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Hybrid Practices:
The Dynamics of University Research and Emergence of a Biotechnology Company

Academic Dissertation

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For my children
Inari and Riina

Lapsilleni
Inarille ja Riinalle

TIIVISTELMÄ

Väitöskirjassa analysoidaan erään Helsingin yliopistossa toimineen, kasvibiotekniikkaa käyttäneen tutkimusryhmän kymmenvuotinen elinkaari ryhmän perustamisesta vuonna 1990 siihen saakka, kun se muuttui tutkimuslähtöiseksi spin-off –yritykseksi vuonna 2000. Tutkimusaineisto koostuu 79 haastattelusta ja laajasta dokumenttiaineistosta, johon kuuluu muun muassa tieteellisiä julkaisuja, tutkimussuunnitelmia ja –raportteja sekä kirjeenvaihtoa. Aineiston laadullisessa analyysissä hyödynnetään useita sellaisia lähestymistapoja, jotka ovat tarkastelleet tiedettä ja yliopistoja työkäytäntöjen näkökulmasta (esim. kulttuuri-historiallinen toiminnan teoria, etnometodologia ja symbolinen interaktionismi). Näin saatujen tulosten perusteella kommentoidaan neljää sosiologista teoriaa, joiden mukaan tiede ja yliopistot ovat viime aikoina radikaalisti muuttuneet. Tarkastelun kohteena olevat teoriat ovat tiedontuotannon malli 2, yliopistojen, teollisuuden ja valtiovallan kolmoiskierre, akateeminen kapitalismi sekä yritysyliopisto.

Väitöskirjan pääosan muodostaa neljä artikkelia, joista kussakin eritellään jotakin tutkimusryhmän elinkaaren vaihetta. Ensimmäisessä artikkelissa tarkastellaan ryhmän laboratoriotyötä ja sitä, kuinka sen käyttämät kokeelliset järjestelmät muuttuivat 1990-luvun alkuvuosina. Toisessa artikkelissa hyödynnetään symboliseen interaktionismiin pohjaavaa sosiaalisten maailmojen näkökulmaa tieteenalojen välisten konfliktien analysoimiseksi siinä yliopiston laitoksessa, jossa tutkittu ryhmä työskenteli. Kolmannessa artikkelissa tutkitaan sitä, kuinka raja tutkimusryhmän perustaman biotekniikkayrityksen ja yliopistotoiminnan välillä tuotettiin ja kuinka sitä ylläpidettiin 1990-luvun lopussa. Neljäs artikkeli puolestaan kokoaa tutkimuksen empiirisiä tuloksia yhteen ja kritisoi tältä pohjalta kahta teoriaa: tiedontuotannon mallia 2 ja teesiä yliopistojen, teollisuuden ja valtiovallan kolmoiskierteestä.

Kirjallisuuskatsaukseen ja empiirisiin analyyseihin nojaten väitöskirjassa eritellään neljää yllä mainittua sosiologista teoriaa. Kiinnittämällä huomiota niiden erilaiseen teoreettiseen statukseen ja tästä johtuviin puutteisiin väitöskirjassa osoitetaan, että tiedettä ja yliopistoja olisi perusteltua tarkastella monimutkaisina dynaamisina kokonaisuuksina, joiden kehitykseen paikallisesti ilmenevät historialliset, poliittiset ja kulttuuriset tekijät vaikuttavat. Näiden käsitteellistämiseksi tarvitaan yleistettyjen teoreettisten kantojen sijaan käsitteitä, jotka tematisoivat tiedettä ja yliopistoja erilaisten työkäytäntöjen näkökulmasta.

ABSTRACT

This doctoral thesis focuses on the trajectory of an agricultural plant biotechnology research group and its transformation into a university start-up company under the auspices of a major Finnish university, the University of Helsinki. The data applied in this study consist of 79 interviews and an extensive body of documentary material including scientific publications, research plans and reports, and correspondences. The qualitative analysis of these materials was informed by conceptual resources drawn from several theoretical approaches that have addressed science and the university organization in terms of work and practice (e.g., cultural-historical activity theory, ethnomethodology and symbolic interactionism). On the grounds of the results so achieved, four sociological theories purporting a change of science and the university institution are discussed. The theories considered include the Mode 2 knowledge production thesis, the triple helix of university-industry-government relations, academic capitalism and the enterprise university.

The main body of the thesis is composed of four research articles, each analyzing a distinctive phase in the agricultural plant biotechnology group's trajectory. The first article analyzes the construction of research objects in the laboratory and the transformation of experimental systems used at the early stages of the group's research. The second paper relates to the social world perspective and investigates the complex organizational ecology of disciplines in the university department where the biotechnology group operated. The third paper makes use of the concept of boundary work and deals with the regulation of the emergent spin-off company at the university. Finally, the fourth article unites the empirical results and criticizes two of the above-mentioned theories, namely, Mode 2 knowledge production and the triple helix.

On the grounds of the literary review and empirical analyses accomplished, the thesis demonstrates the deficiencies in the existing sociological theorizing on the transformation of science and the university. By appraising the dissimilar theoretical statuses of the four theories in focus, the thesis also demonstrates the need to see science and universities as complex dynamic entities whose development is locally shaped by multiple historical, political and cultural characteristics, better appreciated by the practice-oriented sociology of science than the four theories considerd

ACKNOWLEDGEMENTS

Although sociology is not exactly a science of discovery, the process of writing this thesis represents something I was not able to anticipate when I got it started. It was, in the end, an act of discovery of some kind. Like the American poet and writer, Raymond Carver, I have liked to tinker with and mull over with my papers for extended periods of time. I came to understand only gradually what the focus of each of the articles was, and what kind of general discussion might be pursued on their basis. After the fact, thanks are due to the numerous people who have helped me in this process.

Above all, I am indebted to my advisor Prof. Reijo Miettinen. In addition to securing the necessary grants for the accomplishment of the current study, he has had a wide-ranging contentual influence on the thesis. Indeed, I feel myself very fortunate to have had you, Reijo, as my supervisor. Not only have you given ample amounts of time in reviewing various versions of my papers but, more importantly, you have helped me in setting down the methodological starting points for the whole of this dissertation. I have been really amazed by your insights on what is worth seeking and adhering to in the fragmentary field of science and technology studies. Another reason why I am truly grateful to you is that you made it your business to upgrade the level of my work to meet the international standard. Although I was motivated to pursue such a target by myself, I am afraid I would never have realized it without your assistance.

I also had the exceptional opportunity of working with in a vibrant group of researchers at the Center of Activity Theory and Developmental Work Research. It would simply be an overwhelming task to describe the ways in which this community fostered the materialization of the current study. In addition to the researchers working at the Center, its director, Prof. Yrjö Engeström, was responsible for much of the excellent working environment I enjoyed. Thanks a lot for the wonderful graduate school classes and occasional advice at later times. I also want to express my thanks to the members of our research team at the Center. Many thanks to you, Stephanie Freeman, Mervi Hasu, Sampsa Hyysalo, Tarja Knuuttila, Janne Lehenkari, Jussi Leminen, Erika Mattila, Eveliina Saari, Jussi Silvonen and Juha Siltala. Your friendship, feedback and collegial support made this work a pleasurable experience!

At the outset of this study I had the privilege of finding an excellent case example to work on. My deepest gratitude therefore goes to Professors Eija Pehu and Juha Helenius, whose courage, trust and commitment really made this dissertation something unique. Without your help, I would never have succeeded in this task as well as I finally did. I also want to express my compliments to Prof. Pehu's research group, whose members were kind enough to let me wander around their laboratories with so many questions and concerns. The group's numerous partners and colleagues at Boreal Plant Breeding Ltd., UniCrop Ltd., the University of

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In the spring of 2001, I had the pleasure of spending three months at Cornell University's Department of Science and Technology Studies. This visit was made possible by the kind invitation by Prof. Trevor Pinch. At Cornell, and elsewhere, I had the privilege of getting excellent feedback from Prof. Michael Lynch, whose work has substantially influenced my perspective in this thesis. I regret that many of the lessons I learned from his research could not be put into practice on this occasion. While living in Ithaca, I also enjoyed the pleasant company of post-doctoral researchers Drs. Cristopher Henke, François Mélard and Yoshio Nukaga, who provided me with the bulk of my social contacts during my stay. It was great of you to organize an informal discussion circle for the purpose of exchanging ideas about our unfinished papers! I also got to know several graduate students from Cornell's STS department. Thank you Anna Maerker, Sonja Schmid and others for inviting me to your parties and for your attempts to get me on the rink to play floor hockey in the Cornell league – too bad the league rules wouldn't allow it.

Before getting permission to publicly defend this thesis, it was examined by two eminent researchers, Professor Aant Elzinga and Dr. Seppo Raiski. I am grateful for their high-grade feedback, which fostered my learning until the very last phases of this research. As well, having only limited capacity in the English language, I would never have managed to give this thesis the indispensable final touch it was in need of. Thank you John Gage, Henry Fullenwider and Marjatta Zenkowicz, for not only correcting my English usage but for also for providing me with elegant ways to express occasionally quite difficult matters.

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ORIGINAL PUBLICATIONS

I Tuunainen, Juha (2001). Constructing Objects and Transforming Experimental Systems. *Perspectives on Science* 9 (1), 78-105.

II Tuunainen, Juha (2005). When Disciplinary Worlds Collide: The Organizational Ecology of Disciplines in a University Department. *Symbolic Interaction* 28 (2).

III Tuunainen, Juha (in press). On the Possibility of a Research Group-Firm Hybrid Entity. *Social Studies of Science*.

IV Tuunainen, Juha (2002). Reconsidering the Mode 2 and the Triple Helix: A Critical Comment Based on a Case Study. *Science Studies* 15 (2), 36-58.

In the following summary article, these articles will be referred to by the Roman numerals I-IV.

SUMMARY ARTICLE

The Purpose of This Study[1]

The title of the following thesis – 'Hybrid Practices' – has a twofold meaning that relates to sociological studies of scientific practices and the literature that speaks about the transformation of the university institution. First, 'Hybrid Practices' addresses the research work that the object of this study, a university plant biotechnology[2] group, was involved in: the simultaneous production of agriculturally useful end-products and creation of related scientific knowledge. In science and technology studies, the term "hybrid science" has been used to characterize the combination of scientific work with other human practices, such as agricultural production (Gieryn 1999, 251). In this view, science is seen as a deeply societal endeavor where practical utility operates as the paramount justification for scientific research. As a growing body of literature shows, combining theoretical understanding and social use is not an extraordinary feature of scientific practice but, rather, a quite common attribute of not only life sciences (Kimmelman 1992; Kleinman 2003; Knorr Cetina 1982; Miettinen 1998) but other investigative areas as well. According to Donald Stokes (1997, 14) for instance, many investigative fields such as seismology as well as oceanic and atmospheric sciences are closely attached to, and derive their respective research topics from, the dread of earthquakes, storms, droughts and floods. Taking heed of such a perspective, the specific task of the present study is to further elaborate on the idea of "an experimental system" (Rheinberger 1997), hitherto used rather internalistically.

Secondly, the title refers to a corpus of research according to which the entire university institution has been in a state of fundamental transformation. These studies, proliferating in the fields of higher education research and research policy, argue that financial considerations related to global economic competition have penetrated academia. National science and technology policies have begun to emphasize potentially lucrative areas of research, while simultaneously universities have encountered hard times due to considerable cuts in governmental allowances. In consequence, universities' dependence on external funding has increased in tandem with the privatization of research results. In the wake of these developments, so the general argument goes, universities in different countries are in a state of profound change. Terms like "hybrid organizations" (Slaughter and Leslie 1997, 9), "Mode 2 institutions" (Nowotny, Scott, and Gibbons 2001, 79) or "entrepreneurial universities" (Etzkowitz 2003b) have been used to capture these transformations. From the point of view of such claims, the current study presents a challenging case: I regard these ideas more as hypotheses to be put to the test rather than conceptions to be taken for granted.

This thesis thus seeks to re-specify both of these notions of hybridization. To do so, I shall take advantage of my case study of the development of a plant biotechnology research group. The process analyzed covers a ten-year period starting in 1990, when a research group was founded in the Faculty of Agriculture and Forestry at the University of Helsinki, Finland, and ending in 2000 when the group was disentangled from the university to operate as a free-standing commercial enterprise. By taking up various analytical themes revealed by this trajectory, the study seeks to contribute to the current understanding of scientific practice, agricultural science and the commercialization of university research results. While each of these topics is addressed in four articles constituting the main body of the thesis (Articles I-IV), the principal task of this summary article is to bring these analytical strands together under a broader framework. For such a framework, I have chosen the theoretical debate over the transformation that sciences and universities have undergone in the past few decades. During these years, governments have increasingly sought to foster national prosperity by supporting new, potentially profitable technologies such as genetic engineering (Gottweis 1998), while simultaneously universities have increasingly taken up a role as economic engines in their respective regions (Etzkowitz and Leydesdorff 1997).

Roughly speaking, two positions can be discerned in this discussion. First, some authors state that a radical transformation with respect to knowledge production has taken place, as a result of closer relations between the previously isolated institutional spheres of the university, government and industry. Characterized as the "triple helix" by Henry Etzkowitz (2003a), the interaction between these institutions has given rise to the kind of research that not only seeks to advance scientific knowledge but tries to attain socially and commercially viable products as well. Another example of such a radical stance is the Mode 2 knowledge production thesis, which claims that science is increasingly becoming fused with other forms of social practice (Gibbons et al. 1994). Second, whereas the above models see radical metamorphoses in science and the university, there are also more moderate views on the change. These perspectives, expressed as academic capitalism (Slaughter and Leslie 1997) and the enterprise university (Marginson and Considine 2000), basically reassert the general transition in science and technology policy but also keep an eye on the diversity of its effects. That is to say, some disciplines and universities are more easily adapted to the changing conditions emphasizing market and market-like modes of operation than others.

While the above four theories discuss the transformation at a rather general level, my task here is to open up the topic for empirical scrutiny. As such, the present work belongs to a limited set of studies (Kleinman 2003; Packer and Webster 1996; Rabinow 1999; Rappert and Webster 1997; Webster 1994) that concentrate on the relationships between universities and the corporate world in terms of qualitative empirical detail and, by so doing, seek to yield new theoretical insight. The specific character of this study, in contrast to others, is the long

time-span being analyzed. Taking the ten-year trajectory of the research group under analysis, the present research brings to light a wide range of issues central to the larger debate, thereby giving reasons for reconsidering some of the positions taken by other scholars. This is done by addressing the following questions:

1. Scientific research is often attached to such distinct agendas as the creation of a theoretical understanding of particular phenomena, experimentation with instruments and methods, as well as the production of useful social applications. These various dimensions, subsequently called the theoretical, experimental and applied concerns of scientific research, were also part and parcel of the experimental systems developed and used by the examined research group. Therefore my first research question is: how did the theoretical problem-solving, development of instrumentalities and striving for applied purposes intertwine during the progress of the group's work? Answering this question will contribute to a more elaborate understanding of the intrinsic dynamics of the kind of research that is all too often glossed over by using such generalized terms as Mode 2 knowledge production or entrepreneurial science (Articles I and IV).

2. From the study of scientific practice, I will then move on to analyze the research group in the context of the university organization. This will help us to better appreciate the complexities present in the university institution and to overcome the unidimensionality of some perspectives, such as that of academic capitalism (Slaughter and Leslie 1997). To reveal what else might be found in the local organizational ecology of university departments, other than sheer competition for external funding, I will draw ideas from a social world perspective (Clarke 1991; Strauss 1991) to examine how different disciplines present in the Department of Plant Production clashed with one another (Article II).

3. The third issue investigated in this thesis considers what some authors call the entrepreneurial university, that is, a type of university which unites academic research and commercial development in a compatible fashion (Etzkowitz 2003b). This generalized thesis is challenged by my case example, where the research group sought to hybridize university and business activities. More specifically, I shall make use of Thomas Gieryn's (1999) idea of boundary work and ask: how was the group's private business separated from its academic activities under the auspices of the University of Helsinki? (Article III.)

4. Finally, the fourth research question directly addresses Mode 2 knowledge production and the triple helix models. As exemplified by a number of stud-

> ies (e.g., Kivinen and Varelius 2003; Powell and Owen-Smith 1998), these ideas are influential, and are frequently used as vehicles to conceptually understand developments that have taken place in particular countries or fields of research. On the grounds provided by my case example, I will challenge such applications and ask: do these models provide a reasonable basis for interpreting empirical cases? What is their theoretical status? What issues do they tend to disregard? (Article IV.)

The structure of this summary article is as follows. In the next section, I review more closely the four theories – i.e., Mode 2 knowledge production, the triple helix, academic capitalism and the enterprise university – and connect them to analyses that address the case of Finland, in particular. The criticism directed at these positions is also examined in order to prepare the way for specifying my own methodological stance. Basically, I suggest that a useful way of specifying and reconsidering the massive changes these theories speak about, goes through the application of analytical concepts and ideas developed within a broad body of scholarship that emphasizes scientific practice (e.g., Pickering 1992). The section titled Research Data and Methods orientates the reader to the development of the plant biotechnology research group and considers the data, methods and validity of my research, while the section following it summarizes the key findings of my analyses originally published in the four articles. Finally, the work is brought to an end in the concluding discussion where the debate about the transformation of science and the university is revisited by taking note of the empirical research results achieved in this study.

From Generalized Accounts of Science and the University to the Study of Local Practices

In this section, the four theories of transformation will be reviewed. Of the many conceptualizations that have tried to capture the characteristics of the new science and university, I have chosen to focus on Mode 2 knowledge production, the triple helix of university-industry-government relations, academic capitalism and the enterprise university.[3] I begin my discussion by describing the two radical positions – Mode 2 and triple helix – according to which there is a historical discontinuation in the development of science and the university that has only recently unfolded as a result of the global market economy and related governmental policies. I then turn my attention to the two moderate stances, namely, academic capitalism and the enterprise university. In their perspective, transformations have surely taken place but have become materialized in a variety of ways as regards different universities and disciplines. Taken together, all of these theories speak about the English-speaking world, primarily the United States of America, Canada,

the United Kingdom and Australia. Because certain other studies have shown that the situation might be different in some European countries (Clark 1998; Giesecke 2000; Krücken 2003), I shall pay attention to the relevant Finnish literature that either challenges or lends support to these theories. Another reason for taking note of the Finnish applications is the fact that these conceptual models have been used rather extensively in understanding the changes Finnish universities have undergone during the 1990s.

Before entering into a discussion of these theories, it would be useful to address their different theoretical statuses. To do so, I shall apply the general classification of sociological theories as recently presented by Arto Noro (2000). According to Noro, there are three types of sociological theorizing. First, research theory refers to theorizing which is directly linked to empirical evidence. A research theory, then, is a theory developed on the grounds of such evidence. In other words, it is a theory used and further elaborated in empirical social research, such as the present study. Second is general sociological theory, which applies to theories that do not straightforwardly relate to empirical material. Nevertheless, general theories make use of the results achieved by empirical research as well as the scientific discussions taking place at the level of research theories. Examples of general theories mentioned by Noro include, among others, Talcott Parsons's theory of social action and Niklas Luhmann's systems theory. Finally, the third type of sociological theorizing is, according to Noro, the diagnosis of the era. Referring to the German historian of ideas Walter Reese-Schäfer, Noro conceives of diagnoses of the era (*Zeitdiagnose*) as theories that seek to answer existential questions asking who we are and what is the nature of our epoch. Such descriptions of the spirit of the age usually combine familiar material in a novel way, are normative in nature and endeavor to yield new topical insight. Therefore, their relationship to the empirical evidence is complicated: they cannot be used, says Noro, as a means to directly interpret empirical material since we would only find in the data what these theories already imply. In academic writing, examples of diagnoses of the era are evident in books like "The Consequences of Modernity" by Anthony Giddens (1990) and "Risk Society: Towards a New Modernity" by Ulrich Beck (1992).

In the present study, this classification of sociological theorizing will be used to better understand the character of the four theories mentioned above. After briefly summarizing the major viewpoints of these theories, and after attending to the commentary given to them, I will suggest that the Mode 2 thesis represents diagnosis-of-the-era theorizing and that studies on academic capitalism and the enterprise university operate mostly on the level of research theories. Finally, I will argue that the theoretical status of the triple helix model is rather opaque, as it combines elements drawn from all of the three types of theories. Having done that, I shall be better equipped to locate my own analytic approach vis-à-vis the four theories described and discussed.

Radical Transformation of Science and University

Mode 2 Knowledge Production[4]

According to Michael Gibbons and co-workers (Gibbons 2000a; Gibbons et al. 1994; Nowotny, Scott, and Gibbons 2001), a radical shift in the way that knowledge is being produced has taken place: the "Mode 1 knowledge production" has given way to a new mode called "Mode 2." Compared with the previous Mode 1 science, which subscribes to reliable academic knowledge produced within autonomous disciplinary contexts, Mode 2 science takes place in "the context of application." This refers to knowledge produced within open and shifting organizational boundaries, and is managed for the achievement of particular useful purposes (Gibbons et al. 1994, 3-6). A prime example of Mode 2 science is biotechnology, where researchers no longer try to reveal "the basic principles of the world" but seek to produce specific commercial applications and understand "concrete systems and processes" related to such applications (Gibbons et al. 1994, 23-24, 147). Therefore, various locales and practitioners are involved in Mode 2 knowledge production, spanning from researchers from different academic disciplines to industrial scientists and other social actors. Due to the more intensive interaction among them, strong social responsibility and accountability permeate the Mode 2 research process. This is to say, a broader set of social interests act as new quality control criteria vis-à-vis the internal scientific peer review of Mode 1 science (Gibbons et al. 1994, 32-33). The main differences between Mode 1 and Mode 2 knowledge production are depicted in Table 1.

The emergence of Mode 2 science also parallels wider transformations in society (Nowotny, Scott, and Gibbons 2001). As claimed by Gibbons (2000a, 160), the more open system of knowledge production is not "an autonomous development affecting science only; rather it reflects, and is reflected in, the emergence of a more open type of society." Science, in this view, has no intrinsic character but becomes intermingled with the rest of the society where boundaries between major social institutions (e.g., the state, the market, culture and science) have become increasingly fuzzy and transgressed. This is largely due to the intensification of international rivalry in global business and industry. In this connection, the institutional structure of the university has also grown to be more open than it used to be. The demarcation between universities and other organizations, such as industrial enterprises, has eroded as scientists have become responsive to the needs of industry. Universities have thus become "stretched" institutions that encounter competitive and even contradictory pressures, such as creating scientific knowledge while satisfying the demands of mass education (Gibbons et al. 1994, 70-89; Nowotny, Scott, and Gibbons 2001, 79-94).

In Finland, governmental policy has increasingly emphasized the need to strengthen the relationship between universities and other organizations, such

Mode 1 Knowledge Production	*Mode 2 Knowledge Production*
Problems set and solved in a context governed by the interests of a specific academic community	Knowledge produced in the context of application
Disciplinary knowledge	Transdisciplinary knowledge
Homogeneity of skills	Heterogeneity of skills
Hierarchical organizations that tend to preserve their forms	Flat hierarchies and transient organizational structures
Less socially accountable and reflexive	More socially accountable and reflexive
Quality control based on peer review	Expanded system of quality control which is based on a wider, more temporary and heterogeneous set of practitioners, collaborating on a problem defined in a specific and localized context

Table 1. Descriptions used by Michael Gibbons (2000b, 40) to differentiate the two modes of knowledge production.

as governmental agencies and business enterprises (Häyrinen-Alestalo, Snell, and Peltola 2000; Miettinen 2002). In this context, several scholars have discussed the Mode 2 knowledge production thesis. While some have accepted and used the idea quite straightforwardly (Kivinen and Varelius 2003), others have been more critical of it. The latter have claimed, for instance, that the model overstates and simplifies recent developments in the relationship between science and society. Therefore, rather than being an accurate description of the Finnish situation, the model remains a meta-theory or programmatic statement only (Allardt 2002, 303-305; Häyrinen-Alestalo 1999, 46-47).

The empirical study by Mika Nieminen and Erkki Kaukonen basically substantiates this general assessment. Although Nieminen and Kaukonen claim that an "interactive research mode" exists somewhere between traditional contract research and academic research, they do not equate this kind of science to Mode 2. According to the authors, the interactive mode is characterized by "multilateral external contracts, multiple internal and external partners, equal partnership and interactive project design." As distinct from Mode 2 knowledge production, this type of university research has, however, a distinctive academic focus: "from the

researcher's perspective, several contracts may relate to one single research program that represents her/his or her/his department's key know-how and research interests." Moreover, Nieminen and Kaukonen assert that "the research can be also rather 'academic' in the sense that the financier does not seek immediate development utility but new knowledge, even though the study would be conducted 'in the framework of application'" (Nieminen and Kaukonen 2001, 101). Thus, according to the results of Nieminen and Kaukonen, university research is able to maintain itself in the evolving social environment, rather than lose its characteristics in the way portrayed by Gibbons and others.

Triple Helix of University-Industry-Government Relations

A closely related idea to Mode 2 knowledge production is "the triple helix of university-industry-government relations" introduced by Henry Etzkowitz (1998). In Etzkowitz's terminology, the triple helix is a metaphor representing the close interaction and, indeed, increasing overlap between the previously separate institutional spheres of the university, industry and government. As a result, boundaries between these institutions have become blurred. With respect to universities, this involves the incorporation of the traditional academic mission – "the extension of knowledge" – into a compatible relationship with the "capitalization of knowledge": scientists in many fields start to look at their results from the viewpoint of commercial potential in addition to theoretical and methodological advancement (Etzkowitz 1998, 824-829). It also implies the emergence of the entrepreneurial university (Etzkowitz and Leydesdorff 2000; Leydesdorff and Etzkowitz 1998). As elaborated by Etzkowitz, the entrepreneurial university is a hybrid organization which incorporates "the third mission" of economic development alongside scientific research and higher education. This is done, for instance, through university patenting and licensing offices, spin-off firms, business incubators and science parks (Etzkowitz 2003b).

Etzkowitz (2002, 121) presents the emergence of the entrepreneurial university as an irresistible, unavoidable development, "an internal dynamic working itself out." It is also observable all over the world – in North America, Europe, Asia, and Latin America (Etzkowitz, Schuler, and Gulbrandsen 2000; Etzkowitz et al. 2000). Moreover, he states that the trend is going from strength to strength. "The University of the Future" will be a business incubator entirely, meaning that the technology transfer and incubation of new firms will convert from happenstance into a permanent organizational activity that takes place in each and every department. Not even the tensions between academic research, higher education and societal service hinder this development, as the contradictory objectives become reconciled through clear guidelines and elaborate organizational practices (Etzkowitz 2002a; 2003a).

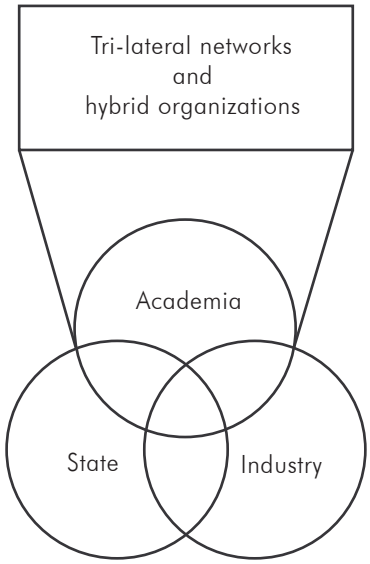

Figure 1. The triple helix model (reprinted by permission of Sage Publications Ltd from Etzkowitz 2003a, p. 302). ©Sage Publications, 2003

Drawing from the triple helix idea, Kaukonen and Nieminen (1999) claim that there has been a long-term transition in Finland towards more intensive university-industry-government relations. These connections began to evolve in the late 1970s, as research expenditures increased substantially in private industry and in governmental research institutes while the universities' relative share of research funding declined. As a result of this development, which continued throughout the 1980s and 90s, the entire Finnish research system was restructured and the priorities of the national science and technology policy were substantially altered (Kaukonen 1987, 24; Nieminen and Kaukonen 2001). The authors, then, assume that these changing funding patterns and the related competitiveness-oriented science and technology policy reveal a transformation of the relationships between universities and industries. That is, the funding patterns are claimed to uncover more intimate university-industry contacts and a blurring of institutional boundaries. In this sense, Nieminen and Kaukonen (1999, 338; also Kivinen and Varelius 2003) argue, it is justified to speak about the development of "a Finnish Triple Helix."

The statistical data used by Nieminen and Kaukonen did not, however, allow for the analysis of actual interactions between universities, government and industries. The notion of the Finnish triple helix was thus left fairly vague as well.

Interestingly, this interface was probed more closely in subsequent studies. While these investigations basically confirmed the transition towards closer contacts between university research, governmental funding, and industrial development, they also revealed that the general pattern was differentiated, not only by university type (Häyrinen-Alestalo, Snell, and Peltola 2000) but also according to scientific disciplines and specialties (Hakala et al. 2003; Nieminen and Kaukonen 1999; Ylijoki 2003). Some universities and departments were simply nearer to government and industry than others. For that reason, the general affinity between the triple helix model and the Finnish situation dissolved even further.

Moderate Transformation of Universities

Academic Capitalism

The thesis according to which university faculties are increasingly engaged in technology transfer and industrial collaboration is further examined by Sheila Slaughter and Larry Leslie. In their study of public research universities operating in Australia, Great Britain, Canada and the United States, Slaughter and Leslie found that over the past 20 years universities have become increasingly affected by "the profit motive" and "market-like" behavior. The background to this development can be found in the globalization of the economy, the emergence of policies aimed at securing nations' industrial competitiveness and the change in the financing of universities from block-grant funding to targeted, competitive funds (Slaughter and Leslie 1997, Ch. 2). In consequence, "the academic capitalism" has evolved within today's universities.

The term refers to "market and market-like" efforts on the part of institutions and professors trying to secure external grants (Slaughter and Leslie 1997, 8-9). Examples of such activities include competitive research funds, consulting, technology transfer, patenting and licensing, as well as arm's-length corporations, spin-off firms and research parks (Slaughter and Leslie 1997, 65). In addition, new kinds of hybrid entities spanning the customary public-private boundaries have been formed; for example, cooperative research centers bring together universities, industries and government agencies within a context of strictly commercial research. Such centers provide long-term funds for large projects, cover patenting and licensing costs and finance the process of bringing research results to the market (Slaughter and Leslie 1997, 149-151). Given these trends, it is no wonder that different disciplines are in varying positions as regards their ability to take advantage of academic capitalism. Clearly, those fields that are closest to the market, such as biotechnology, are better off in this respect than some others, for instance the humanities (Slaughter and Leslie 1997, 7).

The encroachment of the market and market-like mechanisms into academe has not taken place without controversy. Rather – and contrary to the enthusiasm of Etzkowitz – Slaughter and Leslie (1997, 9) consider that research universities are pregnant with contradictions, as their faculties are employed by the public sector while being increasingly autonomous from it. For instance, academics are encouraged to become more active in terms of commercially exploiting their knowledge and expertise, while the number of students has increased as have the teaching responsibilities (Slaughter and Leslie 1997, 61). Other sources of conflicts within universities include attempts by central administration to control and monitor the entrepreneurial activities of researchers as well as unstable norms regulating the ownership of intellectual property rights (Slaughter and Leslie 1997, 146-149, 190-192). Despite these difficulties, Slaughter and Leslie state that the academic, commercial and bureaucratic cultures are integrating and that the distance between universities, industries and governments is decreasing. Universities as special institutions enjoying a measure of autonomy are on the wane, becoming similar to other large, entrepreneurial organizations (Slaughter and Leslie 1997, 222).

As regards the case of Finland, many researchers have sought to understand the ongoing transformation of public universities in terms of highlighting the governmental push towards market and market-like activities (Alestalo 1997; Häyrinen-Alestalo, Snell, and Peltola 2000; Hölttä 1995; Noden et al. 1999; Ylijoki 2003). In her study on varying ideals and practices in three Finnish university departments, Oili-Helena Ylijoki (2003) for instance, claims that researchers have indeed become entangled in academic capitalism: academic capitalism was an everyday reality in all of the departments studied. Yet, in each of them, the common trend was manifested in different ways. For example, researchers in a history department were mostly involved in competing for external research grants while a technology-oriented laboratory functioned like "a quasi-firm" (Etzkowitz 2003b) with fuzzy boundaries and intensive links to commercial development. In addition, while none of the departments was driven singularly by market-oriented values, each combined in different ways market-like behavior with traditional academic values (e.g., freedom of research) (Ylijoki 2003, 328). Quite expectedly, then, the changing funding patterns and policy priorities did not translate straightforwardly into corresponding alterations in the values and practices of the investigated university departments (Hakala et al. 2003, 141).

Enterprise University

If Slaughter and Leslie's study was mostly concerned with the dependence of universities on external funding, a study by Simon Marginson and Mark Considine focused on the changes that have taken place with respect to university governance

and management. After examining 17 Australian universities the writers conclude that there exists a general pattern – indeed, a widespread "revolution" – wherein the university is being made more like an enterprise. The top-down management models and strong executive control of the university are strictly aligned with the changes in the global economy and national higher education policy (Marginson and Considine 2000, 3-4). The authors identify several trends characterizing the transformation. These include, among others, the replacement of collegial decision-making bodies by new managerial structures that make use of incentives, targets and plans of various types and emphasize the authority of individual academic leaders, such as deans of faculties and heads of departments. A related change is the transfer of decision-making power and budgetary autonomy from the university's central administration, down the organizational hierarchy, to faculties and departments. Perhaps contrary to Ylijoki's results, Marginson and Considine also see the role of academic disciplines to be in decline owing to the establishment of interdisciplinary schools and research centers within universities. As a result, the authors maintain, the university is lurching into the economic world at the same time as its internal intellectual coherence is falling into decline (Marginson and Considine 2000, 9-11).

Compared with theories like Mode 2 knowledge production or the triple helix, Marginson and Considine do not, however, proclaim this trend a universal and uniform development. Instead, they claim it plays out differently in various kinds of universities. Old, established universities that embodied robust "academic cultures" were capable of maintaining themselves along conventional lines despite cutbacks in government funding and the emergence of new managerial models. In such institutions, scholarly cultures were self-sustaining and the disciplinary identity of academics retained salience. In spite of reformed faculties, performance drivers and private funding, collegial loyalties among academics remained strong and managerialism was willingly resisted (Marginson and Considine 2000, 193-196, 221-222). Other types of universities – those established after the Second World War and the technical institutes – were less traditionally academic, and thus more open to corporatist tendencies. They focused on industrial relations, were often strong in applied science, emphasized professional education (such as law and medicine) and had weaker disciplinary cultures. Some of the newest universities even sought to remake themselves along the lines of entrepreneurialism, which increased short-termism, reduced academic vigor and heightened dependence on external marketing (Marginson and Considine 2000, 196-202; also Clark 1998).

While the general changes Marginson and Considine describe surely apply to Finnish universities, again there are dissimilarities as regards different organizations. For instance, a study of leadership cultures in eight university departments exemplified just how differently the same legislation and similar kind of a steering system may be put into practice (Kekäle 1997). Although in Finland there

was indeed a general shift in university policy towards managerialism (e.g., individual leadership and the use of performance drivers), it materialized in very different ways from department to department: in some units there were collegial and democratic leadership cultures while in others the leadership was stronger and more individualistic. Consequently, and in line with the earlier results, one may conclude that to appropriately acknowledge the claimed transformations, one needs to maintain a strong sensitivity to the differences within and between local scientific, political or administrative practices. Before arriving at how this might be best achieved, let us further review the response the four theories have received from scholars working in various fields of research.

Reflections on the Transformation Theories

The previously discussed theories evidently capture some of the relevant trends with respect to academe's current dynamics. For instance, the university-industry collaborations that proliferate in some high-technology fields, such as biotechnology, have been demonstrated through an extensive set of studies (Blumenthal et al. 1986a; Blumenthal et al. 1986b; Curry and Kenney 1990; Kenney 1986; Krimsky, Ennis, and Weissman 1991; Powell and Owen-Smith 1998; Webster 1994). Moreover, it has been claimed that the significance of these relations has increased over time: the excitement about industrial involvement in university research should thus not be received with a *déjà-vu* attitude but rather the distinctive nature of the current situation should be appreciated (Geiger 1988).[5] Nonetheless, some of the theories – most notably the radical ones – have been regarded as fairly problematical.

Of the above conceptualizations, Mode 2 knowledge production has been subjected to the hardest criticism. In addition to the fact that it underestimates the relevance of path-dependent trajectories and the boundaries of established scientific institutions, such as universities and disciplines (Jansen 2002; Krücken 2003; Shinn 1999), its assertion that the nature of scientific research has altered has been called into question. For instance, Etzkowitz and Leydesdorff (2000, 115-116) state that Mode 2 science is not a new phenomenon at all. Referring to a dissertation by Robert K. Merton, the authors argue that about half of the discoveries in the seventeenth century had their origins in attempts to solve the problems of navigation, mining and so forth. Therefore, rather than being a novel phenomenon, the Mode 2 type of research is the original form of science prior to its institutionalization into the university in the nineteenth century. Similarly, Benoît Godin (1998, 470-474) referred to a number of historical studies and claimed that research has always shifted between fundamental and applied spheres. On the other hand, Arie Rip (2000, 35-36), describing the European Renaissance from the

fourteenth to the sixteenth centuries, stated that the ambivalent position of the so-called professors of secrets, who collected recipes from different crafts and sold them to sponsors, closely resembles that of present-day biotechnologists and other scientists working in commercially important areas.

Moreover, the protagonists of the Mode 2 thesis also claim that not only have science and the university changed, but so has the whole of society: it has become transgressive, meaning that such modern categories as science, politics, culture and the market have become subject to the same co-evolutionary trends and, thus, have invaded each other's domain (Nowotny, Scott, and Gibbons 2001, 4). The problem with such a perspective is that it leaves no room for science as a distinct social and cultural sphere: the demarcation of science from non-science seems to vanish completely, a position which was recently challenged both empirically and theoretically (Gieryn 1999; Krohn and van den Daele 1998).

This sort of counter-evidence lends support to the judgment that the Mode 2 thesis overstates the change science has undergone while simultaneously dismissing relevant earlier literature and empirical evidence. In Aant Elzinga's (2002, 3; also Albert 2003) view, the model is "one-eyed and reductionist" since it focuses mainly on "a relatively small – albeit significant and dramatically changing – domain of the vast diverse landscape of science in society." Johan Muller (2000), on the other hand, claims that it over-dichotomizes the evolution of science, "presenting it as two discrete ideal types that probably never exist in their pure form in the real world."

The Mode 2 thesis also has a close affinity to the language of science and technology policy and political neo-liberalism (Häyrinen-Alestalo 1999; Krücken 2002; Shinn 2002; Weingart 1997). For instance, Terry Shinn (1999, 172) maintains that the Mode 2 argument easily links to "a partisan political agenda and ideology" rather than "a serious-minded history and sociology." Hence, it seems that "The New Production of Knowledge" is neither an empirical study nor a general sociological theory, but rather a diagnosis-of-the-era type of theorizing (Miettinen 2002; Article IV).[6] As discussed by Noro (2000), descriptions of the spirit of the age usually are normative and seek to generate new topical insight. Therefore they may be effectively used as conceptual devices for policy-making, as recently illustrated by Mode 2 employed in the context of South African higher education policy (Kraak 2000a). This strong affinity of Mode 2 with the diagnoses of the era is further substantiated by Gibbons's (1994, viii) avowal that the style of the work was largely set by a governmental agency, namely, the Swedish Council for Planning and Coordination of Research, and that the book should be taken as a "heuristic for those seeking to understand what is changing in the sciences and what this implies for the future of our principal knowledge producing institutions."

Whereas the Mode 2 thesis has over the years been expanded into an increasingly more abstract and encompassing theory concerning the place of knowl-

edge production in the post-modern age, the other theories have a stronger – albeit occasionally debatable – empirical footing. For instance, the triple helix of university-industry-government relations is clearly on its way from the complex systems-theoretical model, which informed the theory in its early stages (Leydesdorff 1996; 2000), toward a more empirical foundation. In Etzkowitz (2002b, 141-142) at least, ever more attention is given to case studies of different U.S. research universities, such as Stanford and MIT. The triple helix is not, however, intended to be an empirical description only; it involves a strong political aspect as well, as many scholars seek to develop the innovation systems of their particular countries by using the idea as a normative model (de Castro et al. 2000; Sutz 2000). Etzkowitz (2003a, 334) himself occasionally refers to the triple helix as providing a useful framework for knowledge-based economic and social development. In this respect, the triple helix clearly represents just another version of diagnosis-of-the-era theorizing. On the other hand, its theoretical status seems to be simultaneously wavering. As Elzinga (2002, 15, 25) notes, a growing body of empirical research seems to counterbalance and neutralize some of this normative tendency and over-theorization. Therefore it seems that the triple helix model combines an increasing amount of empirical evidence, overt normative orientation and a complex systems-theoretical underpinning, thereby making its theoretical status unclear. How it succeeds in maintaining an equilibrium between these three distinctive components still remains to be seen (Shinn 2002, 605).

Compared with the above two theories, academic capitalism and the enterprise university look like rather conventional research theories. Both of these theories rely on extensive empirical research on the grounds of which more general theoretical claims are being made. As such, they clearly stand in contrast to Mode 2 or the triple helix. This can be nicely illustrated by comparing the arguments that propose an institutional change of the university. First, there is a strong thesis according to which evolvement into an entrepreneurial university is an inevitable, global phenomenon that encompasses different kinds of universities and a whole variety of countries. Etzkowitz (2002a; 2003b; Etzkowitz, Schuler, and Gulbrandsen 2000) substantiates this statement by chronicling the histories of various U.S. universities and holds that other institutes worldwide are emulating these models. On the other hand, more cautious claims are put forth by Slaughter and Leslie as well as Marginson and Considine, who make it quite clear that the validity of their arguments is restricted in various ways. In this perspective, which clearly represent the research theory, various types of universities around the world seem to adopt dissimilar developmental pathways towards entrepreneurialism in so far as such changes are sought for. It seems to be the case that the old established universities with a comprehensive coverage of scientific disciplines and units possess the strongest willingness to resist managerial and corporate influences (Marginson and Considine 2000). The data from the United Kingdom by Rosemary Deem and Rachel Johnson (2003; also Clark 1998, xiv), in addition

to the Canadian study by Mathieu Albert (2003), backs up this conclusion: only a minority of academics was shown to be highly-oriented towards financial activities, such as consultancy or spin-off companies.[7] Thus, when assessed from such perspectives, the stance taken by Etzkowitz seems less justified; he even admits that his argument is based on a convenience sample only, rather than any representative collection of universities (Etzkowitz 2002, 117-118).

In summary, the four theories are substantially different from one another as regards their theoretical statuses. While the Mode 2 thesis clearly represents diagnoses of the era, the nature of the triple helix model is opaque. It involves aspects of all three types of sociological theories, namely, the diagnosis of the era, the general sociological theory and the research theory. Finally, academic capitalism and the enterprise university can be best classified as research theories with the most direct and convincing linkages to empirical analysis. Nevertheless, all of these also work towards the claim that a new kind of science and university is emerging. These new universities, and new modes of science, are always a kind of generic abstraction constituted by more or less convincing empirical material. But as such, they all too easily obscure the intrinsic dynamic internal variance and contradictory tendencies present in scientific practices and universities. Further empirical research is therefore needed. In the present study, I shall make use of analytical concepts and ideas, research theories if you wish, developed by scholars often gathered together under such convenient rubrics as the constructivist sociology of science (Clarke and Fujimura 1992; Lynch 1993) or science as practice (Pickering 1992). On such grounds, I claim, interesting results may be achieved which would further contribute to the discussion of changes postulated to be occurring in science and the university. For the present purpose, the general claims made by the reviewed theories should thus be broken down into a set of more specific research questions. How do we, for example, conceptualize the intrinsic development of a local research program? How do we analyze the relationships between research approaches within a university department? And, what are the dilemmas that arise from an attempt to hybridize academic research and teaching with commercial development? For some analytic insight into treating questions like these, let us next turn our attention to literature in science and technology studies where these kinds of questions have been addressed.

Research Questions Specified

For a number of years, researchers in science and technology studies have stressed the need to understand science as a local and practical accomplishment. This is to say that instead of studying science as knowledge, scholars have moved toward investigating actual scientific work practices in diverse cultural contexts (Clarke and

Fujimura 1992; Pickering 1992). Drawing from many analytic perspectives, such as symbolic interactionism (Fujimura 1996), ethnomethodology (Lynch 1993), pragmatic realism (Pickering 1995), cultural-historical activity theory (Miettinen 1998; 1999; Saari and Miettinen 2001) and actor-network theory (Latour 187), this social scientific research "bandwagon" (cf. Fujimura 1988) has stressed the dynamic nature of scientific work. Science, in this view, is a heterogeneous assemblage of whatever cultural elements scientists use to get their work done. This includes, for instance, acquisition of research materials, tools, instruments and methods through different channels as well as their adjustment to specific tasks they are used for (Clarke and Fujimura 1992). Studies on scientific practices have also drawn attention to the contingent character of the research process. That is, experiments may fail as a result of unanticipated resistance created by research materials thereby enforcing a compromising of goals and a revision of instrumentalities (Pickering 1995). Although some of the literature has concentrated on studying laboratory research *per se*, research on scientific practices is not necessarily bounded by the walls of the laboratory. Instead, scientific research is connected to other social activities in different ways. For instance, new instrumentalities or other societally relevant outcomes may be transferred from research laboratories to industry through collaboration networks, just as wider social problems may influence the work of scientists at the outset of their research programs.

While the present thesis is very much informed by such a perspective, it is equally important to note that I shall make no effort to integrate the various analytic orientations at any deeper theoretical or methodological level. Instead, I will apply and further elaborate on only a few concepts and ideas derived from the study of scientific practices, for the purpose of addressing some of the issues discussed by the four theories reviewed above. In so doing, I shall be engaged in a type of research that might be termed 'local and empirical historicism.' Along the broad lines of constructivism[8] I will seek to understand the trajectory of a local plant biotechnology group, including its research program, network collaboration and other interaction taking place in conjunction with its transformation into a university start-up company. Consequently, the case investigated in this study falls between short-term research projects and entire fields of science. Along with other studies that have focused on analogous trajectories (e.g., Hasu 2001; Hyysalo 2004; Saari 2003), the long time-span of the investigated research program not only highlights the complex and dynamic nature of scientific work and the centrality of instrumentalities for its progress, but also draws attention to the university organization as well as to the commercialization of the group's research results.

Of the many conceptual alternatives available for addressing scientific practices, for this study I chose to engage the idea of an experimental system. In Hans-Jörg Rheinberger's (1997) view, experimental systems are the smallest working units that scientists regularly deal with, and are comprised of two types of elements. The first is a scientific object or "an epistemic thing" that is the mate-

rial entity manipulated for the sake of going beyond the present state of knowledge. This is worked on and influenced by the second element of the system, that is, "technical things" – materials, instruments and methods of an experiment that "determine the space and realm of representation of an epistemic thing" (Rheinberger 1995, 111). I found the experimental system idea to be useful in my study on the internal dynamics of a local biotechnological research program, as it provided me with a loosely structured idea by means of which the interaction between research objects and tools could be comprehended. However, I was not completely satisfied with the concept, since it did not adequately acknowledge the built-in societal motive of the investigated research, that is, the biotechnology group's objective of developing a virus-resistant potato, besides adding to scientific knowledge. Therefore, and drawing from the activity-theoretical perspective (Miettinen 1998), I expanded the realm of the concept to comprise both the creation of new knowledge and the group's involvement in achieving societally useful end-products. As these were complexly intertwined during the course of the research group's work and as there was also a notable shift in the research approaches used, the studied case implied that the theoretical, experimental and applied concerns of research work were interconnected. Therefore, I specified my first research question to read as follows: What were the dynamics of these three dimensions of research work and how did the experimental systems change? (Articles I and IV.)

As noted above, focusing on practice in science is not only useful in investigating the intrinsic dynamics of research work but also offers benefits in understanding the relationships between scientific practice and external stakeholders. Emphasizing the productive, dynamic and contingent nature of action shifts the analytic focus away from formal organizations and institutions to more specified occasions, to see how local research programs become interlinked with their organizational contexts. For instance, Timothy Lenoir (1997, 61) has looked at scientific disciplines as non-monolithic dynamic structures where diverse "disciplinary programs" compete within the political economy of institutions and organizations constituted by neighboring disciplinary fields. This sort of approach (also Cambrosio and Keating 1983; Knorr Cetina 1999), which refuses to see disciplines or organizations as either stable entities or mere cognitive structures, is useful in the sense that it facilitates searching for mundane ways in which lines of research that make up a discipline, or a department, compete, conflict and collaborate with each other. In this thesis, such a standpoint has led me to tap into the idea of scientific social worlds.[9]

In order to study work practices and work organization, symbolic interactionist scholars have developed the social world approach (Clarke 1991; Strauss 1991). Akin to the point made on the disunity of science (Galison and Stump 1996; Knorr Cetina 1999), this perspective does not represent science as a unitary social

institution characterized, for instance, in terms of Mode 2 knowledge production or entrepreneurial science, but conceives of disciplines, specialties and research traditions as interactive groups of scientists who have mutual commitments to certain activities. Such specialties, or scientific social worlds, share various technologies and other resources to achieve their goals (Clarke and Star 2004) such as the molecular biological approach to the genetics of cancer (Fujimura 1996). In my study, the social world approach proved productive in the sense that it cued me to investigate the relationships between the different research approaches present in the university department where the studied research group worked. As there were strong tensions between these approaches, I came to phrase my second research question as follows: how did the disciplinary worlds conflict with each other in the Department of Plant Production? (Article II.)

Another perspective on the university organization present in this study relates directly to the above-mentioned thesis according to which the entrepreneurial university is currently emerging. This position holds that the traditional research university is in the process of being globally transformed into a new type of institution where two distinctive cognitive modes – those of extension of knowledge and capitalization of knowledge – are becoming increasingly integrated into a compatible relationship. In examining this thesis, the concept of boundary work provided me with valuable conceptual insight. Elaborating upon Thomas Gieryn's (1999) earlier work on the boundaries between science and non-science, I transplanted the concept from the rhetorical realm into the organizational and administrative context of the University of Helsinki. As the investigated research group made an attempt to hybridize its academic work with the commercial development of the newly-founded spin-off company – a position straddling both university and business world typical of a "hybrid firm" (Etzkowitz et al. 2000) – a heated debate arose within the university. In this debate, pre-existing administrative rules and regulations concerning commercial activities within universities were locally recreated (Gregg 1999) to facilitate the demarcation of the university from corporate development. I therefore formulated my third research question in the following way: how was the boundary between public university activity and private business activity reconstituted in the context of daily university administration? (Article III.)

Research Data and Methods

The Applied Plant Biotechnology Research Group as an Object of Study

The focus of this thesis is on the trajectory of the Applied Plant Biotechnology Research Group, which operated in the Department of Plant Production at the University of Helsinki, Finland, from 1990 until the autumn of 1999. In late 1999 the group transferred its academic research projects to the University of Helsinki's Institute of Biotechnology, just as the group's spin-off company was establishing its facilities under the auspices of the university's business incubator. By the end of the year 2000, the group had brought all of its academic projects to an end and thereafter continued solely as a business enterprise. In what follows, I present a schematic overview of the Applied Plant Biotechnology Research Group and its research from 1990 to 2000. The purpose of this outline is to orientate the reader to the group's work, rather than provide an analytic account of any kind.

Like molecular biology laboratories in general (Knorr Cetina 1999, 20), the Applied Plant Biotechnology Group pursued research on multiple subjects during the 1990s. In connection with research funding, the group's research was organized as 'projects' with particular 'project numbers.' In research plans and reports, such projects were further characterized by their respective budgets, personnel, objectives, hypotheses, methods and phases, as well as outcomes and results. In addition to the fact that these projects showed a contentual progression in time, they were also intertwined in various ways, giving rise to what was in the beginning called the "Potato Biotechnology Program" and, later, the "Applied Plant Biotechnology Program" (cf. Lynch 1985). Depending on the particular point in time when such descriptions were given, this evolving program was also said to involve a whole variety of larger and smaller research projects that occasionally exceeded formal organizational boundaries. Therefore being unable to give a definitive account of the entire research program, I will schematically depict only what I came to understand as its major lines of work.

The program had its origin in research accomplished at the AFRC Rothamsted Experimental Station in Harpenden, England. In the 1980s, the Rothamsted Station was among those places where new cell and molecular research techniques were being developed. The work involved, among other things, a genetic manipulation program that was engaged in the development of methods for producing genetically modified crops for British agriculture. Part of the program was to determine whether the production of cultivated potatoes could be improved using virus-resistance genes derived from a wild potato species (Rothamsted Experimental Station 1987). This research formed a starting point for the Applied Plant Biotechnology Group's subsequent work. After receiving a substantial amount of financial sup-

port from the major Finnish science-funding agency, the Academy of Finland, the group continued the potato genetic engineering research program initiated at the Rothamsted station. At the outset, three projects were started: one that sought to create a virus-resistant potato by hybridizing wild and cultivated potatoes through cell fusion technology, another seeking to characterize the virus-resistance trait in the wild potato species, and the third to determine the function of a gene – called P1 – of the major potato-infecting virus, Potato Virus Y. Later on, some of these studies were completed, of which several gave rise to further research that focused on natural toxins (glycoalkaloids) found in the potato, investigations on the starch structure of potato tubers, and the development of genetically engineered, P1-gene-mediated virus resistance in the cultivated potato.

However, the group's agenda did not concentrate solely on virus resistance in the potato but involved studies on other plants and subjects as well. For the most part, this research evolved from a project begun after the arrival of the Russian scientist, Viktor Kuvshinov, in 1992, who, together with a doctoral student already working in the group, began to develop genetic transformation methods for turnip rape and to design synthetic insect-resistance genes. In the late 1990s these studies gave rise to a number of new undertakings that concentrated on improving turnip rape feed quality and developing genetic transformation methods for various crop plants. Other new projects that sprouted from the same research involved preventing transgenes from escaping from plants into nature, and developing a system for producing medical and industrial proteins in plants. These two major lines of research are schematically depicted in Figure 2 (see p. 34).

In an attempt to clarify just how the group's social composition evolved, I shall provide three glimpses of its staff – in 1991, 1996 and 2000 (Table 2). The information is derived from research planning and reporting documents produced by the research group and submitted to the Academy of Finland and Biocentrum Helsinki, a large umbrella organization for biotechnological research at the University of Helsinki.

	1991	1996	2000
Group leader	1	1	1
Post-doctoral scientists	1	3	3
Doctoral students	3	7	9
Undergraduate students	2	7	-
Technicians	-	-	3
Total	7	18	16

Table 2. The social composition of the Applied Plant Biotechnology Group in 1991, 1996 and 2000.

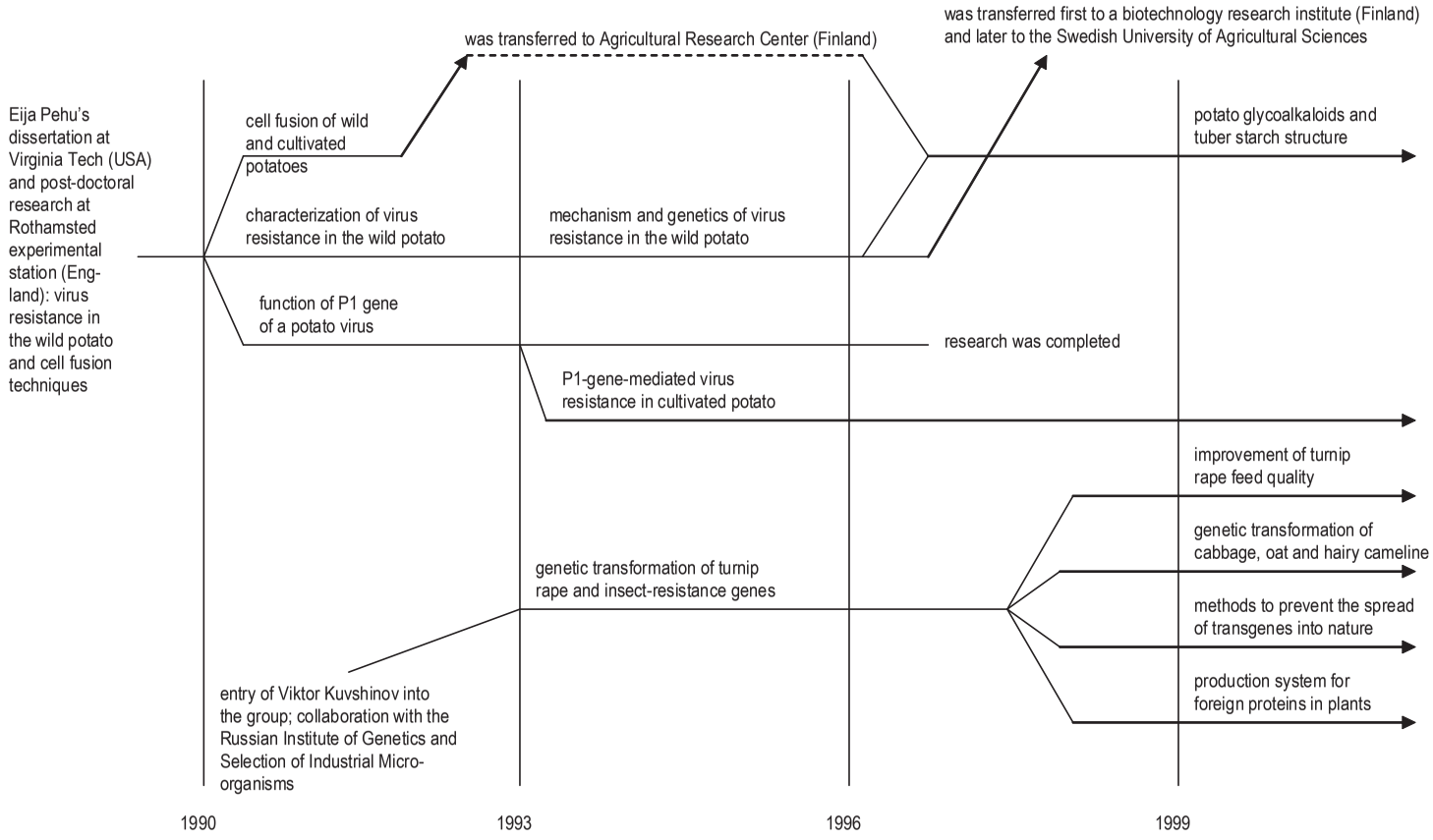

Figure 2. Major research projects of the Applied Plant Biotechnology Group, 1990-2000. After 1999, the projects involve both academic research and commercial development.

In 1991, each of the group's three projects formed the basis of a doctoral dissertation, pursued by a graduate student. Correspondingly, smaller parts of the overall research program formed topics for several Master's theses. Even though there was also a post-doctoral scientist in the group, all studies were supervised by the research group leader, Dr. Eija Pehu. In 1996, Dr. Pehu had become a full professor in her department. Some of her doctoral students had also finalized their dissertations, thus raising the number of post-doc's in the group to three. Research projects were no longer supervised by the research group leader alone, as the post-doctoral scientists had taken up some supervisory roles as well. Reflecting its strong and central position in the Department of Plant Production, the group now consisted of all 14 doctoral and undergraduate students. By the end of 2000, the group had already founded its company and was on the wane as a body of academic researchers. Nevertheless it still had many doctoral students, but, reflecting its metamorphosis into a business enterprise, no longer involved undergraduates, who seem to have been replaced by technicians.

This process of the group's transformation into a biotechnology company was sociologically interesting because it brought to light the diverse ways in which academic research is connected to the applied and commercial use of research results and the problems ensuing therein. Yet, the fact that these issues became so vital for the present study was not anticipated in advance. Instead, the specific research topics I chose to focus on evolved simultaneously as the group's development took shape. As I began my work, I was oriented towards examining the laboratory research carried out by the group and its embryonic industrial and academic collaboration networks. These were to be investigated by tracing the history of the group's research and by ethnographically observing its current laboratory experiments. At this stage, no information on the forthcoming start-up company was yet available and, in fact, its establishment was not topical issue for the group. So, at the same time as I collected historical data on the group's research work between 1990 and 1997, I also began the ethnographic observation of a project on potato glycoalkaloids and tuber starch structure. To my surprise, in the summer of 1998, the group leader told me that the group was in the process of starting up a new biotechnology company. I was immediately interested in this, as it seemed that besides industrial collaboration, I would be fortunate enough to have an opportunity to analyze the materialization of a spin-off company. Unfortunately the project on potato glycoalkaloids was sidetracked in this respect, and I subsequently agreed with the group leader that I would also begin on ethnography of the new project, which had direct commercial and industrial connections.

However, although the necessary confidentiality agreement was signed, my hopes proved premature – not so much because the commercial research did not come to pass as expected, but because the establishment of the firm contributed to the emergence of a crisis between the group and the university adminis-

tration. More specifically, as the tensions related to commercialization gathered momentum, my relationship to the group changed as well: clearly, I came to understand that my presence in some of the group's working meetings was no longer welcomed. This and the fact that the group's researchers began to hold back technical information related to their concurrent research made it plain to me that an ethnographical study of the group's commercial project was not feasible, regardless of its importance to my initial interest. Therefore, given the delicate circumstances, in order to avoid undercutting my opportunity to go on with my dissertation project, I changed my plans, backed away and decided to monitor the commercialization process and the evolving institutional dilemmas more remotely. Consequently, the focus of my research wound up being different from what I had at first anticipated: my emphasis shifted from the group's research practice (Article I) to involve the disciplinary tensions in the Department of Plant Production (Article II) and the administrative and political issues related to the viability of the start-up company within the university organization (Article III). Finally, I summarized and discussed these results with respect to more general issues in the contexts of the triple helix and Mode 2 research theses (Article IV).

Types of Data and Data Collection

A large body of research material was collected between 1997 and 2000. The data were assorted into the following categories: 1) interviews, 2) publications, 3) research plans and reports, 4) official documents, 5) correspondence, 6) ethnographic observation, 7) public lectures and seminars, and 8) feedback materials. Most importantly, the data provided analytic access to the various aspects of the group's research practice, the relationships between scientific approaches at the Department of Plant Production, and the relevance of research funding from the viewpoint of the group's development, plus administrative problems related to the group's spin-off company. The complete set of research material is presented in Table 3.

As is obvious, not all of the data presented in Table 3 was used equally in this thesis. Instead, some data were systematically analyzed while others provided useful background knowledge or contextual information of the group's trajectory only. In each of the original research articles that comprise this thesis (i.e., Articles I, II and III), the analyzed data sets are more closely described. In what follows here, I discuss each category of the data in detail, putting special emphasis on those materials that were most important from the point of view of this thesis.

Interviews. Interviews were used as research data in all four articles. Taken together, they totaled 79, and involved 46 individuals. As regards the investigated research group, interviews centered especially on those researchers conducted or

Type of data	*Number*
1) Interviews	79 in total
- with members of the research group (11 persons)	33
- with the group's partners and colleagues (17)	22
- with university administrators and academic leaders (11)	15
- with representatives of research funding agencies (4)	4
- with representatives of business enterprises (5)	5
2) Publications by the research group	129 in total
- theses	9
- scientific articles	38
- patents	4
- non-scientific articles	78
3) Research plans and reports by the research group	119 in total
- research plans	22
- research grant application forms	31
- research funding decisions	22
- research reports to funding agencies	30
- other documents related to research planning and reporting	14
4) Official documents	92 in total
- public laws and statutes	4
- collective bargaining contracts	1
- university rules and regulations	2
- research group's written contracts	14
- documents concerning the group leader's professorial appointment	43
- other documents	41
5) Correspondence	301 in total
- research group – research partners	16
- research group – university licensing company	9
- group leader – university administrators and academic leaders	75
- group leader – students and faculty members	11
- among university administrators and academic leaders	78
- department chairman – students and faculty members	33
- department chairman – CEO of the research group's firm	19
- department chairman – police	2
- miscellaneous letters and emails	22
- drafts of letters and emails by the department chairman	12
- handwritten notes by the department chairman	15
- memoranda related to correspondence	9
6) Ethnography of Potato Glycoalkaloids and Tuber Starch Structure Project	18 in total
- videotaped and tape-recorded project meetings	16
- videotaped and tape-recorded observation of laboratory work	2
7) Public lectures, seminars etc.	13 in total
8) Feedback on research articles from those investigated	66 in total
- email feedback and related communication	62
- audiotaped face-to-face feedback	4

Table 3. The research data used in this study.

were otherwise associated with the potato virus-resistance studies. I chose this area because it was the original focus of study of the investigated research group, forming the major part of the group's activities in 1997. As I was initially interested in the relationship between the group's research and wider society, I also anticipated that focusing on these studies only might yield sociologically useful data. Since collaboration networks of various sorts were an important mechanism which connected the group's local experiments to the world outside the laboratory, I also interviewed some of the group's partners. These included scientists from the University of Helsinki, the Agricultural Research Center of Finland, Boreal Plant Breeding Ltd. and Cornell University. In addition, to appreciate the group's position within the Department of Plant Production I also talked to colleagues of the group members. Being aware of the disciplinary tensions existing within the department I interviewed other researchers and teachers from diverse scientific backgrounds. By so doing, I better understood the position of the studied research group within the concurrent social ecology of the department. Once it became evident to me that the research group was also starting up a business, I extended my interviews to include relevant university administrators involved in the commercialization of the research results. I also included academic leaders from the university, that is, present and former chairmen of the department as well as deans/vice-deans of the Faculty of Agriculture and Forestry. Representatives of the major Finnish science and technology funding agencies were also talked to in addition to the chief executive officer of the group's emerging firm and three managers of the university licensing company.

The interviews were conducted in several rounds between 1997 and 2000, so I was able to focus the discussions on various aspects of the studied research group's work. The interviewing itself was basically an iterative process starting with rather general questions. In the beginning I concentrated on learning the history and concurrent details of the experimental research the group pursued.[10] At this stage I also sought to place the group's research in the context of the department's research tradition and Finnish agricultural science in general. Later, as I gained more understanding of the group's work and its trajectory, and as my analyses began taking shape, the general themes were supplanted by minute discussions on specific dimensions of the group's research, its relationship to the university institution and its alignment with external stakeholders of various sorts. In this stage, as the establishment of the start-up company became a central issue for the group itself, I also chose to focus attention on various matters related to this development. All in all, the topics addressed in the interviews ranged from details of the experimental research to the changes in Finnish science and technology policy, and from the history of agricultural science in Finland to the latest trends in plant science worldwide. Nonetheless, the greatest attention was given to the group's work, collaboration networks, connections with Finnish R&D policy and changing position within the university organization.

Typically, the interviews were either loosely structured or non-structured and lasted from one to two hours. Basically, when talking with the interviewees, I had prepared an outline for the discussion but did not adhere to it too rigorously. Instead, I let the interviewees shape the course of the discussion and take up any issues they regarded as important from their particular perspective. I tried to ensure that we covered issues that I believed were relevant to the topics investigated. Discovering what these might be was greatly facilitated by my discussions with the many persons representing different academic and administrative positions. This variety of informants provided a multifaceted and sometimes even ambiguous image of the issues and of the events as they occurred. For my part, then, I did not seek to gloss over any cleavages but tried instead to retain them as part of my analysis. Fortunately, I succeeded in maintaining friendly and confidential relationships with the interviewees.

Publications. In addition to interviews, research plans and reports, the scientific publications by the research group members formed, a major source of information concerning the group's potato virus-resistance studies reported in Article I. These were obtained from public libraries through the CVs of the researchers. The scientific articles constituted a series of papers that reported successive phases in the progress of the group's research program. What was characteristic of this data was that it tended to present the scientific work in an accomplished and rationalized manner. The various sorts of contingencies, glitches and *ad hoc* decisions present in any actual research project were simply omitted. This defect was partly addressed by acquiring and using other kinds of data, such as interviews, research plans and reports, as well as correspondences. Compared with scientific papers, non-scientific articles were used less in the study. However, a few of these proved important from the point of view of understanding the vexed relationships between various research approaches present in the Department of Plant Production (Article II). All in all, the non-scientific articles comprised papers in professional journals informing about the group's research results as well as opinion pieces in public debates that the research group members were involved in. Most of the articles belonging to this category were the interviews of the group members and news items about their research published in daily newspapers. With the exception of the few articles mentioned earlier, these were not analyzed.

Research Plans and Reports. As stated, research plans, applications and reports constituted an important data set applied in Article I. These documents, found in the personal files of the research group members and in the archives of the Academy of Finland, were an important supplement to the scientific articles and interview accounts. The research plans and the annual reports to funding agencies constituted a cohesive series of documents tied together by unique names and project numbers, and they raised my appreciation of the organization and the advancement of the group's research. The image so attained was also contributed to by research proposals that were not funded and experiments that had

failed. By using such knowledge as points of reference in interviews, and by combining the resultant understanding with the scientific publications, I managed to cope with the bypassing of unrealized projects and unsuccessful experiments in the retrospective accounts; at least some of the twists and turns of the research practice were thus recovered and subjected to analysis. The correspondence between the research group and its partners, often annexed to the research proposals, was also useful in this respect. The fact remains, however, that Article I still tends to portray the group's research as being more determinate than was actually the case.

Official Documents. This set of data consists of different kinds of official documents that formed important background knowledge applied especially in Article III. These documents were identified by the interviewees and provided me with the formal regulatory framework under which all of the university activities were performed. Formal rules and regulations usually do not embody any immediate guidelines for their local practical application (Gregg 1999). This is why I did not take their relevance to the case at hand for granted. Instead, I further probed their significance and local usage through interviews and available correspondence. Another important set of official documents was comprised of written formal contracts of the research group including agreements of collaboration, contracts concerning intellectual property rights and a loan contract for research instruments. Two of these proved particularly central to the analysis reported in Article III: 1) the loan contract for research instruments agreed upon by the group's company and the Department of Plant Production and 2) the agreement of collaboration signed by the research group, its company and the university's biotechnology research institute. The relevance of these contracts was highlighted by the interviewees or became apparent in the extensive references to them in the email correspondence at my disposal. The latter contract was also complemented by the floor plans of the university's business incubator.[11]

Correspondence. In addition to the fact that correspondence was used as supplementary data in Article I, it formed the prime data for Article III. All of this communication (i.e., letters, email messages, drafts, or memoranda) was acquired either from the personal files of the research group leader or the chairman of the Department of Plant Production. This correspondence, produced from 1998 to 2000, mostly concerned the deteriorating process of administrating and controlling the group's commercial activities, securing the group leader's contribution to undergraduate teaching, and determining the conditions of use of the university's research instruments under the auspices of the group's spin-off company. Forming an extensive set of communication, this data proved useful in analyzing whether or not the studied group might be allowed to run a business within the university organization while pursuing academic research and teaching at the same time. Therefore it was particularly apt material to be used in studying the administrative work of recreating and controlling the fuzzy boundary between public university

activities and private business enterprise.[12] The correspondence is comprehensive as regards any major communication among the participants; even some of the face-to-face meetings are preserved through written memoranda. Nevertheless, to enhance my understanding of the analyzed process I also conducted a series of interviews with the major actors to reveal their reflections on the unfolding course of events.

Ethnography of the Potato Glycoalkaloids and Tuber Starch Structure Project. Data for this category included 16 videotaped research meetings between the research group and its partners working in the Agricultural Research Center of Finland as well as in the Departments of Physics, Chemistry and Pharmacy at the University of Helsinki. It also involved a few hours of observational data from the project's laboratory experiments. This data was omitted from the current thesis and will be analyzed later.

Public Lectures and Seminars. This data set consisted of 10 public lectures and seminars that reinforced my understanding of the group's research program. The same holds true for two tape-recorded defenses of doctoral theses as well as a briefing given by the group's company to the faculty members of the Department of Plant Production in the spring of 1999.

Thesis Feedback from the Individuals Investigated. The original research articles of this thesis were subjected to commentary and review by the actors interviewed and studied. Four tape-recorded meetings with the research group leader and the department chairman were held. I also obtained written remarks from these persons concerning my articles. In addition, I accumulated a total of 62 email messages from 17 persons where various aspects of my work were treated and discussed. This feedback, which proved useful in better understanding the investigated phenomena, will be considered more carefully later.

Data Analysis and Reporting

The research process might retrospectively be described as being composed of two major phases: first was a general orientation to the studied research group's work, in order to find out what might be sociologically interesting in it, what kinds of data were available and what they contained. At the same time, I became acquainted with relevant science studies literature and looked at what concepts or ideas might be used and discussed in my ongoing research. The second stage, then, saw me focus on the detailed analysis of the data, while simultaneously working theoretically on the concepts selected and applied. At this stage, the analytic tools were elaborated and sharpened for their intended use and the respective analyses were framed as regards the relevant themes and debates I wished to address. During this process, my preliminary understanding regarding the is-

sues relevant to the group's work altered, of course, and different articles began to take shape. This process, then, fed back to my data collection in the form of more focused questions in interviews and requests for new documentary materials. In another words, the data collection and the analysis were not theory-free, nor were they governed by a theoretical line of thought set in advance, but rather were intertwined in a complex iterative fashion during the research process; that is, my conceptual resources and research topics were interwoven (cf. Lynch 1985, 19-20; Lynch 1999).

As such, the research process resembles the methodology of symbolic interactionism as described by Herbert Blumer. In Blumer's (1986, 40-46) view, close empirical analysis proceeds in two successive phases called "exploration" and "inspection." In exploration, the analyst attempts to become closely acquainted with the unfamiliar world he or she is set to investigate. In other words, the work is directed at "developing and sharpening his inquiry so that his problem, his directions of inquiry, data, analytical relations, and interpretations arise out of, and remain grounded in, the empirical life under study" (Blumer 1986, 40). A characteristic of this exploration is that it remains flexible, with the study moving in directions not previously anticipated. At the beginning, the scope of research is broad but becomes gradually more focused as one gains a more adequate understanding of his or her particular object of research. At the same time, hypothetically useful conceptual tools evolve. The exploration at this stage turns into what Blumer calls inspection. Here the analyst casts his or her research problem in a theoretical form, defines the relationship between different aspects and instances of the data, sharpens his or her analytic concepts and forms theoretical propositions. As defined by Blumer (1986, 43), inspection means "an intensive focused examination of the empirical content of whatever analytical elements are used for purposes of analysis, and this same kind of examination of the empirical nature of the relations between such elements."

As regards Article I, which analyzes the gradual construction of a virus-resistant cultivated potato in the group's laboratory in 1990-97, the explorative phase might be said to have taken place during the initial stages of the data collection. At that stage, I sought to gain as accurate an understanding of the group's potato virus-resistance studies as possible. I asked the researchers to describe their work in detail while I examined the related articles and other research materials. Altogether, the data set used in this analysis comprised 17 interviews,[13] 24 scientific articles, 13 research plans, 14 reports, 6 theses and 4 letters. By comparing and complementing these different types of accounts with one another, and by drawing from various strands of the practice-oriented sociology of science (e.g., Knorr Cetina 1999; Latour and Woolgar 1979; Miettinen 1998; 1999; Pickering 1995), I sought to understand what the researchers were trying to accomplish through their experimental research and how the individual projects made up the Potato Biotechnology Program. In the data, I recognized that two major sets

of technologies had been used to bring about the virus-resistant potato: 1) cell fusion combined with the genetic mapping of the potato genome and 2) direct transfer of a viral gene into a cultivated potato. These two research approaches were identified by the research group leader as distinct "strategies" or "manners of thought." Aligning my research with Rheinberger's (1997) studies on protein chemistry, and further elaborating on it, I chose to refer to these two distinct experimental systems in the following manner, depending on the source of virus resistance: the wild potato system and the viral gene system. As I further investigated how these experimental systems were exemplified by my case, I also inspected how the group shifted from using cell fusion technology (the wild potato system) to the application of direct gene transfer (the viral gene system). This transition was, then, analyzed in terms of a gradual evolution characterized by network collaboration, *ad hoc* improvisation, resistance, opportunism and informal interaction.

In Article II, I was set to understand the disciplinary tensions and conflicts characterizing the Department of Plant Production during the middle and late 1990s, that is, just before the research group decided to establish its company. To get a grasp of this, I went through my interviews and selected for analysis all excerpts in which the informant spoke about the relationships between different research approaches taken by the department. I also supplemented my data with several non-scientific articles and science policy documents. In total, the data set so composed consisted of 21 interviews with 11 persons,[14] 10 non-scientific articles by the research group members and a few science and technology policy documents. I then coded the resulting interview material in a data-bound manner, that is, according to the topics of discussion treated in them. The thematic categories constituted in this fashion included, for instance, setting an agenda for the department, research methods, research objects, organizational boundaries and so forth. Then, by working with the categories and aligning the analysis with my knowledge of the social world perspective (Clarke 1991; Strauss 1991), I ended up with an analysis which described the complex organizational ecology of disciplines under the auspices of the university department.

Article III investigated an attempt by the group to operate as a research group-firm hybrid entity within the University of Helsinki in 1998-2000. This undertaking was then used as a touchstone for the question of whether the so-called entrepreneurial university (Etzkowitz 2002b) is a viable mode of activity in the context of a traditional European university. In making a stand on this, I applied and elaborated on the ideas of boundary work (Gieryn 1999), accounts (Scott and Lyman 1968) and local re-creation of rules (Gregg 1999). Using such ideas, I made a case for the piecemeal construction of the line between the group's academic work and its corporate activity in the confines of the university. The major data for the article, coming from correspondence provided by the research group leader and the chairman of the Department of Plant Production, was comprised

of nearly 250 chronologically arranged individual letters, email messages, drafts and personal memoranda. I discarded from the data set any documents that were not linked to the relationship between the university and the group's company. The resulting data consisted of a total of 184 documents. After thematically coding the documents according to their content, I went through my interviews and chose speech segments that were related to the issues embodied in the pieces of correspondence. The total number of interviews was 17. I also augmented the data by adding 10 official documents.[15] Thus by coding and investigating these data, I came up with a number of stakes (Gieryn 1999, 15) that were at risk while the boundary between the corporate and university activity was contested. I also illustrated how the research group-firm hybrid entity was finally deconstructed through ongoing boundary work carried out by the administrators.

Compared with the previous three papers, Article IV was not an original piece of empirical research. Rather, it mostly summarized, restated and supplemented the central findings of the above-mentioned studies. In addition, the paper compared and challenged two generic theoretical models, the triple helix of the university-industry-government relations (Etzkowitz and Leydesdorff 2000) and Mode 2 knowledge production (Gibbons et al. 1994). This was done by taking the investigated empirical world as the primary source of theoretical insight: the two theories mentioned above were criticized for their tendency to gloss over several important problem areas that were related to commercial-oriented university research. In exemplifying these, I also drew some empirical detail from a few interviews that had not been previously used. By so doing, I was able to discuss an unsuccessful attempt to transfer the virus-resistant cultivated potato from the research laboratory to agricultural practice via industrial collaboration, a topic that was not included in the other three articles.

In summary, the analysis constituted the iterative processes of investigation during which I moved from rather general and hypothetical ideas towards a more detailed and conceptually informed understanding about the group's longitudinal development, as a whole, and the specific research topics analyzed, in particular. The heterogeneous sets of documentary data were combined and compared in various ways. Moreover, they were juxtaposed with relevant speech segments derived from interviews. By so doing, a relatively detailed understanding about the progress of the group's research was achieved.

During the early phases of the analysis I was mostly preoccupied with reading my research data – whether interview transcripts or documents – from a perspective that Silverman (2000, 122) calls "a realist approach" conventional in the humanities and social sciences (Watson 1998). That is, I took research documents and respondent's answers as describing external facts and events, such as experiments performed in the laboratory. While acknowledging the problematical nature of such a reading – the fact that documents and interview accounts are always reflexive[16] to the circumstances of their creation and use (Lynch 2000, 34)

– my choice might be justified by the fact that, as Silverman (1993, 108) notes, the reading one gives to the data is relative to the research task at hand. As regards Article I, I sought to understand the trajectory of the research group's potato studies that had taken place a few years earlier, therefore having no access to the actual research practice of the group and being forced to rely on retrospective accounts only. Later I became more clearly aware of the limitations of such a reading and began to recognize the multi-voiced character of the analyzed developments. For instance, I addressed the university department where the group worked in terms of conflicting social worlds (Article II) and focused on the incompatible interpretations from the actors involved during the group's attempt to fuse its corporate development with the university activities (Article III). In other words, I gradually moved from texts as more or less transparent windows on external 'reality,' towards a better appreciation of the disparate, lived 'versions' of it.[17] The individual articles, their major research problems, the main data sets and key conceptual resources are summarized in Table 4 (see p. 46).

Validity of Research Results

I sought to validate the results in various ways during the research process. This involved collecting a large and diversified body of research material, as well as using and combining the different sources of data (i.e. triangulating, see Silverman 1993, 156-158). In each of the articles I aimed at making the processes of data collection and data analysis more or less discernible. As regards Article I, I used interviews, scientific articles, research plans and reports as well as correspondence to reconstruct the early stages of the group's research on potato virus resistance. In doing so, I looked for answers regarding how the group's experimental research approach changed, and how the theoretical, experimental and applied concerns of the research work interacted. Because no ethnographic data on the early stages of the group's work was available, my analysis remained retrospective. Despite the fact that it did, indeed, pay close attention to the many contingencies at play during the studied research, the narrative tends to portray the work as more determinate than was actually the case. Another shortcoming of Article I was my limited emphasis on the epistemic dimension of the research investigated. Due to my examination of the group's work in terms of evolving lines of experimental research activity, I slightly neglected the conceptualization of the relationships between epistemic and applied objects of research. While some mutual interactions between these are indeed empirically described, their interconnections were left somewhat open in theoretical terms. For instance, I did not address in detail how the practical usefulness of the research was intellectually built into the theoretical research problems, nor did I discuss my findings in relation to Bruno Latour's

Data	*Research Problems*	*Data*	*Main Concepts and Ideas*
I Constructing Objects and Transforming Experimental Systems	How did the group shift from using the wild potato experimental system to the application of the viral gene system? What were the dynamics of the theoretical, experimental and applied concerns of research during the course of the group's work?	- Interviews with research group members and their partners - Theses and scientific articles - Research plans and reports - The group members' correspondence with their partners	- Experimental system - Object-oriented collaborative network
II When Disciplinary Worlds Collide: The Organizational Ecology of Disciplines in a University Department	How did the disciplinary worlds conflict with each other at the Department of Plant Production?	- Interviews with research group members, their colleagues, university administrators and academic leaders - Non-scientific articles - Science and technology policy documents	- Scientific social world
III On the Possibility of a Research Group-Firm Hybrid Entity	How was the research group-firm hybrid entity practically dissolved? How was the boundary between the university and the spin-off firm drawn?	- Correspondence between and among the research group leader, faculty members, university administrators and academic leaders - Interviews with the research group leader, university administrators and academic leaders - Laws, statutes, rules and regulations - Contracts of the research group - Floor plans of the university's business incubator	- Entrepreneurial university - Boundary work - Accounts - Local recreation of rules
IV Reconsidering the Mode 2 and the Triple Helix: A Critical Comment Based on a Case Study	How accurate are the Mode 2 knowledge production thesis and the triple helix model from the point of view of the empirical results of the case study?	- Summary of the empirical results achieved in the three previous papers - Interviews with research group members and representatives of the university licensing company	- Mode 2 knowledge production - Triple helix of university-industry-government relations

Table 4. The articles, research problems, data sets and main concepts and ideas applied.

(1983) idea about using laboratory research to solve problems present in wider society.

Article II also rested primarily on retrospective accounts given in the interviews. Compared with Article I, however, it better acknowledged the fact that reality is amenable to multiple and often incommensurable interpretations. In this way, I came to address the ways in which the university department fragmented into competing disciplinary worlds. While such an analysis succeeded in portraying the major differences and conflicts between the disciplines under investigation, the research data did not allow me to investigate precisely how each of the disciplinary worlds was constituted *per se*. This might be only a minor defect, however, as studies on the formation of new scientific social worlds exist in the research literature but analyses of how these interact under the auspices of formal organizations are much more limited, if not non-existent.

With respect to Article III, my research data and analysis are apparently the strongest. In addition to the interviews conducted at the time when the emergent research group-firm hybrid entity was dissolved, I was also fortunate enough to have an extensive set of documents, email messages and letters at my disposal. Interestingly enough, such data allowed me to move away from regarding documents as mere mirrors of reality and to begin to acknowledge their actual use as a part of an organizational setting, as suggested by Paul Atkinson and Amanda Coffey (1998, 47). I believe, therefore, that the complexities present in the work of constructing the boundary between the activities of private business and the public university are well preserved. Apart from the specific case example analyzed, I also sought to question the strong claim made by Henry Etzkowitz according to which universities all over the world are becoming similar to a few entrepreneurial institutions found in the United States. While my paper is, as far as I can determine, the sole empirical analysis directly challenging the Etzkowitz thesis, there is evidence pointing in the same direction in other studies as well, as I indicate in the article.[18]

In the absence of ethnographic data and analysis, I found my way to what Steinar Kvale (1996, 244-248; also Silverman 1993, 159) called "communicative validity," that is, testing the validity of the analysis through a discourse with those investigated. Such dialogue proved crucial in the sense that it formed a testing ground for considering whether I had understood and depicted the analyzed processes appropriately and accurately. With regard to the technicalities of scientific work, this is not self-evident, as observed by Michael Lynch. He has pointed out that the descriptions provided by scientists of their particular experiments and theoretical models can be "extremely difficult to understand and even more difficult to assimilate into historians' or social scientists' narratives" (Lynch 1993, 80). These problems were evident in my study as well, and only gradually was I able to understand the examined research program and its development as a social and technical process, simplify the overwhelming detail, and arrive at a somewhat accessible social scientific re-conceptualization.

Further, these kinds of difficulties were not restricted to the experimental work alone but were also present in the social complexities concerning the disciplinary conflicts in the department and in the operation of the group's company under the auspices of the university. In these regards, my work greatly benefited from the opportunity to subject my preliminary analyses to commentary and criticism from those investigated: through such feedback, a more accurate understanding of the examined courses of action was achieved. Such discussions also increased my understanding of the differing and often incompatible versions of the analyzed processes given by those involved. At times, this working method also resulted in my obtaining new relevant documents or otherwise made me more clearly aware of the actors' reasoning as regards the analyzed processes. Of course, my interpretation of situations was not always consistent with those of the persons investigated. In cases of such difference, the criticism from my informants made me check the accuracy of the account I had given with respect to the data, and alter it if necessary.

The manuscripts of the articles were also discussed and criticized in the regular meetings of our research group operating at the Center for Activity Theory and Developmental Work Research and, to a lesser degree, in the post-graduate seminar of the Department of Sociology, University of Helsinki. The scholarly communication that took place on these occasions was essential in directing my analytic process and scientific writing. Many conceptual ideas originated from these discussions, and for me the opportunity to present my papers to colleagues who knew my case example was a convenient way to test the feasibility of my ideas. Finally, before the papers were published they went through a rigorous peer review. The criticism so achieved was fundamental: it proved an essential mechanism in testing the reliability of my application of the concepts as well as in further elaborating the theoretical focus of my papers. Therefore, this scholarly feedback was constitutive to the analysis and helped to enable the intersubjectivity and validity of the final results.

Summary of Empirical Findings Achieved in Articles I-IV

In this section, I give a brief overview of the empirical results achieved in the four articles that constitute this thesis. At the same time, a few steps towards a more elaborate understanding of the proposed transformation of the university will be taken. These steps either refine or challenge ideas presented earlier in connection with the four theories described above. More specifically, I will pursue three items: first, I will describe how the studied research group simultaneously addressed theoretical problems, produced new instrumentalities and strove for useful agricultural applications. By so doing, I illustrate how the emphasis on local research practices yields a more accurate understanding of the dynamics of bio-

technological research than would be achieved through the employment of the idea of Mode 2 knowledge production. Further, the analysis illustrates how the theoretical, experimental and applied elements of the investigated research program dynamically interacted with one another and made it possible for the group to straddle academic research and industrial development. Second, I will examine how the group's research program was institutionalized within the University of Helsinki. This analysis opens up a perspective on the effects generated by the recent emphasis in science and technology policy on biotechnology and related competitive resource allocation. As exemplified by the investigated department, such changes in policy priorities may have a significant influence on the future of individual research groups and university departments. However, these do not directly translate into corresponding effects at the local departmental level but instead become entangled with many other matters. Academic capitalism, the analysis suggests, therefore constitutes just one ingredient in the complex ecology of disciplines in university departments. Third, I will challenge the thesis by Etzkowitz according to which universities worldwide are emulating the entrepreneurial model found in the United States of America. In this respect, the analysis shows how the hybrid entity formed by the research group and the spin-off company was dissolved under the auspices of the traditional Finnish university.

The Intertwining of Theoretical, Experimental and Applied Concerns in Research Work (Articles I and IV)

In this section, I address my first question related to the development of the group's research program. In studies on virus resistance in the potato, two specific experimental approaches were used by the group. First, in 1990-96, the natural virus-resistance trait was investigated in a wild potato species combined with several attempts at transferring that trait to the cultivated potato gene pool. Second, after 1993, a novel genetic engineering approach was developed by virtue of which the potato virus genome could be used as a source of virus resistance. While the previous strategy was extremely complex, and in fact non-productive from the applied point of view, the use of the latter, which partially evolved from the former, was a success: a virus-resistant cultivated potato was developed by introducing a viral gene into the genome of the Finnish potato cultivar, Pito. I conceptualized these research approaches as two distinct but closely interconnected experimental systems: the wild potato system and the viral gene system.

In science and technology studies, the concept of the experimental system has been an important analytical tool widely used and discussed.[19] It has been most systematically elaborated by Hans-Jörg Rheinberger. In Rheinberger's (1997, 28) view, experimental systems are constituted by scientific (epistemic)

objects and technical conditions. To contribute to the social studies of scientific practices, I expanded this conceptualization by using cultural-historical activity theory (Miettinen 1998) and my case example as sources of theoretical insight. Drawing from these resources, I considered the experimental systems as comprising three concerns: 1) the pursuit of a theoretical understanding of the virus-resistance mechanism in the potato, 2) the creation of a cultivated virus-resistant potato, and 3) the development of appropriate cell and molecular biological research materials, tools and methods. In practice, these were, of course, part and parcel of the same research activity and were interconnected in various ways along the sequential progression of the group's research program. This can be briefly illustrated by looking at an early phase of the group's experimentation.

When the researchers started their work, they did not know which genes in the genome of the wild potato caused the resistance effect; these had to be localized first. Therefore, the initial stages of the research program involved producing new knowledge. This was accomplished first by creating suitable plant material by hybridizing the virus-resistant wild potato and the virus-susceptible cultivated potato, and second, by using these potato hybrids as tools for localizing DNA fragments that contained the resistance genes. In this research, elaborate cell and molecular biological techniques were developed and utilized. The application object was addressed, finally, in the third phase of the experimentation. It consisted of the attempt to realize the virus-resistant potato by transferring the localized and isolated DNA fragments to the cultivated potato gene pool.

The experimental systems were not, however, pursued by the studied research group alone, but were set up through an evolving network comprising both academic and industrial partners. Each of these partners had specific instruments and know-how to be employed and tasks to be performed in the joint experiments (cf. Callon 1980; Miettinen 1999). Although multiple attempts were made to materialize a virus-resistant potato by means of the wild potato system, the experiments ultimately failed. Nonetheless, the research proved advantageous in terms of creating new knowledge: the virus-resistance mechanism of the wild potato was better understood, thus making the species a suitable model for further studies on virus movement in plants. In addition, during the course of the experiments, useful research tools, techniques and methods were developed. These later proved crucial to the group as it sought to create a virus-resistant potato using the second experimental – viral gene – system.

With respect to the viral gene system, the research turned out to be successful. As already mentioned, the group succeeded in creating a virus-resistant potato by introducing the viral gene into the genome of the Finnish potato cultivar. As such, the genetic engineering method was novel. It had grown out of two early developments, the first being the gradual accumulation of a variety of molecular biological tools and methods within the group in 1990-93. The second was the learning of a new, non-published research result from Cornell University via

informal communication channels. On these bases, the group decided, impulsively, to set up an experiment to transfer the viral gene into the potato. In the transgenic potatoes so created, an unusual virus-resistance effect emerged.

The research subsequently continued in both scientific and commercial contexts. First, the genetically engineered virus resistance was theoretically interesting and thus its mechanism became a topic for further investigation. Second, the virus-resistance effect was potentially useful in agricultural-industrial production, so it was patented. Along with the methods of its creation, it underwent further development with an industrial partner. In joint research with a Danish plant-breeding company, the interconnection between the theoretical, experimental and applied continued to exist: the applied work contributed to the fundamental understanding of the virus-resistance mechanism while simultaneously being useful from the perspective of breeding work.

In summary, various theoretical, experimental and applied concerns ran through the examined group's research program from the beginning until its end. Indeed, such a blend of objectives seems to be important, if not basic, to several fields of investigation. Previous analyses that have drawn attention to such an orientation range from agricultural science (Gieryn 1999; Kimmelman 1992; Kleinman 2003) to pharmaceutical research (Webster 1994), aerosol physics (Saari and Miettinen 2001) and industrial enzyme research (Miettinen 1998). Although this "hybrid science" (Gieryn 1999) is part and parcel of scientific endeavor, the fact that local research agendas simultaneously address theoretical problems, produce instrumentalities and strive for useful applications should not be obscured by way of adopting indistinct analytic language. This may happen if one starts speaking about "contextualized knowledge" (Nowotny, Scott, and Gibbons 2001) or "Mode 2 science" (Gibbons et al. 1994). Such a vocabulary all too easily glosses over the triple orientation of the investigated research program. While such an orientation is better appreciated by Etzkowitz (1998) in his idea of "entrepreneurial science," even this conception is too offhand: Etzkowitz mentions only passingly the existence of the three elements without actually analyzing them. In my view, their intertwinement should be more clearly acknowledged; understanding their dynamics might give us better starting points for discussing how academic research is connected to social utility and how it may, eventually, turn into commercial product development.

The Organizational Ecology of Disciplines in a University Department (Article II)

My second research question concerned the organizational ecology of disciplines within the Department of Plant Production (called the Department of Agronomy

and Horticulture in Article II) at the University of Helsinki. Operating in the field of plant production research, the department was, indeed, a complex constellation of disciplines. The following five disciplines can be identified: molecular biology, plant physiology, agronomy, horticulture and agroecology (see Table 5).

Discipline	*Area of interest*	*Unit of analysis*	*Research approaches*
Molecular biology - initiated in the dept. in 1989	- breeding novel crop plants - studying vital functions of plants at cellular and sub-cellular levels	- part of an individual plant (e.g., a gene)	- cell and molecular biological techniques - biotechnology (e.g., genetic engineering)
Plant physiology - initiated in the dept. in 1995	- studying vital functions and metabolic pathways in plants	- part of an individual plant	- cell and molecular biological techniques - biochemical analysis
Agronomy - present in the dept. since its foundation in 1896	- studying and developing cultivation methods for field crop plants	- individual plant or plant population	- field and greenhouse experiments - quantitative analysis
Horticulture - initiated in the dept. in 1992	- studying and developing cultivation methods for horticultural plants	- individual plant or plant population	- field and greenhouse experiments - quantitative analysis
Agroecology - initiated in the dept. in 1989 and expanded in 1996	- social and ecological sustainability of farming systems and wider agroecosystems	- farming system or field ecosystem in its social and economic context	- quantitative analysis

Table 5. The five major disciplines of the Department of Plant Production, their areas of interest, units of analysis and research approaches.

Although the disciplines included in the studied department shared the common objective of fostering plant production, they varied considerably in their scientific background and epistemic orientation. To begin with, they made use of dissimilar research approaches and technologies that applied genetic engineering and biochemical analysis on one extreme and quantitative study of field ecology on the other. While such differences created tensions between the disciplines, they also enabled collaboration. Thus, in a manner similar to other biological sciences (Gerson 1998; Wilson and Morren 1990, 71), the disciplines formed a nest-

ed hierarchy of analytical levels ranging from small elements of crop plants, such as molecules and cells, up through individual organisms and plant populations, to complex farming systems and agricultural ecosystems.

This diversity of disciplines was emblematic of the agricultural sciences in general. As with cancer research (Fujimura 1996), plant production research has been undergoing a significant transformation in recent decades. As new breeding methods (e.g., genetic engineering) have become more prevalent (Busch et al. 1991), the focus of physiological studies on crop plants has at the same time shifted from easily observable morphological characteristics to the internal physiological mechanisms of plants (Evans 1975, 18). In this way, the conventional mode of study – agronomical examination of cultivation methods – has been supplemented by new microbiological disciplines, i.e., molecular biology and plant physiology. Moreover, the ecological dimension of agricultural production has gained momentum, giving rise to a new perspective, agroecology, which has been critical of the conventional modes of agriculture (Bawden 1991). Taken together, these developments have made plant production research increasingly fragmented, in accordance with specialized disciplinary boundaries. To address how such fragmentation was instantiated in the studied university department, I turned to theoretical insight from the social world perspective.

According to Adele Clarke and Susan Leigh Star (2004; also Strauss 1991), scientific disciplines, specialties and research traditions can be conceived of as social worlds, or communities of practice that interact in a social ecology. As seen from this perspective, scientific disciplines should not be confused with formal organizations, such as university departments. While organizations typically retain a formal membership, scientific social worlds are often amorphous, lacking clear-cut boundaries or specified membership. Further, social worlds may cross-cut, or reside in, multiple formal organizations, or a single organization may inhabit many worlds. This incompatibility between social worlds and formal organizational structures makes conflict and controversy prevalent in organizations (Clarke 1991, 129-131). From the point of view of the present study, this insight shifts the focus of analysis away from the formal departmental organization to more specified occasions, to see what sources of conflict were at play at the department that influenced the relationships between groups of researchers with dissimilar disciplinary backgrounds. As analyzed in Article II, four such sources were identified, namely: 1) a challenging of the established departmental research tradition of agronomy, 2) a struggle over working space, 3) the extension into the department of an ethical-ideological controversy over genetically modified organisms, and 4) the anchoring of disciplines to different organizational units of the university.

First, as summarized by Clarke (1991, 134-135), reform movements calling for "new 'scientific' and 'physiological' approaches within medicine, agriculture, and biology" have occurred frequently. With respect to the present case example, the need to modernize the department's agronomical research tradition has also

been expressed. These pressures for change were articulated from the perspectives of three disciplines only recently initiated in the department, namely, molecular biology, plant physiology and agroecology. Those representing molecular biology and plant physiology stressed the need for a better understanding of the internal biological mechanisms of crop plants instead of quantitatively studying different kinds of cultivation methods. Agroecologically oriented farming systems research, on the other hand, emphasized the need to grasp the interactions between different components of farming systems (e.g., fertilization and plant protection) instead of investigating these in isolation from one another, as was the case with agronomy. Therefore, two different epistemologies were presented as alternatives to traditional agronomical research.

Second, in addition to diverging epistemologies, problems arose regarding working space at the department. Molecular biology was the case in point. Traditionally, research at the Department of Plant Production had been engaged in quantitative field and greenhouse experiments. When the discipline of molecular biology was introduced into the department, adequate laboratory facilities were provided. However, as the research utilizing these molecular biological methods expanded due to a substantial increase in governmental biotechnology funding (Nieminen and Kaukonen 2001), the working space fell short of what was needed. Efforts to mobilize additional offices and growing rooms for plants were launched. Although this did not reduce the active working space of other researchers, tension was aroused. This is interesting, as it seems to suggest that, in addition to its material-practical significance for the group itself, the expansion of working space was also read from a semiotic perspective. That is, those representing other disciplines took the expansion of molecular biology to be a statement of the changing focus of the department's research (cf. Gieryn 2002, 46-47).

Third, there emerged an interesting ethical-ideological controversy between molecular biologists and agroecologists at the department over genetically modified organisms. Prof. Pehu maintained that agroecology as practiced in the department was a "fully politicized" line of research engaged in an ideological critique against the genetic engineering of plants. According to her, it did not concentrate so much on solving scientific problems but used the words of science to achieve mere political goals. A professor of agroecology disclaimed such a view. He said that he had a great deal of respect for the work being done by Prof. Pehu's group, as it embodied steps towards ecologically sustainable agriculture. Nonetheless, he did not consider genetic engineering fully legitimate either because of its close association with global, industrialized agriculture, which he considered to be harmful to the developing countries. Interestingly, both professors linked this debate to concurrent developments outside the department, thus demonstrating how the local disagreement was infused with the wider societal controversy over genetically modified organisms.

Fourth, the introduction of molecular biology and plant physiology into the Department of Plant Production was part of the effort to modernize the existing departmental tradition that had been based on agronomy. This did not prove successful, however, as disciplines retained their close association to various units of the university's formal organization. Two such entrenchments were identified: 1) the coupling of horticultural science with a specific subsection of the Department of Plant Production and 2) the designation of molecular biology as belonging to the Department of Plant Biology rather than that of Plant Production. While in the former instance molecular biology and genetic engineering were excluded from the scope of research pursued at the Section of Horticulture, the latter showed just how strongly the studied department had been conditioned by its historically based focus on agronomical research and teaching.

As a result, a complex organizational matrix of disciplines was sketched in Article II. This matrix provides an interesting contrast to some of the claims made by those engaged in understanding university departments in terms of academic capitalism. While such studies have stressed the importance of external competitive funding from the point of view of departmental cultures (e.g., Ylijoki 2003), the present research presents a far more complicated picture of the dynamics of university departments. That is to say, the investigated department was not constituted by one dominant discipline but by multiple discordant lines of research that competed with one another. External research funding, in this context, proved important but was not an overriding factor influencing the lives of researchers at the department. While the vast increase in funds channeled to molecular biology and biotechnology influenced the organizational ecology of the disciplines – for instance, through initiating new lines of research or prompting industrial collaboration (see Article IV) – it nonetheless remained just one source of the dynamics. Therefore, one might conclude that the results achieved in this study empirically challenge, but do not necessarily refute, the findings provided by previous research on academic capitalism.

Dissolution of the Research Group-Firm Hybrid Entity (Article III)

My third research question directed attention to the possibility of entrepreneurial activities within traditional European universities. As acknowledged by the Mode 2 and triple helix models, academic researchers may seek to both maintain their university positions and become engaged in the operation of a private company. Such was the situation in the studied case example. Instead of contenting themselves with industrial collaboration or consulting, the research group members sought to create what Etzkowitz and others call "a hybrid firm." The hybrid firm is a company that straddles the public and private spheres of activity: it is a com-

mercial enterprise which is still located "within the university and dependent on the university for a degree of administrative and financial support" (Etzkowitz et al. 2000b, 320). In such a firm, the staff occupies both academic and company positions concurrently, which is what turned out to be the case with respect to the biotechnology group members. I took advantage of this emergent situation and used it as a methodological probe to test whether or not the thesis concerning the arrival of the entrepreneurial university was valid in the case of the University of Helsinki, where several efforts were made during the 1990s to foster the commercialization of research results (Häyrinen-Alestalo, Snell, and Peltola 2000; Pelkonen 2001; Tupasela 2000).

As regards hybrid entities like the one investigated here, the dividing line between university and industry seems to vanish altogether. To understand how the university administration responded to such a development, I used the concept of boundary work. As regards Thomas Gieryn's (1999) conceptualization, boundary work in this case was not a matter of separating science from nonscience but, instead, was constituted by a set of local bureaucratic procedures through which the demarcation of private business from public university activities was pursued by the administrators. Because no clear-cut rules and regulations existed for managing start-up companies in the university departments, determining the conditions for business activity became an issue of considerable contention between the group leader and those in administrative positions. When thematized from the point of view of boundary work, the following issues came up as controversial: 1) the bureaucratic accountability and teaching performance of the research group leader, 2) the loan of the university's research materials and instruments to the research group's firm and 3) the ownership of intellectual property rights. Eventually these dilemmas were worked out through the establishment of two boundaries, a social and a spatial, by means of which the group's business was separated from its public research. These will be briefly described next.

The university administrators wanted to make sure that a line remained between the hybrid community's academic work and its commercial projects. They also wanted to ensure that the group leader perform her teaching duties in the department diligently. Administrative reports and plans concerning her allocation of working time were thus called for; indeed, the administrators believed that she was neglecting her duties as a professor. The group leader, for her part, had a different point of view. She was perplexed and irritated by these requests for giving accounts, believing that they questioned her academic freedom and were detrimental to the department's applied mission. Moreover, she regarded the start-up company as an entirely private matter, with no ties to the university other than the temporary rental of laboratory space. In addition, she maintained that she had done excellent work in her teaching and research.

Apart from the matter of group leader's teaching performance, confusion emerged over the ownership of university property. When transferring to its new

laboratory in the university's business incubator building, the group took with it research materials and instruments acquired through public research grants. The issue was whether the group had the right to do so. Despite a loan contract agreed to by the department chairman and the company's chief executive officer, a serious conflict over the instruments and materials ensued. Not wanting to raise any further complications, the company's chief executive arranged a quick resolution. Some items were returned immediately while others were loaned for a short period of time.

With respect to patents and intellectual property rights (IPRs) the group encountered two intractable problems. In the first instance, the issue was about who had the juridical right – the university or the researchers-inventors – to patent the results of a project concerning biotechnological oat improvement. The debate was associated with the proposed alteration of the governmental IPR policy according to which the property rights for university research might be transferred from individual academics to the university institution.[20] In this case, the group leader insisted that the inventors had the legal right to patent the result. The university lawyers, conversely, expected her group to transfer the IPRs to the university. The clash of viewpoints proved profound and the participants were sidelined by the dispute for an extended period of time. The second problem related to the researchers' and investors' effort to have as large a patent portfolio for the embryonic company as possible, that is, all of the group's patents. However, the group had given the IPRs of its early patents to the university's licensing company, which was unwilling to restore them to the researchers. Here as well, negotiations were deadlocked for a long period of time. Finally, after a change of the licensing company's president, the problem was solved: a mutually beneficial marketing agreement was achieved as regards the one patent while the other was left non-commercialized in the file of the licensing office.

In tandem with the IPR conflicts, the group left the Department of Plant Production and became associated with the university's biotechnology research institute operating in the local science park. In connection with this association, a collaboration agreement was drafted. In this contract, a resolution of the fuzzy university-industry boundary was arrived at by abandoning the hybrid roles of researchers-entrepreneurs and defining separate locations for academic research and commercial development. In addition, the finances of the group were subjected to close scrutiny by the chief of administration at the institute. Although not entirely disconnecting the academic projects from the company, these measures provided a temporary resolution of the acute boundary problem.

The group's combining of its academic work with private business activity provides an apt example when raising the general question about the limits of commercialization within universities. Indeed, it might be used as a touchstone for the feasibility of entrepreneurship in the confines of the traditional comprehensive university. As the case example clearly illustrated, the hybridization of

the academic research group with private enterprise was not possible. The hybrid entity was abandoned and the biotechnology firm sealed away from the university's core academic units in a peripheral organizational position, with the business incubator operating in a science park.[21] Thus, the case example did not lend support to the generalized thesis stating that universities globally are becoming entrepreneurial organizations that pursue knowledge production and commercial development in a compatible fashion (Etzkowitz 2002a). Instead, it revealed some of the basic issues all similar endeavors are likely to encounter: the problems of combining many divergent and, perhaps, contradictory functions of a university. Supported by earlier observations (Krücken 2003; Packer and Webster 1996; Rappert and Webster 1997; Webster and Packer 1997) I would therefore suggest that the new commercial rhetoric, which is often closely linked with current policy concerns, does not necessarily correspond to equally dramatic changes at the level of local departmental practices. While universities certainly are adapting to the changing political conditions that emphasize the market and quasi-market mechanisms, they want to maintain their public character and protect their core units from direct commercial influence.[22] In this respect, the institutional structures and practices of the universities seem to be more stable than the bulk of the discourse accompanying the entrepreneurial university might make us believe.

Conclusion

I began this article by reviewing the recent literature according to which science and the university institution have dramatically changed in their character. Of the many alternatives that have tried to capture the typical features of the current situation, I chose to focus on four prominent conceptualizations, namely, Mode 2 knowledge production, the triple helix of university-industry-government relations, academic capitalism and the enterprise university. I summarized the main points of these theories and considered criticisms directed at them. In addition, I revisited my empirical research results relating to the trajectory of a university plant biotechnology group and connected these with several positions taken by the proponents of the four theories. What, then, might be concluded from this endeavor?

Given the fact that the four theories discussed were dissimilar as regards their theoretical statuses, their assessment must surely acknowledge that difference. To begin with, I claimed that Mode 2 knowledge production was representative of diagnosis-of-the-era theorizing. Such "performative histories" (Godin 1998) are messages sent out from scholarly conversation to a wider learned public. As such, their primary place may not be so much within scholarly discussion but rather in broader societal and political debate. Therefore, as noted by Noro

(2000), one should not simply verify or rebut their general visions. Because they usually contain components that are being developed at the levels of general sociological theories or empirically based research theories, diagnoses of the era may, nonetheless, be partly subjected to sociological perusal. The 'New Production of Knowledge' theorizing did have such elements. For instance, it made use of empirical research to substantiate its claim that the emphasis is being shifted from fundamental research towards more applied results. Due to its all-embracing character, however, case studies like the present one cannot confirm or refute this altogether. What case studies can do, is effectively point out issues that could be better appreciated by such 'diagnoses.' In this respect, it was concluded that the Mode 2 thesis was far too generalized and biased to provide an adequate description of today's science and university, in addition to the fact that it did not distinguish between theoretical, methodological and applied dimensions of research.

The triple helix, on the other hand, was far more opaque as regards its theoretical status. I claimed that it was not exclusively an example of diagnoses of the era but sought to be a research theory as well. In conjunction with many other scholars, Etzkowitz has endeavored to establish a strong linkage between the general claims of the triple helix model and empirical data, thereby making the theory more amenable to empirical scrutiny than would be the case with Mode 2. In this thesis, I took a specific argument developed within the triple helix model – the emergence of the entrepreneurial university – and put it to the test. The results were twofold. First, there can be little doubt that in recent years universities have changed in many respects. Analysts have attributed these changes to the emergence of the global knowledge economy, new public policy priorities and increased economic competition between nations. For instance, as noted by Kleinman and Vallas (2001, 455) "universities are increasingly viewed as mechanisms for enhancing national competitiveness." Second, while not wanting to question such general statements, which may be too obvious to contest, I would argue that the changes universities are going through are neither uniform nor pervasive. Instead of being isomorphic to one another, there are vastly different kinds of universities in today's world. Moreover, each university is comprised of various activities that are not always mutually harmonious. Therefore, it is questionable whether any all-embracing conceptualization of the transformation of the university is defensible as such. In my view, this was exemplified nicely in the analysis of the research group-firm hybrid entity summarized in the previous section. As illustrated by this research, the commercialization of academic research through the spin-off company turned out to be in conflict with other university activities, most apparently public-funded research and undergraduate teaching. Thus the attempt to hybridize public and private activities was willingly resisted by administrators, thereby providing a challenging case for Etzkowitz's totalizing entrepreneurial university model.

On these grounds, the moderate stances taken by Slaughter and Leslie as well as Marginson and Considine proved more reasonable. Although these authors also worked towards generic arguments, they nonetheless were quite well aware of the limitations of their particular research theories. In addition to the fact that the authors clearly indicated that there was considerable internal variance as regards different kinds of universities and disciplines, these theories were also formulated so as to concentrate on just two features of the current university picture: the growing dependence of academics on external, competitive funding (academic capitalism) and the introduction of new managerial instruments and executive power (the enterprise university). While these tendencies are topical in Finland, it is equally plausible that they have become complexly intermingled with many other issues at the grassroots of universities and their departments. Academic capitalism is a case in point. As pointed out above, the increase in external, competitive funding has had a direct influence on the organizational ecology of disciplines at the University of Helsinki's Department of Plant Production. The investigated plant biotechnology group, for instance, was established on the basis of such grants. Furthermore, its subsequent industrial collaboration was stipulated by its patron, the National Technology Agency of Finland. Nonetheless, the analysis also demonstrated the limits of academic capitalism through the study of conflicts between the disciplines. It appeared, in this instance, that many other issues apart from external research funding also played a role. In other words, competition over funding constituted only one ingredient in the complex organizational ecology of the disciplines in the department.

This reflects the fact that academic capitalism – along with the enterprise university – does not speak about scientific practice *per se* but more about the administrative and political issues of academe. Of the four theories, scientific research was more particularly addressed by the Mode 2 knowledge production thesis as well as Etzkowitz's triple helix model. While Gibbons and the other proponents of Mode 2 spoke about the emergence of an entirely new kind of research which is transdisciplinary and application-oriented in nature, Etzkowitz and Leydesdorff, among others, claimed that this is nothing new, arguing that this was the original form of research before its institutionalization in the universities. Not being able to examine this issue empirically, my particular study gave rise to another point of concern with respect to Mode 2, that is, its tendency to describe knowledge production through indistinct and totalizing language, which glosses over all too many important conceptual, material, social and institutional dimensions of scientific practices. Of such intrinsic features of research work, this particular study highlighted the relevance of distinguishing between, and acknowledging the dynamics of, the theoretical, experimental and applied agendas of local research programs. Thus instead of using terms like Mode 2 knowledge production, I chose to speak about specific experimental systems in terms that directly relate to my specific case example. By so doing, I was also able to understand how

patentable innovations and industrial collaboration have sprouted from university research, an issue generally left unexamined by the four theories.

All in all, I believe this summary article has demonstrated the need to see scientific work and universities as complex and occasionally contradictory entities whose developmental trajectories are shaped by multiple historical, political and cultural characteristics. It has also further substantiated the advantages that may be achieved when such developments are addressed in terms of local practical actions taken by those actually involved in university activities. Seen from this perspective, all of the reviewed theories were limited in one way or another. Therefore, I presume, the practice-oriented sociology of science has much to offer for scholars working in neighboring fields of investigation, such as higher education research and research policy. One of the aims of this study was certainly to illustrate these potential benefits.

Notes

[1] A shorter version of this article will be published in the journal *Higher Education* (Tuunainen 2004).

[2] As a general term, biotechnology refers to "any technique that uses living organisms or parts of organisms to make or modify products, to improve plants or animals, or to develop microorganisms for specific uses" (Busch et al. 1991, 1). In this paper, I speak about biotechnology in a more limited sense, focusing on plant-genetic engineering where novel cell and molecular biological research methods, such as recombinant DNA techniques, are used to improve crop plants.

[3] The other theories include, for instance, the entrepreneurial university by Burton Clark (1998), post-normal science by Jerome Ravetz (1999), postacademic science by John Ziman (2000) and convergence theses of various kinds (Kleinman and Vallas 2001; Powell and Owen-Smith 1998).

[4] The central viewpoints that Gibbons and others took up have also been restated by John Ziman (2000). In his view, due to the financial ceilings placed on the funding of universities, scientists have become more responsive to societal needs in their research and more concerned over its quality and impact. As a result, he argues, science is being transformed as a cultural form into what he calls "postacademic science." By and large, this emerging research culture parallels the model presented by Gibbons and co-workers. According to Ziman (2000, 141-146), postacademic science is oriented towards producing proprietary knowledge, strives for a local, transepistemic understanding of practical matters, and incorporates interests of various kinds (cf. norms of science; Merton 1942/1959). Surprising as it may sound, postacademic science is a single

culture. In it, heterogeneous networks constituted by academics and industrialists create knowledge, transcend the traditional boundaries demarcating basic and applied research and form hybrid teams that override old institutional loyalties (Ziman 2000, 148-149).

[5] Roger Geiger (1988, 341-342) lists four reasons for this: 1) industry is nowadays willing to make huge, long-term contractual commitments supporting university research, 2) universities are eager to seek out these contracts, 3) a diverse range of new arrangements have been worked out by universities to facilitate technology transfer, and 4) some of these have been facilitated by governmental bodies.

[6] For an attempt to transform the abstract formulations of Mode 2 into empirical research, see Kraak (2000b) and Subotzky (1999).

[7] A recent study of 89 research-intensive U.S. universities indicated, however, that commercial and academic standards for success have become integrated into "a hybrid regime" (Owen-Smith 2003).

[8] According to Michael Lynch (1998, 29), constructivist movement is a fragmented coalition of heterogeneous social scientific research approaches. As such, it is a useful "handle for presenting and legitimizing academic work" but should not be "employed as a lever to radical epistemological reform." Ian Hacking (1998, 56), on the other hand, claims: "anything worth calling a construction was or is constructed in quite definite stages, where the later stages are built upon, or of, the product of earlier stages. Anything worth calling a construction has a history. But not just any history. It has to be a history of building."

[9] In science studies, disciplines and specialties have gained attention only sporadically, but the interest in examining these is clearly on the rise (Cambrosio and Keating 1983; Fujimura 1996; Knorr Cetina 1999; Lesney 1997; Lynch and Bogen 1997). The major thrust in this line of research is to move from understanding disciplines as organizing structures codified in textbooks, towards analyzing "spaces of knowledge-in-action" (Knorr Cetina 1999, 3) or the creation of epistemic boundaries (Gieryn 1999). That is, the notion of discipline, although an important one, proves insufficient as such, if one is to understand the practical strategies and policies scientists are engaged in. While disciplines themselves may involve complex internal dynamics (Cambrosio and Keating 1983, 326), their interactions may be equally intricate (Abbott 2001).

[10] At the outset, the interviews were organized around a scheme, which was influenced by an activity-theoretical perspective (Miettinen 1998; Cole and Engeström 1993; Engeström 1999b) and early laboratory studies (e.g., Latour and Woolgar 1979; Lynch 1985). As discussed by Reijo Miettinen (1998), the efforts of a scientific work community, or a research group, can be viewed as an object-oriented human activity. As such, this incorporates a complex set of tools, ideas and methods, as well as a specific object of work. As with the activ-

ity of other groups or organizations, research groups as well collaborate with other communities, seek to yield useful outcomes, set up workable divisions of labor, and comply with various kinds of rules and regulations. Using such a view as a heuristic device in the data collection, I asked my respondents about their objects of research, tools, methods and experimental protocols developed and applied, other researchers involved in the experiments, the division of labor within the group, and the group's collaboration networks. By so doing, my attention in the beginning was oriented towards the group's laboratory research rather than the wider organizational and institutional contexts surrounding it.

[11] Additional knowledge was also provided by the file on the research group leader's professorial appointment, CVs of the interviewed persons, documents related to teaching arrangements in the Department of Plant Production, the departmental strategies and annual reports as well as the 1999 peer review report on departmental activities.

[12] With respect to this struggle, the participants also provided me with two sets of strictly confidential documentary material. As was mutually agreed, these documents were not included in the data set nor were they used in any analysis. I was, nonetheless, well informed by their content.

[13] Of these, 11 were conducted with the research group members, five with the group's collaboration partners and one with a representative of a science-funding agency.

[14] The persons interviewed consisted of five professors and five researchers working in the Department of Plant Production as well as the Dean of the Faculty.

[15] The interviews were conducted with the research group leader, university administrators and academic leaders as well as with a representative of a science-funding agency. The documents consisted of public laws and statutes, a collective bargaining contract, university rules and regulations, written documents, administrative memoranda and a floor plan.

[16] Accounts and documents, in this sense, are constituent features of the situations and processes they describe. As Rod Watson (1998, 94) puts it, "descriptions or definitions elaborate those circumstances and are elaborated by them."

[17] Watson (1998, 85) described the usual tendency of sociologists to treat texts as "'conduits' to a reality beyond the text." Texts such as interview transcripts or documents, may, however, be taken as objects of research in their own without discounting their connection to 'realities' of various kinds. From an ethnomethodological perspective, texts may be analyzed in terms of how they actively make sense of the phenomena they describe (Watson 1998, 85; for an application in science and technology studies, see Lynch and Jordan 2000). Symbolic interactionists, on the other hand, treat texts as a means of access

to the meanings attributed by people to their experiences and social worlds (Miller and Glassner 1998, 100). Finally, discourse analysts such as Norman Fairclough (2003, 17) maintain that there are "alternative and often competing discourses, associated with different groups of people in different social positions."

[18] I dismiss Article IV here, as it mostly elaborates upon the data and analyses in the three other papers.

[19] This body of literature involves numerous sociological (Fujimura 1996; Keating and Cambrosio 1997), philosophical (Radder 1996) and historical studies (Gaudillière 1996; Kohler 1994; Löwy 1994; Pieters 1997; Rheinberger 1997; Silverstein 1994; Stillwell 1994).

[20] Currently a legislative proposal which would carry this change into effect is being prepared by the Ministry of Education.

[21] I conceptualized the investigated process in terms of a dissolution and winding down of the academic research. A similar kind of example was also provided by Eveliina Saari (2003, 149-151). As insightfully observed by Seppo Raiski, an alternative perspective in my case study might have been to understand the case example in terms of the organizational differentiation of the university, on the one hand, and of the company, on the other.

[22] For indirect influence, see Kleinman (2003).

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