A NEW REWARD SYSTEM IN ACADEMIC SCIENCE

by

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(Under the Direction of Joseph C. Hermanowicz)

ABSTRACT

This research examines the operation of a new reward system in academic science. Since 1980, the erosion of structural and cultural boundaries between universities and industry has led to a radical acceleration in the commercialization of scientific discoveries in the form of patents and companies generated by scientists in the academy. The implications of this shift for scientific work are dramatic as it introduces new rewards, norms, and career prospects for scientists. To address these problems, sixty-one interviews with academic scientists were conducted at four universities. Drawing on a sampling frame that identified the most prolific commercialist academic scientists in the United States, the study employs a theoretical sampling strategy that compares commercialist and traditionalist scientists at public and private universities, evenly divided between scientists who received their PhDs before and after the onset of commercialized academic science. The analysis focuses on the constitution of moral orders in science, social mechanisms that enable and constrain identification with commercial trajectories, identity work, and the normative structure of contemporary science.

INDEX WORDS: Science, Scientists, Reward System, Higher Education, Professions, Universities, Commercialization, Careers, Entrepreneurialism, Work

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CHAPTER 1

THEORETIC FRAMEWORK OF THE STUDY

Research Problem

Academic scientists today have the opportunity to be successful in a domain of science from which they have historically been structurally and culturally insulated. Prior to 1980, when passage of the Bayh-Dole Act enabled universities to retain intellectual property that resulted from federally-funded research to commercially exploit scientific discoveries, rewards for scientific achievement were primarily accorded through the allocation of recognition such as publication, citation, and membership in honorary academies (Merton 1957a; Zuckerman 1977). Over the past thirty years, however, the opportunity to reap financial rewards and receive new forms of recognition has produced a commercial model of scientific achievement in academe.

Well-documented evidence attests to the emergence of a reward system oriented toward commercial achievement in the wake of Bayh-Dole. Prior to 1981, U.S. universities produced less than 250 patents annually (Council on Governmental Relations 1999). During the 1990s, the total number of patents owned by U.S. universities rapidly increased from over 1,500 in 1992 to 3,000 by 1998 (Henderson, Trajtenberg, and Jaffee, 1998). By 2003, universities had filed approximately 8,000 patent applications and completed 4,516 licensing deals that brought in an excess of $1.3 billion in royalties (Leaf and Burke 2005). The growth of “spinoff” companies was equally rapid: between 1980 and 2000, discoveries by university scientists generated 3,376 startup companies (Pressman 2002). New forms of recognition have become easily observable in academe, as ranks, chairs, lectureships, awards, and university buildings frequently bear the names of corporations (Soley 1998).

Academic scientists are therefore now exposed to two main reward systems that are characterized by different conceptions of the scientific role, and corresponding occupational norms, consequences for the advancement of knowledge, and rewards for scientific conduct. What we may call the "*traditionalist*", or priority-recognition reward system, mandates that scientists advance knowledge by sharing their discoveries with the scientific community through peer evaluation in exchange for recognition of priority in discovery (Merton 1973). By contrast, a "*commercialist*" reward system gives scientists a mandate to contribute to economic development through the dissemination of their discoveries in the market in exchange for profits. Social scientists typically cast traditional and commercial scientific roles as contradictory. Some suggest a process of "asymmetrical convergence" (Kleinman and Vallas 2001) now operates, in which the codes and practices of academe and industry permeate the institutional boundaries that once separated these domains. According to this view, the contradictory logics of the systems are most evident in the tensions between sharing and privatizing knowledge; peer-based and market-based evaluations of work; and whether or not faculty have a financial stake in the outcome of their work. But these systems of scientific rewards are analytical constructions. They highlight the chief characteristics of the cultural schemas available to scientists that motivate, organize, and legitimize a scientific career. The moral orders these career paths exemplify, the pattern by which scientists identify with reward systems, and the forms scientific norms now take thus merit theoretic and empirical analysis: the meanings scientists assign to these aspects of work define the institution of science and shape both the advancement of knowledge and economic growth in society.

This study addresses the following research problem: How does a commercially-oriented reward system operate in academic science? Its core premise is that the traditional notion of the scientific role as a disinterested search for truth now exists alongside a new ethos of science characterized by commercial values and norms that depart from, and are indeed antithetical to, customary standards of scientific conduct. Unlike existing studies of the commercialization of science in the academy that consider the phenomenon from the perspective of its legal-regulatory context, organizational arrangement, or scientific products (e.g., patents and startups), this study situates analysis in the context in which the process of commercialization actually occurs: in scientific careers and the organizational contexts in which they are embedded.

Although a well-established literature exists on the commercialization of academic research, there are two wide-ranging gaps in the current state of knowledge. First, existing theoretic and empirical approaches overwhelmingly focus on products, not producers, of commercialized academic science. Whereas core theoretic explanations focused on the role of the state and the legal-regulatory context that fostered the emergence of commercialized academic science (Slaughter and Leslie 1997; Slaughter and Rhoades 2004) or the changing relations between universities, industry, and the government (Etzkowitz 1983), subsequent studies examined outcomes, such as patent activity (c.f., Henderson et. al 1998; Mowery and Sampat 2001; Agrawal and Henderson 2002; Owen-Smith 2007), university startups (Shane and Stuart 2002), university-industry linkages (Owen-Smith and Powell 2005), and the emergence and role of technology transfer offices (McCray & Croissant 2001; Kaghan 2001) and thus effectively obscured the theoretic and empirical significance of *scientists* in academe.

To be sure, scientists have not been entirely excluded from analysis. Owen-Smith and Powell (2001) present a compelling typology of faculty responses to the changing division of academic labor and highlight the changing meaning of a scientific career. Slaughter and colleagues (Slaughter and Leslie 1997; Slaughter and Rhoades 2004) incorporate interview data that highlight costs, benefits, and manifestations of commercial activity in the academy. But the meanings that scientists assign to their endeavors falls outside the purview of this work. While Etzkowitz's (1983) pioneering work in this general area focuses on the transformation of norms in academe, his data were collected three years after the passage of Bayh-Dole and he focuses on scientists with limited commercial involvement (Etzkowitz 1983). Others have employed surveys that measure scientists' attitudes about the risks that commercialization poses to traditional scientific values (c.f., Louis et. al 1989; Rahm 1994; Glaser and Bero 2005). These studies say little about what commercialization means or why scientists engage in it.

Consequently, existing knowledge lacks an interpretation of the commercialization of academic research based on the perspectives of scientists engaged in (or deliberately divorced from) this very commercialization, yet such an explanation holds the potential to transform how we conceive of commercialized academic research from understandings that focus on products of commercial practices to an understanding based on the experiences of the scientists. The distinction between these approaches and thus the end contribution to knowledge of the scientific profession is significant: the product-oriented approach strips knowledge of its context, whereas the producer-oriented approach produces knowledge that retains the meanings and purposes that scientists assign to their work.

A second gap in knowledge of commercialized science is that academic scientists who eschew commercialization have been excluded from analysis. This is a consequential omission. Scholars assert that the emergence of commercialized science has resulted in a transformation of scientific norms of conduct (Etkowitz 1983) and that commercial behavior among faculty is widely accepted and encouraged (Colyvas and Powell 2006). Given the evidence of commercial activity, these claims are sound, but raise important questions about the operation of a commercial reward system. How do scientists construct “traditionalist” and “commercialist” science? Why do some scientists embrace commercialism while others eschew it? Has a commercially-oriented reward system supplanted the existing order of academic science? How do scientists who eschew commercialized work perceive it? And, why do scientists in fields in which the dynamics of commercialization are most evident fail to be influenced by the new reward system? What are the consequences of commercialized science for non-commercial scientists? Studying these scientists carries both theoretic and methodological significance, for we may only understand the operation of one reward system by understanding the success and failure of the other, as similarly we may only understand why scientists embrace certain norms of conduct through comparison of the motivations that underlie adherence to alternative norms.

Aside from its contribution to knowledge about the commercialization of science, this study will also offer an important contribution to the field of sociology. A key debate in the sociology of professions questions whether such occupations are altruistic or exploitative. Functionalists argue that professions derive moral, legal, and intellectual mandate by virtue of their possession of core professional traits that include the mastery of a body of abstract knowledge, the authority to decide what is and is not in the interests of clients, and the ability to self-regulate the performance of members (e.g., Carr-Saunders and Wilson 1941; Parsons 1949). From this perspective, the academic profession is a moral entity whose commitment to the common good imbues role performance with moral obligation. Conflict and power theorists, by contrast, claim that authority and autonomy are the means by which professions preserve their monopolistic interests and achieve profits at the expense of others (e.g., Johnson 1972; Larson

1977). Others argue that professions are becoming corporatized and marketized (Brint 1994). While the competing perspectives of Parsons (1949), Johnson (1972), and Larson (1977) are core sociological theories of professions, they have been advanced with little empirical analysis that actually investigates the motivations of professionals themselves. As a study that directly examines what members of the academic profession conceive to be legitimate means and ends of an academic career, this research will offer an important empirical contribution to this debate, as it will reveal how members of a profession with substantively different conceptions of their role construct their contribution to society.

Central to this study is the assumption that scientists are of sociological significance and are worthy of study. Scientists extend and preserve knowledge, which is critical to society. Conflict surrounding the appropriate means by which this role is fulfilled is therefore consequential for the profession and its role in society. In this way, the study stands at the heart of a sociological endeavor in its concern for the nature of action, meaning, and order in the academic profession and the institution of science. In light of this focus, there are four specific research questions that must be addressed to fully explain the operation of a new reward system, and which thereby guide the present study:

1) How does the contemporary reward system shape the meanings and patterns of scientific careers?
2) How does a commercially-oriented reward system influence the normative structure of science?
3) What are the social mechanisms by which scientists embrace commercialization?

4) What are the consequences of a commercially-oriented reward system for the institution of science, universities, and the academic profession?

These questions tie together four main categories of scholarly research relevant to the social scientific literature on commercialization: *meanings, norms, mechanisms* and *consequences.* Researchers working in these areas devote varying degrees of attention to scientists. None, however, systematically integrate these research foci to provide comprehensive analysis of the implications of a commercial reward system for scientific work. Consequently, research on scientists constitutes a significant gap in our understanding of commercialization. In the section that follows, I examine the merit the merit underlying each research question.

Orienting Concepts and Research Questions

Meanings

The meaning of work is an extremely relevant, but poorly integrated, research focus in the study of commercialization. Much like the study of industrial sociology (Simpson 1989) and organizations (Barley and Kunda 2001), research on commercialization suffers from an inadequate conception of workers and work. Meaning is a critical empirical omission because workers are not passive objects subject to social forces such as industrialization, technological change, or new reward systems. Rather, such forces are bound in dynamic tension with workers' conceptions of work. The commercially-oriented reward system, initiated by legislation, does indeed influence patterns of scientific, but scientists create their own cultures. They, too, influence the organization of work. It is therefore critical to understand what commercialization means to those most closely exposed to its dynamics.

This insight is at the heart of the study of careers. Sociologically, the study of scientific careers provides a means for understanding the social institution of science and the academic profession (Barley 1989). Careers link individuals with social structure in that social institutions such as science have no reality independent of the individuals who forge career paths within them (Barley 1989). In other words, science and its structural and cultural composition constitutes and is constituted by the actions of scientists as they attempt to achieve career goals. While much is known about the resources, interpretive schemes, and norms of scientific careers that govern achievement under the logic of the traditional institutional order (Hermanowicz 1998; Zuckerman 1977), such attributes of the new reward system are less understood. Most studies stress convergence of commercialist and traditionalist science, empirically exclude traditionalists in doing so, and analytically construct commercialization as the hybrid of "old school" and "new school" science (Owen-Smith and Powell, 2001; Lam 2010) or as a combination of traditional entrepreneurialism (getting grants) plus a profit motive (Etzkowitz 1998). Such emphases contribute to our understanding of where commercialist and traditionalist science may overlap, but they do not illuminate the dimensions of scientific work along which commercialist and traditionalist science diverge. Without evidence of the meanings both traditionalists and commercialists assign to commercialization, we poorly understand the patterns of social differentiation within the academic profession that stem from a commercially-oriented reward system. Because careers are central to the social order of science and the organizations and occupations that comprise it, an empirical understanding of the structure and culture of individual achievement in commercially-oriented science is critical to the assessment of the current condition of science.

Career theorists argue that the ends one pursues in a work career are limited to areas of occupational life that offer the greatest promise of future reward and are thus patterned by individuals' subjective assessments of achievement in work (Van Maanen 1977). Similarly, others argue that scientists engage in boundary work, such as drawing symbolic distinctions between good and "junk" science, to maintain authority over knowledge production (Gieryn 1999). Both theories point to the importance scholars assign to the subjective side of work as a means to understand institutional order. To differentiate commercialist and traditionalist science, therefore, we must consider how scientists conceive their role to understand the moral orders of science. As Durkheim showed, moral order denotes the power of the collectivity over the individual, as collective beliefs, values, and orientations held by members of a group govern the actions and expectations of group members (Durkheim [1915] 1995). Some sociologists of science have drawn upon the concept descriptively, framing the notion of a moral order as a social arrangement of scientific values and passions (Star 1995) or as a collective conscience among scientists about the proper relationship between science and society (Moore 2008). Its fullest theoretical and empirical exposition in this area is found in the work of Hermanowicz (1998, 2009), who argues that "moral orders inform us about how [scientific] careers are structured, interpreted, and understood" (Hermanowicz 2009, p. 25). Moral orders thus convey the aspects of scientific work that define commercialist and traditionalist science. Each is a foundation for a different vision of science that exhibits distinctive expectations, meanings, and practices that define group life.

Greater understanding of the meanings traditionalists and commercialists assign to their work is particularly critical in light of extant arguments in the sociology of science and professions that suggest that status in science is predicated on original contributions to research

(Merton 1973; Cole and Cole 1973). Merton, for example, argues that the preeminence of original research contributions is a function of the centrality of the research role to ancillary components of the scientific role set: "For plainly, if there were no scientific investigation, there would be no new knowledge to be transmitted through the teaching role, no need to allocate resources for investigation, no research organization to administer, and no new flow of knowledge for gatekeepers to regulate" (Merton 1973, p. 521). Because of the functional centrality of the research role, "the working of the reward system in science testifies that [it] is the most highly valued. The heroes of science are acclaimed in their capacity as scientific investigators, seldom as teachers, administrators, referees, and editors" (Merton 1973, p. 521). Abbott's purity thesis expresses this argument in a more general theoretic form (1988, p.188): "A profession is organized around the knowledge system it applies, and hence status within profession simply reflects degree of involvement with this organizing knowledge. The more one's professional work employs that knowledge alone—the more it excludes extraneous factors—the more one enjoys high status." Technologies and commercialization are ancillary to scientific knowledge – the basis of the profession – and thus the arguments of Merton and Abbott posit that fundamental research is the key source of status in science. Given that a commercial reward system is predicated on rewards for the invention of technologies, it would appear that such arguments require elaboration.

The limited applicability of current mainstream arguments such as those regarding status in scientific work exemplifies exactly why greater understanding of meaning is necessary to advance our understanding of commercialization: status, like other aspects of science, is socially constructed. Close empirical observation of the meanings scientists assign to their work provides insight into how scientists construct their worlds and enables reconsideration of theoretical and practical implications of commercialization. A key research question of this study, therefore, is: *How does the contemporary reward system shape the meanings and patterns of scientific careers?*

Norms

One of the chief but unresolved debates produced by the emergence of a commercially-oriented reward system concerns the impact of commercialization on the normative structure of science. The classic conceptualization of the normative structure of science postulates that norms of disinterestedness, organized skepticism, universalism, and communalism comprise an ethos that guides and organizes conduct (Merton 1973). The operation of these norms and their control of the conduct of scientists may be seen in examining the kind of behavior each norm affects.

Disinterestedness stipulates that the advancement of knowledge should proceed uninfluenced by scientists' stakes in the outcomes of their contributions (Merton 1973, 275-277). In other words, scientists' motivation for research should be independent of personal or corporate biases such as financial gain. *Organized skepticism* refers to the social arrangements established to ensure that all scientific contributions undergo a fair and proper process of peer-based evaluation prior to becoming a part of certified knowledge (Merton 1973, 277-278). *Universalism* stipulates the terms by which output is to be evaluated and rewarded (Merton 1973, 270-273). This norm requires that scientific contributions, appointment, promotion, proposals for research funds, and honors are to be judged according to pre-established intellectual criteria and not particularistic social attributes irrelevant to scientific merit such as social origins, personal relations, or social status. Finally, c*ommunalism* holds that scientific knowledge is a product of collective effort and must therefore be shared, not kept secret (Merton 1973, 273-275). When a contribution is made public, the creator holds no individual claim of ownership of the work. Thus, ideas, hypotheses, theories, and empirical findings are made available to others to use in their work.

Old debates over the operation or existence of scientific norms (cf., Mulkay 1969; Barnes and Dolby 1970; Mitroff 1974; Woolgar 1982) received new life with the acceleration of commercial practices in academic science in the 1980s. The primary motivation underlying the debate appeared to center on the fact that commercialized research is allegedly predicated on practices antithetical to the norms articulated by Merton. Whereas communalism stipulates that knowledge be diffused to the common stock of certified knowledge, the new reward system places a premium on privatized intellectual property. The consequence is thus a shift in the nature of knowledge produced in universities from a public to a private good. Whereas the norm of disinterestedness specifies a pursuit of knowledge driven by the cognitive structure of disciplines, the new reward system directs the advance of knowledge toward commercial ends, motivated by the pursuit of profit. Finally, under the traditional reward system, organized skepticism is situated in disciplinary journals, departments, and academies through the peer review process. It is a key basis by which the academic profession regulates itself. Under the new reward system, by contrast, evaluations of patent applications are bureaucratic, subject to criteria irrelevant to the advancement of basic knowledge, and not necessarily assessed by members of a disciplinary community.

Little more than anecdotal evidence, however, was brought to bear on this debate until Henry Etzkowitz published an article based on interviews conducted in the early 1980s in which he claimed that traditional scientific norms had been transformed by or replaced with an entrepreneurial ethos (Etzkowitz 1989). Although Etkowitz’s claim has gained favor among other scholars (Etzkowitz 1998; Colyvas and Powell 2006; Metlay 2006; Stuart & Ding 2006) as research on commercialization expanded, it is not without its methodological problems. First, scholars claim an entrepreneurial ethos has replaced the traditional scientific ethos “by dint of its prevalence” (Stuart & Ding 2006; Owen-Smith & Powell 2001; Colyvas & Powell 2006). To argue that scientific norms have changed, these researchers point to the increased level of commercial practices in academe and characterize the “academic entrepreneur” as having a taken-for-granted status. This argument extrapolates behavior that describes a subset of the academic scientists to the community as a whole. Though substantive in volume, dynamics of technology transfer and industry funding, for example, predominately occur at a limited number of universities and within a minority of academic laboratories (see chapter 2).

A second methodological problem is that scholars have arrived at this claim through an unfalsifiable analytic framework. If one studies *only* commercial scientists (Etzkowitz 1989; Etzkowitz 1998; Owen-Smith & Powell 2001; Colyvas & Powell 2006) and indeed finds that their behavior systematically departs from traditional scientific norms, the apparent conclusions could either be that there are indeed new norms of science or simply that scientists are deviating from traditional norms. It is difficult to claim that the traditional ethos has changed or has been replaced, however, without verifying in the same environments that non-commercial scientists no longer embrace traditional scientific norms.

The claim lacks theoretic eloquence, too. First, scholars claim an entrepreneurial ethos now exists but they have not specified what norms comprise it. In the initial study from which the transformation of norms argument emerged, Etkowitz (1989, p.14) claimed that new university linkages with industry “encourage normative change in science” but he did not specify new norms. The most specific articulation one can find in his work is in a subsequent article. Drawing upon 150 interviews at three points in time, Etkowitz (1998, p. 828) stated the following: “To put it in a nutshell, the new entrepreneurialism is the old one plus the profit motive. Seeking for funds has always been an important activity in the American research system which demands a lot of entrepreneurial energy and phantasy. Therefore, as soon as traditional academic ambitions for the pursuit of the truth could be combined with profit seeking, the door was open for the new entrepreneurialism.” At best, one can only assume that entrepreneurial norms somehow correspond with behaviors seemingly antithetical to traditional scientific norms such as secrecy, and nonscientific motivations for research.

These arguments, and those advanced by Merton (1973) himself, point to the operation of an alternative set of norms for which commercial imperatives run counter to the logic of the institution of science. Nevertheless, research has yet to indicate the content of commercial norms. Therefore, a second research question this study addresses is: *How does a commercially-oriented reward system influence the normative structure of science?*

Mechanisms

A third theme relevant to research on commercialization concerns the social mechanisms that lead group participants to identify with and embrace particular roles and corresponding behaviors. The objective of scholarship in this category is the explanation of why scientists embrace the commercial role. Two broad approaches inform this theme in the commercialization literature: instrumental and normative forms of social structural theory. Each approach implies different social mechanisms that lead to role commitment, but few studies have empirically evaluated the theoretic claims.

Instrumental forms of social theory stress the material context of individual action and are represented in classical sociology by Marx and Weber who emphasized the coercive nature of structure (Alexander 1988). Whether it is the role of the economic system or bureaucracy, such theorists posit that roles follow from external demands. In the commercialization literature, this approach is found in Slaughter and colleagues' theory of academic capitalism (Slaughter and Leslie 1997; Slaughter and Rhoades 2004). The theory argues that commercial activity in the academy is structured according to the demands of capital. From this perspective, through regulatory shifts that promote privatization and commercialization, the neoliberal state seeks to unleash the power of scientists as economic actors by shifting the social allocation of resources away from social welfare functions to production functions (Slaughter and Rhoades 2004, 20). Because organizational survival is predicated on the acquisition of resources, actors within the university utilize state and market resources in such a way that alters the production of knowledge and the relationship between academe and industry. Commercial activities are thus driven by the exigencies of the prevailing economic system and the organizational forms upon which it rests. This theory emphasizes the organization as a locus of social control, and implies that occupational and individual responses under the new reward system are structurally enabled and constrained. In short, it suggests from a macrosociological perspective that scientists embrace commercial pursuits due to the structural properties of their environments that facilitate commercial activity.

The emphasis on structure in the instrumental theoretic perspective suggests that the mechanisms by which scientists embrace or eschew commercial roles varies across public and private universities, as these institutional types exhibit distinct organizational reward systems and infrastructures for technology transfer. For example, a number of scholars show that organizationally-based reward systems (e.g., salary, reduced teaching loads, and graduate assistance) are more generous at private universities compared to public universities and note the difficulties public universities have faced since the late 1970s in maintaining competitive faculty salaries relative to private universities (Alexander 2001; Winston 1999). Differences across institutional types also shape the specific commercial context of academic science. Public and private universities, for example, have different faculty conflict of interest policies. Twice as many private as public universities have specific limits on publication delay and financial interests, possibly suggesting public universities are less willing to place constraints on state mandates for technology transfer (Cho et. al 2000). Institutional distinctions are also evident in technology transfer infrastructures. Friedman and Silberman (2003) find, for instance, that distributions of royalties to scientists' laboratories and general university funds to support research are statistically significantly higher at private than public universities. Similarly, Thursby and Kemp (1999) show that private universities are more effective than public universities at generating commercial licensing arrangements.

Social structural theories in their normative form, by contrast, argue that a system of beliefs and symbols that express moral commitments form the basis of collective order and as a result, social structure is located within actors (Alexander 1988). Normative explanations of action and order first emerged within sociology in Durkheim's (1947 [1873]) characterization of the social division of labor as predicated on moral integration and cultural order, and later in Parsons, who emphasized action and role performance as conditioned by the internalization of norms. Emphasis on the normative basis of commercially-oriented conduct in the academic profession is found in Etzkowitz's (1989) work on the transformation of scientific norms in the academy. From this perspective, identification with the commercial role is a function of the normative order of the contexts in which commercially-oriented careers are carried out.

This point, however, leaves unquestioned the tension between commercial and traditional scientific norms. This argument could potentially explain why scientists socialized to commercial norms embrace commercialization, but it begs the questions: Why do traditionalists become commercialists? How does such a normative shift occur? Such questions indicate different instances in which Etkowitz's (1989) argument is unable to explain role identification. On the one hand, some scientists who have embraced commercialization underwent professional socialization prior to the onset of commercial culture in academe (Weiner 1987). On the other hand, it is conceptually possible scientists embrace the commercial role in contexts in which commercial norms are not prevalent.

Despite a well developed theoretic foundation by which arguments about why scientists commercialize their work may be derived, empirical evaluation suggests our understanding requires further investigation. For example, in a quantitative analysis of the antecedents of commercial behavior, Stuart and Ding (2006) identify correlations between commercial activity and factors such as social proximity to commercialists in academe, but they concede that they are unable to causally explain the social mechanisms by which scientists become commercially active (p.110). Other scholars examining the influence of industry on academic culture likewise conclude that they are unable to "definitively address the specific conduits through which such influences flow" (Kleinman and Vallas 2006, p. 54). The motivations and the processes by which faculty become commercially active thus remain unprobed territory. What is more, the studies that have been conducted overlook the importance of two theoretic dimensions suggested by instrumental and normative theories in the literature on commercialization: *institutional type* and *era of professional socialization*. In light of this knowledge gap, a third question that motivates this study is: *What are the social mechanisms by which scientists embrace commercialization*?

Consequences

The integrative theme of the literature on commercialization is that the new reward system is consequential for science. The influence of capitalism on the distribution of resources in science can be considered in at least two ways: the allocation of effort to questions at the frontiers of research in academic fields and the allocation of money to support research. Scientific fields, for example, are dynamic terrains: they emerge, grow, and potentially decline (Crane 1972; Goffman 1966). One influence on the decline of specialties within a field is scientists' responses to changing opportunity structures (Holton 1972; Mullins 1973; Zuckerman 1978) such as the level of competition among scientists within specialties or competition between specialties for the attention of scientists. Corporate-funding and market opportunities for commercial exploitation are attractive to scientists, especially in the context of a competitive funding environment. Because these incentives are more heavily linked to particular fields, it seems likely that a key implication for science is a decrease in the level of effort allocated to non-market oriented fields, in particular the basic sciences.

The new reward system also influences science through the allocation of financial support. Because universities own the intellectual properties that scientists commercialize in the market, academic entrepreneurialism may benefit science through the generation of royalties which may be appropriated to support the university mission. Some data suggest, however, that the returns to universities from commercial pursuits of most academic scientists are less than expected. For example, a study of technology transfer offices at 200 universities showed that while a small number earned royalties over $10 million dollars during the year 2000, the majority of universities earned only minimal profits (Blumenstyk 2000). It therefore appears that if only a minimal number of academic scientists generate products or companies that result in new revenue streams for universities, then the level of resources allocated to commercial endeavors is an ineffective use of scientific talent in the academic profession.

Furthermore, evidence suggests that commercial endeavors in academic science may threaten the integrity of the research process. For example, one study analyzed 800 papers published in fourteen academic journals and demonstrated that over one-third of lead authors based at institutions in Massachusetts either owned patents, equity, or held positions at companies that stood to benefit from the research (Krimsky et. al 1996). In a detailed analysis of thirteen examples of clashes between academia and industry, Washburn (2005) shows that such conflicts of interest materialize with outcomes such as the suppression of negative studies, the use of paid consultants to appear as objective experts, and the requirement that university scholars seek the permission of corporate sponsors to publish research. An oft-cited dramatic example occurred at the University of Pennsylvania, where a teen died in a gene therapy experiment in which negative results from earlier experiments were suppressed from patient consent forms and institutional review. Twenty-percent of the research budget of the laboratory at which the incident occurred was funded by the startup of company of a University of Pennsylvania professor who, along with the university, held a direct financial stake in the outcomes of the research (Nelson & Weiss 1999). In the wake of the death, the National Institutes of Health found that 80 universities had failed to disclose a total of 652 adverse-event reports in corporate-sponsored research, as they were considered by investigators and sponsors as “confidential commercial information” (Washburn 2005).

Scholarship on consequences such as the changing opportunity structure of academic fields, financial returns of commercialization, and unethical conduct are all inherently connected to the academic profession. But few studies have been designed that enable a general statement about the implications of commercialization for academe's status as a profession, primarily because they overlook the significance of scientists, both commercialists and traditionalists. Therefore, the final research question this study addresses is: *What are the consequences of a commercially-oriented reward system for the institution of science, universities, and the academic profession?*

Conclusion and Overview

I have argued that discussions of the commercialization of research in academic science are incomplete for two reasons. First, studies have either underemphasized or ignored scientists' interpretations of the commercial reward system. Second, studies have failed to account for the meanings non-commercial scientists assign to the operation of commercial rewards. As a result, the existing literature inadequately addresses questions connected to the meanings, norms, mechanisms, and consequences of commercialization. I aim to provide a new view of commercialization, as seen from the perspectives of scientists who work at universities that are among the most commercially-productive academic environments in the United States.

In the next chapter, I account for the design of the study. The central purpose of the chapter is to establish how the methods employed in the study follow from the questions it addresses. I thus explain the logic of the study methodology and the objectives of the sampling strategy. I characterize the universities at which the study was conducted and the scientists who participated in this research. I detail all of the procedures I employed in collecting and analyzing the data. Throughout the chapter, I discuss the data to: explain the sampling frame; account for the appropriateness of the institutions as contexts for this research; and address the significance of the accounts of the scientists who participated in this study.

The remainder of the dissertation proceeds according to the research questions discussed previously in this chapter. But each chapter, we will see, presents findings of relevance to multiple research questions posed in the study. Chapter three examines the influence of commercialization on the meaning of scientific careers (research question 1). I examine four dimensions of scientific work that constitute moral orders of commercialist and traditionalist science: work organization, bases of status, perceptions of visibility, and dirty work designations. The chapter shows that, in contrast to arguments of sociologists of science and professions, commercialist and traditionalist moral orders diverge along these dimensions such that overall, commercialists exhibit a pattern of professional rebellion whereas traditionalists embrace professional purity.

In chapter four, I examine research question 3, the social mechanisms by which scientists embrace commercial pursuits. This chapter takes us into the circumstances in which scientists embrace or forego commercial trajectories. The first part of the chapter considers the temporal context of commercialization by examining the timing of the initial commercial acts that served as turning points in the careers of commercialists and the exposure all of the scientists had to commercial culture during professional socialization. In the second part of the chapter, I consider the factors that attract, facilitate, or constrain scientists' identification with commercial pursuits. Overall, this chapter illustrates that socialization plays a powerful role in enabling and constraining identification with commercial career paths among both categories of scientists.

Whereas chapter four examines why scientists embrace commercialist or traditionalist career paths, chapter five develops an explanation of how this occurs. In this chapter, I examine new forms of social-psychological identity work in science that have emerged in response to the commercially-oriented reward system. In the first part of the chapter, I examine processes of legitimation. This section demonstrates four techniques commercialists employ to cope with perceptions that view commercialization as morally questionable. In the second part of the chapter, I examine processes of disidentification. This section demonstrates four techniques that traditionalists employ to affirm the purity of their identity. The chapter reveals the ways in which the commercialist and traditionalist identities diverge. It also shows how the process of learning definitions of work that define an act as right or wrong is critical to role identification.

In chapter six, I consider the influence of the commercial reward system on the normative structure of science (research question 2). The chapter is organized around the traditional norms of science to assess the extent to which commercialists and traditionalists adhere to "Mertonian" norms of science. The findings specify the circumstances under which commercial activities and commitments are in tension with the norms. The central themes the chapter considers include: whether scientists should be concerned with the utility of their research; whether scientists should be rewarded for commercial activities; and the contexts of secretive behaviors connected to commercialization.

Chapter seven, the concluding chapter, consists of a summary of the study, a discussion of the findings, and recommendations for further research. The chapter begins with an overview of the conclusions of the study. I then organize the discussion around the research questions that motivate the research. Drawing on the findings from each of the empirical chapters, I consider the theoretic implications of the study. Although no new data are presented in this chapter, I will take stock of what we have learned to address research question 4: the consequences of commercialization for science. I will account for the theoretic contribution of the study. The chapter concludes with a discussion of the policy implications of this research and directions for future inquiry.

CHAPTER 2

STUDY DESIGN AND METHOD

The purpose of this chapter is to describe and explain the study design. The chapter is organized into four sections. In the first section, I explain the sampling strategy and introduce the criteria for inclusion in the study. To do so, I present an empirical framework that formed the basis of the sampling frame through which the universities and scientists in the study were selected. In the second section, I characterize the four universities at which the study was conducted and present indicators of commercial activity at each site that attest to the appropriateness of these universities to study's questions. In the third section, I discuss the interviews. I account for the means by which I elicited the participation of scientists and describe the ways in which the sample is consistent with comparative dimensions identified in chapter 1. I explain the interview protocol, its organization, and the questions it poses. In the fourth section, I describe how the interview data were analyzed.

Sampling Strategy

The research questions and theoretic framework in which they are situated are concerned with advancing an understanding of commercialized research that assumes social phenomena cannot be understood without knowledge of the meanings that individuals assign to them (Lincoln & Guba 1985; Strauss & Corbin 1990). Moreover, a quantitative approach to understanding scientists' interpretations of the reward system would impose a limited worldview on the phenomenon (Marshall & Rossman 2006) and is unable to generate the richness or complexity of detail for which a qualitative approach is capable. Therefore, I conducted 61 in-depth interviews with scientists and compared the perceptions of the new reward system across three key dimensions: commercial and traditional academic scientists, institutional type (public and private), and the time period in which they received their doctorate (pre- and post-Bayh-Dole).

As I will shortly illustrate, I studied scientists who are situated in organizational contexts where the dynamics of commercialization are most pervasive. This dimension of the study design follows a straightforward logic. At a minimum, it is necessary to collect data where an infrastructure for commercialization is present. More importantly, because we must consider the views of both commercialist and traditionalist scientists, it is critical to collect data at sites where there is a collective recognition of a culture of commercialization. It would be of no benefit to sample the views of scientists who are unaware of or uninformed about the opportunity to commercialize one's work, for they would lack the opportunity to embrace, eschew, or meaningfully know about commercialization. Furthermore, to fully understand the meaning of commercializing one's work, it is necessary to sample scientists whose careers are indicative of an embrace of the commercially-oriented reward system. In a sense, this is to study the process where it is most "pure" or "potent." The sampling strategy thus seeks the most complete articulation of a commercially-oriented career, for our understanding would be limited by collecting data from scientists who "dabble" in commercialization. It was therefore critical to: establish an empirical basis for what constitutes extensive involvement in commercial activities; know whether there are particular academic fields in which commercialization is prevalent; and determine the universities at which commercialization is pervasive. This information, in turn, provided the criteria for inclusion in the study among commercialists and the basis of half of the sampling frame from which all study participants were selected.

To generate a sample of commercialists, I constructed a dataset that documents the number of patents, academic field, graduate institution, and year of PhD conferral of 2,593 academic scientists. Patent activity provides a robust proxy of commercial engagement. It formally signifies scientists' decision to either commercialize the product of their labor or permit another entity to use their work commercially. While not all patents end up as commercial outputs, data on all patents ever assigned are available through the United States Patent and Trade Office (USPTO). Patent data are thus the most reliable measure of commercial activity in academe because information on patent licensing arrangements or the founding of companies is not systematically available.

I focused on patents assigned to scientists at the 60 U.S. member institutions of the Association of American Universities (AAU) between 1960 and 2005. The AAU is representative of a target population of commercialists because it optimizes inclusion of research-intensive universities. In 2008, for example, AAU universities received 58 percent of all federal research funding and hosted 82 percent of the members of the National Academies of Science (American Association of Universities 2010). The *Dissertations Abstracts* database, which provides the graduate institution, doctoral field, and year in which the PhD is received by any individual who has received a PhD from a U.S. university, provided a database in which I could search for information on any individual listed among the population of 38,228 inventors from which the sample was drawn.

I created the sample because the population was too large to analyze and it contains non-academic inventors who collaborated with university scientists on a patent. To ensure the ultimate sampling frame would include the most commercially-prolific academic scientists, I purposefully employed a set of procedures that would bias construction of the AAU sample toward more commercially-active scientists. Because the inventor dataset indicates both the universities to which patents were assigned and the middle initial or complete name of patenters, I was able to use the combination of *Dissertation Abstracts* and the internet to reliably identify 892 individuals with current university appointments of the 1016 inventors who comprise the top three percent of patenters in the AAU inventor population.[1] The total number of patents held by this group represents 20 percent of the all patents listed in the population. To ensure my efforts identified the largest volume of scientists possible who currently hold appointments at U.S. universities, I also compared names in the AAU inventor population with rosters of scientists in physics, chemistry, biological sciences, engineering, and medical school basic science departments of the top ten patent generating universities (a total of 160 departments). Each of these departments was selected based on the logic that commercialist scientists are more likely to hold appointments in these organizational units than other areas of the academy. I also analyzed assigned patents applied for in each decade between 1960 and 2000 and coded the field, graduate institution, and year of graduation of the scientists in the top tiers of patent productivity. I established a top tier of scientists by selecting a "patent floor" above which I would analyze every inventor (with the exception of the 1960s, which had a small population). For example, I analyzed all inventors who were assigned three or more patents during the 1970s. Selection of the patent floor required that a manageable number of scientists be analyzed while representing a large enough portion of the population to adequately characterize the most prolific patenters in

[1] Individuals for whom I was unable to collect data likely lack PhDs, work in non-university organizations, or were doctors (MDs) whose identification proved difficult to ascertain with certainty.

each decade. For instance, while I was able to code 282 of the inventors with 3 or more patents assigned during the 1970s, using this patent floor for inventors in the 1990s would have required the analysis of about 4,500 scientists (I therefore focused on those who possessed 11 or more patents during the 1990s). Due to previous steps in my coding procedure, I already possessed data on a large number of scientists for each decade and thus the total number of scientists coded in each decade grew substantially beyond top tier patenters. Individuals for whom an academic appointment could not be confirmed were removed from the dataset. These procedures resulted in a sample of 2,593 scientists, which I henceforth refer to as the AAU Sample.

Patenting

To operationalize extensive commercial involvement, I examined patent productivity of the scientists in the sample relative to those in the population. In table 1, I characterize the distribution of patent productivity by decade for the AAU sample and the population. Across the sample and within each decade, the average number of patents per scientist in the AAU sample consistently outnumbers the averages of scientists in the population. Overall, scientists in the AAU sample have an average of 8.25 patents, whereas those in the population average 2.27. The median values of the sample and population are 5 and 1, respectively. Moreover, if we examine

Table 1: Patent Productivity by Decade

	1960s		**1970s**		**1980s**		**1990s**		**1960-2005**	
	Pop.	**AAU Sample**	**Pop.**	**AAU Sample**	**Pop.**	**AAU Sample**	**Pop.**	**AAU Sample**	**Pop.**	**AAU Sample**
Patenters	376	214	2201	413	6044	818	23015	1761	38228	2593
% w/1patent	83%	82%	69%	31%	72%	40%	61%	25%	60%	22%
% w/1-5 patents	99%	99%	97%	86%	97%	79%	95%	60%	94%	54%
% w/6-10 patents	1%	1%	3%	10%	2%	15%	4%	23%	5%	17%
% w/ >10 patents	0%	1%	1%	4%	1%	5%	1%	17%	2%	29%
Avg # of patents	1.4	1.4	1.7	3.5	1.7	3.7	2.1	6.3	2.2	8.3

the proportion of scientists who possess six or more patents in the population of academic scientists, we see that almost half (46%) of the AAU sample comes from the top seven percent of the population. Given these figures, I employed the median value, five patents, as the primary criteria I used for inclusion in the sampling frame. Having five or more patents is a relatively rare phenomenon and as a criterion for inclusion ensures that the sampling frame is not biased towards scientists in later career stages.

I also included scientists in the sampling frame who had founded a company or whose work formed the basis of a licensing agreement, regardless of the number of patents they possess, for these activities constitute extensive commercial engagement. Although a scientist may opt not to participate in a company that licenses a patent based on his or her work, it is frequently the case that – because of expertise – the scientist will consult for or sit on the scientific advisory board of, the company licensing the patent. Forming a company constitutes the most extensive form of commercial involvement, as it entails participation in or governance of a broad set of decisions and processes to coordinate in the design, production, distribution, and marketing of a technology. In sum, scientists in the commercialist portion of the sample had to meet one of the three following criteria for inclusion in the study: *possession of five or more patents*; *generation of one patent licensed by an existing or new company*; or *formation of a company*.

Field

I analyzed the fields in which patenters received their PhDs to identify the fields in which commercially-oriented career paths are most likely to be found and the relative degree to which the dynamics of commercialization are more or less intense. I needed to know whether the study should be conducted in the context of one or several fields. To date, no study has documented the field context of commercialization in academic science. Instead, scholars have relied on "technology classes" (e.g., "Drug: Bio-Affecting and Body Treating Compositions") employed by the United States Patent and Trade Office (USPTO) to speculate about the scientific fields in which patenting is most common. Such an approach says little about the group basis of commercialized science and is methodologically unreliable as any number of fields could patent in a given area. A more reliable approach – that which is utilized here – is to document the field in which known patenters received their doctorate.

A general conclusion to be drawn from an analysis of the field basis of commercialization is that the new reward system exerts a broad influence: fourteen fields are represented in the AAU sample. The top five fields include chemistry (16.9 percent), electrical engineering (15.8 percent), biological sciences (14 percent), physics (11.3 percent), and medicine (11 percent). Other fields that constitute the remaining 30 percent include biochemistry, agriscience, earth science, mathematics, and a variety of engineering fields. I took two steps to ensure these results were consistent with broader patterns in the data. First, I delimited analysis to the 871 scientists who possess 10 or more patents, which indicated a consistency between the AAU sample and the most productive patenting scientists in terms of the distribution of fields. Again, chemistry generated the largest number of scientists (21.7 percent), followed by medicine (16.5 percent), biological sciences (13.9 percent), electrical engineering (12.6 percent), and physics (11.6).

Second, to "control" for historical change in prevalent fields of commercialization, I analyzed the field composition temporally by looking at each decade between 1960 and 2000. I present the results in table 2. Although chemistry experiences a decline in relative terms over the last four decades of the 20th century, the data illustrate that the discipline has consistently ranked above all others engaged in patenting. One may infer from the table that the growth and decline

Table 2: Composition of Commercialists by Field and Decade in Percentage Terms, Selected fields

Field	1960s	1970s	1980s	1990s	Pattern
Chemistry	20.75	22.47	19.20	16.21	Growth/decline
Biological Sciences	5.66	6.67	10.22	16.21	Growth
Electrical Engineering & Comp. Science	23.58	16.05	15.34	14.68	Decline
Medicine	5.66	8.64	12.09	13.09	Growth
Physics	6.13	13.58	14.46	10.51	Growth/decline
Biochemistry	4.72	5.43	7.86	8.93	Growth
Chemical Engineering	2.36	3.95	3.74	4.87	Growth
Mechanical Engineering	4.72	5.68	5.49	4.29	Steady
Materials Science & Engineering	2.36	5.43	3.37	3.58	Growth/decline
Agriscience	9.43	6.17	4.74	3.17	Decline
Bioengineering	0.00	0.49	0.50	0.76	Growth
# of scientists identified in sample	212	405	802	1762	
# of scientists in population	376	2201	6044	23015	
Percent represented in sample	56.38	18.40	13.27	7.66	

of particular fields in patent activity reflects the changing political-economic structure in society and the extent to which universities have been "tapped" for particular outputs. Agriscience, for example, including fields such as agronomy and plant genetics, was among the first fields in the academic profession to play a key role in economic development as it related to university-based research (Geiger 1988), though it has declined over time as the economy has shifted from farming, to manufacturing, to information-based industries. The rapid growth of physics between the 1960s and 1980s and its subsequent decline reflects parallel patterns in funding from the Department of Defense (Slaughter and Leslie 1997) as the Cold War heated and cooled. Finally, the growth in patenting among medical fields, chemists, biochemists, and biologists is indicative of the economic shift towards the exploitation of life science discoveries as a basis of new economic growth. The continual top rank of chemistry may be understood given the broad sectors of the economy to which it is relevant, including old bases of industry such as energy, and newer sources of innovation, such as pharmaceuticals and nanotechnologies.

Given the diversity of fields in which the dynamics of commercialization are found, I did not delimit the study sample to any particular field in order to adequately represent the degree to which the new reward system exerts its influence across academe. Some scholars argue that studies of science should be delimited within fields because different fields are characterized by different career patterns, norms, and practices (Becher and Trowler 2001) or because scientists measure their careers in reference to their specialties (Marcson, 1960; Kornhauser 1962; Ritti 1968). Delimitation to one field, however, would not have been conducive to a practical research design. For example, looking at universities that host five or more chemists (the largest commercialist group, see table 2), 13 universities comprise a total of 92 scientists. Given the nature of the time investments of the scientists to be interviewed, it would have been risky to assume 30 of these 92 scientists would have been able to participate and impractical to travel to numerous universities.

More importantly, this is not a study of chemistry, biology, or physics. It is a study of a process: a reward system that is not only transforming these fields, but many others within the university. Thus, confining the sample to one field would not only have constrained a practical research design, such a design would not adequately represent the extent to which the culture of commercialization has spread throughout modern universities.

The Universities

The presence of commercialists who meet the criteria for inclusion in the study provided the chief basis on which I identified the universities at which I conducted the study.[2] Consistent with the discussion of institutional type in chapter 1, I selected two public and two private

[2] After eliminating emeritus faculty, staff scientists without appointments, and medical school faculty.

universities across the United States. Together, the four universities generated a sampling frame of commercial scientists (N=191). Using the AAU patent data and the AAU sample of scientists, I was able to preliminarily confirm that the four universities in the study met the sampling objective of selecting universities that exhibit a recognizable culture of commercialization. First, analysis of the number of patents assigned to each AAU university between 1960 and 2005 revealed that each university in the study is ranked among the top ten universities in terms of number of patents assigned. Second, by analyzing the doctoral origins of faculty in the AAU sample, I determined that the universities are likewise among the top ten universities in terms of socialization of commercialists. Whereas these rankings provided a preliminary confirmation of the sampling strategy, other indicators of commercial activity, which I will discuss momentarily, unmistakably confirm that the universities in this study constitute environments in which commercialization is pervasive.

In table 3, I present an overview of selected organizational characteristics of each of the universities in the study. All four universities in the study are classified as Carnegie Doctoral/Research Universities-Extensive, a classification that reflects the broad range of baccalaureate and doctoral programs they offer in the humanities, social and physical sciences, and professional education. Table 3 shows that relative to the public universities, the private universities tend to have higher salaries and smaller overall enrollments. Apart from these distinctions, the four schools cohere in ways that reflect the intensity of and their reputations for research. For example, relative to the average AAU university (itself comprised of elite research universities), the universities in this study have on average approximately 1,000 more graduate students and two to three times as many post-doctoral researchers (with the exception of School 1, which is about equal to the AAU average). The four universities spent nearly one to three

Table 3: Organizational Characteristics of Study Sample [a]

	Public		Private	
	School 1	School 2	School 3	School 4
Total number of faculty[b]	≈1700	≈1500	≈1600	≈1200
Average faculty salary[b]	≈$85,000	≈$105000	≈$110000	≈$110000
Total enrollment[c]	≈40,000	≈30,000	≈20,000	≈10,000
Number of fulltime graduate students[c]	≈4000	≈5000	≈5000	≈5000
Number of postdocs[c]	≈500	≈1250	≈1500	≈1000
Total R&D Expenditures 2000-2007 (in billions)[d]	≈ $5.75	≈$4.00	≈$4.75	≈$4.00
Percent of R&D funded by industry 2000-2007[d]	≈2.5%	≈4.75%	≈6.25%	> 6.0%

[a] Reporting of data is approximated (i.e., "≈") to ensure the anonymity of institutions.

[b] Source: Integrated Postsecondary Data System (2007)

[c] Source: National Science Foundation Survey of Graduate Students and Post-doctorates in Science and Engineering (2006)

[d] National Science Foundation Survey of Research and Development Expenditures at Universities and Colleges (2000-2007)

billion dollars more on total research and development expenditures between 2000 and 2007 compared to the average AAU university ($3 billion). Of this funding, each university has received a substantial amount from industry. During the period 2000 to 2007, the average AAU university received about $167 million dollars in funding from industry, or about 5.5% of the average total research and development expenditures. Schools 1, 2, and 3 are relatively close to their AAU peers in terms of the proportion of industry funding received for research. School 4, by contrast, received a substantially greater proportion of industry funding. Given the absolute value of the total expenditures for research, each of the schools received substantially more money from industry than their AAU peers.

Although the public and private schools are generally similar in these aspects of their research infrastructures, substantive differences emerge when we observe indicators of commercial activity. In table 4, I present five indicators of technology transfer activities of the study sample and the average AAU university over the course of the period 2000 to 2007. To ensure the anonymity of the institutions in the study, I have averaged the total level of activity

Table 4: Technology Transfer Activities 2000-2007, Study Sample & AAU[a]

	Publics Avg. (Schools 1&2)	Privates Avg. (Schools 3&4)	AAU Average[b]
Invention Disclosures	2,000	3,340	1557
Patents Assigned	525	999	364
Licenses Executed	1,461	936	446
Startups	52	128	43
Total License Income (In Millions)	$231	$360	$153

[a] Source: Association of University Technology Managers U.S. Licensing Activity Surveys 2000-2007

[b] N=53. Brandeis, Columbia, Princeton, Syracuse, Texas A&M, University of Missouri, and Yale were dropped from analysis due to excessive missing data.

according to the public/private grouping of the schools.[3] Across all indicators, the schools in the study substantially outperform the typical level of activity of AAU universities between 2000 and 2007. The level of invention disclosures, the first notification to a university that a scientist has generated a potential basis of intellectual property, is nearly twice the AAU average among the privates and slightly above average among the public universities. Schools 1, 3, and 4 are among the top 10 AAU universities in terms of invention disclosure, whereas School 2 ranks in the top 25. In terms of the total number of patents assigned between 2000 and 2007, School 3 ranks in the top fifteen whereas School 1, School 3, and School 4 are ranked among the top 10. Relative to the average AAU university, both private schools in the study were assigned nearly three times the level of patents, about 1000 each, whereas the public schools received about one and one-half times more, approximately 500 each.

Whereas invention disclosures and assigned patents provide an indication of the level of commercially-oriented research that scientists conduct at a university, the number of licensing agreements executed with new or existing companies and the money received for it reflects the

[3] Given the magnitude of some aspects of the schools' technology transfer activity, reporting approximations of certain measures does not adequately eliminate the possibility that the identity of the schools could be revealed.

extent to which such scientific work achieves commercial success in the market. All four of the schools are among the top ten universities in the AAU in terms of licenses executed, the only technology transfer activity in which the two public universities in the study outperform the two private universities. School 2 generated the most licensing agreements, followed by Schools 1, 3, and 4. Together, the four schools generated over 5000 licensing agreements between 2000 and 2007, roughly 20 percent of the total level of licensing activity among AAU universities in this period.

In terms of the number of startup companies generated by the schools, over 300 new companies were formed between 2000 and 2007 as a result of technologies developed by the four schools in the study. Schools 3 and 4 generated more than twice as many startup companies as Schools l and 2, whose level of startup activity is slightly higher than the average university in the AAU. Finally, looking at the level of income generated as a result of the technology transfer activities of the universities in the study, we see a familiar pattern. Relative to the average AAU university, Schools 3 and 4 received about twice as much licensing income, or about $360 million each, while Schools 1 and 2 received about one and one-half times the amount, or about $231 million each. Together, the four schools generated nearly 1.4 billion dollars in licensing income during the years 2000-2007.

The Interviews

Contacting scientists

The interviews for this study took place during the Spring of 2010. Potential respondents were contacted by mail approximately three weeks before I visited each university. The purpose of this letter was to describe the goals of the study, describe the scope of respondents' involvement, and to notify the scientists of my intent to contact them by phone to address questions and invite their participation. The letter also emphasized the anonymous nature of respondents' participation, a point I would later reiterate by phone and immediately before each interview. A sample of the contact letter appears as appendix A.[4] At each school, I scheduled interviews with commercialists first to ensure that the traditionalist sample would be derived from the same departments in which commercialists are situated.

Some scientists or their administrative assistants contacted me by email upon reception of the letter, to either express their willingness to participate or decline. The majority of the individuals to whom I mailed contact letters, however, were telephoned approximately one week after they received the initial letter. I telephoned potential respondents to discuss the study in more detail, to explain my interest in their participation, and to ask them to participate in the study. In several conversations, scientists commented on the importance of the study. One noted, for instance, that commercialization in academe "is not well understood." The majority of scientists who agreed to participate either did so outright or asked why they were subject to the invitation. Having established criteria for inclusion in the study, I possessed information about each scientist I contacted that enabled me to effectively communicate the merit of participation and the legitimacy of the study. Most scientists agreed after such explanations. Other respondents agreed after assurances of the anonymity of their participation.

Only one scientist, the first commercialist I contacted, declined out of concern for anonymity. He told me that he was "too paranoid to participate" and noted in vague terms a negative experience that ultimately ended his "love of invention." He later emailed me:

[4] For other models of contact letters to scientists, see Hermanowicz (1998; 2009). Zuckerman (1977) also provides a discussion of the merits and methods of contacting scientists with letters, whereas Rubin and Rubin (2005) advocate this approach in general.

> I’ve carefully considered your study and what you want to do, and find it quite admirable. At this point, I still find myself not to be objective about my own experiences and think I would not be a good subject for your studies. Too much emotion is tied up here that I do not wish to share. I must therefore decline your invitation.

My interaction with this scientist indicates two key aspects of the study design. With respect to its theoretic design, such sentiments allude to the consequential nature of commercialization. Discussing one’s work is a typical, if not regular, occasion in a scientific career. The presence of a discomfort or perceived risk of doing so, forged by a previous experience, suggests that scientists’ involvements in commercialization may bear individual consequences that shape the meaning of work and the career avenues one pursues. Methodologically, my interaction with this scientist offered preliminary support for the validity of the study design, which seeks to examine commercialization in the environments in which its dynamics are most pervasive. Paranoia connotes a fear of retribution, suggesting not only the presence but power of the culture of commercialization at the empirical sites of the study.

Some scientists did not participate because they were not interested, but the majority of scientists who declined did so because of extensive commitments. Frequently in colorful terms, scientists expressed an interest but inability to participate:

> I'm suffocating right now. Every hour I would spend with you would be one less I can sleep.

> Surely there are hundreds of faculty in [this city] alone who don't commercialize their work, many of whom are not junior faculty in a pre-tenure year with small children at home. If there is some reason that only I can fit the bill, please do let me know.

> I really do wish I could help but I am in a very crazy period where I am working to raise much needed funds for me and my lab. Until I solve some of my own needs I cannot afford to help with the many requests I get for my time.

Similar comments about the scope and weight of demands on scientists' time frequently emerged in the interviews, particularly among traditionalists who questioned the ability of commercialists to fulfill both academic and commercial commitments. Such comments point to the value of a scientist's time and thus the value members of the study's sample assigned to participation in this research. Overall, 64 percent of the scientists I either contacted by phone or who contacted me before I could call agreed to participate in the study. This rate of response is only slightly lower than other interviewed-based studies of scientists, whose response rates were approximately 70 percent (Gaston 1973; Zuckerman 1977; Hermanowicz 1998; 2009).

In table 5, I present the distribution of the commercialists and traditionalists who comprise the interview sample. The table demonstrates that the sampling strategy is almost perfectly consistent with the comparative dimensions generated by the theoretic design of the study. The sample is comprised of 32 commercialists and 29 traditionalists. Within each group, scientists are evenly divided between pre- and post-Bayh-Dole eras of entry into science. Similarly, the sample is evenly divided across institutional type. The resulting distribution

Table 5: Distribution of Commercial and Non-Commercial Scientists, by Type of Academic Institution and Cohort

	Commercial		Non-Commercial		
Institution	Pre-1980	1980 -	Pre-1980	1980 -	Total
Public					
School 1	4	4	4	4	16
School 2	4	4	5	2	15
Private					
School 3	3	5	3	4	15
School 4	4	4	4	3	15
Total	15	17	16	13	61

reflects that each of the eight possible categories of scientists (i.e., Pre-Bayh-Dole commercialist at a private university, pre-Bayh-Dole traditionalist at a private university, post-Bayh-Dole commercialist at a private university, etc.) is evenly distributed across the sample, with seven or eight scientists in each category. In terms of gender composition, 52 of the scientists are male and 9 are female (about 15 percent). Among the females, 2 were commercialists and 7 were traditionalists.

The scientists who participated in the study represent 8 academic fields. Table 6 presents the distribution of academic fields among the scientists in the sample as a whole and the commercialist and traditionalist categories. Chemists comprise the largest group of scientist in each of the categories and account for approximately 41 percent of the sample, followed by the biological scientists, who comprise about 31 percent of the sample. This distribution was determined by the department in which each scientist held their primary appointment and does not account for the fact that some scientists received their training in different fields or hold more

Table 6: Distribution of Academic Fields Among Scientists, by Category

Fields	Total Sample	Commercial	Non-Commercial
Chemistry	25	13	12
Biological Science	19	8	11
Chemical Engineering	6	3	3
Electrical Engineering	5	5	0
Biological Engineering	3	1	2
Materials Science and Engineering	1	1	0
Mechanical Engineering	1	0	1
Physics	1	1	0
Total	61	32	29

than one appointment. Furthermore, interview data demonstrate that research specializations traverse numerous areas of scientific inquiry, such that departments and fields obscure the diversity of one's work. One chemical engineer in the study, for example, conducts research on electrochemical power for batteries, drug delivery systems, and nanomaterials for chemical and biological defense – areas of interest to physicists, electrical engineers, chemists, and biologists.

The level of commercial activity among the 32 commercialists in the study is extensive. In table 7, I present selected commercial activities to illustrate this point. The number of patents per commercialist ranged from 0 (in the case of a chemist who started a company based on a copyright) to 250. All of the commercialists had entered licensing agreements. The percentage of a scientist's patents licensed ranged from 14.5 percent to 100 percent. On average, 51.4 percent of commercialists' patents formed the basis of licensing agreements with new or existing companies. This is a particularly high level of commercial engagement, given the patent productivity typical of the scientists in this sample.

Licensing agreements may be entered with new or existing companies. A total of 249 companies have licensed the work of the 32 commercialists in the study. Seventy-eight of these

Table 7: Selected Dimensions of Commercialization among Commercialists

Activity	Average	Range
Patents invented	29.8	0 - 250
Number of patents licensed	10.6	1 - 80
Percentage of patents licensed	51.4	15 - 100
Companies licensed to	6.1	0 - 60
Startups founded	2.4	0 - 15

companies (31 percent) are startups that were formed by the scientists in the study. Seven of the startups are now publicly-traded corporations and nineteen were acquired or merged with other companies.[5] All but six of the commercials had started at least one company. In other words, over eighty percent of the commercial scientists had formed at least one startup. Among these 26 scientists, the number of startups ranged from 1 to 15. Half of the 26 scientists had one startup company, nine had two to four, and four scientists had formed six or more companies.

These figures underscore the significance of the commercialists' accounts. These activities far exceed the criteria for commercialist inclusion in the study (having either formed a company, licensed a patent, or having five patents). Thus, in terms of the both institutional and individual measures of commercial activity presented in this chapter, it is evident that we have located scientists at universities who are well positioned to address the questions that motivate this study. In short, we see that the responses that commercialists and traditionalists give to interview questions are based on extensive exposure to a culture of commercialization.

The interview protocol

In advance of each interview, I prepared a "career map" that documented available information on the doctoral origins, post-doctoral experience, academic appointments, and, for commercialists, the temporal structure of scientists' patent activity and other aspects of

[5] Data on company mortality was not collected.

commercial behavior (such as membership on boards, executive positions, licensing arrangements, or company founder status). The purposes of this procedure were to increase the amount of detail attainable in each conversation (perhaps not otherwise elicited), communicate to the participants the seriousness of the interview, and legitimize the demands on the participant's time (Zuckerman 1977).

Each interview began with a brief introduction to the study. I described the study and its objectives, informed scientists of their rights as research participants, explained the anonymous nature of their participation, and requested their consent to be recorded. The questions I asked scientists were divided into four interrelated sections: 1) Conception of the scientific role; 2) Motivations of entrepreneurialism; 3) Norms of science; and 4) The operation of reward systems. Each section and that questions that comprise it were designed to correspond to specific research questions and related theoretic arguments. However, given the interrelated nature of the sections and the inclusion of open-ended questions, responses are relevant beyond the specific questions they were designed to address. The interview protocol is presented in appendix B.

The first section of the interview addressed research question 1, which seeks to understand the meaning scientists attribute to commercialized science. The interview commenced with this section because the first set of questions pertained to topics which scientists are accustomed to discussing or have given much consideration. In effect, the first four questions collect important data while "warming up" study participants before posing difficult or threatening questions. In the first four questions, I asked scientists to discuss the types of work pursuits that they regard as most esteemed and most trivial, who benefits from their work, and how they perceive their own work to fulfill the ideals of the pursuits they most esteem. Question five directly addressed commercialization, asking whether and why it is or is not a legitimate way to fulfill the academic role. By not posing a direct question about commercialization until question five, any view of commercialization that emerged in discussions of esteemed or trivial scientific work surfaced independently. In the last question in the section, I asked scientists in each group (commercial and traditional) what distinguishes them from the other. The questions in this section were designed this way to generate data regarding scientists' perceptions of an occupational mandate, the extent to which they perceive commercialization to be consistent with it, and the symbolic boundaries they draw between desirable and undesirable scientific work.

Section two of the protocol addressed research question 2, why scientists embrace or eschew commercial practices. I asked scientists to discuss the extent to which they have considered or have actually commercialized their work over the course of their career. I also asked them to discuss the circumstances under which they would or did commercialize their work. By beginning the section in this manner, the questions generated non-directed explanations of the social forces that shape the norms embraced by scientists. The next questions I asked were derived from two specific theoretic issues. I asked scientists to discuss their conceptions of university-based commercialized research as graduate students and the ways in which they were exposed to it, which addresses any role played by occupational socialization. Finally, I asked scientists about ways in which commercialization could or had enabled them to achieve scientific goals that would not be possible through other means.

Part three of the protocol addressed research question 3, which focuses on the consequences of a commercially-oriented reward system for the norms of science. I began by asking scientists what the relationship should be, if any, between research and commercialization. I asked scientists to discuss examples of excesses or abuses to understand what constitutes a departure from the ideal. I also asked about the extent to which consensus exists within departments concerning the appropriateness of commercializing one’s work. I then asked scientists to discuss four issues: if universities should reward scientists for commercialization; if they perceive there to be problems associated with academic researchers accepting money from industry to conduct research; perceptions of flaws associated with basic science as a mode of inquiry; and if scientists should be concerned with the commercial utility of their discoveries. The objectives of this series of questions were to generate a measure of consensus/dissensus surrounding ideal behavior and expectations for role performance and render characterization of contemporary norms of science and their consequences.

The final section of the interview pertained to research question 4 and sought to understand the concrete ways in which a commercially-oriented reward system operates. The questions in this section were designed to generate data on the costs and benefits of commercialization for scientists, universities, and the institution of science and the specific role that a profit-motive plays in scientific achievement (themes which inevitably factored into responses to prior questions). The first three questions focused on costs. In this series of questions, I asked scientists about their perceptions of the costs of commercialization, whether or not they believe or have observed it to corrupt science, and the ways in which they believe it influences scientific progress in their field. I then asked scientists to discuss how science can benefit from commercialization of academic research. The final three questions of the protocol were devoted to understanding how the reward system impacts individual careers. I asked scientists how important making money is to their career in order to understand the way financial rewards operate. In the next question, I asked commercialists to discuss the most rewarding aspect of commercializing one’s work. I asked traditionalists to consider what they anticipate would be the most rewarding aspect of commercialization. Following the standard convention of ending an interview on a positive note, I concluded the interview by asking scientists to discuss the achievements of which they were most proud.

Of the sixty-one interviews, I conducted fifty-nine in person, always in the departmental office of the scientists. Two of the interviews were originally scheduled to take place in person, but were rescheduled and conducted by telephone due to personal circumstances of two scientists. The interviews averaged seventy-five minutes in length. All of the interviews were digitally-recorded and transcribed. A total of 2,802 double-spaced pages of interview data were transcribed in preparation for data analysis.

Other sources of data

I obtained two other sources of data after each interview. I obtained copies of the scientists' curriculum vitae. Scientists' vitae are a formal expression of a scientist's identity, as they summarize their past accomplishments, their training, the individuals they have worked with most directly, and the rewards conferred upon them. The vitae thus provide important information about the careers of the scientists in the study. For example, one important source of information on commercialists included whether or not one lists commercial involvement on their vita, which is one expression of the extent to which such activities are perceived as legitimate components of the academic role. Another important measure the vitae provides (in combination with scientists' patent records and the information provided on post-interview questionnaires) is the timing of commercial activity, which provides some indication of the patterns and trajectories indicative of commercial careers.

Second, I provided each participant with a two-page questionnaire to complete that gathered data on the nature and extent of scientists' affiliations with private and public corporations. I present the questionnaire as Appendix C. The questionnaire did not ask scientists to specifically list their affiliations, it requested a numeric representation of the number of patents, licensing arrangements, companies founded, consultant positions, and corporate roles. The questionnaire then asked for information on salary and income from their appointment and commercial activities during the past year using ordinal response categories. Scientists were asked to estimate the level of revenue they generated for the university in the past year, and the number of companies to which they were currently affiliated in which they and/or the university hold equity. Traditionalists also filled out the questionnaire, though the only relevant questions related to salary and consulting positions. The questionnaire informed participants of the anonymity of the information. All but five of the scientists completed the questionnaire. Approximately 75 percent of the participants filled out the questionnaire in-person. The scientists who did not complete the questionnaire suggested in-person that they would, but when contacted later noted comments such as:

> After looking at the form you left with me, I decided that the information you were requesting was not information that I wanted to provide.

> This is too much detail for me to get together and more than I want to share.

Some scientists opted not to provide information regarding salary or alternative sources of income, but completed the remainder of the questionnaire.

Analysis of Interview Data

To analyze the interview data, I employed an inductive coding strategy. Because of the study's theoretic framework, the interview questions inherently generated data associated with existing concepts, yet such themes functioned as orienting devices rather than theories to be deductively tested. Why? A deductive coding scheme could not capture the range of phenomena in scientists' accounts of their work. Thus, to generate a complete codebook, I employed inductive analysis to identify various meaning units that represent different coding categories that emerged in the data.

Upon completion of the codebook, I analyzed the coded data and the broader theoretical implications of the conventions upon which they were constructed (Rubin and Rubin 2005). I sorted codes to reflect the range of variation of properties and dimensions, weighted according to the emphasis participants attributed to the details. I then employed a constant comparative method (Glaser & Strauss 1967), paying particular attention to central tendencies in the responses within categories of commercialists and traditionalists, public and private university contexts, and pre- and post-Bayh-Dole entry into science. My objective was to cull from the data the range of theoretic properties that helped explain each research question posed in the study. In a sense, this required that I "interrogate" the data to determine the extent to which data converged in support of or counter to an argument. For example, I have argued that existing research claims that commercialization is widely encouraged and embraced within the scientific community but does not provide evidence that can account for the views of traditionalists. By comparing commercial and non-commercial scientists' views of the legitimacy of the commercialization of research, I am able to assess this hypothesis that both groups embrace commercialization. As another example, consider another key question of the study: Why do some scientists commercialize their work, while others do not? The study design permits this question to be addressed across multiple dimensions by assessing the convergence and divergence of views by institutional type and era of socialization. The objective is to determine the patterns that connect the experiences and interpretations of scientists and in turn offer explanations of behavior.

In chapter 1, I specified the research questions of the study and their theoretic motivation. In this chapter, I have accounted for the data collection and analysis procedures used to address them and the significance of the accounts that comprise the basis of the study. Having specified these elements of the study framework, I now turn to findings.

CHAPTER 3

MORAL ORDERS OF SCIENCE

The purpose of this chapter is to compare how commercialists and traditionalists construct and define science. This objective presumes that substantive differences exist between commercialist and traditionalist science. As Gieryn (1999) notes, there is no one "real science", there are many "real sciences." Science is embodied locally, producing alternative and often competing definitions of what science "is." As this chapter will show, even when one "holds constant" the local environment of science, competing reward systems lead scientists within the same field or specialty to respond to different sets of conditions. Such distinctive collective representations create a different understanding of science and the activities of scientists. Our theoretic concern, therefore, is the constitution of moral orders of science.

The chapter is organized around the dimensions of scientific work along which the moral orders of commercialist and traditionalist science diverge. To account for the chief properties of each moral order, I interviewed scientists about their conception of the scientific role. I asked the scientists to discuss the pursuits they most highly esteem, who benefits from their work, and the tasks they shun or regard as trivial. Only after assessing these questions did I directly address commercialization. The chapter considers four dimensions derived from the interviews: work organization, bases of status, perceptions of visibility, and dirty work designations. After considering each, I codify the findings to summarize how commercialists and traditionalists construct competing moral orders of science.

Work Organization

Few concepts are as fundamental to understanding the changing nature of work in an occupation as the organization of work, for the structure of the labor process is critical to the experience of work and reflects broader shifts within an occupation and society. Science is no exception. Here our concern is limited to distinctions in the structural properties of commercialist and traditionalist labs and the corresponding logic each group assigns to the organization of work in their research groups.

Let us begin with the view of a commercialist who left a position as full professor at an elite university and divested himself of equity in all of his startup companies (three) to lead a corporate-funded research institute at a public university. His comments are a useful starting point for differentiating commercialist and traditionalist science because they reveal distinctive logics of scientific work, its organization, and its goals.

> To get tenure at [Stanford], twenty people have to say you're in the top five in the world in your topic, so there's no doubt you have to be good at it. But what you actually do they don't care. And that's a huge weakness of universities because it's just random shots academically – little random bits of knowledge into the ether. If you suddenly have a big societal problem, where the hell are you supposed to turn to get a consolidated response to the problem? Who exactly is supposed to solve big problems that require large numbers of people working together? It's not clear that we have a system that really addresses that. So what we're actually doing at [this institute] is to get faculty working together to make a coherent whole so we can address big questions, multidisciplinary questions. I have still a research group, but I spend a lot of my time meeting with faculty,

> bringing together people with complementary expertise and getting them to work together as teams to go after bigger problems than they ever would as a university professor...We're feeding them materials on one side that they can't buy or couldn't get and then we're doing analyses for them downstream. I have a team of analysts and we write scenarios of what we're trying to accomplish and so these faculty can see where their work fits into a bigger picture and actually people like it quite a lot…People like it because they can see they're contributing to something bigger than they could ever do as a professor. [82]

The account makes striking contrasts. Traditionalists, even those at premier universities who are considered among the scientific elite, produce random knowledge of limited relevance to "big" problems in an uncoordinated fashion. Commercialists, by contrast, make contributions that address important problems and supersede the capacity of traditional scientists. Critical to this logic of scientific work, the view suggests, is a division of labor predicated on hierarchy, coordination and large groups comprised of experienced, rather than aspirant, scientists.

This view and logic of work is not limited to the commercialists in the study who lead research centers or institutes at their universities. Consider table 8, which presents ranges and average research group size of commercialist and traditionalist chemists. I delimit the focus to chemists to control for disciplinary differences in the size of research groups. Commercialists operate substantially larger research groups than traditionalists. Commercialists oversee about eighteen researchers, whereas traditionalists run groups of about nine. It is not the case that traditionalist groups are unusually small, as the average in this study is consistent with the findings of Louis et. al (2007), whose study of chemical engineering and life science research

Table 8: Research Group Size among Chemists

	Commercialists	Traditionalists
Graduate students		
Average	12.8	6.8
Minimum	6	2
Maximum	20	12
Post-doctoral scientists		
Average	7	2.8
Minimum	2	2
Maximum	18	4
Total Group size		
Average	17.9	8.7
Minimum	14	2
Maximum	28	4

groups at the 50 universities producing the most PhD.s showed that the average group has seven to eight graduate and postdoctoral researchers. Nor is it simply that specialties explain the difference. For example, of the three theoretical chemists in the sample, the two traditionalists have groups of two and ten, whereas the commercialist group is comprised of twenty researchers. One of the influences on differences in group size appears to be the number of postdoctoral researchers in commercialist labs. Traditionalists typically have two to four postdoctoral researchers, whereas commercialists have seven on average.

The group differences among the chemists are suggestive of distinctive modes of work organization between commercialists and traditionalists. As we will see, traditionalists run what may be understood as *collegial* labs, in that a small group of researchers work collaboratively on a minimal number of grants. Commercialist labs, by contrast, appear to be *hierarchical*, with the average number of postdoctoral scientists frequently comprising half of their research group. To understand the implications of these differences in group structure, I turn to the interview data.

Commercialists

There are two respects in which commercialists oversee hierarchical work forms. First, commercialists frequently oversee research centers and institutes of which commercialization is a central objective. Both federal funding agencies and corporations sponsor centers and institutes that focus on commercialization. The National Science Foundation, for example, funds centers, which it claims "provides an environment in which academe and industry can collaborate in pursuing strategic advances in complex engineered systems and systems-level technologies that have the potential to spawn whole new industries or to radically transform the product lines, processing technologies, or service delivery methodologies of current industries."[1] Unsurprisingly, such institutes are comprised of and frequently directed by commercialist scientists. Eleven, or about one-third of the commercialists, have held positions (or currently do) as directors in such units.

A second way in which commercialist science is organized hierarchically is derived from the fact that commercialization can entail different scientific products or processes performed by different groups. Thus, even if a commercialist does not oversee his or her own research group and other faculty in an institute, governance of both academic and commercialist research operations in a company permit the scientist to approach work with the same coordination and view of "upstream" and "downstream" components of work. What is more, as implied by table 8, such coordination may occur within academic labs, with the labor process divided according to postdoctoral or graduate roles. Commercialists' subjective accounts show that the presence of numerous postdoctoral scientists in their labs is in part motivated by their commercial objectives. Commercialists typically develop new technologies in areas of science for which companies have limited expertise of the related fundamental science or interest in the necessary

[1] www.erc-assoc.org

development that precedes commercialization. This work is usually unpublishable apart from patent filings, and is thus considered inappropriate for graduate researchers. Postdoctoral scientists therefore frequently conduct work considered nonacademic. For example, a commercialist referenced his desire to commercialize research related to a recent publication in *Science* with one of his postdoctoral scientists.

> The technology still needs work…more developmental work that's not appropriate for a university because it's not really knowledge generation, but it's essential to make the thing go. My post-doc…would do this more practical developmental work and really see if there are any glitches in there that would preclude commercialization [and] get it ready to a point where now a big company might be interested in taking it over. There's that intermediate ground where you have to do more applied stuff, not appropriate for the university, but not yet ready for the big company. He would fit that niche right there. [40]

Another scientist references a similar role of postdoctoral researchers that is motivated by commercial ends.

> To publish a paper you've got to have a story, right? Sometimes companies want to see a quick answer. They don't want you to necessarily put together a complete story. And so from the standpoint of funding students, you have to balance that. And that's why when I get a grant from some place outside, my preference is to hire a postdoc, for example, rather than a student, because I think the postdocs are better able to handle that kind of, you know, a little bit of herky-jerkiness, that comes along a little bit. [26]

The type of work that postdoctoral scientists conduct for commercialists is typically situated at the interface of academic and industrial science, rather than at the core of the knowledge base of a field. As a result, their presence in academic laboratories apart from their own career objectives enables commercialists to pursue both academic and commercialist agendas without violating acceptable standards of conduct for graduate training. Postdoctoral scientists therefore enable a division of labor within commercialist laboratories, wherein different ranks of scientists are dedicated to a different stage of development in the process by which fundamental discoveries are "translated" into commercializable products. This division of labor is particularly common among commercialists who have not formed companies, but whose postdoctoral scientists comprise half or more of the overall research group.

Traditionalists

Traditionalists' discussions of the organization of scientific work emerged in their accounts of their own success and the distinctions drawn between themselves and commercialists. They juxtapose their view of how scientific work should be organized with their perceptions of what takes place in commercialist laboratories. The perception that commercialist labs are organized hierarchically figured prominently among the traditionalists. Consider, for example, the view of one chemist who received his doctoral training from a commercialist.

> I'm a middle manager, I guess if you were to say it in the business world. I can run a group of eight to ten people well. I cannot run a group of twenty-five or fifty people well....The type of science I do, I tend to do a little bit smaller science, science that can be done by a group of eight to ten people. Surely down the road my management skills will get better. I'll be able to manage a large group of people, but I don't necessarily ever

> envision being able to run a group of fifty... Many of the folks who start companies, you know, above a certain group size, groups become hierarchical…The advisor becomes the CEO and then he has his middle managers who each are responsible for their, their next level down of things....The second level becomes something like assistant professors. You know, they're not assistant professors, but they're writing grants, they're managing a group of students, they have a project that is theirs, and so they give reports to the advisor and so forth, but it's split down that way. [47]

Other traditionalists developed similar perceptions of their commercialist peers less directly, but nevertheless similarly characterize commercialist labs as hierarchically-organized. One chemist, for example, said the following in a discussion of a departmental peer.

> This guy has, I know of a couple of students that are graduate students, they were educated by postdocs and now he doesn't hire hardly any students and he has only postdocs and higher ups because that makes the research run okay and so he has given up essentially on the goal of what a university is because he does not train the graduate students anymore. On the other hand, he's also director at [a research institute]. How can you do that? I have a group of twelve people, I'm working day and night on this. They have forty or fifty researchers right down there then he's got some companies and then I think he's on the board of a couple of companies. How do you do that? [89]

As a result of this division of labor, traditionalists claim, the conditions of work for doctoral students depart are characterized by routinized patterns of work.

> The work some of my [commercialist] colleagues have their graduate students do just astonishes me. They do things the way it's done in industry. You will have a bench chemist doing a reaction at 52 different temperatures and 52 different solvents, with no hypothesis, just trying everything in sight, completely mechanical. Where's the scholarship in that? That's a complete waste of the student's time. [They do it] just because they do it that way in industry. They hope that, hit or miss, they'll find the best way of making this reaction work. There is no excuse for making a graduate student do that.

You have observed that this sort of activity takes place here at this university?

> All the time. I don't understand it. Sometimes when I sit through students' progress reports, I will embarrass everybody else in the room by saying, 'What is the hypothesis you were testing here?' And they will look at me like, 'boy are you dumb.' [70]

In contrast to a hierarchical mode of work organization, the organization of work in traditionalist laboratories is collegial. The groups tend not to exceed ten members and entail a close relationship between advisors and graduate students. They are characterized by close collaboration and supervision. Although traditionalists recognize that large groups are indicators of success by virtue of research support, they see a size threshold beyond which the quality of work and their ability to coordinate research begins to deteriorate.

> I've typically had one or two grants, I've run a research group that has probably averaged five or six people so I'm in that middle tier of people…I like to interact closely with my students, I spend a lot of time with them. I've had groups go over ten and I've been

> unhappy with my management of it. You can't spend time with each individual. Say your week has leftover after you've done your 'must do' duties, fifteen to twenty hours a week to take care of your students. If you have twenty students, you've got one hour per week, per student. That's for everything, writing the thesis, writing their papers, discussing the research with them, developing their projects, managing them, whatever that takes, this is one hour a week for a human being and I'm devout to them during this time period. I like a smaller group...I think when students are left to their own devices with expectations that they have a hard time meeting and pressure from a major professor who is demanding but not supportive, I think this creates the environment where scientific fraud begins to flourish. [27]

Traditionalists' preference for fundamental research is a key influence on the size of their group not only because of the scale of the work being done or their preference for a more collegially organized research group, it is also influenced by changes in the funding structure of science that have shifted from the individual investigator to group-coordinated and technologically driven funding goals, the funding structure underlying centers and institutes funded by the government and industry.

> I've had reasonably good funding from the very beginning but what happens is that grants don't increase at the rate of academic inflation and so if a graduate student costs a grant $20,000 a year in 1980 and $66,000 a year now, my grants didn't follow. 1980 dollars [are] woefully inadequate now, but I've been equally successful all along. I had a research group with nearly twenty graduate students, I was very intensely unhappy with that and

> now my group is six students and one postdoc and I see it decreasing fairly rapidly, through no effort of mine. I'm doing the most exciting research of my career right now and there is little chance that I can sustain a group of six plus one for more than another year or so. [This university] has created a harsh environment for people who do fundamental research because those people cannot sustain a career doing exclusively fundamental research. They have to get involved in practical stuff. They have to join these group grants that are directed at fundable stuff.

What kind of grant, for example?

> You have the Air Force Office of Scientific Research or NSF and they are looking to increase their budget, just to keep up with inflation and so they have to sell somebody in Congress that they're doing something neat, they say we are going to create a new program focused on a particular problem and in order to make this happen, we won't work through the individual investigator direction, we will create large interdisciplinary, possibly inter-university programs where we try to get five to ten scientists working together with an obscene amount of money on a problem which we decide is important and we sell it to Congress. Then you have a five year-ish program where these people have more money than they've ever seen before to do something which they may or may not be interested in and qualified to do but they do it and then they get used to that level of funding and they become parasites. [46]

Distinctions in the organization of work in traditionalist and commercialist research groups are thus shaped by the scientific goal (knowledge creation or technology production), conventions of acceptable supervision and guidance of graduate students, and exigencies of funding. Traditionalists' preference for small or moderate sized research groups are presented in light of perceived inadequate supervision, insufficient contact, and the strain of large groups they observe in commercialist groups. In contrast to the commercialists, whose work requires buffering doctoral students from intellectual property or commercial operations, the collegial organization of traditionalist research groups is predicated on the fact that the goals of discovery and training overlap consistently. Commercialists, on the other hand, employ large hierarchically-organized groups around the goal of solving problems they view as "bigger" than traditional scientific work.

What are the implications of divergent patterns of work organization between commercialists and traditionalists? The patterns of organization suggest qualitatively different modes of scientific work. Scholars of group processes have long considered group size critical to group behavior. Notably, research suggests that large teams are better than individuals at solving problems, but less creative (Manners 1975). Louis et. al (2007) draw upon this pattern to argue that large research groups are best at "normal" science and less likely to generate novel ideas, whereas small research groups are more likely to contribute breakthroughs. This claim is consonant with the views of study participants. Traditionalists frame the objectives of their work as long-term, large, and transformational. Commercialists, traditionalists argue, are focused on short-term non-scientific problems. The commercialists in this study have indeed made important scientific breakthroughs, but it is unquestionable that they are committed to generating solutions to societal problems. What is less clear, however, is the approach to science that doctoral students who train in large commercialist research groups will develop. A comment by a commercialist highlights this concern, and supports the claim that research groups organized around commercial objectives inculcate a problem-solving, rather than breakthrough-oriented, mode of role performance.

> I think we're going to get a very different product of graduate student that comes out of research universities. While they may be more entrepreneurial, they may not be as deep scientifically. What kind of ramification is that going to have? It's hard to say. I think that they may have a nose for picking the kinds of problems that have great commercial impact. While nobody could deny that that would necessarily be a bad thing as far as society is concerned, it could end up like combinatorial chemistry where we're just canvassing these sort of large volumes of data, but we're really not progressing very far. We're churning through a lot of data, but we're not drilling down deeply enough on any one point to really make a truly substantial discovery. [74]

It is noteworthy that his account, particularly in its emphasis on "churning" and "canvassing", parallels that of his traditionalist peer, quoted above, who described the scientific approach of her commercialist colleagues as "completely mechanical." The similarity of these views implies that, independent of whether one embraces or rejects commercialization, there is agreement that commercialization can be to the detriment of advancing knowledge.

Group size may also have implications for the conditions of scientific work that influence both the principal investigator and the individuals who comprise the remainder of the research team. Three sets of evidence are of relevance to the conditions of work. First, Manners (1975) showed that consensus decreases as group size increases. Other research indicates that shirking, free riding, and fraud are less visible in large groups than in small groups (Isaac & Walker 1988).

Finally, Louis et. al (2007) found that larger academic laboratories are more likely than small laboratories to exhibit secretive behavior. These findings suggest a higher potential for conflict or perceptions of inequity in large groups. In the context of scientific labs, such issues may arise in relation to different lines of scientific inquiry, given that lines of research move at different speeds and produce results of varying levels of interest (Louis et. al 2007). Commercialist laboratories may therefore be likely to experience and have to manage internal divisions, whereas it is likely that such problems are less pervasive in traditionalist labs. How scientists organize work within their research group has been shown to depend on their goals. Consequently, we must turn to the bases of status in commercialist and traditionalist science.

Bases of Status

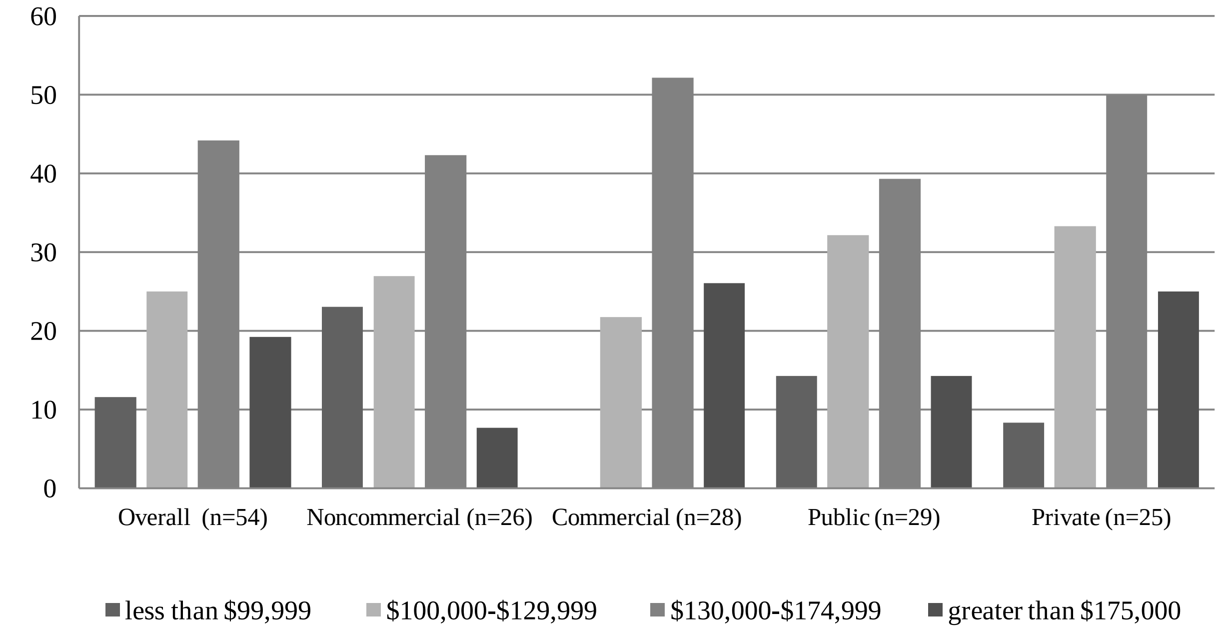
Science, like law and medicine, is a high status profession. Entrants to such professions are presumably motivated by the desire for elevated professional standing and therefore the selection of particular specialties and task domains are likely influenced by the desire to maximize one's intraprofessional prestige (Heinz and Laumann 1982). In science, particularly within the context of the academic profession, the highest status accorded to a scientist has historically been connected to fundamental discoveries. Between 1901 and 1972, for example, only fourteen percent of Nobel Laureates received the prize as a result of invention (Zuckerman 1977). It is therefore no surprise that sociologists of science have long argued that original contributions to knowledge are the chief basis of status in science (Cole and Cole 1973; Merton 1973), just as sociologists of professions make similar arguments about the relationship between intraprofessional status and the content of one's work (Abbott 1981).

These arguments work well in historical contexts of science in which economic and power considerations are less influential than professional norms and values. Buffered from the standards of criteria and worth external to the profession such as economic value or practicality, original contributions to fundamental research provided the only means by which a scientist could attain elevated status within the community. Inspection of differences in commercialist and traditionalist science, however, reveal that the bases of status in science are no longer as uniformly organized according to the level of professional purity of one's work. To fully evaluate this development, let us consider two objective properties of professional status, income and client type, before turning to the scientists' subjective constructions of status. To be sure, income and client status can reflect both intra- and extraprofessional status. Intraprofessional status refers to a status assigned within a profession by professionals themselves, whereas extraprofessional status refers to status assigned by the public (Abbott 1981). Both forms of status merit consideration specifically because commercialization entails extraprofessional commitments, interests, and social networks.

Income matters because it is tied to occupational position, a basic form of scientific recognition, albeit one traditionally viewed as less important to scientists than awards or visibility (Cole and Cole 1973). The average salaries of the scientists, which I present in figure 1, thus permit us to draw tentative, but suggestive, conclusions about the differential bases of status between commercialists and traditionalists and influence of commercialization on organizational reward systems.

On average, about half of the scientists in the study earn between $130,000 and $175,000 through their nine-month salary. One-third of the scientists in the study earn below this amount. Comparing the distribution of salaries between commercialists and traditionalists, one observes a

Figure 1: Average salaries of scientists[a]



a: Seven scientists did not provide salary data.

strong tendency for commercialists to earn more. Approximately eighty percent of the commercialists in the study earn over $130,000, whereas only fifty percent of traditionalists fall into this category. Twenty-six percent of the commercialists have earnings that exceed $175,000 per year, whereas about eight percent of the traditionalists fall within this range. It is also the case the scientists at private universities earn more than their peers at public universities, although this does not explain the difference between commercial and non-commercial since the scientists were evenly distributed across each of the comparative dimensions of the study.

Income is commonly associated with extraprofessional status, but the data suggest that salary differences among scientists provide a basis for intraprofessional distinction among scientists in the same field. Universities have other missions apart from science, but they are quintessentially scientific organizations. Incomes thus express what is valued by the administrative strata within the scientific community (Geiger 2004). Furthermore, although status relations within a profession are rarely established with the knowledge of the income of one's peers, commercialists nevertheless establish reputations based, in part, on the generation of personal, departmental, and organizational wealth. Two illustrations from the interview data demonstrate this point. To illustrate this point most clearly, the first example draws on interviews with commercialist and traditionalist chemists in one department at a private university. The scientists in this department referenced several commercialists in interviews, but one example occurring with greater frequency than the others made it clear that income now provides a basis for intraprofessional status.

> We had somebody here, [Baxter], my colleague in fact, here in chemistry, who discovered [a medical application of an element] and the sales for that were $400 or $500 million a year, which is not a blockbuster, but it's something that brings in money to him, to the department, and to [the university]...it was a million or a million and a half a year for the department, for the [university] and for the inventors. [45]

> The main support that the chemistry department had outside of what [the university] gave it was from a patent on [a compound] and shortly after that it was from a patent on the use of [an element] for [medical work]. That's [Baxter's]. The chemistry department benefitted to a tune of several million dollars a year from the income from a patent. [46]

> There is some disagreement on how appropriate [commercialization] is, but this department in particular has had a patent that probably brought in $20 million to the

> department from [Baxter] and so we would not be in the shape we're in now if we hadn't had that. [48]

Second, the interview data indicated instances in which perceived intraprofessional status was elevated solely because of income generation. One account that suggests this pattern is from a commercialist, now at another private university, who completed his postdoctoral studies at the department referenced above.

> Let's face it, [Baxter] was always sort of an outcast in the department there. I mean there were a few of the inorganic chemists who…were close to him and they really loved the guy, but it was only when he started making money for the department that all of a sudden it was...'[Baxter], [Baxter] you're wonderful.' [74]

A second example from a traditionalist at another university parallels this account.

> The most prominent guy currently on the campus is [Walters]. He's the one who gave [this university] the monopoly on [an organism]…He's perfectly happy to let [the university] take these damn [organisms] and do what they want with them, but they won't leave him alone, he's worth too much. He started out in complete obscurity, he wasn't even a professor, he was in some adjunct status and when he discovered these [organisms], the university instantly promoted him and fixed him up. This new building over here [*pointing out the window*] is his building. [32]

Income thus comprises a basis of esteem, prestige, and respect (Geiger 2004) for commercialists – even in instances in which that may not be their motivation. For some, it becomes the chief basis of reputation, elevating one from obscurity to prominence within a community. With the exception of blockbuster technologies, income is also primarily a locally-delimited dimension of social status, decreasing beyond departments, universities, and commercialists' professional networks.

Client status comprises a key dimension of professional status along which commercialist and traditionalist worlds diverge. Researchers have previously suggested that the meaning of clients in academic science is ambiguous. Goode argued that "client" is not in the lexicon of academics as the cause of learning, not individuals, is served (1957). Similarly, Braxton (1999, 142) states that "the knowledge base of an academic discipline is the client of scholarly role performance", because the goal of role performance is the creation of knowledge. These views represent an insular conception of the academic profession, and consequently do not fit well with the external, non-exclusively scientific commitments of commercialists. Knowledge and the cause of learning are not clearly served by commercialization. Indeed, board representation, officer positions, and consulting work performed by commercialists are indicative of the conceptions of clients most sociologists of professions employ in considering clients (cf. Heinz and Laumann 1982). Because status connotes a system of relations between people, the professional networks of scientists are thus important to consider as they reflect differences in the groups through which scientists seek to exercise their power, alternative bases from which esteem or respect may be sought, and thus different bases through which professional reputation may be constructed.

Information collected on the post-interview questionnaires makes unambiguously clear that corporations are regular clients of commercialist scientists. Consider table 9, which presents the range and average corporate positions of commercialist and traditionalist scientists held during the past year. In terms of ongoing positions, such as officer or board positions on directorates or scientific advisory boards for which there is sustained interaction, the average commercialist has engaged in regular contact with seven corporations (the positions do not overlap within an organization), some of which they played a role in founding. Over the course of their career, approximately one-third of the commercialists have held chief officer positions, two-thirds served on corporate boards, and nearly all were on scientific advisory boards and consulted. Consulting, although not exclusively a sustained interaction with a corporation, comprises another frequent non-academic commitment among the commercialists. Overall, the average commercialist in this study has engaged with about fourteen companies for varying lengths of time. As commercialists' vitae indicate, the positions range from startup companies to well-known corporations with extensive resources such as Becton Dickinson, British Petroleum, DuPont, Eli Lilly, General Electric, Intel, Merck, Monsanto, and Pfizer. Traditionalists, by contrast, engage in extremely limited forms of industrial relations. Only one has had a position

Table 9: Corporate Positions

	Commercialist		Traditionalist	
	Range	Average	Range	Average
Officer	0 - 3	0.62	-	-
Director	0 - 9	2.29	-	-
Scientific Advisory Board	0 - 17	4.42	0 - 2	0.08
Total ongoing positions	1 - 23	7.06	0 - 2	0.08
Consultant	0 - 22	8.07	0 - 4	0.48
Total positions	2 - 45	14.8	0 - 4	0.48

on a scientific advisory board. Six, or about one-fifth of the traditionalists, have consulted. Of these, four scientists consulted for one corporation, whereas the remaining two had consulted for three and four corporations. In terms of the association between client status and intraprofessional status, the key difference between commercialists and traditionalists is that commercialists possess broader and potentially more powerful external networks through which influence can be exercised and reputations constructed.

Measures of income and client type indicate that commercialists enjoy a status among nonprofessional audiences that traditionalists do not. It is noteworthy, for example, that the traditionalists rarely consult. As I will show in chapter 4, the lack of such connections is a reflection of commitment to fundamental research rather than a lack of opportunity. The higher salaries and connections to powerful corporations suggest that external definitions of valuable or esteemed scientists or scientific research differ from what scientists have traditionally constructed as the key basis of esteem. Although this is understandable in the context of corporations, to the extent that higher salaries are indicative of what universities assign as esteemed or important, the findings suggest a shift in the basis of scientific status at elite institutions. The key question we must now consider is what commercialists and traditionalists construct as esteemed.

Commercialists

The hallmark of esteem among commercialists is societal impact. They believe that the most important thing a scientist can do is to extend the reach of one's work beyond the scientific community. With minimal variation in emphasis, their view of science coheres around notions of impact, utility, application, and extrinsic value.

The most esteemed pursuit is to provide new knowledge that is as impactful as possible….I have a personal taste for applications and I really like projects. I like doing science that goes from very basic levels, all the way through to applications, and the reason I like applications is multifold, but they're fun. I think it's useful to help humanity, and we can do that as scientists by applying the things we learned for practical benefit. [28]

To translate our technology and science into society. To make our understanding and control of materials a benefit to people…To promote the development of the technology in the public or private sector outside the university, and to expand jobs and benefits of that technology in terms of society and people, citizens of the country and the world in that regard. [41]

Those of us who are interested in applied work are usually interested in trying to enhance the impact of that work and commercialization is one form of impact. Some things that I have done in my career, I do because of scientific curiosity, not because it is commercializable. Other things I do in my research I do because I think they address problems that somebody cares about, somebody being the marketplace. Most of my own research has focused on manufacturing technologies. I see manufacturing on the critical path between science and the patient or science and the customer. I've attempted to focus on work associated with that manufacturing path in order to exploit science, [to] develop the technology that's necessary to deliver it and I get a lot of satisfaction from seeing it have impact in the marketplace. [53]

Commercialization thus connotes "delivering" science to society, a view of scientific work in which extrinsic benefits of knowledge are extracted to solve societal problems and reciprocate societal support for science. The presentation of this view regularly contains an overture to the importance of basic research because it provides the initial basis for commercialization of research and because it is recognized as the unique claim of university scientists. For commercialists, however, the organizational framework of fundamental research is interpreted as a niche within which they are able to pursue problems that are neither purely academic nor purely commercial.

> A university is the best environment for engaging in pursuits that are abstract, which perhaps do not have an immediate application. But at the same time, if one can keep an application in mind while pursuing these questions, that's almost the better. [55]

> We are unconstrained by any economic driver to come up with some particular solution to a problem and we're unconstrained [by] timelines of when the problems we choose to investigate might be solved. The absence of that constraint provides an opportunity to search in areas of knowledge that there's so little known that you can have a long range view of it. You can attack those problems and that's different from a scientist in a company who is economically constrained to work on something that's going to affect the company's bottom line and is under a time constraint that whatever problem they have has to be resolved in a certain amount of time that is going to have an impact on the company's lifetime. [66]

This interpretation of the university environment is the essence of the *modus operandi* of the commercialist: academic scientists, by virtue of the absence of market-oriented and temporal constraints and the possession of fundamental knowledge rarely found in commercial organizations, are positioned to provide socially and economically valuable solutions to societal problems. Consequently, societal impact competes with scientific impact as the basis of status. In most cases, particularly as one's career unfolds and adherence to traditional scientific norms recedes (see chapter 4), societal impact is considered the ultimate achievement.

> What I'm most interested in is impact – having an impact on the world. That's the first priority. And then there's a second priority of entertainment, doing things that I find interesting.

What would be an example of a university scientist having an impact?

> Specifically, in graduate school I had been doing [biophysical research]. That work affected a very small number of people that might care about the stuff I was doing, but it turned out the work I did [as a post-doctoral researcher] to automate [a critical scientific technique] ended up changing the course of the world in a significant way, and, in fact, in billions of people. So it's just not a tough call. If you've got so many hours in the day to spend, how do you want to spend them? And if you can do something that has a big impact, then for me that time is better spent than if you have something that has little or no impact. [34]

Among the types of pursuits a university scientist can engage in, what to you is the best or most esteemed?

> Well, in our case the development of new medical instrumentation that potentially can help patients on a worldwide basis is exciting, fun, and productive. Whether it does actually get to benefit patients depends on whether or not you can successfully commercialize it, which means interacting with industry.

Why is that more important, for example, than explaining phenomenon or advancing new knowledge?

> I get a lot of fun out of the applied science end of things and I started as a high energy physicist and [did very basic work]. That was fun and it was good science, but to me making a new non-invasive technique for [medical diagnosis] that's going to help millions of people is a little more exciting. [30]

What it means to have an impact as an academic scientist shifts as one gains new measures of achievement, such as one's most highly cited article, the size of a research grant, honors, or in the case of commercial scientists, societal impact. Each achievement sets a standard for how future accomplishments are evaluated.

Contributions to fundamental research no longer wield the exclusive claim to eminence in science. Moreover, the bases of status in science now include activities and outcomes ancillary to academe's distinctive claim of fundamental research, such as the development of commercial products, solutions to nonscientific problems, and economic development. Consequently, commercial activities compete with or replace original contributions to knowledge as the basis of status in the moral order of commercialist science.

Traditionalists

Existing research has clearly indicated that traditionalists view original contributions to knowledge as the most esteemed scientific pursuit (Cole and Cole 1973; Merton 1973; Zuckerman 1977; Hermanowicz 1998). Status is derived from transforming the way that one's peers think about their field. In his vision of the most esteemed pursuits in science, for example, one chemist said:

> The pursuit of knowledge is the central aim for science in the academy. To try to answer questions that have not yet been answered and then also to be the storekeepers of knowledge in terms of the questions that have been answered to make sure that those things remain known and available and are taught to people. [47]

Traditionalists place a premium on originality and priority in discovery and are primarily oriented to their academic peers. The intended impact of one's work is primarily the scientific community. Therefore, the only means by which one enhances their status within the community is by advancing the community.

Constructions of status among traditionalists reveal how the ways in which traditionalists and commercialists make sense of their social environment fundamentally diverge. Whereas commercialists interpret their social position as a mandate to pursue societal impact, traditionalists' interpretations legitimate fundamental research as their exclusive mandate. Such constructions frequently either espoused the relative superiority of fundamental science, but they also entailed derogation of the types of activities commercialists embrace.

Creating new truth.

Why is that the most important thing?

We have the luxury of being paid to think and the most valuable product is new ways of understanding how things work…I have never or almost never tried doing anything except curiosity driven experiments. [46]

Discovery-based work is personally most exciting to me. It's also what is not duplicated anywhere else. If it's very clearly goal-oriented, likely to result in something either profitable or medically-useful in the short term, then I feel like that kind of work is probably naturally pursued vigorously by the private sector…I have a sense about science that is explicitly short-term goal-oriented – like producing a device, technology, or a drug – is always trying to find the fastest route to something of use, as opposed to see what the most interesting thing is, that's true. [54]

I would have to say it's basic research to the extent that a lot of science and technical research can be done just as easily in industry. I think that the roles should be pretty separate from one another, or ideally, could be separated from one another. So the sorts of things that industry won't do because they're not profitable, but are nevertheless interesting for fundamental reasons, I think, is really what the mission of a research university ought to be. [37]

I think the discovery of new knowledge and the mentoring and education of future scientists.

Why are those most important?

> A university is something that should be open. [Universities are] supported by government mostly…because they play a different role than commercial enterprises do in the health of the country and economy and you know, they have a different role. [39]

A noteworthy feature of these accounts is the extent to which traditionalists' constructions of esteem are framed in opposition to characteristics of commercialist work. Traditionalists stress openness and contributions to the scientific community, whereas commercialist work entails elements of secrecy and commitment to societal rather than scientific impact. Profitability is viewed as a potential threat to science by traditionalists, whereas commercialists view it is a criteria by which scientific work can be judged.

Three preliminary conclusions may thus be drawn based on the ways in which commercialists and traditionalists construct status in science. First, the views suggest a contest among moral orders, particularly to the extent that traditionalists view the embrace of commercial activities as a threat to their view of how science should be done and their ability to pursue fundamental research. Traditionalists thus exhibit a *threatened* social-psychological attitude, whereas the attitude of commercialists is *unthreatened.* In contrast to a perceived threat, commercialists view their position as enabling a preferred, entrepreneurial, course of activity. Second, the views indicate that commercialization is not as widely-embraced by scientists as some researchers have argued. The traditionalists' accounts varyingly reflect tolerance, reluctant acceptance, or rejection of commercialization, but not encouragement. Third, these accounts reveal a competition of reward systems wherein one subset of scientists seek status through contributions to the community while another pursues status through contributions that primarily impact society and bring indirect, nonscientific returns to the scientific community.

Visibility

Distinctive conventions of scientific work are revealed in the ways that commercialists and traditionalists discuss their achievements and the outcomes of their work. Often framed by scientists under the colloquialism of "impact", the scientists' perceptions of who benefits from their work highlight, in a general sociological sense, different ways in which reputation within a social system is constructed. In the context of science, these accounts are conceptually tied to visibility, or the extent to which a scientist and his or her work is known within the scientific community.

Visibility is a critical component of a scientific career, for rewards are predicated on contributions. Scientists cannot be rewarded if the relevance of their discoveries to scientific advance never become known. How scientists communicate their ideas is therefore critical to both the operation of the reward system and individual careers (Cole and Cole 1973). Visibility and its antonym, invisibility, connote a continuum representing the potential spheres of influence attainable over the course of a career. At one end are invisible scientists whose contributions are unpublished, uncited, or constrained to functions ancillary to research such as training and teaching. Such scientists have limited and exclusively local influence. At the opposite extreme one finds the scientific immortal, or scientists whose transformative contributions to knowledge create visibility and influence that lives beyond them through eponymous laws, theorems, or branches of science. More common occupants of this extreme of visibility are scientists who have received the Nobel Prize or membership in the National Academy of Sciences. As the Coles

(1973) found, most other awards are not highly visible to the national community of scientists. Thus, for the neither invisible nor extremely visible scientists, other accomplishments such as citation and appointments at elite universities provide traditional means by which visibility may be enhanced.

The continuum is representative of the stratification system in science. Zuckerman (1988, p. 526) notes, for example, that "a small number of scientists contribute disproportionately to the advancement of science and receive a disproportionately large share of rewards and resources needed for research." The majority of the population of scientists is thus clustered toward invisibility. Visibility, it should be noted, has a temporal aspect. One's position along the continuum varies over the course of a career, as the acquisition and maintenance of a reputation requires ongoing contributions in fields that are typically characterized by hectic patterns of life rather than and Trowler (2001). What is more, visibility may expand or decrease based on advantages experienced early in the career. The principle of the "Matthew Effect" suggests that those who achieve, or who are associated with, eminence early on in their careers will accrue disproportionate visibility over the course of their career because visibility can be converted into resources for research (Merton 1973). Scientists of lesser repute, by contrast, are unable to accrue visibility in this fashion. We may thus conclude that visibility is a dynamic aspect of a scientific career and that although scientists may attain high visibility, sustaining and exceeding that reputation is exceedingly difficult after one's "climb to the top" plateaus. In asking scientists about who benefits from their work, we access their views of where they have had the most influence. In doing so, baseline conventions of the attainable may be identified in each moral order, indicating how each group responds to an environment in which progress and development, not stability, is the normal expectation (Becher 1989). As we will see, commercialists stress societal impact to the exclusion of the scientific community, whereas traditionalists tend to emphasize scientific and local achievements.

Commercialists

Only nine, or about twenty-eight percent of commercial scientists referenced visibility within the scientific community. Among these scientists, only two exclusively noted the scientific community as the group for whom their work has had the largest or broadest outcome. The relatively low number of commercialists who emphasized scientific impact is particularly surprising given that nearly all of the commercial scientists are highly productive and highly-cited researchers at well-recognized U.S. universities. Overall, commercialists' explanations of their influence are predominately framed in terms of societal benefits. Within this general finding, two patterns are apparent. First, scientists who received their PhD before 1980 exclusively emphasize society when asked who benefits from their work. That is, their responses contained no referent to any individual, group, or body in the academic scientific community. The following responses are representative of this pattern.

> Well, some of the techniques that we've invented are available in every hospital in the world and it's probably saved a lot of lives by reducing risk in the amount of radiation, in the amount of contrast material and made the test less invasive by not having to leave catheters sitting in brains for long times, because we can now do it in real time. So all of those things, I think have been successful, and there have been a number of techniques that we've been involved with in x-ray and MR and other techniques, the CT where we have made things a lot less risky. So, it's satisfying.

So who do you think benefits from what you've done as a scientist?

> Patients. [30]

> Has anything shown up that benefits the public? Yes. I developed all of the biochemical engineering things necessary to make the operation of a fomenter, let us say, capable of making a vaccine for 7 billion people in a hurry. Does it sound like a reasonable thing to be able to do if we needed? Well, I've already done that and that's all been published in years past….So basically I have reduced the cost of making viral vaccines to less than the cost of the bottle you put it in, and have developed the technology that on demand, could make 7 million doses of any particular virus that you can grow in less than 90 days. So I think that's on the table, something which not only has the potential of being valuable, it's being used around the world and it's the only known way that I know. And I'm reasonably acquainted with what's possible in the vaccine business to actually save us from a horrific pandemic in the time available. [49]

> You have to realize that, I would say in a 20 year period, I have been involved with probably something between 10 and 15 startup companies and the fact is that many thousands of people have jobs right now, because of my technological ideas and the efforts that arose from those. And that's still going on right now. Okay? Because I'm typically working with five companies at each point in time. [92]

A noteworthy feature of these scientists' accounts of their impact is the magnitude of the results. They are not unique in this respect. Each of these scientists and others who received their PhD before 1980 have commercialized work in ways that have large-scale and even global reach. Considered alone, it is not difficult to understand why scientists who have over three decades of experience in academe look back and cite such accomplishments as evidence of influence and impact on the world, particularly because these scientists underwent professional socialization in an era in which their role models were unable to claim such accolades.

In the context of their scientific influence, however, commercialists' emphasis on their commercial achievements to the exclusion of their scientific impact is difficult to reconcile, given the level of success suggested by their appointment at premier research universities. One explanation of this pattern originates in the view that the majority of scientific contributions, even among the most successful scientists, are viewed as "bricks in the edifice" or incremental. According to this interpretation, achievement in an intensely competitive environment is viewed as routine despite the fact that such accomplishments set them apart from the majority of academic scientists in the world. This is seen in the way that scientists discuss such accomplishments in relatively mundane terms.

> I've certainly done in my own career, the usual kinds of things, published hundreds of papers and gotten the usual kinds of research awards and gotten lots of teaching awards and things like that. [67]

> You check off the boxes of the things you've accomplished. Like so many papers, so many federal grants, talks, blah, blah, blah. Then one of the boxes is, 'started a company,' and that adds cache to your resume rather than a bad thing. [34]

> Publishing in top journals is great, but it's pretty common. It doesn't feel like a really unique thing. [37]

Having achieved heightened visibility through publication, citation, and the achievement of tenure within elite scientific departments, commercialists redefine originality such that the scientific pursuits through which visibility is initially acquired are attributed as normal and incremental, while societal impact is framed as unique and providing a new form of visibility. Commercialization provides a new outlet for visibility and rewards once one has "arrived" and solidified one's place in the elite strata of science, particularly because the difficulty of acquiring more visibility only increases upon the establishment of one's position in this environment.

The prestige commercialists assign to external visibility is so potent that it forms the basis of aspiration among commercialists who are in earlier stages of their career. The commercialists who received their PhD after 1980 similarly emphasize societal impact, but in contrast to their more senior commercial peers, their perception of visibility is framed in a future tense.

> I think ultimately I hope patients benefit. You know, I hope people with diseases that could be ultimately treated with drugs that are based on the ideas that we, you know, learn about how cells work and how disease is caused or the very molecules that we make get used as drugs. Either way, I hope that's, I hope that's the benefit. [95]

> There are multiple constituencies that benefit from my work. In terms of the research itself, we're working in two primary areas and one of them is in biomaterials area in which case we're actually developing material systems which we hope will ultimately

> make some difference in providing a pharmaceutical advantage for the delivery of drugs or in providing material systems that in some way provide medical development that can help others. On the other hand, we're also working on energy problems…These are the kinds of things that we're looking at and in that case, I think, there's benefit to the greater world in terms of addressing energy issues and energy problems. [44]

> I think a lot of people…are benefiting from these new reactions they can use to prepare those protein-based pharmaceuticals and so forth, so a lot of those types of companies we hope longer term that a lot of our materials for environmental applications will catch on industrially, but using proteins to make materials is a very new thing. It's the kind of thing that everybody wants to do, but people don't really know how yet, so that's what we hope to do. [86]

In contrast to the concrete and definitive accounts of commercialists who completed their PhD before 1980, these commercialists similarly stress societal benefits but in ways that evoke notions of an envisioned future. As such, they are frequently characterized by multiple possibilities rather than concrete results. This pattern is particularly striking given the relative accomplishments for which the scientists are in position to choose in explaining their influence. Each of these commercialists has achieved tenure in an elite department, indicating that in the eyes of their peers their contributions to their field have at a minimum been influential, if not potentially transformative. The desire for status as an eminent commercialist leads scientists to make sense of their present status in light of a future vision, as the perceived rewards obscure or outweigh perceptions of past accomplishments.

Traditionalists

Patterns of influence among traditionalists fall into two patterns. First, the predominant mode of visibility among traditionalists is generated exclusively within the scientific community, even the scientists are able to acknowledge that their ideas have influenced commercially-oriented research or products. These scientists recognize their influence primarily through awards received for research and citation, but also the careers of their graduate students.

To what extent do you feel you have been successful as a scientist?

> I've been in this business for 36 years. I've produced a lot of graduate students. I've been recognized by my peers with a lot of prizes. I have reshaped the way people think about a lot of classic problems and I think that I am one of the most important people in the world in [my area of research]. People use my terminology, they use my books, I've enjoyed doing it, and I'm quite proud of the work I've done. So, I'm not shy about this.

When you look over the 36 years you have worked as a professor, who do you think benefits from what you do?

> I don't care. But, I know that…a lot of the [scientific] techniques that I've developed are used in practical areas. A lot of the models I've used for representing spectra are used. A lot of the pattern recognition techniques I've developed can be used to separate out different kind of phenomena. I think that a lot of people that I don't even know about use what I do and I'm not that concerned about the applications.

Why don't you care who benefits from your work, as you say?

> Because I know it’s good. I know it’s fundamental. And I publicize it, but I don't go looking for anybody other than grant agencies to pay for it. [46]

Visibility, for traditionalists, is solely predicated on the extent to which one is able to observe influence on the scientific community. Citation of one's work and honors conferred by the community provide the most important metrics of success for a scientist, as they reflect the influence of one's research contributions. The scientist's account further reveals how scientists actively seek to enhance their reputation through self-promotion and in doing so a clear distinction is drawn between moral orders of commercialist and traditionalist science. Traditionalists seek to increase their visibility only within the context of the scientific community and reject alternative bases from which reputation could potentially be constructed.

Visibility derived from commercial influence thus plays no motivating role in traditionalist careers. This is particularly evident in the case of a molecular biologist who, more than any other traditionalist in the study, claimed a substantial societal impact resulting from his work. Notice that despite the magnitude of the product, influence beyond the scientific community is a secondary metric of satisfaction and not the basis of his sense of influence.

> I think society benefits tremendously. In my case, my research has been purely motivated by an interest in basic knowledge and yet very early on, with the discoveries that I made, it had direct applications to biotech…One third of the world's supply of recombinant human insulin is now made...following the basic pathway…that we worked on in my lab. I never would have guessed that.

Would you say that's the biggest impact you've had as a scientist?

> No. Commercially, yes. But intellectually, no. Intellectually to me the most satisfying thing is the basic knowledge that we've gained, that it had a practical application is gravy, but it certainly isn't the core of what I find most appealing. [87]

The aspirations of traditionalists in early career stages reflect a similar orientation. One traditionalist in the second year of her appointment as an assistant professor noted the following:

> A lot of what I do, I think of as an art form. It's useful to society in the way that art is useful to society…

Do you hope that your work will translate into something that improves the well being of people?

> I think I'm more motivated by the possibility of changing the way people think. That probably means changing the way other scientists think because most people at large don't have an opinion about the hypotheses that I'm testing one way or another. I would like that not to be quite as true. In terms of societal goals, I'd like scientific literacy and appreciation for the fun of scientific discovery to be more broadly accessible, but it's not currently and it's unlikely to be broadly accessible in my professional lifetime, I think. So that's really not what's driving me. I would like to change the way other scientists think. [54]

A second pattern of visibility among traditionalists is an emphasis on graduate training as a sphere of influence. This pattern holds independent of objective measures of visibility because, taking for granted that consistent research funding is a prerequisite fact of life at the departments in this study, the training of future scientists is constant regardless of the peaks and valleys of one's visibility within the community. For those whose research productivity declines over time, aspirations for influence and visibility are scaled down, as emphasis is placed on training scientists or teaching, rather than research. Consider the view, for example, of a chemical engineer who has published 40 articles (one hundred less than the average participant in the study).

> I think I've become known more and more as a dedicated teacher and my colleagues are looking at me less and less as an innovative researcher, because I'm not always putting in every spare moment into trying to figure out some new angle on something.

Does that bother you?

> Quite a bit, but that's the way I am. I'm sort of reconciled to it. It has limited my advancement, but in academia you don't get many promotions, so a lot of it is sort of outside visibility or personal satisfaction, and I think I've probably relied more on the personal satisfaction of doing what I think needs to be done, rather than pursuing external recognition.

So who do you think ultimately benefits from your work?

> The students. Realistically, in a given year, a couple grad students have a couple research advances and innovations. So long term, if a student or a faculty member has two or three really significant ideas in their career, they have an impact, and I sort of realized that...I was worried I was going to burn out, just, you know, the next six months I [have] to do something. I can't live on the edge like that, so I've sort of calmed down a bit. And when I talk to students...I had some students back visiting from a company they were recruiting for, and one of them mentioned some example I had used in class five years ago. I thought wow, that...You know, she remembered that, that's cool, and that doesn't show up on my paycheck, but that's part of why I went into the faculty position. [24]

For the few scientists who fall into this pattern, appointment and the conferral of tenure in a top department enhance visibility, but the inability to sustain or progressively heighten one's reputation within the field leads scientists to alter their aspirations for influence to a decidedly more *local* orientation. Almost invariably, the scientific visibility of the traditionalists in the study overshadows their local influence, but these scientists still stress the broader implications of local influence. Why? Because visibility varies over the course of a career. Transformational discovery may be the hallmark of traditionalist visibility, but local influence is the constant of a career. For example, a traditionalist whose visibility in science is indicated by his membership in the National Academy of Sciences listed his peers and graduate students when asked about his impact and then stated that:

> There are only a few of us who are going to do something in Kuhn's language that shifts the paradigm. Most of us are going to put little bricks in the edifice and we do a piece of research and it's useful and it advances science and it's useful science, but there's this huge multiplier effect from our graduate students. I have graduate students who are teaching at research universities, at liberal arts colleges, government labs, and in industry and each one of those is teaching a different cohort of people and in some cases doing things that are profoundly important. So if you were trying to step back and ask from society's point of view, 'Why do you want to have research universities?' It's to discover new knowledge, but it's to transmit that knowledge into society at large and in particular get the next generation up to speed so that they can go engage in that enterprise. [25]

For traditionalists, therefore, individual visibility is variable over the course of a career but influence is always viewed as collective. It is in this respect that traditionalists construct societal influence. Societal impact is not a motivating factor for traditionalists; it is the product of collective contributions. Notice, for example, the means by which the following biologist frames her influence. She, too, is a member of the National Academy of Sciences and a pioneer female scientist, having received her appointment as an assistant professor in 1972.

> I think, I hope, humanity benefits. I mean, I think I helped lead. We all know more because of what scientists do.

If you see society as the entity that benefits from your work, in what ways are you able to observe that impact?

> Well, you observe that impact when your students go out and do things. You observe that impact when people quote your work and do something beyond your work. [50]

Whereas commercialists frame societal impact in the context of their individual inventions or companies, traditionalists frame societal influence collectively and indirectly, through reference to the profession at large and their students. Although some traditionalists are able to cite specific examples of the ways in which aspects of their work have been incorporated into practical or industrial applications, most believe that the grand problems society faces will only be solved through the collective influence of the scientific community.

> …In chemistry, we say we do things that support many areas of human life that are important. We are essential to the practice of medicine, we are essential to the

> manufacturing industries of many types who are essential to the electronics industry so our work ultimately underpins a very important function to the economy in the medical establishment and other areas, so look around. Chemicals, right? Some chemist has devoted their time to developing the yellow dye that's in here [*pointing to a legal pad*] or the fiber that's in your jacket or your pen, you know? It’s all chemistry stuff. We're proud of that, we do that. That's the big picture, but when I do my day to day activities, I'm not designing a pen, I'm not designing your fibers in your jacket, right? Nor am I designing a medicine, I am studying fundamental properties of molecules that hopefully will ultimately lead to advancement in some of these areas or areas that we've never heard of before. My work is not focused on the societal impacts of our research, it is focused on understanding the science, with the faith that this will ultimately translate into bigger consequences that may be read by others, other chemists or engineers or doctors or researchers in other areas that need a substance that has certain properties that so happens that we know how to make...[Solving societal problems] happens through the accumulated wisdom of many different kinds of scientists. What gets you up in the morning is I think not these things that might result from our work, its cracking the little nuts that are on our table at the moment, that's the way that I look at it. [27]

Dirty Work Designations

The pursuits that scientists shun or designate as trivial indicate conventions of moral orders that designate negative role sets, or tasks that depart from desired modes of conduct. The reward system is reflected in such conventions in that the tasks scientists designate in this manner bear qualities considered intrinsically unrewarding and/or extrinsically unworthy of professional honor. Traditionalist and commercialist science converge upon three designations of activity: administrative responsibilities, “job shop” or directed scientific work, and incremental science. No distinctive differences were found in the level of emphasis either group gave to any particular activity, nor were any differences found in scientists’ rationalizations of any attributed triviality of the three categories.

The first category is comprised of activities ancillary to research that scientists deem as either boring or “necessary evils.” University committee work and the administrative aspects of running a laboratory, for example, were referenced by scientists as “pure baloney” or the “crap quotient you just have to do.” These tasks constitute the “dirty work” aspects of science (Hughes 1951; Ashforth and Kreiner 1999), or practices that are legitimate aspects of the academic role that are regarded as tedious or unrewarding, but nevertheless required.

> Some of the bureaucratic paperwork that you have to do, either for having grants or local [university] paperwork, some of that, I do it. I don’t shun it, but it’s not really that valuable of time. [93]

> I don’t think committee work is not valuable, but it takes a tremendous amount of time so it’s a drain on time…One quarter of [science] is painful, bureaucratic nonsense. [84]

The performance of such tasks carries no threat of sanction from one’s peers because it is recognized within the local environment as a necessary, albeit unrewarded, component of one’s academic duty. These qualities of dirty work explain why it its designation as trivial is common among both traditionalists and commercialists.

In contrast to trivial but otherwise normal aspects of academic work are tasks that scientists believe should be shunned. These are activities that may be permissible but are regarded as inconsistent with or not in the best interests of the advance of knowledge, training, or teaching. The activity representative of this category is referred to by scientists as "job shop" science, which denotes research in which an organization such as a firm, corporation, or research agency approaches a university scientist to work on a defined problem or perform a specified task. It is not the nature of the external organization that matters but the way in which the work is organized. Thus for the commercialist job shop work may be rejected for lack of interest or because it does not advance one's group:

> I won't do anything which is just work for hire. Somebody calls me up and says they want me to examine a patent and testify, I say no. Because I'm not interested in it. If I'm not intrinsically interested in it, I don't do it. [92]

And for the traditionalist, job shop work may be rejected for somewhat similar reasons:

> I would never work on somebody else's problem. If there is a request for proposals in a very carefully defined area…if they're too directed I don't want to do it because someone else thought they could decide what was important. I'm not willing to trust someone else's judgment... I have the luxury of working with the best graduate students. I'm not going to subject them to some narrowing experience. [46]

The activities that scientists shun are those that they view as a threat to their autonomy, authority, or that potentially degrade the training of graduate students. In contrast to the localized nature of attributions of dirty work, shunned tasks are more frequently subject to individual discretion. Scientists may not gain the universal respect of their peers for consulting or working on targeted research, but neither would they be subjected to informal or formal sanctioning on the basis of their peers personal preferences.

The most prominent dirty work designation among commercialists and traditionalists is incremental science, or the publication of research contributions that have little impact on knowledge. Scientists regard this type of practice as normal, acceptable, and not even wrong, in general, but nevertheless beneath the caliber of science expected of them. Notice in the following responses the ways in which scientists emphasize the legitimacy of incremental scientific contributions.

> There are projects [where] you'll read a paper in a journal that takes result 'A' from one lab and result 'B' from another lab and combines them together to make a new result that actually is new and is a contribution, but isn't surprising, you know? It's the incremental work that might be important to do, but just isn't that interesting because you're not surprised that it works… It's much more important to have one really great paper that presents a fundamentally new idea, rather than twenty papers that are small variations on the same theme. [37]

> I think the power of modern molecular biology techniques is such that it's really trivially easy to turn the crank and publish something that is simply not illuminating. It's not that

> it's wrong. It's not the data are even of poor quality. [But] it's like writing the alphabet over and over again. The problem is that it's good for students to publish papers and it's good for labs to publish papers. [54]

Scientists' views of incremental publication thus represent an expression of the ideal conduct of a scientist as it exists in the institutions studied. The scientists are encouraged to conduct high-risk, high-impact science, but they must nevertheless publish and do so in the best journals possible. Otherwise, in terms of the outcomes of their research, there is little that sets their visibility apart from their peers with whom they compete.

Commercialists and traditionalists similarly construct a negative role set comprised of administrative responsibilities, "job shop" research, and incremental publication. What ties these activities together, apart from the fact that none have any impact on visibility, is that almost inevitably, and despite attempts to avoid doing so, scientists end up engaging in these pursuits. Dirty work designations diverge, however, around two patterns of conduct: book writing and commercialization. As we will see, themes that bind these designations together are commitment and money.

Commercialists

The key dirty work designation that differentiates commercialists from traditionalists is writing books, including monographs and textbooks. From the perspective of the commercialist, this activity does little to advance knowledge, is influenced by a concern for profits, and is a poor investment of one's time. Each of these themes is captured in the following response of a commercialist who was asked about activities he shuns:

Writing books would be one activity I shun. There are some people that feel that writing books is a useful pastime. I feel that that is something that is not worth the time invested in terms of the impact.

Why is that?

Most books now are done by publishers who are more out to make a quick profit. So a lot of them are 'short time' books, like hot topics, but you know, a lot of time invested and yet, you know, they're basically out of date in a short period of time.

Are you referring to textbooks or monographs?

More to monographs. Textbooks, there could be some significant value to that, but for me, neither would not be an effective use of time. [26]

Commercialists vary in which form of book they reject, but the critiques that the scientists make of book writing as an activity are similar throughout the accounts. A notable aspect of this critique is the extent to which it overlaps with some scientist's concerns about commercialization. Notice in the following account how the scientist responds when asked about this overlap.

Are there pursuits that you shun?

Textbook writing. I see a lot of wasted effort on producing yet another low level textbook that might or might not outsell the others. I do not think of that as a scholarly pursuit. I view that as largely a selfish endeavor.

Could one make a similar critique of the commercialization of research?

> Well, I'm not going to argue with that, actually. I'm going to say that when I participate in [my company], I don't view that at all as being part of my job as a professor. I view that as something that I'm doing with my free time, if you will, off salary, you know, different, even though the university has a stake in the success, because they own the patents… [But] let me finish with the books, because the books, to me, are overvalued as academic exercises. I think that writing yet another textbook contributes virtually nothing to academics and the progress of humanity, and therefore cannot be viewed as a fitting enterprise for an academic scientist. [28]

That commercialization and book publication share similar qualities is not lost on commercialists. In chapter five, when we examine how commercialists legitimate their role, we will see that the time investment and potential for profits associated with book publication leads many scientists to compare their commercial activities with writing books. Commercialists construct book publication as a dirty work task because they perceive the activity as offering minimal returns to science at the cost of commitment. Traditionalists, we shall see, construct commercialization as dirty work in a very similar light.

Traditionalists

Traditionalists tacitly accept commercialization by virtue of its overwhelming presence within their departments and their institutions. Commercialization occupies an unmistakable presence on the campuses of the institutions in this study and departmental funding and building construction serve as material reminders of the presence of commercial culture. As one traditionalist noted:

> When you walked into this building, you walked by a framed patent on the wall. It's actually [‘Spalding’s’] patent and that is there to tell you, you just walked into the ‘House of R&D’, this building. There is the constant reminder that the university profits in real ways from this enterprise, but there is something slightly apologetic about that. Everybody realizes that this is a very risky business and that you sometimes have to hold your nose and plow through it. [32]

Acceptance of the presence of commercial culture, however, does not equate to participation. In this respect, commercialization is a property of one's environment in the same respect as other forms of dirty work. Traditionalists shun commercialization because they perceive it as a departure from ideal scientific conduct and potentially detrimental to the scientific role. Consider, for example, a chemist who received his PhD before 1980, whose view reflects the ambivalence toward commercialization characteristic of traditionalists:

> The ability for academics to get patent protection for discoveries made with NIH, NSF, DOD money was a pact with the devil. In another way, I think the conservative republicans who pushed for this were right in that… unless a company has exclusive rights to intellectual property, it will not invest the money to do what needs to be done.

In what way is this a ‘pact with the devil’, as you say?

> I think it will always be the case, as with textbook writing and other things, that it can be overdone. And it is overdone by some people. There's downward pressure on teaching loads because these businesses are taking up peoples' time and so they say: ‘How can I do my job properly if I have to teach all these stupid undergraduates?’ You get this kind

> of distancing from what is the central core function of the university as a result of these involvements. Where do you draw the line? I mean there's a lot of money to be made here by a few people, but most of these things never pan out. [27]

The general orientation traditionalists maintain toward commercialization is characterized by three perspectives: recognition of opportunity costs external to science derived from exclusively fundamental academic science; recognition of risks posed to academic science by virtue of engagement in commercialization; and recognition of the rarity of instances in which the human and material resources invested in commercialization bear fruit that merit commitment to commercial objectives.

Traditionalists therefore shun commercialization for two key reasons. The first is derived from their conception of ideal role performance. They perceive commercialization as an excessive distraction from the key objective to which scientists, in their view, should be committed: advancing knowledge. The following scientist succinctly expresses the view characteristic of traditionalists.

> It's hard to balance one's commitment to pure science and to commercialization would be my judgment and I think that if one is going to try to commercialize something and spend a long time there, that is going to have an impact on your ability to pursue pure science. I think that academic scientists should be pursuing pure science. Commercialization should be if something happens to fall in your lap, you know, perhaps you should consider commercializing it just for the good of the scientific community in

> that it provides society a payback if you will, but it is a distraction from thinking about pure science in my view. [69]

Commercialization among traditionalists is a practice unworthy of commitment unto itself. From their perspective, it is an activity that commercialists should pursue if the opportunity presents itself and the outcomes are beneficial to science, but as its own objective commercialization it is not a legitimate motivation for scientific research.

A second reason traditionalists shun commercialization is derived from conflicts of commitment and conflicts of interest and the threat that they perceive these outcomes pose to universities and the integrity of the profession. Among some, commercialization is perceived as legitimate on the assumption that such conflicts are managed.

> We're committed to ideas being created and put out there for other people to build on. To the extent that one has a financial investment in that, is there then a hesitancy to bring it to fruition as fast or publish? And to what extent does that put your students in a grey zone, in terms of are they working for your company or are they working for you as a professor?...There's a broader issue in my mind that this is part of, too, whether it be commercializing products, serving on committees or being on advisory boards, you know, how often are you here and if your outside commitments whatever they are take you away from here an unduly amount, then who's minding the farm? [64]

> There ought to be a limit. At some point you want to have people that don't have a commercial conflict of interest in a given statement. If somebody from the outside world

> comes to the academic and says: 'Are genetically modified foods dangerous?' You don't want to only have people that answer that have a commercial interest in genetically modified foods…I have no dog in that fight. I'm not making any money. I'm not trying to be on the board of Monsanto, for example. If somebody hears my opinion, if it's an intelligent opinion – and I must say all scientists are very reluctant to give an opinion because we all really know we don't know as much as we should know to be giving good advice – but at least I'm not conflicted. If every biologist at [at this university] has some commercial connections, at what point can the community as a whole trust their judgment?…Maybe this is a hopeless dream. Maybe it's a hopeless dream to have experts that can venture opinions on subjects that they're knowledgeable in, but they're not conflicted in. Maybe that's just a naïve, impractical idea now, but I don't think so. [80]

Traditionalists shun commercialization due to perceived threats to organizational and professional processes and outcomes. Organizationally, commercialization may influence departmental responsibilities such as committee service and teaching loads in ways that place the burden on traditionalists. Professionally, the concerns centers on the integrity of scientific knowledge as it conveyed in journals or to the public, the training of future scientists, and the preservation of pure science.

Summary

I have sought to determine what constitutes and differentiates the moral orders of commercialist and traditionalist science. To do so, I have considered four dimensions of scientific work generated by the study. In table 10, I present a codified representation of these

Table 10: Moral Orders of Commercialist and Traditionalist Science, by Dimensions of Work

	Traditionalists	Commercialists
Work organization	Collegial	Hierarchical
Bases of Status		
Income	Average/Above Average	Above Average
Predominant external positions	Scientific	Corporate
Professional product	Knowledge	Technology
Visibility sought	Scientific impact	Societal impact
Dirty work designations	Administration	Administration
	Directed research	Directed research
	Incremental publication	Incremental publication
	Commercialization	Book publication
Overall modal pattern	Professional purity	Professional rebellion

patterns that shows the ways in which the moral orders of commercialist and traditionalist science are socially differentiated.

The organization of work underlying commercialist and traditionalist science diverge in terms of goals, organization, and size. Traditionalist research groups may be understood as collegially-organized or characterized by collaborative, craft-like organization of work in which group structure is relatively flat with the exception of the position of the advisor, who works in close proximity with scientists in training. The accumulation or overlap of grants causes traditionalist groups to grow to potentially large sizes, leading to dissatisfaction as work organized in this manner detracts from scientists' ability to effectively train scientists, which they view as a central objective of their role. The size of commercialist groups, typically twice that of traditionalist labs, is derived from multiple funding sources and the presence of numerous postdoctoral scientists to whom commercial tasks are allocated. At a minimum, work organization in commercialist labs is segmented, but not hierarchical, in that a division of labor within a laboratory does not necessarily entail one strata controlling and coordinating the other. Yet there is considerable evidence to the contrary. First, an informal social hierarchy exists by virtue of postdoctoral scientists' status as PhD recipients. Second, postdoctoral scientists are commonly referred to as an advisor's "lieutenants", which additionally suggests hierarchical lines of command. Third, the rewards generated by graduate and postdoctoral work are asymmetrical, as the latter stand to gain financially from their work. Fourth, traditionalists who received training from or are peers of commercialists observe that commercialist labs are hierarchically-run, with some portion of doctoral training being led or coordinated by postdoctoral scientists.

Examining three bases of status in commercialist and traditionalist science – income, client status, and esteemed professional outcomes – the data show that the earnings of commercialists (not including royalties) exceed their traditionalist peers. Commercialists and traditionalists may be differentiated further by the status of their clients, or the predominant external positions they maintain outside their university roles. Traditionalists engage in almost exclusively intraprofessional or purely scientific forms of client work, such as service on journal editorial boards, departmental evaluation committees for other universities, and service to federal and non-profit scientific organizations such as the National Science Foundation or the National Academies of Science. Among the traditionalists, client work primarily entails the advisement on the administration of science. The commercialists, by contrast, predominately serve corporate clients. To be sure, they serve in similar positions as traditionalists, but by the number and duration of their commitment to the directorates and scientific advisory boards of corporations, corporate clients comprise their predominant external commitment. Ultimately these positions serve an economic function. Even if advisory roles are delimited to scientific consultation, corporations are predicated on expansion of profits, not the advance of knowledge. The two may overlap, but scientific interests remain secondary.

As objective bases of status, the higher income of commercialists relative to traditionalists, coupled by the fact that commercialists predominately serve groups motivated by economic interests while traditionalists serve groups motivated by scientific interests, raises the question of whether intraprofessional prestige is accorded based on professional values or criteria external to science. The data show that commercialists and traditionalists are split along these lines, revealing their adherence to distinctive reward systems. As Shils (1968) notes, the criteria to which groups assign the most weight in evaluating status are those which have the most likelihood of protecting or improving a group's position. Traditionalists perceive commercialization as a threat to science's commitment to the production of knowledge, which they assign the highest status, whereas commercialists assign the most esteem to the production of technology, a course of activity for which commercial rewards and incentives have improved their position by virtue of higher income and broader networks of influence.

Consistent with their view of status, commercialists construct their visibility through their contributions to society. A noteworthy attribute of commercialists' construction of visibility is the extent to which their emphasis on technologies created or groups affected stresses these outcomes to the exclusion of scientific contributions. Two other patterns reveal the weight assigned to these pursuits. First, commercialists frame scientific achievements as mundane or ordinary features of a scientific career. Second, among commercialists who became scientists after 1980, visibility is framed in terms of a future self and predicated on eventual societal impact, despite existing achievements within science. Commercialists thus seek visibility through societal impact and construct their influence as cosmopolitan (Gouldner 1957-58), or in reference to groups beyond their universities and for that matter, beyond the academic scientific community.

Traditionalists, by contrast, construct their visibility in terms of scientific achievement. That is, reputation is based on the impact of their research on other scientists. As a result, traditionalists whose rate of publication or citation has declined stress local spheres of influence, primarily in terms of impact on graduate students but also vis-à-vis administrative positions, whereas those whose publication and citation have led to rewards from the scientific community stress both the influence of their research and their graduate students. In this respect, traditionalists construct both local and cosmopolitan influence (Gouldner 1957-1958). What is more, even in instances in which traditionalists are able to claim influence in industrial research, these outcomes of their work do not factor into their constructions of visibility. Meaningful or substantive societal impact is primarily viewed as the result of the collectivity, not in terms of individual contributions.

The dimension along which commercialist and traditionalists most closely resemble one another is their view of pursuits that depart from ideal modes of conduct. The convergence of the scientists is because “dirty work” is a ubiquitous feature of occupational life (Hughes 1958). The tasks that they avoid or regard as trivial – administrative work, directed research, and incremental publication – do not substantively contribute to commercialist or traditionalist respective goals of societal or scientific impact and thus the scientists designate them as departures from the ideal conduct. Nevertheless, they are orderly features of their environment and episodic events within a career. Even great scientists assume administrative positions and publish less than transformative research findings.

The resemblance of commercialists and traditionalists along this dimension ends here. Commercialists shun book publication, whereas traditionalists shun commercialization. From the perspective of commercialists, book publication is a wasted effort that does not advance science, but commercialization is an unconventional scientific activity that should be rewarded based on the rarity with which one successfully has societal impact. Traditionalists also acknowledge the rarity of commercial success but, by contrast, shun these pursuits based on the views that the potential risks outweigh the potential rewards and because they do not see them as fulfilling the goal of discovery.

Overall, the dimensions of the traditionalist and commercialist moral orders of science coalesce into two general modal patterns that characterize the nature of scientific work within each. The work of traditionalists is characterized by *professional purity*, whereas the work of commercialists is comprised of *professional rebellion*. Traditionalists' views of science and the practices in which they prefer to engage, much as the arguments of Abbott (1981), Merton (1973), and the Coles (1973) suggest, are governed by professional criteria only. Intraprofessional status is a function of professional purity, or the ability of traditionalists to exclusively work within the knowledge base around which the profession is organized. The essence of this pattern is found in the parsimonious manner in which the senior peer of a newly appointed traditionalist challenged him: "All we want you to do is change the way we think about science" [69]. The moral order of commercialist science, by contrast, is characterized by professional rebellion. Commercialists reconstruct the goals of science to reflect fidelity to societal rather than professional problems, they organize their work around this end, and they construct standards of eminence and success that depart from the community of academic scientists. Merton referred to such patterns of adaptive behavior as rebellion (Merton 1957), yet he never postulated that such patterns would comprise the basis for eminence in science.

Indeed, these findings reveal that the arguments of sociologists of science and professions are unable to account for commercialization as a basis of intraprofessional status of scientists who occupy the ranks of the elite. Abbott (1981, p. 824) states that "the academic professional's high status reflects his exclusively intraprofessional work." In his view, engagement with the "squalid reality" of nonprofessional issues mocks the "pristine abstraction" of professional work and therefore professional engagement with applied or impure science would be considered professional defiling (Abbott 1981, p. 825). Yet as the evidence here clearly indicates, the "dirty work" at the frontlines of professional practice that most scientists avoid and regard as irrelevant is, in the eyes of the commercialists, the basis of power, eminence, and achievement. In the case of the commercialists, therefore, the purity thesis does not hold.

CHAPTER 4

THE TRANSITION TO COMMERCIALIZATION

The last chapter presented the scientists' current views of what science is and the conventions of scientific work that define what it means to be a scientist in the context of an intensive commercial culture. However, the views of the scientists, particularly among those scientists who began their careers well before the acceleration of commercial activity in academe, were developed through varying experiences and exposure to commercialization. Thus, this chapter will place commercialization in career context. Specifically, I will closely examine the conditions that prompted scientists to embark on a commercial career path by focusing on the factors scientists claim were influential in their transition to commercialization and the temporal patterns underlying these changes. Accordingly, I consider the extent to which traditionalists considered a commercial trajectory and show how the circumstances of their exposure to commercialization in science foster conceptions of commercialization that depart from those held by the commercialists. In doing so, we move toward a better understanding of why a commercially-oriented reward system fosters unconventional scientific pursuits among some while reinforcing conformity to traditional means of achievement among others.

In tracing the paths that lead to commercial career trajectories, this chapter will stress the ways in which scientists make sense of their work. Undoubtedly, factors such as prior achievement, institutional prestige, local commercialist peers, and an institutional commercial culture are environmental properties conducive to the adoption of commercial behaviors (Stuart and Ding 2006). What, however, explains why scientists depart from or conform to conventional behavior when these factors are "held constant"? Moreover, both categories of scientists operate within environments characterized by extensive resources for research and commercialization. A complete answer requires that we examine the timing of commercial trajectories, the influence of socialization, and the ways in which scientists make sense of commercial career paths within these environments.

In what follows, we peer into the circumstances that motivated scientists to embrace or eschew commercial career paths. The analysis is derived from data on the timing of commercial turning points and from scientists' responses to two main questions. I asked commercialists to discuss what factors initially motivated them to commercialize their research. Given the extent of their commercial practices, the interviews focused on the initial commercial act as it constitutes a critical turning point in their career. To address the same issue among traditionalists, I asked them to discuss what extent, if any, they have considered or pursued commercial behavior over the course of their career. Although scientists with patents were excluded from the traditionalist category in the sample, we must consider if traditionalists have had opportunities to commercialize their work and, if so, what their responses tell us about the influence of the reward system. The second question focused on conceptions of and exposure to commercialization during professional socialization.

I will first consider the temporal context of commercialization by examining the timing of initial commercial behaviors. I then look at scientists' first exposure to commercial culture and the influence of professional socialization on conceptions and behavior. Comparisons in this section are contextualized first by era of professional socialization and then by orientations to commercialization. I then turn to a consideration of the factors motivating scientists to transition to commercial career paths. I begin by describing traditionalists' experiences and views of commercial opportunities and then turn to the patterns in the commercialists' accounts of their initial commercial behaviors. The concluding section summarizes the findings in the chapter.

The Temporal Context of Commercial Turning Points

Before turning to why scientists do or do not commercialize their work, it is useful to understand when commercialization typically occurs in the context of an academic career. The temporal patterns of commercialization are important to understand because they describe ways in which commercial careers may differ from traditional careers. This information also enables more precise theoretic explanations of commercialization in that temporal patterns may call our attention to specific career stages in which the transition to commercialization is likely to begin.

The key turning point with which we must concern ourselves is the initial transition to commercialization. For some scientists, deciding to patent one's work can be an important determinant of a future career course, in that it may initiate a status passage in which one's role and identity begins to shift (Glaser and Strauss 1971; Barley 1989). During this shift, new commercialists develop new interactional partners such as other commercialists, technology licensing administrators, investors, and industrial executives and scientists. New directions in research begin. Other research trajectories may be closed off in order to avoid conflicts of interest. The transition also entails a shift in the way scientists present themselves to their peers and graduate students, as temporary or prolonged requirements of secrecy may preclude open discussions of certain laboratory matters. Correspondingly, a new commercial identity begins to develop, as these changes foster different career expectations, commitments, and ambitions for achievement. The way that one begins to envision one's future self takes new form.

This status passage does not describe all academic scientists who patent. It is, however, representative of the commercialists in university environments where entrepreneurial activities are commonly found. As the data presented in chapter 2 indicate, most scientists in the population of academic scientists with patents rarely exceed one or two patents. For the commercialists in this study, by contrast, the initial patent filing initiated a process that would repeat itself many times over the course of the career, in all cases followed by numerous licensing arrangements and in most (about eighty percent), company formation. For the ideal typical commercialist, therefore, the date of the first patent filing is a reliable referent by which to mark the onset of the transition to commercialization.

In table 11, I present the distribution of ranks the commercialists had attained at the time of their initial commercial act. The table requires careful interpretation, as different cultures of commercialization characterized science before and after 1980 when the Bayh-Dole Act was passed. Commercialization was neither fully institutionalized after the legislation, nor was it nonexistent prior to it. For example, eight of the fifteen commercialists who received their PhD before 1980 initiated their transition to commercialization before the Bayh-Dole Act was passed.

Table 11: Distribution of Commercialists, by Rank at First Commercial Act[a]

Rank	PhD before 1980[b]	PhD 1980 or later
Full	7	1
Associate	2	5
Assistant	4	7
Post-Doctoral	1	3
Graduate Student	-	1
Total	14	17

a Measured as the filing of the first patent or first company. Two scientists formed companies during the 1970s prior to filing any patent.

b One scientist was excluded from this category because he worked in industry before appointment to a full professor position

One scientist filed his first successful patent during the late 1960s, two scientists started companies during the 1970s, and the remainder filed successful patents during the 1970s. As indicated in the table, most of these scientists were well established at the time of their transition to commercialization, having earned tenure at elite universities. Among the seven scientists who were full professors at the time of their first commercial act, six did so from two to seventeen years *after* the Bayh-Dole Act was passed. Engineers comprise the four individuals from the pre-1980 cohorts whose first acts were initiated as assistant professors, fields in which commercial behavior at this career stage is less likely to be regarded as unconventional, independent of historic context. The one scientist whose first commercial act occurred during his post-doctoral work did so in the context of a position in a basic research laboratory of a corporation during the mid-1970s. The modal career pattern that characterizes the stage at which these scientists initiated commercial turning points may thus be understood as *establishment*. That is, commercialization among scientists who entered academe prior to 1980 was predominately initiated well after proving one's worthiness among the scientific elite, signified by the achievement of tenure.

Whereas cohorts who entered science before 1980 were established and in later career stages when they began their commercial trajectories, those who received their PhDs in 1980 or later were decidedly more likely to initiate a commercial career path as their career were in stages of *initiation* or *escalation*. These patterns indicate identification with the commercial career path prior to establishment, a period of time during which scientists have yet to fully secure their membership among the elite. As indicated in table 11, the majority did so prior to the conferral of tenure. It is noteworthy that eleven of the seventeen scientists whose careers began coincident to the acceleration of commercialization in academic science were assistant professors, post-doctoral researchers, or graduate students. On one hand, it suggests that as commercial culture has institutionalized in academe, commercial and traditional academic incentives compete in earlier stages of the scientific career. According to Merton and Zuckerman (1973), we would expect an embrace of nontraditional academic incentives after establishment and evidence of traditional academic achievement, much as was evident in the case of the early cohorts of commercialists. It is not simply the case that the later career stage of commercial turning points among this latter category of scientists is explained by the legal institutionalization of commercialization. This may play a role, but so too does professional socialization, and the legal argument does not explain away the number of scientists starting companies or patenting before 1980.

A more detailed view of the timing of the transition to commercialization depicts similar patterns of establishment, initiation, and escalation. Table 12 illustrates the timing in scientific age, or the years since one's PhD. that scientists began their transition to commercialization. In all but five cases, the first patent is the first commercial act. Five of the scientists started

Table 12: Initial Transition to Commercialization, in Years Since Ph.D.

	PhD before 1980	PhD 1980 or later
First patent filed		
Average	10.7	4.9
Range	3 - 22	-4 - 13
N	17	14
First company founded		
Average	23.9	11.2
Range	1 - 46	2 - 21
N	13	12

companies before or during the same year of their patent filing.[1] Echoing the previous table, Table 12 shows that the earliest cohorts of commercialists generally filed their first patent roughly a decade after completing their PhD. Given that almost all of the scientists completed post-doctoral training (typically two to three years), we might assume that these scientists began commercial trajectories in close proximity to achieving tenure, anticipating but not yet entering the middle of their careers. Indeed, it was not until these scientists were well beyond the early stages of their career that they tended to start their first company, about twenty-five years after completion of the PhD on average. Three of the scientists who exhibited extensive patenting and licensing behaviors only recently formed their first company, approximately four decades after finishing their doctoral training in the late 1960s.

The scientists who started their careers in 1980 or later, by contrast, began to engage in commercial behaviors before, during, or very soon after completing their PhD. On average, the later cohorts of commercialists filed their first patent about five years after graduate school, just after their post-doctoral training during the first few years of their appointments as assistant professors. Company formation appears to begin in close proximity to tenure. Two of the scientists started companies as post-doctoral researchers. Notably, each took eight years to receive tenure, a period longer than all other commercialists.

Although it is difficult to draw precise comparisons between the commercialists who began their careers before and after the acceleration of commercialization in academic science, tables 11 and 12 clearly suggest a shift in the formal organization of the academic career. Earlier cohorts of commercialists' middle and late stage engagement with commercial behavior follows a more general pattern in academe in which conformity to norms and commitment to traditional

[1] Companies not based on patents may be nevertheless based on intellectual property such as computing algorithms. Alternatively, such as the case was for one scientist who sought early entry status into the biotechnology industry, they may be initiated by not yet complete but anticipated technologies.

forms of achievement become less stringent relative to behaviors in early career stages. More recent cohorts of commercialists appear to identify with and commit themselves to commercial forms of achievement much earlier in their careers. Commercial rewards, these findings suggest, are now influential for some very early on in scientific careers. Given the early stage at which commercialist behavior appears to begin in the contemporary conditions of science, we must look more closely at the ways in which scientists are exposed to and influenced by commercial incentives.

Socialization

Scientists' views of and transitions to commercialization must be placed in the context of their professional socialization. Both traditionalist and commercialist normative orientations bear the imprint of the process of professional socialization to science, beginning with intensive training in graduate education programs (Hagstrom 1965) but also continuing during post-doctoral training and throughout the course of the scientific career. Additionally, the temporal characteristics of the commercial careers of recent cohorts of commercialists suggest that graduate and post-doctoral training may be contexts in which some scientists begin to identify with the commercial role. Finally, other scholars have argued that undergoing doctoral training at universities in which commercialization is prevalent is a key influence on the transition to the commercial role (Stuart and Ding 2006).

A total of twenty-four universities comprise the contexts of doctoral training for the 61 scientists in this study. In table 13, I list these universities arranged according to the year in which a technology transfer office (TTO) was formally established. Although unsurprising given the elite status of the universities in this study, the table reflects the fact that the scientists

Table 13: Doctoral Origins of Scientists, Sorted by Year of Technology Transfer Office (TTO) Establishment

University	TTO Est.
1. University of Wisconsin	1925
2. MIT	1940
3. University of Minnesota	1957
4. University of Utah	1968
5. Stanford	1970
6. Johns Hopkins	1973
7. University of Iowa	1975
8. Harvard	1977
9. Caltech	1978
10. Cornell	1979
11. UC Berkeley	1979
12. UC San Francisco	1979
13. UC Los Angeles	1979
14. Columbia University	1982
15. University of Michigan	1982
16. Yale	1982
17. Emory	1985
18. University of Pennsylvania	1986
19. University of Chicago	1986
20. University of Alberta	1987
21. Carnegie Mellon University	1992
22. University of Illinois	1995
23. Freie Universitat Berlin, Germany	N/A
24. Heriot-Watt	N/A

underwent training at high-caliber research intensive universities, many of which are among the most elite scientific environments in the United States. With respect to their commercial traditions, a majority of these universities established TTOs before the Bayh-Dole Act was passed. A subset of them exhibits longstanding commercial operations, most notably the University of Wisconsin, MIT, the Universities of Minnesota and Utah, and Stanford University. While other studies (Stuart and Ding 2006) use the presence of a TTO as a proxy for institutional entrepreneurialism, it is useful, but imperfect. Most of the universities have some degree of commercial behavior preceding the establishment of the TTO. For example, with the exception of Columbia, Yale, and Emory Universities, each of the U.S. universities was assigned patents prior to Bayh-Dole. Nevertheless, the table suggests that the majority of the scientists, both commercialist and traditionalist, were trained at institutions in which commercial practices were present.

A more precise view of the potential for exposure to commercial culture is provided by examining the extent to which the scientists were trained by commercialists. Given the extreme time commitment and dedication demanded during socialization to science, the laboratory can become an insular world. Even if a university exhibits commercial cultures, particularly if they are not prevalent in the department in which one receives his or her training, a graduate student is unlikely to be directly exposed to commercial practices. Of greater importance is the fact that one's advisor, perhaps more so than any other element of socialization, shapes the style of scientific work and the conception of the scientific role (Zuckerman 1977). In table 14, I present the number of instances in which scientists received training from scientists who either patented extensively or had founded a company before or during the participants' professional

Table 14: Number of Scientists with Commercial Advisors

	Academe		Industry Post-Doc	Total Scientists Trained by Commercialists
	PhD	Post-Doc	Post-Doc	
Commercialists				
Ph.D before 1980	1	0	4	5
Ph.D 1980 or later	7	5	2	12
Traditionalists				
Ph.D before 1980	0	0	1	1
Ph.D 1980 or later	3	1	2	5

socialization. As we will see in shortly turning to the subjective data, these figures represent the potential for, not necessarily exposure to, commercialization. Overall, approximately one-half of the commercialists received training from either a commercialist in academe or a fundamental scientist or engineer working in an industrial lab, whereas approximately one-fifth of the traditionalists received training in these contexts. Unsurprisingly, the overall number of scientists trained by commercialists is lower before 1980 relative to the number who entered science that year or later. This modal pattern suggests that both institutional and individual exposure to commercialization among *all* scientists who entered academe prior to 1980 was low. The table presents mixed results for the effect of training under a commercialist on identification with the commercial role. On one hand, twelve of the seventeen commercialists in the study ultimately received training from established commercialists either as graduate students or post-doctoral researchers. This could partially explain why these scientists were likely to transition into commercialist roles at such early stages of their career. On the other hand, the number of traditionalists who received training from commercialists, although minimal, calls into question the extent to which this condition leads to exposure to or identification with commercial practices. Having documented these structural characteristics, I now examine scientists' views of commercialization as they underwent professional socialization.

Traditional Socialization: 1960s & 1970s

The scientists who began their training during the 1960s and 1970s told almost uniform stories of their professional socialization and the characteristics of science at the time. The cohorts of scientists who entered science in the 1960s did so in a "golden age" of academic science, one that was driven by a doubling of federal spending on academic research and a substantial increase in the percentage of funds devoted to basic research (Geiger 1993). In the

1970s, funding was in retreat, more programmatic, and university expenditures were on the rise. It was a decade of incubation for the biotechnology revolution and development of new forms of university-industry organizational arrangements, leading presidents and administrators of several universities to meet to discuss how academic values could be protected from these shifts (Geiger 1993). The accounts of both traditionalists and commercialists who trained during these eras suggest that in these decades, the normative structure of science remained well insulated from these developments. That is, the accounts indicate the individual and institutional exposure to commercialization, regardless of what local presence it may have had, was low among these senior cohorts of scientists.

Traditionalists and commercialists who began their professional socialization in this era were trained in contexts that sought to foster extreme commitment to fundamental science. Regardless of whether one was a scientist or engineer or whether one trained at an Ivy League university or a technical institute, training in this era overwhelmingly focused on fundamental science. Industrial science and notions of commercialization, moreover, were shunned and in many cases, functioned as objects of disdain. Consider, for example, the accounts of four traditionalists who began their training in the 1960s and 1970s.

> I had very little exposure to it because at that time the chemical engineering department at Stanford did have a very definite basic science focus and it was only later really that there was commercialization of research. [42]

> Certainly the sense at Chicago was science was pure. It did not get involved in industrial things, even though eighty percent of the PhDs in chemistry go into industry…There's

always been in chemistry a kind of connection that was a little bit standoffish. A good example of that is that the American Chemical Society used to be run by the industrial chemists. I remember when I was a graduate student, you didn't join the American Chemical Society, it was considered beneath you because it was guys interested in making better soaps and things like that. Not pushing back the frontiers of science.

How would you characterize the view that you had as an Assistant Professor entering this university?

Oh I was still in a Chicago mode. Pure, pure science is what I wanted to do. And I didn't think about anything else. [43]

At Caltech there was no industrial affiliate arrangement. Nobody admitted wanting a non-academic job. Nobody would have considered, maybe they would have considered getting rich off of their discoveries, but they wouldn't have bragged about it because it wasn't pure. Very much there was a spirit of science is a pure enterprise that you do because it's there, and you don't do it to get rich. [70]

It was 1964 to 1969, the Silicon Valley didn't exist, no biotech company existed. Cohen and Boyer were just getting started. There was one guy at Stanford who discovered the birth control pill and it was a huge commercial success but he was looked down on. Everybody knew he was a great chemist, but he had done something that wasn't quite legit, he'd actually gotten involved with a company. The argument was that a real research scientist doesn't dirty his hands with money, that's a concern that's likely to turn your head and take you away from the most important problem, and it sure did for

> Djerassi. It turned out he caused a world revolution, so you've got to admit he had a big impact, but there were people who felt that impact or no, this is just not what a scientist should do. [32]

The scientists' accounts reflect a condition of *ideological closure* (Light 1980) or an exclusive emphasis on one model of professional behavior. In such an environment, the potential for role improvisation is limited, unrewarded, or it is punished. Meaningful participation in the scientific community solely entailed fundamental contributions to knowledge, hence the status of industrial science as beneath or outside of the profession.

The commercialists' accounts of science reflect the same world characterized by the traditionalists. They described commercialization as "unthinkable" or themselves as "unaware" of it. Still others, a minority who disclosed a disposition to technical utility, characterized themselves as "unusual" because of this orientation. That the majority of commercialists who trained in this era did not embrace commercial behaviors until well after their careers were established in part is a reflection of the commitment to traditional means of achievement forged during socialization. Consider, for example, a commercialist's depiction of his training.

To what extent were you aware of or exposed to commercialization during your training?

> Zero. And it so happens that I did my post-doctoral work in Cambridge, England, in the very, very best laboratory in the world by any standard. When I went into that building there were 28 people there. Within seven or eight years there were six Nobel Prizes that went within that group of 28 people. Okay? That's demonstrably the best place in the

> world ever and the person I worked with had two Nobel Prizes ultimately. He only had one when I started. So I wasn't thinking of anything commercial. [81]

This scientist, like other commercialists and traditionalists, identified with ultimate scientific achievement as a result of socialization in an environment characterized by elite standards of performance. The enduring influence of this training explains why it was over two decades after graduate school, twelve years after promotion to full professor, that the scientist filed the first of forty-four patents. Strong socialization processes that seek to create unequivocal notions of professional identity and belonging are not quickly unraveled.

The similarity of the socialization experiences of commercialists and traditionalists in the 1960s and 1970s may reflect fidelity to pure science, but as Ignatieff (1983) notes all institutions are "promiscuous around the edges." Academic science was no exception. Frederick Cottrell (Berkeley), George Dick and Gladys Henry (Chicago), George Minot and E.J. Cohn (Harvard), and Harry Steenbock (Wisconsin) are among the most notable and earliest pioneers of the commercialization of university research (Weiner 1987), but they were also followed by a number of other scientists who actively patented. There was only one commercialist in the study for whom socialization before 1980 fostered identification with commercialization and it was clearly because he trained under a scientist who was among this early group of university scientists forging commercially-oriented careers. This scientist, who filed his first patent in 1968, specifically cited the influence of his graduate advisor when I asked him what factors led him to commercialize his work.

> Well, first of all you have to remember that I worked for [Campbell]. He's probably the greatest example of applying what one finds and I was greatly influenced by him…When I received the assistantship offer from him, it wasn't an offer, it was a command…This was the place to go. And I was fortunate to really work for a prime example of [applying scientific discovery]. I mean he was truly creative. He had this discovery that cured [a disease]. That was an important scientific discovery. [29]

If a pantheon of commercialists existed, "Campbell" would unquestionably be among the pioneers. He ultimately brought millions of dollars in revenue to his university and his reputation lives on in the accounts of commercialists at this university, a reflection of the pattern of commercialization as the basis of intraprofessional status observed in chapter 3. In the same way that training under an elite traditionalist was enduring and consequential for the previous commercialist we considered, apprenticeship under "Campbell" had an equally powerful influence on this scientist. Exemplifying the symbolic interactionist concept of "taking on the role of the other", the scientist received an appointment in Campbell's department and has generated hundreds of patents and two companies, bringing revenue to the university beyond the precedent set by his commercialist "master." It was not the case that Campbell and his student were not exposed to a similar culture described by other scientists who underwent training before Bayh-Dole. But, as the first at his institution to forge a commercial path, Campbell eased the way of those who followed in his footsteps.

> [This university] was in a very unusual place because the academicians had already benefitted from the [Campbell] patents…We were less uncomfortable than maybe at

> other institutions, but I didn't really feel that much that I was violating an academic rule. Commercialization was seen as a bastardization, if you will. It's sort of...this isn't what science is all about. A lot of change was beginning to happen, some of it good, some of it not. One of the byproducts is that you're in science to make money. In those days you definitely weren't in it for money. You were in it because you wanted to find out something new. So I think the change at that point began to happen with the Bayh-Dole Act where now university inventions mean something economically. [29]

With the exception of this case, professional socialization prior to 1980 may not be viewed as encouraging identification with the commercial role. Apart from the seven scientists who first transitioned to commercialization before 1980, professional socialization appears to have constrained, rather than enabled identification with commercialization. The majority of scientists trained during these eras acknowledge a widespread view of commercialization as *dirty*. That is, both traditionalists and commercialists tended to view commercialization as morally questionable, tainted, or as a distortion of pure science. When we examine the accounts of commercialists who began their scientific careers after 1980, this pattern shifts among traditionalists and reverses among commercialists.

Socialization in a Commercial Context: 1980s – present

The scientists who underwent professional socialization after 1980 did so as science became more structurally and culturally differentiated, a process driven by accelerated commercialization and new organizational forms that fostered tighter links between university scientists and industry. Professional conduct shifted from ideological closure to *ideological pluralism,* in that the mode of role performance was no longer limited to commitment to fundamental science, but could include commitment to the development of technologies and the acceptability of profiting from one's work. Commercial norms and rewards did not replace the traditional normative system of science, but nevertheless entered into academic environments. Almost invariably, scientists who completed their training in the 1980s and 1990s expressed an awareness of these developments, reflecting a general pattern of high institutional exposure to commercialization. For some traditionalists and commercialists, they only observed the rise of commercialization at a distance. Apart from these scientists, two patterns define these cohorts. Among the commercialists, exposure to commercialization during socialization was strongly influential in their own identification with a commercialist role. For these scientists, commercialization was *elemental*, or a standard component of the scientific life. Among the traditionalists, exposure to commercialization fostered negative views of it, in turn affirming or increasing commitment to traditional means of scientific achievement. These scientists, as we will see, viewed commercialization as a career threat or distraction.

We begin with the scientists for whom professional socialization was central to the transition to commercialization. The experience of one scientist in particular is indicative of the broader shifts occurring between science and industry during the late 1970s and early 1980s. The scientist finished a PhD in chemistry at Stanford in 1981, thus his training bridges the broad shift in commercial culture. Note the parallels between his story and the more senior scientists we have already considered.

> When I was in grad school, the culture was that people who work in academics are smart, people who work in industry are less smart. So there was that idea which was quite strong and I shared it. I learned maybe that's not correct, but I shared that and felt that at the

time. And then there's also the idea that if you take money from industry or dabble in money-making activities then you're sort of tainting the well. You're not a real scientist anymore. You're kind of a money grubber and, 'What's that about?' And even sometimes taking money from a company to support your research I think was problematic. [34]

The superiority of academic science, the taint of profiting from one's work, and the emphasis on disinterestedness are themes indicative of the scientists whose professional socialization occurred in earlier decades. This scientist's training ultimately entailed a process of learning and unlearning these components of the academic role. After graduate school, the scientist held two postdoctoral positions, the latter of which was at Caltech in the lab of an established commercialist who at the time had already founded a company (and has since founded over 10). As he indicates, this period was formative in his identification with the commercialist role.

I left graduate school with my PhD. and joined [Roth's] Lab. We came out with some ideas and [Tuchman], in particular, who is a big name in this field, he was at that time a 'lieutenant' in [Roth's] lab and he was a co-inventor on my first patent, so he sort of guided me through the patenting process which I didn't know anything about. It was just all new to me, but it seemed like fun. I have generally a positive attitude towards learning new things and it was sort of exciting. It was just a learning thing.

Did you think anything would come of it?

Didn't really know. I thought it would be cool if it did. I don't know that I was worried about it one way or another. I was just going like...oh, we should do a patent. Great, let's

> do a patent. I didn't have to think it through particularly. Again, it was just something to do that sounded cool. [34]

Other scientists who characterized their graduate experiences as "intensely fundamental" or "hard-core science" described similar post-doctoral experiences. The accounts of such scientists were laced with appeals to the eminence of their advisors and the prominence or size of their companies. The key process emphasized in such accounts was the learning of the new role.

> I saw that he was able to wear these different hats....I became solvated if you will, in a different kind of environment, and that was formulative in the sense that it taught me how this could be done and showed me that it actually could indeed be done, and so that was great. Everyone [at the company] knew that I was working with him, and he was the big boss, and so I kind of had carte blanche, and it was just a wonderful experience. So that was very influential. [28]

It is noteworthy that the scientists portray the shift from a traditionalist to commercialist role as somewhat unproblematic. Theoretically, we would assume that such a transition would entail ambivalence between traditional and commercial norms. Four factors potentially explain this. First, it could be the case that the scientists underwent relatively weak socialization processes in which only moderate commitment to traditional scientific norms occurred, yet this would not only be uncharacteristic of socialization to science, it would be unrepresentative of the scientific environments in which they were trained. Second, and relatedly, the relative ease with which postdoctoral scientists "unlearn" the traditionalist role could be a function of limited independent experience. This explanation suggests that the short duration of independent performance of the traditionalist role offers few opportunities for rewards to ossify commitment to a traditionalist identity and role. Decorated military officers who spend substantial periods of their life in the military continue to identify with the role and culture as they transition into civilian work and retirement, for example, whereas those who leave the service at the termination of their first term subsequent to boot camp are more likely to shed themselves of a military identity. Third, it could be the case that the transition is eased by virtue of the level of prestige, power, and success allocated to the established commercialists under whom training is received, giving them, in the eyes of those who learn from them, unquestionable moral license. As Merton, Reader, and Kendall (1957) aptly noted, socialization occurs primarily through interaction with people who are significant for the individual. Coupled with the observable shifts toward commercialization in the broader environment, training under scientists reputable both within science and the commercial community may trigger a shift from a traditional to commercial identity (Stuart and Ding 2006).

This is particularly the case for commercialists who trained under established commercialists in their doctoral training. For these scientists, commitment to the commercial role is learned as a component of socialization to science. Socialization to commercialization under an established commercialist occurs both indirectly and directly. Role aspirants emulate standards of performance. Therefore, even when commercialization is not an explicit component of the content of professional socialization, or even discussed, its legitimacy is taken as self-evident by virtue of example.

My basic philosophy about doing transformative science and the sweet spot of conceptual advances that also lead to practical applications was inspired by my PhD advisor. He won the Nobel Prize in chemistry, the highest scientific honor you can possibly get. He started his own company and many of his inventions and discoveries are now commercialized. I had a very good role model.

Was he commercializing his work during this time?

It was at a formative time but he had always patented things that he thought might be useful. At the time, he had not started any companies. That actually happened only after I left, but it was clear at the time that he was appreciative, he had a very good sense of both what was good science, you know new discovery, change the way people think, and how new science can enable new technologies and so again this place where you can actually do both.

That entered into your, sort of tacitly, it entered into your training as a scientist?

That's right.

Did you have conversations about this?

It was more a question of watching him operate, so never really any explicit conversations about how you would commercialize technology. So it was really learning from his example, more than any direct conversation about how to do it. [66]

Indirect exposure entails a limited form of socialization to commercialization as it primarily functions as an endorsement of the activity, rather than a process of acculturation.

Other commercialists, particularly those in engineering fields, were directly exposed to the commercial role through their training. In contrast to emulation, commercialization becomes a learned behavior derived from discussion, collaboration, and the encouragement of one's advisor.

> My advisor saw the work that I was doing and he said this is really, really practical. This is simple. It has all the attributes. He's a serial entrepreneur. He wanted to start companies. That was part of what made him tick, so he was really the driver. It was the surroundings, the environment, the fact that [this university] makes it not only easy but contractually obligates me to make disclosures and the fact that my advisor wanted me to take this idea and make a company out of it. [62]

The acceptability of commercialization is without question when one is socialized to it under an established commercialist. Scientists-in-training learn to view commercial behaviors as normal aspects of a scientific career. The operation of commercial rewards thus influence scientists' aspirations alongside traditional rewards, but absent the meanings assigned to commercialization learned by earlier cohorts of scientists.

The meaning of commercialization, however, is not unequivocal. The traditionalists developed a negative view of commercialization during professional socialization. For this group, exposure to commercialization entailed lessons of cautionary tales and examples of career outcomes to avoid rather than positive identification with the role. For some traditionalists, close observation of commercialist advisors provided an opportunity to be exposed to negative aspects of the commercial role, thereby leading to an aversion of commercial pursuits and an affirmation of traditional science.

My advisor at Berkeley has a company. I was able to observe things that he chose to do that he lamented, would complain about, or did not seem to actually enjoy. I think personality-wise he and I were similar, we got along quite well, and so I was more or less able to gauge that if he didn't like something, I was probably not going to like that same thing, and he never really seemed to like being in charge of [ChemCo].

And so you never envisioned the possibility of making money off of research that you do as an attractive scenario?

No....I'm actually surprised that anyone pays me anything to do what I do. It's also something that I would do even if they didn't pay me, so it's always kind of surprising when things that you just want to do, that somebody pays you for it. [47]

There was something that went on after [my advisor] started his company that I wonder if he might have caught if he wasn't doing two things at once. One paper in particular that got published that I think he wishes didn't now….There was actually a mistake in the paper…[which] was done in his collaborator's lab. It's very clear now that there was a problem with it, but [at the time] he wouldn't back down. He fought to the death more or less and in this case, it had a pretty big impact on his career. It was pretty clear that was the end of his possibilities. A guy got the Nobel Prize in his area, oddly he got it alone. I think everybody who's familiar with the field thought that was crazy. This was around the time when it was being talked about a lot. Who would get this prize? And I think that's why he fought so hard is he thought if it went south, he would definitely not get it. So you wonder, right? You wonder whether that would have been different had he been fully focused on this project instead of his company. [52]

> My advisor did a lot of consulting. There was another guy that I was aware of, who's very well known, who did a lot of commercialization and I think that he did not pay attention to his grad students and the quality of his research wasn't as good. I think it was a conflict. That's one thing that influenced my thinking about it, was seeing it from the graduate student point of view, I don't think he did right by the students that were in his group. But he made tons and tons of money. [39]

In these and other cases following this pattern, commercialization is viewed as a consequential distraction from traditional scientific means of achievement. Dissatisfaction, opportunity costs, and negligence in one's role become associated with commercial behavior, outweighing whatever value is assigned to commercial incentives. These concerns do not universally produce a view of commercialization as morally questionable, but they do affirm traditionalists' identification with and commitment to the conventional means of achievement and reward by neutralizing the potential for positive identification with the commercial role.

Some traditionalists, however, did develop negative conceptions of commercialization during their training through adoption of their advisor's view, observation of commercialization as a constraint, and negative environmental developments such as litigation between universities and commercial firms.

> During the time that I was a graduate student, a long-standing legal battle between UCSF and Genentech was being resolved. It seemed ridiculous because all of us were barely born when Genentech was founded. But that certainly was noticeable. My own advisor's attitude towards commercialization definitely tilts toward the negative. She was annoyed

and upset when during my graduate career they started a number of intensive seminars partnered with Bay area venture capitalists. They always had names like 'From Idea to IPO.' She hated what she called all the 'whispering in the hallways.' Her scientific, cultural, and intellectual ideal is complete openness and honesty and she really lives that. She's an extreme case of communicativeness, of unpublished results, of methods in development and it's rarely bitten her in the ass and she says that it's totally worth it, but that people who often behave rather badly in terms of poaching tend to behave better towards her. I mean she has a strong sense that you behave like this, and much of the world will rise to meet you.

Did commercialization ever occur to you as something that you were interested in doing?

No, no. And quite notably not, because my very, very close friend, classmate and bay-mate, the guy that I worked with my back two feet from for all of graduate school was very interested in industry and the private sector and he participated in a couple of 'Idea to IPO' type seminar class-based competitions and it seemed like a distraction. It was a distraction for him at that time. It worked out better for me to focus on my PhD. research. [54]

Traditionalists who are trained by scientists with negative views of commercialization do not develop expectations of commercial behavior, largely because they emulate the scientific role as it is enacted by their advisors. Coupled with exposure to pitfalls, distractions, and paths that failed to pay off in economic or scientific currencies, socialization in such contexts leads to conceptions of the academic role that cast commercialization as a career threat, despite its presence in the environment of science in the form of commercial peers, federal agendas for technology transfer, and institutional encouragement of innovation.

Variance in the exposure to commercialization during professional socialization is consequential for the ways in which scientists envision future career trajectories. As a mechanism of the transition to commercialization, socialization *constrained* identification with the commercial role among the commercialists who entered science before 1980, whereas it was a *key factor* leading the more recent cohorts of commercialists to depart from traditional scientific career paths. Why? Scientists socialized during the 1960s and 1970s lacked any commercial referent that could be projected into an envisioned career path, whereas for commercialists trained after 1980, successful commercialist advisors provided points of reference on which future career trajectories were aimed. A commercialist career script, validated by scientific or commercial eminence, was at the disposal of the scientists to follow. For the traditionalists trained after 1980, exposure to commercialization similarly provided reference points for a scientific career, largely in the form of trajectories and circumstances to avoid. The observed patterns also suggest “hybrids”, or commercialists with traditionalist mentors and traditionalists with commercialist mentors. As observed in table 14, all but five of the commercialists trained prior to 1980 were trained by traditionalist graduate and postgraduate mentors. Among all of the traditionalists, six were trained by commercialists. What explains the switch in career paths? To understand such a pattern and those in which scientists emulated their mentors, we must look closer at the circumstances and considerations of commercialization.

Social Mechanisms of the Transition to Commercialization

Having demonstrated the timing of transitions to commercialization and the role of socialization, I now describe recurrent themes that emerged in the analysis of the circumstances under which scientists consider commercializing their work. No singular factor explains why scientists either end up commercializing their work or conforming to traditional scientific means of achievement. These choices are shaped by a combination of influences such as training, field, cohort, career stage, achievement, federal research agendas, and institutional resources, to name but a few factors that shape a scientist's orientation to traditional or commercial scientific practices.

My analytic approach to identifying regularities in scientists' accounts of commercialization is informed by Dan Lortie's (1975) analysis of occupational recruitment. Lortie (1975) shows that occupations possess "recruitment resources" that attract some persons and repel others. Lortie focuses on attractors and facilitators. Attractors are features of the occupation that offer advantage or benefits to workers, whereas facilitators are social mechanisms that move people into an occupation (Lortie 1975). Whereas Lortie employs these concepts to consider how people choose from alternative lines of work, they may also be productively employed to examine how people consider alternate career paths within an occupation.

Commercialists

It is commonly assumed that funding constraints are a key factor motivating scientists to commercialize their research. Scholars of academic capitalism (Slaughter and Leslie 1997; Slaughter and Rhoades 2004) and other researchers (Etzkowitz 1998), for instance, cite the decline in federal research funding as a key mechanism encouraging commercialization. It is possible that funding difficulties prompted commercialists to seek industrial funding or generate resources from their research to confer stability on their research endeavors, but none of the scientists cited this as a key factor in their decisions to commercialize their research. The commercialists were quick to criticize the research agendas of federal funding agencies, particularly their programmatic nature and the emphasis on applied research, but none disclosed difficulties in gaining funding. Instead, the commercialists' experiences indicate that four attractors were influential in their transition to commercial practices: material benefits, protection, societal impact, and tangibility. Receiving equal emphasis in their accounts were two facilitators, social origins and unintentional entry, which prompted the pursuit of commercial opportunities. For the commercialists trained since 1980, we may add professional socialization as a facilitator, given the central role it played in their identification with the commercial role.

Material benefits, protection, societal impact, and tangibility constitute characteristics of commercial involvement that lead scientists to view a commercial career path as offering advantages or benefits that exceed the opportunity cost of sustained commitment to traditionalist science. As we will shortly see, the ways they vary among scientists reflect unique aspects of how they function. Exposure to each attractor thus leads scientists to confront commercialization and traditionalism as competing alternatives. To understand why the commercialists ultimately embraced a new career trajectory, we now examine the content and appeal expressed by each attractor.

Material Benefits

Material benefits are an institutionalized component of a commercially-oriented reward system. They are organizationally-based rewards present at each of the universities that incentivize the commercialization of research with financial returns. Broadly, the material benefits derived from commercialization may be distributed to scientists, their universities (including their own department, the technology transfer unit, and other administrative units), or organizations within the private sector. Among these, there are two forms that directly impact scientists, personal remuneration and funds for research.

When I asked scientists about the factors that influenced them to commercialize their work, rarely did the commercialists emphasize personal material benefits as influential. Typically, when money was mentioned, its role was downplayed and the amount was dismissed as minimal. The accounts of the following scientists exhibit a representative outlook.

> What I saw was that [commercialization] was a good way of trying to stimulate new research in the sense that once you had a company interested in this, they wanted to take it to commercial technology. This scheme of funneling money back to the PI for the research grant, the amounts usually are small enough that whatever goes back to the PI is not, in my opinion, not a good motivator, not a big motivator. But the fact that a lot of the licensing fee originally came back to the PI in the form of a research grant, was an accelerating factor. [26]

> My primary motivation was to get money out of the company to fund a research grant that I could get for students in the lab doing more chemistry…Just to be mercenary about it, we were able to use the fact that they were very interested in licensing this technology to leverage a research grant out of it simultaneously with the licensing contract so we not only got the license but we got a research grant that allowed us to fund work for about six years. That's the only financial incentive for me to patent anything. It’s not my major

> incentive. I have fifteen patents, of which eight were licensed…I put in a new bathroom in my new house off of some of the licensing income I got back from one of them, but it doesn't affect the way I operate. I'm not in this business to make money. It’s not enough money to be an incentive for me to do it a lot. [66]

Commercialists emphasize the financial benefits that return to their laboratories while downplaying personal remuneration. Lortie (1975) found a similar phenomenon of downplaying material rewards in his study of the factors that attract individuals to teaching. Despite the substantive differences in average salaries, academic science, like teaching, is often defined as underrewarded. Both occupations are also characterized as fulfilling a social role in which money and prestige should be of limited or no motivation. This is particularly the case for academic scientists, who have traditionally been characterized as disinterested experts who are committed to their work for its intrinsic value. According to Lortie, these types of normative pressures limit the extent to which the answers of scientists would indicate the actual role played by material rewards (Lortie 1975). Also, commercialists frequently stress the high probability of failure in commercial endeavors. It could be that scientists viewed the likelihood of substantive financial gain as unlikely and thus material benefits were of limited influence, but this is improbable. It is not unusual for rarely attained rewards to motivate extreme commitment in science (Zuckerman 1977; Hermanowicz 1998, 2010). Thus, even if unlikely, the goal of “hitting the jackpot” could be operative in scientists’ embrace of commercial activities.

Commercialists who entered science prior to 1980 generally *dismissed* the attraction of material benefits: they suggested that personal income and resources for research played little or no role in their decision to commercialize their work. This position was less prominent among commercialists who started their careers after 1980. This subset of commercialists generally characterized money as a *legitimate motivation* for commercial involvement. The tones underlying these latter accounts ranged from casual to emphatic. For example, one scientist framed his embrace of commercial practices as following established precedent at his university, an institution that provided one of the earliest models of technology transfer.

> The patenting aspect was just something people do around here. We have a technology licensing office which returns twenty percent to the inventors and with a young family that's attractive, and it also returns funds to the graduate school. So I just started patenting my ideas as they came along. [30]

Another tells a story of the formation of his first company, which occurred largely in response to the development of gene sequencing technologies. This story, like a handful of other accounts of company formation, follows a narrative that departs from the "typical" story of a company that begins with a discovery in a lab with apparent practical value that follows a path to the marketplace (i.e., "from idea to initial public offering"). Note, for instance, that the company preceded the discovery.

> …And so that all came into place and the data started pouring out and then some friends and I simply started thinking...well, what can we do with that data stream? And we realized that most of it was gibberish. It was just sequence information, but there was a subclass of genes, of DNA sequences that we could recognize with a pretty high probability and they turned out to be potentially very interesting, so we called around and

> asked whether anybody would give us money to set up a company to explore the utility of that data stream and we actually were able to raise a large amount of money in a short period of time…That's how we got the first one started and it's been quite successful. I made a lot of money and all my colleagues and all the scientists we hired all made a lot of money and the first products are just about ready to come to the market and they'll be pretty big products.

What were you hoping to accomplish in starting this company?

> I think we were interested in exploring whether we could do something useful, but we were certainly by that time interested in whether we could also make a lot of money. [82]

When we examine the material benefits that arise from commercialization, even from the perspective of one year, the potential influence of money is not easily dismissed. In table 15, I present an ordinal distribution of past year (2009) commercial income of the 27 commercialists who completed this portion of the post-interview questionnaire. These figures represent only *one year of earnings* and are thus a static depiction of the financial gains resulting from commercial activities. It is therefore difficult to know whether they underestimate or overestimate average financial returns over time. Given the extent of commercial involvement and the time necessary for a company to profitably utilize a technology for production (and thus pay licensing fees over extended periods of time), however, one can assume such returns are of moderate duration.
To contextualize the magnitude of financial rewards, it is useful to recall that data from the post-interview questionnaire show that commercialists in the sample earned at least $100,000 and at most $200,000 in their nine-month academic salaries[2]. At first glance, table 15 suggests that

[2] Four commercialists earned over $200,000, but the specific figure is unknown.

Table 15: Past Year Commercial Income, Commercialists

	Patent Royalties	Consulting	Company positions
less than $20,000	16	14	17
$20,000-$29,999		1	
$30,000-$39,999	1	2	
$40,000-$49,999	1	3	
$50,000-$59,999	1	1	
$60,000-$69,999	1		
$70,000-$79,999			1
greater than $120,000	5	5	1
Not applicable.	2	1	8
N=27			

when commercialists received royalties from a patent, consulted, or were paid for their position as an officer or board member of a company, the majority (approximately two-thirds) of them earned less $20,000 during 2009, but this pattern belies the probable earnings of the scientists. For example, consider that just under half of the commercialists did not enter any category indicating they earn over $20,000 per year in commercial income, but of these scientists, only three do not hold equity in at least one company. Not accounting for equity or prior earnings, we could conservatively estimate that for this subset of the commercialists, the financial rewards derived from commercial activities are potentially weak: at most resulting in a onetime allocation of money equaling five to ten percent of a commercialist's nine-month salary.

A less conservative, but still cautious analysis suggests a different material reward pattern. If we conduct a case-by-case analysis of the remaining commercialists and exclude entries under $20,000, it becomes clear that financial incentives are large relative to nine-month academic salaries. Six scientists earned at least $20,000 to $60,000. Ten scientists, approximately one-third of the commercialists, had commercial earnings that ranged from a minimum of $120,000 to a minimum of $270,000 during 2009. The commercial income of scientists in this category was therefore half of, equal to, or greater than their nine-month academic salaries.

In addition to these earnings, commercialists may also have equity, or an ownership interest in companies they have founded, licensed a patent to, or with whom they served as an officer or consultant. The post-interview questionnaire collected these data as they pertain to current ownership. Some scientists suggested that forming a company and selling it to a larger company is the best route to financial earnings. As one scientist told me when discussing the companies he had founded:

> I'm always involved in startup companies that are sold to big companies with two names. My goal is [to sell a startup company to] the really big one, to Proctor & Gamble. [49]

Indeed, one scientist in the study disclosed in the interview that he earned $40 million as a result of having ten percent ownership of a company he cofounded and sold for $400 million. The earnings from equity can be quite lucrative, particularly when one's company is absorbed by a larger existing corporation. Although we are unable to assess the actual values of scientists' ownership interests, we may at least determine the number of streams from which income may potentially flow. I present these figures in table 16. It is notable that five scientists did not complete this portion of the questionnaire, but readily discussed commercial practices. This could either reflect a perception that inquiry into one's finances is inappropriate or a concern that ownership disclosures taint one's professional integrity. If we assumed that these five scientists did not own equity in any company during the past year, we would make three conclusions. First, nine or just over one-quarter of the sample had no equity interests during 2009. Second, twenty-

Table 16: Companies in Which Equity is Held, Commercialists

# of companies	Scientists
0	4
1	12
2	7
3	1
4	1
10+	2
N=27	

three scientists, or about seventy-two percent of the commercialist subset of the sample, had equity in at least one company during the same period. Third, approximately one-third of the commercialists possessed equity in two or more companies.

Material benefits may not be influential, but even conservative estimates of commercial income suggest that their magnitude could be difficult to overlook in considering the pursuit of commercial endeavors. Moreover, recalling that on average, over half of each commercialist's patents formed the basis of at least one licensing agreement, it is difficult to conclude that such incentives are not learned and affirmed over time. If material benefits are influential, such persuasions are likely only disclosed in circumstances of extreme trust. In short, social desirability, scientific norms, and the data on commercial returns suggest that the limited disclosure of financial motivations in the subjective data does not fully represent the extent to which money encourages or sustains commercial involvement.

Protection

Some commercialists in the study, independent of cohort, stressed the notion of protection when explaining their initial entry into a commercial career path. Protection is a *circumstantial* attractor in which commercialization becomes an appealing choice due to the context of one’s work. Commercialists stress protection in two respects. The primary notion of protection reflects scientists’ concern for priority rights in discovery (Merton 1973). For these scientists, commercial turning points were triggered by a desire to claim priority rights of unforeseen commercializable discoveries. Merton (1973) demonstrated in great detail that concern for priorities in discovery, or the assertion of a claim to having made a discovery before one’s peers, is an integral part of the social relations between scientists. Claims to priority characterize the process by which scientists accumulate and protect intellectual property. “Ownership” of a discovery is actuated through demonstration that one is the first to have made it. Invention is no exception. Galileo, for example, fought to protect his claim to having invented the “geometric and military compass” (Merton 1973, p. 287).

The commercial turning points of scientists for whom protection played a role typically occurred during consulting work that resulted in an unpremeditated commercializable technology. In this context, priority rights attract scientists to commercialism due to opportunity costs such as misappropriated symbolic recognition or potential misuse of one’s work. Consider, for example, the following account of a commercialist who stressed protection when explaining his first patent.

> I started working with a company that was trying to solve a problem, but in fact, they really only had about an 85 percent solution. The other 15 percent was what I knew better than they did. Okay? I worked to develop this widget. To protect myself, I decided once I got this working, it was clearly superior to what they had, and I thought well okay, I should patent this, because it did have a couple of good ideas in it.

What would you be protecting yourself from?

> I thought it was a good idea. It was a technological advance and I just wanted to be sure that, you know, if they were relying on my knowledge, that I was protecting myself. So it wasn't so much about the money. It was more about the principle. [26]

Framed in such terms, protection functions as a symbolic counterpart to material benefit. Sustained commitment to traditionalism in this context would offer the disadvantageous circumstance of misallocated credit. Thus, by ensuring that credit – symbolic or material – is distributed fairly, protecting one's claim to priority attracts some to commercialization.

A variant notion of protection may also be invoked as a trigger of commercialization in less ethical situations involving deceit or dishonesty. During the emergence of the biotechnology industry, for example, one commercialist made a discovery that had obvious implications for applications in health. When I asked him about his first patent, he told the following story, which took place in during the 1970s when he was an assistant professor:

> There were some things that happened that led me to believe that the biotech area was going to be full of sharks and people who were willing to steal, including here on the faculty. A senior faculty member who was tangentially involved in [this field] came to me and specifically stated that he heard about [my discovery] and that he felt that it would be best if we sold this off as personal consulting to [PharmaCo] and that he would set it up, because he was a buddy with a guy who headed [PharmaCo] vaccines. Well, as soon as I heard that, I knew what that was. That was called theft, and so I just dropped anchor, that was it. As soon as a full professor at [this university] suggested stealing from

> it for a personal benefit, I knew my only ethical pathway was to get the patents free and clear, belonging to [the university] so that [it] could decide what to do with them. [49]

In this example, protection attracts the scientist to a commercial career path by offering symbolic cleanliness. A paradox exists, however. On the one hand, entering a commercial career path could taint the reputation of a scientist, particularly in a cultural context in which commercial practices are viewed as morally questionable. Without disclosure, on the other hand, one potentially fails to protect a discovery from the "dirtiness" of misappropriation, misuse, and professional misconduct. Formal commercialization of one's discovery therefore offers the legal and ethical safety of institutional protection, which may be viewed as advantageous relative to cultural sanctions or disapproval resulting from commercialization. The desire for protection thus nudges some scientists into commercial career paths by preserving symbolic rewards and cleanliness.

Societal impact

As we have already observed in the previous chapter, societal impact is viewed by commercialists as the most esteemed pursuit in academic science and a key basis for visibility. It is thus unsurprising that societal impact enters commercialists' accounts of the factors motivating them to commercialize their work. Societal impact is a type of attractor tied to *product aesthetic,* or the psychological appeal one finds in the products of their work. For the commercialists who entered science prior to the Bayh-Dole Act, the technical and practical utility of one's research is expressed as serendipitous societal impact.

You filed your first patent in 1982. Why was that something that you did?

> Well, because it was a unique invention and my colleague at the time said we've got something that's pretty fantastic, which we both agreed, and we should really patent it because it'll be good…

What were you hoping to accomplish in commercializing this work?

> Basically to make these reagents generally available. I mean we only published I think a grand total of two papers and a review describing this stuff. As a matter of fact I...just at lunch with my co-inventor yesterday, and we [were discussing that] the amount of recognition we got for this was so close to zero. [81]

> The first commercial endeavor was a patent that I filed with my postdoctoral advisor. And this was now over twenty years ago. This was on a very nice idea…for doing [very sensitive diagnostics]. And it was clear to us technically, that [the invention] was a great idea. It turned out we were about twenty years ahead of our time. [92]

Among these scientists, societal impact is framed in serendipitous terms: an unexpected discovery uncovered in the course of research or an old solution to an emergent or new societal problem. This presentation of societal impact as a motivation mirrors explanations of why scientists should commercialize their work that are found in the statements of federal funding agendas, university mission statements, and the Bayh-Dole Act itself. It also approximates the notion of altruism in that there is a sense of obligation to enable access to inventions that others may find beneficial for their own objectives. Finally, it suggests that invention is secondary to scientific discovery, not a motivation for it.

Tangibility

Scientists who began their appointments in the 1980s or later referenced a variant notion of societal impact that reflects a different product aesthetic: invention is framed as an intrinsically valuable objective, not an unanticipated outcome of basic scientific research. Consider this pattern in the following accounts and note the emphasis on the material form of one's discovery.

> I had worked hard for five years to get to the point where I could have…a mainstream product…I was hoping to go down into a Best Buy and buy a TV or some kind of display that had my technology in it. Maybe it didn't say it out on the box, but I would know that. And, I have seen the displays at [Electronics Inc's] headquarters. [63]

> The first project I worked on is one of the three most common [process] technologies used throughout the world, and these machines are sold all over the world, and I saw that happen and you know, I visit companies now...and they don't know that I had anything to do with it, and I love that....Here's these machines used all over the world and that started on my white board in my conference room. I just love telling my kids that. I mean they're proud of it. It's just cool because people found this useful…I've got over 200 publications, and it's very important for students to publish and things like that, but I don't need any more publications. Yes, it's great that people read about my ideas, but you know, it's not enough to me. [51]

The implicit, but key, attractor across these accounts is *tangibility*. The attraction of commercialization for these scientists is the ability to perceive their work as materially existent or capable of being realized beyond a discovery's status as mere truth. For example, when I asked one chemical engineer what led her to commercialize her work, she explicitly emphasized tangibility. Notice how, like the scientist quoted before her, tangibility separates traditional scientific and commercial rewards.

> I think two things. I think every scientist that has an innovative idea wants to see that idea brought to a level fruition that's satisfied and in some cases it is to see this sort of intellectual pursuit influence the field in general and that is satisfying. But there is a different kind of excitement and a different kind of satisfaction that comes from not only influencing your fields of study, but also influencing what is happening in the everyday world. That's something that can only happen if there's commercialization.

You said there's a different kind of feeling. What's different about it?

> I think it becomes more tangible. So tangibility is probably one of the words one would use. It becomes something that you can now explain to your grandmother or your kid. You see that do-hickey over there? That's because [of my work]...and I can actually say that. [44]

Another commercialist, just beginning his first company, expressed his motivation in similar terms.

> On a very personal level, I have relatives who suffer from pain as I'm sure most of us know people who suffer from chronic pain. I suppose if we could do something to bring new medications to market that would help my mother, my uncle, et cetera, I would have tremendous personal satisfaction from that. Did I ever think that would be possible? Not in my wildest dreams. Do I think it's possible as we sit here in April of 2010? I think it's still an incredible long-shot, but if it happened, I would love it. [74]

Tangibility drives the intrinsic value of commercialization perceived by commercialists who entered science after the Bayh-Dole Act. It explains why these cohorts of commercialists view commercialization as a legitimate end in itself, rather than an unanticipated consequence of their work. The rewards offered by tangibility, and thus its function as an attractor, include both the aesthetic value of materiality and a basis of distinction from traditional scientific work beyond published truths.

Circumstances of commercialization force scientists to confront a decision between traditionalist and commercialist science. At the intersection of these career paths, material benefits, protection, societal impact, and tangibility attract the commitment of scientists to commercialization as a result of the perceived benefits or advantages they offer. Whereas attractors are a component of the evaluative framework within which scientists select between commercialist and traditionalist commitments, facilitators are pre-existing or environmental conditions that ease scientists through the commercial turning point. For commercialists who entered science after 1980, for example, we observed that professional socialization under an established commercialist functions as a core mechanism by which scientists embrace commercial career trajectories. The accounts of commercialists who entered science before this time, suggest two facilitators unique to these scientists: social origins and unintentional entry.

Social Origins

Commercialists who trained before 1980 suggested longstanding dispositions to technical utility and framed their accounts of their first commercial acts laced with overtures to their non-scientific pasts. For some within this group, the desire to discover and create technological solutions to societal problems was framed as a personal disposition that influenced the selection of research fields and problems. For a minority, it was a rationale for founding companies or patenting research before the Bayh-Dole Act. For these scientists, such social origins that celebrate or value practical utility do not cause one to become a commercialist. They are social-psychological mechanisms that ease one's entry into a role. Consider, for example, a physicist now working in a department of electrical engineering. He began his explanation of why he commercializes his work by stressing a disposition to utility at an early age.

> My experience may be slightly unusual in that regard. So as a teenager, I became quite interested in science and electronics. In fact, I was fairly good at doing subjects like physics. It just appealed to me and I think that I had the right kind of intellectual apparatus to deal with it...I was doing a combination of playing about with electronics as a teenager and doing physics in high school and so on, and then when I went to university…When I looked around for somewhere to do graduate work…some places really turned me off. The guys who were more over into pure physics didn't really appeal to me, because I couldn't figure out why they were doing what they were doing… They didn't seem to me to have any connection to stuff that was particularly useful… [69]

Whereas some scientists framed early applied interests as factors motivating their career decisions all along, others tended to simply make sense of their present commercial activities in light of their pasts. More specifically, these commercialists suggest that their commercial activities were informed by values internalized as a result of their socioeconomic origins.

> I'm an old practical farmer out of Kentucky. That's where I want to be engaged. I don't want to give up my intellectual drive and ability to understand, but I want to be involved in [technology development]. [41]

> My first time I filed a patent, my senior professor told me that that was not the done thing, that you should not file patents, that isn't what academics did. But I didn't grow up in an academic society, I grew up in a farming society and I understood a couple things. The constitution gave the congress the right to set up patents, and I looked around and I said if I have something that's of value, how is it going to get out there? [49]

> I have always wanted to get something that would actually get used. Okay? I mean, I come from a working class family. It's always been sort of focused on, "What's the practical application?" Right? And so, almost all of my work, ultimately, even the most fundamental work that I do, I always start with what's a problem that science technology can solve? I tend to be driven more by solving problems rather than the basic science per se. [26]

It is noteworthy that the appeal to one's socioeconomic origins is found almost exclusively among commercialists who underwent professional socialization prior to Bayh-Dole. The explanation lies in the era in which they were trained, in that commercialization was not only unconventional but also shunned. In this respect, commercialists' appeals to an ever present disposition to utility either vindicate prior departures from scientific convention or legitimate a "suppressed self", permitting a deviant identity to "come out of the closet." By claiming a longstanding affinity to a practice now deemed legitimate by many in the community, individuals are able to place themselves into a position as moral entrepreneurs whose values are now celebrated. The accounts are also suggestive of the role played by non-scientific reference groups. That is, through other contexts of socialization, scientists are exposed to alternative motives for work.

Although it is difficult to obtain precise information about the influence of social origins on commercialist scientists, it is clear that the relationship is limited to senior cohorts of commercialist scientists. That is, as an attractor to commercialization, it is *operative* for the senior cohorts and *irrelevant* to the more recent cohorts of commercialists. Only one scientist who received his PhD. after 1980 claimed a similar influence, although he grew up in [Europe]. He explained what led him to commercialize his work:

> I grew up on the coast of [Finland]. You didn't go to the hardware store and buy the right equipment. You always did the right job with the wrong tool, so to speak. Everything was always improvised and fixed on the spot. Every grown person in my childhood was sort of a functioning engineer in some sense, somebody who builds the stuff, fixes stuff, and if they didn't, they would have a pretty nasty livelihood under those circumstances. They

were all farmers and fishermen…We were always fixing problems. My father was an engineer, but he was the worst ever when it came to sort of having tools. He always did things with the wrong tools, right? He wasn't an expert of anything, but he could do everything. [62]

The subtle or indirect role played by one's social origins makes it difficult to assign great salience to it as an explanation of why a scientist may commercialize their work, yet we need not discount or dismiss its influence (Peters and Fusfield 1982; Etzowitz 1983). Like other social mechanisms, social origins operate in conjunction with other influences or may only become operative under particular circumstances.

Unintentional entry

Scientists regularly framed the circumstances leading to their initial commercial experiences as unintentional or in terms of "a confluence of events." They suggested that their commercial involvement was either accidental or inadvertent. When framed in this manner, the catalyst of commercialization is suggested as beyond rather than within a scientist. The generic form of this theme entails the convergence of specific actors in space and time in a way that produces an opportunity structure for commercialization, but other environmental properties clearly factor into the onset of commercial activities, including the field and status of the scientist, the prestige of their university, and the state of scientific knowledge.

Again, this attractor to commercialization is *operative* among the scientists who received their PhD before 1980, but *irrelevant* to those entering science after this time. In fact, several within the senior group of scientists either formed their first company or applied for their first patent before the Bayh-Dole Act was passed. In addition to the influence of their expertise, their status as “pioneers” of commercialization is in part the result of their location at premier U.S. universities, earlier decisions to select specific fields of study, and the advance of knowledge within those fields. The exemplar of this circumstance of commercialization is found in the 1970s when the potential for applications and technologies resulting from developments in molecular biology were apparent to scientists in academe and industry.

What led you as a scientist to see commercialization as something that you should do?

> The decision to get involved came down to a telephone call in this office in about 1977. That’s the same year I made the discovery [that led to the Nobel Prize]. I got a call from…an alum who asked me to fly to San Francisco to consult on a biotechnology investment. I’d never talked to anyone in the private sector in my life in this context. So I was perfectly happy. I flew out and in walked [Jacobs] and [Huntsman] and this was the first investment in [BioTech Inc]. I listened to their story and I told [the alum], ‘I don’t know if you’re going to be able to make a buck out of this, but those guys are going to do what they just said [create specific technologies]…I think it’s totally possible to do and I think it’s technically within the reach of this group of people.’ They made the investment. I came back, started a conversation about biotech, met a very good investment company here and wanted to put together a group of scientists to talk about starting a company in this region. It sounded like fun…and we got engaged and invited a number of European scientists and [BioChem Inc] started in 1978. We were in the right place, it was a new technology, and no one understood it better than we did.

As I understand, this wasn’t on any claimed intellectual property. You didn’t file for your first patent until 1982.

> No, no. This was new technology that we anticipated that we could use for pharmaceuticals, energy, and intermediate chemicals, and we were going to do all three. It was the energy crisis. So we struck a lot of deals with large corporations…It was just, take this technology which was in the laboratory and make it a useful investment, grow our firm. [41]

Another scientist who formed two companies in the 1970s tells a similar story.

> Somebody knocked on the door and said 'I understand you're an expert on this. Can you help me with that?' So we invented at the time a device for measuring [a physiological process] and I have a couple of patents on that…The guy came in and we sat down in here, came up with the idea of how we can do this, and we got a couple of patents on that. We started the company because he had this urge to commercialize it. Then I got involved with another company at the same time that funded us. So in a way I got involved in two companies instantly with one guy walking in the door and solving a problem…He had gotten his PhD here maybe five years before and he asked his advisor 'who should I talk to about something like this?' and they told him go talk to this guy. So he came to me, sat in here, and within a few hours we came up with these things.

That sounds ad hoc.

> It is ad hoc…I mean I've had so many things like this happen over a lunch. I had a friend who was a neurologist, best neurosurgeon on campus. We had lunch one day with another guy who was an optics guy, and we filed four patents after that lunch, and that became a company. These things happen all the time. [73]

In these accounts and the others of which they are representative, the stage of scientific knowledge, the scientists' relevant expertise, their visibility within the scientific community, and the prestige of their institutions all precipitated the phone calls or events that triggered the initial commercial involvement.

Unintentional entry does not, however, necessarily preclude an existing desire to commercialize one's work. It is interesting to note that there is substantial overlap among the scientists who framed their involvement in this respect and those who stressed the practical aspects of their social origins. The scientists who were involved in commercialization prior to 1980 thus "ended up" as commercialists through a combination of external triggers and personal dispositions, yet they do not frame their transition in instrumental, active terms. Those who actuated an embrace of commercialization more recently, however, frame their transition as circumstantial while they exhibit intent. A chemist, for example, began the story of the founding of his company as follows:

> So that was a confluence of two people walking down the hallway in opposite directions. I mean…[*laughing*] it really was. I had been actively trying to transform my research program to one that was focusing more on the directions of renewable energy. So my work has long been in the area of nanotechnology…But I didn't quite see what I'll call the 'killer app', for what we were doing….And then suddenly one day down the hallway, comes this guy, [Rob James], who is now my collaborator in this company and he just stopped me and asked me if I knew anybody who could do electrochemical measurements. And I simply replied, 'Well we're doing them all the time. Let's talk.' [26]

A chemist at a different university shares a similar story.

> I was consulting for [Oil Co] and in between consulting visits, we had made this discovery and then we subsequently found out, on my next consulting visit, they said they were very interested in this material that somebody else had been working on. At the time, we told them well we've got a better way of doing this. It was a lucky confluence of events. We made the discovery, we were going to publish the paper anyway, then it was quite clear we not only had a patentable discovery but we already had a party that was interested in possibly licensing it [66].

The attribution of one's entry to commercialization to "lucky circumstances" functions similarly to the institutional norm of humility in science. The norm of humility leads scientists to place limits on their accomplishments, in essence claiming only limited credit for their actions (Merton 1973). Particularly for those who were socialized to traditional means of achievement, citing a confluence of events allows scientists to claim only limited "blame" for an action that is morally questionable by some. It conveys the sense of secondary consideration or unintended consequence, rather than primary motivation or a chief objective.

The analysis of attractors and facilitators of commercialization among the commercialists point to divergent patterns among the scientists trained before and after 1980. The general tendency of commercialists trained before the acceleration of commercialization is to present their initial entry into commercial career trajectories as indirect or not their central motivation. They frame their early experiences as resulting from the serendipity of research, the origins in which their lives began, or unintentional circumstances. They rarely present their transition to commercialism as sought out or motivated by the institutionalized financial goals that result from legislation. These patterns overlap somewhat with commercialists trained during the 1980s and 1990s, although there is a more pronounced identification with commercialization as an intrinsically valuable activity.

Traditionalists

Empirically, traditionalists constitute the “negative case” by which we may assess the influence of the commercially-oriented reward system on the transition to commercialist career pursuits. Apart from the influence of socialization previously considered, we are left to explain why, if commercially-oriented rewards currently predominate, traditionalists are not persuaded by commercial incentives. The findings we consider in this section indicate that explanations related to talent or differential opportunity structures fail to explain the lack of commercialization among the traditionalists. The discussion is organized around a consideration of social mechanisms that constrain identification with the commercial role. As we will see, traditionalists are exposed to the same incentives and opportunities as commercialists, but are untempted or “repelled” by commercialist pursuits. Five constraints are considered: talent, opportunity structure, indifference, career burdens, and goal incompatibility.

Talent

A basic hypothesis one might consider suggests that scientists do not commercialize their work because they are either not successful enough or lack the scientific expertise necessary to do so. If we examine the publications and honors of traditionalists, we find little evidence to support this claim. In table 17, I present the publication records of chemists and biologists in the study (research articles only). I delimit consideration to these fields to control for field specific differences in publication. A comparison of the number of research articles published suggests

Table 17: Number of Publications, by Commercial Orientation and Rank

	Chemists		Biologists	
	Commercialist	Traditionalist	Commercialist	Traditionalist
Total publications				
By first appointment	8.9	9.7	6.4	5.5
At promotion to associate	22.9	26.9	15.4	12
At promotion to full professor	42.3	38.7	32.4	27.1
N=	13	12	8	11

that, at each career stage, the commercialists and traditionalists publish at nearly the same rate. As a proxy for talent or expertise, publications therefore suggest little difference between commercialists and traditionalists. Furthermore, by virtue of their appointment at four of the premier universities in the world, the traditionalists in this sample hold positions that suggest a level of excellence rare within the scientific community. Among the twenty-five tenured traditionalists (four are assistant professors), six are fellows of the National Academy of Sciences, nine are fellows of the American Academy of Arts and Sciences, and eight of the scientists occupy endowed professorships. Coupled with their records of research, these honors – and the more numerous field specific awards received by all of the traditionalists – suggest there is little basis for the claim that traditionalists do not commercialize for lack of expertise.

Opportunity Structure

A second reason one might expect that traditionalists do not attempt to commercialize their work is that work contexts of these scientists are characterized by opportunity structures not conducive to commercialization. For example, in their study of structural characteristics that influence scientists' transition to commercialization, Stuart and Ding (2006) argue that commercialist co-workers, commercialist coauthors, eminence of commercialist coworkers, publication, and institutional prestige are key attributes of work contexts that encourage scientists to commercialize their work. Given the theoretic framework of the sample of this study, in particular the identification of commercially-prolific institutions, departments, and scientists and the selection of traditionalists who work in the same environments, it is difficult to conclude that these organizational characteristics explain why the traditionalists in this study do not commercialize their work.[3] Indeed, given the regularity of interaction, access to the same technology licensing resources, and to some extent, coauthorship between traditionalists and commercialists, one could argue that these traditionalists have a greater opportunity to "learn" commercialization than most other scientists in the United States.

Furthermore, for the majority of the traditionalists it is not the case that the scientists' research foci are irrelevant to technological or commercial concerns. Only two scientists explicitly claimed that no ties exist between their research and the potential for technology transfer. The career of one of these scientists suggests that in some cases the perception may be selective. For example, earlier in the interview the scientist noted industrial scientists' citation of his work. Additionally, he noted having consulted for four companies over the course of his career. The purpose of this illustration is not to suggest that most scientists could commercialize their work but for lack of desire. Rather, the point is that among some scientists, commitment to the study of fundamental problems renders any consideration of practical applications cognitively unrealistic.

Indifference

Indeed, adherence to the institutional goal of science is the basis by which traditionalists are not persuaded by the commercial incentives and opportunities that surround them. As a result, some traditionalists attribute the lack of commercial activity to a lack of interest.

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[3] Stuart & Ding (2006) do, however, provide a useful model by which to understand the role work characteristics play in the temporal and institutional diffusion of commercial practices in academic science.

Consider, for instance, a selection of traditionalists' responses when asked about the extent to which they have considered or sought to apply their work commercially.

> I haven't. I'm not interested in it. [39]

> In some areas where I've worked there is definite industrial interest. Many companies make membranes for things like desalination or purifying protein solutions or other kinds of applications and so various times I've been a consultant with those companies helping understand what their device was doing and sometimes how to improve it, but that's as close as I've gotten.

Given these connections, does commercialization not attract you?

> It just doesn't interest me that much. The commercial side of things doesn't interest me that much. [42]

Indifference, in this respect, expresses the norm of disinterest (Merton 1973), in that the scientists' accounts implicitly reflect commitment to selecting problems motivated by scientific rather than nonscientific concerns. More generally, this reveals traditionalists' commitment to the institutional goals of science in that identification with the goal of advancing knowledge is unperturbed by potential career paths that depart from that objective.

Career burden

Other traditionalists have developed a definition of commercialization as a career burden. For these scientists, commercialization is viewed as a threat to two important resources in a career: time and reputation.

As a person who does simulations, I could easily develop a package, copyright, and then sell, and there are folks in computational chemistry who do that, who have you know, licensed their software…I really don't want to deal with it. I've thought about starting up a software package based on things that we do, but I really don't want to have to deal with customers and support and you know, advertising and so forth. [47]

I've never had the time to even think about it. I mean I've always been so struggling to keep up…I got so far behind on publishing work that we've done that I think that was also one reason why...I found it so hard just to keep up with my own area and my own line and my students and post docs, that I've really never had a chance to think about that. [81]

It's never occurred to me to do that, ever. You have to convince people that what you're doing is really very promising. Most of us are trained, as scientists, to be very skeptical about things, you know you look at your own results very critically and you interpret them as conservatively as you can, so you don't get caught out on a limb and have to be embarrassed by taking it back and so on. When you go out to get a millionaire and go back to your small startup company, you can't say well you know it may not amount to anything…The last thing you want to have happen is you get caught having made a claim that's not justified, it can be the end of your career. You can keep doing science but people take you less seriously if you've ever done that. [32]

Traditionalists frame commercialization as an undue burden, the weight of which overshadows any potential benefits that one might derive. At a minimum, commercialization in this respect is little different from academic dirty work that distracts traditionalists from commitments they regard as of greater importance. At a more opposite extreme, and in a pattern opposite to that of commercialists, traditionalists perceive commercial activities as a potential threat to one's reputation. Such interpretations prevent commitment with the commercial role, because both construct commercialization as of no benefit to one's career.

Goal incompatibility

A third group, traditionalists who noted some form of interaction with industry, show that commercial incentives operate, but adherence to basic research prevails – often due to perceived inconsistencies in the goals or interests of the scientists and representatives of industry with whom they met. With few exceptions, these traditionalists characterize their interactions with industry as brief and uninteresting. Consequently, the scientists developed definitions of commercialization unfavorable to identification with a commercial career path. The following scientists are traditionalists trained prior to 1980. Like the traditionalists trained more recently, these scientists similarly have a negative reaction to exposure to commercialist culture.

> Once in a while I have been on committees, you know, external advisory committees to some industrial lab. I do that a lot for universities but I stopped doing it for companies.

Why did you stop?

> It was just a waste of time for me. It wasn't interesting. [43]

To what extent have you sought or considered to apply your work commercially over the course of your career?

> Very little, if any. I've met with a couple of companies…a consulting type of thing…I try and explain to them what I know and what they can think about, but I don't think they're excited about it. And I don't think I'm excited about it. [52]

> I've served on a couple of drug company boards. I didn't particularly like it.

Why not?

> I thought they were more interested in what they could patent then what they could find out. [50]

The regard for industrial science as uninteresting is derived from the fact that in the eyes of the traditionalist, the solutions are unchallenging, the problems are short-term, and the goals are unrelated to knowledge. As one scientist put it, "it's just boring." [91] For more recent cohorts of traditionalists, the operation of commercially-oriented rewards are neutralized when exposed to negative circumstances during training. For those whose exposure occurs later in the career, commitment to traditional means of achievement appears to undermine the influence of commercial incentives.

Even when there are relatively direct connections between a scientist's research foci and a company's interests, traditionalists appear willing to share tacit knowledge related to a problem but nevertheless unmotivated to pursue commercialization.

> I had a brief involvement with [Lighting Co] and they were interested in something close to what I was doing…From the very beginning it was a disaster and so I have not been interested in any sort of arrangement with any company since then. [46]

> Actually there's only been one time that I really thought about it very seriously at all. We had done some experiments that got a bit of press and somebody from a company approached me. I and a post-doc went over to this company and visited with them. It was clear the piece of our work they had read about wouldn't work [for their purposes]. We saw that from the get-go. There was another thing we had done that might work and so we showed them that approach. They got really excited about this and they were saying…if you could [do this] the world would beat a path to your door…And so this company got really excited…I could have tried to build a relationship with that company and put a lot of my effort into tuning up this process…I just didn't want to do that. [25]

Only one scientist among the traditionalists has a sustained consulting relationship with a company, for which he received between $80,000 and $90,000 a year in fees. Note in his account that despite the advisory role he played and the obvious connection between his research and technological or commercial opportunities, he nevertheless remains uninterested in conducting any form of commercially-oriented research.

Have you ever considered or sought to apply your work commercially at any point in your career?

> No.

But you do have some ties such as your service on the board of [*Pharmaceutical Inc*].

> Absolutely. I was a consultant for a company called [LifeSci Inc] when it first started and I actually made a lot of money on that, they gave me a stock option, I did well by it, and I advised them on their practical goals. But I didn't feel that I wanted to do that in my own lab…The entire world's supply of [a virus] vaccine is made [following a process I developed] and I wouldn't have tried that in my own lab, but to see it happen with my advice was exhilarating. [87]

Traditionalists do not view commercial opportunities as worthwhile investments of time, and neither interesting nor attractive. Although they work in areas of science that have obvious connections to commercial technologies and have similar opportunity structures as their commercialist peers, adherence to the goals of science preclude identification with the commercial role. In general, traditionalists appear to be repelled by, rather than attracted to, commercial involvement. Any flirtation with the notion of commercial involvement is neutralized with the perception of contradictory interests between university and industrial science. In short, for traditionalists, opportunities to commercialize research lead to an affirmation of the goal of advancing knowledge and the traditional means of achievement.

Summary

This chapter placed commercialization in career context. In essence, it asked: Why do some scientists become commercialists, while others do not? To address this question, I examined the temporal basis of commercialization and the factors scientists stressed in explaining why they did or did not pursue commercialist career pursuits. In their totality, the scientists' accounts encompass a set of social mechanisms that facilitate or constrain identification with commercialist career pursuits. In table 18, I begin to summarize these findings.

Table 18: Influence of Socialization on Transition to Commercialization

	Doctorate before 1980		Doctorate 1980 or later	
	Traditionalists	Commercialists	Traditionalists	Commercialists
Institutional exposure to commercialization	Low	Low	High	High
Immediate exposure during socialization	Low	Low	Moderate	High
Nature of socialization	Ideological closure	Ideological closure	Ideological pluralism	Ideological pluralism
View of commercialization	Dirty	Dirty	Career threat/ Distraction	Elemental
Career pattern at turning point	-	Establishment	-	Initiation/ Escalation

The majority of the scientists in the study trained at universities with elements of commercialist culture prior to the Bayh-Dole Act of 1980, but those who underwent their training before this legislation was passed were not exposed to or aware of commercial incentives during their graduate and postdoctoral training. As indicated by the growth of technology transfer offices presented in table 13 and the analyses of university assigned patents considered in chapter 2, commercial culture escalated during the 1960s and 1970s. Nevertheless, it could not be considered predominant or established in this era. This is most evident in the consistency of commercialist and traditionalist accounts of their professional training, which is best understood as ideologically closed. The prevailing culture was embodied by an exclusive emphasis on one model of professional behavior: commitment to fundamental research. This was the case whether one was trained as a scientist or an engineer and whether one trained at an institution thought of as extremely pure (such as the University of Chicago) or one characterized by commercial precedent (such as the University of Wisconsin). Thus, despite limited or nascent elements of commercial culture during this time, scientists who trained during this period viewed commercialization as dirty, immoral, or for the untalented. How powerful was the influence of this perception? Most of the commercialists from these cohorts initiated their first commercial practices only upon entering a career stage of establishment, brought about by the achievement of tenure. Socialization thus operated as a constraint to identification with commercialist pursuits for both commercialists and traditionalists.

The patterns and consequences of socialization among scientists who completed doctoral training in 1980 and afterward depart from their elder peers. Institutionalization of commercial culture, accelerating in the wake of the Bayh-Dole Act, was clearly visible at the universities where these scientists trained – even from the perspective of labs run by traditionalist advisors. A moderate proportion of traditionalists received doctoral or postdoctoral training from commercialists, whereas the majority of commercialists trained under already established commercialists. In contrast to prior eras of science, training after 1980 is characterized by ideological pluralism, in that science no longer exhibited a singular devotion to fundamental research. Thus, scientists in training could observe models of traditionalist or commercialist science in departments or colleges beyond whatever mode was practiced within the lab of one's doctoral training. By virtue of its undoubted presence, rather than developing a view of commercialization as dirty, traditionalists interpreted it as a career distraction or a potential threat to their career goals. In contrast, by virtue of their proximity to commercialist culture, would-be commercialists viewed commercialization as elemental to the scientific life and the meaning of a scientific career. In this view, commercialization is as integral and ordinary a practice as experimentation. As a result, socialization enabled rather than constrained identification with commercialist pursuits among would-be commercialists who began their careers after 1980. As a consequence, initiation of the turning point toward commercialist career trajectories frequently occurred with the initiation of the career itself (during doctoral or postdoctoral training) or as it escalated (before tenure).

The interviews revealed four other social mechanisms in underlying identification with the commercial role, which I present in table 19. Four basic resources produced by commercialization attract scientists to the commercial role. Independent of the era in which they entered science, commercialists noted that protection attracted them to commercialization in circumstances such as industrial consulting or in the event of discoveries with serendipitous technological implications. In these contexts, commercialization becomes appealing because it provides the means by which one may legally claim priority rights of discovery. Consistent with commercialists' conceptions of intraprofessional status indicated in chapter 3, we see that societal benefit enters into the attraction of commercialization, but its meaning as an attractor varies according to when scientists were trained. This difference is derived from an aesthetic of invention, or the ways in which commercialists identify with their technologies. For scientists

Table 19: Social Mechanisms of the Transition to Commercialization

	Doctorate before 1980	Doctorate 1980 or later
Attractors (by dimension)		
Circumstantial mode	Protection	Protection
Product aesthetic	Societal benefits	Tangibility
Material benefits	Dismissed	Legitimate
Facilitators		
Socialization	Constraint	Key factor
Social origins	Operative	Irrelevant
Unintentional entry	Operative	Irrelevant

who received their doctorate before 1980, societal benefit is a legitimate basis of status, but it is one achieved serendipitously and not an objective in its own right. These scientists framed their initial technologies as accidental discoveries, the fruit of well-established and elaborated research agendas. By contrast, the more recent cohorts of commercialists also touched on the notion of societal impact, but did so predominately in terms of tangibility, or the appeal of seeing one's self in the products of one's labor. Consistent with the early career stages at which their commercial trajectories unfolded, these scientists view tangibility an independently worthy objective to pursue in a scientific career, the acceptability of which is predicated on the prevalence of commercialization and its elemental status in their training.

Material benefits also attract scientists to commercialization, but like societal benefits, differs according to the context of socialization. Given their initial embrace of commercialization as dirty, a characterization that suggests mixing money and science taints the latter's integrity, scientists who trained in the 1960s and 1970s acknowledge the attraction of material benefits such as personal, laboratory, or institutional revenue returns, but downplay or dismiss the centrality of money as an attractor. Commercialists who entered science since 1980, by contrast, see making money from science as a taken for granted facet of the scientific life. Although they frame personal profit as secondary to societal benefits, these scientists nevertheless construct personal profits as legitimate aspects of and attractors to commercialization.

The differential ease with which societal and material benefits attract more would-be commercialists to the commercial role is clearly a function of socialization. More recent cohorts of commercialists developed a view of commercialization as legitimate early on in their careers and thus money and the status associated with societal impact are natural attractors of commercial pursuits. Earlier cohorts of commercialists, as a result of socialization that constrained identification with commercial pursuits, framed their role shift as facilitated by social origins and unintentional entry. The interpretation of social origins as facilitating entry into the commercial role suggests that the institution of science “came around” to these scientists’ commercial orientation. In this case, the suggestion is that predispositions to commercialization were restrained by virtue of the dominant culture of fundamental research. Other scientists who trained before the 1980s suggested that commercialization was the result of a confluence of events and framed the social circumstances prompting commercial trajectories as the product of random encounters, phone calls, and office visits. Social origins and unintentional entry facilitate identification with the commercial role by shifting the catalyst of this role shift from internal motivation to external circumstance, thereby permitting the scientists to dispose of the view of commercialization as dirty learned during socialization.

In the traditionalists’ accounts of their consideration of commercialization, the chapter reviewed six factors that potentially constrain the pursuit of a commercialist career (see table 20). Although opportunity structures such as relevant research foci, commercialist culture, and technology transfer offices undoubtedly influence the ease with which a scientist could pursue commercial trajectories, neither they nor scientific talent are able to explain why traditionalists in the study are not influenced by commercial incentives. All the scientists in this study are located within elite departments and are characterized by only minimal differences in research productivity and citation – a distinction surfacing only after conferral of tenure. If both groups of scientists inhabit the same commercially-intensive organizational environment and are generally of equivalent talent, what factors moderate the influence of commercial incentives? Traditionalists framed their aversion to commercialization in three ways, suggestive of the overall influence of socialization: as an activity to which they are indifferent, a burden or hazard

Table 20: Influence of Constraints on the Transition to Commercialization

Constraints	Influential?
Opportunity structure	-
Talent	-
Socialization	+
Indifference	+
Career burden	+
Goal incompatibility	+

in one's career, or as a goal incompatible with their view of science and scientific careers. Each of these interpretations is the product of socialization to the institutional goals of science, but they reflect specific iterations of commitment to fundamental research that are manifest in the context of a commercial reward system. The most benign or neutral position is found among traditionalists, who despite clear technology transfer opportunities, suggest they are simply indifferent or uninterested in such pursuits. Other traditionalists, based on observations from their doctoral training or their current peers, view commercialization as a pursuit that threatens their ability to fulfill the goal of advancing knowledge, either because commercialization consumes one's time or because it is perceived as a risky endeavor on which to stake one's scientific integrity. Finally, some traditionalists, particularly those who at some point in their career served as industrial consultants or had some exposure to commercial arrangements, interpret commercial trajectories as incompatible with their role and goals as scientists. Interpretations that construct commercialization as uninteresting, a burden, or incompatible with career goals constrain identification with commercial trajectories. In each view, the commercial career path is disadvantageous and unappealing.

Overall, this chapter suggests that variance in the context of professional socialization leads to fundamentally distinctive commitments among the scientists. For traditionalists, socialization fosters conformity to the goal of advancing knowledge such that opportunities to engage in behaviors that depart from this goal either affirm their commitment to science or are dismissed outright. Commercialists' adherence to this ethos, by contrast, is either undermined after their careers have been established (in the case of the senior commercialists) or less developed due to alternative commitments in early stages of the career (among the commercialists in earlier career stages). To more closely consider the social-psychological basis by which conformity and departure from convention occurs, I now consider patterns of identity work in which the scientists engage to construct and affirm a desired professional self.

CHAPTER 5

IDENTITY WORK AND THE NEW REWARD SYSTEM

In the last chapter, I examined why scientists embrace commercialist or traditionalist roles. This chapter develops an explanation of *how*, by examining new forms of social-psychological identity work in science that have emerged in response to the commercially-oriented reward system. Perceived dirtiness of commercialization creates a challenge for self-identity regardless of whether one embraces or eschews the commercial role. For commercialists, the challenge stems from the fact that the very role with which they identify carries a pervasive stigma. For traditionalists, self-identity is challenged by virtue of a mere alternative professional role and its potential impact on perceptions of integrity of the academic scientist.

Identity work refers to individuals' attempts to maintain a coherent sense of self-identity in the context of shifting experiences (Giddens 1991; Alvesson and Willmott 2002). As Giddens (1991) points out, maintaining a coherent self-identity is particularly challenging as a result of the decreasing influence of tradition in society, because changes and disruptions in patterns of events destabilize basic ideas about the self and the relationship between the individual and society. Change-oriented situations in science arguably lead to increased identity work in which both commercialists and traditionalists develop interpretive schemes and concomitant self-identities to make sense of their place in a changing academic profession and to thereby achieve their goals.

The chapter's analytic objective is prompted by two patterns in the data that were revealed in chapters three and four. In chapter three, we found that traditionalists view commercialization as "dirty work." How commercialists construct and enact a professional identity in a role transition that others view as undesirable is problematic, for one's reputation and visibility within the scientific community may carry a stigma of moral taint. In chapter four, we observed senior cohorts of commercialists socialized to view commercialization in similarly negative terms. In this respect, the role transition is problematic for the self: guilt or shame associated with the performance of dirty work must be overcome to sustain both a positive self-identity and persistence in one's role.

What is more, *all* scientists encounter *ongoing* reminders of the questionable desirability of commercialization in a broad stream of accounts that are often highly-publicized, such as: official statements on the challenges that commercialization poses to research integrity (American Association of Medical Colleges 2002; Association of American Universities 2001; U.S. Department of Health and Human Services 2009); scholarship revealing instances of commercially-related research that has endangered human well-being (Washburn 2005; Skloot 2010); publications in prestigious scientific journals that suggest commercialization constrains communication (cf. Blumenthal et. al 1996) and which produce research findings favorable to industrial sponsors (cf. Bekelman et. al 2003); congressional probes of commercially-related research misconduct (Grassley 2008); and criticisms of close ties between universities and industry made by respected figures in the academic community (Bok 2003; Krimsky 2003). These voices, not to mention similar accounts in major newspapers and trade periodicals, such as *The Chronicle of Higher Education*, merely highlight a chorus whose refrain implies commercialization is, or often can be, a dirty, undesirable practice. For commercialists, these circumstances create a paradox, the essence of which is captured in the following perspectives of two biologists:

> [Commercialization] was unthinkable to me when I was young. I never really thought about it when I was young. In fact, I went through a very long period of time where even though I was offered money to work with companies, I mean research money in my lab as well as consulting money, *I didn't take any money into my lab because I didn't want it to be contaminated by any kind of industrial money*. [82]

> In those days I was trying to convince people [a physiological process] had something to do with cancer so we developed a test system and kept on improving it and I never patented it. I just gave it away. Thousands of companies in the world now use it and I just sent it free to everybody. But nowadays I would have patented it. *In those days I was trying to convince other scientists I was right and I thought it would be a conflict of interest because I was making money out of all that*. [83]

The chapter is organized in three sections. In the first section, I will identify four legitimation techniques that commercialists employ to present themselves in a positive light. I draw upon research in the study of deviance, social psychology, and work to inform the patterns of legitimation considered. In the second section, I will examine four techniques of disidentification employed by traditionalists to affirm the purity of their identity. In this section, I will draw on theories of identity to formulate a typology of such techniques. In contrast to the previous chapters, which isolated data analysis to specific subsets of interview questions, both sections of this chapter draw from various sections of the interview because identity work techniques surfaced throughout the interview protocol. In each section, I note the interview context of responses so that the patterns may be fully understood. Finally, in the conclusion, I will consider the implications of identity work for scientists, and of dirty work for the academic profession.

Techniques of Legitimation

The conceptual and theoretic tools that motivate this section are derived from previous research in social psychology, deviance, and the sociology of work which suggests that one must subject the reasons and meanings of commercialization to analysis beyond their function as motivating factors. As Mills (1940, p. 904) explained, "The differing reasons [people] give for their actions are not themselves without reasons." In an influential article, Sykes and Matza (1957) built on this insight and argued that allegiance to a set of norms oppositional to conventional norms requires individuals to develop techniques of neutralization that rationalize and legitimate inconsistent behavior. Neutralization techniques, they argue, seek to dissolve views of actors' motivations such that one may preserve a positive self-image despite their involvement in deviant behavior (Sykes and Matza 1957). Borrowing heavily from neutralization theory, Scott and Lyman (1968) incorporated these techniques into their formulation of the sociology of accounts, which they describe as socially approved vocabularies that serve as explanatory mechanisms for deviance. Drawing upon these overlapping schemes, numerous studies in the sociology of work have demonstrated how workers secure a positive identity in the context of morally questionable work (e.g., Thompson 1991; Thompson and Herred 1992; Ashforth and Kreiner 1999) and, less commonly, how professionals account for misdeeds (Pogrebin et. al 1992).

In asking scientists about factors motivating commercial involvement and their views of commercial practices in academic science, I found that commercialists engage in four practices that seek to legitimate commercialization: reframing, role distance, social comparison, and professionalism. Each practice, through varying means, seeks to neutralize imputations that commercialization is harmful and permits a positive presentation of self.

Reframing

Occupational ideologies are action-oriented beliefs held by members of an occupation to make sense of their work (Trice 1993). As coherent belief systems, occupational ideologies specify role expectations and the nature of the relationship between members of a profession and society. According to Ashforth and Kreiner (1999), workers will engage in self-serving ideological techniques when work is characterized by conflicting interpretations, such as when segments of the scientific profession view commercialization as contaminating while others view it as esteemed. Ideological techniques have their greatest impact on securing internal legitimacy, because by sharing beliefs within an occupation, members persuade others to identify with the role (Ashforth and Kreiner 1999). Learning to legitimate commercialization thus plays a critical role in a scientist's socialization to, and persistence in, commercial involvement.

I begin by depicting the experiences of scientists commercializing their work for the first time and demonstrate how new commercialists learn from others how to legitimate their role through the process of *reframing*, whereby morally questionable practices are imbued with positive meaning. On the one hand, reframing is necessary simply because the meaning of a role always differs from the outside looking in; hence the necessity of normalizing a work setting when one enters a new career path (Van Maanen 1977). Thus, even a scientist socialized to a favorable view of commercialization will ultimately reframe its meaning because, prior to transitioning to the role, occupational myths belie what one learns through experience. On the other hand, reframing is necessary because the negative stigma must be overcome in order to embrace the role. The first scientist considered is a chemist at a public university. During the late

1990s, he completed his graduate and post-doctoral training at Harvard and the University of California Berkeley, respectively. He began his appointment in 2001 and was promoted to associate professor in 2007. At the time of the interview, the scientist told me that “literally, we are forming [the company] as we speak.” In describing why he has started to embrace commercial practices, he explained:

> Originally I didn’t want to have anything to do with starting a company, right? Because I’ve seen other colleagues, I’ve seen the drain on their time…and it’s always again in competition with other kinds of things. [But now,] it’s something new and I do think that over time this will really help me as a scientist for sure. A lot of people who have done this say that that’s the case. I used to work for [Arthur Copes] who started many, many companies and so I went to talk to him about this and I said I’ve been approached about this and what do you think? I’ve always thought I would never do this and he’s like maybe it’s time, and sort of talked to me about the things he’d learned through the years and why he thinks it made his group better to run. I mean I’m certainly not doing it for the money. If we make big cash that’s great, but I’m not naïve enough to think that especially your first time running a company when I don’t know what I’m doing that we’re going to be successful. It’s just kind of an interesting new dimension to add to the scientific career.

Why is this something you thought you would never do, or were opposed to before?

> Just that I’d never want to do that or something like that. There’s some close colleagues I’ve had over the years where I’ve seen them do this kind of thing and you always underestimate the amount of time it’s going to take and the complexity of this task

> because it sounds simple. It sounds like I'm just going to run some science in this company and it's going to be the same as kind of running my group here, so that's not very hard for me now. But it's not like that. I'm already discovering that. There's all these other weird things that you have to do, that we have no training for, and we're not very good at. I've seen that really consume a majority of their time in some cases and, that, I'm not willing to do. I have this job, I like this job most days and so I definitely want to keep that as my primary focus and so that puts an upper limit on how much I'm going to allow the company, this company, any company, to put demands on me…The key is to keep it something where it's reversible, I can pull out anytime I want. There are no issues with that really, or at least I have to delude myself into thinking so. I'm going to have to explore that as I go and, yeah, I'm worried about the time commitment because we already don't have any time, so it makes it worse…So if I shift some things around, there is a little leeway in our schedule where I can make a little bit of time and if it tries to consume more, I'll just say no. [86]

In the year preceding the interview, by practice and outlook, the scientist could be considered a traditionalist. Indeed, his view of commercialization as a threat to role commitment is indicative of the views of traditionalists illustrated in chapter three. A turning point occurs by reframing the meaning of commercialization from a threat to an enhancement. Facilitated by the guidance of a colleague, the scientist learns a meaning of commercialization opposite to his prior conviction: "Commercialization doesn't threaten your time, it makes your group run better," the frame implies. Commercialization is thus more palatable, if not positive, when it is framed in terms of an efficient rather inefficient use of time. His account suggests a shifting career perspective, although he is not yet fully relieved of a traditional conception of a scientific career: he retains an exit strategy from commercialization.

A similar pattern characterizes the experience of another chemist currently forming his first company. This scientist is a chemist at a private university who completed his PhD. at the California Institute of Technology, followed by a post-doctoral position at the Massachusetts Institute of Technology. In 2005, six years after beginning his current appointment, he was promoted to associate professor. He describes himself as "first and foremost a basic scientist interested in fundamental questions." During the year preceding the interview, in collaboration with a colleague and a doctoral student, he filed for and received his first patent, and formed his first company. He describes the events that led to a commercial trajectory.

> I've been here about ten years now and prior to this past year, I never had any intention really of patenting anything. What triggered us down this pathway, if you will, is a company got in touch with us after reading one of our publications and was very much interested in the work and one thing led to another and it made sense to patent the work. At that point I started to have to worry about, at what point can we go out and start to share this information in the public domain? I will be honest, it bothers me…I think the way it was put to me by my colleague was very influential. He said, 'From the standpoint of you as an educator, you as an advisor, you will learn things in going through the process of starting a company that make you a better advisor.' That was really the selling point for me. I really struggled with this. Is this something I really want to do? And when it was put to me in those terms, that I think I'll come out of this whole process and be a

> better advisor for it, then I was convinced it was the right move for me…I wrestle with this a lot and I still do even though we've obviously now taken the plunge so to speak…

Do you think this would have ever happened had somebody not approached you after your paper?

> For me personally, yeah, I think it probably wouldn't have happened. I think I would have daydreamed about the possibilities. I would say...look, I know I've got something exciting here, but I'm just not sure I would have been motivated enough to go off and do it on my own. I think the company just kind of tipped it in that direction. [74]

The ambivalence and apprehension that color the scientist's account of his transition are derived from a dissonance between a previously envisioned and present self. Like the account of the prior scientist, the chemist's concerns are quelled by infusing aspects of commercialization considered to be problematic with positive meaning. In this case, the scientist learns to reframe commercialization as a practice that will enhance the ability to train and guide graduate students. The need to learn how to legitimate the new role is particularly essential given the extent to which a self-identity departs from what was forged in professional socialization. The scientist describes himself as having trained in an era in which there was a "huge push" for academics to develop companies.

> [My doctoral advisor] and I would talk about these things and we'd kind of hold our nose up and say...that's not our job. That's not our role in this job. And this is all about sort of mountain-climbing and ego-boosting…My sense coming out of grad school was, this is not what academics is all about. We shouldn't be doing this kind of thing.

Why not?

> I just think that it's taking us away from our primary mission. Our primary mission is to be teaching and worried more about that and less about financial reward, less about the private sector, the application side. I see the commercialization much more as technology and much less about hypothesis-driven science and again the academic in me has always been more driven by the questions of basic research…I could be accused of being hypocritical because I was pretty vocal about this ten years ago, just like my advisor was. 'We're here to teach and do academic research and our job is not to start companies.' And I used to say things like, 'when my day job gets sufficiently easy for me, I'll think about [commercialization].' I was very facetious. As if this job would ever get easy. [47]

As a result of reframing, expectations for the future begin to take new shape.

> The idea that I would understand anything about human physiology was so foreign to me a few years ago and now I see myself getting more and more interested in it and I think that there are some really exciting opportunities and as I learn more about it, I discover that there are even more problems for me as a basic scientist – a basic scientist who can design and make molecules.

Now that this is starting to materialize, what have your thoughts been about financial incentives?

> I can't deny not knowing these people well, but people like the [Stokes] of the world... [Stokes] was an outstanding [academic] chemist…[who] started a company right at the beginning of the craze of combinatorial chemistry, made $30 or $40 million in personal wealth and actually left his job and builds airplanes in his garage now. Do I see myself

> having a career like that? As we sit here today at the age of 40, no. When I daydream about these kinds of things I relish in the idea that I might have enough of a revenue stream coming in to support my research that I don't have to worry about the vagaries of the government funding system. Although I do think writing NIH grants is good for the soul, I'd rather not be beholden to it. I'd rather be able to write my own checks for my research. That would be terrific. My wife and I do all right. We live in a very expensive part of the world, but we both work and we have a nice life. We don't live in a rich house and we certainly don't have a lifestyle that requires a lot of money. Look at my office and how Spartan it is. This is how my life is. This is the person I am. I don't think having thirty million in my bank account would probably change me in any way. A few more nice dinners out, but I don't think I'd drive a Tesla or anything of the sort. People laugh when I say to them that really my goal would be to make enough money that I wouldn't have to write another research grant. There is some truth to that. I think the other thing I would love to be able to do is to give money philanthropically. That to me would be wonderful. To have that kind of money to be able to help out a university like [this one], to give money to my alma maters, which I love dearly…I would love to be able to give lots of money to those departments to fund graduate fellowships or undergraduate fellowships. That would be personally extremely rewarding if I someday had that kind of money. [47]

The scientist shifts from being "first and foremost a basic scientist" to one who can "design and make molecules." The addition of invention to the role set of the scientist catalyzes new dreams for a career future that depart from the scientific goal of advancing knowledge. Future goals entail solving societal problems, but also lifestyle construction. Financial incentives enter fantasies of the future and lead in many directions, including professional autonomy, philanthropy, leisure, and material possessions.

Patterns of ambivalence and learning to legitimate are also found in the accounts of commercialists whose transition to commercially-oriented careers occurred in the past. For example, a scientist at a public university discusses the formation of his first company when asked about the goals of his commercial practices. Notice in the account the scientist's initial aversion to embracing financial incentives and the pattern of reframing.

> One thing I learned from the first company, which was interesting. I did learn something from that first little company because we met with [another scientist] and he sat down with me and [my collaborator]. 'So why do you want to start a company?' he asks. This is one of those fun stories. So [my collaborator] goes, 'Well, we're really interested in having another vehicle for doing research' and then I basically said something similar. So now the guy looks at us and he goes, 'If nobody in this company is doing it to make money, it's probably not going to be successful.' And at that time I was still struggling with that sort of moral issue of admitting to myself, much less others – I hadn't done that either – that a motive for making a company was to make money and I wasn't there at that time. I was...even in my own mind, 'Oh. No, I'm going to do research and it's just going to be in a company' and I really hadn't thought it through thoroughly because if I would have, I would have thought that was dumb. By the time I started [my second company] that thought had matured a little bit in me and so I now was interested in making money. It actually was a pretty straight-forward calculus. This invention was a

big hit, like a home run slammed way out of the park and made a ton of money for [the company], like billions.

What was the logic behind having a company to do research?

There was no logic behind that really. It was just because I wasn't willing to admit to myself that money is not what I...you should start a company for.

So it was a rationalization?

I think so. [34]

Like the other chemists, the scientist's account of his first company reflects a process by which an existing view of the meaning of commercialization requires transformation. Constrained by his adherence to traditional scientific norms, the scientist had unsuccessfully attempted to enact an ideology of commercialization that could be consonant with traditional science. The evidence of the outcome of reframing may be observed in the meaning the scientist now assigns to commercialization. For example, consider a comment made later in the interview regarding technology licensing offices:

> [They are] doing their job of going around and seeing what uncollected diamonds are lying on the floor. If there's a bunch of uncollected diamonds lying on the floor that someone is going to vacuum up and they'll be in the landfill, why not go pick them up? So really to me, commercialization is just a resource allocation issue by the university and the tech transfer office. [34]

Notice the extreme difference in the meaning surrounding money, as it shifts from a questionable, illegitimate motive for action to a simple issue of resource allocation, without which opportunity costs would manifest.

At this point in the chapter we have heard from commercialists trained during both contexts of socialization sampled in the study. The accounts considered demonstrate how learning to legitimate the commercial role through reframing resolves cognitive dissonance (Festinger 1957). Regardless of context of socialization, each of the scientists exhibited ambivalence, apprehension, or incomplete acceptance of the commercial role as a consequence of conflict between their new activities and how they want to see themselves. Because the practices and norms that correspond to a commercialist self-concept depart from existing cultural expectations in science, new commercialists perceive their departure from traditional scientific convention as morally questionable. The discrepancy in self-concept motivates behavioral and attitudinal change, the result of which is the internalization of meanings of commercialization that substantially depart from the views to which the scientists were socialized. Consequently, dominant mechanisms of social control are rendered inoperative. Reframing is thus a technique critical to transforming traditionalists into commercialists. It is a form identity work that occurs as one develops a new professional identity as a commercialist. To sustain this identity and to persist in the commercial role, however, commercialists must learn other techniques to legitimate externally as they become known for, or associated with, commercialization. With the capacity to neutralize the association of commercialization with royalties, companies, industrial affiliation, and commercial norms, a commercialist identity begins a course toward institutionalization.

Role Distance

Rarely do expert workers or craftsman seek to disassociate themselves from the fruits of their labor. Rather, classic depictions of expert labor emphasize both the extent to which workers see themselves in their products and the extent to which an inability to do so results in alienation (Mills 1951). Indeed, as I showed in chapter three, commercialists frame their visibility in terms of their ability to cite broad societal impact through their technological inventions. In circumstances in which the commercial role is called into question, by contrast, creating symbolic distance between one's sense of self and one's creations provides a means by which one minimizes investment in that role. The intention is to minimize one's association with negatively perceived aspects of commercial involvement, such as the notion that commercialization threatens one's commitment to university responsibilities, or the ability to buffer one's academic work from industrial influences.

This is a difficult feat, particularly because of the permanence of association derived from property rights or the paucity of free time in a scientific career. Consider, for instance, a scientist founding a company for the first time who expressed reservation about both time commitment and the management of conflicts of interest.

> I guess I've also rationalized it as, it's not my company, it's my co-worker's company. I'm just kind of a glorified consultant. Okay, co-founder is next to my name.

Don't you have an equity interest?

> Absolutely. There's no question that I have a real financial stake in this and I put my own money into it. But I guess I just don't...in my mind, I just don't see it as my company. Truth is, the ideas for this company are largely out of my own brain. My collaborators

> across the street, I love them dearly, but I think they would be the first to tell you, this was my electro-property and me coming to them saying...'Guys, I have something here, are you interested?' [74]

The intellectual property underlying the endeavor, he suggests, is recognizably his, yet he symbolically resigns himself to the status of "glorified consultant." As a technique of neutralization, role distance may here serve both a personal and public objective. For the commercialist, rationalization may function as a means by which one sustains a positive identity. For others, rationalization may form the basis of attempts to persuade the scientific community of one's "cleanliness." by placing the "dirty work" at a distance.

Other scientists engaged in role distance by suggesting that commercialization occupies a peripheral rather than central component of their work. It was not uncommon, for example, for scientists to suggest that their involvement in commercial activities entailed little of their time. They framed their participation as a "bonus," an "adjunct to basic academic life," "an interesting dimension to add to the scientific career," and "something done off salary," and thus not a threat to the performance of their university obligations. Other externally directed acts of role distancing target one's colleagues. Consider one scientist whose commercial activities resulted in past year revenues of $1,000,000 for his university and over $270,000 in personal commercial income. He noted downplaying or distancing himself from these circumstances in the way he presents himself to his colleagues.

> Clearly having more money in some ways has made my life easier to manage. I hire people to fix the house, I hire people to do things that I otherwise would have to spend

my time doing….[But] I don't want to live in a way that shows money, because it would separate me from the colleagues that I value working with. [41]

The most subtle use of role distancing is found in the omission of commercial activities from a curriculum vitae and professional webpages. Ten of the twenty-five scientists (approximately 35 percent) who had founded companies at least one year before interviews occurred did not disclose this information on a CV.[1] Without subjective data it is difficult to conclude that the omission of commercial activity from one's CV serves the purpose of downplaying involvement, but it is nevertheless a curious omission. As historical maps of one's achievements, experiences, and affiliations, a scientist's curriculum vitae provides the most detailed public account of the scientific career, from the momentous to the minute. To not disclose a product of one's work, particularly one that entails commitment of greater duration than, for example, a guest lecture or participation on a committee selecting a journal editor, is therefore suggestive of purposeful role distancing.

Social Comparison

Another way to neutralize negative aspects of a social object is to selectively compare it to something similar that is deemed inferior, a downward social comparison. Doing so enhances the appearance of the object in question by virtue of its position relative to a more disreputable, discredited, or dirtier referent. This is a subjective portrayal, for it involves disparagement and attributions of inferiority in order to portray the object at hand in a more positive light.

[1] One scientist whose first company was being incorporated in close proximity to the interview date was not included in this count. Academic conventions in constructing a CV suggest that omitting commercial involvement from one's CV is commonplace and not limited to scientists in this study.

Whereas members of some dirty work occupations engage in selective social comparisons in reference to other occupations (Ashforth and Kreiner1999), commercialists' use of this technique involves comparing commercial activities in reference to other practices or regularities of academic life – an intra- rather than interoccupational comparison. A common example of this technique, confined to commercialists at public universities, involves comparing commercialization of research with the publication of books and textbooks. This practice emerged most frequently when I asked the scientists about the impact of commercialization on collegiality.

Do you have colleagues who you would say are opposed to this sort of activity?

> Probably in the humanities I would say…But some of these humanities don't hesitate to write a book. Commercialize a book. So what's the difference between that and a company, right? Same basic concept. The time spent to write a book, versus starting a company has got to have some analogies there. That's more what I'm referring to. Not how much money...Whether it fails or is a success, is one issue. But getting to that point where it's out there, is another issue. [93]

At a minimum, selective comparison invokes the social norm of reciprocity (Gouldner 1960) in that the legitimacy of commercialization is framed as "no worse" than publishing a book or textbook. In doing so, commercialists and traditionalists accord themselves autonomy indicative of academic work. In this respect, distaste is neutralized by attributing equivalence to a longstanding academic tradition. More frequently, however, public commercialists disparaged book publishing as having little or no value.

Do you believe commercialization has any impact on collegiality within the department?

> I don't think so. One thing that you're not covering that I think is actually similar and no one questions...[is] if I write an undergraduate textbook in organic chemistry, you know, I can make a ton of money and I can spend a lot of time doing that, and people view that as perfectly reasonable, scholarly work, but it doesn't really...It's not forwarding the field, it's not...So if you compare that to entrepreneurship, I have to rate entrepreneurship for the most part, ahead of writing a textbook. People update those all the time and what it does is it just makes a lot of money for them and the publisher, and that's it. [36]

Note that in both cases, the scientists avoided directly addressing the question posed to them. In contrast to reframing and role distancing, which constitute sustained modes of legitimation that begin once one enters the commercial role, social comparison represents a reactive technique. Theorists of social comparison argue that this technique is prevalent when focal actors experience threat (Gibbons & Gerrard 1991). Given that the question asks participants to reflect on potentially negative outcomes related to their commercial involvement, legitimacy of commercialization is "called into question."[2] At public universities, where scientists engaged in textbook writing may be more likely to be found than in private institutions, commercialists have a foil at their disposal in which they may "put on trial" in order to present commercialization in a more positive light.

[2] To be sure, the question could be interpreted in a positive light. If there were any assumptions underlying it, one could be a belief that commercialization potentially enhances collegiality. For instance, material benefits may be viewed to enhance the resources and reputation of a department, or contributed to societal welfare.

Commercialists similarly utilized selective comparisons in other instances. Again in response to questions potentially interpreted as threatening, scientists responded by comparing allegedly negative aspects of commercialization to traditional aspects of academic work.

Do you see it as problematic that patent reviewers are not experts in the areas you do research in?

> Well, there is a difficulty in getting patents through the patent office because the people that are judging patents are really not that, what should I say...really don't have the background that the scientist has. On the other hand if you look at...we could say that NIH funding tends to mediocrity. Why? The study sections are populated by average scientists, right? There are very few very creative and gifted scientists that responsible for the big ideas and they...sure, some of them serve on study sections, but it's a lot of work. If you do it a few times, you're not going to do it again because it takes a whole month out of your productive lifetime to be on one of those study sections…You're asking average people to recognize brilliant new ideas and it doesn't always happen. [29]

What are the conflicts that emerge for universities as a result of a relationship between academe and industry?

> The only one that I think would be a concern is if industry biases the selection of research problems. If it creates somehow incentives that distort what it is that the university would be doing from some sort of first principles. Having said that, however, I want to say that I think it's a negligible concern relative to NIH. So if you ask me: what's the biggest danger to science? I consider NIH the biggest danger to science or at least biological

> science, because it distorts the whole field towards biomedical science which is a very narrow piece of the biological universe. [82]

The credibility of commercialists' comparisons is not in question: the concern is not with the veracity of the comparison, but the necessity. Indeed, this view may not be limited to commercialists and this fact illustrates how selective comparison operates. When challenged, the commercialist is able to point to *central* aspects of academic work and comparatively frame commercialization as superior to the object of comparison. The result of social comparison is thus a self-justifying mandate for commercial autonomy.

Professionalism

Even if social comparisons are effective, the technique does not alleviate the concern that commercialization entails its own set of problems, namely ethical issues. Commercialists thus commonly invoke the notion of professionalism, and in particular a code of conduct, that uniquely enables them to deal with the ethical aspects of commercialization in an appropriate manner. Such invocations were prevalent when discussing motivations for commercialization, conflicts of interest, and the appropriate balance between research and commercialization in universities. For example, when discussing his views of the "right reason" for engaging in commercialization, one scientist stated:

> We probably have sort of a...it sounds self-serving...a little bit more altruistic attitude than the general population in the United States, I believe. [63]

‘You can trust us,’ the comment suggests. The technique seeks to neutralize the stigma associated with the pursuit of profit through appeal to an ethical code. It is indicative of the appeal to professionalism described by Thompson in his study of the strategies by which funeral directors cope with the stigma of dealing with death (Thompson 1991). In that study, the universal nature of dirty work renders professionalism a technique at the disposal of all members of the profession. Some scientists in this study, by contrast, suggest their commercial practices are governed by a moral license unique within the scientific community. Some expressed this idea by emphasizing individualism.

> There’s all kinds of hazards and pitfalls that one can fall into. I’m kind of lucky because I have a strong internal compass that keeps me from getting in trouble on these kinds of issues. I’m basically, ‘science first.’ When I’m working in my university environment in my university lab then my job is to do the most interesting and important science as effectively as possible that I can think of doing. And I don’t actually care whether it’s going to [get] money for me or not. [34]

The comment acknowledges an inherent potential for unethical behavior but neutralizes such negative elements by emphasizing, through attribution to fortune, the possession of a unique moral license. Notice that, in this instance, this is achieved in conjunction with recalibrating the significance of the role played by money, such that it is of little or no concern to his actions. Whereas this scientist’s comment emphasizes individual possession of such a moral license as unique within the scientific community, other scientists frame the possession of the unique code of conduct as institutionally specific.

> You can easily say that there are universities that are in the second tier, who are getting too close to being engaged in more contemporary issues to be able to support themselves, and are qualifying their activities. I think there's a lot of concern in biomedical research communities now that universities are so engaged in translation for their own economic benefit…They may not have as long a view as they need to have, but I think the most valuable universities in the country, and I think I'm sitting in one, and I think Stanford's another, are playing an incredibly valuable role to society. We are criticized for being too close to the private sector. That's fine. [41]

Do you think universities are getting 'carried away' with this?

> Some universities are. If you look at the mission of the technology licensing function within [this university] it's very clear: it's to enhance the impact of research….Other universities are set up for licensing offices to really try and get that blockbuster and sort of have a vision that there's going to be great financial gain. I think that's a mistake because history teaches us it ain't so easy and most ideas don't generate big gain, and if you focus on just trying to get the big one, then you're missing the point of trying to enhance the impact of research that you're part of. So I think some universities do get it wrong. [53]

Broad moral license is derived from being in a "top tier" entrepreneurial university. It is suggested that scientists at other institutions do it for the wrong reason (material gain) and thus abide by an inappropriate ethical code. Relative to other techniques that neutralize or positively imbue work content, professionalism enables a positive work identity by ennobling the workers themselves. The accounts point to moral hazards that individuals and institutions risk encountering and the necessity of recalibrating the role played by financial incentives. By presenting one's self or institution as among the few who abide by a moral code that can evade such moral pitfalls, commercialists construct themselves and their institutions in a positive light.

Reframing, role distance, selective comparisons and professionalism are techniques that resolve the identity instability created by adaptation of the commercial role and allow commercialists to present a positive self in light of a pervasive stigma associated with their activities. Internally, these techniques unbind commercialists from the influence of traditional norms, allowing them to persist in the commercialist role. Commercialists, however, are not the only scientists for whom the dirtiness of commercialization requires identity work, for the emergence of this role, and potential association with it, influences traditionalists' sense of self-identity. I thus turn to a consideration of the implications of this stigma for identity construction among traditionalists.

Techniques of Disidentification

The objective of identity work among traditionalists is the affirmation of purity. In this respect, professional identity is influenced not only by what traditionalists *do* as scientists (traditionalist scientific work), but also by what traditionalists *do not do* (commercialization). The underlying social psychological point is that identity construction also entails "never identities" based on exclusion and actions that are possible, but not enacted (Mullaney 2001). The basis of this line of thought begins with Durkheim's ([1915] 1999) notion of the negative cult, a group that mandates obligations by prescribing abstinences rather than ways of participating. In this respect, group identity is derived from avoidance of actions that may be tempting (much in the way that financial gain may be), but must be resisted. Whereas Durkheim alluded to the notion of a "never identity," Ebaugh's (1988) research on role exit made it explicit. Her work showed how in the process of becoming an "ex," an individual de-emphasizes a prior identity by avoiding actions associated with an "old self." Building on this insight, scholars now argue that just as behaviors that "individuals *used to, but no longer, do* are central in determining identity, so are acts in which one has *never* engaged" (Mullaney 2001, p. 4 [italics in original])[3]. The quintessential "never identity" is the virgin, whose identity as such is predicated on the irreversible consequences for identity of succumbing to the act. Just as the social identity of the virgin is predicated on sexual abstinence, the professional identity of the traditionalist is derived, in part, from commercial abstinence.

In the American academic profession, the notion of a "never identity" is a decidedly modern feature, brought forth by the emergence of the new reward system. For the traditionalist, the introduction of a commercial role that embodies values and behaviors that are inconsistent with what is prized produces a basis for identifying as *not a commercialist*. Traditionalist identity construction is thus based not only on identification with the dimensions of work considered in chapter three (what they *do*), but *disidentification* with the commercial role. Traditionalists perceive that the "dirty" nature of the commercial role contradicts valued aspects of the self and thus engage in what I refer to as *techniques of disidentification*, to affirm their purity. Such techniques represent how traditionalists define who they are based on who they are not. I consider four means by which the process of disidentification occurs: social comparison, disavowal, ritual identification, and retreatism.

[3] See also Aronson, Blanton, and Cooper (1995); Dukerich, Kramer, and Maclean Parks (1998); and Elsbach and Bhattacharya (1996).

Social comparison

Like commercialists, traditionalists use social comparison as a key means of identity work. Both patterns entail disparagement of one object in favor of another, yet they differ in their objective. Commercialists make comparisons to portray their role in a more positive light, whereas traditionalists engage in comparison to differentiate their work from commercialists.

The following account, representative of social comparison among traditionalists, illustrates how disparagement of commercialization is used to differentiate commercialization from traditionalism. The scientist was discussing industrially-sponsored research at universities and stated:

> Directing resources at a defined project is a waste of time, because in order to define something before it's appropriate to define it, you end up working within the incorrect and incomplete knowledge base. The best bang for the buck is developing new ways of thinking about things, new methods for doing fundamental tasks and training people to use their brainpower rather than to harness it to accomplish the idea of a stupid person.

What do you mean by that?

> Managers can't possibly know very much about science. And so if a manager, based on his outdated and incomplete education, sets a goal and is too restrictive in his defining of the objectives, you have a person who has tremendous capabilities just dying. I still remember when I was an assistant professor, I visited [HighTech Inc] in California and there was this huge room filled with recent PhD.'s in carrels and they were all dying. There was nothing for them to do that was worthy of their PhD. They were prisoners. And after three or four years, they would not be capable of doing anything useful ever

> again. We train people to define their own problems, not accept problems, and to solve them and to go where the problem leads them. [46]

In colorful terms, the account lays out differences between traditionalist and commercialist science. Self-definition is honed through this juxtaposition of references to each mode of science. Traditionalism is presented in a favorable light to indicate what one is not. As it were: traditionalists are autonomous, commercialists are subject to external control.

Disavowal

A core basis of disidentification is *disavowal*, or disclaiming connection with, or responsibility for, commercialization. Through disavowal, traditionalists affirm their identity by rejecting commercial incentives as having no binding force on their behavior. Given the continual operation of commercial incentives, disavowal is a sustained form of identity work in which the purity of one's identity as a traditionalist is reflected in continued rejection of commercial temptation. The manifestation of such identity work may thus be observed in scientists' accounts of encounters with commercial incentives, such as consideration of career paths associated with commercialized science.

> The first job that I was offered was to work at [a research institute]. I didn't seek that job and I didn't take it, but it was to develop a basic trick that I had used in [my thesis] as a way of developing [an application]. I was not remotely tempted. [25]

> I was offered a job at [Energy Inc] during the 1980s, which I turned down. I was too interested in the theoretical aspects and I felt that often the applications were so easily

seen from what you did that there was no point in trying to capture a patent. And so I never thought about that, and never thought about starting a company. [43]

A more telling perspective from which to examine disavowal is how traditionalists respond when asked to hypothetically consider circumstances in which they could envision commercializing their work. Sequenced in the interview protocol after the scientists had already explained why they do not commercialize their work, this interview question was designed to generate data on conditions of acceptability through descriptions of the contexts in which commercialization could be seen in a more favorable light.[4] That is, the question draws attention to specific incentives that would motivate one to succumb to the temptation to commercialize one's work. The modal pattern in the responses, however, was nearly invariable rejection, succinctly captured in the words of the following chemists:

It's not for me. I think others will do that. [43]

No, not for me personally. [27]

"Not for me" reflects a basic disassociation with the commercialist identity, reflecting the desire among traditionalists to separate themselves from commercialization. Two categories of responses that reflect adherence to traditional rather than commercialist incentives may be found

[4] The utility of this analytic approach may be understood by considering reactions to killing. In different places and times, killings may be defined as criminal or non-criminal. The focus on the acceptability of commercialization, much like the focus on the criminalization of killing, is to ask how an act is defined in one way in some circumstances, but not others (e.g., Cooney 2009).

within this general pattern of rejection. Some traditionalists responded by repudiating financial incentives:

> I told you my motivation wasn't money. Having a company or even being the chief technology officer is something many of us don't want. It's a big distraction from what you have to do.

You were never motivated to attempt to commercialize any of your research?

> We did some interesting things that actually haven't come to market yet after twenty, thirty years. We made the first portable device that could monitor your [physiological process], a computer-based device. The motivation at that time was and now still would be let's write a paper about this, so we disclosed all of the technology in publications rather than worry about 'let's start a company.' [31]

> I don't think so. We have plenty of money and it's not like we're anxious to do this. I don't know what the motivation would be, so mostly it's just stuff that's published and people take it and that's fine. [63]

> You know, I'm not sure I would do that myself. I'm not interested in trying to sell anything. [70]

Thus one mode of rejection is predicated on repudiation of financial incentive. Through disassociation of the self from the influence of commercial motives, traditionalists affirm their adherence to the traditional incentive of recognition through publication.

A second and related pattern of rejection is framed by contrasting the originality of commercialist and traditionalist contributions. This rejection calls into question the validity of the most basic commercial act: patenting.

> One of the things I have a little trouble with is how different does something have to be to be novel and patentable? I think a lot of the things, some fraction of the stuff that's being patented is ridiculous and I can't imagine how the patents are going to stand up. [24]

> I think you need things that can be patented at times, right? But, you know, you can't patent gravity. I mean some people consider aspects of commercialization as very analogous to that. You know, Newton found an apple landed on his head and he didn't get a patent. So [it is not as if] any time you drop something you have to pay the Cambridge University. [52]

Such identity work entails trivializing key referents of commercial behavior, such as the patent, in ways that construct incentives as unpersuasive. The incentive to patent, in this respect, is non-binding on traditionalist behavior because it is viewed as departing from the traditionalist perception of originality. Such referents challenge the traditionalist sense of self and thus their deflection serves to frame them as not associated with the traditionalist identity.

Disavowal affirms the traditionalist identity by severing any perceived tie to behavior that commercial incentives could be seen as having. Traditionalists deny responsibility for, and association with, such behavior by framing commercialization as a practice engaged in by others, but, for them, the basis of a "never identity."

Ritual identification

Traditionalists may be able to disavow and disparage the commercial role, but the co-presence of reward systems creates an unavoidable predicament for them. Referents to the commercial reward system are pervasive. Although many of these referents, such as patents or and industrial sponsorships, are easily rejected, others related to the goal of commercialization are less easily avoided due to the structure of funding. When a scientist applies for funding, many agencies require them to articulate extrinsic, ancillary goals of research beyond the creation of knowledge. Thus, for traditionalists, who exhibit intrinsic commitment to science, the identity threat is that they must participate in aspects of the commercial role despite the fact that they do not identify with it. Consider how one traditionalist describes the situation:

> I have had uniform funding from NSF my entire career. The last few years, they have really upped the broader impact [section] of your proposals, you now have to explain the broader impact of your research very carefully and many view that as a corruption of the NSF process. I do.

In what way is it a corruption?

> It forces you to commit minor fraud. Somebody who’s trying to get greater precision on the bond length of a carbon oxygen bond – that doesn't have societal impacts. And if you say it does, you're basically lying. You're being pushed into the situation where you say ‘well somehow this knowledge will translate into a better drug for cancer.’ Well, bullshit. You know it doesn't. It’s a puppet thing. We're forced to be a puppet in that sense. Not that it’s not a good idea for us to pay some attention to that but different kinds of research have so much more opportunity for this kind of stuff than other areas. Are you going to

suppress those areas where you can't make a broader impact statement honestly? I wrestled with this. Something you do that does not have any direct impact on citizens of the United States doesn't mean it's not important to research. That's the part that bothers you. Should we all be working on making better bicycle tires? [27]

The scientist's imagery of fraud and puppetry reflect the rejection of the commercialist imperative for societal impact. But the implication of such renouncement entails a loss of funds for research. This paradox leads traditionalists to engage in what may be called *ritual identification*, in which the traditionalist participates in, but privately rejects, this minor but nevertheless important commercial act. Merton's formulation of ritualism as a mode of adaptation invokes the basis for such identity work (Merton 1968, p.205): "It is, in short, the mode of adaptation of individually seeking a *private* escape from the dangers and frustrations which seem to them inherent in the competition for major cultural goals by abandoning these goals and clinging all the more closely to the safe routines and institutional norms." [original italics] Traditionalists, absent an abundance of funding for fundamental research, must embrace certain behaviors of the commercial role, but do so in what might be considered an automaton-like fashion.

I always write, you know we're looking at sexual reproduction and we will find this pathway through which social influences change the brain. So I can, with some confidence, say girls who grow up in a house without fathers menstruate at a different time by a delta of one year than those who grow up with fathers. That's clearly a social impact on the reproductive cycle. It's not a lie to say we may be able to find out how this

> is working by finding the pathway, *but do I believe I'll find it? No. Do I believe that if we did find it, we'd understand who [should] remedy that? No…It's not my thing.* [63]

The emotional content of ritual identification can include relative indifference, apathy, and antagonism. For example, a young theoretical chemist noted that, although he is not pessimistic about overtures to societal impact, he observes pessimism in his colleagues.

> My colleagues groan all the time that in any presentation that they give, any paper they write, or any proposal they give, they have to write that stupid part of the paper where they have to connect it to broader impacts and all of this. So like 'what a pain, I shouldn't have to do that'…Those same people I think, tend to give a very much of a cursory broader impact. It's like well, 'I have the same stock six sentences that I write at the beginning of my papers and then just go on and do my thing.' [47]

This characterization, through its emphasis on apathy and the response to it, depicts the general sentiment among traditionalists regarding obligatory participation in commercially-oriented acts. It evokes "going through the motions" in order to get to what is truly valued.

Some traditionalists, however, infuse ritual identification with antagonism. Antagonism reaches an extreme particularly when traditionalists discuss the National Science Foundation, which until recently was a major source of funding that lacked a prerequisite referent to societal impact in evaluating proposals.

Fifty years ago, forty years ago, it was pure science. 'We think that this is an interesting problem that we'd like to get into and look at and think about.' We can't do that anymore. If we go to NSF [or] to NIH, we have to have an application in mind. Even though it's a pure science pursuit, it's like 'we want to figure out a way to tether down dyes onto a surface so we can make solar cells, die sensitized solar cells in a better way.' But what's the pure science for me? I'm not really going to make a die sensitized solar cell or really try to optimize it. So you write a proposal around die sensitized solar cells and say you know, 'we think that a new way of attaching dies would be something good.' Am I really going to spend all my time trying to optimize dies on solar cells that essentially can get up to five or six percent efficiency so that I can say I have the best? Don't think so! That's not my interest…I'll let those funding agencies spin the data that they get as they want.

What are the consequences for science of these broader impact statements at NSF?

I don't even want to talk about NSF. You can talk about any other agency. NSF is just a basket case in my view.

Why is that?

I don't want to talk about it. I'll just stay off the record about that.

You are off the record, period. This is an anonymous interview.

It's just a basket case. It's not the National Science Foundation anymore, it's all about society building. Okay? Let's take it back to where it's supposed to be, National *Science* [*emphatically*] Foundation. If they want to change their name, that's fine. It's not outreach. We already do outreach as it is. We're already over committed as it is, let's give scientists some money so they can pursue basic science. [69]

Ritual identification is a form of identity work that permits traditionalists to obtain funding: they feign interest in commercialization for official purposes, but renounce commercialization otherwise. It is, in effect, a "fact of life" for traditionalists that they must tie their work to extrinsic goals regarded as secondary to the creation of knowledge.

Retreatism

Additional responses to commercialization suggest a final form of identity work: *retreatism*. In contrast to ritual identification, in which the commercialist identity is rejected by going through the motions, this mode of response entails abstinence and withdrawal. Entertaining commercial incentives is considered "selling out." According to this view, it is more honorable to reject commercial means and ends altogether. The purity of one's identity is maintained by ending the career, free of taint. This means of disidentification is apparent in scientists' late career phases. A chemist at a private university, discussing the shift in funding toward technologies and applications, reflects this pattern of response:

> Support for fundamental research has withered. There are a lot of scientists out there and they have nothing to do, they have bad students, and out of desperation, they think 'I'm a clever guy, let's see if I can do something, because there's no way that I'm going to be able to do the kind of fundamental research that I was trained to do as a graduate student.' When you look around, there are a few first rate places where most of the faculty are able to remain intellectually active and doing what they want to do for most of their career. I think that the vast majority of colleges and universities in the United States [have] situations where people cannot keep their research going for more than about 10 years and that leaves 20 to 25 years of either becoming bitter and just basically not doing

> anything or trying to do something that's challenging. Technology is easier to succeed at than basic science simply because all you need is a small thing and then you can work at developing it. [46]

Having completed his PhD. in 1971, this traditionalist suggests his own retreat is imminent:

> I have a fairly pessimistic view from my own experience and you know it makes me, I'm not looking forward to retiring but this is one of the reasons why I think that I might like to retire – that I see the kinds of science that I love and am good at being choked off. So I'm pretty pessimistic and at some point, it will be the thing that leads me to say, okay, I'm turning off the lights in my lab. [46]

A biologist at a public university presents a similar response. Like the traditionalist just considered, he completed his PhD. in the early 1970s.

> Government funding ultimately boils down to what congressmen believe is important, and since half of them don't even believe in evolution, you have to worry a little bit about well 'his grandmother died of this, that and the other' and that's why they're supporting the Heart Institute. I have a friend that works at the Kidney Institute and they're having trouble now, because people were expecting them to work on kidneys. This guy works on fly extermination, and he's having to morph over to something that he can a little bit better justify as being kidney related…And the National Science Foundation has been forced to differentiate themselves from NIH and the way they've done that is by having

their focus being on education and minorities – social engineering as far as I can tell. So now I can't go there, because you know, how is my work going to affect high school teaching? [70]

The apparent incredulity that characterizes this scientist's response reflects habituation in a culture of science where only a loose coupling connected research at the bench to societal goals.

I have a tiny, little dribble left of the money I had, because I stretched it out, but that's going to end. The university's going to pay a minimum amount. So what I'm saying is, it's a stupid system the way it's set up…Science is like lemmings, everybody rushes towards what looks like the hottest and new thing…We have to maintain a core of people that aren't motivated towards curing a disease, but are just motivated towards understanding what we don't know. My whole career has been letting the fly literally take me where it will, and I follow. That doesn't lend itself to writing grants very well, because there you have to say: 'Where will I be in five years?' I don't know. [70]

The scientist neither embraces a system he rejects nor engages in ritual identification, actions that could possibly permit sustained participation in the research role. Instead, he initiates retreat.

Summary

The objective of this chapter was to identify the ways in which a commercial reward system creates new forms of identity work in the academic profession. The objective is predicated on the fact that both traditionalists and commercialists are aware of a moral taint or dirtiness surrounding the commercial role. In short, the emergence of the commercial reward system and its perceived "dirtiness" has had a destabilizing effect on notions of identity in science. I identified two broad processes, legitimation and disidentification, and illustrated eight respective techniques by which the processes occur. The processes, and the techniques that enable them, are summarized in table 21.

Table 21: Identity Work among Commercialists and Traditionalists

	Commercialists	Traditionalists
Modal Process	Legitimation	Disidentification
Objective	Negation of moral taint	Affirmation of purity
Techniques	Reframing	Disavowal
	Social comparison	Social comparison
	Role distance	Ritiual identification
	Professionalism	Retreatism

Commercialists engage in techniques of legitimation, the goal of which is the negation of moral taint. This is particularly necessary for commercialists socialized in contexts of ideological purity, in which they developed a view of the commercial role as dirty, but it is equally the case for commercialists who entered science more recently. Despite the fact that some groups actively promote commercialization (such as the *Association of University Technology Managers* or university technology transfer offices), there is nevertheless a pervasive stigma associated with the commercial role, a view that is embraced by both traditionalists and other critics of commercialization in and beyond the academic community. Thus, all commercialists engage in techniques of legitimation to maintain a positive self-identity and to attempt to present a legitimate professional self to the scientific community.

Four techniques constitute legitimation-based identity work. Reframing permits scientists to embrace the commercial role, by transforming the meanings one learns outside of commercial culture to meanings created by members of the group. Meanings are frequently reframed such that they oppose moral taint. Thus, commercialization is transformed from a constraint on graduate education to an enhancement of group leadership; money is transformed from something dirty or contaminating, to a worthy goal. Having observed the importance of reframing to scientists forming companies for the first time, one need not assume these are recollections of a neutralization process or an ex post facto excuse (Minor 1981; Minor 1982). As Hirschi (1969) suggested, even in the event that neutralizations do occur as after the fact rationalizations, they may function as moral release mechanisms that facilitate future departures from convention. That is, in learning how to neutralize and cope with the stigma associated with a preferred course of action, one's identification with and commitment to a role could persist unconstrained by the pressures of social control that were previously operative.

The data suggest that this is indeed the case among commercialists, as role distance, social comparison, and professionalism comprise techniques employed at all stages of the commercialist career. Role distance involves private and public acts in which commercialists deemphasize their role. An example of a private act is the commercialist who frames his or her involvement as an adjunct of their work, whereas public acts of role distance are exemplified by omitting commercial referents from one's CV or limiting the signs of financial success. Thus, dirtiness may never be disowned or eliminated, but it can be downplayed to permit one's peers to focus on more positive referents of self.

Social comparison seeks to neutralize moral taint by contrasting commercialization with traditional aspects of academic work. In doing so, commercialist activity is enhanced, and its own stigma disparaged. Textbook writing provides a readily available foil, given that both it and commercialization of research are tied to money making ventures. But commercialists will equally disparage the peer review process and federal funding sources as greater threats to the integrity of science. Social comparison thus presents commercialization as socially superior to any comparative referent. Even if non-commercialists view such comparisons as specious, this technique provides an interpretive scheme which reinforces the positive identity of commercialists.

Finally, commercialists utilize professionalism, which is predicated on appeals to altruism and recalibration of the role of money. Professionalism entails both individual and institutional framing. Individually, professionalism is employed by commercialists who stress altruism and the ability to draw boundaries between their commitment to research and the role of financial incentives. Institutionally, commercialists employ professionalism by suggesting they are members of a unique organizational culture that engages in commercialization for societal impact rather than economic gain. In both instances, the dirtiness of commercialization is neutralized by suggesting that financial incentives, a key referent of moral taint, play only a limited role in commercial acts.

Identity work among traditionalists is motivated by the destabilization of the traditional academic role and the aura of taint associated with commercialization. Perhaps because of the pervasiveness of commercialization in the departments in which the traditionalists are situated, the scientists seek to affirm their own purity, despite the fact that they do not actually participate in commercial culture. Commercialization in this respect constitutes the basis of a “never identity” for traditionalists, as the referents of commercial culture are drastically inconsistent with what traditionalists prize. Traditionalists therefore engage in four techniques of disidentification, the goals of which are to affirm purity by repudiating commercial referents.

Like commercialists, traditionalists engage in social comparison. The technique is simply the inverse of the process employed by commercialists. Traditionalists disparage referents of commercial culture to portray traditionalism as superior to commercialism. For example, the identity of the traditionalist is constructed as autonomous, whereas commercialists are portrayed as subject to influence and control of non-scientific groups. The purity of the traditionalist identity is thus affirmed by contrasting it with a dirty comparative referent.

The technique of disavowal affirms purity by unraveling any perceived grasp that one could assume financial incentives have on the behavior of traditionalists. This interpretive scheme surfaces particularly when traditionalists are pressed to come up with any imaginable context in which commercialization could be viewed as worthy of their consideration. The fact that most responses to this question centered on the rejection of financial incentives reflects the fact that traditionalists see money as the hallmark referent of the commercial role. As a modal pattern of rejection, the technique reflects an interpretive scheme that actively distances traditionalists from dirtiness by presenting a self-identity that is not conditioned by money.

Traditionalists may rely upon disavowal and social comparison to present a pure self, yet they must nevertheless dirty themselves to the extent that funding proposals require them to associate their work commercial goals. Consequently, traditionalists employ ritual identification, in which they enact ceremonial overtures to commercial goals while privately rejecting them. Commercialization is framed as a “never identity” because traditionalists derogate their obligation to participate in such ceremonial acts as “bullshit,” “puppetry,” and “fraud”, terms which reflect personal offense at having to permit any commercial referent to be associated with their work.

A more extreme technique is exemplified by traditionalists in late career stages who embrace retreatism. Here, the purity of one's identity is preserved by ending one's career. Rather than embracing ritual identification, traditionalists in late career stages may interpret the alignment of funding mechanisms with commercial referents, such as societal impact, as sufficient reason to leave science altogether. To be sure, some scientists may have no choice but to abstain, to the extent that federal funding for fundamental research limits the choices of research pursuits for those dedicated to basic research. Such patterns, however, only solidify the purist identity of the traditionalists, who see themselves as having never "sold out."

Four implications merit further consideration. First, the point that all occupations entail dirty work has not been overlooked by sociologists (Hughes 1958), but rarely, if ever, have social scientists who study science considered this as a productive line of inquiry. By contrast, this chapter has revealed that dirty work is indeed a salient issue in scientific work and the analysis of it uncovers aspects of scientific identity not previously considered.

Second, sociologists should more closely examine the relationship between professional differentiation and dirty work. The general theoretic implication uncovered in the examination of identity work among commercialists and traditionalists is that non-practitioners of dirty work are influenced by the taint of their peers. The stigma of a professional role may thus not only influence the identity work of the individuals who perform it, but also the identity work of members of the professional community who seek to disassociate themselves and affirm their own purity.

Third, the collective nature of identity work among commercialists may permit speculation about the cultural processes by which the commercial role was institutionalized.

Legal change alone could not legitimate commercialization. It remained then, and remains now, a questionable, and even morally improper activity. "The greater the salience of social perceptions of dirtiness, the stronger the culture of the relevant occupation or workgroup will be" (Ashforth and Kreiner 1999, p.418). It could be, therefore, that such identity work processes played a broader role in the creation of commercial culture in academic science and the institutionalization of the commercial role.

These implications will be explored further in the conclusion. In the next chapter, I look in detail at a final implication suggested by these findings: the notion that the commercialist role departs from the normative structure of science. If techniques of legitimation enable commercialists to embrace, and persist in, commercialist practices that depart from conventional scientific behavior, what then is the relationship between commercialization an "ethos of science?"

CHAPTER 6

TENSION WITHIN THE NORMATIVE STRUCTURE OF SCIENCE

A basic sociological tenet is that groups have norms. A social norm, Goffman (1971, p.95) explains, “is that kind of guide for action which is supported by social sanctions, negative ones providing penalties for infraction, positive ones providing rewards for exemplary compliance.” Norms thus function as a form of social control that regulates the activity of groups through the allocation of sanctions for conformity and deviance. Academic scientists, like other groups, are guided by social norms, yet the operation of the normative structure of science is unique relative to other occupations and professions in the social division of labor. Whereas most occupations are subjected to simple, technical, or bureaucratic modes of social control (Edwards 1979), the hallmark of social control in academe is self-regulation. It is not the case that academic scientists perform their roles independent of other modes of control (cf. Rhoades 1998), but particularly among the segment of academics considered in this study, expectations for and standards of role performance are predominately the product of professional norms. To fully understand the operation of scientific norms, we need to specify the conditions under which commercialization creates tension with the traditional scientific ethos.

In this chapter, I thus examine how traditional scientific norms operate in the context of a reward system that encourages the commercialization of research. The goal of the chapter is to understand how commercialists and traditionalists construct the ideal relationship between academic science and commercialization. Framed in this way, we may draw conclusions about the normative structure of science based on scientists’ views of appropriate conduct. Whereas some studies of scientific norms have sought to determine the prevalence of scientists' acceptance of norms by posing questions directly related to the norms described by Robert Merton (Blissett 1972), I employed a set of open-ended questions that would enable me to infer the presence of or tensions with traditional norms without directly asking scientists about norms. My discussion is based on five questions I asked the scientists:

What should the relationship be between university research and commercialization?

Should universities reward scientists for commercializing their work?

Do you see any problems associated with academic researchers accepting money from industry to conduct research?

Do you perceive there to be flaws associated with basic science as a mode of inquiry?

Should researchers be concerned with the utility of their discoveries?

The chapter is organized according to three scientific norms that emerged in the study, each discussed in turn: disinterestedness, universalism, and communalism. Organized skepticism, an additional norm outlined by Merton (1973), received minimal indirect reference by the traditionalists who espoused the view that commercialization requires "cheerleading", "marketing", or "pushing what is currently hot" by scientists in ways that are counter to the notion of detached scrutiny of discoveries. Thus, although organized skepticism is influenced by the commercial reward system, I do not place it under consideration here because of its limited empirical prevalence and thus its eclipsed empirical relevance to the study of commercialization in science. In the final section, I conclude the chapter by summarizing the findings: the discussion will highlight the ways in which a "normative" ethos increasingly bears a tenuous hold on the structure of science.

Disinterestedness

The norm of disinterestedness governs motivations for research and thus provides institutional control of problem selection. The basic prescription of this norm is that research should be done for the "sake of science", not for the sake of rewards. In other words, research and discovery are ends in themselves, not professional recognition, influence, or financial success. The importance of this norm is that it leaves only scientific criteria by which to determine what research one should undertake. Disinterestedness is thus critical to the reward system in its connection to a central scientific universe of "discourse" (Storer 1966): scientists obtain rewards for having solved problems of collectively agreed-upon significance to the advance of knowledge. As a consequence, scientists continually work on problems of relevance to the scientific community and science advances through professional coordination. The key aspect of disinterestedness we must therefore consider is the extent to which an emphasis on commercialization undermines, at worst, or strains, at best, the use of scientific criteria in selecting problems for research.

Commercialists

There are two ways in which tension underlying disinterestedness among commercialist scientists is manifest, both of which are related to the criteria by which commercial scientists select research problems. As we observed in chapter three, commercialists, though centrally integrated into the scientific community, exhibit an orientation to audiences beyond the academic profession. This orientation is associated with a belief that scientific work should have an end beyond science for science's sake:

I think basic science is really important and a lot of our work is pretty basic science, even though it has downstream applications. But I think everybody, whether they're doing basic science or applied science, needs to be able to explain to people why they're doing what they're doing.

Why?

Because we're funded by the American people. I mean American people have a right to know how their money is being spent and why it's being spent the way it is, and I think people are perfectly smart. They can understand even complex things if you explain it to them clearly.

What about the argument that a basic scientist need not justify an end, based on precedents of useful applications that were discovered by accident or emerged decades later?

I think to a certain extent that's right, but they should still be able to explain to people why what they're studying is important. There's no luxury to being a gentleman scientist. You know, that's just arrogance. I think that that's just wrong. [36]

I think that when the funding is coming from a source such as the government, that there should be an end to the query. So I think that when we talk about dark matter, that's a good place, right? And supercolliders. I think that there has to be an appreciation for why that science is important. I think there are reasons, but I do think that the people who write those proposals need to be able to lay out the argument for that. But as long as you can lay out a reasonable argument for a mode of inquiry that leads us to some greater understanding and it's not just because, "Why?" I think that's reasonable. [44]

> This is a hard thing. Increasingly, agencies and companies and so on that fund research want to have some way to measure return on investment and it's very hard to measure it when the outcome is published papers, graduating PhD students, and things like that. It's much easier if an agency looks at the stuff they were funding here and said we were supporting the students that later founded [InfoTech Inc] and they can point to a huge impact, an economic impact. That is a valid thing to do and at the end of the day if all we did was support research at this end and there was never any connection with the real world or with the commercial world, then that's probably swinging the pendulum too far the other way. [75]

In contrast to the idea that research and discovery are intrinsically worthy endeavors, commercial scientists emphasize that the academic profession should be able to justify the merit of its activities. The scientists' emphasis is not placed on explaining the intrinsic value of fundamental knowledge to society or how such contributions are important. The scientists suggest, by contrast, that the activity indicative of disinterested science – asking "Why?" – is insufficient, even arrogant. Moreover, the traditional outcomes of disinterested science such as contributions to knowledge and future scientists are no longer viewed as worthy products of science in that they lack an obvious or immediate economic impact. In such perspectives, we observe how the embrace of basic scientists by commercialists that we observed in chapter three is predicated on its potential or planned connection to utility. Notice in the following accounts how utility "enriches" basic science.

Do you think that basic scientists should be concerned with the utility of their discoveries?

> Absolutely. I think that's the mark of a good scientist to think about the abstract question and also to think about how it might apply to a real world problem. I think every scientist ought to be concerned with how somewhere down the future their work might be translated into real world application.

Why?

> The science becomes richer if you can connect it to some real world problem. You have more basis for empirical investigation if you can get data from the real world, and so I think just from that perspective it makes sense to think about the connection to the real world and certainly from the perspective of societal benefit, which is something that the scientist will feel good about. [55]

Do you think that basic scientists should be concerned with the utility of their discoveries?

> I think that in some sense it could enrich their work and direct their work. I think even fundamental work in my mind at least becomes more exciting when you see sort of an end goal. I get bored really quickly when people are digging into esoteric detail. I sort of...I'll glaze over it very quickly. To me it has to have an application and an impact. [62]

Disinterestedness can also be constrained in industry-funded research at universities. As noted above, the key function of this norm is to channel scientists' interests toward problems the scientific community determines as worthy of inquiry based on criteria within one's field. The operation of disinterestedness would thus be undermined under circumstances in which the selection of research problems is influenced by actors outside of the community of academic scientists, for such an arrangement intermingles scientific and non-scientific interests that could include money, politics, or religion, for example. Of course, we could begin with the assumption that industry-funded research perfectly overlaps with the scientific criteria the academic community uses to designate which problems are most worthy of investigation. Among many commercialists, industry-funded research is essentially perceived in this way.

> If a company comes and says, we have this very real problem and we wonder if you have a way of solving it, we love that. I don't think there's anything different about being inspired by a company's problem and being inspired by a sort of problem of nature, let's say. [44]

What about universities accepting money from industry for research, do you see any problems associated with that?

> No. And I've husbanded over the last twenty years, at least three, probably four relationships of that nature…I think the system at [this university] has to be modeled as a way in which the faculty members have the privilege or option of applying for these arrangements. They have a research idea and they ask [Pharma Inc] are you interested in investing some money in this research idea? And then [Pharma Inc] and [the university] as a group took a bunch of ideas and said these are the ones that we think are valuable, we will make those investments. It wasn't a company telling [the university] or faculty members what to do, it was a company saying we are interested in this area and we'll make so much money available on a year by year basis in this area, if you're interested in

> that, then we'd be pleased to consider what your ideas are and we'll make judgments with our colleagues at [this university], you know, from a company's perspective, from a pure intellectual or not so pure intellectual perspective as to what's valuable, and we'll select, in as fair a way as we can, what we're going to do with these resources, and that works. We've had no problems at all. [41]

These accounts permit us to relax the assumption that the interests that motivate industrial sponsorship of research and the criteria by which academic scientists select research problems are naturally consistent. The scientists quoted above either acknowledge that the origin of the problem is industrial or that the selection process is from a "not so pure intellectual perspective." Such an acknowledgement should not, of course, lead one to conclude that companies that fund academic research are sinister. This misses the analytic point that merits our attention. Disinterestedness is presumably undermined because non-scientific criteria compete with scientific criteria in the determination of the problems on which scientists should work. Scientists in the study suggested that the industrial consortia or specific industrial sponsorship projects at their universities did not provide general research funds for universities to distribute across all colleges or fields. Corporate financial contributions to universities are thus motivated by some form of interest, yet it is true that companies have historically provided unrestricted funds to scientists to use as they see fit. The accounts of the following scientists, however, suggest that more often than not, industrially-sponsored research is driven by the research concerns of industry, not the scientific community.

I would say that over the last twenty years, the budgets of companies for those unrestricted types of support have vaporized. Twenty years ago when I started my career, it was possible for me to get, you know, thirty-thousand or fifty-thousand dollars a year of this unrestricted money from a variety of different companies who were interested in the basic problems we were interested in. [The company perspective is] ‘if we seed that, we're likely to make ourselves more visible to students, we're supporting an area of research which we know is important to our company.’ But over the last twenty years, the chemical industry has not had a great road and it’s been very hard. So all of those sorts of unrestricted funds have almost exclusively dried up. Now companies are in a mode where if they're going to make an investment in academic research, they are being very actuarial about it, they want to know what is the benefit to my bottom line. They treat it not unlike a research project in their own organization, they want to know what's my level of investment, they want some net present value associated [with] what's likely to come out of that research and what cost is that going to become over the long term. That's what informs this discussion, so now the difference between what you're talking about – the [Chem Co] Award – is that moneys are almost all gone. Now most companies are targeting their research specifically to areas where they really think it’s going to solve problems for them or that somebody is doing an aspect of research that they can't afford to do themselves, but if they seed it at a university, they get some access to technology at a price that would be cheaper than if they did it in house. [66]

Do you perceive there to be problems associated with researchers accepting money from industry to perform research?

> No, I don't see any problem with it at all, again my main concern is just how it is presented in a paper, I want to know where the pursestrings are and I'd like to see at least a reasonably balanced approach in the publication of the work… I'm hoping for a big grant from an oil company right now to do some research. I myself have turned down research from companies but it wasn't so much an ethical reason, it was because I like to be free to do what I want to do and I don't like companies telling me to do this experiment next and then do that experiment next.

They had a particular set of problems?

> Yeah they very clearly want you to do something and I would say well that's nice but I don't really want to do it. I have multiple companies come and say you know we'd like to fund a post doc and I say well I want to be free to choose what research this post doc does. What we could do is you could give me the money, we could do some research for a year, give you a report of what we have done and if you like it, you could try it again but that's not really what they want. They want to tell me what experiments they want done and that is just totally abhorrent to me, but it's not abhorrent because its ethically abhorrent, it's abhorrent because I personally want the freedom to do what I do. I don't want people telling me [what to do], I want to be my own boss. [72]

For some scientists, industrial sponsorship is an episodic event that is subject to the types of negotiations described above. Commercialists may also regularly conduct industrially-sponsored research as part of a broad portfolio of basic and applied research and federally and industrially-funded projects. For some, disinterestedness is thus episodically constrained when problem selection is derived from non-scientific interests of the funding source. Among other commercialists, the orientation to use-inspired or commercially-oriented work consistently undermines the notion of doing research as worthy in and of itself. In extreme circumstances, disinterestedness may be rendered inoperative. Consider, for example, the case of the following biologist who left a private university to accept a position as the head of an industrially-sponsored research institute at a public university.

Why did you take this position?

> [At my previous university], we already had a huge grant from [Oil Co], [UtiliCo] and [AutoCo]. We had $200 million from those companies and that had pretty much soaked up all the capability in this area there and it wasn't possible to double fund from two energy companies. So I decided to come to [this university] and throw my lot in here. I agreed to come because it seems to me if you want to make change, one of the ways of making change is to grab one of the big energy companies and help them do the right thing. From my interactions with [EnergyCo], I became convinced that there was a lot of will inside the company to try and change how they were making energy. They wanted to evolve the company. And so I felt I could help them do that. The resources they were putting into it were enough to make a significant impact and so that's why I decided to come.

Did you ever envision that you'd be in this position?

> I never envisioned it, no. It's out of proportion to what I envisioned and the opportunity was so outstanding. That's why I was really willing to separate myself from my creations and take a financial penalty. I'm paid up at the chancellor's level, so as a university

> professor, I'm in the top rank, for sure. It's negligible compared to what I made in my companies. [82]

In the prior accounts of industrial sponsorship, we observed that commercial scientists' must negotiate whether industrial sponsorship will facilitate their basic or applied research interests. These scientists often characterize these arrangements as synergistic partnerships, to the extent that sponsorship facilitates their own research agenda. In the last account, however, the scientist divests himself from the products of his labor and adopts the goals of his industrial sponsor as his own. The issue is not whether the scientists' motivation is altruistic or egoistic. Our concern is the extent to which such arrangements undermine the operation of disinterestedness.

Traditionalists

If entrepreneurial norms have transformed, undermined, or displaced traditional scientific norms, we would expect traditionalists to share the views of commercialists, in terms of the importance of utility in selecting basic research problems and the merits and goals of industrially-sponsored research. There is little support for this claim. The notion that research and discovery are intrinsically valuable pursuits independent of an end was almost invariably present in traditionalists' accounts of their work. As we will see, it was not uncommon for the scientists to appeal to the intrinsic value of discovery through acknowledgement of great scientists or great discoveries.

> Basic science means that you're trying to discover new things. You don't necessarily always have to have a use for what you discover. I mean what does an astronomer do

with their knowledge, that they find a new planet? What good is that, right? You don't know what. [31]

Should Einstein have been worried about how his understanding of nuclear physics might have generated a nuclear weapon? No. What he did had beauty and value intrinsic to the discovery and that's true of any basic science. I certainly don't feel that one should be concerned, in designing a course of research, about its application. Not at all. The university would fundamentally change if that were a consideration. [87]

The notion that the university would "fundamentally change" if basic scientists' had to be concerned with utility reflects the basic premise that disinterestedness performs an important role in the production of knowledge. Disinterestedness regulates the institutional allocation of scientific effort by channeling knowledge production toward collectively agreed upon problems. Consider the source and objective of motivation the following scientists emphasize.

I think utility is the not the word that I would use. I'd say the importance of discovery. The community ought to value research that might not lead to results that actually go into a human body in 10 years, but something that within 50 years, 100 years, would lead to something really enabling…Sometimes it is the case that scientists can study a very small problem, a very limited scope and not a whole lot of vision. [37]

I think utility in terms of the effect on the field. I think you want to do something that isn't totally obscure.

Do you think that they should be concerned with the potential practical implications or technologies?

> No, because I don't think you ever have the foresight to know. As I've mentioned, this whole business with DNA restriction, that was a curiosity. Nobody ever dreamed when they first started working on it that it was of interest. [92]

These comments reflect the belief among traditionalists that researchers should select problems that are of value to one's peers and anchored in scientific criteria. Scientists thus face the challenge of selecting important and not "obscure" problems or ones of "limited scope." This is not to say that there is absolute consensus within fields about what problems are the most scientifically consequential and soluble (Gieryn 1978; Zuckerman 1978). The point is that the scientific community, in observing disinterestedness, collectively attempts to identify and solve the most important problems that constrain the advancement of knowledge.

Traditionalists thus see disinterestedness as central to optimal knowledge growth in the role the norm plays in regulating problem selection. According to this view, focusing on utility undermines knowledge because it draws scientific resources away from problems motivated by scientific criteria. Such a utility orientation to research can create an intractable problem in the minds of some scientists: potentially important avenues of discovery may be foreclosed.

> [A concern for utility] distorts science. If you already know what you're trying to prove you are not going to discover unknown things. You're just going to rediscover what's already known or pound the path down a little bit harder that's already been beaten in that direction. I don't think that's what science is for and I really think fundamental research

> should be so undirected that you leave yourself wholly open to being told something by nature that you didn't go looking for.

How would you respond to someone who claimed this is a purist view or idealistic?

> I would start citing the examples of people who, in the course of doing pure research, discovered something that became a revolutionary development and I would challenge them to show me that number of cases where somebody went looking for a specific thing that had the same revolutionary change. [32]

> Ultimately, that's probably not the best way to do things because again you can't anticipate what the utility will be. We're going through a complete revolution in molecular biology with respect to the role of RNA and I think that there is no way that the people who discovered small RNAs and RNA interference had any idea how vast the potential applications would be, but they're huge. Were they to have been driven by what they thought the applications would have been, I think they would have been led astray. They wouldn't have stuck with that. [54]

Indeed, it was relatively common for traditionalists to cite discoveries that were serendipitous, but vastly influential, to advancing knowledge.

> There are lots of things that I can point to that people did that seem very self-serving and not of any application [at first blush, but which] became critical. One of my favorite examples is cell cycle control. Lee Hartwell decided it would be kind of cool to understand how yeast cells divide. He basically said 'I bet I can get mutants that mess

> this up'….Most of what we understand about cancer stems from his sort of fundamental discovery of how a yeast cell divides…If he had to apply his research he probably would have worked for Budweiser or something, right? There's an intrinsic value to letting people explore. I think anybody who forgets that, is not learning from history. [52]

For the traditionalist, disinterested role performance is legitimated by a collective memory of monumental basic discoveries that provided fundamental breakthroughs and serendipitous technological developments.

The traditionalist orientation to disinterestedness is also evident in their views of industrially-sponsored research. There are some traditionalists who do not view industrially-sponsored research as inherently problematic. That is, they do not perceive industrial sponsorship as a means to exert undue influence on scientific discovery. The question for these scientists, however, is whether or not funding is predicated on conditions, or the attachment of "strings."

> We don't do contract research. If the company wants to donate $50,000 to a person's lab, they'll take it, but they can't do it with any strings on it. [85]

> The scientists benefit because they have money to do the things that they think are most important, but that presupposes that the money comes with absolutely no strings attached. It's hard to tell a stockholder we just donated $10 million to [a university] and we don't know what's going to happen to our money, it's very hard to do that, that's the basic conflict. [32]

> You can't be doing work for service. That's completely unacceptable. If a company needs a particular type of experiment to do and you happen to know how to do that experiment, you should not agree to be paid to do that experiment. I think its fine to have people from the company come and say, show us how to do this. But you should not do fee-for-service. I just don't think that's appropriate….We've gotten money from [Pharma Inc] and other companies, but that's been all no strings attached…They should not be involved in directing the research. Any time they're getting involved in what's going on in your laboratory, if they think we would potentially benefit long term from more research being done in this general area and aren't concerned about what the particulars of that research is, I can maybe see that…But it's a very tricky business to start having the money go back and forth between companies and funding what's going on in the lab. So I am pretty unenthusiastic about the money going in that direction. [52]

The premise of this position is that scientists' believe that science and scientists should be undirected by influences outside the community of science. Disinterestedness is operative in the sense that the criteria by which scientists select their problems originate from scientific, not external, concerns. Yet as the following account of a chemical engineer illustrates, unrestricted industrial sponsorship may nevertheless undermine the operation of disinterestedness.

> [Industry funding] has the potential of being problematic, but if structured in the right way it can also be very helpful, so I got money from [Auto Inc] and it was a no strings attached kind of a gift.

You said it has the potential to be problematic. How so?

> The problem could be if in pursuit of corporate money I spend a lot of time just coming up with demonstrations. So the amount of time I spend on basic research, that is squeezed and I spend more time at making the work flashy or attractive to these companies in the hope that these companies will then give me money. That is going to compromise the integrity of my research program. It's going to take me away from what I really ought to be doing, and it might even cause me to rear away from the bigger questions in pursuit of specific questions that I think might be of interest to the company. [24]

Industrial sponsorship constrains disinterestedness through the introduction of non-scientific incentives and alternative audiences for one's work. Traditionalists believe that commercial incentives, symbolic or material, divert a scientist away from the problems defined by authorities within a field as most critical to scientific progress. From this perspective, even unrestricted funds are seen as a threat to science because of their source. Commercially-based unrestricted funding is viewed as a soft form of influence that leads scientists to shift their interests away from the frontier of research and toward areas of science associated with the concerns of a company. Such a perspective scarcely acknowledges the possibility of that an overlap could exist between industrial concerns and the research frontier.

Disinterestedness, recall, is focused on institutional control of problem selection. The norm achieves this social feat by prescribing the motive for role performance: the importance of the work for the advancement of knowledge. As Merton explains, "the translation of the norm of disinterestedness into practice is effectively supported by the ultimate accountability of scientists to their compeers" (Merton 1973, p. 276). Merton's point is that rewards are allocated by scientists. It is through citation by one's peers, for example, that one accrues visibility.

Therefore, the only way to elicit peer recognition is to select and solve problems that experts within a field mutually agree merit attention. By contrast, when one solves trivial or non-scientific problems, there is no allocation of recognition or prestige.

Under this form of social organization, optimal scientific progress is sustained by the value of peer recognition in that the rewards are greatest for solving the biggest scientific problems in the eyes of one's peers. However, the stability of disinterestedness and, by extension, the allocation of effort to key scientific problems, is constrained by the introduction of non-scientific criteria as motives for research. Commercialization introduces non-scientific audiences to whom scientists can select as reference groups. It also introduces non-scientific rewards for role performance. Scientifically- and industrially-consequential problems compete for the attention of scientists and thus undermine the idea of a community in which problems are defined and effort extended to address them. Insofar as commercialists and traditionalists differentially perceive what problems are the most important on which to focus, we would expect perceptions of how work is to be rewarded to differ, too. We thus turn to universalism.

Universalism

Universalism denotes a mode of evaluation. Are scientists and scientific research judged by a universal frame of reference or by particular attributes? In the context of the current discussion, our concern is whether commercialization interferes with universalism. The analysis builds upon prior work that has examined whether science is universalistic (cf. Cole and Cole 1973; Zuckerman 1977; Gaston 1978). This literature examines inequalities in the distribution of rewards by analyzing how social attributes such as institutional affiliation, gender, and race influence the allocation of awards.

Universalism holds that scientific merit and the quality of role performance serve as the only basis for decisions regarding appointments, promotions, fellowships, honors and other rewards or resources for research (Merton 1973; Zuckerman 1988). In considering the relationship between commercialization and universalism, our objective is to understand scientists' perspectives of the role commercialization plays in the operation of rewards. I therefore asked the scientists if they believe universities should reward scientists for commercialization. With few exceptions, all universities claim the intellectual property that underlies any potential royalties derived from commercialization and permit scientists a share of such royalties that result from their inventions. Universities therefore do reward scientists for commercialization but they also posses other formal and informal processes of allocating rewards to scientists, including the distribution of laboratory space, teaching requirements, resources for research, and promotion and tenure. The majority of the scientists in the study believed that commercialists legitimately deserve royalties resulting from successful commercialization. The realm in which the views of commercialists and traditionalists diverged was promotion and tenure.

Commercialists

From the perspective of the commercialists, successful commercialization of science should be incorporated into decisions regarding promotion and tenure. They point out that commercialization should never be required, but that it is a form of scientific output that should be considered when evaluating the work of a scientist.

Do you think the university should reward scientists for commercializing their work?

> I do and I've been on committees where I've said you know, young assistant professors should get something, in addition to just publication credit and committee credit. They should get some credit for patenting and commercialization activities. [30]

> It certainly goes into the package. If people are successful in implementing a new idea and then they raise resources to do that, they convince other people too, that certainly is a good thing, but for tenure right, there's three aspects to it...I mean there's multiple aspects to it, but we do an internal evaluation, we do an external evaluation, and so I think you have a pretty good idea based on all these different things. [36]

This perception among commercialists is derived from three positions. First, because commercial practices are associated with scientific work, commercialists view them as a component worthy of consideration in the same way that consulting, public service, or other activities factor into the list of activities in which scientists are engaged. Second, commercialists believe they should be rewarded because their work has contributed to their own visibility, or that of the department or university.

> I think it's meaningful or reasonable in our greater community to recognize when there's just a wonderful industry academic collaboration. I've seen awards like that in my own society, the American Chemical Society. People see that these academicians and these industrial scientists got together and they created a world of knowledge which has become central to both manufacturing something and some critical understanding of

polymer science. Well, in that case, wow! That's one of those "wow" things and we all kind of appreciate it. And when that person is being evaluated for something like tenure, those kinds of awards are important to hear. [44]

Should they be rewarded beyond [royalties] with pay raises and things like that? What is the goal of a research university? I often think that if fame is part of it, we are rewarded for fame, and so to the extent that [commercialization] gains fame for someone in the university there should be some additional reward there. [72]

Visibility, we recall from chapter three, has always performed a role in the scientific reward system. Only discoveries that come to be known can influence the advance of knowledge and therefore the appropriate allocation of rewards is dependent upon the effective communication of ideas (Cole and Cole 1973). Visibility is gained through the publication of high-quality work, but a scientist's visibility also benefits from honorific awards, departmental prestige, specialty, age, and collaborators. Commercialization, in the eyes of its practitioners, comprises another means by which a scientist can enhance their visibility which is of benefit to the scientist and their department.

The third basis of commercialists' belief that commercial activities should factor into promotion is the idea that the market constitutes a form of "peer" review. This idea ties back to a pattern we considered previously in which commercialists view utility as enriching the quality of scientific research. Commercial success is viewed as an indicator of quality, because it expresses the level of utility present in the commercialist's invention.

> If an investigator has patents that have been licensed and commercialized, then there is a peer review on the quality of that intellectual property and it's the marketplace. So I think factoring that into the impact assessment is a legitimate thing to do. [53]

> One curious thing about us as scientists is we're so obsessed with other people's opinions and assessments. When somebody goes up for tenure, why do we need to get outside letters? We don't need to, but we do. We are perfectly capable of judging their science as good science or bad science, but no department does that, right? So you can kind of take [commercialization] as a type of letter or vote of confidence. If somebody has developed something that's interesting enough to the world that they can actually apply it in companies using it and so forth, that means they did a good job in terms of finding something interesting and making it usable, right? So I would view that favorably. [86]

The value added by utility is determined by marketplace success, according to these scientists. In this sense, commercialists support a reward system in which the judgment of a non-scientific audience about the value of one's commercial practices factors into organizational distribution of rewards. In sum, commercialists believe they should be rewarded for their commercial endeavors monetarily and in consideration of promotion and tenure. They see commercialization as *institutionally relevant*: a rationale that constructs commercial activities as legitimate, esteemed, and beneficial to university and departmental prestige.

From the perspective of the university, there is an inherent interest in rewarding commercialists who generate revenue because institutions prosper when their commercialists are successful. But how universities reward commercialists is often controversial because doing so can create a climate of inequity. In some cases, how universities reward commercialists is perceived as an encroachment on professional autonomy. In a biology department at one of the public universities in the study, for example, the preservation of a stream of income generated by one scientist, "Robinson", conditioned the administration's role in hiring a new scientist. "Robinson" has generated several million dollars in university revenue through licensing of his patents. With a nine-month academic salary that exceeds $200,000, he is among the highest paid scientists in the sample. According to him, universities should not reward scientists for their commercial activities.

> I think universities should reward the intellectual aspects. That's what we're hired for. The commercialization, we end up benefiting anyway. So I think that would start degrading what an academic institution is all about if we say...'Gee, Robinson has hundreds of patents, he should have the highest salary in the university.' That's not what we're at all about. We're about teaching, we're about doing basic research, we're about finding out new things and that's what the university should judge us on. [29]

In contrast to his ideal, other scientists in his department perceive that their university does indeed consider commercialization as an important factor in how organizational rewards are allocated.

> Let me just give you an example that illustrates that there is not one-hundred percent consensus about [commercialization]. You know "Robinson" is the [technology] guy here. He brings in a lot of money, the tech transfer office loves him and they take care of

him. He is not young anymore so a few years ago, the tech transfer office decided that in case "Robinson" were to croak off, we needed to have an expert in place here who knew [his technology]. The technology transfer office actually put up the money for us to hire a new professor, younger than "Robinson" but also in that area. That was the only string attached to that money, there had to be somebody who worked with [that technology]. People didn't like that. They didn't like the idea that the department would let its future direction be dictated by commercial needs. The new faculty member didn't exactly get a resounding majority. We hired him but it was not something we did cheerfully or wholeheartedly. [32]

This example departs from the universalistic ideal: the determinant of a hiring decision was a candidate's ability to sustain the university's investment in a specific technological area. The case illustrates the restriction of careers on grounds other than competence, because the economic interests of the university preceded more general scientific merit.

Traditionalists

Traditionalists' views of whether scientists should be rewarded for their commercial practices fall into three patterns. The first, found among a minority of the scientists, is that commercialists should not receive any material reward for their inventions or technologies. The following scientists made comments representative of this view:

I think basically things that NIH funds that end up having amazing commercial implications, that money ought to come back to the government. I don't think that's legitimate to then go off and own the company. I think that's the price that scientists

> ought to pay to have NIH support is that then the taxpayers whose money that is, get a big chunk of whatever benefit there is from that. Not just the item, but the money from the item. [80]

> I always find it a little weird that many of the fundamental discoveries that go on to be companies come from NIH funded research, right? So is it good that the government is basically providing startup money for all of these things? And then individuals are profiting off of what they found based on that. As far as I know, the NIH gets almost nothing out of that. The university gets royalties, scientist gets royalties, but the entity that probably funded all the research, is getting nothing other than presumably, you know, taxes from the economy. [52]

Such observations focus on the ethics of ownership rather than the distribution of rewards for scientific work, yet they reflect the extent to which scientists' question the merit of allowing scientists and universities to profit from their work.

There are two reasons why traditionalists oppose the university-based allocation of rewards for commercialization. First, traditionalists oppose what they believe would be an inequitable distribution of rewards for role performance. This view focuses primarily on potential differences in the conditions of work between commercialists and traditionalists.

> What I'm struggling with is if they're rewarding people for that, are they not rewarding people for other things? Does it create a class or caste system? That would be my worry,

that people are seeing that that's where the brownie points are and so is there a push to commercialize more things. [64]

I think the commercialization is generally reward enough. I mean I don't think they should be given less responsibility. If you want to get out of teaching, you should be doing something else for the department. Not you know, you get out of teaching for you know, bringing in a million dollars to the university, because the truth is, if you're bringing in million dollars to the university, you brought in a million dollars for yourself too. And you know, if that's going to be your sole goal, then you should be in the commercial world, right? [52]

Do you think that the university should reward scientists for commercializing their work?

No. In fact it's always worked the other way around that the universities get a kickback from the professors who have their companies. Those guys buy us laboratories and stuff and we pay for it in ways that are subtle. I mean, a very simple example would be that some person who has an important industrial tie and who would never consider resigning his university job because he likes the prestige of being a university professor, he would never go and be a chemical company owner, but he gives us a bunch of money and in return he gets to teach whatever he wants, and all the hard work gets done by the peons, the assistant professors who have to teach the terrible courses. That's absolutely common in our department. And probably at other departments, too. The tycoons write their own ticket when it comes to the jobs they do in the department. [70]

> They should not reward them and there should be a system which the ones who do not do commercialization but do good science, they should not fall behind. But this is what's going to happen, you know? [89]

Traditionalists recognize that their universities benefit monetarily from successful commercial activities of their scientists, but they reject the notion that commercialists should receive special resources to encourage commercial success. The finite nature of laboratory space, money, and faculty means that support for one scientist's obligations may come at the expense of his or her peers. Such a scenario represents the threat that some traditionalists fear and reject: indirect "punishment" through positive discrimination. Commercialists benefit from activities that traditionalists forego or reject, at the expense of traditionalists.

The second and chief reason traditionalists oppose university allocation of rewards for commercialization concerns the consequences for scientific progress. As the organizational apparatus through which material rewards are distributed, the university influences the scientific reward system through its impact on opportunity structure in science. The recognition that motivates a career may come from the scientific community, but universities control the environments in which careers are carried out. As a consequence, universities can influence the advancement of knowledge by incentivizing particular activities or fields, potentially at the expense of others. To examine this point, consider the comments of two scientists.

Should universities reward scientists for commercialization?

> Well, I don't think I have a clear answer to that. If you have to push me to one, then I would say probably not. Somebody's research, patents and so on…they benefit from that.

> [There is a] licensing fee that can benefit their research or even their personal life. That is perfectly fine. But I don't think that commercialization should be part of promotion considerations because then one is forced to question what is more important: fundamental research or commercialization? [33]

Do you think that universities should reward scientists for commercializing their work?

> No, I don't think the university needs to reward them because I think that there's financial rewards.

So you wouldn't be in favor of the university creating incentives that encourage their scientists to commercialize their work?

> For every dollar of federal grant money I get, [this university] gets 68 cents. So without doing anything commercial, just in the course of doing what I have to do to have my research program float, I bring in a lot of money to the university. If commercial ventures are in order of magnitude more, it wouldn't be crazy for them to incentivize it. I'm fine with commercialization as long as it's sort of just a plus and not here are two potential priorities…That doesn't seem at least for a basic science department like the right priority. [54]

From the perspective of traditionalists, fundamental research is the chief priority of the university. To provide an incentive for commercialization, they claim, would constitute an undue threat to this objective. Traditionalists do exhibit a permissive view of commercialization, but such tolerance is contingent upon commercialization's status as an adjunct activity of, and not a motivation for, science. This is to say that traditionalists view commercialization as *institutionally irrelevant.* According to this view, commercialization is a legitimate ancillary activity of fundamental research, but it should not be the institutional goal. In this view, formal organizational rewards for commercialization threaten the allocation of effort to the goal of science. This view of rewards, motivation, and scientific progress is evident in the account of a biologist who hypothetically envisions how commercial rewards could influence his scientific commitments.

> I think its fine if the rewards are rewards of the sort that stimulate fundamental research. [For example] if the money I make on a patent were put not into my pocket but into my lab, I think that's fine, because then I could use that money to do fundamental research. I think if the reward encourages me to spend more time with a company or to be a little more inclined to have a student work on a project relating to the company, that is a corrupting influence. I think science should be pure in the sense that your only concern is to answer a fundamental question. If you stumble across something useful, all so much the better, but if you're always keeping your eye on the bottom line, you're not behaving the way a really good scientist behaves, I think.

Why does a good scientist need to behave that way?

> What happens in good science is that a person dukes it out with Mother Nature. She knows something that you don't know and you're trying to get it and you can't be out there in the ring with Mother Nature if you're always looking back to see if the meter is running. I think that really good science is generally done by people who focus exclusively on it and I think that everything that distracts you from that is likely to make you do less good science. I feel that very strongly. [32]

Traditionalists thus oppose incentivizing commercialization because it threatens professional commitment. According to this view, science and commercialization are opposite goals that should not compete.

Do you think that universities should reward scientists for commercialization?

> No, I don't. Why would I want to incentivize that? If they want to do it, they should do it. We have a liberal consulting policy and they should stay in that framework. If you want a company, our attitude around here is we let you go absent on leave for a couple of years, we hold your job for you, you can go out and do it and if you want to really do the company, do that and say good-bye. [85]

What are the effects of commercialization on the operation of a universalistic reward system? Merton points out that "the institution of science is but part of a larger social structure with which it is not always integrated. When the larger culture opposes universalism, the ethos of science is subjected to serious strain" (1973, p. 271). Whereas Merton's usage of the word "opposes" reflected a discussion of ethnocentrism and science, the basic point is that under certain societal circumstances, the scientific ethos is threatened by non-scientific interests. The preceding analysis suggests that the current organization of science and the economy constrains the operation of universalism. Universities stand to gain financially from the commercial outcomes of their scientists. This circumstance creates an organizational incentive to develop environments that encourage commercial pursuits. In the same way that institutional affiliation and doctoral origins are achieved statuses that may be observed to foster particularism, so too could one's status as a commercialist provide a basis for particularism.

How are scientists affected by the absence of universalistic standards of evaluation in an era of commercialized science? One possibility suggested by the interview data is that commercialization could function as a new basis for the accumulation of advantage: hiring procedures could favor doctorates from commercialist laboratories or institutions; resources for research could be unevenly distributed among scientists in ways that favor commercialists; or distribution of commercial royalties could be circulated primarily to other commercially-oriented science, for example. Commercialists and traditionalists could therefore potentially pursue careers in fundamentally different opportunity structures.

The Coles point out a question tied to this discussion: are criteria of evaluation "which are irrelevant from the point of view of the individual scientist, very much relevant to the institutional goals of the system?" (Cole and Cole 1973, p. 68). Can discriminating based on commercialization benefit science? According to Merton, "to restrict scientific careers on grounds other than lack of competence is to prejudice the furtherance of knowledge" (Merton 1973, p. 272). Commercialists believe they should be rewarded in addition to their scientific competence because their activities enhance their universities and demonstrate the value of science. The potential benefit of functionally irrelevant evaluative criteria is therefore predicated on the notion that such activities bring money and societal support to the institutional goal of science. Traditionalists, by contrast, believe that such benefits are at best short term and that rewards for commercialization distort the direction of science.

Communalism

Among the scientific norms, communalism is the most directly tied to the reward system in terms of the individual scientists' behavior (Hagstrom 1965). In order to achieve priority in discovery, scientists must share their findings, for without sharing one's results it would be impossible to achieve any credit or recognition for one's work. Communalism thus aligns individual and institutional goals by predicating the allocation of rewards upon sharing findings with one's peers.

Communalism merits close attention in analyses of commercialization for two reasons. First, there is an apparent incompatibility between sharing and secrecy. Second, the notion that knowledge is "owned" or the property of any entity is contradictory to the tenet of communalism that knowledge is the product of social collaboration and therefore, public. Given these conceptual tensions, it is no surprise that some scholars believe commercialization may undermine communalism.

Studying secrecy in science is akin to the study of scientific fraud, as practitioners of secrecy or fraud are unlikely to willingly reveal such indiscretions. Moreover, the university itself is highly protective of any instances that would potentially discredit a scientist or tarnish its own reputation. In the interviews, when scientists disclosed observations of concrete acts of secrecy, they did so coupled with appeals to the anonymous nature of the interview. Other scientists, including some who directly observed such matters while serving on conflict of interest committees, declined to disclose details. In this section, therefore, I organize the discussion around the types of constraints on communalism that emerged in the interviews, rather than organizing it according to whether a scientist is a commercialist or not.

There are five ways in which secrecy undermines the operation of communalism. The first context we consider is the review process, in which secretive behaviors entail *poaching* and *blocking*. Proposals for funding, in particular, constitute one of the earliest contexts in which preliminary ideas and findings are presented to one's peers. According to some scientists, such reviews present an opportunity for reviewers to poach ideas or undermine threats to their own agenda by blocking the progress of their peers. As the boundaries between science and commerce have been rendered more permeable, these practices may also be driven by commercial concerns. Consider the following story, shared by a commercialist chemist.

> I got a review from the NIH where an anonymous reviewer wrote ‘Well, this is all really interesting work, nice proposal, but I happen to know that there’s a company that I can’t name, that is working along similar lines, and they’re pretty far along with their technology and it really is just as good as what these people are proposing. Therefore, this grant should not be funded.’ So, unnamed company, anonymous confidential information, unnamed reviewer, clear conflict of interest, because the person is consulting with the company or else they wouldn’t have the information. They used that information to sandbag a proposal and this is accepted by the NIH reviews. Now when that happens, you know, you’ve gone too far, frankly. Okay? [92]

One interpretation of the above scenario is that the norm of universalism was undermined because non-scientific reasons factored into the evaluation of the work. Another is that the rejection may have been legitimately based in terms of “efficiency”, in order for scientists to avoid repeating work already carried out. Nevertheless, the example demonstrates how secrecy may enter science as a result of commercial interests and degrade its cooperative nature.

A second example in which commercialization undermines communalism is the way in which results are shared in meetings. In this context, secretive behaviors include *posturing* and *positioning*. Like reviews, scientific meetings provide a context in which ideas and discoveries are shared prior to formal peer review or certification as accepted knowledge. In this example, the restriction on sharing information is direct. A traditionalist chemical engineer shared the following story:

Do you perceive costs from the commercialization of research?

> When people are playing their cards close to the chest or when they're being proprietary about something, I see that as being antithetical to the pursuit of knowledge. When I see some seminar speaker from academia who says 'I'm doing this, I'm doing that, we've got some really nice results, but I can't talk about them yet because we're patenting them, or because it hasn't been released by the sponsor,' I always think, why are you bothering to waste our time with this? Talk about it when you can talk about it, but when people get into being coy about commercializing something, I feel like they're trying to have it both ways. They're trying to say I've done it, but I'm not going to tell you about it...A few of my colleagues are impressed by that and all psyched up and eager. I'm put off by it. There are negative impacts in that it leads to some positioning and some staking of territory and it's just bad behavior. I don't see a lot of real value. In a sense it slows things down if they're making a big deal out of it and then not saying anything about it…They're trying to scare away the competition. "I've already done some good stuff in this area, don't waste your time, you're behind me." And depending on what you know about the person, you decide gee, they've probably done it or you decide they're blowing smoke. [24]

This and the preceding example illustrate an important point. Science is at once communal and competitive. Scientists engage in competition with one another for priority rights in discovery, but do so by drawing upon the collective work of the community and by sharing the results of their work such that they can be replicated. To be sure, scientists point out that it is not uncommon to hold discoveries "close to the chest", referring to the tendency to delay publication until results have been verified. The perception in the preceding account evokes posturing to protect one's position in the field. This entails the "bluff", or the use of unverifiable and secretive results in an attempt to preliminarily assert one's claim to priority of discovery in an area of research.

A third example of secrecy that results from commercialization in science is interaction between scientists and the doctoral students they advise. As we will shortly see, the secretive behaviors that arise in this context commonly entail *restriction, rerouting,* or *misappropriation*. In this context, the threats to communalism may be subtle, informal, and occur in more than one way. The simplest way is that the ability of a student to publish his or her research is restricted. The following commercialist chemist's discussion of industrial sponsorship at his university is suggestive of this example.

> Can the student describe any of that research in a thesis? If it's really meant for commercial purposes, the answer to that would be, of course, no. Or, yes, but it would take two years and the student has to work in sort of this guarded way. That to me is not appropriate at all.

Should one be surprised if an academic would accept such a project?

> No, you shouldn't be. There are plenty of people who do. Or they go out and they hire a post-doc to work on that kind of problem. Post-docs are the forgotten soldiers in all this because they're not working towards a thesis and so I think in many ways, I mean, post-docs are being exploited all over this country in research labs as technicians that are basically there to get the intellectual property in place to start the company…And then there's always the question of, whose ideas really are they? [74]

The characterization of the practice of utilizing doctoral students in commercially-oriented projects as commonplace is noteworthy because the university policy prohibits it. A traditionalist chemist in the same department corroborates the point:

> I have seen people who had their graduate students actually doing research at their companies which is strictly against the rules and would have been a firing offense if it had been widely known. The students in question were severely conflicted and had a lot of problems. I'm talking about something that's really confidential, because I hear about it in ways that absolutely aren't public channels. But yeah, to the extent that flawed individuals are doing these things, they end up doing things that are injurious to their students, and that's the worst thing you can do. [70]

Although other scientists at the university considered above suggested they had not observed such practices, that commercialists at two other universities reported similar scenarios suggests that it is not uncommon.

> Sometimes people push the boundaries of not having students involved in research that is related to companies that you are involved with. You don't want your supervision of a student to be influenced by what you know as confidential to the company that might have financial benefits. [53]

> I've seen professors in our department, which is a very good department, who got carried away with commercial things and just that was their passion, but that's very rare.

How would you say somebody gets carried away?

> Well, if they start doing things in the lab that have a direct commercial thing and then tell a student not to talk to anybody. It gets an unpleasant anti-science kind of thing. I tend to be very open about ideas. I've always felt I learn more in conversation and if somebody occasionally steals an idea, usually I'm a year ahead of them anyway and so I tend to be much more open than most of the people about their work. Scientists are just like anybody else, they can get very possessive and not everybody is rigorous about crediting their rivals and all of that. [83]

The violation of communalism is detrimental to the progress of science, but the consequences for individuals vary according to one's career location. The consequences of delaying publication are highest at the outset of a career, because publications are critical to attainment of an academic appointment. Over time, particularly after the achievement of tenure, the sanctions for secrecy progressively diminish. Thus, the individual consequences for violating communalism are the highest for doctoral candidates.

What is more, the stakes vary according to the nature of how proprietary research is organized. The least deleterious circumstances are sponsored research agreements with major companies. Large corporations are able to patent quickly because they possess extensive legal resources to protect their technologies. Thus, scientists and graduate students are able to freely discuss related research among peers, at meetings, and in scientific publications. Startup companies founded by professors, by contrast, are less well-equipped. With fewer financial and legal resources at the disposal of such companies, there is an increased incentive for scientists to hold onto trade secrets. The problems this presents for commercialists and doctoral students is evident in the account of the following scientist.

> A couple of years ago, I left [my startup company] completely because I saw a major conflict of interest coming…A small company can take the attitude that they don't want the patents because the patent becomes public and if a big company wants to take it, they just can infringe on it and influence the court and throw 1,000 lawyers after one poor lawyer for the startup, so there is something to say for a small company doing trade secrets, keeping everything secret. That means that they don't want your students knowing what's going on because they're going to graduate and they're going to go to a competitor and they're going to tell them trade secrets that aren't patented, so now this whole issue with secrecy becomes a minefield because you don't have the patenting which then you can talk about afterwards. It's just indefinite trade secret. I could see the whole net and the whole web out there and before it got dicey, I just completely cut off relations with [ChemCo]. [40]

The problems presented for communalism and the implications for graduate students are equally complex in the context of the process by which scientists create their companies. Consider the scenario portrayed by a commercialist electrical engineer.

> I want to start a company and I see that there are certain things I need to put in place first. I need to do these experiments. So I say, okay, fine. I have some unrestricted funds. I have these students working on these projects that are sort of related. I'll have them do it. So I know a little bit. I take risks out of the company. First of all it's a pure scientific idea. You work on it for a while. It started to coalesce in my mind that it is actually a company now. Where do I stop the students from working on that? When do I say you can't work on it anymore? I mean that could be a problem in itself because they might have worked on it for four years, almost done, and should I stop them from working on that now, in the most profitable or in the direction that I actually think is most exciting because it actually leads to a real impact? Or instead do I sort of divert them to sort of a less fruitful avenue of investigation? Now that is real...those are real dilemmas, right? Where do you draw the line? This is a slippery slope type of a situation. I started out with a great scientific idea. I ended up with a company. [62]

Secrecy impacts thus impacts the graduate student in at least four ways. First, their ability to publish certain research may be constrained or restricted. Second, their ability to pursue problems of interest – such as those related to projects that lead to commercial outcomes – may be prevented due to the proprietary interests of a firm or their advisor. In this scenario, an advisor's interest in commercializing may lead him or her to "reroute" or divert a student away from commercially profitable intellectual property. Third, a student's ability to communicate with their advisor or other scientists may be constrained by secrecy associated with the laboratory in which they work. Fourth, in the worst scenario, is misappropriation: the ambiguity of distinguishing between publishable and proprietary research may hinder a scientist's ability to assign credit to the student for intellectual property, and for the student to recognize it themselves.

Secrecy undermines the operation of communalism in a fourth way: withholding, or the suppression of negative results. In the research policy and higher education literature, the suppression of negative results is framed as a conflict of interest (cf Blumenthal et. al 1996). From a sociological perspective, the suppression of negative findings violates communalism because this action places a cloak upon the truth. On the one hand, there is little reward associated with negative findings. Journals rarely publish such research findings, despite the fact that doing so could prevent other scientists from repeating the same failed effort. Such suppression constitutes a routine, but potentially harmless departure from communalism. On the other hand, when negative findings are suppressed to avoid undermining the market potential of commercialized research, communalism is clearly violated. Among the minority of scientists in the study who suggested this is a problem, it is a commonly held belief that the problem is limited to clinical research at university medical schools.

The final way that secrecy undermines the operation of communalism is the most obvious and the most elementary: failure to publish or discuss one's work. Among the traditionalists, withholding research results and failing to communicate with one's peers comprised the central concerns:

> I think people might hold their ideas a little more closely, not be as open about what they're trying to do. Money, particularly to people who aren't used to being rich, probably makes a lot of difference, but it could distort things a little bit. [25]
> The most negative thing [is if] the desire to meet all of the legal requirements for establishing ownership of the idea in order to patent and commercialize causes you to be secretive with your colleagues for a long time. [54]

> I think we should always have a dialogue. We should always try to find each other. Of course that's a big problem, at the moment you have commercialization, you have secrecy and there is now, now there is secrecy going over there [*referring to a biology building*]. Everybody will deny it. But when I see that the students can't get the seminar room because the whole floor is off limits, then I say this is not right, okay? This is a university. [89]

> I mean it's not that somebody is trying to get rich on the sly, but there is this question of now, you know, how much of this is publishable? [91]

At each university in the study, policies prohibit restrictions on publication of research results under any circumstance. Furthermore, the universities permit periods ranging from 30 to 90 days to allow commercialists and their sponsors to review the potential intellectual properties disclosed in a publication. They may then file invention disclosures or provisional patents, if necessary. For commercialists to engage in secretive research or fail to publish findings due to an agreement with any company not only departs from the scientific norm of communalism, it violates the policies at their university.

Commercialists provide numerous examples of how commercialization encourages secrecy in science, but when discussing their own practices, they present accounts of their work that are consistent with university policies.

> If you look at my publication record, I've never, in fact, I try to rush things out for the public use. My list of publications is probably much longer than my list of patents…So, yes, if I see something in our work, I immediately send the disclosure over to the tech transfer office, but that doesn't stop me from writing it up and getting it into the public domain because you can file a provisional application right away and your intellectual property is protected. You should not hold back that public information. [29]

> Not allowing you to talk about something at a meeting where it really is professionally important for you to do that, that's a conflict. So [my research group] talks about this and I said of course, there's a gray area where sometimes we'll say maybe we shouldn't discuss this aspect of the project, because we should try to protect that before we do that. Well, if you give us enough lead, the technology transfer office is really good about the night before you go to give a talk, filing that provisional patent. So we really work hard to make sure that people's careers are not damaged, and I would never, ever put a graduate student on a research project that wasn't publishable, that had only benefits for the company. [36]

> Holding up publications? Sometimes. We've been in the process of publishing something and we say, okay, let's wait until the patent's filed next week, so that before we send back the proofs. I mean it's not a huge delay, it's just a legality so that you don't...there's no possibility that it would be called public information before the patent is filed. [45]

> There is an element of secrecy in that there's a couple of projects that I've discussed with my students that we're not going to tell the world about until we're ready to disclose it, but the intention is always that we will publish those and the students will get papers because they have to have that, right? So I think maybe it's more of a delay in disclosure than anything else.

Do you see that as having any aggregate influence on how knowledge is produced?

> In my field I'd say it's pretty minor. By the time you get something worked out, chances are within a year, you're going to want to publish it anyway, right? So that's plenty of time to send it through an office for disclosure and then you can publish it and then it's as if you hadn't disclosed it at all. Do people keep trade secrets? Maybe. We're not supposed to do that. I would consider that unethical to publish a paper and withhold information from the experiment...do people do that? Perhaps they do. [86]

Moreover, some commercialists provide examples that illustrate the practice of communalism and a commitment to the science that informs their commercial practices. Consider, for example, the response of a commercially-oriented biophysicist when asked about tension between sharing and secrecy.

> Somewhat. I think there's merit to the tension and there is a trade off. If there are trade secrets that are not fully disclosed because a company needs time to get the advantage that they need to get because of the risk they're taking, some of that's justified. I try to separate concepts from specific implementation sorts of things, so you wouldn't share with the whole world a specific code that you're using to help [MedTechCo] get a product out there based on your science, but you shouldn't hesitate to call your colleague at the University of Utah who is using the equipment [of MedTechCo's competitor] and talk about the concepts that are going on...It's not a completely black and white area. It has to be looked at carefully, I think. [30]

This example and others we have considered suggest that communalism bears a tenuous hold on behavior among commercialists. The example makes clear that commercialists see a place for both secrecy and openness in science.

Summary

The purpose of this chapter is specify the conditions under which commercialization creates tension with traditional scientific norms. The broadest pattern revealed by the findings is that traditionalists adhere to the institutionalized norms of disinterestedness, universalism, and communalism, whereas commercialists attitudinally and/or behaviorally reject them. The chapter established this pattern by analyzing scientists' constructions of the ideal relationship between commercialization and academic science. In table 22, I summarize dimensions of adherence to the norms of disinterestedness and universalism among the commercialists and traditionalists. Commercialists' rejection of, and traditionalists' adherence to, disinterestedness is observable in

Table 22: Adherence to Disinterestedness and Universalism

	Commercialists	Traditionalists
Disinterestedness		
Problem selection	Utility	Scientific criteria
Reference groups	Industrial	Academic
Vision of scientific growth	Targeted	Undirected
Universalism		
Royalties	Legitimate	Legitimate
Professional rewards	Legitimate	Illegitimate
Rationale	Institutional relevance	Institutional irrelevance

the criteria by which they select problems, the reference groups to whom each group is committed, and their distinctive visions of scientific growth. In the view of commercialists, utility is the hallmark of excellence in scientific research. At an extreme, some commercialists frame pure intellectualism as arrogant or selfish because there is no connection to "real problems." Modally, commercialists frame their commitment to industrially-oriented research in terms of a moral obligation to society and funders of scientific research. Whether in extreme or moderate form, the emphasis on utility in problem selection renders departure from disinterestedness a pervasive feature of commercialist behavior. This pattern is exacerbated by the commercialists' commitment to industrial reference groups, whose funding encourages commercialists to work on problems that are not purely determined by the knowledge base of one's field. Commercialists reject industrial collaborations, sponsored research, and other activities that threaten their autonomy. Ultimately, however, they embrace a vision of scientific growth predicated on particular targets: societal problems. For traditionalists, by contrast, disinterestedness remains a prevalent and powerful norm for behavior, as exemplified by their rejection of the notion that scientists should be concerned with utility of their work. Utility, for traditionalists, means selecting problems according to scientific criteria that have the greatest likelihood of moving a field forward. This orientation and their rejection of industrially sponsored research reflect their commitment to fellow academics as their primary reference group. A common position among traditionalists is that industrially sponsored research is acceptable, but only if is offered without conditions. Others take the more extreme position that the mere concern for the attainment of industrial research funding pushes one away from the research problems considered most important by the scientific community. Problem selection and rejection of industrial sponsorship more broadly reflect traditionalists' commitment to undirected scientific growth, a vision these scientists base on two factors: the abundance of basic discoveries that serendipitously provided important technological developments; and scientists' inability to know what problems – practical or scientific – might be important in the future.

A similar pattern of rejection and adherence respectively characterizes the orientation of commercialists and traditionalists to the norm of universalism. The core divergence among the groups is found in the issue of whether scientists should be rewarded for their commercial activities. With only limited variance, both commercialists and traditionalists believe that the royalties derived from commercialization are a legitimate reward. Commercialists believe commercial success is a legitimate basis for promotion and other professional rewards because they see commercialization as institutionally relevant. That is, they view it as a legitimate contribution to science, and they believe that commercial success enhances individual, departmental, and organizational visibility. As a result, they construct the market as the ultimate form of peer review and hence, marketplace success, they argue, should result in career rewards. Traditionalists, by contrast, view commercial success as irrelevant to advancing knowledge and thus reject the idea that commercialists should be promoted or rewarded for their commercial endeavors, apart from royalties. The notion that commercial success is institutionally irrelevant is seen in traditionalists' views that rewards for commercialization would create the wrong priority for scientific work and cause fundamental and commercially-oriented scientific work to compete for the commitment of resources.

It is unsurprising that commercialists do not openly embrace secrecy beyond organizationally permissible periods of time during which scientists are able to delay publishing in order to file invention disclosures. In contrast to disinterestedness and universalism, commercialists do not espouse beliefs indicative of a norm that encourages secretive behavior. Moreover, the scientists in were relatively protective in disclosing details of prior incidents at their universities in which commercialists were admonished for inappropriate secretive conduct. Nevertheless, the accounts of secrecy that did surface in the interviews suggest a typology of constraints on communalism, which I present in table 23.

Table 23: Typology of Constraints to Communalism

Context	Secretive behaviors
Peer review	Poaching and blocking
Conference presentation	Posturing and positioning
Publication	Delaying and withholding
Graduate training	Restriction, rerouting, and misappropriation

In the context of peer review, reviewers' statuses as consultants or owners of intellectual property enable poaching and blocking behaviors. That is, their evaluation of scientific research may enable them to block or take ideas from research related to scientific areas for which they have commercial ties. In this respect, secrecy interacts with the departure from universalism, in that scientific work is being evaluated according to non-scientific criteria. In conference presentations, commercialists may present work that implicitly suggests they are ahead of other labs working on similar problems without providing explicit details of their findings that would substantiate their position based on appeals to intellectual property. This behavior may be understood as posturing, in that the claims could be misleading. It also constitutes positioning, in that such behaviors have the goal of laying claim to scientific turf that is yet be settled. Publication is the most typical context for secretive behavior, although delaying and withholding research results are licensed by organizational policies that permit temporary departures from communalism. The possibility for more egregious withholding of scientific discovery is heightened in the context of startup companies that are ill-equipped relative to larger corporations to protect their intellectual property. In such contexts, there is an incentive to not disclose one's finding as intellectual property.

Finally, graduate training introduces the possibility for three other secretive behaviors. First, communication between graduate students and their advisors or with other scientists may be restricted. This occurs when laboratories are engaged in research for which intellectual property has yet be filed or because relevant areas of research that occur within the advisor's commercial endeavors cannot be discussed with advised graduate students. Some scientists may reroute the research trajectories of their students when it is clear that their research is heading toward commercially-oriented research areas, either because it would delay the student or, against the preference of the advisor, move the student into an area of research for which the advisor maintains a commercial claim. Equally, as indicated by the observations of some of the scientists, a commercialist could support such a direction to the extent that a graduate student is essentially doing commercial work that benefits the company and perhaps the student, even though such practices are prohibited by universities. Finally, an egregious, but ambiguous violation of communalism in the context of graduate training is the misappropriation of ideas from graduate student research for the benefit of commercial pursuits. Occurring in conjunction with rerouting, this violation of communalism could happen when basic scientific research leads to potentially lucrative commercial trajectories from which graduate students are steered away, such that the commercialist's claim to the area is protected. These issues are ambiguous or "gray" in the eyes of commercialists because it is unclear at what point a pure scientific idea could be the basis of potential commercially-related research.

Behaviorally and attitudinally, commercialists reject disinterestedness as an appropriate norm for scientific conduct. Evidence of departure from the norm of universalism, by contrast, is primarily attitudinal. That is, commercialists believe the reward system should operate according to broader, not purely scientific criteria. The evidence, however, does not suggest behavioral departures from this norm. Nor do commercialists support secretive behavior that exceeds organizationally permissible forms, yet the findings indicate a sufficient number of accounts that suggest such behavior departures from communalism may be more prevalent than many commercialists suggest. Whether behavioral or attitudinal, departure from traditional scientific norms, and the divergence in the views of commercialists and traditionalists about science and commercialization, point to substantive intraprofessional conflict. The traditional scientific ethos, it appears, bears a tenuous hold on the structure of science.

CHAPTER 7

A FRACTURED PROFESSION

The preceding chapters have been organized with the goal of understanding how a commercially-oriented reward system operates in academic science. The first chapter specified a theoretic framework in which scientists are considered central to understanding commercialization. In the second chapter, I described the research methods and sources of data that I used to address this problem. Four empirical chapters followed. In each, I examined the role of commercial rewards through comparative analysis of the meanings that scientists assign to their work. As a result of this approach, we gain a perspective of a commercial reward system that is situated in the contexts in which commercialization occurs: scientific careers. Each empirical chapter was motivated by a distinctive theoretic concern, generated inductively using modal patterns in the data. Table 24 presents the overall modal patterns of work uncovered in the preceding four chapters.

Table 24: Overall Modal Patterns of Work among Scientists

	Commercialists	Traditionalists
Moral order of work	Professional rebellion	Professional purity
Early career view of commercialization	Dirty; elemental	Dirty; career threat
Professional identity work process	Legitimation	Disidentification
Normative orientation	Departure	Conformity

Chapter three examined the meaning of work. I identified four dimensions of work that constitute moral orders of commercialist and traditionalist science: work organization, bases of status, visibility, and dirty work designations. In examining these aspects of the scientific role, we learned that the moral orders of commercialist and traditionalist science are respectively characterized by professional rebellion and professional purity. These patterns, I will argue shortly, suggest that predominant theories of status in science require elaboration.

Chapter four considered the social mechanisms that lead scientists to depart from customary standards of professional behavior. We observed the temporal context of commercial turning points and the attractors, facilitators, and constraints to commercial trajectories. From this perspective, we saw that while all scientists who entered science before 1980 developed an early career view of commercialization as “dirty”, the meaning of commercialization shifted as subsequent cohorts of scientists began their careers. Most commercialists who entered science after 1980 viewed it as an elemental component of work, whereas traditionalists entering science during the same period viewed commercialization as a career threat or distraction.

Chapter five examined the identity work behind adaptation to, and rejection of, the commercial role. I presented the processes by which commercialists and traditionalists maintain a coherent self-identity in response to ongoing reminders of the questionable desirability of commercialization. In observing the ways in which scientists legitimate and disidentify with the commercial role, we gained an understanding of the specific processes which enable commitment to commercialist and traditionalist career paths.

Finally, chapter six considered the normative structure of science. We observed the ways in which commercialization is in tension with the traditional ethos of science through scientists’ perspectives on rewards, problem selection, and the ideal relationship between commercialization and research. We learned that traditionalists evince steadfast conformity to traditional norms of science, whereas the normative orientation of commercialists is best characterized as departure. These patterns, I will soon argue, require us to revisit prevailing arguments about a transformation of scientific norms that has resulted from commercialization.

The intellectual merit of the approach I have taken in each of these chapters is predicated on two related critical assumptions that I address at the beginning of this study. First, existing research has underemphasized the theoretic and empirical importance of the scientists whose careers constitute the commercialization of research and it has ignored altogether the importance of non-commercial scientists to understanding the commercial reward system. Second, I have argued that the only way that we may fully understand how a scientific reward system operates is to examine the success and failure of its influence. These assumptions enable an understanding of the operation of the commercial reward system that is based on the experiences of scientists who inhabit the environments in which it is pervasive.

In this chapter, I account for the empirical patterns, summarized above, that have been uncovered by this approach. I will consider the theoretic implications of the study findings. That is, the purpose of this chapter is to account for the ways in which my approach to studying commercialization enables a novel understanding of scientific work. I will divide this discussion into four sections, organized by the orienting themes and associated research questions identified at the outset of the study: norms, mechanisms, meanings, and consequences. After discussing the findings tied to these themes, I consider directions for future research and the policy implications of the study findings. The study concludes with a summative statement on its contributions.

Norms

In chapter 6, I considered the following question: *How does a commercially-oriented reward system influence the normative structure of science?* The overarching conclusion is that commercialization has engendered normative conflict in science. That is, the examination of scientists' views of the relationship between commercialization and science shows that commercialists and traditionalists embrace different conceptions of appropriate conduct in science. I considered three of the norms formulated by Merton: universalism, disinterestedness, and communalism. The degree of conflict is greatest surrounding disinterestedness and universalism, whereas all of the scientists generally expressed disdain for secretive behavior. To be sure, the accounts indicated numerous observations of secrecy in science, but none of the commercialists suggested that such behavior factored into their work, apart from publication delays considered permissible by university policy.

In previous research, scholars have suggested a different conclusion than normative conflict. The dominant position in the literature is that of a transformation hypothesis (Etzkowitz 1989,1998; Colyvas and Powell 2006; Stuart and Ding 2006). This hypothesis states that commercialization has transformed the norms of science and, in its taken-for-granted and venerated status, has become an institutionalized component of the scientific role. My findings indicate that there is a deep normative conflict that these scholars do not acknowledge because they exclude traditionalists in their analyses. Commercialization may indeed be institutionalized by virtue of both a legal mandate to do so and the location of this activity in premier universities. Nevertheless, this study makes clear that it is subject to contestation, rejection, and disapproval.

It is therefore more accurate to characterize commercialization as having contested legitimacy. Aldrich and Fiol (1994) point out that a distinction exists between socio-political legitimacy, in which actions are mandated by the state, and cultural-cognitive legitimacy, wherein actions are comprised of shared meanings used widely in sense-making. Institutionalists also argue that once practices become legitimated, they are "built into" the social order such that practices are reproduced without substantial mobilization and are resistant to contestation (Jepperson 1991). These arguments help us clarify the institutionalized status of commercialization. The cultural cognitive dimension of commercial legitimacy is much weaker than its socio-political dimension. Commercialists and traditionalists do not clearly share a conception of commercialization as desirable and appropriate. It is not the case – as Hannan and Carroll's (1992) definition of legitimacy implies – that there is little question in the minds of scientists that commercial practices are the natural way to effect the collective interest of science. Nor, as Berger and Luckman's (1967) formulation of the process of legitimation suggests, do scientists share the view that "this is how things are done" in reference to commercial practices. These positions hold among some scientists, but do not predominate within the profession. By one measure of commercial practices, patenting, evidence suggests that only 9 to 16 percent of academic scientists patent their research (National Science Board 2004; Stephan et. al 2007). Commercialization is sanctioned by law and championed by proselytizers, but it is not a shared presumption in the collective consciousness of the academic profession. Nevertheless, the socio-political legitimacy of commercialization – bolstered by the combined weight of legal mandate, corporate and university interests, and economic uncertainty – renders it resistant to cultural contestation.

The transformation hypothesis, furthermore, does not specify how exactly commercialization has transformed the normative structure of science. To a limited degree, Etzkowitz (1989;1998) implies that commercialization and traditionalism coexist, but the weight of his argument rests on the idea that traditional norms have *changed*. This suggests that one would expect to find broad departure from or rejection of Mertonian norms, whereas this study clearly indicates that among traditionalists, the norms of communalism, universalism, disinterestedness, and organized skepticism play a powerful role in their rejection of commercialization and their orientation to fundamental research. Commercialists, by contrast, reject these norms, as seen in their descriptions of what they value, their criticisms of traditional norms, and their practices.

These findings indicate that commercialization has transformed the norms of science by institutionalizing a set of counternorms, or a normative orientation that is counter to the traditional scientific ethos. As Merton argued, "A major characteristic of social institutions is that they tend to be patterned in terms of potentially conflicting pairs of norms. This sets a task for those governed by the institution to blend those imposed inconsistencies into reasonably consistent action" (Merton 1973, 33). Merton's position operates under an assumption of a singular, priority-recognition reward system. This assumption is made evident by the examples he uses to illustrate the presence of counternorms. For instance: scientists need to share discoveries, but they must do so without rushing into print. The example assumes that one could not be rewarded recognition by being secretive or delaying disclosure, but does not consider the possibility of alternative rewards for secrecy (such as money). Counternorms may be built into social roles governed by an undifferentiated reward system that requires consistent action, but the presence of differentiated roles and rewards within the same social system enables episodic and chronic adherence to counternormative behavior that is inconsistent with predominant norms.

The presence of commercial counternorms is a consequence of external influence on professional self-regulation: the Bayh-Dole act operated as a sociopolitical legitimation of commercialization that established an alternative reward system, thereby seeding the development of a corresponding alternative scientific ethos. Weber described this type of phenomenon as driven by bureaucratic rationalization:

> Bureaucratic rationalization…revolutionizes with technical means, in principle, as does every economic reorganization, 'from without': It first changes the material and social orders, and through them the people, by changing the conditions of adaptation, and perhaps the opportunities for adaptation, through a rational determination of means and ends (Weber 1921[1968], p.1116).

In this respect, the Bayh-Dole Act constituted a technical, or formalistic, means by which the social order of science could be better aligned with the interests of the prevailing political and economic order (Slaughter and Leslie 1997). Prior to the passage of the law, conditions of adaptation in the academic profession primarily entailed the patterns of adaptation characterized by scholars such as Hagstrom (1965) and Hermanowicz (2009), who describe the conditions of retreatism from the research to other roles and ritualistic publication. Hermanowicz (2009) shows that rebellion is most likely in environments that allow the greatest degrees of decoupling from the institution of science, exemplified by communitarian environments of science in which teaching tends to be valued more than research. This research thus shows new conditions of adaptation in elite environments, as the Bayh-Dole Act's decoupling of career paths from traditional means and end of science appears to have heightened rebellion as an adaptive process.

Having considered the catalyst of commercial counternorms, we must now account for the content of this orientation. In their approach to the formulation of counternorms, most scholars (Merton 1973; Mitroff 1974; Jain, George, and Maltarich 2009) draw primarily upon conceptual antithesis. Mitroff (1974), for example, proposes that the Apollo scientists he studied alternate between: interestedness and disinterestedness; secrecy and communalism; particularism and universalism; and, organized dogmatism and organized skepticism. Mitroff offers evidence to support these claims, but there is little reason why norms and counternorms must be diametrically opposed. For example, commercialists depart from disinterestedness in the ways they select problems, frame reference groups, and envision scientific growth, but these dimensions of departure are not necessarily predicated on a self-interested pursuit of financial gain – which is a common point of emphasis in the literature on commercialization (cf., Bok 2003). This is not to say that commercialists reject material rewards or do not pursue them. The point is that derivation of a counternorm using conceptual antithesis potentially introduces an alien logic that could defeat the effort to describe a particular social system. For example, to seek evidence that supports the argument that commercialists are motivated by financial self-interest would ignore the finding that commercial practices are deeply situated in a symbolic universe in which visibility is achieved through societal impact.

The approach I take to formulate the normative basis of the commercial ethos derives normative elements from commercialists' orientations to dimensions of work codified in the main findings of the study. The result, I argue, is a norm we may simply refer to as *commercialism*. Commercialism is a substantive rationality, or means-end calculation made in reference to a "value postulate," or an individual or group's preference for certain ultimate values or actions (Kalberg 1980). For commercialists, this is very clearly societal impact, as seen in the fact that this achievement constitutes the essence of eminence and visibility in science. Kalberg (1980) points out that substantive rationality entails a "valid canon" or a unique standard against which events are selected, measured, and judged. His point is that a value postulate, such as societal impact, implies clusters of values that organize action. Commercialists' orientations to the dimensions of work codified in the main findings of the study are tied to four values: control, efficiency, calculability, and predictability. I present this constellation of values and the dimensions of work from they are derived in table 25[1]. In the remainder of the discussion, I will characterize the content of commercialism by describing each of its constituent elements.

Table 25: The Norm of Commercialism

Dimension of Work	Commercialist orientation	Element of Commercialism
Professional product	Technology	Control
Work organization	Hierarchical	Efficiency
Reference groups	Industrial	Efficiency
External positions	Corporate	Efficiency
Problem selection	Utility	Calculability
Visibility sought	Societal impact	Calculability
Product aesthetic	Societal benefits/Tangibility	Calculability
Material Benefits	Legitimate	Calculability
Vision of Scientific Growth	Targeted	Predictability

Before turning to that discussion, an important theoretic point merits attention. As one can see in table 25, the elements of commercialism appear to tie to what some may characterize as bureaucratic rationality. In Kalberg's (1980) formulation of Weberian rationality, this type of rationality is formal, or means-end calculation made in reference to universally applied laws or regulations. If commercialism were a formally rational norm, commercialists' actions would be committed only in reference to calculations to the "laws of the market" or federal commercialization laws, rather than being organized by a value postulate of societal impact. But commercialists do not create technologies because the Bayh-Dole Act or university policies say they should or must. The point is that formally rational patterns of action can be fulfilled by

[1] These dimensions are drawn from tables in each empirical chapter with the exception of results describing identity work among commercialists and traditionalists, which focuses on *outcomes* of normative orientations, rather than the expectations, beliefs, or evaluations of appropriate behavior that inform group norms.

substantive rationality, a point that Kalberg makes in describing how capitalism was maintained by Puritans' ethical substantive rationality:

> The believer religiously inspired to value disciplined, methodical work and the accumulation and reinvestment of money brought a systematic component to economic activity…The Puritan's selection of the means-end rational *means* (a constellation of impersonal values) to fulfill his goal of resting secure in the certainty of salvation (a goal that could be realized only be the acquisition of wealth) eventually provided *one* impetus for the formal rational organization of economic enterprises (emphasis in original) (Kalberg 1980, p.1163).

The underlying point is that normative regularities of action uphold legitimate orders. Thus, formally-rational capitalism was made possible by the substantive rationality tied to the ethos of Protestantism (Weber 1904[1958]), just as a contemporary, formally-rational bureaucratic and capitalist order is sustained by the substantive rationality of the commercial ethos and its constellation of values: control, efficiency, calculability, and predictability. I turn to discussing each of these elements that compose a consideration of commercialism as a scientific norm.

Control

The purpose of commercialization – the reason why the end product of commercialist labor is technology – is to control uncertainty. There are three dimensions of uncertainty commercialization seeks to control: the uncertainty of societal problems; market uncertainty; and professional uncertainty. Commercialists create technologies because they believe various uncertainties in society require their expertise. Control of societal problems, as we have seen, has been a central theme in commercialists' accounts of the meaning and motivation of their work. Specifically, this element of commercialism forms one dimension of status among commercialists (alongside income and external positions). Each product that commercialists create addresses some aspect of uncertainty in society, whether it is tied to health (e.g., HIV therapeutics), the environment (e.g., biofuel), war (e.g., lightweight protective materials), or other problems that commercialists and funding organizations believe requires attention.[2]

A second, more tacit, dimension of control through commercialization is an attempt to control the uncertainty of economic markets. Market control was a key factor in the legislation of the Bayh-Dole Act and it remains important to the corporations that fund university research, to universities, federal funding agencies, and to coalitions that are connected to university commercialization. A concrete example of the concern for market control are regional economic development strategies that build within states coalitions of government, industry, and universities in an attempt to create jobs, business, and profits. A desire to control economic markets, however, *need not* motivate commercialist research. But this concern is difficult to separate from a commercial ethos, for such control is a reflection of commercial achievement. The imperative for market success rarely occupied a prominent position in commercialists' accounts, but it frequently came up in various perspectives on their work, including: the number of jobs they had created; the level of revenue their research had generated; and their belief that the market constitutes a type of peer review on the quality of their work. In this way, individual and collective commercial interests coincide in the value of control.

A final dimension of control pertains to professional uncertainty. A key uncertainty commercialization addresses for scientists is the creation of resources for research, which many

[2] Further evidence of the goal of controlling societal uncertainty is found in a report of the National Research Council called *The New Biology Initiative*. The NRC charged a panel of members from the National Academies of Science and Engineering and the Institute of Medicine to address the following question: "How can a fundamental understanding of living systems reduce uncertainty about the future of life on earth, improve human health and welfare, and lead to the wise stewardship of our planet?"

scientists recognize as the most endemic uncertainty of a scientific career. This concern figured prominently into commercialists' discussions of the material benefits of commercialization as a motivation for research. The inverse aspect of this dimension of commercialism is observed among traditionalists in late career stages who, rather than adopt any element of a commercial orientation to acquire funds for research, retreat from the research role altogether. A second way in which we observed how commercialization is tied to the control of professional uncertainty was in commercialists' discussions of societal views of science. When commercialists explained whether scientists should be concerned with the utility of their research, the notion that scientists should be able to justify their research to taxpayers reflects a general concern among commercialists in how the profession secures and sustains its mandate. Without such a justification, it is implied, the status of the academic profession in society, and thus the stability of a scientific career, is less certain (a perception tied to a broader concern for distrust of scientists in society).

Efficiency

A second element of the norm of commercialism is efficiency. Efficiency is predicated on finding the optimum means to a given end. There are three aspects of the study findings which reflect how commercialists are oriented to efficiency: hierarchical work organization; industrial reference groups; and external positions on corporate boards.

As we observed in chapter 3, one of the basic ways in which commercialist and traditionalist moral orders of work diverge rests in the organization of work. Commercialists state that commercially-oriented research is appropriate for postdoctoral scientists, but not graduate students. They therefore organize their research groups hierarchically, which provides a division of labor that optimally addresses both commercial and academic goals. The efficiency rests in the presence of a stratum of positions dedicated to commercial goals. This mode of organization may also enhance the efficiency of a commercialist to the extent that postdoctoral scientists play a role in the training of graduate students, which enables a commercialist to attend to other commitments.[3] The most extreme form of hierarchical work organization, the research institute, seeks to create efficiency through coordination of the labor process surrounding societal problems. Such institutes reflect Friedson's (1984) notion of an administrative professional elite, in which commercialists organize other professional and staff scientists around a general or specific societal problem. Under such organization, some commercialists claim, academic scientists are able to solve "big" problems that could otherwise not be solved by individual scientists.

Commercialists' orientation to industrial, rather than scientific, reference groups expresses another dimension of efficiency. Identification with industrial reference groups reflects a belief among commercialists that the most effective way to address societal problems is by "grabbing the reigns" of corporate organizations that possess the most extensive resources with which problems may be addressed (which reflects how these reference groups may also be tied to the element of control). Such resources include extensive financial resources, expensive analytic instrumentation, a reserve of industrial scientists who perform routinized scientific work considered inappropriate for graduate students, and an organizational apparatus for the distribution and marketing of products. It is in this sense that industrial sponsorship is interpreted by commercialists as enhancing, rather than distorting, the scientific endeavor. In the same respect, we see that commercialists' corporate board positions as a basis of status also reflects the

[3] For a detailed view of how hierarchical work organization creates efficiency for commercialists, see Pearson (2009).

belief among commercialists that industrial reference groups provide a key conduit through which influence on societal problems can be exercised.[4]

Calculability

A third element of the norm of commercialism is calculability, which refers to the need to be able to quantify scientific impact. Calculability is perhaps the foremost element of commercialism because of its role in problem selection. The key criterion in commercialist problem selection is utility: whether or not a problem has a material impact on society. Commercialists view the traditional outcomes of disinterested science such as contributions to knowledge or preparation of future scientists as insufficient because they lack an obvious or immediate economic impact that can be materially quantified. According to commercialists, the utility factor can be calculated, first in the form of a technological product or a company, and then in the economic and societal impact a problem actuates materially.

Calculability is intrinsically tied to the commercial ethos because performance is incentivized monetarily by universities. Consequently, visibility provides a second expression of the quantifiable value of one's work because societal impact is tied up in the universality of one's inventions. The commercialist whose invention annually brings millions of dollars to a department shines bright, whereas he or she whose patent remains unlicensed at the technology transfer office is commercially obscure. That is, the breadth of one's influence is determined by the market and, therefore, the quest for societal impact expresses the value of calculability.

[4] Although not a major finding of the study, patterns in the data suggest that the use of technology in commercially-oriented research may also be seen as contributing to efficiency. DNA-sequencing technologies provide an example in biological science, whereas combinatorial chemistry technologies represent similar tool use in chemistry. We may recall the commercialist who discussed searching through a data stream of "gibberish" for information with which he could make money, whereas others referenced "churning" and "canvassing" data without a hypothesis. Such processes are facilitated through technology that accelerate the rate by which data may be processed. Thus, the efficiency is derived from the fact that aspects of the search for technologies may be automated.

Similarly, the legitimacy of material benefits for the achievement of societal impact is an additional aspect of the value of calculability.

A fourth dimension of calculability is found in what attracts scientists to commercially oriented careers. Commercialists have an affinity for quantifiable societal impact and the tangibility of the fruits of their labor. Tangibility, we recall, refers to emphasis on the material form of one's discovery, such as seeing one's product in the everyday world, knowing patients who have been helped by one's therapeutic drug, or inventing a device present in the majority of household televisions. For commercialists, knowledge alone "goes into the ether," and thus lacks material substance, whereas technological products may be materially quantified.[5]

Predictability

A final element of commercialism is predictability. Predictability is expressed by commercialists' belief that the long term progress of knowledge should be predicated on particular targets: societal problems. The notion of predictability is embedded in commercialists' belief that research should have an *end* beyond knowledge: a point at which research should "end up." The advance of knowledge, they believe, should be directed toward sources of uncertainty in society. A second manifestation of predictability is observed in the extent to which commercialists seek to achieve societal impact by mobilizing the resources of corporations through research partnerships and officer and advisory positions. Such arrangements embody the value of predictability because corporations have existing interests in specific problems. These arrangements embody predictability because resources allocated by organizations for scientific research incentivize a particular direction.

[5] One might argue that the traditional ethos includes notions of calculability, in that scientists count numbers of papers, or citations, but the distinction rests in the *end*. Traditionalists stress the *intrinsic* value of research and are thus not concerned with calculating the value of truth, such as the financial value of the discovery of a distant planet.

Summary

Because existing scholarship has excluded non-commercial scientists in their studies, scholars have overlooked the level of normative conflict that surrounds the appropriateness of commercialization, which this study shows in great detail. What is more, such studies claim that the norms of science have transformed, yet they do not specify what norms underlie the commercial role. This research, by contrast, indicates that a counternorm of *commercialism* is the essence of the commercial ethos. It is embodied in the following elements: control of societal uncertainties; efficiency in the organization of work and resources for science; calculability of impact; and the predictability of scientific growth.

This study has therefore elaborated a longstanding assumption of scholars of commercialization in three ways. First, it indicates that traditional professional norms remain operative. They have not been transformed, as the hypothesis suggests. Second, the study shows that the transformation of norms in science is derived from the emergence of a counternorm, *commercialism*, which entails a constellation of formally rational values. Third, the study shows that this shift has caused normative conflict, but despite such conflict, commercialization remains institutionalized in academe.

Mechanisms

A second research question posed in this study is: *What are the social mechanisms by which scientists embrace commercialization?* This question is one of the least developed areas of knowledge in the literature on commercialization. Our primary understanding of what leads scientists to commercialize their work is based on Stuart and Ding's (2006) analysis of the social structural antecedents of commercial activity in academe. In that research, they show that scientists are more likely to become commercialists when they work in departments comprised of other commercialists, when they coauthor research with other commercialists, and when their commercialist peers within a department are prominent in the scientific community (Stuart and Ding 2006). But clearly, other factors shape such interactions.

Existing knowledge points to properties of work contexts that lead scientists to embrace commercial trajectories, but says little about features of the content of work or the timing at which commercial trajectories are most likely to occur. At a descriptive level, therefore, this study builds upon existing knowledge by revealing meanings assigned to work within such contexts that explain why scientists are likely to be persuaded by the influence of peers or outside industrial interests, including: social origins that value practicality; desire for societal impact; desire for tangible products of labor; commercial priority rights; and material benefits. These may be considered cultural antecedents of commercialization that mediate peer or network influences. In other words, collaboration between a commercialist and traditionalist could heighten the probability that the latter scientist would embrace a commercial trajectory, as Stuart and Ding (2006) suggest, but identification with such a career path could either be further enabled by a desire to materially impact society, or constrained by a steadfast adherence to disinterestedness, which we observed among traditionalists.

Furthermore, Stuart and Ding's (2006) study offers no cultural explanation of why, within work contexts comprised of prominent commercialists, subsets of scientists select the career paths that they do. Stated differently, their study nicely illustrates contexts of commercial contagion, but offers little by way of an explanation of how individuals get "infected." Predominant theories of deviant behavior in the sociology of science generally focus on anomie to explain such an outcome. According to this argument, scientists reject traditional modes of conduct to limit strain associated with the inability to achieve recognition. Drawing on Merton's (1957b) initial formulation of a theory of deviance, Hagstrom (1965, 228) argued that "anomy in science can be specified as the general absence of opportunities to achieve recognition." This proposition would lead us to expect that scientists embrace commercial trajectories because they are unable to achieve recognition through culturally approved means: original contributions to knowledge. The study provides mixed results for this argument. On the one hand, the anomie perspective suggests that one response to strain is rebellion, or the substitution of novel goals and means for conventional goals and means, a formulation that characterizes the modal pattern observed in chapter three. The moral order of commercialist science is characterized by a reconstruction of the means and ends of achievement in science, such that visibility is sought through invention and societal impact rather than contributions to knowledge and scientific recognition. On the other hand, commercialists never characterized their careers with disillusionment, ambivalence, or unrealized expectations. They did frame publication in mundane terms and they also actively derogated traditional aspects of the scientific enterprise such as peer review and funding mechanisms, but they never did so in connection to unfulfilled career goals. These patterns suggest that commercialists adapt through rebellion, or by reconstructing the goals and means of science, but not because they experience anomie.

This pattern leaves open a possibility that merits speculation: commercialists rebel in an anticipatory manner to avoid perceived future sources of strain. This interpretation finds support in general theories of deviance. Kaplan (1972; 1986) argues that deviant adaptations function to permit avoidance of experiences that result in self-devaluation and enable substitutions of new evaluative standards that may be more easily achieved. One reason that rebellion is observed in the absence of anomie may therefore be that commercialization provides an alternative route to, and source of, recognition in an environment of science in which expectations to achieve are pronounced, but difficult to fulfill. This form of rebellion could potentially characterize commercialists who entered science prior to 1980. Many of these scientists began their careers during the "golden age" of science and, over the course their careers, have witnessed declining funds for basic research (Geiger 1993). Chapter four showed that it was not until after career establishment that these scientists initiated a commercial trajectory. This suggests that the scientists' commercial turning points occurred during career stages in which past conditions for science were likely perceived as more favorable than an anticipated future. If this is the case, one might conclude that commercialization as professional rebellion functioned as an adaptation to anticipated strain.

The perspectives of anomie or anticipated strain as the basis of commercial trajectories illuminate potentially interesting lines of inquiry, but they lack the empirical traction of an alternative explanation that appears clearly in this study: commercialization is learned. The data in this study most clearly conform to a theory of deviance rarely considered in the sociology of science: differential association.[6] First formulated by Edwin Sutherland (1947), and then elaborated by contemporaries (Burgess and Akers 1966; Akers 1988), the principle of differential association argues that deviant behavior is learned in a process of interaction with others. The core idea is that deviant behavior is a function of exposure to definitions of behavior that are favorable to, or condemning of, departures from norms. Applied to the current study, the theory suggests that scientists learn to become commercialists or traditionalists through socialization. Scientists become commercialists by learning motives and rationalizations for

[6] This is certainly not because a differential association theory of deviance is not without merit in the study of science. The primary reason for the underutilization of this theory in the study of deviance in science is that such studies overwhelmingly focus on misconduct such as fraud and plagiarism.

action favorable to commercial practices, whereas scientists conform to traditionalist conventions through exposure to motives and rationalizations unfavorable to commercialization.

The suitability of this theoretic framework to understanding deviance in professional careers is evident in the emphasis it gives to core career concepts: temporality, rewards, and models for behavior[7].

> The probability that persons will engage in…deviant behavior is increased and the probability of their conforming to the norm is decreased when they differentially associate with others who commit [deviant] behavior and espouse definitions favorable to it, are relatively more exposed in-person or symbolically to salient…deviant models, define it as desirable or justified in a situation discriminative for the behavior, and have received in the past and anticipate the current or future situation relatively greater reward than punishment for the behavior (Akers 1998:50).

Identification with commercialist and traditionalist behaviors, I argue, is subject to an interactive learning process. Through association and identification with reference groups, scientists learn definitions, or attitudes or meanings attached to an action. Definitions include orientations, rationalizations, and other evaluative or moral attitudes that define an action as right or wrong, desirable or undesirable, and justified or unjustified (Akers and Sellers 2009).

Applied to commercialist and traditionalist careers, approving definitions favorable to commercialization are neutralizing or positive. The development of approving definitions, we recall from chapter five, plays a prevalent role in the legitimation of commercial career paths through reframing, social comparison, role distance, and professionalism. Each technique makes the commission of a commercial act justified. Reframing imbues commercialization with a positive meaning, such that scientists who once viewed commercialization as dirty learn to see it

[7] This is perhaps unsurprising, given Sutherland's interest in professions and white collar deviance.

as justified, for example, because without it, valuable inventions for society would "end up in a landfill." Role distancing justifies commercial behavior by characterizing such activities as peripheral, rather than central, components of one's professional identity. Social comparison provides an evaluative justification, by characterizing commercialization as of greater moral desirability than, for instance, publishing books. And finally, professionalism defines the commission of commercial acts as desirable because, despite the potential moral hazards that could accompany the inclusion of a profit motive in science, select scientists and universities possess the ethical "compass" necessary to act permissibly. Each of these techniques, we observed in chapter five, weakens the restraint of attitudes that are unfavorable to commercial career trajectories.

Conversely, traditionalists reject commercial career trajectories because they possess definitions unfavorable to commercial behavior. The definition of commercialization as undesirable is seen in the techniques that traditionalists employ to affirm their own purity and reject association with commercial referents: social comparison, disavowal, ritual identification, and retreatism. Social comparison, for traditionalists, disparages commercialization by framing commercialists as having less autonomy in the pursuit of problems relative to traditionalists. Disavowal defines commercialization as undesirable by framing commercial incentives as unpersuasive. Ritual identification renounces commercialization: traditionalists may be required to make overtures to societal impacts in their work, but they nevertheless define this as undesirable, rather than accepting the commission of such acts as valid. Finally, retreatism defines commercial behavior as so undesirable that scientists in late career stages would prefer to retreat from the research role to avoid any participation, symbolic or otherwise, in commercialist science.

The development of favorable or unfavorable definitions of commercialization is primarily influenced by the reference groups with which scientists are in differential association. Primary reference groups include the research groups in which one receives doctoral and postdoctoral training (represented primarily by the advisor), one's scientific peers (departmental and otherwise), and one's collaborators (often exemplified primarily by one's research group). Other groups, such as university administrators, corporate executives, industrial scientists, venture capitalists, and one's family, may also have varying degrees of influence on conformity and deviance.

Reference groups and their influence vary as the scientific career unfolds. This is due to differential levels of priority, duration, frequency, and intensity of association (Sutherland 1947; Burgess and Akers 1966). That is, the earlier association occurs (priority), the longer it lasts (duration), the number of times it occurs (frequency), and the importance of the individuals with whom one is associated (intensity), the greater the effect that an association will have on behavior. There is a temporal process by which primary associations are most influential on conformity and deviance.

Given the high intensity and priority of doctoral training, the meanings and career conceptions to which one is exposed during this stage bear an imprint on the future in a powerful manner. For commercialists who entered science after 1980, this is evident in the fact the majority of these scientists (71 percent) were exposed to a commercialist conception of science as doctoral or postdoctoral scientists. From the onset of their careers, commercialization was an elemental component of socialization. As we saw in chapter four, factors such as tangibility and societal impact were influential early on in their careers. Similarly, we observed that the initial commercial trajectory in these scientists' careers occurred prior to the conferral of tenure (5 years after PhD), followed by an acceleration of commercial involvement and the establishment of industrial reference groups: the typical commercialist among this group started his or her first company eleven years after completion of the PhD. Why does this pattern occur? It is not until after tenure that priority and intensity of academic reference groups could recede, because it is only then that one faces lower sanctions for deviance from conventional norms. Even if a commercialist is situated in a commercially-intensive department in his or her first appointment, organized skepticism is socially-arranged in the scientific community at-large. Consequently, there could be circumstances in which an abundance of approval for commercialization exists locally, but prior to tenure, one must nevertheless demonstrate one's worth as a scientist externally, through publication and citation. For this group of scientists, therefore, the imprint of a commercialist mentor catalyzes the commercial career, the paths of which are unleashed as scientists achieve tenure. At this point, commercialists are "free" to align themselves with industrial reference groups because the intensity and priority of pre-tenure reference groups recedes. In short, the lowering of sanctions for departure from conventional career paths allows these commercialists to enact the careers modeled by commercially-oriented reference groups of early career stages.

Scientists beginning before 1980, who are trained under traditionalists and commercialists, identify with commercial career paths because socialization is ongoing and reference groups shift. Thus, the priority and intensity of commercialist departmental peers or industrial sponsors will ultimately compete with, and potentially outweigh, the influence of one's advisor, but traditionalist socialization bears a powerful imprint. For example, the five commercialists who trained under traditionalists, on average, started their first company 18 years after completing their PhD. This is noteworthy because the year of company establishment falls at the midpoint between the average among commercialists who entered science before 1980 (company established at a scientific age of 24, on average) and the other commercialists whose careers began after the Bayh-Dole Act (company established at a scientific age of 11, on average). One might speculate that the difference is explained by whether one undergoes training in an ideologically-closed context of socialization in which commercialization is defined as dirty or an ideologically plural context in which a broader acceptability of commercialization may exist. What can be said with certainty, however, is that for these categories of scientists, favorable definitions are developed much later in the career – a point we considered closely in chapter five in examining commercialists in the process of establishing companies for the first time (as associate professors) and more senior commercialists' comparisons of early and late career stage definitions of commercialization.

For traditionalists, lower sanctions for deviance from convention may not perturb existing levels of priority, duration, frequency, and intensity of scientific reference groups. A likely explanation is that the absence of punishments for departure from the traditionalist role is not influential on the rewards for adherence, either because of past success or anticipated future rewards. Similarly, adherence to traditionalist roles may be reinforced through the allocation of "punishments" for departure. In chapter four, for example, we examined a pattern among traditionalists in which interaction with industrial reference groups resulted in "wastes of time" and "disasters." Among traditionalists trained after 1980, a similar pattern of reinforcement was considered in which "punishment" is anticipated, insofar as the exposure to commercialization resulted in the development of unfavorable definitions of commercialization.

Summary

Whereas Stuart and Ding's (2006) study points to the importance of work context in the explanation of why scientists become commercialists, the present study builds upon knowledge by showing what occurs in such contexts that leads to career path identification: the development of definitions favorable and unfavorable to commercialization. Peer influence operates through the transmission of rationalizations, meanings, and orientations to commercialization that lead scientists to adopt a favorable or unfavorable view of commercial practices. Favorable definitions lower normative restraints of commercialization and foster the adoption of the formally rational values of commercialism, whereas unfavorable definitions create or ossify normative opposition to commercial practices. Because meanings of work in the organizational contexts of science are equivocal, reference groups with whom scientists differentially associate play a critical role in whether one develops favorable or unfavorable definitions of commercialization. What is more, the timing of differential association plays a critical role, which is seen in the fact that early definitions forged during the crucible of identity formation are difficult to destabilize.

Meanings

How does the contemporary reward system shape the meanings and patterns of scientific careers? Some studies indirectly inform our understanding of the meanings of commercialization, but three in particular directly address this theme (Owen Smith and Powell 2001; Kleinman and Vallas 2001; Lam 2010). There are two general characteristics of these studies. First, they give limited or no emphasis to non-commercial scientists' interpretations of science. Lam (2010), for example, interviewed 34 scientists with commercial involvement, three of which she inductively categorized as “traditional” or of the belief that academe and industry should be separate. Owen-Smith and Powell (2001) interviewed 80 scientists, but exclusively focused on one specific traditionalist (in their terminology, “old school”) in the presentation of their results. Kleinman and Vallas (2001) advance a theoretic argument about meaning through a synthesis of studies, rather than by drawing upon empirical evidence.

Second, perhaps because of the limited analytic inclusion of traditionalists in their research, these studies generally stress convergence in the meanings of academe and industry. Vallas and Kleinman (2001) speculate that the normative codes and practices of industry have empirically combined in ways that produce structures of knowledge characterized by “asymmetrical convergence.” Owen-Smith and Powell (2001) similarly stress convergence in saying that academic and commercial life scientists are members of a “single technological community.” Lam (2010), though she nicely illustrates aspects of boundary work employed by the scientists in her study, concludes that scientists are collectively attempting to create coherent professional identity without undermining the core logic of academic science. These views stress acceptability and incorporation of industrially related interpretive schemes and understandings of science.

This study offers a contribution that departs from a general conclusion of convergence in the meanings of commercialism and traditionalism. I do so through inductive derivation of dimensions of work along which commercialists and traditionalists diverge. In contrast to approaches that seek to understand the meaning of commercialization through analysis of boundaries of commercialism and traditionalism (Owen Smith and Powell 2001; Lam 2010), the findings produced by this study are generated by analysis of the core of each domain. That is, the meaning of work in this approach is arrived at through comparative analysis of scientists’ descriptions of what they value and disparage – whether or not that is specifically tied to elements of commercialism – rather than relying primarily on responses to directed questions about whether science and industry should be distinct. The latter types of questions are important, and employed in this study, but played only a muted role in the analysis of meaning. The result is that we observe new career scripts and status beliefs in science. In the remainder of this section, I consider each element – contested orders and career scripts – in turn.

Contested Orders and Career Scripts

In chapter three, we observed a contest among orders of science, embodied in divergent modes of work organization, constructions of visibility, dirty work designations, and bases of status among traditionalists and commercialists. The contest centers on scientists' views of what science "is": how career paths within it should be constructed, what constitutes attainment, and how rewards should be allocated. We see, furthermore, that in addition to the ways in which moral orders of science are distinctive across organizational contexts of science (Hermanowicz 1998, 2009), the presence of competing reward systems *severs* the stability of moral orders within specific organizational contexts, thereby fostering conflicting interpretations of careers.[8] As a result, the meaning of an elite career in science is now characterized by competing career scripts and institutional logics. To conceptually formulate such distinctions, it is therefore critical to consider the objective and subjective dimensions of careers. After doing so, I consider the implications of contested orders and career scripts for status in science.

[8] Hermanowicz's (1998; 2009) research, which focuses solely on the traditionalist reward system, demonstrates that differentiation of the academic profession occurs across three contexts of science: elite, pluralist, and communitarian. The role set of science is critical to this conceptualization because moral orders are organized around valued actions and beliefs: elites emphasize research in the presence of teaching; communitarians emphasize teaching in the presence of research; and pluralists are relatively committed to both aspects of the academic role. When we consider the influence of *competing* reward systems on the constitution of moral orders, we see that fissures of differentiation in science occur not only between roles, but also within them. That is, two elite scientists within the same department may be characterized by drastically different roles and identities, such that the career paths they pursue diverge more than they overlap.

The sociological notion of a career and Anthony Giddens's (1984) theory of structuration share an emphasis in how institutions jointly constitute and are constituted by individuals living their daily lives (Barley 1989). In Giddens's theory, social organization includes institutional and interactional realms. The institutional realm reflects a social system's logic, or an abstract framework of objective social relations comprised of signification (symbolic codes), domination (power structures), and legitimation (moral mandates). The interactional realm, by contrast, refers to arrangements of individuals as life unfolds within a system. At this subjective level, signification, domination, and legitimation respectively inform specific acts of communication, power, and sanction. According to Giddens (1984), the influence of institutional and interactional realms on one another produce modalities comprised of interpretive schemes, resources, and norms, or as Barley (1989) argues, a set of career scripts that encode contextually appropriate behaviors and perceptions.

I draw on this conceptual framework to illustrate how moral orders organized by traditionalist and commercialist career scripts are tied to conflicting institutional logics. I begin with the traditionalist career script, presented in Figure 2, because it serves as the baseline from which commercialists careers depart. The institutional form of legitimation among traditionalists is the discovery and transmission of knowledge – the moral mandate of traditionalist science. The institutional realm of legitimation informs the interactional realm to the degree that traditionalist norms encode the behaviors of scientists. In other words, the enactment of communalism, universalism, disinterestedness, and organized skepticism reproduces the institutional logic of science and allocates recognition to scientists who in the interactional realm uphold such norms. The institutional form of domination refers to the power structure of traditionalist science. Power among traditionalists connotes one's location in the social system

Figure 2: Traditionalist career script

Institutional Realm	**Legitimation** Discovery and transmission of knowledge	**Domination** Knowledge-based power structure	**Signification** Eponymy, immortality, scientific visibility
Modalities / Career scripts	***Norms*** Traditionalism: communalism, universalism disinterestedness, organized skepticism	***Resources*** Contributions to knowledge	***Interpretive schemes*** Affirmation of purity
Interactional realm	**Sanction** Distribution of recognition	**Power** Institutional location, academic rank, federal funding, research resources	**Communication** Eminence through scientific impact

and the resources with which one wields influence. The essence of power is thus derived from one's contributions to knowledge, the "units" through which scientists interactively achieve tangible power and exercise authority, found in the caliber of one's institution, one's academic rank, and federal and organizational resources for research. Upholding the institutional mandate through contributions to knowledge results in material power, but it is equally tied to a rich symbolic universe in which the objective signification of power is found in scientific visibility. Eponymy and immortality, for instance, institutionally operate as a system of symbolic codes that encode how traditionalists make sense of their role. At the interactive level, this is manifested in an expression of achievement that is at once simple to understand and difficult to fulfill: "change how we think." The result is an interpretive career modality in which identity construction revolves around the affirmation of purity, so as to avoid the sanctions that result from a perception that one is committed to anything other than the advance of knowledge. In sum, the traditionalist career script is comprised of Mertonian norms, communal knowledge resources upon which achievement is based, and an interpretive scheme that stresses working within the organizing knowledge of the profession. These are the modalities, or an actor's stock of "practical knowledge," that traditionalists draw upon as they construct a career within academe.

The impact of the commercially-oriented reward system is seen in both the ways in which a commercialist career script departs from the modalities of traditionalist careers and the corresponding differences in the institutional and interactive logics of commercialization. I present the commercialist career script in figure 3. The moral mandate of commercialist science is to control uncertainty through the development of technologies that address societal problems and lead to economic development. This institutional mandate encodes the behavior of commercialists through the allocation of financial rewards for adherence to the elements of commercialism – control, calculability, efficiency, and predictability. Just as the traditionalist is unable to receive recognition without adherence to communalism, the commercialist is unable to receive financial rewards if they do not adhere to calculability, or the achievement of a quantifiable material impact on society. The institutional form of domination of commercialist science is economically-based. At the interactive level, this means that commercialist power and authority is tied to financial and industrial resources that can be mobilized toward particular problems. Such power is therefore derived from the level of industrial funding and the power of the corporate firms through which commercialists wield authoritative influence as advisors, directors, officers, and founders. Such interactive power is achieved through the essence of the commercialist role: the creation of commercial intellectual property in the form of patents. With

Figure 3: Commercialist career script

Institutional Realm	**Legitimation** Control of societal and market uncertainty	**Domination** Economic-based power structure	**Signification** Commercial visibility
	↕	↕	↕
Modalities / Career scripts	***Norms*** Commercialism: control, calculability, predictability, and efficiency	***Resources*** Commercial intellectual property (patents)	***Interpretive schemes*** Legitimation of commercialism
	↕	↕	↕
Interactional realm	**Sanction** Distribution of wealth	**Power** Corporate positions; industrial funding; industrial alliance.	**Communication** Eminence through societal impact

respect to the institutional form of signification among commercialists, the chief symbolic code of commercialism is visibility through societal impact. This institutional logic informs interactive communication in the ways in which commercialists communicate the meaning of their role. Career scripts among commercialists perpetuate the institutional logic through a rich interpretive scheme of techniques that legitimate working outside of the organizing knowledge of the profession. The career script of commercialist careers is thus represented by formally rational values of the norm of commercialism, the acquisition of intellectual property through which power may be attained, and an interpretive scheme that equates eminence with working in occupational turf ancillary to the profession's knowledge base.

Distinctive traditionalist and commercialist career scripts are not simply alternative paths taken in science. They are tied to a restructuring of the institution of science and status within it.

The significance of traditional status attainment in science, much like the professional identity to which it corresponds, is destabilized by the presence of a competing alternative. Conceptually, commercialist and traditionalist attainment could coexist within science without disrupting power relations. This is unlikely, however, not only because of the tendency of social systems to organize around hierarchy rather than egalitarian structural forms, but because of the evidence presented in this study tied to the allocation of rewards. I now turn to a consideration of commercialization and status in science.

Status Beliefs

This study has a suggested a new status distinction in science: commercial attainment. Commercial attainment as the basis of scientific status is of importance to social inequality, for status yields influence. Status provides an evaluative hierarchy between social groups (Weber 1968) and between individuals (Ridgeway and Walker 1995; Goffman 1967). Competing career scripts are tied to status beliefs held among commercialists and traditionalists that associate greater social esteem and value with people in one group over another.

According to Merton's (1973) and Abbott's (1981) "purity thesis", greater social esteem, and thus power, are accorded to professionals who work within the organizing knowledge of a profession than to those whose work entails factors extraneous to the knowledge base. Clients, applications, and extrascientific motives, in this view, are impure and professionally defiling. The thesis therefore suggests that, by virtue of commercialists' orientations to external reference groups, the centrality of application to their work, and material dimensions of motivation, traditionalist attainment would occupy the upper strata in a scientific status hierarchy. It appears, however, that a new status hierarchy has emerged in science.

Status hierarchies among individuals are organized according to how distinguishing attributes, for instance whether one is a commercialist or traditionalist, evoke shared beliefs about the groups to which they belong. By virtue of its presence, and to the extent that it can be associated with variance in conditions of work, definitions of worth, and the allocation of rewards, commercial attainment therefore threatens traditional status in science in its potential function as a significant axis along which social relations are organized in science. Conceptually, therefore, the presence of commercial attainment as a new form of status in science suggests a fertile context for *competing* status hierarchies. As Ridgeway (2006) points out, simple in-group favoritism does not create a status belief since it results in competing perspectives about which group is better. A status belief forms when both those in the social category favored in the status belief and those in a disfavored category agree, as a matter of social reality, that group members of the favored category are accorded greater respect than those in the disfavored group. The question therefore emerges: do traditionalists concede that commercialists are seen within the profession and society as better than traditionalists? Without question: although they do not personally endorse such a view, traditionalists view the major scientific organizations, universities and funding agencies, as favoring commercialists. Traditionalists similarly believe that public citizens lack an appreciation for the value of basic science. Commercial attainment thus functions as a status belief with significant implications for social relations within the academic profession.

What evidence in the present study is suggestive of the ascent of commercial attainment in the scientific status hierarchy? First, the pervasive concern among traditionalists that commercialists' commitment to corporate firms shifts ancillary role "burdens" such as departmental committee responsibilities and teaching to traditionalists. Similarly, the belief held by some traditionalists that commercialists can "write their own check." Both patterns suggest the potential for unequal conditions of work that vary by workload and nature of task. A pattern that potentially exacerbates variance in the conditions of work is the extent to which commercialists may occupy positions with commercially tied or funded research institutes that alleviate scientists of departmental obligations. Second, it appears that commercialists, independent of their royalties, earn higher salaries than traditionalists. With royalties, which are frequently higher than a nine-month academic salary, commercialists very clearly earn more than traditionalists. Such distinctions reflect differing levels of status, which could potentially have implications for job satisfaction among traditionalists who may view themselves as less valued by their universities. Third, the tendency of traditionalists to view themselves as threatened by the emphasis placed on commercialization, both by the funders of research and by universities, suggests the ascent of commercial attainment in the status hierarchy. Such a pattern colors much of the data from traditionalist interviews and is seen particularly in the techniques traditionalists employ to disassociate themselves from commercialist referents, especially retreatism.

If inhabitants of pervasive commercial contexts appear to acknowledge the presence of a status hierarchy, a question that follows is whether such status beliefs spread widely within the academic community. As Ridgeway and Balkwell point out (1997), such an outcome depends on structural conditions that shape how people from each categorical group encounter one another. One factor Ridgeway and Balkwell (1997) point to is material resources that systematically advantage one group over the other. Two such structural conditions support diffusion of the belief that commercial attainment is favorable to traditionalist attainment. First, the disparity between funding for commercially-targeted science and fundamental science could create a general recognition that commercialists are more valued. A second factor could be the recognition that commercialists tend to be concentrated in elite research universities. Universities considered to be elite commercialist institutions such as Caltech, MIT, Harvard, and Wisconsin are both associated with commercial institutional identities and are materially richer than more traditionalist institutions, which could perpetuate commercial attainment as a status belief.

Belief formation in local contexts likely occurs within the context of professional socialization under established commercialists and through the social psychological processes considered in chapter five. In the context of professional socialization, acquisition of the belief in the superiority of commercial attainment may come through an association of a scientist's standing in the local influence hierarchy with commercial status as a salient categorical difference. For example, if doctoral students see that commercialists have larger lab operations and more funding for research, they may associate such dimensions of influence with their advisor's commercial status. Even graduate students trained under traditionalists could form similar beliefs. What matters is that individuals in the process of socialization are exposed to situations that lead them to develop the view that commercial status is associated with higher standing and power. Thus, the presence of prolific commercialists within one's department or university could contribute to this belief, even if such exposure does not occur within one's research group.

Whereas such associations may allow commercial status beliefs to develop, the identity construction processes revealed in chapter five may contribute to the diffusion of these beliefs. Consider social comparison, for example. Such a technique not only seeks to reverse the attribution of "dirtiness" to commercialist work, it seeks to project the superiority of commercialism over traditionalism by disparaging referents of traditionalist science.

Interactively, therefore, identity construction processes attempt to convince individuals of the favorable nature of commercialization in a status hierarchy.

Summary

Career scripts are rarely singular due to variation in the environmental properties of the organizational contexts in which they unfold. Hermanowicz (1998;2009) shows how variation in what is valued within the scientific role set creates unique career scripts. This research reveals fissures that occur when a singular role is differentially valued. Whereas existing research generally stresses convergence in the meaning of commercialist and traditionalist science, I demonstrate that a multidimensional level of divergence pervades these realms within academe. The sociopolitical legitimation of an institutional logic predicated on technology and economic development has fostered competing moral mandates, power structures, and symbolic universes in science. These elements of institutional logic shape and are shaped by the daily lives of scientists, as they seek to influence their world, the ways in which they are rewarded for doing so, and the meanings they assign to their work. The result is the existence of competing career scripts characterized by conflicting norms, interpretive schemes, and forms of power and authority. The essence of such conflict is found in a status contest that stands to reverse the meaning of attainment in science and the ways in which such attainment is converted into power. Moreover, in showing the ascent of commercial attainment has unsettled, if not surpassed traditional scientific attainment, as evidenced by scientists' interpretations, this research shows that a purity thesis of professional attainment finds little support in academic science.

Consequences

What are the consequences of a commercially-oriented reward system for the institution of science, universities, and the academic profession? The consequence of commercial rewards in academic science, as I have shown so far, is the creation of normative conflict, conflicting professional identities, conflicting career scripts, and competing status hierarchies. The resulting image that emerges in such an analysis is a fractured profession and thus a tension – played out in the context of scientific careers – that is reshaping higher education. Here, I consider four broad ways commercialization is changing scientific work in U.S. universities.

First, *commercialization has caused the academic profession to cede power over the direction of knowledge production*. The degree to which control has been relinquished is unclear, but it is unquestionable that industrial sponsorship, commercially-oriented federal sponsorship, and lower levels of consensus within fields surrounding problem selection undermine the ability of the profession to exclusively designate the direction of knowledge. In short, science is more susceptible to non-professional problems. Discussing social science research, Klima (1972) and Ben-David (1974) emphasize that discontinuities in this area that are driven by the fact that problem selection in social science is heavily influenced by swings of public opinion (i.e., the "problems of the day"). The underlying point is that inquiry driven by external rather than internal logic creates dissension within fields. In this logic, there is little difference between studying discrimination in the workplace or therapies for arthritis. Both constitute external rather than internal logics of inquiry. Science has always been subject to fads, yet it can be said that in the past, the origins of fads in science were primarily internal – the result of paradigmatic thought and theories. Commercialization accelerates faddism, but does so through an externally driven logic through the creation of rewards for successfully solving problems that matter more to society rather than advancing one's field. In some instances, the two ends – societal impact and advancing the frontier of one's field – may be coincident. History offers an abundance of examples of such discoveries. The difference now, however, is that what was once considered the fruit of science, has now become its end.

A related, second, pattern reshaping higher education is that *commercialization ruptures consensus – even in what are characteristically viewed as "high-consensus" fields*. Consensus refers to the extent to which scientists within a field exhibit agreement with respect to problems that merit research and the methodological and theoretical frameworks utilized in science. Hermanowicz (2009; 2011) includes definitions of success as another referent of agreement within this list. Commercialization undermines consensus by creating conflict in problem selection. Such conflict has two dimensions: whether or not scientists should consider the utility of their research; and whether success is defined by scientific or societal impact. One implication of this outcome could be greater difficulty in attaining funds for research if particularistic criteria compete with universalistic criteria in proposal evaluation. The relevancy of a problem to societal impact, for example, could undermine the potential of a project that otherwise meets theoretical and methodological merit considerations. A second implication suggested by Hermanowicz (2009, 2011) is that lower consensus may result in a greater number of scientists considered "elite" due to multiple standards of success by which achievement may be judged (commercial attainment and traditional attainment). However, although there may be more ways to define oneself as successful, this may not lead to greater job satisfaction in science. On the one hand, traditionalists may succeed according to longstanding definitions of achievement in science, but could do so in the context of unequal distribution of rewards and conditions of work if universities assign more value to commercial work. On the other hand, job satisfaction could increase to the extent that industrial sponsorship provides an alternative to retreatism from the research role. Some scientists who are unable to attain federal funding for research, for example, could be content or happy to work on industrially-sponsored problems over the alternative of not conducting research. Such a possibility would not appear to characterize the traditionalists in this study, but could hold true in contexts of science wherein levels of federal funding are typically lower. This suggests one line of inquiry for future research.

A third pattern by which commercialization is reshaping higher education is its *potential impact on accountability*. Commercialization provides new ways to define success in science, but in doing so, it may ironically set higher education up for failure and intensified scrutiny. The ability of academic science to discover and transmit knowledge is unquestionable, as seen in the level of research output generated by universities (both "bricks in the edifice" of knowledge and transformative findings). But it is also difficult to evaluate externally. The notion that the public lacks an ability to understand knowledge carries the benefit for the profession of autonomous progression toward the goal of advancing knowledge.

The control of societal problems, by contrast, provides benchmarks of success more easily understood by the public and funders of science. Such metrics could include technological therapies to societal problems, jobs, or revenue. In accepting the control of societal and economic problems as part of its mandate, the academic profession may potentially subject itself to greater scrutiny, and greater likelihood of failure, for having embraced a goal it is not traditionally designed to achieve. Stated differently, the knowledge base of the profession is organized in terms of the discovery of new knowledge, not the creation of new things. Arguably, universities are better at creating knowledge than they are jobs and societal solutions. This argument does not discount numerous examples of commercial and societal success that have resulted from academic science, but the magnitude of societal problems and the rarity of blockbuster technologies suggests a lofty mandate for higher education.

The irony of this situation is that the segment of the profession that rejects commercial goals may be the most likely to suffer the consequences that result from commercialists' failure to fulfill such goals. This possibility is suggested by instances of public disparagement of science, in which politicians, media, and citizens mock the legitimacy of basic science. The point made in such cases is that it is a waste of taxpayers' dollars to support scientific research not characterized by utility. Conversely, broadening the standards of success could enhance the public's view of science. The legitimacy of science in the public eye could be bolstered to the extent that commercial products and economic development express the value of the scientific endeavor. This could have a positive impact on the autonomy of the profession in that regional or national economic success stemming from university science may lower public criticism and encourage the allocation of greater resources at the federal and state level to universities.

Fourth, the ceding of control, rupturing of consensus, and likelihood of heightened accountability together embody a broad consequence: *professional polarization*. The academic profession is fractured because, positioned at the center of a tug of war between the production of knowledge and societal/economic control, its internal structure is subject to strain. We have already considered the ways in which such strain produces cultural cognitive conflict, new norms, and new career paths. The level of analysis to which we must now attend is the objective structural consequences that result. The chief pattern we have observed is a division between a segment of the academic profession that seeks exclusive professional control over the definition and coordination of work and another that views market and bureaucratic forms of control as enhancing rather than constraining their power.

Numerous sociologists have formulated various typologies that characterize such institutional logics. On the independent professional control side are notions of collegiate occupations (Johnson 1972), occupational control (Friedson 2001), and independent reward systems (Crane 1976). The underlying characteristic of such organization is that professions define the needs of consumers, the manner in which such needs are fulfilled, and the processes by which rewards are allocated. Formulations of control that are counter to professional control include patronage occupations (Johnson 1972), market and bureaucratic control (Friedson 2001), and heterocultural reward systems (Crane 1976). The institutional logic of control characterized in these formulations describes a situation in which the needs, coordination of work, and rewards are heavily influenced or determined by consumers or superordinate, market-oriented organizations.

Academe is polarized between these antinomies. However, neither segment of the profession is cleanly situated in either an exclusively professional- or bureaucratically-controlled social position. On the one hand, traditionalists are subject to the influence of their federal patrons. On the other, commercialists are not automaton-like merchants of capitalism. Both segments exercise levels of technical and coordinative autonomy rare even among the professions. In short, the important point here is not the nominal status of academe as a profession. Rather, the concern centers on social disorganization. Academe is characterized by what Abbott (1988) would refer to as an internal jurisdictional conflict, the implications of which include a lower ability to control its members and a less effective claim to jurisdiction. With respect to a lower ability to control its members, there are two associated modal patterns. First, we observed a shift in contexts of professional socialization from ideological closure (uniformity) to ideological pluralism (differentiation). Second, in examining distinctions of career paths of commercialists, we observed that commercialists who entered academe after the Bayh-Dole Act generally depart from traditional modes of conduct much earlier than senior commercialists. Such patterns suggest long-term consequences tied to cultural diffusion in which labor markets could increasingly distribute commercialists across institutions of science, leading to more widespread adoption of economic development as a mission of universities. A corollary of such a pattern is found in the increasing emphasis on research in universities in which teaching is a core value, but research is nevertheless expected. Professional self-regulation begins with socialization of aspirant members. It is the process by which a profession reproduces itself. Therefore, heterogeneity in socialization processes suggests a profession that is guaranteed to slowly transform over time, away from tradition.

Whether or not polarization implies a less effective claim to jurisdictional control is largely tied to two factors. The first, over which the academic profession wields influence, is the extent to which the profession integrates corporate patronage into its turf. This is a two-sided issue. On the one hand, successful commercialists do not interpret their alliances with industry as an encroachment on their professional control. As I have demonstrated, they believe such alignment enhances the reach and quality of their scientific work. On the other hand, if the integration of industrial support becomes a growing form of patronage in science that competes with federal funding, such a dynamic would indeed constitute encroachment on professional control. Such a funding structure would present a scenario in which scientists who otherwise reject commercially-oriented work wield less influence on the definition of problems that should be studied and the manner in which such work is carried out. This dimension evokes the second factor that influences the efficacy of academe's claim to jurisdictional control, one over which it wields relatively little influence: the economic outlook for higher education. Schuster (2011) notes a “checklist” of financial uncertainties pervade higher education in times of economic uncertainty: falling endowment values, lower state appropriations, lower-enrollment revenues, and federal budgetary pressures. The continuation of this economic circumstance is to the detriment of both commercialists and traditionalists, because both federal funding agencies and research-intensive corporations are subject to economic downturns. But traditionalists are likely to suffer more, if only because commercialists have alternative (industrial) sources of funding they would accept with little hesitation. The perpetuation of an economic downturn could thus destabilize the jurisdictional control of the academic profession to the extent that the opportunity to perform research is increasingly tied to whether one can attain industrial funding. In this instance, the primary avenue of encroachment is tied to problem selection, more than technical control of work.

Summary

Existing research has focused primarily on consequences of commercialization tied to research integrity and conflicts of interest. This study has considered such patterns, but given that the weight of the analysis centers on work and careers, the research leads us to consider underemphasized consequences that result from commercialization. I have highlighted four ways commercialization is reshaping higher education. First, commercial rewards lead the profession to relinquish power over problem selection to external influence. Second, commercial rewards disrupt consensus. The advantage of greater dissensus may entail broader job satisfaction due to more flexible definitions of success, but the costs of this could include the inefficient advance of knowledge. Third, in accepting a commercially-oriented mandate, the academic profession potentially subjects itself to public scrutiny and accountability. This could be advantageous in that successful fulfillment of societal and economic goals could enhance the legitimacy of science and potentially result in greater social and material support. On the other hand, failure to live up to these objectives could be detrimental to material support for science, particularly for traditionalists who eschew or reject commercially-oriented research. Finally, these trends amount to a fractured profession, polarized by adherence to distinctive visions of science. Increasing polarization may lead to both greater social disorganization and a less effective claim to jurisdictional control of work.

Directions for Future Research

This study suggests three paths for future inquiry. First, having identified a basis for stratification in higher education, future research should investigate how commercial status is converted into patterned social inequalities, by which I am referring to differences in power possessed by commercialists and traditionalists. The chief question is whether commercialists are rewarded more than traditionalists. Inequality of rewards could include the distribution of income, organizational resources, access to influential positions, and honor or deference. Special attention would need to be paid to differences in the allocation of rewards across fields. We have observed that commercial rewards enable the disruption of consensus within fields, such as chemistry, that exhibit greater consensus relative to other academic fields. Nevertheless, future research should pay attention to how commercial status interacts with varying levels of consensus across fields.

Two models for studying stratification in professions provide avenues by which investigations could proceed. One approach is found in studies such as Cole and Cole's (1973) research and Gaston's (1978) study of the reward system. These scholars draw upon citation analysis to examine whether factors such as rank of doctoral department, quality and quantity of research, and prestige of current department, influence the universal allocation of recognition for research. Future research should examine if, when these other factors are controlled for, commercialists are significantly more likely to achieve higher visibility through citation. A similar project could examine visibility by asking scientists within specific fields to indicate the extent of their acquaintance with a sample of commercialist and traditionalists to generate a visibility score, reflected in the percentage of respondents who express familiarity with each scientist. The analytic objective would be to decompose explained variance of visibility according to factors previously discussed.

A second approach to studying stratification and commercial status comes from Heinz and Laumann's (1982) study of lawyers Chicago. In that project, the researchers employ a survey-based strategy that could be applied to scientific work across fields. This approach would entail analysis of the prestige order by having scientists rate specialties within their fields. One could then determine whether commercial relevancy of fields appears to be an important determinant of field prestige. In addition to commercial relevancy, imputed characteristics tied to funding, publications, and intellectual challenge of specific specialties could be constructed through survey questions and used to calculate the net effect on prestige of each characteristic in a multivariate model.

A second line of inquiry for future research is needed to better understand commercial career paths. This study has provided numerous insights into the factors that shape the embrace and rejection of commercial careers, but its insights are limited by an inability to study these dynamics longitudinally. To fully investigate the ways in which attractors and facilitators operate requires attending to the development of such interpretive schemes (e.g., tangibility, commercialization as elemental to training) to understand how they alter patterns of action and interaction as well as how these career scripts evolve over time. Hermanowicz's (2009) longitudinal study of physicists provides a useful model for such a line of inquiry.

A particularly fruitful line of research in this direction would be to investigate the association between specific social mechanisms of path identification and institutional context. The factors that motivate identification with or rejection of commercial career paths are likely to vary across elite, pluralist, and communitarian contexts of science. For example, theories of deviance in the sociology of science generally emphasize alienation and anomie as explanations for departure from conventional career paths. This study found only minimal evidence of anomie, but this may be potentially be explained by the characteristics of scientists in the institutional context I studied: scientists at these universities are less likely to experience anomie by virtue of their organizational integration into elite universities. Scientists at lower-ranked universities, by contrast, may pursue commercial career paths because they are unable to achieve success through traditional means. Thus, the association between institutional context and commercial trajectories merits further attention.

A third line of inquiry for future research should focus on the implications of commercialization for graduate education, which was a pervasive theme in the data. Such a project could potentially form the foundational basis for the type of longitudinal study described above. The specific concern for this line of inquiry is how larger group size, conflicts of interest, and the prevalence of commercial referents impact professional socialization. This research would provide a project ideal for ethnographers of science because interviews are less capable of capturing the temporal dynamics of how significant events (such as commercial discoveries) alter interpretive schemes, the operation of hierarchical lines of authority, and the extent of graduate-advisor interaction.

Policy Implications

The study of commercialization ties to a policy framework because the institutionalization of commercialization in academe was catalyzed by federal policy. As Slaughter and Rhoades (2004) point out, the Bayh-Dole Act and other federal patent laws and policies do not dictate commercialization, they create opportunities for it. Universities and the scientists who constitute them are thus not “corporatized” or acted upon in this sense. Rather, scientists in the academic community actively participate in commercialist activities with the mandate of judicial law and federal and state policies. Thus, the implications of this study’s findings for policy are tied less to a discussion of the relative advantages or disadvantages of a federal technology transfer policy for innovation or economic development. They are, however, tied to general research funding policies that support a technology transfer regime, and organizational policies related to the impact of commercialization on work within universities.

Before turning to funding and organizational policies, a consideration of the efficacy of the Bayh-Dole Act does merit our attention, given its thematic relevance to the issues in this study and the viewpoints of the scientists. If the goal of the Bayh-Dole Act is to promote discoveries that bear on societal problems, how effective is this policy? One way to address this question is to consider it in the context of a specific area of concern. Human health provides a useful basis of consideration, given the volume of commercially-oriented research that addresses it. Two illustrative cases call into question the effectiveness of the role of patenting scientific research in the creation of therapies for or solutions to human illness. The first comes from a survey conducted by the *British Medical Journal* which asked British physicians to rate the most important medical and pharmaceutical “milestones” in history (Godlee 2007). Of the resulting top fifteen discoveries, only two (the pill and chlorpromazine) were patented or resulted from a previous patent.[9] By a more industrial metric, a list of top selling pharmaceuticals worldwide published by *Chemical and Engineering News* magazine makes a case for patenting, but not a powerful one.[10] One analysis of the 46 top pharmaceuticals finds that of these drugs, 20 lack a patent or are not based on existing patents, four were discovered in the process of curiosity driven research, and two were discovered in U.S. university labs prior to the Bayh-Dole Act (Boldrin and Levine 2008). In short, the analysis suggests that half of the top selling pharmaceuticals in the world do not owe their influence to patents. These two cases reflect the position among traditionalists' that commercial incentives are not necessary for scientific knowledge to lead to discoveries of societal relevance. Were one to take the example of top selling pharmaceuticals into consideration of how research policies should be formulated, it would appear to be the case that research policy should not emphasize one mode of inquiry to the detriment of the other, given the relative equal division between patent-based and non-patent-based pharmaceuticals.

However, as has been seen in this study, federal funding research policies clearly emphasize targeted research. One may engage in basic research, but only if the research problem can be tied to a societal problem such as Alzheimer's disease or the development of K-12 science and math teachers. The results of this study suggest that policies that have as their objective the treatment of such problems would benefit from greater consideration of scientists' interpretations of their work. Societal problems are not unimportant to traditionalists or commercialists, but most would argue that academic scientists lack the need for a regulatory roadmap toward the

[9] The other "milestones" included penicillin, x-rays, tissue culture, ether (anaesthetic), public sanitation, germ theory, evidence based medicine, vaccines, computers, oral rehydration therapy, DNA structure, monoclonal antibody technology, smoking health risk (Godlee 2007).

[10] http://pubs.acs.org/cen/coverstory/83/8325/8325list.html

solution of such problems. Yet both categories of scientists embrace distinctive visions of such a "laissez faire" science.

Among traditionalists, "free market" science entails solving societal problems through the autonomous pursuit of scientific problems. This view is predicated on the abundance of curiosity-driven scientific discoveries that had tremendous implications for societal problems, such as those referenced above. This perspective is likewise embedded in adherence to the traditionalist reward system. In the view of traditionalists, science needs no external regulation because it already contains a system of rewards that operates well. Frivolity in problem selection rarely bears rewards, if ever. By contrast, traditionalist utility – working on problems deemed important by past research and by experts in a field – is seen as pushing back the frontiers of science, often with great implications for societal outcomes. The social constraint in this situation, however, is that society is unable to evaluate why seemingly frivolous but scientifically important problems (e.g., the mutation of yeast) are often of tremendous value to societal problems (e.g., understanding of cancer).

Among commercialists, "free market" science entails a freedom they already enjoy, the targeted pursuit of societal problems, but free of a programmatic agenda of federal funding agencies. As we observed in commercialists' use of social comparison, some commercialists disparage federal funding as a greater threat to science than industry. Commercialists view industrial reference groups as enabling the freedom to pursue lines of inquiry they consider important, and federal funding agencies as a source of distortion in science. And thus a point of convergence emerges among traditionalists and commercialists: the view that federal research agendas distort the direction of science. Traditionalists would also characterize industrial influence as a source of distortion, but they are free to reject economic incentives – though only to the extent that they can link their research with commercially relevant targets and ritually identify with such goals.

The policy risk produced by federal research programming of societal targets is therefore the suffocation of important lines of inquiry in fields of research that are not clearly relevant commercially, but of potential importance to known and unknown problems, now and in the future. Federal funding agencies need not incentivize particular problems because the opportunities for commercial attainment created by the Bayh-Dole Act already allocates rewards to such problems for the subset of scientists who value such rewards. Were federal funding agencies to attenuate the emphasis on societal targets, commercialist and traditionalist reward systems could potentially coexist with less tension. Otherwise, fundamental research suffers under the strain of both federal and organizational reward systems that favor commercialist research. It is important to encourage scientists to think about how their work affects society, but policies that push this goal too far are likely to undermine the sought-after objective.

A second area of policy informed by this study pertains to how universities organizationally manage commercialization and its consequences. Here the concern centers on universities' ability to shape two commercially-related conflicts of commitment: between commercial involvement and departmental commitment; and between commercial involvement and doctoral training. Both issues concern how commercial activities are weighed against the provision of quality graduate and undergraduate education. Institutional commitment to undergraduate education is not always evident in institutional policies, because commercialists can be released from teaching loads by hiring adjunct and part-time instructors (Lee and Rhoads 2004). Such policies are incentivized by the fact that universities benefit from both commercialist royalties and lower instructional costs. This problem is less likely to be a concern for doctoral training. Some evidence in the study, however, generated by traditionalist, and to a lesser extent, commercialist accounts, suggests that the involving nature of commercialist laboratories often results in postdoctoral scientists assuming oversight roles in doctoral training.

Other conflicts of commitment tied to doctoral training include the allocation of commercialist research tasks to doctoral students and secrecy within research groups. Current university policies toward oversight of doctoral training, frequently explained to me by many commercialists, typically include a written and oral disclosure to a commercialist's research group of his or her commercial interests and the avenues that may be pursued if a student feels such interests are detrimental to their training. When asked how effectively such disclosures and other policies tied to external commitments are able to regulate behavior, most scientists replied "not much." Close oversight of scientific administration of research groups and time commitments would be difficult to interpret as anything other than an intrusion of professional autonomy, which presents a challenge for how universities prevent misconduct in the context of graduate training and ensure departmental commitments are not disregarded. A useful framework within which existing policies could be optimized could include initiating committees within departments so that regulation is more localized and discipline-peer based (in contrast to financial conflict of interest committees, which are typically university-wide).

The purpose of the present study was to develop an understanding of commercialization based on the interpretations of scientists. These interpretations are important because most of what is known about commercialization is derived from levels of analysis that do not include scientists. The findings of the study expand the work of previous researchers. I offer of a view of what commercialization means to both commercialists and traditionalists. I identify and explain the dimensions along which these views are at odds with one another. In addition, I develop lines of inquiry heretofore absent or undeveloped in this literature, including the temporal context of commercial careers, the cultural antecedents of commercial career trajectories, the identity work that commercialists and traditionalists enact as a result of a new reward system, the normative structure of commercialism, and the redefinition of status in science. In short, I offer a view of social conflict in science previously unseen.

According to Cole and Cole (1973), apart from intellectual controversy, science is an anomaly in society due to its lack of social conflict:

> …There is very little basic questioning of the legitimacy of the social structure of science by any identifiable group or stratum. There are sporadic cries that rewards are unfairly meted out to individuals, but no organized movement of protest exists against the structure of the reward system or the basis on which the performance of scientists is judged. This relative lack of social conflict is a sociological anomaly (Cole and Cole 1973, 83-84).

Perhaps science is no longer as anomalous as the Coles suggest. Or perhaps, as the evidence here appears to indicate, the difference in the state they described and that which I have described amounts to no less than a transformational shift in science.

One of science's greatest strengths, its universality, could also be its Achilles. The fruits of science contain both natural and societal applications, but the quest to extract such fruits by any logic that undermines the advance of knowledge could surely be to the detriment of society by severing the meanings, norms, and identity that make academe an integrated, rather than fractured, profession. The organization of scientific work should therefore neither be exclusively wed to a traditionalist nor commercialist approach to science. Science's status in society will continue to be shaped by both the relatively old and the relatively new reward systems, and by the day to day lives of scientists as they seek meaning and success at work.

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APPENDIX A

Contact Letter to Scientists

Date

Dr.___________________
Department of_________
____
____
____

Dear Professor ________:

I write to ask for your help. I am a PhD candidate in sociology at the University of Georgia under the direction of Dr. Joseph Hermanowicz, Associate Professor of Sociology (jch1@uga.edu). I invite you to participate in a research study entitled *A New Reward System in Academic Science?* that is being conducted under the auspices of the National Science Foundation. The purpose of the study is to understand scientists' perceptions of the scientific role and the relationship between research and the commercialization of scientific discoveries. The study design calls for interviewing scientists who either possess substantial or no experience in commercialized activities such as patenting, licensing arrangements, and the formation of companies. Because most studies of commercial activities in science underemphasize the importance of scientists' views of their work, this study holds the potential to generate important findings about what commercial opportunities mean for science. The study presents the unique opportunity for you to convey knowledge about careers and science acquired from your experience in academe.

Your participation would involve participating in an interview, conducted by myself, that would last approximately an hour. I could meet you in your office at an agreed upon time. The interview would consist generally of questions related to your views of the scientific role, the relationship between research and commercialization of discoveries, and the rewards of a scientific career. The interview would be audio-recorded to retain accurate information, and the recording would be destroyed upon the completion of the study. Your name would not be attached to the recording or to any notes or transcripts made from it. Participation and all interview material will be strictly confidential. The results of the study may be published, but your personal and institutional identity will in no way be associated with your responses. Your involvement in the study is voluntary, and you may choose not to participate or to stop at any time without penalty. Nevertheless, I very much hope you are able to participate, for the success of this study depends on you.

There are no known risks or discomforts associated with this research. If you have any questions about this research project, please feel free to send me an email at drjohnso@uga.edu. Questions or concerns about your rights as a research participant should be directed to the University of Georgia Institutional Review Board, 612 Boyd GSRC, Athens, Georgia 30602; telephone (706) 542-3199; email address irb@uga.edu.

I will call you shortly to invite your participation and address any questions or concerns you might have. Please understand how greatly I appreciate your time and help with this request.

Sincerely,

David R. Johnson

APPENDIX B

Interview Protocol

The New Reward System in Academic Science: Interview Protocol

This is a study about the reward system in academic science. The questions I would like to discuss concern one's conception of the scientific role and the commercialization of scientific discoveries. Some of the topics I will address ask you to make personal judgments about your own career and various professional issues. Your participation in this study is strictly confidential. With your consent to begin, I will record the interview. Interviews are normally recorded to accurately keep track of information. Subsequently the tape will be destroyed. Your identity and that of your institution will be carefully concealed in any published work. Your participation in this study is important. However, should you at any time wish to stop, you may do so without consequence, and at any time you should feel free to ask me questions concerning the interview or the study. May we begin?

A. Conception of the scientific role

1. Among the types of pursuits that university scientists can engage in, what, to you, is the best or most esteemed?
   Probe: Why?

2. To what extent do you perceive your own work to fulfill the ideals that underlie these types of pursuits?

3. Who do you think benefits from what you've done?

4. Which types of pursuits do you shun or regard as trivial?
   Probe: Why?

5. Is commercializing scientific discovery a legitimate way to fulfill the academic role?
   Probe: Why or why not?

6. What distinguishes you from peers in your field who [do not] commercialize their work?

B. Motivations of entrepreneurialism

7. To what extent have you considered or sought to apply your work commercially over the course of your career?
   Probe: Aside from patents or startups connected to your own research, what sorts of ties do you maintain with industry?

8a. [*Traditionalists*] Under what circumstances, if any, would you attempt to commercialize your work?
8b. [*Commercialists*] What factors influenced you to commercialize your work?

9. What conceptions of university-based commercial research did you have as a graduate student?
Probe: In what ways were you exposed to it during this period?

10. In what ways [has/would] commercializing your work allow[ed] you to achieve your goals as a scientist that [aren't/wouldn't be] possible by other means?

C. Norms of Science
11. What should be the relationship, if any, between research and commercialization?
Probe: Do you believe this relationship gets abused? If so, how?
Probe: Do you believe universities have gotten "carried away" with this relationship?" If yes, How so?"

12. Is there a collective understanding among your peers in this department concerning the appropriateness of commercializing one's work?

13. Should universities reward scientists for commercializing their work?

14. Do you see any problems associated with academic researchers accepting money from industry to conduct research?

15. Do you perceive there to be flaws associated with basic science as a mode of inquiry?

16. Should researchers be concerned with the utility of their discoveries?

D. The operation of reward systems
17. Do you perceive costs to the commercialization of research?

18. Do you believe university-based commercial research corrupts science?
Probe: Are there examples in your own career where commercial involvement interfered with or was incompatible with scientific progress?

19. How do you perceive the privatization of scientific discoveries by peers in your field influences scientific progress?
Probe: What would happen to scientific progress in your field if the majority of academic scientists working in it sought to commercially exploit their work?

20. How can science benefit from increased commercialization of academic research?

21. How important is it to you to make a lot of money?

22. Do you feel that the rewards you have received have been commensurate with the amount of research you have contributed to your field?

23a. [Traditional scientists] What do you think would be the most rewarding aspect of commercializing your work?

23b. [Entrepreneurial scientists] What is the most rewarding aspect of commercializing your work?

Probe: Why?

I have one remaining question I would like to address.

24. What have you found to be the most rewarding accomplishment of your career?

Probe: What sets this achievement apart from other accomplishments?

Probe: What was most rewarding about this accomplishment?

Probe: [For entrepreneurial scientists] How does this achievement compare to the success you've attained in your [commercial/non-commercial] endeavors?

APPENDIX C

Post-interview Questionnaire

Study of the New Reward System in Academic Science

Department of Sociology
113 Baldwin Hall
The University of Georgia
Athens, Georgia 30602-1611

Information provided on this form is strictly confidential and will be used for research purposes only. At all times your identity will remain anonymous.

1. In what year were you born? 19__.

2. Please indicate the extent to which you have participated in the following activities while holding a university appointment:

a. The number of discoveries for which you have been issued a patent: _____

b. The total number of patents you have licensed: _____

c. The total number of companies you have founded: _____
Of these companies (*please indicate a number*):
___ has/have had an initial public offering
___ has/have been acquired or merged with existing companies.
In ___, I have acted as a chief officer (CEO, CTO, CSO, etc)
In ___, I have served as a director on the company board.
In ___, I have served as an advisor on the scientific advisory board.
In ___, I have served as a consultant.

d. The total number of existing companies to which you have licensed patents: ____
Of these companies (*please indicate a number*):
___ is/are publicly-traded corporations.
___ is/are private corporations/LLCs.
In ___, I have acted as a chief officer (CEO, CTO, CSO etc)
In ___, I have served as a director on the company board.
In ___, I have served as an advisor on the scientific advisory board.
In ___, I have served as a consultant.
In ___, I have assumed no official position.

e. *Excluding* companies I have founded or with which I have entered patent licensing agreements:

In ___, I have acted as a chief officer (CEO, CTO, CSO etc)
In ___, I have served as a director on the company board.
In ___, I have served as an advisor on the scientific advisory board.
In ___, I have served as a consultant.

3. Please indicate your past year's *nine-month base university* salary. (Do *not* include nonuniversity income, e.g., research grants, consulting fees, royalties, honoraria, etc.)

1. ( ) less than $70,000
2. ( ) $71,000-$79,999
3. ( ) $80,000-$89,999
4. ( ) $90,000-$99,999
5. ( ) $100,000-$109,999
6. ( ) $110,000-$119,999
7. ( ) $120,000-$129,999
8. ( ) $130,000-$139,999
9. ( ) $140,000-$149,999
10. ( ) $150,000-$174,999
11. ( ) $175,000-$199,999
12. ( ) greater than $200,000

4. Please indicate the percentage of your past year's university salary supported by a research grant or contract *with companies* to which you have licensed a patent or founded: _____

5. Please indicate your past year's nonuniversity income received from consulting fees.

1. ( ) less than $20,000
2. ( ) $20,000-$29,999
3. ( ) $30,000-$39,999
4. ( ) $40,000-$49,999
5. ( ) $50,000-$59,999
6. ( ) $60,000-$69,999
7. ( ) $70,000-$79,999
8. ( ) $80,000-$89,999
9. ( ) $90,000-$99,999
10. ( ) $100,000-$109,999
11. ( ) $110,000-$119,999
12. ( ) greater than $120,000
13. ( ) Not applicable.

6. Please indicate your past year's nonuniversity income received from patent royalties.

1. ( ) less than $20,000
2. ( ) $20,000-$29,999
3. ( ) $30,000-$39,999
4. ( ) $40,000-$49,999
5. ( ) $50,000-$59,999
6. ( ) $60,000-$69,999
7. ( ) $70,000-$79,999
8. ( ) $80,000-$89,999
9. ( ) $90,000-$99,999
10. ( ) $100,000-$109,999
11. ( ) $110,000-$119,999
12. ( ) greater than $120,000
13. ( ) Not applicable.

7. Please indicate your past year's nonuniversity income received from board or officer positions in public and private corporations.

1. ( ) less than $20,000
2. ( ) $20,000-$29,999
3. ( ) $30,000-$39,999
4. ( ) $40,000-$49,999
5. ( ) $50,000-$59,999
8. ( ) $80,000-$89,999
9. ( ) $90,000-$99,999
10. ( ) $100,000-$109,999
11. ( ) $110,000-$119,999
12. ( ) greater than $120,000

6. ( ) $60,000-$69,999
7. ( ) $70,000-$79,999

13. ( ) Not applicable.

8. Please estimate the level of revenue your commercial activities have generated for the university during the past year: ________________________

9. Of the companies you have founded, licensed a patent to, or with whom you have served as an officer or consultant, in how many do you currently hold equity?

10. Of the companies you have founded, licensed a patent to, or with whom you have served as an office or consultant, in how many does your university hold equity?

Please seal the completed form in the enclosed envelope and return to the interviewer. Your assistance in this research effort is greatly appreciated.