-sixties, it is possible that the scientists writing papers with their students who didn't see them as collaborators may have been physicists. Berelson (1960) found that physicists, unlike biological scientists at the time, tended to assign projects to their students.

Chapter 6

The career strategy of biomedical scientists supported by competitive standard grants and how it affects the organization of research and training

Professors hired in the 1960s and 1970s seem to have seen building a larger lab as a necessary consequence of obtaining multiple grants, one which was not necessarily welcomed. A professor hired in one of these universities in 1965, for instance, told me that when he began as a faculty member he wanted to have a small lab and keep working at the bench himself. However, since he later lost his federal grant, and was able to continue to do research by getting funding from industry, which meant he had to change research areas, he thought the "biggest mistake" of his career was not trying to build a bigger lab from the start.[1] However, given that the practice prior to the increase in competition for federal grants, beginning in the 1980s, was typically to have one grant and renew it, it seems unlikely that he would have had this goal initially.

The imperatives associated with competitive federal grants and their institutional accommodation by the medical faculties of these universities, however, seem to have made trying to build a larger lab itself a career strategy for biomedical scientists. Among new investigators I talked to and postdocs I interviewed, building a larger lab was often an explicit goal.

In this chapter, I examine the effect of this strategy on the current local organization of research and training in individual labs, and on post-graduate training more generally. Through the practices of investigators and the experiences of students and postdocs in the large labs I studied, the findings suggest that there are changes in the organization of research and training as the lab group of the investigator successful in getting multiple grants gets larger. Although not a representative sample, the work histories of trainees who have been in both large and small labs provided access to the detailed ways in which the social organization tends to differ in labs of different sizes, and at different stages of an investigator's career.

The findings show that while a master-apprentice type of relationship between students and professors might be found in the lab of a new investigator, as a lab grows, there tend to be key changes in the practices involved in organization of research and training. More specifically, the findings indicate that the nature of the delegation of experimental work by investigators to trainees (described in Chapter 5) changes as the lab group grows. In new and small labs, data show that the investigator tends to delegate the experimental work to students, but continues to direct that work with close examination of their results and instructions about what steps to take next. In larger labs, in contrast, students work much more independently, and are often much more responsible for the direction of their projects. As a result of these changes in individual labs over the career of investigators successful in getting multiple grants, there is differentiation in the organization of research and training among biomedical labs.

The main argument of this chapter is that the strategy necessitated for investigators supported by competitive grant support, obtain multiple grants and building a larger lab group if successful, is associated with key changes in the organization of research and training as the lab grows. As a result, there are significant differences in the organization of research and training between small and large labs. These differences also appear to mean that the two stages of postgraduate training, graduate studies and postdoctoral work (both are typically necessary to obtain a position as an academic position in the biomedical sciences) are best done in different types of labs. The findings indicate that the small lab of a new investigator may be the best type of lab in which to do graduate studies, while large labs may tend to be a better environments for postdoctoral work. The findings also indicate, however, that these considerations were not widely understood by beginning graduate students.

I. Organization of research and training in new labs: existence of a master-apprentice relationship

Newly hired investigators usually do not have multiple grants. As a senior investigator told me, a new investigator is probably not going to be able to get more than one grant until he or she gets the first one renewed. New investigators therefore tend to have small lab groups with just a few members. As accomplished researchers with current technical skills, they often work at the bench themselves, highly motivated to get results and publish papers to get their first grant or renew their first grant, in order to get tenure as well as apply for multiple grants. A postdoc from one of the large labs I studied describes his experiences as a beginning graduate student in the lab of a brand new investigator who was just getting his lab started. Unlike most graduate students, he is actually being trained by the investigator himself:

> I: ...So [the project] was trying to understand why [enzyme x]....It was just breaking out. So, the experiments were, let's see if we can prove that [enzyme x]

is necessary for preventing cells from dying.

A: Right....OK, so you started just doing some experiments, were you learning techniques from someone, from him?

I: Yeah, from him... Basically, more or less reading the protocols from journals and doing them myself. I knew how to use, see I knew how to work in the lab, I knew what a pipette was, I knew how to measure microliter volumes, I knew how to handle cells, so I could read a procedure and go yeah, I can understand that.

A: And then follow it, and ...

I: Yeah, and then if I had any questions, I'd ask him.

A: OK. Him, rather than, you weren't assigned to the tech or ...

I: No, oh no.... Yeah, he's working in the lab too, he had a lab coat on, sitting at the bench too.... "So Brian, how do I, uh, where's, how do I handle X, how do I re-suspend it, what do I , what concentration do I use." You know this interaction....

A: Right, you didn't have to wait 'til lab meeting, or... you just asked him.

I: No, talked to him every two minutes.

A: Right , right. Just back and forth.

I: He was like a senior, senior, senior postdoc. That had his own lab, I mean he had just gotten his lab. [2]

His comment about the investigator being like a senior postdoc refers to the fact that investigators, unlike trainees, do not usually work at the bench. In fact, in his case he does not really have a distinct project, he and the investigator were working at the bench on the same project:

> A: And so when you first went into that lab, did you have a particular project?
>
> I: ... we talked about some things. I had never heard of signal transduction before. I had...this was a brand new field for me.
>
> A: .... OK. So what did you do first when you went into that lab?
>
> I: Well, I knew how to grow cells, and how to do cell based assays, so he said let's think about this hypothesis and just jumped into that. It was receptor signalling, and so we had some tools, and it picked up really quick.
>
> A: So you actually started on the bench right away, doing some experiments that he thought were a good idea?
>
> I: Yeah, yeah.
>
> A: But did you have a defined project?
>
> I: ... Not really. Not really. It was very laid back. It was "Let's just do these experiments for six months and see how it goes." [3]

New investigators typically train their technicians too. Another postdoc in one of the large labs I studied whose research experience began as a technician in the lab of an investigator who had been hired about two years before he started:

> A: What did you do when you went into that lab? Did you work with a more experienced person?

I: [The investigator] basically guided the project at the very beginning. Like she was actually showing me what to do, it wasn't the other technician.

A: OK, so she, she - right, because the lab is quite small.

I: Yeah.

A: And was that project related to some of the other projects in the lab?

I: At that point, no, the main project for the lab was not the one that I was working on. It was kind of a side thing. I think that's why she wanted another technician, for this. [4]

He worked on that project for a year, before they gave up on it, and he began working on the main project in the lab. His work on the project was still closely supervised the investigator herself:

A: ...so for that [project], there were other more experienced people in the lab, so were you doing things with another more experienced person at first?

I: No, the way it worked, is basically I was, you know, the technician, so she would just tell me the project basically. Like she wanted to know the developmental stages that might occur. And then she just told me to do it.

A: ....And then you would design the experiments, or ?

I: Like I would just do them (laughs) [5]

Even after he later became a graduate student in that lab, the close supervision of his work by the investigator continued. As he outlines below, the investigator closely supervised the projects of *everyone* in the lab at the bench, examining their results, discussing them, and giving them specific instructions about how to proceed:

I: Like, she was always around looking at what everybody was doing.

A: ...Is she actually working at the bench?

I: Well now, no.

A: At that point.

I: At that point, every now and then. For the time that I was a technician there, she was doing some work on the bench.

A: ...OK, right so you start this new project...how does it go from one thing to the next?

I: Well, basically, she would [for instance] look at the light box and see what was there and then like come up with ideas about what the next step would be, and then discuss them. And then she would actually write out, kind of, what she wanted you to do.

A: Ah, OK.

I: So you would put that in your book, or whatever.

A: OK, and you would take it from there....

I: Yeah, yeah. What she wanted. Like, she really controlled what happened in the lab.

A: 'Cause she would actually look at the results and then sort of make decisions from there.

I: Yes. [6]

In sum, the data suggest that new investigators are often actively doing experimental work themselves at the bench. A master - apprentice type of relationship between students and the investigator might be found in these new labs, where investigators themselves train incoming graduate students and technicians, and closely supervise their projects.

II. Small but growing labs: technical training by other lab members and close supervision of projects by the investigator

If the investigator is successful in obtaining multiple grants, the lab grows as he or she hires more lab members. Once the investigator has trained lab members, these graduate students or technicians are often given the responsibility for training of incoming students in the use of techniques. Another postdoc outlines that the initial technical training he got as a new graduate student in the small lab of a new investigator was from a PhD student:

> A: ...what did you do when you first went into the lab?
>
> I: I went in, the student showed me all the techniques I needed to know they use and for the first few days I did some reading. And then I just...I learned a bunch of techniques. And then just followed the PhD student's protocols. Basically he established a system to study a different set of mutations and he said well you can study this mutation. [7]

Similarly, one of the postdocs in one of the large labs I was studying was initially trained as a beginning graduate student by a technician in the small lab she did her PhD in:

> A: And [you] worked with the tech.
>
> I: Greg Haddon....Very, very competent. Good in everything....I learned everything from Greg in the first year. *Everything from Greg.* It wasn't until I was, had my, enough experience, you know, sort of came up to him, that I would have to start going outside the lab to get the technical help I needed. [8]

As an older professor explained earlier, this delegation of the training of the new students in the use of techniques is part of the career strategy for investigators:

> In an ideal situation when you have a number of maturing students in the lab, the senior graduate students will then instruct the junior graduate students....When you get down to the smaller lab, more of the direct work devolves to me, I have to show them how to do things. [9]

Although new investigators with small labs might not be showing the students how to use techniques themselves, *they were actively involved in supervising the experimental work on their projects.* In the case of the first student above, the investigator was still in the lab a lot when he started as a graduate student, working on her own projects. He received constant feedback on his results during their daily interactions at the bench:

A:...How was your supervisor involved in your work then?

I: She was very young, assistant professor, so she was there every day, constantly. She was practically there all the time wondering what's going on. Asking questions as she - very one on one.

A: Did she actually do experimental work herself? At that point?

I: Once in a while, but not on these projects. She had her...some other projects going so she would like do some lab bench for the first year or two that I was there. So you'd find her on the bench in the morning once in a while doing things. But then towards the end she didn't. [10]

Investigators with small labs might instead supervise the projects at a weekly lab meeting. However, as a postdoc in one of my large labs who was trained in a PhD lab that had four or five members explains, this supervision still consisted of close examination of the results of all lab members, and specific instructions or suggestions about what to do next. The lab meeting context also allowed for input by other lab members:

A: So how would you say that your supervisor was involved in your project?

I: ...She drove the direction of the project for the first year that I was there.

A: How did she do this?

I: Well, we had very intense lab meetings on a weekly basis, it's not like [in the big lab she is doing her postdoc in]

A: OK, and how did those work?

I: They are not at all like [those in this lab]. Everybody, we would all sit down, we would bring our binders of results, and the rough blots [experimental results] and everything would just go on the table. And she would go around [the table] and see what everybody's progress was for that week...

A: Right.

I: And you know, Chen would start. And he'd show his blots for the week. "And well, you were having that problem last week and I told you to do this and did you do it. No. Why not?" And so, there would be this type of interchange.

A: Right.

I: And so, the plans for next week would be spelled out in lab meeting.

A: Ah, she...herself, actually set them.

I: Yep... Well, she would set them, we would maybe agree or disagree. But everybody else in the lab would have input as well... So, it was very informal, and I think a better way to run [a lab meeting] than what we do right now [here in the large lab]. [11]

The findings therefore suggest that once an investigator has trained a number of students and technicians, responsibility for training new incoming students in the use of techniques is delegated to those lab members. Although students are usually not trained in the use of techniques by the investigator in most small labs, the data suggest that the investigator is closely supervising the projects in the lab, through on-going examination of results and specific instructions or suggestions about what experimental work to do next, in the case of both graduate students and technicians.

In the last chapter, the findings showed that in a lab supported by standard competitive grants, unlike in the past, students' projects are assigned to them. The findings in this chapter suggest that in new and small labs, they are delegated only the experimental work on those projects, not the direction of those projects. This close supervision of projects by new investigators is probably directly related to their vested interest in the productivity of those first projects for their own careers.

III. Organization of research and training in larger labs with standard competitive grant support

The lab groups of investigators successful in getting several grants could get larger quite quickly. A postdoc in one of the large labs I studied describes how the lab he did his PhD in grew over the first four years of his degree in the late 1990s:

> I: When I started, there were three students, a couple of post docs and then eventually those students became seven students, a few post docs and technicians. At one point, there was close to 20 people. [12]

Unlike in a new or small lab where the investigator is often very involved in experimental work, the investigator with a larger lab has usually moved away from the bench. Not only do they not train the new students at the bench themselves in the use of techniques, or do experimental work themselves, in the large labs I studied, the investigator does not spend much time in the lab *at all.* A Masters student in one of these labs explains that he had come to understand his supervisor's distance from the bench in terms of a division of labour in the lab, where the students did more manual work and the supervisor did more of the mental labour, which included a lot of management, necessitated by the fact that it was a large lab:

> A: Okay, when did you start really realizing that this, his job, was a bit different than you thought?
>
> I: Well, I mean, the obvious thing is he works in an office, we work on the bench. There is more...well, I mean the thing is they do more brain work. It's a whole lot more on the thinking level. They read papers, they discuss papers, they ask questions about papers, they think about them. They think of projects, so it's a lot more...it's a thinking position. You think a lot, [rather] than doing things on the bench.... You think of the experiments as opposed to doing them.... I [also] didn't know... .you'd need a lot of management [as an investigator]. I thought just knowing the science was enough because you're a science director.... but there is a lot of coordination. The students. The projects. The equipment. And the grants. Since it's a big lab...[13]

A. Working more independently

As in small labs, the trainees' work is typically organized around a project, and they are not usually trained in the use of techniques by the supervisor, but by other lab members. A former graduate student in one of my large labs describes her initial technical training (note this is very early in my research, note that I start by assuming the student must do a lot of reading when beginning a project):

> A: Okay. And so when you first got to the lab, then what did you do? Did you read lots of papers?
>
> I: I read some papers, but [the investigator] doesn't like people that read too much. He wants people to work (laughs).
>
> A: ... Okay, so you had some idea how to start then?
>
> I: Well I ask [the investigator], I ask other people with more experience, that's the process of learning, you know. If he's not able to tell you experimentally or because it's a new technique or because he doesn't have time, he says go and ask this person who's done it before. So that's what I did.
>
> A: Okay. Who was that person?
>
> I: [A PhD student] showed me how to do the technique. He was doing it for himself for another gene, so it was very easy for him. And he was also finishing up his PhD, so...
>
> A: He was...
>
> I: One of the best candidates to explain to another grad student. "Poor graduate student, such a big project, how to go on" (laughs) [14]

What seems to differ most in terms of organization of research and training between large and small labs is the amount of interaction between the trainee and the investigator, and as a result, the supervision of the projects. As a student in one of my large labs said of her professor, "We don't really talk to him, he's always busy or away, we talk to each other." This situation came as a surprise to many new students:

> I: ....and there's very little interaction. I barely see him for the whole week and I actually feel lucky if I get a "Hello". And if I get a "How are you?" well that's a bonus (laughing) So I mean, it's understandable because there is a lot of students.
>
> A: Right.
>
> I: But there could be a little more people interaction, I guess maybe it's something I need more, I'm not sure. [15]

In large labs the investigator is not usually giving the new student daily or regular specific instructions about how to proceed with their project. This means that in many large labs, students tend to work more on their own from day-to-day than in small labs. A senior student in one of my large labs explains that the advantages of his lab do not include supervision, and sums up how the nature of supervision in his lab differs from that in many other labs:

> I: The main difference I would say is that we have to warn people that they have to be independent. Because you have to be very independent in [this] lab. That's the main difference. Money wise, we're very good. Equipment wise, we're very good.... You have to be independent compared to other labs where you get more supervision .... let's say, let's put it this way. Even if you're a Master's student,

> you have to be a PhD student. In the sense that you have to do all the reading and all the [planning] yourself. A lot of Master's students in other labs are being taught from top to bottom.... Basically there'll be someone there every day to tell them, okay, "yeah, that's good data, you'll do this experiment".... That's not the way it works in our lab. [16]

Instead, as a postdoc in a large lab I studied explains, new students in the two large labs she has worked in "work independently," seeking out other lab members for guidance on their project:

> I: I would say those students will be very, you know, work independently
>
> A: But sort of under the guidance of, at least someone that's...
>
> I: ... I would say the project is independent. You need like support, technical support.
>
> A: To learn techniques and...
>
> I: Yeah, also to, to interpret your results, how to continue your project, you have to ask around. Usually there's one person that knows better than other people about your project. So, you need to ask them. [17]

A second year Master's student in one of my large labs explains how this worked in her case:

> I: I talked to Mark, I did a lot of work with Mark. Mark ... he was very interested

> in ...he was actually... I didn't even go to him, he came to me because he wanted to know more. Like, so that he helped me out a lot. Adrian is one I can go to for guidance if I want.... I definitely would go to Nick. Depending on what your problem is you go to different people because you know who can help you where. I still go to Ed. I mean Ed has always been there no matter what. Because he trained me for the first few months and it was just like a basic. It can't be theory because he's not working... ...almost no idea what I'm working on. But if it's technical stuff I can go to Ed. And on different things I'd go to Serge sometimes, go to Henry sometimes, just depending on what you want. [18]

After students achieve a certain level of technical competence, they didn't tend to do this as much. A postdoc who already had a Master's degree when he began his PhD in a large lab explains the difference:

> I: Basically, I just didn't spend a lot of time - from a technical point of view, I was just more confident. I just do it. I don't have to ask, go around asking people how to do things. [19]

B. Lab meetings: a means of communication in large labs

Large labs in the biomedical sciences commonly have weekly lab meetings attended by the supervisor and most lab members. In two of the three large labs I studied (and others that the trainees had been in), there were weekly lab meetings of approximately one hour, attended by the investigator and most members of the lab. I attended all of these meetings while I was present in these labs. Unlike the lab meeting described for the small lab earlier, where all students informally present their results at the same meeting, in large labs the format is often one where between two and four trainees presented the results of their work in to the whole group using an oral presentation with overhead or projected slides. All of the trainees in the lab (and in one lab, the technicians too) were scheduled to present, several at each lab meeting, on a rotating schedule which saw all graduate students and postdocs making presentations but not more than once every five or six weeks.

In contrast, lab meetings in small labs did not tend to involve presentations. A postdoc who did graduate studies in a small lab explains:

> I: ...we had lab meetings every week, yeah, we just went around the table and talked about our work, it was very informal. It was less formal than [this large] lab, our lab.
>
> A: So you didn't have ...people doing a longer presentation.
>
> I: No. [20]

Some students who started graduate studies in new or small labs reported not having lab meetings, at least while their labs were small. A postdoc who had been in a lab that had four lab members (one or two other students and a technician), in addition to the investigator, during most of the time he was a graduate student explains:

I: At that point, we didn't really have formal lab meetings - didn't happen 'til quite a few years later, where they became formal....It's because, like we were so, like everybody *knew* exactly what everybody was doing. There was no *need* really.

A: OK.

I: Yeah, it was really small. [21]

One of the main purposes served by a lab meeting in a large lab was as a means for lab members to communicate both the topics and the results of their research to both the investigator and the other lab members. First, since the investigator does not usually spend time in the lab, formal presentations by trainees at lab meetings allow the investigator to keep up to date with the work going on in the lab, and to have an opportunity to have intermittent input on their project. Second, in large labs, without lab meetings, other members of the lab would not necessarily know what the other lab members were working on. This is important given that trainees are expected to consult with other lab members on technical issues and for guidance on their projects. A senior student in a large lab explains how his presentations in lab meetings are designed to inform other lab members as well as solicit comments and criticism:

I: When I present .... I summarize what I'm doing. I give a little explanation and then I say what I'm doing. In my view this is...it serves many functions ... So I educate people on what I'm doing. I educate them on the technique I'm using. I present stuff that they may not have been exposed to before...I remind them of what I'm doing and then I present my data. I say what, what's working and what's not working. And I like to get feedback. So this is why I think there should be lab meetings…get criticism, suggestions, educate people.

A: Right so the different sections of your presentation are for different audiences...

I: Absolutely. Okay, that way it works for everyone. Some people they just come with a piece of paper or an overhead and they... and they just explain in ten seconds and that's it. Obviously either they're stressed or they're not comfortable presenting that and they don't understand that the criticism is actually something useful. So very difficult to get criticized. I don't like it but I do it because I know it is a good thing for me. [22]

Newer students responded to their own scheduled presentations at lab meetings by trying to have something to present, so they worked as a form of pressure to get something done. Unlike senior trainees, newer students tended to see the lab meeting more as a forum in which their progress and competence would be judged than as one where they might receive useful criticism.

In sum, in large labs, the investigator has typically moved away form the bench, and students were typically initially trained in the use of techniques by other lab members, as in smaller labs. However, since the investigator has moved out of the lab, students work much more independently than in small labs, instead seeking out guidance from more experienced members of the lab for technical support on their projects. Lab meetings in large labs typically provide the main vehicle for communication of results to the investigator at intervals of several weeks or months, as well as between lab members working at the bench about projects being done and techniques being used.

IV. Re- introduction of close supervision of projects in a large successful lab: an episode

In one of the large labs I studied, where the investigator was supported primarily by standard grants, the nature of supervision was changing. The older and former students in the lab reported having been trained in the use of techniques by an established member of the lab, and then subsequently working more independently themselves on their projects, as in other large many labs. And as in other large labs, they presented their results at intervals of several weeks at a weekly lab meeting.

However, about two years before I came to the lab, the investigator had introduced two new practices in an attempt to provide more supervision to the newer students. However, these practices were not very successful, and four of the five Master's students who had been in the lab for at least a year were planning to leave the lab.

In the detailed access to this episode in the lab's history provided by the work history interviews with the trainees and my participant observation, the reasons for difficulty in trying to provide close supervision in a large lab of a very successful scientist become evident. And in the experiences and reactions of both senior and junior trainees during this episode, a more detailed understanding begins to emerge of both the needs of new graduate students, and of the key stages that graduate students go through in training.

A. Each student presenting results weekly

Although students in the large labs often worked independently of the supervisor for weeks at a time, in this lab, most trainees actually presented their results to the investigator *once a week.* Most trainees attended not one, but two weekly lab meetings: a general lab meeting like the described in the last section, where only two to four trainees would present at intervals of five or six weeks, and another one where they met in a smaller group with the investigator and the members of the lab working in their research area and presented results obtained in the previous week. These small group meetings had been a new initiative in this lab about two years before, an attempt to provide more supervision to the students.

Here it became evident that the attempt of an investigator in a large lab with many trainees to supervise new students projects' using formal presentations of results in a weekly lab meeting was problematic. A Master's student who had been in this lab for a year and a half describes his experience with this kind of supervision:

> I: ...the problem is, the lab, whole system. The way it works.
>
> A: Can you elaborate a little bit about that...
>
> I: ... in our lab, especially, a lot of people complain about lack of supervision. We don't have a lot of supervision, even though we have a lot of meetings. I personally get the feeling that when I go to meetings it's just to get destroyed. So whatever you've been doing this week you'll just get it...you'll get it scrapped.

And most of the time that's what happens because you'll never...out of our supervisor we never get any...I never got any congratulations and that's a problem for some people.... we don't have a lot of supervision, and also the fact that sometimes we don't get the feeling that we don't know where the focus is. Let's say I'm doing one experiment one week, I show him the results, he says no...he tells me "Why did you do this?" I said "Well, last week you told me to do so."

A: Yeah (I've been attending the lab meetings)

I: But the week after he decided no, it wasn't worth it.

A: On the basis of your results though.

I: On the basis of my results or sometimes...sometimes it's totally random. I get the feeling that his - I guess, I guess he - since he's been working in the field he knows where he's going, but we don't know where he's going, because he's not telling us.

And he really...and if you have a problem, he's not always going to tell you, "Well you should try this or you should try that." It's more like "Try to solve this," but not by doing this, he's never giving you a hint [about what to do].

A: Okay.

I: ...obviously because probably the lab is very big and he doesn't have time to take care of everyone, so everyone has to work for themselves.

A: Right.

I: And do their own thinking. But many times, I mean, I get the feeling that one week I could show the same blot one week and it's fine and the next week it would be wrong.

A: And...

I: And it's exactly the same blot. I would just take the same overhead, show it twice, two weeks in a row and he would have two different reactions....so he doesn't remember all the time, so he tends to forget. Obviously, it's normal, there's so many people working. But also at the same time it becomes excessive. There's so many meetings that you're wasting time on preparing the meeting, trying to make it look nice and presentable, you could be doing experiments.

A: Okay.

I: So what happens is we do extra hours to compensate. And that's why we're known as one of labs where people stay the longest hours, do the longest hours, work the hardest...

A: Because... why is that?

I: I would say one of the reasons because we're wasting time with meetings...too many meetings. ...ever since I started we always had these two meetings a week. One the mini one and the big general one....Yeah, so the problem is...We don't get a feeling...we don't get any help from him.... It's the main problem I think. Most people say there's no supervision. People don't get any help from him and you have to seek out your own help. And you have seek out...you have to get your own ideas. And when I go to meetings I just cross my fingers hoping that he's going to be in a good mood today and he's not going to destroy my project too much.

A: Oh, okay, yeah.

> I: If it's nicely presented usually he's okay with it....you know, if it's clear and simple, like the experiment worked well, it goes okay. But when there's complications, he doesn't give you a lot of help. And, I mean, personally I get a feeling sometimes he makes you feel stupid a little bit.... So he says "Why did you do this, why did you do this, it was useless." So if he tells you something like that it's useless...well it was a good idea at the time. ... Sometimes a week later, something he said "Don't do it" [about], you don't do it and the next week he says "Why didn't you do it". So, well, "You told me not to do it." [23]

His main complaint, echoed by several other new students, is that, although the investigator comments on his results each week, he usually does not give him specific ideas about what to do next, and that he does not necessarily remember earlier comments. As a result, the student often experiences these encounters not as supervision, but as a negative judgment on his work. There was also a frustration that the investigator discusses results of their work, but not the science, and how what they are doing fits into the larger picture.

If lab meetings were seen as a form of pressure to have something done to present, then in this lab there was more pressure since students had to present their results weekly as well as at intervals of several weeks. But for newer students in the lab, the format of this lab meeting itself, a formal presentation to the group, created stress on its own. As the student above notes, having to present their weekly results in an oral presentation before the group is a time consuming process. In smaller labs, on-going results are discussed informally as they are produced at the bench or around a table at a lab meeting.

In addition, the use of a presentation format to discuss weekly results also created confusion about the purpose of the presentation for the newer students. In the comments below, a more experienced Masters student who had been in the lab for twice as long as the student above, and who had had some success, explains why he doesn't like having to make formal presentations each week:

> I: I don't like...the reason I don't like [weekly presentations at] lab meetings is because you're presenting pieces of data. What I like is to have the whole picture and present it as one.
>
> A: ... And the disadvantage of presenting pieces?
>
> I: For me it's not gratifying....and also, like, if I present pieces, I don't have enough time myself to prepare an argument if [the investigator] asks me a question. So he likes to ask me shotgun questions.
>
> A: Right ...tell me [about] something you would have run? A western?
>
> I: Like a western...
>
> A: So there you've got your western up there and he asks you something...
>
> I: But it's a question that is not related to the western, but it's a question like about how does this fit into the whole picture...I kind of know the whole picture but I don't know how to explain it....I'd rather have a lab meeting [presentation], like, once every two months or three months, so now I can present a decent chunk of data.

A: I see. At least several pieces. But this way you're presenting every week.

I: So every week you go and then...even the things that did not work...

A: I can sort of see now. If you were left [to present] up to every two or three months, you wouldn't present anything that didn't work. You would present only (laughing)

I: Yeah, only the things like that are presentable. [24]

Despite the fact that the investigator is holding these smaller group meetings as a means of discussing on-going results, for newer students having to use a presentation format creates a great deal of stress since they associate presentations with presenting about things that "worked." Senior trainees specifically noted this attitude in the junior students, and their reluctance to present their weekly results if they didn't have anything that had worked:

A: I've talked to younger students who say that basically now, you know, I have so many lab meetings that I just...I'm working to the lab meetings all the time and I barely have anything...I can't get anything done...

I: See, that's because they say that they have to show data all the time. But they should show crap as well. Because the crap might not be crap, first of all, and if it's a technical problem they should ask for help as well.

A: Someone else might be able to see.

I: Absolutely..."I tried to make this construct...I had this sequence and I had this...I don't understand why I have that." And you think "Oh, I'll just redo it" but then you run the risk of having the same thing happen again.

A: So maybe you should ask someone before you get there.

I: Okay. Or another example...Alexa had these fuzzy bands on her western when she never had that before. So she just didn't present it. I said can you describe what you did and what you've got. And I told her it's probably because of the detergent you had in your buffer that you don't have usually. You know that's the...I've done the mistake before...

A: And got fuzzy bands.

I: Yeah, so she probably has the same problem. So she's going to try … that's crap that you bring to that meeting…[25]

Use of a presentation format confounded this exchange for newer students since there was an assumption that presentations involved presenting their successes, and were the basis on which their progress and competence would be evaluated.

The combination of the formality of presentation in front of the group on a weekly basis with a lack of success in getting results, and the sense in some cases that they didn't get enough supervision from the investigator as well as his distance from them seemed to create the impression, echoed in most Master's students interviews, that he didn't know what they were doing. This comes out in the attitude of the Master's student being discussed by a postdoc in this lab:

I: …Every lab meeting, she just stands and says what she did….I would tell her "Please present your figures because you are making [the investigator] frustrated.

You don't present data to him, you just talk to him, he doesn't know what you are doing. You need to present slides." She says...she always says to me "He has no idea what I'm doing and he doesn't care, so why am I going to bother." And I said to her, you are absolutely wrong. She says "No I'm not, I know." I go ..."Well, you're absolutely wrong." But she refused to believe me. [26]

B. Differentiation in supervision in this lab

Unlike the first student above who was working most closely with someone else in the lab who had been in the lab only one year longer than he had, some new students in this lab had actually received supervision at the bench from a senior student or a postdoc. A Master's student in that lab that had chosen a project (from a choice of two) which meant she would work with one of the senior PhD students explains that she ended up being very closely supervised by this senior student during her first two years in the lab:

I: We don't get supervision [from the investigator], I got amazing supervision...Rita was there following day by day. She knew exactly what I was doing. I remember...it was actually really funny but I think it was the second week I was there...I left...I hadn't gone to the bathroom all day (laughing) and I think at like 4:00 in the afternoon I went...like took off for a split second ... and come back and Rita was looking for me. Just to tell...not that, she wasn't mad at me, like "Where were you...but...

A: No, but in that time she had...

I: She had noticed that I was gone....She knew what results I was getting on what day. She understood everything.

A: How did it go? Did she make little comments that would be enough to sort of like...I guess in their absence you'd just be wondering, or keep doing the same thing over and over, without any change...

I: Yeah, and she would just look at the results and because she knew what I had done, she'd understand why it didn't work. "You forgot to do this didn't you." [27]

Students in many large labs did not have bench level supervisors because the investigator does not assign students to work with other lab members, as in two of the large labs I was in. But even in this lab, until recently, only some new students had access to the support of a senior trainee. Another Master's student, now assigned to work with a senior student named Mark, who actually began in the lab as a "research student" prior to graduate studies and received a lot of supervision from two other senior trainees, Rita and Leo (a postdoc), explains that he considers himself "fortunate" compared to other Masters students in the lab:

I: So, and Mark also had a lot of confidence in me ... And also previously...previous to that I had Rita and Leo...and they were constantly supporting me back then. But now that...

A: What do you mean by supporting you?

I: So they would give me tips, like...I have to say that I was quite fortunate compared to other Master's students right, because I had a lot of support from Rita and Leo and Mark, so they are three senior students who has the most confidence - well who [the investigator] has the most confidence. So they were always giving me tips on how I would be a better scientist and better...

A: What do you mean? With specific new techniques or how to...

I: Techniques or how I think...also...because [the professor] has a huge lab as you know and he cannot just go individually and supervise.

A: Right.

I: So it was the job of the senior students to go...it was like a hierarchy. So at that time it was quite...it was quite picky because these senior students would not help basically people that they don't like or (laughing)...But I was fortunate that they really liked me, so all three really helped me a lot.[28]

Compared to having comments on your results once a week from the investigator, this student describes his supervision during his hours in the lab (normally starting in the afternoon) from these senior members of the lab as "constant" at the bench level, and the reason he was able to become technically "independent" a lot faster:

I: ... for example, like Mark comes in early and he goes home...back then, he would go home like 6:00, but Leo and Rita would come in like 4:00 or 5:00pm and they would...

A: Yeah, stay late, I already heard this, yes.

I: So I would come in and I would get supervision...constant supervision from Mark and then Mark goes off and I would get from Rita and Leo.

A: Okay.

I: Yeah, so there was a constant help. So in terms of mastering techniques it was quite good for me. So if one were to become independent after one year in usual cases, I was independent by the time I spent six months in this lab.

A: Okay, because of this kind of help.

I: Yeah. [29]

However, trainees are by definition temporary members of a lab. Senior students and postdocs are quite likely to leave the lab well before the newer student does. A Master's student describes her fear when she found out about the senior student's upcoming departure from the lab to do a postdoc elsewhere:

I: ...I was very scared. When I knew she was leaving I was freaking out. But no, I was okay. I mean the fact that I didn't have someone between communicating with [the investigator] and that was a big change. Because he doesn't...he communicates to his top students, his seniors, and he doesn't acknowledge the juniors that much which is very bad....

A: But since you had had this pretty good close supervision at first...

I: Oh absolutely. Had I not had that...and Ed told me when he started he had nothing. He was like thrown in there and he was depressed for like a year and half... [30]

For new students in the lab with this type of close supervision, the senior person also acted as an intermediary between the new student and the investigator. At the lab meetings, I observed that the investigator directed his questions about the newer students work to the senior student or postdoc that they were working with, if they were working with one, and the newer student tend to interact directly with the investigator.

C. Ideas: Where to go (experimentally) from here

But although having to communicate directly with the supervisor was a major change, the main difference for this student above when the senior student left was the change in the *nature* of supervision she got. Her experience with both types of supervision allows her to clearly articulate how the supervision by the senior student differs from that of the investigator himself:

> A: How was it...how has life in the lab been different since she left, for you?
> I: I don't have...she's not the messenger anymore between [the investigator] and I. So I had to go directly to him. I have to manage on my own a lot more, which is good and bad. Good, because I can say by that time I was independent. Bad, because I need guidance and she's not there as much. [31]

"Independence" for new students in this lab refers to having technical independence, in terms of mastering techniques. They are still not autonomous workers at that point, however, because they need guidance about how to interpret their results, and what to do next. She continues, outlining that experimental "ideas" are main type of guidance a student needs at that point:

> I: If I'm desperate I call [the senior student, who was now in a new lab]
>
> A: Okay, it's not something you do on a regular basis ...protocols not working, it's her protocol or something like that?
>
> I: Or just also ideas, she has really good ideas. She knows what's possible and what's not possible. Whereas [the investigator] doesn't because he doesn't work on the bench anymore…He doesn't understand necessarily understand what is or isn't possible, what experiments are possible or not, he just knows theory. [32]

In our discussion of her exchanges with the investigator at recent lab meetings that I attended, it becomes evident that because she too now gets direct feedback from the investigator at lab meetings, but she also contacts this former student sometimes, and gets conflicting advice. However, as suggested above, she clearly believes that ex-senior student is the expert at the experimental (bench) level:

> A: You were trying to do phosphorylation or something. You kept telling him you were trying all these...different phosphotases, and then he would say, where did you get the protocol. And I think you said it was Rita's, and then he said well...
>
> I: Yeah, he was saying where did you get this...where did you the protocol? Did you try Rita's protocol?

A: Yeah, and then you said, okay, I'll contact her, and the next week you said you did talk to her…

I: I did talk to her and she told me to do these things. He doesn't agree with what she told...that's another thing, he tells me do this, she tells me don't do this, do that. He is the boss and I have to do what he says, but I know she knows best. As far as this particular thing...'cause he's, because he's the boss, he's all over the place, right? So he doesn't understand the small things anymore. He wants to understand the big picture. Which is frustrating because sometimes he forgets. And it's understandable because he's all over the place.

A: Right, he's involved with everybody's...he knows something about everybody's project.

I: And there are so many collaborations, I mean look at the papers he got his name on...

A: Right. So there are many things going on.

I: And it's just really frustrating…[33]

Having experimental "ideas" is a stage that postdocs I interviewed described achieving at some point after they mastered techniques. One of them describes how experimental ideas drove the work in his PhD project and how, eventually, the ideas were actually his ideas:

I: It's a pathway, right…you'd get your results, and then you'd think of where else can it lead, what else does this mean, what protein can be involved, what. And it just grew, it just spread, it was simple.

A: So you were basically just designing the experiments out of the last experiments...

I: Yeah. There was never any hypothesis like, "this transcription factor regulates this gene"

A: "Let's do these 65 experiments and..."

I: To prove it all. Never that.

A: No, OK.

I: It was like, OK, [enzyme x] may be involved, how do we show that it's involved. Well, is it inhibited? So we throw a drug on it, and it inhibits it. The cells died. Cool! So it might be involved. Well, so let's use another inhibitor to see if it's specific. Yeah, it is, so that's cool. Well, let's look at different cytokines, and see what the different cytokines do it, yeah they can. Then let's try to break down, let's try to break down what's actually happening in terms of apoptosis. Phosphylserine, membrane exposure, let's measure that. Then developing these assays takes time. So. And then as the literature came out, you know, that [enzyme x] was involved in apoptosis, and that [enzyme x] regulates all these different [enzymes] like [enzyme Y], and [enzyme Z], it was "let's study [enzyme Z] and let's study some downstream effectors of it."

A: OK. So with the literature, and with how the experiments were going, it was a constant conversation with him about what was going to happen.

I: Yeah... at first. And then it more became more me focusing on it, and having ideas, and saying "We should look at this, we should look at this..." [34]

The reason for the newer student's frustration with supervision through weekly lab meetings by the investigator should now be clearer. The investigator comments on the results once weekly, but does not necessarily give the student suggestions as to what is going wrong, and specific ideas about what to do next. New students who had worked with senior members of the lab indicate that supervision from the senior student involves that student being very familiar with their work at the bench, such that they can understand where things are going wrong, and suggest specific "ideas" for how to proceed. This close supervision is similar to that provided by investigators in new and small labs, described at the beginning of the chapter. The separation of the investigator from the bench level ("he doesn't know about the small things") , and the numerous projects in the lab ("he forgets") mean that his specific comments about what to do at the technical level can be frustrating for the student.

More advanced students and postdocs thought that presenting their work in the lab meeting might generate useful technical suggestions about their work. However, they didn't necessarily expect this assistance to come from the professor, but from other experienced lab members. As they advance, key experimental ideas often come from the literature and from other researchers in the field. As several senior students and postdocs in the large labs said or implied, after many years away from the bench in large labs, especially as technologies change rapidly and as they move into using model organisms not used in the lab, their supervisors might not have any particular technical expertise in some of the areas they were working in. A senior student explains his supervisor's involvement in his work:

A: How would you say your supervisor was involved in your work?

I: Not very much in your work...like it's mostly [with] paper writing and sometimes I got a couple of [technical] suggestions but not very much on that level because I think he wasn't...he doesn't understand to this day very well the Drosophila [fly] stuff that we do....Mostly his suggestions were from molecular biology point of view.

A: So when ...you got some results and you had decided where to go next. You were presenting in lab meetings, where were the decisions about where to go next really coming from....

I: Yes. So mostly our...my suggestions came from other researchers in this field, at meetings...

A: Meaning conferences?

I: Yeah, conferences.

A: ...What kind of stuff gets presented, it's obviously not as complete as a paper.

I: No, it's posters. You can get very excellent stuff.... It varies tremendously...

35

He explains, for instance, that at one point he was doing work where he was using a technique to over-express the protein he was interested in:

I:...everybody's doing that at the time in this lab, so I did that because everybody was doing that. Then I went to a meeting and saw a very sophisticated method of where you could …have cells over-expressing protein in different tissues so that was a big break because I said "oh let's use that". We got the flies from the lab and started using that [with] every cells I could think of. I was trying this, that, this...so that work that gave me an interesting result. It's a very powerful tool. So that's what I did….

A: …with flies...your suggestions are coming from elsewhere.

I: They were coming from elsewhere….nowadays [the investigator is] very much into signaling and this is what I'm looking at. So he knows more of what I'm doing now... And now he's very curious of what I'm doing. Because I'm using this technique, RNA interference. And he's been trying to get people to work on that.

A: The knock down.

I: His knock down, yeah, for the RNA i…. Yeah, so knock down ...in fly cells it's even more easy to do. .. It's super easy to make, like if you do a transcription that's been around like decades. So you did it in a tube, you get your double RNA, yourself. You get mgs of it and what you do is you take micrograms, 20 of them approximately and you put it on your cells and they'll stop making that protein. It's really easy. So I had troubles with that at first. He gave me a couple of good suggestions. So he's not useless for that, but for the fly stuff...I had to learn everything on my own, so that was very difficult. [36]

V. Involvement with cutting edge research

Because the investigator is involved in cutting edge research, he sometimes asks students to use techniques, and as with the student above, model animals, that no one else works with in the lab. If a new student had been assigned a project involving use of a new technique not already used in the lab, even a student with bench level supervision might flounder. The effect of not getting a project to work after trying for one or two years could be devastating for a student's motivation and commitment to staying in graduate studies. One of the students who had been assigned to a senior student but who had been given two projects which involved the use of new techniques, neither of which had given her any positive results, explains how her frustration with a complete lack of success in getting results led at her decision to finish with a Master's degree. (although she wanted to leave graduate studies, the investigator wanted her to stay to do a PhD):

> A: Tell me about that. You made the decision, you say last winter.
>
> I: ...Right. I mean, I'd always thought that I was going to do a PhD, I mean that's what I wanted to do. I wanted to stay in academia... Like, I loathe science but I really, like I really really liked research, I was really like... this is what I want to do, and I mean I had done...it wasn't like I came in and didn't know what I was getting into, but I mean, I guess I didn't know what I was getting into in this lab, and I was just... I don't know, I was just so frustrated that everything I did I couldn't...I felt like I hadn't accomplished anything and it was even more frustrating when you have experience....like in my other lab was able to accomplish things and like, you know, was considered like a decent student and

> then you come here and it's like " Do it again, do it again, do it"...I'm like, it's just...it makes you feel like... So I got to a point where I was just really frustrated, and I said okay I've got to this turning point, do I want to do another five years of this, which is what it would probably take for me to get a PhD in this lab and I was like five years from now like I'm just going to be so angry and bitter .... I could already tell that it was making me into like, you know, when you're not sleeping well and you're just like so frustrated and you're not having a good time. I mean I knew that I couldn't do another five years of it so...[37]

VI. New practice in this lab: Supervision of projects by senior trainees

Senior students and postdocs in large labs usually expected to have to train other students in the use of techniques. In this lab, however, instead of asking new students to work with a student or postdoc already in the lab in order to learn techniques and get started, the investigator was now assigning the responsibility for the *supervision of projects* to senior students and postdocs. As is evident in this interview with a new postdoc in this lab, he expected to work alone on his own projects, but instead is being asked to take responsibility for projects in the lab, and as a consequence, closely supervise other lab members:

> A: So, how would you say [the professor] is involved in your work...
>
> I: In my work? ....I define my work as like, uh, I really wish it wasn't like this from the beginning, but it turned out this way, and I don't mind it, but I

think…like I wish I was just maybe doing my own thing.

A: OK.

I: But I'm not….So he wants me to be the leader of this group, the [molecule X] group, he explicitly told me that…. He says to me, I want you to be in charge of this… I want you to know what's going on with everybody in this group so me, Brian, Paul and Susan.[38]

The investigator comes to him with projects to be done, and since he can't do them himself, he delegates them to the other members of this group:

I: A few months ago, I felt he gave me the responsibility to do all these things by having these other people do them for him, or for me, or like…He wants to see me successful I think, but I can't do all the work. So he says why don't you get, give the work, this part of the work to so and so, that part of the work to so and so, and meanwhile you do that part.

It is the postdoc who is assigning these projects to the other students:

I:….they've never talked to him about what they were going to do, they've talked to me…So basically [molecule X project] came, I pass it to Susan . Another gene came, [gene A] I gave it to Brian. So I help Brian, I help Susan…

The investigator also came to him to develop use of a new technique for the lab:

I: He wants me to do it. I said no, no , no. He says "Get Paul to do it."

A: ...But does that still leave you on the hook for keeping an eye on what Paul is doing?

I: Yeah, because he's always asking me how it's working out.

He describes how he goes about this with one of the newer students in the group:

I: So like every time I find a day where ...he hasn't done anything, I say "Hey, what's going on with that? He like, "Oh yeah, I'm doing it." So you'll see him back at the bench, making ...and I'm making sure, and my responsibility, I think, is to make sure everything gets done. [39]

Only some of senior trainees in the lab were assigned new students, but each of these people had been assigned several other trainees. A Master's student assigned to one of these senior students explains, and draws a parallel between the work of this person and a principal investigator (PI):

I: So Mark, that's why he's getting a lot of ... of students. He's really good at supervising.... He's not a PI...he's not a boss yet. But if he does become a PI I totally envy his students (laughing).

A: ...oh I see ...as far as you're concerned he's functioning as a PI for his students.

I: Yeah, he's a mini PI in the lab.[40]

Of course, since he is supervising the students at the bench, he is more like an investigator with a small lab than the investigator in this large lab.

In fact, with the introduction of this new practice, the investigator in this lab can arguably be seen as attempting to re-gain the advantages associated with small labs - good training for all of the students, and close supervision of the research projects. However, since these new "supervisors" are trainees themselves, assigning them this responsibility creates additional pressures for them, and new differentiation in training and supervision in the lab. First, having this new responsibility could create new pressures for these senior trainees to stay in the lab longer. Second, although many students will get better supervision, not all students will, since senior trainees not may accept or not like the students they are assigned (and vise versa). Below a student who had very good supervision from his "mini - PI" describes the situation of another student assigned to this same senior student at the point where the senior student thought he was close to finishing his degree:

> I: But the thing is in the case of Claire, it's quite unfortunate for her, because Mark has...you know he's been around seven years he's sick and tired of this lab. So he's trying to leave. ...so he wanted to leave and then Claire came and Mark was...he thought he was finishing me off...like he was getting me independent, and getting Beth independent...Beth's pretty independent.
>
> A: Yeah.
>
> I: Claire needed a little more supervision because she didn't have previous... she didn't have the previous [lab] experience.

A: OK.

I:....So, but the thing is Claire did not get special time with Mark... the thing is like getting a good supervision from Mark it makes it a little easier for you to get independent.

A: Okay, because you get a good grounding...getting you up to speed on the techniques...

I: And how to think and...and also to answer your questions about the project. [41]

In fact, since the senior student was leaving, the investigator tried subtly to re-assign the supervision of her project to a new postdoc. However, since this is not something trainees are expected to do, and this new postdoc had already been assigned two new students, he told the investigator that although he would help her out when she needed it, he did not want to be responsible for her project.

The process of supervision of projects is a major responsibility for the senior trainee. The new postdoc above had begun to notice that it become his main activity in the lab, and as such was affecting his own work, and therefore possible future career:

I: Well...for a post doc to become a professor, you have to really focus on something in your post doc, do it on your own, get it done as quickly as you can, like within three years, and then become an assistant professor. But I spent the last few months playing the supervisory role, and I feel like everything is going forward except for my main project, which is a little bit on the back burner. It's going, slowly, but...maybe in the next year it will develop into something big...but...because now I feel like I spend a lot of time in the supervisory role, not supervisor but...like a leading role in making sure they get it done... I feel like...I think if I get good at this, and make it successful, maybe a company would be more attractive role for me, where I can play a [that kind of role] in the company. [42]

VI. Two stage training and the career progress of successful PIs

Since the institutionalized career strategy of investigators supported by competitive standard grants is to obtain multiple grants and, as a result, build larger labs, labs in the biomedical sciences range in size. As the findings of this chapter suggest, important differences in the organization of research and training between large and small labs mean small labs are probably better environments for doing graduate studies, and large labs for doing postdoctoral studies.

More specifically, as postdocs with experience in both types of labs suggested, it was better to do graduate studies in the small lab of a new professor, getting lots of motivated supervision, and to do a postdoc in a big lab where you had independent projects, more resources and the reputation of the supervisor to give you the best chance of getting out a lot of publications and being able to take a project from the lab. One of these postdocs explains:

I: ...the thing that I've always thought, and what I have been told, is that you should get into the lab of a rising star, who's just starting out, that way, it can be you and them. And you know, you can learn all the good stuff, all the techniques, all the ...'Cause they'll be right in there with you doing the experiments....the rule of thumb is that you want to do your PhD, or your graduate work with a small lab where you can get one-on-one interaction, you know, you can work with a supervisor who's hungry to get his lab going, and to be successful. And you get a lot of experience that way, so that's what I aimed for. It was a small lab, he was starting out... he was getting grants...He had two other students, and a technician...

A: Who had given you that advice....

I: Oh, ... well, my fourth year research project with the inorganic ... inorganic chemistry guy - said the same thing, "You want to go to a little lab for your PhD, and an empire lab for your postdoc"...You want to get the hands-on experience with the ...

A: With the small lab.

I: Yep... And that's why the PhD students that are in [this large lab] are getting kind of killed. I don't know if any of them have mentioned to you how frustrating it's been for them. [43]

Many graduate students in large labs do not get a bench level supervisor, including those in the large lab he was actually doing his postdoc in. And if students have a bench level supervisor in a large lab, it does not necessarily mean that they will get good training. He continues:

I: Yeah, and they will never see that PI....you might get good training in that lab, and there'll be lots of money... but ....if you go to [a large lab] you might get stuck with a second year postdoc who has bad habits....You don't know who it will be.

Another postdoc who had been in a small lab during her PhD and had close supervision from the principal investigator explains how although she felt that she had been well trained as a student in that lab, she didn't feel that getting good training was possible now that that the lab had grown:

I: ... in the lab, at the beginning, when I came, there was a great core and we helped each other a lot. [The investigator] was involved a lot... What's happened now, [the head technician] was telling me, said "You know, the lab's so big, [the investigator] doesn't have a handle on the projects. Everybody's coming to me for definite direction," but Greg has [been] moved to another area. [Her lab] exploded basically and moved into other areas and there is no one directing the projects, these kids are floundering. ...So it's become like [this large lab] or [another large lab], where you don't go to that lab unless you know what you are doing, and you can drive your own project.

A: You've got considerable research experience before

I: Yeah, I would never recommend now for someone who was in my situation [a beginning graduate student, with little lab experience], it's moved now, she's up in the hierarchy of the high level PIs with big productive labs. And, you won't get

> what you need, as a, as a student.[44]

However, since these relatively new developments are not widely understood, many beginning graduate students are not aware of these considerations. This was obviously not something understood by new graduate students who had chosen to come to the large labs I studied. Having this strategy at the beginning of graduate school would demand a lot of understanding of the relatively new imperatives for academic careers in this area and/or advice about what to do from experienced researchers. It was evident that many students had not received advice from experienced researchers when they were choosing their labs.

In fact, students that had chosen the large labs I studied on their own (as opposed to being recruited or choosing the lab because they had already had experience in the lab) had in many cases made this decision based on the reputation of the investigator as a top scientist. One Master's student explains that her decision to come to the large lab I was studying was made on the basis of her supervisor's reputation and record of publishing in top journals, thinking that this was the best strategy for a student who wanted to pursue an academic career:

> I: Well, the lab that I decided not to go to [one in which she had worked for a period when she was an undergrad]...it's a pretty big lab and ... when I tell people that...they're like "Oh my God, why did you leave that lab". So it kind of made me think that if I was not going to go with that lab, I had to go to a lab that was of that calibre or better.

A: Okay.

I: I mean, it's not the right reason to necessarily choose your lab. But I know that [now].... I knew that [the investigator in this lab] is very well respected, he publishes in extremely good journals...When I told my other supervisor, I'm like, "Okay I've decided I'm going to go to ... [this investigator's lab]" and no one said "Oh are you sure" or (laughing). He was like "Oh, that's a very good lab". You know....I think it was more that kind of idea. It was like my other supervisors, it's the same thing ....someone who's very well respected, publishes well and when I was...I mean I always thought I was going to do my PhD - I was setting myself up for a career in science so I wanted to go to...I wanted to go someplace where I was going to publish in like top journals. I was going to come out of the lab with a name attached to me that was going to take me places.[45]

Perhaps because professors with reputations as top scientists tend to have large labs in a competitive granting environment, and because all of the labs she had worked in had been large labs, the labs she considered before beginning graduate studies were all larger labs. But she realizes now the type of relationship typical with the supervisor in a small lab might have been what she needed as a graduate student:

I: ...I guess I never considered going to a small lab because my other experience had been in a larger lab and I don't necessarily like to have...don't think I would like to have a supervisor that was like looking right over and like have you done this, what are you doing, and like sitting right next to me. And the idea of a professor working on the bench next to you, at that time, seemed like it would be a bit too much for me. ...But like, even in Japan [where she worked in a lab as an exchange student] , I mean, none of the labs [were small]. And in the other lab [in Canada] the professor didn't work in the lab so...I mean, that wasn't what I was used to. And all the labs that I actually interviewed with were larger size labs and I didn't even consider working in a smaller lab. But actually now I don't know if that was maybe the best decision. [46]

Of course, she was right about the environment in a large successful lab being advantageous for starting an academic career, but, like many other students and postdocs I interviewed, did not clearly understand when she started graduate studies that being considered for academic positions would mean that she would have to do at least one, and maybe two, postdoctoral appointments. Instead, many students initially thought that getting a PhD would be the qualification needed. This is understandable, given that a postdoc is not an official degree, and that these changes in the expected requirements for an academic career are relatively recent. A postdoc describes his initial assumptions about the value of a PhD and the structure of training, and his frustration when he realized that the PhD was not enough:

I: And that...when I finished I would have such a high level of training that I could get a very prestigious kind of job or something well-paying.

A: Something what?

I: Better-paying...prestigious in the sense that...you're not going to go back and start doing dirty work, like you can go from PhD, quickly from a post doc to an assistant professor or something like, bang, bang, you know, instead of...struggling. You realize towards the end of grad school that, there's just a - it's an uphill battle, like...like, hey, you did your PhD, well now you got...you wanna become a professor, automatically you gotta do a post doc. After your post doc, maybe a second post doc. And then become an assistant professor...oh gawd...

A: So when you started grad school you didn't necessarily...

I: I wasn't informed enough I think, because I...I just liked being in grad school.[47]

Summary and Discussion

The findings show that in the lab of a newly hired investigator, a master-apprentice relationship between scientist and students might be found, where the investigator is often actively working on his projects at the bench, training the graduate students and technicians in the use of techniques and supervising them at the bench. However, as the lab of an investigator grows in size, this relationship changes.

In small labs, after the investigator trains a few lab members, the findings indicate that training of the further new students in the use of techniques is delegated to these lab members. However, the data suggest that investigators with small labs still closely supervise these projects, typically examining the results in detail, and often giving specific instructions about what to do next. While the investigator must delegate the projects in order to get them done, the findings suggest that as new investigators they delegate only the experimental work, not the direction of project, and closely oversee the work on those projects.

In large labs, in contrast, the investigator usually no longer does experimental work himself, and has moved away from the bench completely. As in small labs, the training of students in the use of techniques is delegated to lab members. *The findings indicate that the important difference in practice between small and large labs supported by standard grants is that the investigator in a large lab typically no longer closely supervises the projects.* Instead, students in large labs tend to work much more independently, instead seeking out guidance on experimental matters from other lab members, and as they advance, from the literature and from researchers outside the lab.

Changes that encouraged having multiple grants to ensure better chances to remain funded also created a new possibility: the opportunity for investigators to build competitive advantage in science through "critical mass." [48] The principal investigator of one of the large labs I studied, for instance, had moved to Canada in the early 1990s specifically for this reason. In Britain, the director of his institute decided when and whether he could have more labour in his lab, limiting his ability to compete. In Canada, his own success in getting grants allowed him to hire more lab members. [49] Similarly, when investigators have several grants, if they lose one, they can continue to do the project by distributing the research expenses onto the remaining grants. In his lab, for instance, materials and services were charged to one grant for three months, then to another, etc. If he lost a grant, the charges would be made to one less grant. Although further evidence would be necessary to show it, I suggest that the investigator in a large lab is probably relying more on "critical mass" with respect to productivity, that is, some of the projects will result in papers, some won't, but with more trainees and projects, he or she will have still have productivity.

In the episode with changing supervision arrangements in the large lab I studied, the investigator attempts to re-introduce the close supervision associated with small labs into the lab with two new practices. The investigator tries both to supervise the projects more closely himself at lab meetings, as well as assign senior graduate students and postdocs in the lab responsibility for supervision of projects. However, the findings show that it might be difficult for an investigator with a large lab who has left the bench to supervise projects closely. Effective close supervision may demand more familiarity with the student and what he or she is doing at the bench level, as the greater success with delegating supervision of students and projects to senior trainees suggests. Although this second arrangement results in better supervision for some students, it does not guarantee it to all students. Importantly, delegating this supervision to senior trainees represents a major increase in work and responsibility for a person who is officially classified as a "trainee." Given that this work will tend to pull the trainee away from work on his or her own projects, it may put the trainee at a disadvantage compared to those in other labs, in the sense that it may derail or postpone a postdocs first-author publications, and lengthen the degree in the case of senior students. It could potentially actually reduce the productivity of the lab since senior trainees are likely to be the most productive members of the lab.

The experiences and reactions of trainees to the changing and differentiated supervision in this lab begin to reveal details about the stages that a student passes through as a graduate student from beginning student to autonomous worker in the lab, and how the organization in large labs may make it difficult for graduate students in large labs to get good training. New students need to master the use of techniques, and findings indicate that getting training at the bench from an experienced researcher helps. After they achieve mastery of techniques, they still benefit from guidance from more experienced lab members in order to interpret their results and to give them ideas about what experimental work to do next. At some point, the successful student passes from this stage to having his or her own experimental "ideas" and becomes an autonomous worker in the lab.

The findings suggest that as labs grow the organization of research and training in them changes, and more generally, that there is differentiation in the organization of research and training across labs. As a result, the needs of new graduate students are likely to be met in a small lab of a new investigator, and those of postdocs in large labs. More independence for the trainees and a critical mass strategy in large labs means that a new student may not be able to get good training in a large lab. For postdocs, however, this independence, combined with access to greater resources and reputation of an investigator with a large lab mean that will probably be a better environment to achieve their goals of developing a good publication record. However, in Canada at least, since these developments are relatively recent, they are not widely understood. Without advice from professors or other experienced researchers, most beginning graduate students will not be aware of these considerations.

Conclusion

In conclusion, evidence presented in this chapter demonstrates another aspect of the influence of external funding and its institutional accommodation on the organization of research and training in the biomedical sciences in Canada. The career strategy necessitated by dependence of biomedical scientists in these leading universities on external funding means that he or she does not have a choice about applying for multiple grants and therefore building a larger lab group if successful. The main argument of this chapter is that that as the lab group of an investigator supported by competitive standard grants grows, key changes in the organizational practices tend to occur, such that there are usually significant differences the organization of research and training between small and large labs.

Evidence suggests that the most important change in organizational practice that occurs as the lab grows is that the investigator no longer closely supervises the experimental work on the projects being done by lab members. Instead, trainees work much more independently from day-to-day, seeking out guidance from more experienced members of the lab for technical support on their projects. Lab meetings in large labs typically provide the main vehicle for communication of results to the investigator at intervals of several weeks or months, and between lab members working at the bench about projects being done and techniques being used. Generally, there is more supervision in small labs than in large labs, but the actual amount of supervision in any lab will depend on the practices used in that lab. The findings suggest that it may be very difficult, once the investigator has moved away from the bench, as investigators in large labs usually have, to closely supervise projects in the lab.

The intense supervision of projects reported for new labs, which the data suggest can be similar for both graduate students and technicians, is likely related to the importance of these projects to the development of the new investigators own career, due to their dependence on external funding. As outlined in Chapter 4, institutional accommodation of external funding by the medical faculties of these universities means that getting tenure and continuing to do research (as well as being able to get more than one grant) are directly related to the investigator being able to maintain external funding. Since renewing their grants will depend on competitive productivity, the new investigator has a large vested interest in the direction of each of the projects that they have delegated.

Since this study finds as others have (Delamont and Atkinson, 2001) that new graduate students benefit from close supervision in order to progress through key stages of training, the evidence suggests that new graduate students may not be able to get good training in large labs. Since these differences in the organization of research and training between large labs are not well understood, beginning graduate students are not aware of these considerations.

The evidence suggesting that significant differences exist in the organization of research and training between small and large labs is an important contribution to the existing literature. Existing studies have documented disciplinary differences (Knorr-Cetina, 1999) as well as differences between in disciplinary culture (Traweek, 1988). Recent studies of the organization of work in large labs in the biomedical labs (Owen-Smith, 2001) argue that their findings are probably generalizable. Similarly, recent studies of the socialization of graduate students argue that certain features of training, such as training of new students by postdocs are features of training in the life sciences. Findings of this study show that, at least in Canada in the biomedical sciences, where investigators are supported by standard grants, supervision differs depending on the size of the lab. Having postdocs supervise newer students, for instance, would generally only occur in a larger lab with the resources to attract and support postdocs.

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[1] Interview with retired professor, August 15, 2002
[2] Interview with postdoc, September 26, 2003. He emphasizes that he knew "how to work in a lab," because this is a key issue when you have to start your project in the lab right away. His undergraduate curriculum did not, as most in this area don't, give him much technical experience. However, he had spent two work terms at a pharmaceutical company lab as part of a co-op undergraduate program (includes both study terms and work terms arranged in industry), and then another year and a half in that same lab as a technician when he graduated.
[3] Interview with postdoc, September 26, 2003
[4] Interview with postdoc, September 22, 2003
[5] Interview with postdoc, September 22, 2003
[6] Interview with postdoc, September 22, 2003
[7] Interview with postdoc, December 12, 2002
[8] Interview with postdoc, December 12, 2002
[9] Interview with older professor, October 3, 2002
[10] Interview with postdoc, December 12, 2002
[11] Interview with postdoc,
[12] Interview with postdoc, December 12, 2002. His supervisor was recruited herself to a US university about a year and a half before he finished his degree, and took a large part of her lab group with her.
[13] Interview with Master's student, October 23, 2002
[14] Interview with former doctoral student, October 16, 2002
[15] Interview with Master's student, October. 23, 2002
[16] Interview with doctoral student December 6, 2002
[17] Interview with postdoc, December 5, 2002
[18] Interview with Master's student, November 1, 2002
[19] Interview with doctoral student, December 12, 2002
[20] Interview with postdoc September 22, 2003
[21] Interview with postdoc, September 22, 2003
[22] Interview with postdoc, September 22, 2003

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[23] Interview with doctoral student, December 6, 2002
[24] Interview with Masters student, October 29, 2002
[25] Interview with doctoral student, December 6, 2002
[26] Interview with postdoc, December 12, 2002
[27] Interview with Masters student, November 1, 2002
[28] Interview with Masters student, October 29, 2002
[29] Interview with Masters student, October 29, 2002
[30] Interview with Masters student, October 29, 2002
[31] Interview with Masters student, November 1, 2002
[32] Interview with Masters student, November 1, 2002
[33] Interview with Masters student, November 1, 2002
[34] Interview with Masters student, November 1, 2002
[35] Interview with doctoral student, December 6, 2002
[36] Interview with doctoral student, December 6, 2002
[37] Interview with Masters student, October 24, 2002
[38] Interview with postdoc, December 12, 2002
[39] Interview with postdoc, December 12, 2002
[40] Interview with Masters student, October 24, 2002
[41] Interview with Masters student, October 29, 2002
[42] Interview with postdoc, December 12, 2002
[43] Interview with postdoc, September 26, 2003
[44] Interview with postdoc, September 18, 2002
[45] Interview with Masters student, October 24, 2002
[46] Interview with Masters student, October 24, 2002
[47] Interview with postdoc, December 12, 2002
[48] The idea that competitive advantage in science could be obtained with large group of trainees has existed since Julius Leibig, a chemist, developed the first 'large scale' university research laboratory in a German university in the early 19th century. By the 1840s, Liebig had a critical mass of chemistry students (Clark 1995, 24-5). According to Holmes: "Liebig's command of so large a group of advanced students to whom he could give experimental projects useful to both their training and to his interests enabled him to exploit new research openings with a swiftness that made it hard for chemists operating alone, or with only a few students, to compete with him." (cited in Clark 1995, 25).

[49] Interview with older professor, October 6, 2003

Chapter 7

Conclusions

I. Main findings and conclusions

The purpose of this study was to investigate the relationship between the current organization of research and postgraduate training in labs in the biomedical sciences in leading research universities in Canada, how it has changed since the 1960s, and how both are related to external influences, particularly research funding.

The findings suggest that there has been a transformation in the social organization of research and training over the last few decades. More specifically, the processes and mechanisms associated with these changes indicate that they are due primarily to the changes in practice made by biomedical scientists in response to an increase in competition for federal funding beginning in the 1980s, after institutional accommodation of it by their universities made their careers dependent on it.

There has been an assumption by many researchers in the social studies of science that the social organization of academic science has not changed much in the last century. Those scholars that have been concerned with change in the organization of academic science have primarily been looking for the effects associated with the commercialization of research and the involvement of faculty with industry.

Evidence in this thesis, however, suggests that the social organization of research and training in the biomedical sciences, as well as the career of an academic scientist, in leading universities in Canada has not been stable since the 1960s, but have been transformed, constructed over time in interaction with a dynamic institutional context. The findings suggest that the main influence is the extensive research funding provided by the federal government and its institutional integration by leading research universities. On the basis of the findings in this thesis, it is suggested that an exclusive focus on changes brought about by the increasing commercialization of research and increasing links of faculty to industry will obscure fundamental changes in the social organization of the biomedical sciences in Canada over the last few decades and the processes by which they occurred.

A. Theory of transformation in the social organization of biomedical research in universities in Canada since the 1960s

Evidence found in this study suggests a theory of transformation in the social organization of labs in the biomedical sciences in leading universities in Canada since the 1960s. The main argument is that dependence of an academic career on competitive federal grants led investigators to change their work and organizing practices, which in turn led to a transformation of the organization of research and training in the biomedical sciences in these universities, through the following steps:

1) availability of extensive federal funding for biomedical research in universities with the creation of a federal granting agency in 1960,

2) institutional integration of external funding by universities, such that biomedical scientists' careers became dependent on it,

3) an increase in competition for external funding from the primary source, the federal granting agency,

4) two new practices on the part of biomedical scientists to try to ensure they had the best chance of maintaining competitive external funding, given its allowable expenses

a) applying for multiple grants, and

b) primarily recruiting trainees (graduate students and postdocs) instead of technicians

5) institutionalization of several practices which incorporate trainees into the production of faculty research and publication

6) institutionalization of a career strategy where the scientist attempts to build a larger lab, which results in

a) changes in the organization of research and training in individual labs as the size of the lab groups grows,

b) differentiation in the organization of research and training between small and large labs

By the 1960s, a system of extensive federal funding for the research of biomedical faculty in Canadian universities existed, following the creation in 1960 of the federal granting agency for biomedical research, the Medical Research Council (later the Canadian Institutes for Health Research). The federal agency has been the primary source of research funding for biomedical scientists in universities and their associated hospitals and research institutes, primarily through one type of grant, the operating grant, which historically has been awarded to an individual professor, has a short duration, specific regulations and allowable expenses, and is renewable.

Evidence found in this study suggests that the small size of most biomedical labs in the 1960s and 1970s was related to the structure of research funding. By the late 1970s, tenured faculty were typically supported by one grant from the Medical Research Council. Although these grants were initially won on a competitive basis, they were renewed on a much less competitive basis. Once a professor had a grant, he or she was reasonably assured of continued funding for his research through grant renewals, as long as he or she was publishing at an acceptable rate of one or two papers a year. Under these conditions, the academic lab typically had a small group, made up of the investigator (funded scientist) with a few lab members paid from the grant, usually a technician and maybe a graduate student or two.

By the late 1970s, institutional accommodation of external funding by the medical faculties of these universities had occurred. One aspect of this institutional accommodation was the disappearance of university-based sources of research funding and technical personnel for research, such that the federal operating grant to the individual faculty member became the primary source of funding (and personnel) for most biomedical scientists. Secondly, medical faculties in these universities had incorporated federal funding into their institutional decision making, such that being in possession of external funding became one of the main criteria for receiving tenure and continuing to direct a lab. In other words, institutional accommodation of external funding meant that, unlike in the social sciences and the arts, being awarded and maintaining external funding had become a requirement for an academic career in the biomedical sciences.

Evidence suggests that the key to understanding the transformation of research and post graduate training in the biomedical sciences in leading universities in Canada are changes in the requirements for an academic career that occurred in the 1970s and 1980s. The key mechanism creating change was 1) institutional accommodation of external funding making a requirement for an academic career, followed by 2) an increase in competition for federal funding, the primary source of external funding, beginning in the 1980s, which meant that professors had a significant chance of losing their grant. Without external funding an assistant professor would not be given tenure, and a tenured professor would maintain a faculty position, but not an identity as someone who directed research and supervised graduate students. These changes meant therefore that careers of biomedical faculty were now dependent on successfully competing for external funding on an ongoing basis.

The main argument is that dependence of an academic career on maintaining competitive federal funding led investigators to change their work and organizing practices, resulting in a transformation of the organization of research and training in the biomedical sciences in these universities. Given the serious consequences associated with losing their grant, biomedical scientists responded to an increase in the competition for grants with two new practices in order to have a better chance to stay funded, 1) applying for multiple grants, instead of relying on the renewal of one grant, and 2) recruiting trainees, instead of technicians as in the past, to work as research assistants. Since the standard operating grant allows investigators to hire only graduate students, postdocs and technicians, but has always stipulated that graduate students are to be paid an annual stipend which is approximately half the rate of the salary which is paid to technicians, after funding became more competitive, investigators began to recruit trainees instead of technicians to try to maximize productivity on these grants.

These two practices led to significant changes in the social organization of research work in biomedical labs in these universities. Applying for multiple grants and their renewals in a competitive grant environment meant that a professor had to spend more time on grant-related work than in the past. In addition, if the investigator obtained multiple grants and the lab group grew in size as he or she hired more people in order to get the projects done, the investigator also had to spend more time managing a larger lab. The findings of the study suggest therefore that a plausible explanation for the fundamental change in the basic structure of university-based research in the biomedical sciences in Canada over the last few decades, the shift from the situation in the 1960s and 1970s where a typical lab had a small group, to the current situation where many labs have many more members, most of which are graduate students and postdocs, can be found in the initial changes in practices made by biomedical scientists after the grant support on which their careers were dependent became fully competitive.

B. Interdependence of research, training and academic careers in the biomedical sciences with competitive grant support

The findings also suggest that these new practices by investigators created conditions which led to further changes in practice, transforming the organization of research, training and careers, so that they are currently highly interdependent.

The dependence of investigators on trainees is associated with the incorporation of graduate students and postdocs into the production of faculty research, such that the scientific productivity of the investigator becomes dependent on them. This involves several institutionalized practices: 1) delegation of the experimental work on their projects to trainees as the trainees' main project, 2) sharing scientific credit with trainees in the form of co-authorships on research papers, 3) informal integration of scientific credit into the structure of training, such that evaluation for graduation and academic positions is on the basis of publication with their supervisors. This delegation-credit-evaluation system, which findings suggest was not institutionalized before investigators became dependent on competitive funding, can be seen as aligning the goals of the trainee with those of the investigator, given the imperatives of competitive funding, with a change in the use of the reward system in science.

The argument here is that when investigators are dependent on competitive federal grants, the necessity that investigators obtain multiple grants and therefore have multiple projects means that they cannot do all of the work themselves. The projects are delegated, and, given the allowances for expenses on standard operating grants, the projects are primarily delegated to trainees. The extension of credit to trainees and requiring them to publish papers to reach their own goals of graduating or obtaining a position can be seen, in turn, as an informal requirement which motivates the trainee to produce publishable results out of self- interest, but with benefits which also accrue to the investigator, since the authorship credits are shared. These findings therefore suggest that, in addition to being cheaper and generally more motivated than technicians, graduate students' and postdocs' trainee status provided an additional resource for investigators dependent on their work for indirectly controlling and managing work in the lab, perhaps especially relevant given that labs were often larger than in the past, that did not exist with technicians. Further, since it is the investigator that actually decides what will be published, who will get authorship credit, as well as when and to what journals the co-authored papers will be submitted, these practices have also led to a means of more direct control. Trainee status also provides another potential organizational resource to investigators because although trainees' projects are now usually incorporated into the investigators' research, they can obtain scholarships and fellowships from various institutions, so that the investigator might not have to provide them with funding or part of their funding for considerable periods of time.

Dependence on competitive grants has transformed academic careers in the biomedical sciences. As outlined earlier, loss of external funding before tenure can end a biomedical scientist's academic career altogether. However, the strategy of obtaining multiple grants in order to have a better chance to stay funded means that biomedical scientists must also build larger lab groups, and then delegate the experimental work on the projects in order to get them done. This means that an investigator successful in getting several grants currently does not have the option of remaining an active experimental scientist who carries out the work on his projects at the bench him or herself.

As the evidence presented in this thesis suggests, as the lab grows, the new investigator begins a process of delegating the work he or she initially does himself at the bench. The new investigator with a small lab tends to delegate just the carrying out of the experimental work, not the direction of the experimental work, often examining the on-going results in detail and giving specific directions for what experimental work will be done next. In large labs, in contrast, the trainees work much more independently, and are often responsible for direction of their projects too. Although in both small and large labs the investigator is usually dependent on the experimental work of trainees, the differences in the practices of investigators with small and large labs suggest that investigators with one or two grants and those with many grants are employing different strategies for organizing research. It is likely that the dependence of the investigator's career on the productivity of those first projects leads to close supervision of those projects in new and small labs. For an investigator with a large lab, in contrast, the greater demands of directing a large lab, including directing research in the epistemic sense of choosing and designing the multiple projects, overseeing the writing and publishing of the papers, as well as competing for and managing the funding, equipment and the lab members, mean that an investigator, at first a skilled experimentalist with current techniques, has little choice but to progressively separate himself from experimental work, if he or she is successful in getting multiple grants, and to allow the student or postdoc to work more independently. Although further research would be required to investigate this issue, it is possible that investigators who have developed large labs are using a critical mass strategy with respect to productivity, relying on the probability that some projects will result in publications, some won't, but with more trainees and projects, the investigator should still have productivity.

An argument that the investigator with a large lab supported by competitive grants has little choice but to cede not just experimental work at the bench, but close supervision of the students and direction of the experimental work on the projects, is supported by the evidence from the episode with changing supervision arrangements in one of large labs I studied, which indicates that the distance of the investigator from the bench may make it difficult (and as a result, counterproductive) for the investigator with a large lab to attempt to supervise the experimental work on the projects.

The findings indicate that the direction of their experimental work at the bench level seems to better provide new students with what they need in terms of training in the use of techniques and the basis on which they will eventually be able to develop experimental "ideas" of their own. If the investigator with a large lab wants to do this, however, under the constraints associated with support from standard competitive grants, which allow the investigator to hire only trainees and technicians, delegation of this responsibility means having to delegate it to senior trainees. Since this can be a major responsibility involving a lot of work, it will change the content of the senior trainees' work significantly, and in the case of a postdoc, may confound his or her goal of obtaining a position as an academic scientist if it interferes with his or her own productivity.

In sum, the career strategy necessitated by dependence on competitive grants means that the professor in the biomedical sciences, at first a skilled experimentalist, is obligated to give up not only bench work, but close supervision of students and their projects as his or her lab group increases in size. As a result, the organization of research and training typically differs considerably between small and large labs. Because the separation of the investigator from the bench in a large lab means that new students will often be much more responsible for the direction of the experimental work on their own project, new graduate students in large labs may experience a less favourable training environment than their fellow students in the lab of a new investigator. However, these differences in organization between large and small labs do not seem not widely understood, especially by beginning graduate students.

C. Dependence on competitive grants transforms the academic lab into an organization, and the occupation of academic scientist in the biomedical sciences

Work can be seen as structured in two principal ways, either on an organizational basis or an occupational basis; work structured on an organizational basis involves "work tasks being designed by some people, who then recruit, pay, coordinate and control the efforts of others to carry out those tasks" (Watson, 1987: 169). In the past, work in science was seen as structured on an occupational basis, with a scientist working at the bench and training graduate students in a master-apprentice relationship.

The findings of this study suggest that dependence on competitive grants in the biomedical sciences necessitates a career strategy of obtaining multiple grants, which in turn necessitates a division of labour on the projects, where the investigator chooses and designs the projects and delegates the experimental and technical work on them to others in the lab. The evidence therefore indicates that the academic lab, under a system of competitive standard grant support, has to operate on an organizational basis, and that the lab is an organization. [1]

In Canada, as in the U.S., assistant professors in the biomedical sciences typically develop their own research programs and establish their own labs. The structure of the federal grant system means that the resources for research have historically been channeled primarily through individual investigators. Institutional accommodation of this funding means not only that such funding is available, but it has become a requirement for an academic career as a biomedical scientist. After an increase in the competition for federal grants in the 1980s, this dependence resulted in the possibility that labs could disappear. In other words, academic labs in these universities became organizations potentially subject to "creative destruction," [2] as are firms, as a result of a competitive process based on productivity, organized by the federal granting agency and accommodated by the medical faculties of these universities.

The career strategy for investigators involves building a larger lab group, and as the findings show, this changes the tasks of the investigator as he moves out of work at the bench to become a kind of research manager. If occupations are associated with groupings of tasks, then these findings suggest that dependence on competitive grants means not only that work must be structured on an organizational basis in academic labs, but this dependence has transformed the occupation of academic scientist. People working in academic labs supported by standard grants are actually usually either research managers (investigator), or people who primarily do experimental and technical work (trainees and technicians).

Given the allowable expenses on standard grants, the investigator supported by competitive standard grants recruits and pays trainees as research assistants in order to try to maximize productivity. Importantly, the trainee is working on faculty research as a paid research assistant, but this part of faculty research is his or her *main project.* As previously mentioned, far from being just cheaper and more motivated than technicians, the trainee status of a graduate student and a postdoc is also an organizational resource for managing and controlling research work. Through sharing scientific credit and informal integration of scientific credit into the structure of training, the investigator and trainees, unlike technicians, have the common goal of publishing papers in order to reach their own goals. Graduate students and postdocs therefore have a dual role, they are not just trainees but essential workers in the production of funded research, as technicians were in the past, whether they eventually become independent scientists themselves or not.

II. Implications of findings

The findings of this study have several important implications for the sociology of science and more generally, the social studies of science (especially for the study of the production of research, laboratories, and research training), as well for the sociology of higher education.

A. Change in the social organization in the biomedical sciences due to interaction with a dynamic institutional context and the social studies of science

The main theoretical implication of the findings of this study is that the institutional context of academic science is an important key to understanding recent change and current variation in the social organization of academic research. More specifically, the analysis suggests that it may not be possible to fully understand the social organization of research and training in the biomedical sciences without understanding the particular constraints and opportunities associated with research funding available, in specific national, institutional and historical contexts. Where external funding exists, it suggests that it is important to understand how the academic institution has integrated the existence of this funding into its own system, how competitive it is, the duration of grants, as well as how specific regulations and allowable expenses associated with that funding may be influencing organization. Externally funded research systems, such as those in Canada and the US, may give considerable influence to outside agencies not just over the types of research that will be done (e.g. funding targeted to specific types of research), but over the social organization of work and training in academic labs. Given that Canada, like many other countries, is increasing and changing the structure of state investment in the biomedical sciences in universities as a means of promoting economic growth and remaining internationally competitive, it is likely that further change in social organization of research and training and careers in this field will occur.

An assumption of those concerned with the effects of commercialization of research is that it is diminishing the autonomy of academic science, through loss of autonomy over choice of topics, and/or making some types of research more rewarding than others, as well as restricting the free flow of information and research materials (Kleinman and Vallas, 2001:456). Concern among science studies scholars about the autonomy of science has also focussed on the effects of military funding on the topics and content of research. [3] This study suggests that a more subtle mechanism is, however, at work in this respect. Polanyi (1962) defined the autonomy of science as the situation where "the choice of subjects and the actual conduct of research is entirely the responsibility of the individual scientist, [and] the recognition of claims to discoveries is under the jurisdiction of scientific opinion expressed by scientists as a body." The findings in this study suggest that biomedical scientists dependent on external funding have a limited form of autonomy, even with investigator- initiated standard operating grants where the investigator proposes the topic, because competitive conditions necessitate a career strategy which leads investigators to have to delegate the experimental work on their projects, which due to the constraints and opportunities associated with allowable uses of grant funds means delegating it to trainees, which also means that the amount of supervision of projects and of new students the investigator can provide changes as the successful investigator's career advances. In other words, the biomedical scientist dependent on competitive standard grants in these leading universities has a limited form of autonomy because he or she has considerable constraints with respect to the conduct of research and training.

B. Laboratory studies, social organization of production of research, scientific authorship and the social studies of science

The findings of my study extend the recent ethnographic study of the social organization of research first because they show that organization of research differs not just between disciplines (Knorr-Cetina, 1999), and between cultures in the same discipline (Traweek, 1988), but *as a result of different funding structures.* In the biomedical sciences in Canada, findings show that a shift from dependence on a less competitive to a more competitive system of grant support led to a different organization of research and training, moreover, that organization usually changes over the course of the career of an investigator successful in obtaining multiple grants, such that there are typically considerable differences in the organization of research and training in large and small labs. Findings also show that the dependence of biomedical scientists in Canada on the primary form of external funding available, competitive federal operating grants, has created a particular type of lab composed of temporary research personnel, primarily trainees, which differs from the research labs in other countries (e.g. France). Although a similar federal system of grant support for the biomedical sciences exists in the US, and there are likely some similarities in its effects, careful study will be necessary in order to understand the organization of research and training and its relationship to external influences.

Sociologists have only recently begun to conduct laboratory studies which examine the social organization of research in the life sciences (Knorr-Cetina, 1999; Owen-Smith, 2001). Existing studies have used a production framework, largely ignoring the other process taking place in these laboratories, research training, and the implications of the fact that many of the workers in these research groups are trainees. Findings in this study suggest that researchers studying the production of research in Canada must recognize the implications of the fact that research workers in labs are primarily trainees in order to understand the organization of research. Existing studies, for instance, have suggested that the means of management and control in biomedical labs is object-centered management (Knorr-Cetina, 1999) and skepticism at the lab level (Owen-Smith, 2001). Evidence indicates that in academic labs in Canada supported by competitive standard grants, lab directors depend *primarily* on the work of trainees, and their trainee status has been used as an important resource for controlling and managing research.

My findings have implications for the emerging study of scientific authorship. The increasing multiple authorship of papers in the biomedical sciences is assumed to be a phenomenon resulting from increasing collaboration. I suggest, however, that the dependence of investigators on competitive grants may have changed the primary reason for the occurrence of multiple authorships in the biomedical sciences, at least in Canada, from collaboration among scientists, to a division of labour with trainees in their own labs. The practices of delegating the projects to trainees and extending credit to them means that almost all of the research papers published in the biomedical sciences in these universities will have at least two authors from the same lab, the trainee and the investigator. Only *some* papers from *some* labs have, in addition, collaborators from other labs. The current near-universal multi-authorship of research papers in the biomedical sciences from these universities, therefore, is arguably best understood not as the result of collaboration between scientists in different labs, although they are common, but as the result of a division of labour with trainees (necessitated by having multiple grants and a need to have competitive productivity), with which unlike technicians in the past, authorship credit is shared.

Gibbons at al. (1994) have argued that the main change in the organization of scientific research is from single investigator research to multidisciplinary research done in larger groups. The findings of this study indicate that, in the case of biomedical sciences in Canada, the change from single investigator research is indeed a main change, but that the current organization of research might differ mainly as a result of being done in larger lab groups (whether or not they are multidisciplinary or not), and its dependence on a division of labour with trainees.

The findings also have implications for the study of scientific productivity. Much of the literature that examines scientific productivity has analyzed it as a product of variables associated with individual scientists (e.g. Cole 1979: Long 1978; Fox 1983, 2005). The findings provide further support for Stephan and Levin's (2002) argument that while the investigator remains key to understanding scientific productivity in the life sciences, it should be studied with the lab as the unit of analysis.

C. Graduate studies in science and the sociology of education

Studies on the production of research, graduate education, research universities, and federal funding have been for the most part been part of separate literatures. Gumport (2005) observes that the graduate research and education nexus is both under-studied and under-theorized, and argues that graduate education, research and federal funding in the US have become interdependent. Evidence in this study suggests, in the case of the biomedical sciences in Canada, that the key to understanding that interdependence is understanding that the careers of biomedical scientists are dependent on research funding with specific constraints on the allowable expenses on grants, which meant hiring trainees. However, since there are fields in which academic careers are not dependent on research funding, this finding emphasizes that it will be important to study the relationship between graduate education, research and research funding *by field*.

In the sociology of education, few studies have examined the processes involved in graduate education. The recent studies on research training in the sciences have been done from a socialization framework. The findings of this study clearly indicate that this perspective is not adequate to understand the organization of research and training in the biomedical sciences in Canada and the US (and likely in many other science fields) since graduate students have a dual role as trainees and paid workers in the production of faculty research, where the investigators productivity, and more generally, the production of externally funded research depends on their work as research assistants, whether or not they become independent scientists.

Further, the findings of this study suggest that in the biomedical labs supported by standard competitive grants, the organization of training is dependent of the organization of research, and that as labs grow this changes, resulting in a considerable difference in the organization between small and large labs. This differentiation between labs suggests the possibility that where scientist's careers are dependent on competitive standard grants, generalizations about the organization of training in a given field may not be possible.

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1 Watson (1987: 169) defines formal organizations as "social and technical arrangements where in which a number of people come or are brought together in a relationship where the actions of some are directed by others towards the achievement of certain tasks."

2 Schumpeter (1964 [1939], 1942) cited in Stinchcombe (1997: 13).

3 See Special Issue - Social Studies of Science, Vol. 33, No. 5 (October 2003).

Reference List

Abbott, Andrew. 1989. 'The New Occupational Structure: What are the Questions?' *Work and Occupations* 16(3): 273- 291.

Association of American Universities. 1998. Committee on Graduate Education: Report and Recommendations. Washington , DC: Association of American Universities <http://www.aau.edu/reports/GradEdRpt.html>

Association of Universities and Colleges of Canada. 2002. 'Full-time faculty at Canadian universities by field and sex.' Available at <www.aucc.ca/en/research/fac_f_s.htm> (accessed April 2004).

Association of Universities and Colleges of Canada. 2002. 'Sponsored research by source of fund.' Available at < www.aucc.ca/en/research/res_fund.htm > (accessed April 2004).

Barley, Stephen R. 1996. 'Technicians in the Workplace: Ethnographic Evidence for Bringing Work into Organization Studies.' *Administration Science Quarterly* 14: 404-441.

Barley, Stephen and Gideon Kunda. 2001. 'Bringing Work Back In.' *Organization Science* 12(1): 76-95.

Barley, Stephen R. and Julian E. Orr (eds.). 1997. *Between Craft and Science: Technical Work in U.S. Settings*. Ithaca, NY: Cornell University Press.

Barnes, Barry. 1977. *Interests and the Growth of Knowledge*. London: Routledge and Kegan Paul.

de Beaver and Rosen. 1978. ' Studies in scientific collaboration: Part I - The professional origins of scientific co-authorship. *Scientometrics* 1: 65-84.

de Beaver and Rosen. 1979a. 'Studies in scientific collaboration: Part II - Scientific co-authorship, research productivity and visibility in the French scientific elite, 1799-1830,' *Scientometrics* 1: 133-149.

de Beaver and Rosen. 1979b. ' Studies in scientific collaboration: Part III - Professionalization and the natural history of modern scientific authorship.' *Scientometrics* 1: 231-245.

Ben-David, J. 1977. *Centers of Learning: Britain, France, Germany, United States.* New York: McGraw-Hill.

Berelson, Bernard. 1960. *Graduate Education in the United States.* New York: McGraw-Hill.

Biagioli, M. 1998. 'The Instability of Authorship: Credit and Responsibility in Contemporary Biomedicine.' *FASEB Journal* 12: 3-16

Block, F. 1994. 'The Roles of the State in the Economy.' In N.J. Smelser and R. Swedberg (eds), *The Handbook of Economic Sociology*, pp. 691-710. Princeton: Princeton University Press.

Bloor, David. [1976] 1991. *Knowledge and Social Imagery*, 2nd ed. Chicago: University of Chicago Press.

Breslau, Daniel. 2002. 'Pierre Bourdieu (1 August 1930 - 23 January 2002).' *Social Studies of Science* 32(4): 631-635.

Burawoy, Michael (ed). 1991. *Ethnography Unbound: Power and Resistance in the Modern Metropolis.* Berkeley: University of California Press.

Burawoy, Michael. 1998. 'The Extended Case Method.' *Sociological Theory* 16:1:4-33.

Burawoy, Michael. 2003. 'Revisits: An Outline of a Theory of Reflexive Ethnography.' *American Sociological Review* 68: 645-679.

Burawoy, Michael, Joseph A. Blum, Sheba George, Zsuzsa Gille, Teresa Gowan, Lynne Haney, Maren Klawiter, Steven H. Lopez, Sean O'Riain and Millie Thayer. 2000. *Global Ethnography: Forces, Connections and Imaginations in a Postmodern World.* Berkeley: University of California Press.

Callon, Michel. 1995. 'Four Models for the Dynamics of Science.' In Sheila Jasanoff, Gerald E. Markle, James C. Petersen, and Trevor J. Pinch (eds.), *Handbook of Science and Technology Studies*, pp.29-63. Thousand Oaks, CA: Sage Publications.

Callon, Michel. 2002. 'From Science as an Economic Activity to Socioeconomics of Scientific Research.' In P. Mirowski and E-M Sent (eds.), *Science Bought and Sold*, pp. 277-317. Chicago: University of Chicago Press.

Canadian Association of University Teachers. 2004. *CAUT Almanac of Post Secondary Education in Canada 2004.* Ottawa, Canada.

Canadian Association of University Teachers. 2006. *CAUT Almanac of Post Secondary Education in Canada 2006.* Ottawa, Canada.

Caplow, Theodore and Reece J. McGee. 1958. *The Academic Marketplace.* New York: Basic Books.

Clark, Burton R. (ed.). 1993. *The Research Foundations of Graduate Education: Germany, Britain, France, United States, Japan.* Berkeley: University of California Press.

Clark, Burton R. 1995. *Places of Inquiry: Research and Advanced Education in Modern Universities.* Berkeley: University of California Press.

Clarke, B.L. 1967. 'Communication patterns of biomedical scientists.' *Federal Proceedings.* 26: 1288- 1292

Cole, S. 1979. 'Age and Scientific Performance'. *American Journal of Sociology* 84(4): 958-77

Collins, H.M. [1985] 1992. *Changing Order: Replication and Induction in Scientific Practice.* Chicago: University of Chicago Press

Cozzens, Susan E. 1986. ' Editors Introduction.' *Social Studies of Science* 16(1): 9-22

Delamont, Sara and Paul Atkinson. 2001. 'Doctoring Uncertainty: Mastering Craft Knowledge.' *Social Studies of Science* 31(1): 87-107.

Delamont, Sara, Paul Atkinson and Odette Parry. 2000. *The Doctoral Experience: Success and Failure in Graduate School.* London and New York: Falmer Press.

Delamont, Sara, Odette Parry, and Paul Atkinson. 1997. "Critical Mass and Pedagogic Continuity: Studies in Academic Habitus." *British Journal of Sociology of Education* 18(4): 533 - 549.

Denzin, Norman K. 1989. *Interpretative Interactionism.* Newbury Park: Sage.

Drucker, Peter. 1993. *Post Capitalist Society.* New York: Harper Business.

Etzkowitz, Henry. 1999. 'Bridging the Gap: The Evolution of Industry-University Links in the United States.' In Lewis M. Branscomb, Fumio Kodama, and Richard Florida (eds.), *Industrializing Knowledge*, pp. 203-233. Cambridge, MA: MIT Press.

Etzkowitz, Henry. 2002. 'Research Groups as "Quasi-Firms": The Invention of the Entrepreneurial University.' *Research Policy* 1383: 1-13.

Experimental Medicine Job Listing at McGill University. Retrieved March 30, 2007 from http://www.medicine.mcgill.ca/expmed/emjl/

Fitzgerald, David. 2006. 'Towards a Theoretical Ethnography of Migration.' *Qualitative Sociology* 29(1): 1-23.

Fligstein, Neil. 1990. *The Transformation of Corporate Control.* Cambridge: Harvard University.

Fligstein, Neil. 2001. *The Architecture of Markets: An Economic Sociology of Twenty-First Century-Capitalist Societies.* Princeton: Princeton University Press.

Fox, Mary Frank. 1983. 'Publication Productivity among Scientists: A Critical Review. *Social Studies of Science.* 13(2): 285-305

Fox, Mary Frank. 2005. 'Gender, Family Characteristics and Publication Productivity among Scientists.' *Social Studies of Science* 35(1): 131-150.

Freeman, Richard B., Eric Weinstein, Elizabeth Marincola, Janet Rosenbaum, and Frank Solomon. 2001. 'Careers and Rewards in Biosciences: The Disconnect Between Scientific Progress and Career Progression.' <http://www.grad.washington.edu/envision/resources/studies/html>

Fujimura, Joan H. 1988. 'The Molecular Biological Bandwagon in Cancer Research: Where Social Worlds Meet.' *Social Problems* 35(3): 261-283.

Fuller, Steve. 2002. 'The Road Not Taken.' In P. Mirowski and E.-M. Sent (eds.), *Science Bought and Sold*, pp. 444-461. Chicago: University of Chicago Press.

Gibbons, Michael, Camille Limoges, Helga Nowotny, Simon Schwartzman, Peter Scott, and Martin Trow. 1994. *The New Production of Knowledge: The Dynamics of Science and Research in Contemporary Societies.* London: Sage.

Gille, Z. and S. O'Riain. 2002. 'Global Ethnography.' *Annual Review of Sociology* 28: 271-95.

Gillmor, C. Stewart. 1986. 'Federal Funding and Knowledge Growth in Ionospheric Physics, 1945-81.' *Social Studies of Science* 16:105-33.

Glaser, Barney. 1964. *Organizational Scientists: Their Professional Careers.* Indianapolis, New York: Bobbs-Merrill.

Glaser, Barney and Strauss, Anselm.1967. *The Discovery of Grounded Theory: Strategies for Qualitative Research.* Chicago: Aldine.

Godin, Benoit and Yves Gingras. 1999. 'The Impact of Collaborative Research on Scientific Production.' *Association of Universities and Colleges of Canada Research File* Vol.3, No.3.

Gumport, Patricia. 1993a. 'Graduate Education and Organized Research in the United States.' In Burton R. Clark (ed.), *Research Foundations of Graduate Education: Germany, Britain, France, United States, Japan*, pp. 225-60. Berkeley and Los Angeles: University of California Press.

Gumport, Patricia. 1993b. 'Graduate Education and Research Imperatives: Views from American Campuses.' In Burton R. Clark (ed.), *Research Foundations of Graduate Education: Germany, Britain, France, United States, Japan*, pp. 261-93. Berkeley and Los Angeles: University of California Press.

Gumport, Patricia. 2000. 'Learning Academic Labour.' *Comparative Social Research* 19:1-23.

Gumport, Patricia. 2005. 'Graduate Education and Research. Interdependence and Strain. In Philip Altbach, Robert Berdahl and Patricia Gumport (eds), *American Higher Education in the Twenty-First Century. Social, political and economic challenges,* pp.425-461. Baltimore and London: Johns Hopkins University Press.

Guston, D. 2000. *Between Politics and Science. Assuring the Integrity and Productivity of Research.* Cambridge: Cambridge University Press.

Hackett, Edward J. 1987. 'Funding and Academic Research in the Life Sciences: Results of an Exploratory Study.' *Science & Technology Studies* 5 (3/4): 134-147

Hackett, Edward J. 1990. 'Science as a Vocation in the 1990s: The Changing Organizational Culture of Academic Science.' *Journal of Higher Education* 61(3): 241- 279.

Hackett, Edward J. 2001. 'Organizational Perspectives on University- Industry Relationships.' In Jennifer Croissant and Sal Restivo (eds.), *Degrees of Compromise: Industrial Interests and Academic Values.* Albany, NY: State University of New York Press.

Hackett, Edward J. 2005. 'Essential Tensions: Identity, Control and Risk in Research.' *Social Studies of Science* 35(5): 787-826.

Hacking, Ian. 1992. 'The Self -Vindication of the Laboratory Sciences.' In Andrew Pickering (ed.), *Science as Practice and Culture.* Chicago and London: University of Chicago Press.

Hagstrom, W.O. 1965. *The Scientific Community.* New York: Basic Books.

Heffner, A.G. 1981. Funded research, multiple authorship, and subauthorship collaboration in four disciplines. *Scientometrics* 3: 5-12.

International Committee of Medical Journal Editors (ICMJE). 1997. ' Uniform Requirements for Manuscripts Submitted to Biomedical Journals,' *JAMA*, 277 (2): 928.

Jordan, Kathleen and Michael Lynch. 1998. 'The Dissemination, Standardization and Routinization of a Molecular Biological Technique.' *Social Studies of Science* 28 (5-6): 773-800.

Kaiser, David (ed.). 2005. *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives.* Cambridge, MA: MIT Press.

Kalleberg, R. (ed.). 2000. 'Introduction.' *Comparative Social Research.* 19:

Katz, J. Sylvan and Ben R. Martin, 1997. 'What is research collaboration?' *Research Policy* 26: 1-18

Keating, Peter, Camille Limoges and Alberto Cambrosio. 1999. 'The Automated Laboratory: The Generation and Replication of Work in Molecular Genetics.' In Michael Fortun and Everett Mendelsohn (eds.), *The Practices of Human Genetics*, pp. 125-142. Dordrecht, Boston: Kluwer Academic Publishers.

Keefe, Jeffrey and Denise Potosky. 1997. 'Technical Dissonance: Conflicting Portraits of Technicians.' In Stephen R. Barley and Julian E. Orr. (eds.), *Between Craft and Science: Technical Work in U.S. Settings*, pp. 53-81. Ithaca and London: Cornell University Press.

Kleinman, Daniel. 1998. 'Untangling Context: Understanding a University Laboratory in the Commercial World.' *Science, Technology and Human Values* 23(3): 285-314.

Kleinman, Daniel and Steven P. Vallas. 2001. Science, capitalism, and the rise of the 'knowledge worker': The changing structure of knowledge production in the United States. *Theory and Society* 30: 451-492.

Knorr, Karin D. 1981. *The Manufacture of Knowledge.* New York: Pergamon Press.

Knorr-Cetina, Karin. 1995. 'Laboratory Studies: The cultural approach to the Study of Science.' In Sheila Jasanoff, Gerald E. Markle, James C. Petersen, and Trevor J. Pinch (eds.), *Handbook of Science and Technology Studies*, pp.140-166. Thousand Oaks, CA: Sage Publications.

Knorr-Cetina, Karin. 1999. *Epistemic Cultures: How the Sciences Make Knowledge.* Cambridge, MA: Harvard University Press.

Knorr-Cetina, Karin D. and Michael Mulkay (eds.). 1983. *Science Observed: Perspectives in the Social Study of Science*. Oxford: Pergamon.

Kodama, Fumio, and Lewis M. Branscomb. 1999. 'University Research as an Engine for Growth: How Realistic Is the Vision?' In Lewis M. Branscomb, Fumio Kodama, and Richard Florida (eds.), *Industrializing Knowledge*, pp.3-19. Cambridge, MA: MIT Press.

Laredo, Philippe and Philippe Mustar. 2000. 'Laboratory activity profiles: An exploratory approach.' *Scientometrics* 47(3): 515-539.

Latour, Bruno and Steve Woolgar. 1979. *Laboratory Life: The Social Construction of Scientific Facts*. Beverley Hills: Sage Publications.

Li, Alison. 2003. J.B. *Collip and the Development of Medical Research in Canada: Extracts and Enterprise.* Montreal and Kingston: McGill-Queen's University Press.

Li, Peter S. 1985. 'The Use of Oral History in Studying Elderly Chinese Canadians.' *Canadian Ethics Studies* 12(1): 67-77.

Long, J. Scott. 1978. 'Productivity and Academic Position in the Scientific Career.' *American Sociological Review*.

Lynch, Michael. 1985. *Art and Artefact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory.* London: Routledge and Kegan Paul.

Lynch and Woolgar. 1990. Cited Ch.2, p.6

Mangematin, V. and S. Robin. 2003. 'The Faces of PhD Students: Management of Early Careers of French PhDs in Life Sciences.' *Science and Public Policy* 30(6): 405-414.

Mannheim, Karl. [1936] 1970. *Ideology and Utopia: An Introduction to the Sociology of Knowledge*. New York: Harcourt, Brace and World.

Marcus, George E. 1983. *Elites: Ethnographic Issues.* Albuquerque: University of New Mexico Press.

Medical Research Council of Canada. 1966a. Survey of Research Personnel in the Medical Sciences, 1965-66. Report No. 1. Ottawa, Canada.

Medical Research Council. 1966b. Annual Report on Support of University Research, 1965-66. Ottawa, Canada.

Medical Research Council of Canada. 1980. *Report of the President 1979-80.* Ottawa, Canada.

Medical Research Council of Canada. 1984. *Report of the president 1983-4.* Ottawa, Canada.

Medical Research Council of Canada. 1985. *Report of the president 1984-5.* Ottawa, Canada.

Merton, Robert. [1942] 1973. 'The Normative Structure of Science.' In Norman W. Stant (ed.), *The Sociology of Science.* Chicago: University of Chicago Press.

Mirowski, Philip and Ester-Mirjam Sent (eds.). 2002. *Science Bought and Sold.* Chicago: University of Chicago Press.

Mirowski, Philip and Robert Van Horn (2005) 'The Contract Research Organization and the Commercialization of Academic Research', *Social Studies of Science* 35(4): 503-548.

Mulkay, Michael. 1979. *Science and the Sociology of Knowledge.* London: George Allen and Unwin.

National Academy of Sciences. Committee on Science, Engineering and Public Policy. 2000. *Enhancing the Postdoctoral Experience for Scientists and Engineers.* Washington, DC: National Academy Press.

National Research Council. 1998. *Trends in the Early Careers of Life Scientists.* Washington, DC: National Academy Press.

Natural Sciences and Engineering Research Council of Canada. 2000. *Performance Report for the Period ending March, 31st, 2000.* Ottawa: Government Review and Quality Services, Treasury Board Secretariat.

Owen-Smith, Jason. 2001. 'Managing Laboratory Work through Skepticism: Processes of Evaluation and Control.' *American Sociological Review* 66:427-452.

Owen-Smith, Jason and Walter W. Powell. 2001. 'Careers and Contradictions: Faculty Responses to the Transformation of Knowledge and its Uses in the Life Sciences.' *Research in the Sociology of Work* 10:109-140.

Owen-Smith, Jason, Massimo Riccaboni, Fabio Pammolli and Walter W. Powell. 2002. 'A Comparison of U.S. and European University- Industry Relations in the Life Sciences.' *Management Science* 48(1): 24-43.

Parry, Odette, Paul Atkinson and Sara Delamont. 1997. 'The Structure of Ph.D Research.' *Sociology* 31(1): 121-129.

Pickering, Andrew (ed.). 1992. *Science as Practice and Culture.* Chicago; University of Chicago Press.

Polanyi, Michael (1962). 'The Republic of Science.' *Minerva* 1: 53.

Schumpeter, J.A. 1942. *Capitalism, Socialism and Democracy.* New York: Harper Torch Books.

Schumpeter, J. A. [1939] 1964. Business Cycles: A Theoretical, Historical and Statistical Analysis of the Capitalist Process [Abridged by Rindigs Fels]. New York: Harper Torch Books.

Shapin, Steven. 1989. 'The Invisible Technician.' *American Scientist* 7: 554-63.

Shapin, Steven. 1995. 'Here and Everywhere: Sociology of Scientific Knowledge.' *Annual Review of Sociology* 21: 289-321.

Shinn, Thierry.1988. 'Hierarchies des chercheurs et formes de recherches.' *Actes de la Recherches en Sciences Sociales* 74: 2-22.

Shinn, Thierry. 2002. 'The Triple Helix and New Production of Knowledge: Prepackaged Thinking on Science and Technology.' *Social Studies of Science* 32(4): 599-614.

Sismondo, Sergio. 2004. *An Introduction to Science and Technology Studies.* Oxford: Blackwell.

Slaughter, Sheila and Larry Leslie. 1997. *Academic Capitalism: Politics, Policies, and the Entrepreneurial University.* Baltimore: Johns Hopkins University Press.

Slaughter, Sheila, Teresa Campbell, Margaret Holleman, and Edward Morgan. 2002. 'The "Traffic" in Graduate Students: Graduate Students as Tokens of Exchange Between Academe and Industry.' *Science, Technology & Human Values* 27(2): 282-313.

Slaughter, Sheila and Gary Rhoades. 1996. 'The Emergence of a Competitiveness Research and Development Policy Coalition and the Commercialization of Academic Science and Technology.' *Science, Technology and Human Values* 21(3):303-339.

Smith, M. 1958. 'The trend toward multiple authorship in psychology.' *American Psychologist* 13: 596-599.

Snow, David A. and Leon Anderson. 1991. 'Researching the Homeless: The Characteristic Features and Virtues of the Case Study.' In J.R. Feagin et al. (eds.), *A Case for the Case Study*, pp. 148-173. Chapel Hill: University of North Carolina Press.

Social Studies of Science, Special Issue - Vol. 33/5 (October 2003).

Stephan and Levin. 2002. 'The Importance of Implicit Contracts in Collaborative Scientific Research.' In Philip Mirowski and Ester-Mirjam Sent (eds.), *Science Bought and Sold*, pp. 412-430. Chicago: University of Chicago Press.

Stinchcombe, Arthur. 1997. 'On the Virtues of the Old Institutionalism.' *Annual Reviews of Sociology* 23: 1-18.

Strauss, Anselm and Juliet Corbin. 1998. *Basics of Qualitative Research: Techniques and Procedures for Developing Grounded Theory*, 2nd ed. Thousand Oaks, CA: Sage.

Traweek, Sharon. 1988. *Beamtimes and Lifetimes: The World of High Energy Physicists.* Cambridge, Mass.: Harvard University Press.

Watson, Tony J. 1987. *Sociology, Work, and Industry*, 2nd ed. London and New York: Routledge & Kegan Paul.

'The World of Post-Docs.' *Science* 285 (September 1999): 513 -1535.

Yin, Robert K. 1994. *Case Study Methods: Design and Methods*, 2nd ed. Newbury Park, CA: Sage.

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Operating Budget Module

Application number

Name of principal applicant and institution

Year

Financial Assistance Requested

Provide estimates for a full year of the quantities of human resources required for each human resources type (i.e., research staff and research trainee) under the column entitled "No." as well as the pro-rated salary amount per research staff or trainee. For part-time and/or shared resources, please indicate fractions (i.e., 0.5). Amounts must be in Canadian funds. Please refer to the Grants and Awards Guide for stipend levels. If the operating base changes significantly for subsequent years, copy this page and provide estimates for each year accordingly.

* Section applicable to UI and Rx&D research programs only. These applicants must complete page 1 for each year requested.

Each budget item must include the applicable provincial and federal taxes. Federal taxes should be calculated using the following after-rebate percentages: universities 2.3%, hospitals 1.2%, other institutions 3.5%.

For RCTs please calculate budget on a per patient as well as annual basis. Use of page 1 of the CIHR budget form is not mandatory for applications to the Randomized Controlled Trials Program only.

Sample

RESEARCH STAFF (excluding trainees)	No.	Salary	Benefits	CIHR	OTHER FUNDING SOURCES		TOTAL
					Cash*	In-kind*	
Research Assistants							
Technicians							
Other personnel (specify on page 3)							

RESEARCH TRAINEES	No.	Stipend	Benefits	CIHR	OTHER FUNDING SOURCES		TOTAL
					Cash*	In-kind*	
Postdoctoral Fellows (post PhD, MD, etc.)							
Graduate Students							
Summer Students							

MATERIALS, SUPPLIES AND SERVICES				
Animals				
Expendables				
Services				
Other (specify on page 4)				
TRAVEL				
TOTAL OPERATING				
EQUIPMENT				
TOTAL REQUEST				

Canada

Name of principal applicant and institution

Human Resources

For each applicant (nominated principal applicant, principal applicant(s) and co-applicant(s)) indicate the hours per week to be spent on the proposed project.

Name	Role	Hours week

Sample

Name of principal applicant and institution

Employment history (for the past 12 months of personnel to be employed on grant)
For each individual to be employed on this grant, list his / her position at the time of application, current salary rate ($ / annum, excluding benefits) and current source of funding. Additional pages may be added.

Name	Position	Current Salary Rate	Current Source of Funding

Sample

Name of principal applicant and institution

Details of Financial Assistance Requested

On additional pages:

1. Provide full justification of all budget items relative to the proposed research.

   If you include a need for research personnel and trainees, state their roles and explain why you require the level (in terms of qualifications and salary) that you are requesting.

   Itemize the expendables and services; for example, number and cost of animals, nature and amounts of reagents, numbers of subjects, or number and cost of printing survey instruments. For travel requests, indicate the purpose of the trip(s), the people that will be traveling, and their destination(s).

2. For maintenance and / or equipment items included in this operating budget, itemize your maintenance / equipment items and indicate:
   a) the availability and status of similar equipment;
   b) the anticipated extent of utilization;
   c) reasons for choice of specific type, model or service contract, in relation to alternatives;
   d) where applicable, the necessity for upgrading existing equipment or service contract.

3. If you are requesting or hold start-up funds to equip a new laboratory, please detail any funds you have received or have applied for from other sources for this purpose (e.g. institutional sources) and how you intend to use these funds.

sample

Name of principal applicant and institution

APPENDIX 1
Supporting documentation

a) Cost quotations for equipment or service contracts. For equipment or service contracts costing more than $10,000, attach at least one cost quotation. For items costing more than $25,000, at least two competitive quotes must be provided.

b) For items costing more than $25,000, letters attesting that the equipment is not currently available at your institution from Department Head(s), Dean(s) and / or Research Institute Director(s).

Note: **No other information may be attached to this module**. Any additional material will be removed from the application prior to being sent to reviewers.

Sample