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Expert Judgment versus Market Accounting in an Industrial Research Lab

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Abstract

Accounts of change in contemporary research in industry and the academy often note the increasing coexistence of market and academic norms and practices. This article suggests that, at least in industry, these conflicting norms and practices are often preserved by loose coupling between market pressures and the research organization. Based on a two-year case study, this article examines the imposition of tight coupling at an industry lab that had previously been able to maintain some of norms and practices associated with the academy. Tight market coupling limited the role of judgment in the governance of research. First, a market-based quantitative project selection system delegitimized expert judgment. As a result, projects with obvious market and technical uncertainties were more difficult to justify. Second, objective, market-based oversight also limited the role of expert judgment. Tracking the return on investment of scientists' hours with time cards had the effect of discouraging time spent on learning or exploratory work. In many cases, researchers argued that displacement of judgment for

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a quantitative governance system caused work to shift away from research and toward product development.

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How might efforts to govern research systematically affect research itself? Some scholars suggest we ought to expect to see more efforts to manage science by the numbers (Moore et al. 2011; Leslie 2010; Benner and Tushman 2003) given the growing emphases on the role of science in economic growth (Slaughter and Rhoades 1996; Lave, Mirowski, and Randalls 2010; Mirowski 2011; Berman 2011, 2014; Stephan 2012) and on quantification and accountability as cornerstones of modern management (Porter 1995; Power 1999, 2008; Strathern 2000). This article reports on one such effort. Drawing upon ethnographic, interview, and archival data, it examines the use of market data to plan and manage research systematically at an industrial lab over a two-year period. The new market-driven system ended a regime of loose coupling, which did not represent a golden age by any stretch but was able to tolerate the often conflicting goals and norms of scientists in the labs and the market-facing business units they supported. Systematization of research planning and management according to market-based metrics left little room for subjective judgment in research, which researchers argued made it more difficult to continue pursuing research rather than development work.

Loose coupling and ceremonial compliance are often described as adaptations to situations in which genuine compliance with stated goals and procedures would interfere with the performance of difficult-to-plan work (Meyer and Rowan 1977, 1978). Loose coupling allows for interaction when standardization across groups might be counterproductive (Weick 1976; Orton and Weick 1990). Many of the research environments described by Science, Technology and Society (STS) scholars as containing contradictory norms and practices can also be seen as being loosely coupled (Vallas and Kleinman 2008; Stark 2009). These environments preserved certain academic norms and practices such as a high degree of researcher discretion in choosing and managing projects while retaining commercial practices such as securing intellectual property (IP) rights and searching for market outlets for research products. The lab I studied largely functioned this way since its founding.

Tighter coupling reduced the role of judgment in two ways. First, tighter coupling required that market data and projections rather than expert judgment determined which projects were worthwhile. Second, ongoing estimates of return on investment via time cards, rather than the subjective assessment of scientists or managers, determined whether time was well-spent. In sum, quantitative market analyses rather than expert judgment became the means by which to govern research. The new system not only negatively affected morale; many argued it made the lab less viable in the long run. Researchers reported that they felt that many of their new projects were more akin to product development, whereas their previous more open-ended research projects had served to cultivate unique, and thereby valuable, expertise.

I argue that one of the important functions of loose coupling in these hybrid research settings is to preserve the role of judgment in research. Indeed, in this case, not all executives, many of whom had been scientists, believed the market-driven system would help. They and other scientists protested that real uncertainty was constitutive of research and could not be quantified (see Dennis 1987; Shapin 1994, 2008). Nonetheless, with the constant threat of insolvency looming, a new Chief Executive Officer (CEO) saw governance based on accountability to market-based metrics as the only legitimate course he could pursue. The new, more "objective" system did not represent a power grab by management. Instead, it shifted authority and responsibility for making decisions from individuals to a set of intendedly objective procedures (Porter 1992, 1995). Consequently, reliance upon judgment was kept to a minimum, from the CEO down to individual researchers. This article suggests that quantitative systems to make research accountable may bring about an end to loose coupling and thereby drive out norms and practices associated with the academy, even where they once facilitated research.

Sustaining Contradictions in Research Environments

Asymmetrical convergence is often used to describe the changes in US science in the past few decades. Asymmetrical convergence describes a process of two-way influence between the norms and practices of science and the marketplace but suggests that the marketplace will ultimately have the upper hand (Kleinman and Vallas 2001; Vallas and Kleinman 2008). The asymmetrical convergence thesis arose in response to surprising trends observed in industry and university settings related to the life sciences. On one hand, scientists in industry enjoyed freedoms typically associated with the academy—they had a surprising degree of autonomy, could publish widely, and maintained broad intellectual networks (Owen-Smith and Powell 2001; Shapin 2008; Vallas and Kleinman 2008; Evans 2010). On the other hand, academics increasingly secured IP rights, obtained funding from corporations, and looked for practical, often economically relevant outlets for their work (Owen-Smith and Powell 2001; Colyvas and Powell 2006; Vallas and Kleinman 2008). Scholars even observed institutionalized pressures toward convergence, such as the growth of government-funding programs that intended to bring industry and the academy together (Berman 2008, 2011; Mirowski 2011).

An important aspect of the asymmetrical convergence thesis is the *sustaining* of *contradictory* norms and practices. Settings undergoing asymmetrical convergence display competing market and academic logics. Academic and industry settings shape one another through flows of information, people, and materials, thereby keeping these contradictions active (e.g., Powell et al. 1996; Evans 2010; Powell and Sandholtz 2012). Norms, logics, and practices intermingle but remain unreconciled (Owen-Smith and Powell 2001; Vallas and Kleinman 2008; Stark 2009). Described as a network form of organizing (Powell 1990) or as a heterarchy (Stark 2001), the end result is the same: the market relies more on science, science relies more on the market, and contradictory practices coexist.

Settings that are able to preserve significant contradictions are often loosely coupled. There are many definitions of loose coupling. At the most general level, the elements of a loosely coupled system are *responsive* (in contrast to decoupled systems) and *distinctive* (in contrast to tightly coupled systems; Weick 1976; Orton and Weick 1990). For instance, in a loosely coupled arrangement, the research lab and business division might be in dialogue about technological goals but retain important differences in norms and practices, such as expectations around autonomy or how to evaluate performance. Meyer and Rowan (1977, 1978) use the notion of loose coupling to describe how schools separated the technical requirements of teaching from the institutional pressures to appear modern and rational. They argue that ceremonial compliance is an adaptation to modern managerial myths when the realities of work preclude such foresight and coordination. Loose coupling allows conflicting practices and systems of belief to coexist.

A recent literature on accountability suggests that loose coupling may be coming to an end in many domains of work (e.g., Power 1999; Strathern 2000). Hallett (2010) describes a shift from loose to tight coupling in an elementary school under a new principal. He calls this an instance of accountability myths becoming real. Following up on Meyer and Rowan's

(1977, 1978) prediction that tight coupling would lead to significant conflict, Hallett finds that increased surveillance and control by administrators caused teachers "epistemic distress"—they questioned their basic assumptions, routines, and even identities. Tight coupling negatively affected the quality of instruction and the experience of teaching. Teachers and staff eventually sent a volume of forty-four grievance letters to the school district office but to little avail. While Hallett nicely captures the effects of tight coupling in the context of education, there remains no study of how tight coupling might affect research work.

The dominance of an accountability logic in professional managerial circles means that financial pressures at industrial research labs might be met with tight market coupling (Power 1999, 2008), representing, in those instances, a reversal in the asymmetrical convergence trend. In such a scenario, myths of transparency and accountability would be realized in the practice of research. From the 1980s to early 2000s, when scholars conducted many of the studies behind the asymmetrical convergence thesis, large private- and public-sector investments afforded biotechs and their university partners a great deal of freedom. In contrast, corporate research labs in computing, communications, and pharmaceuticals/chemicals have been under greater shareholder pressure and more concerned with business relevance since the early 1980s (Varma 2000). Thus, we might expect to see deviations from the asymmetrical convergence thesis in these non-life science industries sooner.

This study considers an effort to impose tight market coupling at a cash-strapped lab in the information technology industry. It highlights the potential for the affinities between scientism and neoliberalism (Moore et al. 2011; Mirowski 2004, 2011) to encourage a form of tight coupling, in which the market is held up as the ideal and exclusive arbiter of what projects are supported and whether hours worked are valuable (Robson 1994). Without being able to rely on subjective judgment, scientists and managers found it increasingly difficult to justify research with apparent and unquantifiable uncertainties. Consequently, researchers complained that many ITLab projects began to resemble engineering or product development. The case suggests that in settings in which the logics of science and the marketplace intermingle, loose coupling might be essential to preserving research (Stark 2009).

Cycles of Decoupling and Recoupling in Corporate Research

Although the term *loose coupling* is generally not used, histories of corporate research labs reveal that the degree of coupling is at the heart of cyclical struggles between corporate research labs for greater freedom and headquarters for greater business relevance. New business-inclined research directors or strained corporate finances would push labs toward greater accountability. Executives in charge only gave up on greater accountability when they realized such efforts were interfering with the conduct of valuable research (Hounshell and Smith 1988; Reich 1985; Shapin 2008).

The trouble caused by greater accountability can be seen as the byproduct of inescapable difficulties in measurement (Dennis 1987). Efforts at detailed measurement have long been viewed as a potential threat to research. As Charles Kettering reflected on becoming head of research at General Motors in the early twentieth century:

> I told Mr. Sloan [then CEO of General Motors] that I would take it [the job] on three conditions—that I would have no responsibility and no authority, and that I would never be held accountable for the money I spent. I don't think you can run a research laboratory any other way. The minute you take responsibility or authority, you quit researching. You can't keep books on research, because you don't know when you are going to get anything out of it or what it is going to be worth when you get it. (Boyd 2002, 118)

Kettering argues that any effort to measure costs and benefits leaves scientists and managers concerned about whether they will be held accountable at some point in the future. Because of the uncertainties of research, effective accountability would make it difficult to continue doing research.

In a review discussion, Hounshell (1998) describes the futility of trying to design methodologies for accurately measuring the costs and benefits of research:

> As a historian, I cannot—and I will not—say that history "proves" my argument. But I can definitely say that in the past, some very smart people have wrestled with the issue of measuring the return on investment in R&D and have frankly admitted that it is an intractable problem. Moreover, they concluded that any scheme they might develop was so flawed as to be dangerous if used alone for decision making; consequently, they relied on other criteria. (1998, 6-7)

What are these other criteria? Essentially, the labs relied on expert judgments and exist within a regime of loose coupling. In some cases, industrial labs would use readily observable criteria—such as giving scientists credit for publishing in top journals (Hounshell and Smith 1988). Yet, amorphous and subjective factors were often more prominent. They would also evaluate scientists on the extent to which they contributed to the research of others and the social climate of the lab (Reich 1985). As experts in the fields they oversaw, managers had a sense of the quality of their staff's work without need for objective criteria or metrics (Reich 1985; Hounshell and Smith 1988; Shapin 2008).

Rather than relying on traditional managerial methods to create order, corporate labs functioned as honorific status groups (Barnes 2007). Put another way, they functioned as guilds. As managers, well-known scientists had the instant respect of their staff and wielded more clout when it came to protecting the autonomy of researchers from short-term business pressures (Reich 1985; Shapin 2008). I next describe how I studied a shift toward greater accountability in a contemporary industrial lab.

Setting and Data

ITLab (all names pseudonyms) is a wholly owned subsidiary of ITCorp, but as explained below, it is only partially funded by its parent. ITLab has been in operation for over four decades and has contributed in very significant ways to computer hardware and software technologies that are ubiquitous today. Its past and present scientists have won numerous national and international technical awards including inductions into the American Academy for the Advancement of Science. Dozens of alumni became university faculty. It employs around 150 scientists. When I began my study, ITLab was one of the most academic-leaning industrial labs (at least related to computing and communications) in operation. Almost all scientists there publish papers, many have more papers than patents, and some of the scientists I interviewed counted academics among their chief research competition. The ability to publish and present research widely was a major selling point to top PhD graduates.

From June 2009 through May 2012, I had access to ITLab. I gained access to ITLab through a three-month summer internship in its social science group. After the internship, I was asked if I wanted to stay as an unpaid visiting researcher for a year to conduct fieldwork and interviews, an offer that was extended two more times. This position gave me unfettered access to the building. I was given an office, access to the e-mail network, and there were almost no legal restrictions on what I could see since I was bound by the same confidentiality agreements that cover employees.

I collected most of the data for this article from June 2009 through August 2010, during which I was at ITLab Monday through Friday from about 9 a.m. to 6 p.m.[1] From September 2010 to August 2011, I visited

Table 1. Interview Data.

Characteristic	Number
Software researchers	15
Hardware researchers	19
Managers[a]	19
Business personnel	19
Male	44
Female	8
Total individuals	52
Total interviews	82
Average duration	73 minutes

[a]Most of these were middle managers who also engaged in research. The remaining managers had technical PhD's but devoted all their time to administration. Business personnel included business development, legal (intellectual property), and marketing. I interviewed some individuals more than once.

ITLab one day a week, typically conducting an interview or having lunch with scientists whom I had gotten to know during the previous year of my study. I scheduled my visits to coincide with meetings that I thought would be relevant to the study. In the third year of my position as a visiting researcher, I had gathered enough data and only visited every two to three weeks in order to catch up with certain informants or attend meetings that appeared extremely relevant to my study.

I conducted eighty-two interviews with scientists, managers, and executives that lasted between 45 and 125 minutes, with a mean of 73 minutes (Table 1). The population I interviewed was quite diverse in terms of disciplinary background. From the physical sciences, I was able to interview individuals with expertise in optics, fluidics, electronics, materials, particle manipulation, and physical chemistry. From computer science and mathematics, I interviewed researchers who worked on algorithms, machine learning, natural language systems, statistical planning and optimization, security, and networking.

For the first five months of my study, I became acquainted with the lab, read internal documents, had lunch with a variety of scientists and businesspeople, and began conducting interviews. For the next eight months, I shadowed three research groups in parallel. One group was a semiconductor materials group, another researched miniature electronic devices, and the last worked on security and communications software. For each of the shadowed groups, I attended their weekly meetings, planning and brainstorming meetings, and some of their meetings with other companies. I also attended workshops, regular lab and organization-wide meetings, and dropped in on interesting meetings I saw in the online calendaring system. I took notes in all meetings and audio-recorded roughly half of them. Typically, I spent twenty hours a week gathering observational data.

In addition to observations, I consulted several informants regularly to gain their perspective on certain events in the lab and advice on how to study certain features of "ITLab life." I became relatively close to four scientists and one IP licensing manager. I attended non-company social events such as baby showers, poker nights, and dinner gatherings with my informants. In addition to these five individuals, I was on friendly terms with a number of other scientists and businesspeople with whom I would occasionally have lunch or chat with in the hallways, in the cafeteria, by the coffee machines, at the ITLab gym, or drive with to a meeting at a company or university.

Timeline and Summary

I use a timeline to provide a brief overview of the case (Figure 1). Prior to the bursting of the Internet bubble, ITCorp funded ITLab with very general mandates and weak oversight. After divestiture (2002), ITLab spent several years in relative anarchy; many thought the organization would be shuttered. Within ITLab, a power struggle ensued in which Jon Knolle (as noted earlier, all names are pseudonyms), an executive from operations who had not been a scientist became CEO.

My observations began in June 2009. In the middle of July 2009, the CEO hired a vice president (VP) of business development (BD) he thought capable of implementing a rational research management system, which became known as the "portfolio system." The new VP grew the business staff, introduced the portfolio system for projecting returns on investment, and instituted a complementary system of time card accounting of costs. The "strategic reviews" served as the moment for cutting projects that fell below certain market criteria.

Concerns about morale and the rapid departure of prominent scientists began to be expressed by scientists and some managers in early 2010. The number of government grant applications, reflecting a desire for autonomy, increased dramatically throughout the period of study. ITCorp fired CEO Knolle in December 2010. Recognizing that many of the morale problems were due to the short-term focus on commercial funding, the new CEO tried to address the morale problem through minor concessions to encourage government and internal funding for more exploratory research (a slight loosening of coupling).

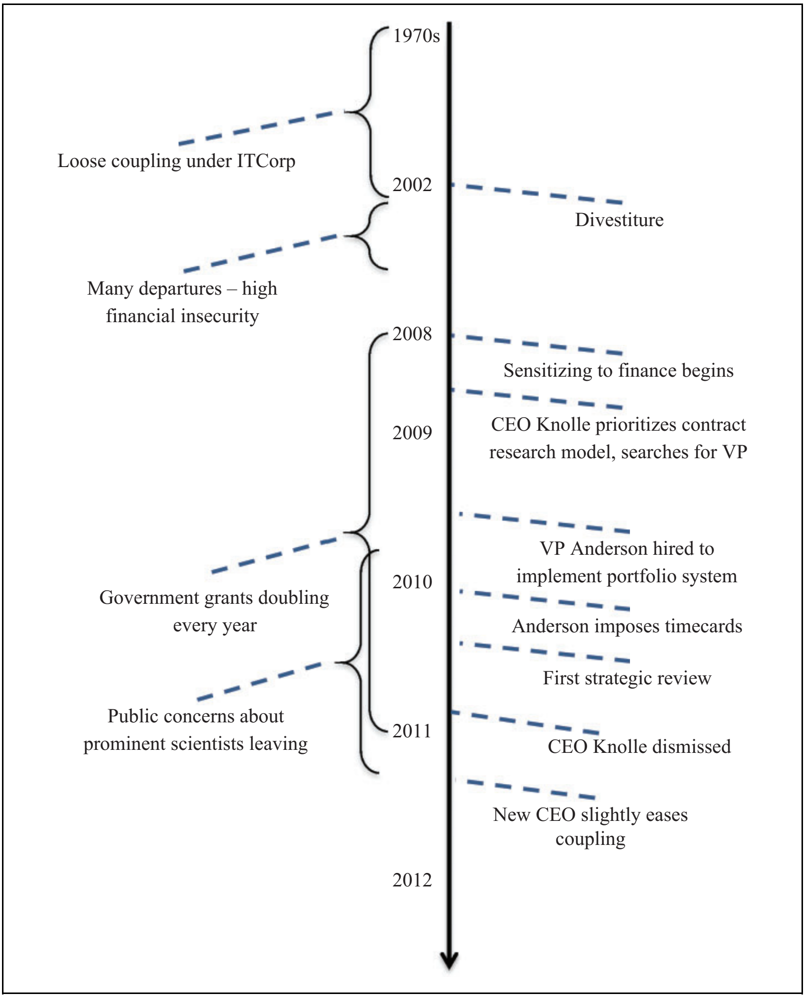

Figure 1. Timeline.

End to the Era of Loose Coupling at ITLab (1970s-2002)

For its first two decades of existence, ITLab had enjoyed a great deal of autonomy. This autonomy and large research budgets had left it rich with well-known scientists. As ITCorp became less profitable in the 1990s and early 2000s, *use-inspired basic research* (think Pasteur) decreased while *pure applied research* (think Edison) increased (Stokes 1997). The late 1980s and 1990s witnessed a migration of well-known scientists to academia, research fellow positions at large companies or nonprofits, and start-ups. With only a few prominent scientists left in each lab, the guild model of governance had weakened.

During the crash of the Internet bubble (2000-2002), ITCorp nearly went bankrupt. In order to strengthen its balance sheets, ITCorp divested itself of ITLab. For the first time in its history, ITLab was no longer guaranteed full funding. In fact, for much of the six years after being spun out of ITCorp, ITLab faced potential insolvency. Senior executives believed that the way out of this predicament was commercial funding: large companies would be attracted by ITLab's reputation and could afford to pay well above project costs.

In order to become an outward facing commercial lab rather than an in-house lab, the types of people in management positions would need to change. For the first time in its history, someone with primarily a business background was promoted to be CEO of ITLab. Yet, Jon Knolle, the new CEO, was bothered by the perception that he was unqualified to lead the lab. He educated himself and his staff about methodologies for managing research. He amassed a library of business books referred to in the building as "the innovation literature" in his office. Business school professors and independent consultants regularly met with ITLab management to offer to impart their techniques for the professional administration of research. They left copies of their articles from specialty journals such as *Research Management* and the *Journal of Technology Transfer* as their marketing materials. They led several off-site meetings to train business staff on how to better link research to the market, including how to select and oversee projects, and how to sell research capabilities.

Apart from Jon, much of the drive toward tighter coupling came from Jeff DeGroot. Jeff was widely respected as a scientist at ITLab. Yet, he had worked in business roles for two decades. Jeff described his former life as a scientist as a period of naïveté and idealism; that as a researcher he had aspired to "light and truth." He changed after working at a start-up with founders and investors he characterized as ruthless. He believed, upon his return to ITLab, that most scientists at ITLab needed to undergo the same "cultural transformation" that he experienced, which would lead them to attach greater importance to money. He thought that his identity as a scientist left many usefully confused, explaining that his role in intensifying the commercial focus of the lab was that of a "wolf in sheep's clothing."

Jeff and Jon spent over a year searching for the right manager to implement a quantitative system for selecting and overseeing research projects. After interviewing thirty-five candidates, they hired Cliff Anderson as VP of the BD group. Cliff had a PhD in the natural sciences but got an MBA after working as a researcher in industry and becoming frustrated by his managers' laissez-faire attitude and lack of methodology. Upon his introduction to ITLab, scientists at a company-wide meeting, Cliff stated that his three principles for management were "transparency, metrics, and accountability." Cliff soon hired eight staff in the BD group and created a marketing group of five. Most of this staff was not technical, but the few who were had experienced a similar disillusionment with research, "So I said enough of this R&D crap. I really like to work on stuff because I can actually see it, make it into a product and be useful. So I then went into a technology transfer function." In total, staff dedicated to sales, marketing, and strategy grew from about four to twenty.

Whereas the BD team had served a consulting role before—advising researchers on markets and helping them pitch their work—its function was now explicitly managerial—playing a large part in selecting and overseeing research projects. Reflecting this change, Cliff centralized all the BD staff into a separate floor such that they could better coordinate their efforts and be consistent about processes at ITLab.

In addition to granting more power to professional administrators, executives sought to increase awareness of financial issues. Lab and middle managers were told they would be held accountable to certain financial targets, encouraging them to discuss finances in staff meetings more regularly. The VP of finance began to visit lab meetings every quarter to provide a financial update, replacing technical talks that would have taken place in the past. Archives revealed that prior to 2008, there were none of these talks; after, they occurred every quarter. These meetings were uninteresting to researchers and I observed the number of laptops being used increase from a mere handful to an overwhelming majority of the audience. Finance also began sending e-mails that mandated spending freezes for equipment and supplies in order to meet quarterly goals, regardless of the needs of ongoing experiments or research.

In summary, the most apparent way to assure the stability of ITLab was to increase the predictability of projects and obtain contracts from corporations with deep pockets. To do this, ITLab expanded its BD, marketing and finance groups to guide research projects toward commercial relevance. I describe the creation of an intendedly objective financial system for overseeing and guiding research work in the next section.

The Case for Quantitative Financial Oversight (Mid to Late 2009)

CEO Knolle hired VP Anderson to make ITLab "more of an organization" by implementing a systematic approach to managing research. A month into the job, Anderson was convinced that the lack of structure was due to a shortage of will rather than the nature of research, "ITLab historically has had no ability to calculate an NPV (Net Present Value), an IRR (Internal Rate of Return), or an ROI (Return on Investment). We have no ability to make accurate future financial projections ... the fact that we can't do this accurately just means that we don't have a good tool to use." Rather than attributing the difficulty of accounting for research to its inherent uncertainties, Anderson believed that it was a matter of methods and effort in collecting appropriate data.

The first step in modeling ITLab research was to find out what researchers were doing with their time. VP Anderson noted, "I really believe that what I like to do is to create order out of chaos, and you can't only clean half your house. I feel like part of that is a near complete understanding of what is going on from an investment perspective." BD and finance created a list of all activities by asking middle managers to identify all work that accounted for more than half of an employee's hours. Yet, translating research time into discrete projects for modeling purposes was difficult. While some hours could straightforwardly be assigned to particular funders, goals and milestones, others were murky. Some hours spent doing research could be counted toward multiple projects or toward none at all.

In Anderson's estimation, there were over 200 projects at ITLab. He intended to cut the total number of projects at ITLab to fifty or sixty so that they could be better understood and managed. As one of his staff summarized, "There are way too many projects at ITLab given how many BizDev people we have." Anderson complained that the number of skunk-works (unofficial) projects "really scares me" and that "we are pretty damn loose about what projects we decide to tamper in." From the point of view of professional administrators who participated in managerial communities of practice such as the Churchill Club and the Industrial Research Institute, the lax oversight of projects and the resulting incoherence of ITLab's activities appeared irresponsible.

The Design of a Market-based Governance System (Mid to Late 2009)

The portfolio system would begin with a means for organizing and analyzing market data. The first step was to divide projects into a two-by-two table

	Existing ITLab Market	New Market
New Technology	Revamp (15%)	Options (10%)
Existing ITLab Technology	Core (60%)	Adapting (15%)

Figure 2. Ideal portfolio allocations.

of high and low market and technical uncertainties (Figure 2). The largest share of projects, roughly 60 percent, was supposed to be in a category referred to as "core." Core projects had a 90 percent probability of technical success because of their incremental nature and had the potential to address markets with which ITLab had experience. The expectation among business staff was that these projects would also be ITLab's most profitable. Fifteen percent of projects would be dedicated to using an old technology to pursue a new market ("adapting") and another 15 percent to pursuing a new technology in an old market ("revamp"). Only 10 percent of projects would have the goal of creating a new technology that would address a new market called "options" (from "real options").

Using these definitions, however, half the *existing* projects at ITLab represented options, meaning there were five times too many projects "high" in market and technical uncertainties. Anderson publicly stated that many of these options projects would need to be stopped and the others would need to be closely managed in an "aggressive VC (venture capitalist) style" in order to ensure that no money or effort was going to waste.

Despite the best efforts of the BD team, the aspiration to find an objective market-based approach made it hard to escape a conservative bias in selecting research projects. They scored projects on four criteria. The first criterion was *potential profit*, which would account for 40 percent of a project's total score. This measure had to, in one way or another, rely upon comparisons to already existing markets (making it conservative). The second criterion was *time to potential revenue*, which favored work that could quickly be made relevant to the market (30 percent). The third was the potential value of the IP, which was to be based on the breadth of the property rights (broader in newer fields) and the size of the market (larger in established markets; 20 percent). The fourth criterion was *business model*—how was money to be made from the research (10 percent)? When I observed internal discussions of this model, the IP staff were quick to point out to the BD group that, put together, criteria favoring near-term revenue (profit, time to revenue, and business model) accounted for 80 percent of a project's score. It was very difficult to come up with a "data-driven" model for rating research that would not have a conservative bias. Data exist only for existing markets, and so the more different the world imagined by the research, the more tenuous the use of data about existing markets (Shapin 2008, 132-45).

Naturally, project scores had to be weighted by risks. Many of these risks were in fact unquantifiable uncertainties. A couple of the senior executives who had been scientists resisted, for quite a while, the idea of assigning probabilities to undefined events (i.e., the outcomes of many research projects). They argued that it was impossible to assign a probability to something—that is, to assess risk—if no one knew what that thing was. These executives relented after realizing that CEO Knolle and VP Anderson saw the quantitative portfolio system as essential. If the system were to be implemented, uncertainties would have to be included somehow. The question was now how much the translation of an uncertainty into a risk strained credulity. If it strained credulity too much, it made a project difficult to justify.

Dan Renke was the BD staff member who designed the technical aspects of the portfolio system. He adapted a number of worksheets from ITCorp product divisions to attempt to translate many uncertainties into risks. He did his best to change the worksheets, originally intended for use in an engineering setting, such that they would apply in a research setting. Renke summarized how risks were assigned:

> Each of the risks has worksheets. Technical risk has a worksheet here. It has this tree approach. This is what ITCorp uses [in product development] and we've adapted this from ITCorp. There's a hardware and software worksheet, if a project has both, you do both and you use the higher risk estimate for scoring purposes. This [item on the technical risk worksheet] asks whether there is a prototype, simulation, etc.

Worksheets covered *technical*, *business model*, and *execution* risks. Often, uncertainties were translated into risk using discrete stages that indicated what was known and unknown about the possibility of a project's success. For instance, on technical success: having only a simulation gave a project a 1 percent chance of success; a bench-scale proof of concept: 10 percent; and a prototype: 70 percent. Of course, what counts as an appropriate and convincing simulation or prototype differs widely across research fields and industry applications. Renke himself had encountered these different standards when he tried to commercialize the same ITLab technology in a few industries. Yet, he and other executives had to elide these distinctions in order to have a consistent system for evaluating projects.

The challenges with designing a methodology for financial forecasting of research are nicely captured by Shapin (2008) in his discussion of uncertainty in industrial science. Knightian uncertainty is defined as risk that is impossible to calculate (Knight 1921). Many uncertainties in research are Knightian—not only do people lack the information to calculate the odds of desirable and undesirable outcomes, totally unanticipated outcomes may also be possible. Shapin writes,

> One of the most mundane, yet characteristic, features of any research properly so called is uncertainty—uncertainty in its outcomes and uncertainty in the procedures employed to secure outcomes. If one defines research as an inquiry into the relatively unknown, then neither the exact shape of the eventual results, nor the methods which will be successful in securing those results, nor the time and resources required for success, nor the likelihood of success, nor, finally, the consequences of findings can be exactly specified in advance of undertaking the research. (2008, 132)

There is no research without uncertainty and uncertainty cannot be measured. These uncertainties plague any attempt to estimate both the costs (uncertainties in the conduct of the work) and the benefits (uncertainties regarding outcomes) of research. Thus, a project selection and oversight system biased against unquantifiable uncertainties might encourage engineering or development work at the expense of research.

Making Management Mechanically Objective (Mid-2009 Onward)

In order to ensure the legitimacy of the portfolio system, managers would need to follow strict processes for assigning scores and evaluating risks. As the scoring worksheets suggested, Renke and the BD group sought to make the process of assigning scores as objective as possible. Their aspirations fit Porter's definition of mechanical objectivity, "It [mechanical objectivity] means following the rules. It implies personal restraint. Rules are a check on subjectivity. They should make it impossible for personal biases to affect outcomes" (1995, 4). In order to assess research projects properly, not only would scientists' judgments need to be excluded, but the idiosyncrasies of the business staff would also need to be excluded. As Renke puts it,

> The only reason we can do this now is that BizDev has been centralized so theoretically we are objective even though we all have our biases. But, that's

> the idea. We don't want researchers to put scores in. We don't want lab managers to put scores in .... BizDev will own this and try to normalize the process. We have regular meetings, we police ourselves. We have to be honest here because otherwise the process will lose its integrity.

This was not only an aspiration to mechanical objectivity—it even echoes Porter's "accounting ideal" in which the desire to avoid the appearance of subjectivity leads to a desire to minimize variation across individuals (1992). The most objective member of the community is the most procedural and the least influenced by special knowledge or personal judgment. Through disciplined averaging, BD hoped to provide more objective assessments of a project's market potential.

Yet, the challenges inherent in relying upon mechanical objectivity in research came into relief when experts strongly advocated for a research project but market data simply did not exist. One example that I witnessed over many meetings and interviews was an effort to build a fast, portable, and inexpensive medical diagnostic device. Funded by the US Defense Department to build a water-contaminant sensor, Diane Kalman had repurposed the system she had built in her spare time to count white blood cells. These counts could tell doctors that an antiviral regimen stopped working and needed to be changed. For the HIV-infected in rural developing settings, simply traveling to a clinic and getting the results weeks later posed a barrier to proper treatment. Kalman had recreated the functionality of a medical instrument the size of a small room in a device that could fit in a backpack. Moreover, the device provided results in minutes and was built from a few hundred dollars of parts. From global health scholars to pharmaceutical executives to scientists at medical instrument companies, every expert who saw the system was stunned at its potential. Many of these outsiders had volunteered significant time to help Kalman.

Kalman now needed $100,000 to fund herself and colleagues to build more rugged prototypes for testing at nonprofits. The trouble was that executives had agreed that funding would only be allocated to projects whose market potential could be specified. Medical instrument makers had tested Kalman's device and found the performance to be equivalent to their products but were unwilling to undercut their existing businesses by licensing-in the technology. Other market opportunities posed too many uncertainties—they often required multiple partners and regulatory change. Internally, Kalman argued that even if there was no obvious path for commercialization, it was reasonable to bet that at least one of these opportunities would materialize. Yet, her assessment or that of outside experts was no longer a legitimate consideration for her lab director, who concluded, "The money is not the issue, Diane. We have $100,000. It's a programmatic issue." The problem was not respect for her abilities or questions about the quality of her results—it was that she could not provide a market rationale. She had to put the project aside and only resumed a year later when she won another government grant.

Thus, the growing emphasis on procedure at ITLab meant that the portfolio system was not a power grab in the traditional sense. A few of the executives who had put a stop to Kalman's project were scientists and understood her system's novelty and potential, but they believed their options were constrained by the need to adhere to an objective process. Hamstringing experts for the sake of objectivity required that administrators also hamstring themselves. As Porter describes of accountants who saw the role of judgment heavily curtailed in their work, "this is authority as Barry Barnes defines it: not so much power plus legitimacy, but power minus discretion" (1992, 642). Tight coupling to the market diminished scientists' and middle managers' authority while giving power to executives who themselves had a very little discretion.

To recap: ITLab sought a quantitative market-driven system for managing research. The system was premised on the objectivity of the market as a means of selection and oversight of research projects. To legitimate the system, BD would have to formalize market analyses as much as possible. This represented an end to loose market coupling. The next section considers some consequences.

Making Researchers Mechanically Objective (Mid-2009 Onward)

The need to reallocate scientists to projects based on the vicissitudes of the market led to a rethinking of what types of employees would "fit" at ITLab in the future. Ideal scientists would be so adaptable as to be interchangeable. If ITLab was to truly be responsive to the market, scientists could have no personal attachment to their work. As one of the BD staff remarked,

> As it looks like something is gaining traction in the marketplace then we're going to have to move people around. That's hard from a researcher point of view, the way that ITLab researchers are. So that's the other thing, how can we make a more flexible workforce to be able to move people around to double down on investments as we need to during the course of the year, not just on a yearly timescale, but a quarterly timescale maybe . . . . this also

> means we should consider hiring more generalist researchers, not necessarily the best specialists in a particular area.

The model of the scientist as devoted specialist was no longer appropriate if ITLab was to quickly respond to market needs. At biweekly and monthly meetings of labs, senior managers echoed this sentiment—reminding staff of the need to hire go-getter generalists rather than the best specialists.

Recognition at ITLab also moved toward commercial achievements and away from technical ones. I observed over e-mail and in large meetings repeated celebrations of recently won commercial contracts. In contrast, scoring in the ninety-ninth percentile on an National Institutes of Health grant review or obtaining a government grant three times larger than any commercial project would go unmentioned. Honorary titles such as "principal scientist" or "research fellow" were previously reserved for individuals who had made major intellectual contributions. Now they also went to scientists who attracted a great deal of commercial funding, regardless of the quality of their research. Whereas publication in a prestigious journal was previously one of the best accomplishments a scientist might have in a performance review, the new top criterion was bringing in commercial funding.

The commercial emphases suggested that rather than relying on the ascetic calling to science (Weber [1918] 1958) scientists would need to find meaning somewhere else—perhaps the organization's goals. A surprisingly similar trajectory to that of ITLab is reported by Mirowski and Van Horn (2005) regarding pharmaceutical Contract Research Organizations looking for a "new breed of scientific researcher . . . who appreciated the pragmatic importance of narrowly formulated questions as well as cost containment innovations, and was less held in the thrall of academic advancement" (p. 510). In order to respond to changing market demands, ITLab scientists would need to be of a more entrepreneurial bent—willing to leap at whatever's lucrative rather than devoting themselves to deep exploration of a few areas. Not only were personal goals and passions no longer a consideration in selecting projects, they became cause for skepticism.

In addition to tightly coupling project selection to the market, executives tried to connect project oversight to market data. VP Anderson wanted to track hours worked to assess the return on investment of projects. He lobbied CEO Knolle for the company-wide use of time cards. Initially resisted by other executives, they eventually accepted them as an experiment (although it did not end while I was there). Time cards were not unheard of at ITLab—they were just not frequently used. Two labs that had

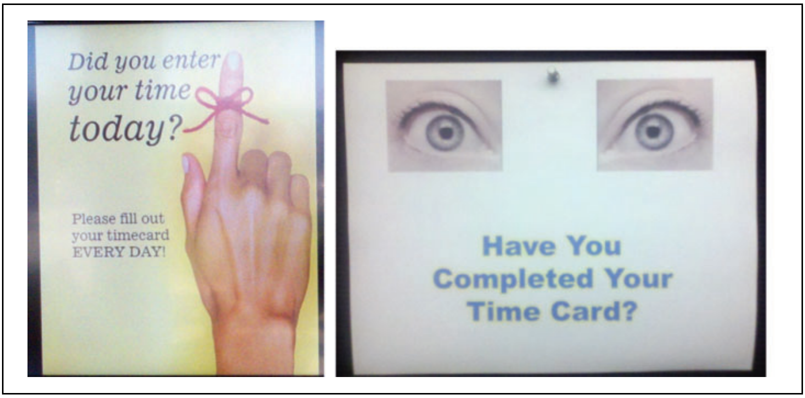

Figure 3. Time card reminders.

significant amounts of US Department of Defense funding required scientists to complete time cards on a biweekly basis. The BD group claimed that time cards would now need to be completed every day in order to be accurate. Finance created extremely detailed categories of time use—each project had categories reflecting anticipated activities. BD posted signs in hallways to remind scientists to fill out their time cards (Figure 3).

BD and finance tried to assure scientists that time card data would not be used for employee evaluations. Yet, it was difficult to dismiss the worry that hours allocated to categories such as "professional development" (the category used for writing articles or attending conferences) represented a liability. These "liability" categories were considered essential for scientists' careers and for ITLab's claim to house valuable expertise. At several organization-wide meetings, I saw scientists ask if management's goal was to undermine scientists' careers. In one lab meeting, a recent PhD asked how she could categorize reading articles into the time card system. A manager who was an ardent supporter of the ITLab's commercial efforts retorted, "You read articles on your own time." Time cards changed how researchers thought about what they did. They could rely less on their sense of what was important and instead had to consider their ability to justify explicitly how time was spent.

Time cards also affected interactions within the lab that were important to the conduct of research. Attending talks and meetings out of curiosity or a sense that they were relevant had been quite common prior to 2008 and 2009. As one scientist who had been at ITLab over thirty years noted, "With time cards, there are these unintended disincentives, people feel like they can't even have time to communicate or to collaborate outside of the things that they are able to assign hours to. And that is very destructive." Having to account for each hour worked had a negative effect on social relations in the lab (see Leslie 2010 for a similar consequence in a forensic lab).

One physicist described his way of avoiding the constraints of the time card surveillance in order to think clearly with his colleagues. Every two weeks, he invited colleagues, postdocs, and PhD interns to his house where they would discuss research problems in the garden, "We really try to be focused with no distractions of people coming in or the phone ringing or a manager on the side saying, 'Now you guys are already talking for half an hour and haven't had any ideas.'" Although it exaggerates the severity of oversight, this quote captures the perceived need for freedom from constant explicit justification in order to do research.

To summarize, tighter coupling to the market changed project selection and project oversight. The need to prioritize projects based on the vicissitudes of the market meant that researchers would need to tamp their expectations about being passionate about their work or developing deep expertise. Moreover, the use of time cards had the effect of discouraging activities that supported research but that could not be accounted for in advance. Both these changes had the effect of de-emphasizing the intrinsic and collegial motivations in research. They represented the exclusion of subjectivity and subjective judgment. The next section considers scientists' and managers' painting of the problem as one of trust in the goodwill and judgment of scientists (with opposing conclusions).

Loose Coupling, Trust, and Ignorance (Mid-2009 Onward)

Meyer and Rowan (1977, 1978) emphasized the role of purely ceremonial inspection in facilitating loose coupling. Ceremonial inspection ended at ITLab with the introduction of the portfolio system and time cards. Managers could no longer claim ignorance about how scientists spent large amounts of their time. Now, knowing exactly what scientists were doing, the only way for managers to allow scientists to use their judgment in choosing how they spent their time was to trust them. Trust in the goodwill and judgment of scientists is often described as the working solution that allows research to proceed despite its uncertainties (Polanyi 1946; Shapin 1994, 2008). Yet, supporters of the portfolio system claimed that personal trust was no longer necessary because judgment was less necessary. In any case, they believed it would be foolish to trust individual scientists. In contrast, scientists argued that expert judgment was the only way of coping with the uncertainties of research, making trust (even implicit via ignorance) essential.

Explicit criticism of the portfolio system focused on the idea that Knightian uncertainty could be transmuted into risk, making expert judgment less critical and thereby making trust a nonissue. As many interviewees noted, if planning research accurately were possible, there would be no need to bother doing the research. A week after the announcement of the portfolio system, one scientist/middle manager said,

> Well, I went and worked on my resume that day. To me, if senior staff takes that seriously, we are in a crash-and-burn failure mode. And I think that way only because it seems like it was a tool developed to substitute for good judgment. So I think good judgment is essential among managers, and good judgment means that a manager looks at ideas, evaluates them on their own terms, without a rubric or a standard, decides if they're good or not; if they're good, champions them, raises money for them, and defends them, and if they're bad, he kills them. Right? And that's true at my level, it's true at my boss's level, and it should be true at the CEO's level. So I don't see how anyone could systematize that type of judgment because technology has never been the same. Technology is the product of people constantly making judgments about how to place their bets. And if you knew of a system that could make bets, and make them better than good judgment, you would be a billionaire, right? And maybe Warren Buffett could do it, but Warren Buffett has said he can't, that he just uses good judgment, and he has no systematic way of making decisions. Peter Lynch said the same thing. So a couple of famous people who systematically beat the market said there's no system for doing it.

Many were skeptical of the possibility for evaluating research according to a system of rules. A good analogy was financial markets. If no methodology could optimize market investments, how could rules be devised to optimize research investments? Scientists argued that the ongoing need to accommodate new knowledge and new uncertainties made judgment indispensable.

Judgment, and faith in judgment, became even more important when faced with technical *and* market uncertainties. The linked uncertainties of technology and market could deepen the problem of translating uncertainty into risk, casting even more doubt on the resulting estimates. A physicist with multiple government grants noted,

> What made ITLAB really famous long ago was not looking for replacement technologies in existing markets, but having a vision of new markets. This is, of course, where the business people have the biggest problem. All of a sudden they put their head in the sand because it's a gut feeling. It's a judgment.

The fact that no obvious market existed for many technologies created a chicken-and-egg problem that many scientists found extremely frustrating. Without a market to point to, they could not justify a project; without research results, they could not justify a market. The only way out of this problem, scientists argued, was to trust their judgment.

A challenge for advocates of relying upon judgment was how to hold individuals accountable. How would scientists and their managers be held accountable when projects lost money? As one experienced computer scientist noted,

> The past CEO of ITLab, who became chief strategist of ITCorp for a while, told me that he used to tell the people in ITCorp that he could guarantee them that fifty percent of the work at ITLab was of absolutely no value. The only problem was that he didn't know what fifty percent it was. So unless you have that kind of attitude and that's conveyed to the business executives and they're willing to live with that then you don't really have the freedom to do great research.

When finances were tight, temptation grew to end loose coupling and the "logic of good faith and confidence" (Meyer and Rowan 1977; Hallett 2010). When it became naive to trust the motivations and judgment of scientists, failure became a sign of bad faith, thus reinforcing the need for an objective system of planning research (Sitkin and Stickel 1996). The only sure way to reduce the chances of "failures" was to pursue projects with fewer uncertainties.

To some executives, allowing scientists to use their judgment seemed foolish because they simply were deeply skeptical of scientists' motivations. One executive hired to create a company based on a research project echoed this suspicion: "If you look at the motivations of the researchers—there's a number of people in this place who are playing in their own something. And they're being paid an awful lot of money to play in their own something ...." Within his own projects, he hired engineers to single-mindedly focus on "digging a ditch." He even encouraged lab directors to hire more non-PhDs because he believed they were less distractible workers. Particularly coming from non-research environments, business-people tended to think that researchers needed to be tamed or reined in because the chaos of research at ITLab suggested that, left to their own devices, they would not pursue ITLab's business interests in good faith.

These managers viewed good faith efforts as the product of explicit incentives rather than a combination of the subjective and contextual factors. For instance, Anderson framed research motivation in terms of fear of being thrown out on the street. Signaling the change in culture he hoped for but had not seen, Anderson viewed the environment of start-ups as a good model,

> I think we have to be really critical about what makes a healthy startup. In my opinion that means knowing that you can lose your house after you've mortgaged it, knowing that you might be going without a paycheck for several months. It's kind of that hungry-passionate feeling .... I have a concern that we can't duplicate that within ITLab.

Anderson believed that individuals did their best work when confronted with strong extrinsic pressures. Yet, he ignored the fact that start-ups were primarily engaged in commercialization—a very different mandate from an industrial lab. He had not succeeded in making ITLab as high pressure an environment as a start-up. On the contrary, researchers were so uninspired by their work that the parking lot was mostly empty by six every evening.

Disenchantment (Early 2010 Onward)

As ITLab pursued a more commercial strategy, the nature of work shifted away from research and toward engineering, which many researchers found less compelling. Some of these researchers described their previous function as gaining a theoretical understanding of a technology's performance. Many new projects, particularly those funded by outside firms, consisted of adding a feature to an already existing product, making a system more reliable, or more manufacturable. The pressure in these projects was so high that when I asked some researchers why they did not publish on their new projects, they said that they did not have time to understand the principles underlying their work sufficiently in order to make it interesting to an outside audience.

This shift occurred for two reasons. First, external commercial firms in information technology, who were potential licensees or buyers of ITLab technologies, began requiring ITLab to further develop its technologies before contracting with ITLab for those technologies. These firms had faced years of cost cutting themselves and thus were less willing than before to license-in relatively unproven technologies. As one BD person who had been trying to sell ITLab's technologies noted, "They have to be able to see that you're reducing the technical risk for them. They're not going to do it. They don't like to invest engineering resources to reduce technical risk." Management encouraged researchers to reduce the market and technical risk of their projects in order to make them commercially viable, which essentially meant more engineering or development work. After one such presentation by management, one older researcher protested that "this is the kind of thing that we used to call engineering." But management's view was clear: ITLab would have to do more engineering and development work to get new technologies closer to product in order to assure firms that licensing-in a technology made sound business sense.

Second, because they were incentivized by commissions and held accountable to quarterly goals, ITLab's BD staff tried to obtain commercial contracts even when they involved the outsourcing of another firm's development work. For example, in one large project, the lead researcher was unable to help the client because the challenges were engineering challenges: making a system easier and cheaper to manufacture. As the BD person attached to that project explained,

> If you look at Joerg's skills set and Wen's skills set, Joerg is a much better engineer, right, and Wen is—I don't want to say is a much better scientist but Wen is a scientist, right? So where it came time to deliver something to the customer where you had to take manufacturing sensibilities into account, the manufacturing design rules, Joerg is just much better suited to doing that.

Outsourced development often required the use of engineering skills rather than research skills. The increase in contracts that involved outsourced development work contributed to many scientists' perception that the portfolio system encouraged more mundane and predictable work. Not only was this type of work less appealing to researchers, most thought of themselves as poor engineers and thus ill-equipped to handle such work.

Beyond the change in the nature of work from research to engineering, ITLab faced the challenge of matching substantive expertise to client needs. Many scientists found themselves assigned to projects that seemed to be a mismatch for their expertise. While some complained about this in interviews, perhaps the best indication comes from an internal forum post with fourteen approvals,

> . . . I feel like a commodity researcher who is expected to fill whatever role happens to be needed based on the work that was brought in, regardless of

> whether I have the right skills and certainly without concern for my interest level. As a result, I rarely work on a project I'm interested in and I often feel like I am not producing world-class work, which is what our customers are paying for .... Of course I'm willing to chip in from time to time when it's needed, but it's deeply dissatisfying when you're always chipping in and never flourishing in your work ...

Being told to work on anything a customer would pay for led some to feel as if they were interchangeable. Not only was it difficult for scientists to make significant contributions, it was difficult to develop expertise and identify with their work.

The importance of the link between personal attachment and expertise was highlighted when it came time to cut projects that lacked acceptable financial projections. VP Anderson noted that having been a scientist himself, he knew that it was hard to stop work on a labor of love. Yet, one of the early steps in the portfolio system was to conduct a review of each project in the lab, complete with estimates of market potential created by the BD staff. One computer scientist whose project was stopped worried that his colleagues would stop being personally invested in their work, "In some sense we're getting conditioned to not be passionate about ideas, because you are as good as your next contract." He went on to state, "If you take a passionate person and tell them to work on something that they don't care about, it's like killing their soul."

The simultaneous subtraction of personally significant projects and addition of poorly matched projects led to complaints that the lab culture was deteriorating and that knowledge was becoming shallow. In a consultancy's survey of ITLab, most businesspeople described ITLab as "cutting edge" while most researchers did not. Scientists publicly stated that "ideas are getting really stale in the trenches" and argued that "our current system seems to guarantee that we will lag the market rather than lead it." In private, a few senior scientists who had been at ITLab their entire careers confronted VP Anderson and told him that he was running ITLab into the ground and that it would have been better off without Anderson or his staff. Moreover, in interviews and observations, I heard widespread concern about a hemorrhaging of talent—"we're losing some of our more creative lights." Away from managers, scientists joked about their desire to work for other employers.

Finally, many scientists used government grants to pursue deeper research that better fit their expertise and intellectual interests. ITLab's official policy discouraged this, as BD staff argued that scientists should only pursue government funding if it had clear relevance to a commercial market. One BD director elaborated the position, "That's exactly the situation we don't like, which is when researchers get government funding and they say, 'Don't bother me, I'm covered.' The truth is we look at the opportunity cost: could they be working on something that is more valuable?" Even though government funding was seen as allowing researchers to gain depth in a certain area, executives discouraged it if the link to future commercial work was unclear. Despite the admonitions, scientists knew ITLab was too desperate to turn down money and applied anyway.

Managers Confront the Problem of Judgment (Late 2010 Onward)

Some senior managers had been concerned all along that excluding judgment from the management of research would harm research and thereby harm ITLab's viability. In interviews one year after the unveiling of the portfolio system, they were willing to articulate their alternative vision for governance at ITLab. At least two of the four lab managers, both of whom had been scientists, felt that encouraging the cultivation of deep expertise (with an eye to fields that would be important externally) was the most important job of management. They pointed out that the original reason for having strategic reviews was to determine which research fields were growing in order to decide which ITLab groups should expand. In contrast, if a research field appeared to be declining externally, management would not replace departing researchers or reassign those who remained to new groups.

Thus, the alternative to the portfolio system that these executives advocated was one in which cultivating expertise was the immediate goal, with increased revenue based on that expertise a by-product. Within this model, projects would be nested within programs and programs would be nested within groups. Groups would last at least ten years, programs at least five and projects anywhere between six months and three years. Even if any particular project had no tangible technical or economic success, it would at least contribute to the repertoire of tools in the program and expertise within the group. As one of these directors summarized, "You need to focus on learning so that people have expertise and [ITLab has] more advanced technology." To some executives who had been scientists, the overarching priority in choosing projects ought to be the deepening of expertise.

This alternative system did not require detailed market modeling or predictions. It did not have the portfolio system's biases against the uncertainties endemic in research. One director who had (ironically) spent twenty years designing statistical planning systems described his approach to research management as moving between technical and market uncertainties in a project, "You should have some early answers early on, but it doesn't have to all be hashed out and over time you'll improve on your answers." For him, management's goal is to decide which technical and market uncertainties should be addressed next; that goal is accomplished by relying on managers' common sense, scientists' technical expertise, and the BD group's market expertise. Given this perspective, it made little sense for new projects to be assessed centrally, without relying upon the judgment of other members of the research program.

Moreover, all of the lab directors disagreed with CEO Knolle's exclusive focus on obtaining commercial research funding and discouragement of pursuing government grants that did not have clear commercial relevance. Lab directors were worried about the hollowing out of expertise given the nature of the commercial work ITLab was securing, "... [O]verall this year, but hopefully not long term, there has been more of a short-term perspective. Clients we see are interested in something that impacts their bottom line this year or next year ...." Projects to assist large companies in developing soon-to-be products tended to contribute even less to the accumulation of expertise. In a parallel vein, the IP group argued that patents resulting from constantly shifting foci and related to the products of major corporations would be overly narrow while know-how would remain superficial.

Even the portfolio system's greatest advocate, VP Anderson, began to see the limitations of a purely quantitative approach to project selection. After a senior IP manager had decided to leave ITLab, he confronted VP Anderson and argued that the portfolio system created incentives that would produce an "aversion to variance." This suggested that even if ITLab was not intentionally averse to risk, its discomfort toward large negative deviations in the outcome of any individual project made it behave as if it was risk-averse. In the meeting, Anderson replied, "We're asking some similar questions. I am going to admit that I don't have the luxury of asking the question about our aversion to variance, but I think it's a really good question ...." And despite his early confidence that the central problem in financially modeling research was finding the right tool for the task, Anderson added, "I do worry about our ability to predict." The meeting revealed that Anderson himself had begun to see the limits of financially modeling research. Yet, in public, Anderson only conceded that the conservative allocation of projects in the portfolio system (60 percent in "core") had been a bit unrealistic for a research center.

Eventually, executives at ITCorp noticed the declining morale brought on by the departure of ITLab's most prominent scientists (these executives were also in touch with directors at ITLab and had access to annual surveys showing a decline in job satisfaction). In addition to the aforementioned changes in the substance of work, the constant emphasis on profitability had taken its toll. A top-voted comment on an internal forum in the summer of 2010 read, "I realize we are a business, but everything is about money/cash/profits. This cash-only focus seems to really bring down morale." In December 2010, quite abruptly, an executive from ITCorp headquarters sent a company-wide e-mail announcing that CEO Knolle was retiring and thanking him for his service.[2] The next day, the executive flew to California to explain to an openly disbelieving audience that CEO Knolle had done very good work, had retired, that ITLab appeared to be on an upward trajectory, and thus staff should expect no major changes in operations.

While many scientists and middle managers (and even the lab directors) were optimistic about potential for improving the research culture under a new CEO, real change was slow. The new CEO was much more supportive of government-funded work and did not see it as inferior to commercially research. But while he asked at every meeting he attended how morale might be improved, he did not accept key suggestions, such as eliminating time cards or abandoning the idea of a portfolio system.

Meanwhile, over the course of the study period, scientists continued to find other ways to increase their autonomy and pursue work of greater interest. For example, government grant applications grew from twenty-three in 2007 to thirty-four in 2008 to forty-five in 2009 and jumped to over eighty in 2010. Some suggested adopting the policy of allocating 10 to 20 percent of time to personal projects, which is well known in industrial labs. Additionally, a few experienced scientists put together a committee for a "senior hire"—a famous scientist who could use informal authority to energize and organize a few groups within ITLab. Finally, scientists advocated for a scientist-run program to support small exploratory projects. Under the new CEO, they began this program, but it represented a little less than 2 percent of the ITLab's budget.

By opening up room for government grants and allowing scientists greater control in choosing projects, ITLab had, to a small extent, loosened coupling to the market. Yet, the reversal remained too timid and the foundations for a return to the guild model of governance at ITLab had been weakened too much (as evidenced by the desire for a senior hire). In several cases, scientists who left ITLab for another corporate lab turned around and hired away the best of their former ITLab colleagues. Scientists still raised concerns about the seemingly high turnover rate (particularly among the most published and well-known scientists), and the shallowness of research work. The lab could not return to its glory days of the 1970s and 1980s when managers defended the high variance that comes with high autonomy to executives. Tight market coupling may have been maladaptive in practical terms, but its legitimacy made it very difficult to challenge.

Conclusion

Facing constant financial pressure, ITLab executives seized upon a market-based methodology that they thought would promise higher and more consistent revenues than an informal system of governance. They ended an era of loose coupling that had kept the goals and practices of business and research separate but in dialogue. The portfolio system represented an effort to tightly couple research work to the marketplace. In the interest of space, this case study focused on the displacement of judgment by quantitative market criteria in only two aspects of governance: project selection and project oversight. It showed how the exclusion of judgment made it difficult to justify research projects and activities, tending to replace research work with more predictable engineering and development work.

Despite the seeming straightforwardness of the market-driven approach, it did not translate into financial security, much less significant profitability for ITLab. Client firms, from Fortune 500s to venture-backed start-ups, tended to be much more price-sensitive than anticipated. Several of the largest commercial contracts, particularly those based on clear deliverables, expanded dramatically in costs. Sometimes, these projects went deep into the red because of the lack of expertise in engineering and development at ITLab and the inherent difficulties of accurately forecasting the work required in such projects. When I stopped visiting ITLab, the sense of precariousness had not lifted—a key government grant or sale of a technology license before the close of the fiscal year remained crucial to keeping the lab solvent.

Until the introduction of a market-driven planning system, ITLab had successfully preserved the conflicting goals and norms of science and the marketplace. There had never been a reconciliation of the ideas and practices associated with pure academic research and the competitive marketplace. Since its founding, people had always argued about whether it ought to be more focused on contributing to the business or doing deeper and more autonomous research. An active effort to sustain the contradictions in settings in which the academic and market norms and practices are converging can be seen as pursuing an *incompletely theorized agreement*. An incompletely theorized agreement allows parties to pursue common goals at one level while disagreeing about their premises or implications (Sunstein 1995). These agreements allow patrons of science and scientists to disagree about goals, norms, and practices and still cooperate. Tight coupling represents a threat to this agreement because it seeks intentionality and logical coherence across all levels—it demands reconciliation where tension and contradiction prove more fruitful (Sunstein 1995; Stark 2009). This case illustrated the consequences of reconciling the contradictions of science and the marketplace in the context of an industrial lab, but one could imagine similar consequences for efforts seeking to reconcile scientific practice with the external pressures faced by academic or government organizations.

The case study also suggested that a key error that enables the overreliance on rules in scientific governance is the misrecognition of expertise as objective and impersonal. As Porter argues, "The public rhetoric of scientific expertise studiously ignores the tacit knowledge and informed intuition that go into science" (1995, 7). This is the view of the expert as rule-based system (Dreyfus and Dreyfus 2005). If scientists were merely rule-based systems, perhaps research could be managed in the way VP Anderson initially hoped. Even former scientists like Anderson could lose sight of the importance of tacit and subjective knowledge in the conduct of research. Weber wrote of one future in which the disenchantment of the world yielded "specialists without spirit" ([1958] 2012, 182). Yet, in this study, scientists themselves hinted that there may be no "specialists without spirit."

Reliance upon subjective judgment may be a prerequisite for the ongoing reenchantment of research. As Saler (2006) points out, the *Oxford English Dictionary* defines enchant in a negative sense as "delude, befool" and in a positive sense as "delight, enrapture." These are simultaneously true in describing the enchantment of science. Facing uncertainty in science is being aware that one might be wasting time or on the heels of a discovery. The ongoing need for subjective judgment to cope with this uncertainty makes research a deeply personal endeavor. This article's goal was not simply to point out the contradictions of rationally planning research but also to suggest that efforts to govern research systematically and impersonally might backfire precisely because they leave little room for subjective considerations. It was the exclusion of subjectivity that concerned Feyerabend over fifty years ago when he offered philosophers who believed they could formulate a programmatic logic of scientific discovery, "They can keep science; they can keep reason; they cannot keep both" (1957, 231).[3]

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Notes

1. In addition to these, I conducted sixty interviews related to intellectual property that I do not count here since they bore only tangential relevance to this study.
2. ITCorp stated that CEO Knolle was simply retiring and because of "legal concerns" was reluctant to specify any issues in public forums. Nonetheless, senior managers intimated that the loss of key researchers, falling morale, and reluctance to pursue government funding led to the dismissal.
3. Feyerabend is using *reason* in a way that is more similar to our understanding of *rationality*. He goes on to write, "Reason, at least in the form in which it is defended by logicians, philosophers of science and some scientists, does not fit science and could not have contributed to its growth. This is a good argument against those who admire science and are also slaves of reason . . . ." (1957, 231).

References

Barnes, Barry. 2007. "Catching Up with Robert Merton Scientific Collectives as Status Groups." *Journal of Classical Sociology* 7 (2): 179-92.

Benner, Mary J., and Michael L. Tushman. 2003. "Exploitation, Exploration, and Process Management: The Productivity Dilemma Revisited." *Academy of Management Review* 28 (2): 238-56.

Berman, Elizabeth Popp. 2008. "Why Did Universities Start Patenting? Institution-building and the Road to the Bayh-Dole Act." *Social Studies of Science* 38 (6): 835-71.

Berman, Elizabeth Popp. 2011. *Creating the Market University: How Academic Science Became an Economic Engine*. Princeton, NJ: Princeton University Press.

Berman, Elizabeth Popp. 2014. "Not Just Neoliberalism Economization in US Science and Technology Policy." *Science, Technology, & Human Values* 39 (3): 397-431.

Boyd, Thomas Alvin. 2002. *Charles F. Kettering: A Biography*. Frederick, MD: Beard Books.

Colyvas, Jeannette A., and Walter W. Powell. 2006. "Roads to Institutionalism: The Remaking of Boundaries between Public and Private Science." In *Research in Organizational Behavior: An Annual Series of Analytical Essays and Critical Reviews*, edited by B. Staw, 305-53. Amsterdam, the Netherlands: Elsevier.

Dennis, Michael Aaron. 1987. "Accounting for Research: New Histories of Corporate Laboratories and the Social History of American Science." *Social Studies of Science* 17 (3): 479-518.

Dreyfus, Hubert L., and Stuart E. Dreyfus. 2005. "Peripheral Vision Expertise in Real World Contexts." *Organization Studies* 26 (5): 779-92.

Evans, James A. 2010. "Industry Collaboration, Scientific Sharing, and the Dissemination of Knowledge." *Social Studies of Science* 40 (5): 757-91.

Feyerabend, Paul. 1957. *Against Method*. London, UK: Verso.

Hallett, Tim. 2010. "The Myth Incarnate Recoupling Processes, Turmoil, and Inhabited Institutions in an Urban Elementary School." *American Sociological Review* 75 (1): 52-74.

Hounshell, David A. 1998. "Measuring the Return on Investment in R&D: Voices from the Past, Visions of the Future." In *Assessing the Value of Research in the Chemical Sciences*, edited by National Research Council, 6-17. Washington, DC: National Academies Press.

Hounshell, David A., and John Kenly Smith. 1988. *Science and Corporate Strategy-DuPont R&D: 1902-1980*. Cambridge, UK: Cambridge University Press.

Kleinman, Daniel Lee, and Steven P. Vallas. 2001. "Science, Capitalism, and the Rise of the 'Knowledge Worker': The Changing Structure of Knowledge Production in the United States." *Theory and Society* 30 (4): 451-92.

Knight, Frank H. 1921. *Risk, Uncertainty, and Profit*. Boston, MA: Hart, Schaffner & Marx; Houghton Mifflin.

Lave, Rebecca, Philip Mirowski, and Samuel Randalls. 2010. "Introduction: STS and Neoliberal Science." *Social Studies of Science* 40 (5): 659-75.

Leslie, Myles. 2010. "Quality Assured Science: Managerialism in Forensic Biology." *Science, Technology, & Human Values* 35 (3): 283-306.

Meyer, John W., and Brian Rowan. 1977. "Institutionalized Organizations: Formal Structure as Myth and Ceremony." *American Journal of Sociology* 83 (2): 340-63.

Meyer, John W., and Brian Rowan. 1978. "The Structure of Educational Organizations." In *Environments and Organizations*, edited by M. W. Meyer, 78-109. San Francisco, CA: Jossey-Bass.

Mirowski, Philip. 2004. *The Effortless Economy of Science?* Durham, NC: Duke University Press.

Mirowski, Philip. 2011. *Science-mart: Privatizing American Science*. Cambridge, MA: Harvard University Press.

Mirowski, Philip, and Robert Van Horn. 2005. "The Contract Research Organization and the Commercialization of Scientific Research." *Social Studies of Science* 35 (4): 503-48.

Moore, Kelly, Daniel Lee Kleinman, David Hess, and Scott Frickel. 2011. "Science and Neoliberal Globalization: A Political Sociological Approach." *Theory and Society* 40 (5): 505-32.

Orton, J. Douglas, and Karl E. Weick. 1990. "Loosely Coupled Systems: A Reconceptualization." *Academy of Management Review* 15 (2): 203-23.

Owen-Smith, Jason, and Walter W. Powell. 2001. "Careers and contradictions: faculty responses to the transformation of knowledge and its uses in the life sciences." In *The Transformation of Work (Research in the Sociology of Work, Volume 10)*, edited by Steven Vallas, 109-140. Emerald Group Publishing Limited. http://www.emeraldinsight.com/doi/pdfplus/10.1016/S0277-2833%2801%2980023-6.

Polanyi, Michael. 1946. *Science, Faith, and Society*. Chicago: University of Chicago Press.

Porter, Theodore M. 1992. "Quantification and the Accounting Ideal in Science." *Social Studies of Science* 22 (4): 633-51.

Porter, Theodore M. 1995. *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life*. Princeton, NJ: Princeton University Press.

Powell, Walter W. 1990. "Neither Market Nor Hierarchy: Network Forms of Organization." In *Research in Organizational Behavior: An Annual Series of Analytical Essays and Critical Reviews*, edited by Barry M. Staw and Larry L. Cummings, Vol. 12, 295-336. Greenwich, CT: JAI Press.

Powell, Walter W., Kenneth W. Koput, and Laurel Smith-Doerr. 1996. "Interorganizational Collaboration and the Locus of Innovation: Networks of Learning in Biotechnology." *Administrative Science Quarterly* 41 (1): 116-45.

Powell, Walter W., and Kurt Sandholtz. 2012. "Chance, nécessité, et naïveté." In *The Emergence of Organizations and Markets*, edited by John F. Padgett and Walter W Powell, 434-65. Princeton, NJ: Princeton University Press.

Power, Michael. 1999. *The Audit Society: Rituals of Verification*. Oxford, UK: Oxford University Press.

Power, Michael. 2008. "Research Evaluation in the Audit Society." In *Wissenschaft unter Beobachtung: Effekte und Defekte von Evaluationen*, edited by Hildegard Matthias and Dagmar Simon, 15-24. Berlin: Springer-Verlag.

Reich, Leonard S. 1985. *The Making of American Industrial Research: Science and Business at GE and Bell, 1876-1926*. New York: Cambridge University Press.

Robson, Keith. 1994. "Connecting Science to the Economic: Accounting Calculation and the Visibility of Research and Development." *Science in Context* 7 (3): 497-514.

Saler, Michael. 2006. "Modernity and Enchantment: A Historiographic Review." *The American Historical Review* 111 (3): 692-716.

Shapin, Steven. 1994. *A Social History of Truth: Civility and Science in Seventeenth-century England*. Chicago: University of Chicago Press.

Shapin, Steven. 2008. *The Scientific Life: A Moral History of a Late Modern Vocation.* 1st ed. Chicago: University of Chicago Press.

Sitkin, Sim B., and Darryl Stickel. 1996. "The Road to Hell: The Dynamics of Distrust in an Era of Quality." In *Trust in Organizations: Frontiers of Theory and Research*, edited by Roderick M. Kramer and Tom R. Tyler, 196-216. Thousand Oaks, CA: Sage.

Slaughter, Sheila, and Gary Rhoades. 1996. "The Emergence of a Competitiveness Research and Development Policy Coalition and the Commercialization of Academic Science and Technology." *Science, Technology, & Human Values* 21 (3): 303-39.

Stark, David W. 2001. "Ambiguous Assets for Uncertain Environments: Heterarchy in Postsocialist Firms." In *The Twenty-first-century Firm: Changing Economic Organization in International Perspective*, edited by Paul DiMaggio, 69-104. Princeton, NJ: Princeton University Press.

Stark, David W. 2009. *The Sense of Dissonance: Accounts of Worth in Economic Life*. Princeton, NJ: Princeton University Press.

Stephan, Paula. 2012. *How Economics Shapes Science*. Cambridge, MA: Harvard University Press.

Stokes, Donald E. 1997. *Pasteur's Quadrant: Basic Science and Technological Innovation*. Washington, DC: Brookings Institution Press.

Strathern, Marilyn. 2000. *Audit Cultures: Anthropological Studies in Accountability, Ethics, and the Academy*. London, UK: Routledge.

Sunstein, Cass R. 1995. "Incompletely Theorized Agreements." *Harvard Law Review* 108 (7): 1733-1772.

Vallas, Steven Peter, and Daniel Lee Kleinman. 2008. "Contradiction, Convergence and the Knowledge Economy: The Confluence of Academic and Commercial Biotechnology." *Socio-Economic Review* 6 (2): 283-311.

Varma, Roli. 2000. "Changing Research Cultures in U. S. Industry." *Science, Technology, & Human Values* 25 (4): 395-416.
Weber, Max. (1918) 1958. "Science as a Vocation." *Daedalus* 87 (1): 111-34.
Weber, Max. (1958) 2012. *The Protestant Ethic and the Spirit of Capitalism*. New York: Courier Dover.
Weick, Karl E. 1976. "Educational Organizations as Loosely Coupled Systems." *Administrative Science Quarterly* 21 (1): 1-19.

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