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Science and the Public Good in the American Research University

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By

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Committee in charge:

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ABSTRACT

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This study uses the public controversy that erupted around the research partnership between the University of California, Berkeley and British Petroleum founding the Energy Biosciences Institute as an entry point into a three-part analysis in the sociology of knowledge, broadly defined. A systematic reconstruction of the controversy and review of existing scholarship on changes in the American science regime, transformation of public research universities and the politics of emerging technologies indicate that both lay and scholarly accounts on these related phenomena can be organized roughly under two headings. Under one heading, observers draw on an implicit or explicit attachment to the Enlightenment ideal of reason to wage critiques that are not adequately attentive to the conditions of production of science and technology. Under the other, observers offer detailed accounts of university-industry research relations, changes in universities and developments in technoscience, but without adequate critique.

To move beyond this impasse, I propose to resuscitate Robert K. Merton's proposal to theorize science as an institution in relation to its societal context, but dispense with the idea of "relative autonomy" that informs both his and Pierre Bourdieu's theories of science. Taking the Energy Biosciences Institute as a signpost for key forces shaping science and technology today, I argue that the production of reliable knowledge is strongly interdependent with economic capital and technology, and that this interdependence renders the stipulation of autonomy obsolete. Combining this argument with insights from science and technology studies regarding (i) the variety of products of research, (ii) the dissolution of the boundary between science and technology; and (iii) how technoscience is inherently political, I argue that it is the nature of associations and engagements that shapes science's perils and promise for different constituencies. In this framework, the basis for critique is no longer the loss of autonomy, but the lack of democracy resulting from inequalities of various forms of capital. This proposed shift in the basis of critique helps advance the definition and defense of the public interest in debates on universities and on technoscience.

for Barış

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LIST OF ACRONYMS AND ABBREVIATIONS

AAUP	American Association of University Professors
ANT	Actor-Network Theory
ASUC	Associated Students of the University of California
BIO	Biotechnology Industry Organization
BC	Budget Committee, Berkeley Division of the Academic Senate
BDP	Berkeley Daily Planet
BP	British Petroleum
CAPRA	Committee on Academic Planning and Resource Allocation, Berkeley Division of the Academic Senate
CEO	Chief executive officer
CNR	UCB College of Natural Resources
CNRS	Centre National de la Recherche Scientifique
COR	Committee on Research, Berkeley Division of the Academic Senate
DIVCO	Divisional Council of the Berkeley Division of the Academic Senate
EBI	Energy Biosciences Institute
ESPM	UCB Department of Environmental Science, Policy and Management
FTCR	Foundation for Taxpayer and Consumer Rights
GA	UCB Graduate Assembly
GHG	Green house gasses
GMO	Genetically-modified organism
HSPH	Harvard School of Public Health
HSSF	*Humanities and Social Sciences Faculty*
KALW	Local public radio station based in San Francisco
LBNL	Lawrence Berkeley National Laboratory
LCA	Life-cycle analysis
LCFS	Low-Carbon Fuel Standard
LRDP	Long Range Development Plan
NASA	National Aeronautics and Space Administration
NADI	Novartis Agricultural Discovery Institute
NIH	National Institutes of Health
NPSS	*New Political Sociology of Science*
NSF	National Science Foundation
PMB	UCB Department of Plant and Microbial Biology
R&D	Research and Development
RSTF	Report by Senate Task Force on University-Industry Partnerships
SCA	Strategic Corporate Alliance
SEF	*Sciences and Engineering Faculty*
SSK	Sociology of Scientific Knowledge
UCB	University of California, Berkeley
UCB-N	*University of California, Berkeley-Novartis Agreement*
UCLA	University of California, Los Angeles
UCORP	University Committee on Research Policy
UCSD	University of California, San Diego
UCSF	University of California, San Francisco

UIRR	University-Industry Research Relation
UIUC	University of Illinois at Urbana-Champaign
VCR	Vice Chancellor for Research

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CHAPTER ONE – INTRODUCTION

"A TREMENDOUS DAY FOR MOTHER EARTH"

On February 1st, 2007, Robert Birgeneau, then Chancellor of the University of California, Berkeley, opened a press conference stating that it was "a historic day on the Berkeley campus" and invited the then chairman and president of British Petroleum America, Robert Malone, to the podium.[1] Standing between then California Governor Arnold Schwarzenegger and Chancellor Birgeneau, Robert Malone announced that British Petroleum (BP) was joining the University of California, Berkeley (UCB) in a partnership, which he believed "would have an immense positive impact" on the world. With $500 million of BP funding to be provided over ten years, the partnership would found the Energy Biosciences Institute (EBI) to conduct research in the biosciences for the development of biofuels.

Robert Malone was followed by prominent functionaries who shared his excitement.[2] President of the UC system, Robert Dynes, began his remarks by repeating how good a day it was: "This is a really good day for California. It's a good day for UC but it's really a good day for California. It's a tremendous day for mother earth." Governor Schwarzenegger mentioned AB 32 ("the Global Warming Solutions Act of 2006, which set the 2020 greenhouse gas emissions reduction goal into law"[3]) and the executive order he signed in January 2007 establishing a Low-Carbon Fuel Standard (LCFS)[4]; and stated that this partnership was the perfect complement to those moves in making California "the leader in the clean energy revolution." Illinois Governor Rod Blagojevic likened EBI's research agenda to NASA's space programs: "This is a historic day for California, for Illinois, and for the rest of our country. We can do in biofuels with EBI what NASA did for space. We can lead our country into this new century being right and smart about the environment." UCB Chancellor Robert Birgeneau echoed Blagojevic, describing the partnership as "our generation's moonshot." BP's chief scientist, Steven Koonin, talked about the promise of biofuels, and his excitement about bringing academic and private sector scientists to collaborate in novel ways. Several other speakers commended the collaboration among public and private institutions to address major challenges of the day.

Yet, not all those who heard about the partnership and its research agenda were as excited and optimistic. Soon after the press conference, faculty members, students, and environmental and public interest organizations began to voice their concerns through public statements, administrative means, protests and teach-ins. A group of students and postdocs initiated a campaign, *StopBP-Berkeley*, and put up an elaborate website compiling statements, op-eds, and news of protests and teach-ins critical of the partnership.[5] In the first protest on March 1st, 2007, two students dressed in white coats with BP logos poured molasses in front of California Hall to invoke the imagery of oil spills in expressing their objection to BP's arrival on campus.

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[1] http://www.berkeley.edu/news/media/releases/2007/02/01_ebi.shtml

[2] These included, in order of appearance, UC President Robert Dynes, California Governor Arnold Schwarzenegger, Illinois Governor Rod Blagojevic, UCB Chancellor Robert Birgeneau, University of Illinois at Urbana-Champaign Vice Chancellor for Research Charles Sakowski, Director of Lawrence-Berkeley National Laboratory Steven Chu, BP Chief Scientist Steven Koonin, and California Senators Don Perata, Richard Ackerman.

[3] http://www.arb.ca.gov/cc/ab32/ab32.htm

[4] http://www.energy.ca.gov/low_carbon_fuel_standard/

[5] http://stopbp-berkeley.org

Critics viewed the attributes highlighted and celebrated in the press conference, the magnitude, the novelties and the mission of the partnership, to be causes of concern. Many expressed surprise at hearing the news so late in the process and charged the university administration with lack of transparency, discussion and due process. Faculty critics raised questions about personnel decisions, renting of campus space for proprietary research, academic freedom, conflicts of interest and intellectual property; and sought to increase the role of the Academic Senate in the negotiation and the implementation of the research agreement. Some highlighted how availability of funding and administrative bias led to the growth of entrepreneurial disciplines at the expense of others, and the shrinking of alternative conservationist research programs. Students and faculty members joined representatives from environmentalist and public-interest organizations to criticize the proposed research focus. Drawing on the broader debate on biofuels, they opposed the expansion of a biofuels industry involving genetic modification, industrial agriculture and global supply chains with potentially unmanageable socio-economic and environmental impacts. All throughout, critics insisted that UC Berkeley is a public university with a responsibility to serve the public interest.

The public controversy lasted until the fall of 2007 and received considerable coverage in local and national media. The signing of the agreement was somewhat delayed and took place on November 14th, 2007.[6] By then, the EBI had already issued its first call for proposals and had begun to settle in its temporary location in Calvin Hall on the Berkeley campus.

Science, Universities and Democracy

The coming together of BP and UC Berkeley to found the Energy Biosciences Institute lies at the intersection of key processes shaping knowledge production in recent decades: increasing university-industry research relations, transformation of public research universities, and growing struggles over controversial technoscience. The EBI episode, while rooted in part in the "spirit" of protest that resides in Berkeley, brought to the fore a set of pressing real-world problems. How should universities govern their relations with private industry so as to address concerns about skewing of research agendas, distortion of research findings, conflicts of interest, secrecy and loss of open access? How is growing entrepreneurialism impacting university governance, growth of fields and departments, academic freedom and universities' ability to serve the public? How can science and technology be employed in addressing major socio-economic and environmental problems in the interest of the many? What are the conditions of production and dissemination of reliable knowledge on emerging technologies such as biofuels? Participants in the EBI controversy drew on various discourses, some very dated and some newly emerging, to address these real-world problems.

This study joins the ongoing debates on these questions by focusing and working on three problems in the sociology of knowledge, broadly defined: (i) How to characterize the conditions of production of knowledge, science and technology? (ii) How to analyze the ongoing transformation of universities? (iii) How to grasp the perils and promises of science and technology for contemporary democracies? In *State Nobility*, Bourdieu ([1989] 1996, p. 5) notes how "the sociology of education is a chapter, and not a minor one at that, in the sociology of knowledge and the sociology of power, not to mention the sociology of philosophies of power." In a similar vein, I work towards a reconstructed sociology of knowledge that contributes the sociology of the academia and the political sociology of science. The remainder of this introduction will (i) elaborate these lines of inquiry in a little more detail, (ii) provide a brief

[6] http://www.berkeley.edu/news/media/releases/2007/11/14_ebisigning.shtml

overview of the EBI controversy as background to the analyses that follow, and (iii) end with a summary of chapters.

Science/Truth

From the Enlightenment to the present day, many have argued that the production of reliable knowledge (truth) requires autonomy. Others have opposed this view pointing out the ways in which science is embedded in society or intertwined with different forms of power. These views have been posed in historically-specific terms and have always come with normative-political connotations on real-world arrangements. Take for example, Timothy Clark's (Professor, Art History) comments at the Faculty Forum organized by the Berkeley Division of the Academic Senate to discuss the UCB-BP partnership:

> I, like many other members of the Senate, have deep misgivings about the deal being struck between British Petroleum as I like to call it and the university. … It is really no answer to those of us with worries about the terms of cooperation in this case to tell us that "it happens all the time." Of course I know as a historian that the university as a form of life came into being and continues in being only through a constant negotiation of its interests against those of the dominant social and ideological powers in the world around it. … The conflict of interest isn't obscure. The boardroom wants profits and products; the lab wants, or ought to want, the truth, however inconvenient that truth may prove to its real world sponsors. The tension between one imperative and the other ought to be explicit in any deal the university strikes. (Timothy Clark, Forum transcript)

This idea that science is the pursuit of truth and requires autonomy has had a strong influence on the development of the sociology of science as a field. Early sociologists of knowledge proposed to trace knowledge to social groups, but refrained from applying this principle to "science" (Marx 1978; Durkheim [1912] 1995; Mannheim [1936] 1955). In the 1930s, Robert K. Merton took a big step, pointing out that scientists were also socially-situated individuals. He then responded to the problem of how science could produce truths by positing that scientists were governed by a set of scientific norms. Soon, Thomas Kuhn and the new sociology of scientific knowledge, took the next step, which was to discard the epistemological discourse about how scientific findings were validated, and to argue that there was no problem to be solved because scientific knowledge was only locally true. Since then, most students of science have limited themselves to theorizing the achievement of credibility.

Against this background, Bourdieu and Latour stand out as two theorists who have attempted to build a sociology of *truth* in the post-Kuhnian era. Each has stipulated specific conditions for the production of scientific truths. Bourdieu proposes to approach science as a field and, like Merton, posits that the production of scientific truths requires relative autonomy. Latour argues that scientific truths, like technoscientific artifacts, are assembled by and require the extension of associations of all kinds. Comparing Bourdieu's and Latour's theories of truth with a focus on their ontological and epistemological assumption reveals the deeper problem to be addressed. Bourdieu's attempt to explicate how science as a historical activity can produce trans-historical truths is still predicated on epistemological/rationalist premises. Latour's claim to have overcome relativism by rejecting the very terms with which it is defined, amounts to a neutral, almost approving stance vis-à-vis all kinds of associations in the production of

technoscience. In the end, these attempts are unable to take us beyond the terrain defined by the options of taking an epistemological position and adopting relativism.

To move beyond this impasse, I propose to resuscitate but radically reconstruct Robert K. Merton's proposal to theorize science as an institution in relation to the societal context, but dispense with the idea of "relative autonomy" that informs both Merton's and Bourdieu's theories. Taking the EBI as a signpost for key processes shaping the production of science and technology today, I argue that the production of reliable knowledge is strongly interdependent with economic capital and technology, and that this interdependence renders the stipulation of autonomy obsolete. Combining this argument of institutional interdependence with insights from science and technology studies regarding (i) the variety of products of science, (ii) the dissolution of the boundary between science and technology; and (iii) how technoscience is inherently political, I argue that it is the nature of associations, interdependencies, engagements that shape science's perils and promise for different constituencies. In this framework, the basis for critique is no longer the loss of autonomy, but the lack of democracy. This proposed shift in the basis of critique helps advance the definition and defense of the public interest in debates on universities and on technoscience.

Universities/Academic Freedom

Like "science," the modern university has also long been associated with the idea of the "autonomous pursuit of truth." Founders of the University of Berlin (1810) were strongly influenced by Kant's writings on reason as "autonomous judgment," and defense of the freedom of the philosophical faculty as central to the mission of the university. The birth of the American university and the initial defense of academic freedom in the United States were similarly connected in the work of American scholars influenced by the German model. Responding to the conflict between then newly rising social and political scientists interested in social reform, and the representatives of big business in trustee boards and foundations, the newly founded American Association of University Professors (AAUP) argued that the universities should be governed in the public interest, and hence, that scholars should be allowed to convey the results of their scholarship "without fear or favor":

> The lay public is … highly needful, in the interest of society at large, that what purport to be the conclusions of men trained for, and dedicated to, the quest for truth, shall in fact be the conclusions of such men, and not echoes of the opinions of the lay public, or of the individuals who endow or manage universities. (AAUP 1915, p. 294; emphases mine)

Recent discussions also emphasize the importance of academic freedom to the mission of the university. In a forum convened in 2003 by the Systemwide Academic Senate of University of California, Robert Post (2003, p. 3) observed that "[d]ebate about the scope and limits of academic freedom is debate about the purposes and functions of the university." In response to intensifying debates on the impact of university-industry research relationships on universities, the AAUP has proposed a set of recommended principles. One of these principles states that "the university must preserve its academic autonomy—including the academic freedom rights of faculty, students, postdoctoral fellows, and academic professionals— in all its relationships with industry and other funding sources" (AAUP 2014, p. 4).

In the meantime, most scholarship on the ongoing transformation of universities is divided between (i) similarly normative commentary and (ii) a seemingly neutral sociology of

organizations and professions. To move beyond these alternatives, I seek to situate definitions and defenses of academic freedom and university autonomy within a fresh account of the transformation of the American research university. Bourdieu's work on the university, like his sociology of science, is a good starting point. In *Homo Academicus*, Bourdieu ([1984] 1988) approaches Kant's model of the university as an empirical question, and argues that the French academic field in the 1960s was structured in ways that support Kant's distinction between the higher and the lower faculties, with arts and sciences oriented to a type of scientific capital that derives from the pursuit of knowledge for knowledge's sake. Employing Bourdieu's approach to analyze American research universities in the late twentieth-early twenty-first century requires taking into account (i) centrality of research as scientific capital; (ii) multiplicity of sources of funding for research; (ii) changing epistemic goals in different disciplines. Such an analysis helps explain faculty members' stances on university-industry partnerships, as well as the content of their arguments on academic freedom and university autonomy.

Democracy/Public Interest

Scientific knowledge is increasingly constitutive of life and death on the planet. It leads to an array of technologies with variably distributed benefits but without certainty about potential risks. It is indispensable in addressing complex socio-economic and environmental problems, but is increasingly produced and mobilized in connection with politico-economic interests. These observations hold in many domains from health to finance, and are extremely well-illustrated at the nexus of global warming, energy and ecology. Science and technology are absolutely crucial for addressing global warming. The development and implementation of alternative technologies is dependent on political and economic support, and needs to proceed amidst the impossibility of scientific consensus on risks and benefits. One member of the *StopBP-Berkeley* campaign explained his position on this state of affairs as follows:

> There are also key issues about democracy. This is not a generally accepted account of how science works or should work, but it's true that science is a form of politics. And politics needs to be democratic or else it's going to lead to concentration of power, terrible inequities and violence and everything else ... especially when you are looking at these kinds of intersections of science and urgent global political issues with massive uncertainties. This is extremely political and it's gotta be dealt with politically. Science is an attempt to escape from the political sphere and it cannot be allowed to work that way (Interview #4).

This argument that science is a form of politics raises the important question of how to understand the place of science in a democratic order.

One possible starting point for a discussion on the relations between science and democracy is the work of a number of Anglophone intellectuals of the 1930s and 1940s, who held that science and democracy belonged together because they possessed similar attitudes and techniques (Hollinger 1996). Most of these intellectuals attributed these attitudes and techniques either to individual scientists and democrats or to science and democracy in the abstract. Robert K. Merton's writings constituted a distinctive contribution in that Merton proposed to model science as an institution governed by the so-called scientific ethos.

Much water has flowed under the bridge, and there now exist a burgeoning literature on the tensions between science, expertise and democracy. Yet, much of this discussion proceeds

without an adequate account of the institutional conditions of the production of science and technology. While Merton's thinking on the subject relied on his view of science as an autonomous institution, Merton's critics have gone too far in the other direction, throwing the baby of institutional analysis with the bathwater of autonomy. Hence, work in science and technology studies highlights how science and technology and the political order are mutually constitutive, and that the democratic management of technoscience is central to the democratic governance of societies. Scholars focus on how social movements engage with science and the programmatic efforts to increase public participation. Attempts to reintroduce the institutional perspective to examine the relations between science, politics and economy tend to resort back to the idea of "relative autonomy." I contend that the interdependence between economic resources and scientific advance translates into the dominance of the possessors of economic capital over the definition of good science, good technology and green energy. Arguably, the central tension is less between autonomous experts and the public, but between those who possess economic and political capital and those who lack.

Overview of the EBI Controversy[7]

The process leading to the founding of the EBI had been initiated in June 2006 by BP's invitation to five research universities to submit proposals for housing a research institute focusing on biofuels.[8] Responding to the call, Beth Burnside, then Vice Chancellor for Research at UC Berkeley, had led the effort to put together UC Berkeley's proposal to house the institute in collaboration with Lawrence Berkeley National Laboratory (LBNL) and the University of Illinois at Urbana Champaign (UIUC). The proposals of the two UC campuses that were competing for the grant, UC Berkeley and UC San Diego, both included a letter from Governor Schwarzenegger to BP's Group Chief executive Lord Browne, pledging "$40 million to support construction of appropriate facilities for the EBI, if one of the UC campuses [were] selected" (EBI proposal, pp. 90-91)[9].

After the press conference, faculty and students expressed surprise at hearing about the partnership so late in the process, and demanded access to the proposal UC Berkeley had submitted to the BP. The response of the university administration was mixed: the proposal was first opened to review in the Academic Senate Office and only subsequently made public. In the meantime, it transpired that UC Berkeley had committed to allocating seven full-time faculty positions (FTEs) in areas in line with the new institute's agenda (EBI Proposal, p. 87) without full consultation with the Budget Committee of the Berkeley Division of the Academic Senate. UCB Chancellor's mention of Chris Somerville, who at the time had an appointment at Stanford University, as the director of the institute, also added fuel to concerns regarding personnel decisions. The proposal discussed renting campus space for proprietary research to be conducted by BP scientists (EBI proposal, p. 11) was also a major bone of contention.

Concerned faculty and students highlighted these issues to charge the administration with lack of transparency, discussion and due process in the making of the partnership. In raising this charge, they drew on the experience of another major partnership that UC Berkeley had entered into amidst similar controversy. In November 1998, UC Regents had signed a five-year research agreement with the Novartis Agricultural Discovery Institute (NADI), a research organization

[7] See Appendix A for a chronology of the controversy.
[8] These included, in addition to UC Berkeley, Cambridge, MIT, Imperial College London, and UC San Diego.
[9] EBI proposal available at http://www.berkeley.edu/news/media/releases/2007/02/EBI_Proposal.pdf

part of Novartis Inc. [10] In that instance, UC's Department of Plant and Microbial Biology (PMB) had put out call for proposals from industry partners. According to the agreement, NADI provided a total of $25 million to UC Berkeley's Department of Plant and Microbial Biology (PMB), with a departmental committee including two Novartis representatives allocating funds to research projects. In response to concerns and criticisms, the UC administration had sponsored two reviews of this partnership: one internal (Price and Goldman 2002) and one external (Busch et al. 2004).[11]

The external review was conducted by a team of researchers from Michigan State University and concluded with a set of recommendations regarding how university-industry research agreements. Most notably, the Michigan State report recommended that UC Berkeley should "avoid industry agreements that involve complete academic units or large groups of researchers" and "encourage broad debate early in the process of developing new research agendas." Hence, upon hearing about the news of the UCB-BP partnership, critics asked why the recommendations of the Michigan State report were not heeded.

In response to the initial flurry of inquiries, the Berkeley Division of the Academic Senate organized a Faculty Forum (March 8th, 2007) featuring major architects of the partnership as well as two skeptics among the panelists.[12] Vice Chancellor for Research, Beth Burnside, reviewed the proposal submitted to BP, and explained how they had to work at "warp speed" to enter the competition. Jay Keasling (Professor of Bioengineering and Chemical Engineering), like the scientists at the press conference, argued biofuels was a promising option to pursue, and that the EBI would fund programs to investigate environmental and socio-economic impacts right from the beginning. David Vogel (Professor of Business and Public Policy) and Shankar Sastry (Professor of Electrical Engineering and Bioengineering) talked about corporate responsibility at BP and the tradition of corporate partnerships at Berkeley, respectively. Two of the panelists took less laudatory positions. Robert Reich (Professor of Public Policy) discussed academic freedom issues. Ignacio Chapela (Professor of Environmental Science, Policy and Management) waged a strong criticism of the partnership, describing it as a "Faustian deal" and as "prostitution". These presentations were followed by a question-and-answer period full of incisive exchanges.

Faculty members who were not satisfied with this forum took further actions. The online "Faculty Shared Governance Petition" (March 12th, 2007), addressed to the chair of the Berkeley Division of the Academic Senate, was signed by 112 faculty members (mainly from the humanities and the social sciences) and called for convening "a blue ribbon Ad Hoc committee to review those aspects of the partnership that impinge on the Senate's mandate."[13] Another smaller group of 17 faculty members (mainly from the humanities, social sciences and environmental research and policy fields) organized to call a special meeting of the Academic Senate, invoking the principle of shared governance and proposing that the contract be subject to senate review before its signing.[14]

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[10] UCB-Novartis agreement, like the EBI agreement, belonged to a newly expanding category of research agreements, increasingly referred to as "strategic corporate alliances " (SCAs), that is agreements made by colleges, departments and colleges as a whole, as opposed to those made by individual researchers (AAUP 2014).

[11] http://www.berkeley.edu/news/media/releases/2004/07/external_novartis_review.pdf
This report was subsequently revised and published as a book (Rudy et al. 2007).

[12] http://webcast.berkeley.edu/event_details.php?webcastid=19207

[13] http://facultysharedgovernance.org/index.php

[14] http://academic-senate.berkeley.edu/sites/default/files/committees/forum/meetings/div_specialmtg_0407.pdf

While these moves targeted the university administration, they led to a mobilization among faculty from sciences and engineering in favor of the partnership. Viewing the critics' call for a special meeting of the Academic Senate as an attempt to block the partnership, advocates put up their own petition and drafted a substitute resolution.[15] This resolution argued that existing policies were sufficient to protect the standards and integrity of research, and that the critics' motion constituted an attack on academic freedom. The two groups of faculty came to a clash at the special meeting of the Academic Senate held on April 19th, 2007. In the meeting hall, the advocates of the partnership had the majority and their substitute resolution was ratified. The latter reaffirmed the principle of academic freedom of individual faculty to receive funding from any source subject to university regulations; and proposed a senate committee to take part in the EBI negotiations. A five-member advisory committee comprising chairs of relevant senate committees participated in the EBI negotiations. Yet, they did so under pledge of confidentiality until the contract was finalized and made public.

The student campaign against the UCB-BP partnership continued beyond the faculty uproar. On April 26th and May 2, StopBP-Berkeley organized two teach-ins that brought together faculty, studens, journalists and NGO representatives to discuss the implications of the agreement for the public university, and the promises and risks of biofuels research. When the fall semester began, *StopBP-Berkeley* organized one other protest on October 4, 2007, featuring a Trojan horse and the heading: "Beware of Oil Companies Bearing $500 million Gifts."

After the signing of the agreement in November 2007, the opposition subsided. Soon thereafter, two reports ensued. Pursuant of the resolution accepted at the special meeting, the Divisional Council (DIVCO) at Berkeley convened a task force to formulate principles to govern future university-industry partnerships. This task force, comprising five Senate members who had been involved in the EBI contract negotiations and three other members with relevant expertise, produced a report titled "Report by Senate Task Force on University-Industry Partnerships" (RSTF)[16] and presented it to the DIVCO in October 2008. DIVCO endorsed it and provided it to the Vice Chancellor for Research, Graham Fleming. The second report, *Big Oil Goes to College* (2010) was sponsored by Center for American Progress and prepared by journalist Jennifer Washburn. This report analyzed ten research collaboration contracts between leading energy companies and major American universities, and concluded that these contracts fell short of protecting universities' commitment to "independent academic research, high-quality science and academic freedom" (Washburn 2010, p. 3).

In December 2014, BP unexpectedly announced that it would use its contract option to pull back one-third of its funding for 2014-2015, and more of it in the remaining two years of the partnership. Since then, the EBI has had to cut back its research on converting plants to fuel, eliminating research and staff positions, and to look for replacement funding.[17]

Summary of Chapters

The dissertation comprises four chapters that begin to carry out the analyses outlined in this introduction. Chapter two, "Two Sociologies of Science in Search of Truth," compares Bourdieu's and Latour's theories of truth. Bourdieu argues that the scientific world is a field

[15] This petition was mentioned at the special meeting of the Faculty Senate, but I have not been able to locate the signee list.

[16] http://academic-senate.berkeley.edu/sites/default/files/recommendations-reports/tf_uip_final_report.pdf

[17] http://alumni.berkeley.edu/california-magazine/just-in/2015-02-20/not-so-fast-uc-berkeley-biofuel-research-takes-hit-bp-oil

(with specificities) requiring *relative autonomy* to produce trans-historical truths. Latour argues that actor-networks produce and uphold scientific truths, and insists on the need for *associations*. I show that Bourdieu and Latour have *opposite* stipulations because their theories are informed by oppositional ontological and epistemological assumptions. They work with very different definitions of "sociology" and take starkly different positions on rationalism: while Bourdieu argues that Kantian rationalism can be grounded in the sociological account of the scientific field, Latour rejects epistemology in general and rationalism in particular as part of the modernist settlement.

Chapter three, "Towards a New Political Sociology of Science" joins recent work in the new political sociology of science calling for renewed attention to the institutional relations between science, politics, and the economy. Yet, I argue for taking a further step and unsettle the idea of the "relative autonomy of science." Drawing on my case study of the UCB-BP partnership, I argue that biosciences advance in heteronomous fields. That is, in these fields, the pursuit of scientific and economic capital is deeply interdependent, and the distinction between science and technology is strongly blurred. In making this argument, I resuscitate and fulfill Robert K. Merton's agenda to theorize science as an *institution* embedded within society, but criticize his unexamined attachment to the idea of autonomy, and his critics' lack of attention to the institutional workings of science. While Merton suggested that science develops best in a liberal order, and whereas current scholarship focuses on public participation, I argue that the interdependence of scientific and economic capital undermines democratic governance.

Chapter four, "Academic Freedom in the Entrepreneurial University," analyzes the polarizing conflict that occurred among the UC Berkeley faculty following the announcement of the UCB-BP partnership. First, I examine why the two sides took clashing positions. My response highlights that the university is a divided entity, and that the necessary conditions of production of knowledge shape the epistemological goals of the faculty in different fields differently. In the entrepreneurial science and engineering disciplines, research requires substantial amounts of funding, and relations with industry create increased opportunities for gaining scientific authority and administrative influence. More strikingly, the blurring of the boundary between basic and applied research has led to convergence in the epistemological goals pursued in academia and industry. Hence, faculty in entrepreneurial sciences and engineering posit that industry funding increases their academic freedom. In the humanities, social sciences and the conservationist disciplines, researchers pursue knowledge that is not readily funded by private industry, and are concerned that the relative scarcity of extramural funding constrains the livelihood of alternative research paradigms and the influence that these disciplines have in university governance. Hence, they come together behind the cause of defending university independence through administrative and faculty senate mechanisms. Second, I argue that both the discursive and organizational outcomes of the conflict (weak definition of academic freedom, weak oversight over university-industry partnerships) were shaped by and in favor of the science and engineering poles, increasing their dominance through their relations with the industry.

Chapter five, "Universities and the Public Good in the Age of Climate Change" approaches the contentions over the meaning and the terms of the UCB-BP partnership as part of a broader struggle over the terms "public," "public good," and "good science." Prior studies have described the consolidation of the emphasis on universities' role in innovation and economic growth as a contribution to the public good. This chapter sets out by observing that the EBI had a number of novelties that disrupted the existing settlement. In addition to being the largest-to-date research agreement between a private corporation and a public university, (i) it was founded with

an explicit, applied mission (biofuels); (ii) sought to bring industry and university scientists under the same roof; and (ii) promised to address the socio-economic and environmental implications at the same time as it would be developing the technology. Throughout the EBI controversy, advocates presented these novelties as necessary for addressing major problems such as global warming. When faced with criticism, they employed other strategies (invoking "peer review," scientization) that other scholars have identified in similar or related technoscientific controversies. Material, organizational and discursive factors empowered these advocates in defining a particular response to global warming and a particular vision of the university as in line with the public interest, While the definition of the latter cannot be settled once and for all, it is clear that universities constitute a key a terrain, albeit an uneven one, for ongoing contentions.

CHAPTER TWO

TWO SOCIOLOGIES OF SCIENCE IN SEARCH OF TRUTH: BOURDIEU VERSUS LATOUR

The sociology of science seeks to theorize the social conditioning of science. This theorizing seems to undermine the validity of scientific knowledge and lead to relativism. Since the 1930s, scholars have taken various routes to reconcile the influence of social factors on scientific knowledge with the long-standing idea that science is the objective pursuit of truth. In the 1950s, one common route was to distinguish between "external" and "internal" factors: external/social factors did influence science but somehow did not pollute the product (Shapin 1992). The sociology of science pioneered by Robert K. Merton theorized science as a bounded institution where a set of norms governed scientists' behavior and ensured that they would uncover nature's laws (Zuckerman 1988). In these studies, the implicit consensus was to refrain from examining the *validation of scientific knowledge*. In the 1960s, the preceding consensus began to be challenged. Thomas S. Kuhn's ([1962] 1970) account of scientific paradigms punctured the boundary between social factors and scientific content, and undermined the idea of continuous scientific progress. In the 1970s, with the emergence of the new sociology of scientific knowledge (SSK), the specter of relativism appeared in full. The new SSKers rejected both the philosophical foundations for scientific truths and the Mertonians' focus on scientific norms at the expense of scientific content. They called for building a *thoroughly* sociological account of scientific facts (Barnes 1974, 1977; Bloor [1976] 1991; Collins 1985; Lynch 1993; Mulkay 1979; Pickering 1992). This move led to much debate. The new SSKers were criticized for attacking scientific rationality and objectivity (Brown 2001; Bunge 1991, 1992; Gross and Levitt [1994] 1998; Ross 1996). In response, they insisted on the social and local character of scientific knowledge and developed distinct responses to the question of relativism (Barnes and Bloor 1982; Collins 1981; Knorr-Cetina 1982; Shapin 1994).

This paper compares two prominent attempts to build a sociology of science that overcomes relativism. Bourdieu and Latour began to publish in the sociology of science around the same time (Bourdieu 1975, 1976; Latour and Paolo 1977). In their early writings, both engage with epistemological debates on the correspondence between knowledge and reality. In subsequent writings, both address relations between science and society. Importantly for the purposes of this paper, both argue that theorizing the social conditioning of science need not lead to relativism. Yet they develop very different sociologies of science. Bourdieu argues that the scientific world is a field with specificities, which under certain conditions, allow it to produce trans-historical truths. In contrast, Latour argues that scientific truths are produced and upheld by actor-networks. More strikingly, the two theorists stipulate opposite conditions for the production of scientific truths: while Bourdieu emphasizes the need for the relative autonomy of the scientific field, Latour emphasizes the need for associations.

My analysis reveals that the two theories are informed by oppositional ontological and epistemological assumptions. To begin with, Bourdieu and Latour work with very different definitions of "sociology." Bourdieu ([1989] 1996) holds that sociology, like all sciences, should define its object and calls for a sociology that examines the reproduction and transformation of the underlying structures of social worlds. In contrast, Latour (2005) defends a non-objectivist sociology that refrains from defining a domain called "the social" and that describes the

associations through which various outcomes are assembled. Perhaps more importantly, the two theorists work with different epistemological assumptions. From his early work on developing an epistemology for sociology to his final lecture at Collège de France, Bourdieu embraces rationalism. In contrast, Latour's trajectory is oriented toward problematizing epistemology in general and rationalism in particular. Notably, the two theorists concur in rejecting the idea that the truth of a statement depends on its correspondence to reality. Yet, their contrasting positions on rationalism lead them to oppositional positions on the specificities of science.

After reconstructing and comparing the two sociologies of truth, I turn to evaluating what each theorist has attempted and achieved, and make a set of related arguments. First, what defines each theorist's attempt to overcome relativism is basically his position on the possibility of epistemology. Bourdieu proposes to overcome relativism by rejecting the division between epistemology and sociology. He argues that Kantian rationalism can be grounded in the sociological account of the scientific field. In contrast, Latour proposes to overcome relativism by rejecting the epistemological discourse through which it is defined. Second, in both theories, the outcome variable, "truth", lacks an independent definition. Rather, it is derived from the theorist's epistemological position and defined with reference to how it is produced. I argue that this interdependence is more problematic in the case of Bourdieu than Latour. Bourdieu talks about "trans-historical truths," but his assertion that truths produced in relatively autonomous scientific fields are "trans-historical" is an epistemological one. In other words, Bourdieu's sociology of truth overcomes relativism only through an epistemological assumption. In contrast, Latour defines truth as an assembly produced and upheld by actor-networks. He rejects the distinction between credibility and validity: truths assembled through associations are all there is. Because Latour does not seek to define truth as anything more than what is assembled by associations, his theory of truth is self-sufficient. Third, while Latour's notion of truth is less problematic, his ontology is more so in that it does not provide the tools to capture the various resources, power differentials and boundaries that inform the production of scientific truths. Finally, I argue that both theories verge on providing an answer to the question of how science should be organized, interestingly for different reasons: Bourdieu's because of its epistemological assumptions and Latour's because of its ontological assumptions.

Theories of Science in Society

Prior to the 1930s, philosophers of science generally proceeded as if science could be interpreted without attending its socio-historical context (Shapin 1992, p. 336). Beginning in the 1930s, historians and sociologists began to situate science in society, but their theories of how socio-historical factors influenced science and scientific knowledge were limited. For early historians of scientific ideas, science unfolded according to its own logic (Sarton [1936] 1957; Clark 1937). Marxist historians highlighted the influence of material factors on the origins of modern science but not the content of scientific knowledge (Bernal 1939; Farrington 1961; Goldsmith and Mackay 1964; Hessen [1931] 1971; Zilsel 2000). In the 1950s, scholars increasingly distinguished "external" factors influencing the development of science and scientific knowledge from "internal" ones (Bunge 1991; Kuhn [1968] 1977; Shapin 1992). In one formulation, external factors referred to "a variety of social factors" and internal factors to "changes within science and rational thought generally" (Barber [1952] 1962, pp. 80-81). This distinction helped reconcile the influence of social factors on science with the idea that science developed rationally (Hall 1952; Lilley 1949; Merton 1938; Shryock 1936).

Robert Merton offered a neat and influential solution to the dilemma of doing sociology of science and preserving science's claims to objectivity and truth (that is, the problem of socio-historical factors polluting science and scientific knowledge). Merton (1942, p. 117) posited that (i) science is an institution with a specific goal, namely, certified knowledge; and that (ii) it is governed by a set of institutional imperatives, commonly known as "the scientific ethos." These institutional imperatives inform scientists' behavior and enable them to uncover nature's laws. To achieve its goal, science needs institutional autonomy; the more autonomous it is, the stronger the scientific ethos and the more successful science can be in its pursuit of truth. This model of science as a bounded institution solved two problems at once: while non-scientific imperatives were kept at bay, the scientific ethos moved scientists towards truth.

In his early work, Merton envisioned a broader agenda for the sociology of science and scientific knowledge. He had a strong interest in analyzing relations between science and its societal context.[18] He noted that the sociology of knowledge required analysis of the social conditioning of "not only error or illusion or unauthenticated belief, but also the discovery of truth" (Merton [1945] 1973, p. 11). Yet, the sociology of science that developed under his leadership focused on the internal workings of the scientific community, examining the social processes that upheld the scientific ethos (Ben-David 1971, 1981; Ben-David and Sullivan 1975; Cole and Cole 1973; Collins 1983; Merton 1938; Storer 1966; Zuckerman 1977).

With Thomas S. Kuhn's *The Structure of Scientific Revolutions* ([1962] 1970), the Mertonian solution began to be undermined. Kuhn refuted the assumption that social norms governing scientists and the product of their work were things of different nature: the scientific paradigm (content of knowledge) ordered the community. In Kuhn's model, the external/internal boundary also became porous such that "external" factors could cause a crisis in scientific paradigms. Perhaps most importantly, Kuhn's thesis of incommensurability attacked the idea of continuous progress in science.

In the 1970s, the new sociology of scientific knowledge (SSK) developed a full-fledged critique of the Mertonian paradigm for the sociology of science. The new SSKers argued for going beyond the Mertonian focus on professional norms and mechanisms to examine how social factors influence scientific *knowledge*. In this task, they set out by rejecting epistemological foundations for the scientific truths (Ben-David 1981; Shapin 1995). They argued that reality could lead to multiple representations and therefore could not resolve scientific controversies or determine the truth of scientific representations (Knorr-Cetina and Mulkay 1983, p. 3). They also rejected the idea that "scientific judgment is sufficiently determined by unambiguous criteria of method" (Shapin 1995, p. 303). Against the realist and the rationalist foundations, the new SSKers called for a *thoroughly* sociological analysis of scientific knowledge (Barnes 1974; Bloor [1976] 1991; Lynch 1993; Mulkay 1979). The Strong Programme insisted that true and false claims need to be explained symmetrically; that is, with reference to the same set of causes (Barnes and Bloor 1982; Bloor [1976] 1991; MacKenzie 1981; Shapin 1975). Early work in the new SSK focused on discarding the external/internal divide and showing how factors generally deemed external (e.g., economic and political factors) were involved the making of facts (MacKenzie 1978, 1981; Shapin 1979) Over time, the new SSKers began to challenge "the taken-for-granted division between "social versus cognitive or

[18]His dissertation analyzed the factors that supported the emergence of modern science in seventeenth century England (Merton [1935] 1938/1970). Two of his early articles, frequently cited for introducing the concept of the "scientific ethos," were as much or even more concerned with the societal conditions that support the autonomy and progress of science (Merton 1938, 1942).

natural factors" "(Shapin 1995, p. 289) and to argue that the social was constitutive of the scientific (Knorr-Cetina 1981; Pickering 1984).

These moves by the new SSK led to considerable debate not only among sociologists of science (Ben-David 1981; Collins 1982; Gieryn 1982a, 1982b; Knorr-Cetina 1982; Krohn 1982; Kuhn 1992) but also across disciplines of history, philosophy and the natural sciences (Brown 1984, 1989; Bunge 1991, 1992; Gross and Levitt 1994; Hacking 1999; Nelkin 1996). As the new SSKers emphasized that reality could not resolve scientific controversies, their opponents criticized them for denying the existence of the real world as the criterion of truth. As the new SSKers highlighted the social and contextual aspects of scientific practice, their critics were disturbed by what they saw as the questioning of the scientific norms and logical/rational procedures that underlie scientific rationality and objectivity. These disagreements often coalesced into a debate over relativism. Critics argued that the new SSKers' anti-realism and social constructivism amounted to saying that there were no objective truths. In response, the SSKers developed various positions on the question of relativism (Barnes and Bloor 1982; Collins 1981; Knorr-Cetina 1982; Meja and Stehr 1988; Shapin 1994). One main route was to argue that the sociologist's task was not to judge the truth or falsity of knowledge but to examine the bases of its credibility (Barnes and Bloor 1982; Gieryn 1999; Shapin 1995).

Against this background, Bourdieu and Latour stand out as two theorists who claim to have overcome the terms of these debates, that is, to have successfully theorized how science is both social and able to construct truths. Bourdieu's initial work on science dates back to his attempt to develop an epistemology for sociology (Bourdieu et al. [1968/1971] 1991). Beginning with an article published in 1975 up toand ending with his last lecture at Collège de France, Bourdieu (1975, 1990, 1991, 2000, 2004) developed the argument that discarding idealist or realist epistemologies need not lead to relativism. Instead, approaching the scientific world as a field allows one to theorize how science as a historical activity can nonetheless produce trans-historical truths. Latour began his work on science in 1975 with a study of Professor Guillemin's laboratory at the Salk Institute (San Diego, CA) and subsequently spent much of his career working with scientists and engineers. Over the years, he employed actor-network theory to challenge the idea that truth derives from correspondence to an outside reality or from the purity of science, arguing instead that it derives from associations (Latour 1987, [1984]1988, 1999).[19] In the next two sections, I reconstruct these two theories of truth. We will see that they differ greatly in their ontological and epistemological assumptions. This is the precise reason that their comparison proves to be fruitful.

Bourdieu: The Scientific Field as the Site of Scientific Reason

1. Construction of Scientific Facts: "Applied Rationalism"

In reconstructing Bourdieu's sociology of science, it is necessary to begin with *The Craft of Sociology* ([1968/71] 1991) for two reasons. First, *The Craft* develops and presents the epistemological principles that informed Bourdieu's *sociology*, including his sociology of

[19] As background to the following comparison, it is also important to note that Bourdieu and Latour were contenders on the French sociological scene. Latour published his first article on science in Bourdieu's journal *Actes de la recherche en sciences sociales* (Latour and Paolo 1977). For his later commentary on Bourdieu, see Latour (1993, 1998b, 2004b). For Bourdieu's views on Latour's sociology of science, see *Science of Science and Reflexivity* (2004). For a discussion of the latter, see Mialet (2003). Other comparative analyses of their work include: Schinkel (2007), Albertsen and Diken (2004), Albert and Kleinman (2011).

science. Second, it presents an early formulation of Bourdieu's thinking on *science*, including topics such as "how sciences construct their object and knowledge" or "how the natural and social sciences differ."

In *The Craft*, Bourdieu et al. ([1968/71] 1991) begin with the observation that sociological thought and practice have suffered from "misconceived" views of the natural sciences, and they seek to formulate an epistemology for sociology. Following Gaston Bachelard's view that *only* the central point on the spectrum from idealism to realism, i.e. applied rationalism, captures the nature of scientific practice, they argue that social sciences need to develop their own "applied rationalism." In Bachelard's work, applied rationalism is summed up by the premise "the fact is won, constructed and confirmed" (Bourdieu et al. [1968/71] 1991, p. 11). In this framework, scientific propositions are won in the sense that they are achieved through a *break* with everyday or commonsense appearances; they are *constructed* through what is always a theoretical act; and they are *confirmed* by the criterion of "fidelity to the real" (Bourdieu et al. [1968/1973] 1991, p. 66).

For Bourdieu et al., what gives Bachelard's premise its meaning is the element of rationalism. The three research operations (breaking with commonsense appearances, construction and validation) are not successive phases but epistemological acts found at each step of scientific work (Bourdieu et al. ([1968/1971] 1991, p. 57). Yet rationalism, or "the primacy of reason over experience," means that there is "an *epistemological* hierarchy" in which the break with appearances informs construction and construction informs validation (Bourdieu et al. [1968/71] 1991, p. 11; italics mine). This hierarchy defines the role of theory in scientific work: "[T]heory constitutes the fundamental precondition for breaking with appearances, object construction and experimentation" (Bourdieu et al. [1968/71] 1991, p. 63).

These epistemological preliminaries are clearly linked to some familiar aspects of Bourdieu's sociology. Adhering to the principles that science requires a break with commonsense appearances and constructs its own object, Bourdieu argues that the proper object of sociological knowledge are objective relations.[20] Read as an account of how sciences construct knowledge, *The Craft* reveals Bourdieu's position on the idealism-realism debate: scientific facts are constructed but in a rational way. This raises the question of what guarantees the rationality of the construction. In *The Craft*, the basis of scientific reason or theory is not elaborated; "science" figures as an abstract subject. Beginning with his 1975 article, Bourdieu expands the Bachelardian description of scientific practice to argue that science produces trans-historical truths because the scientific world is a field with specificities (Bourdieu 1975, p. 33; Bourdieu 1991, pp. 20-25). In *Science of Science and Reflexivity*, he states:

> It follows that the Bachelardian vision of scientific work, which I summed up in the formula 'the scientific fact is won, constructed, observed,' has to be expanded and completed. It is tacitly thought that the construction has to be validated by experiment, in a relationship between the experimenter and his object. In fact, the process of knowledge validation as legitimation … concerns the relationship between the subject and the object, but also the relationship between subjects regarding the object. … The fact is won, constructed and observed in and through the dialectical communication among subjects. (Bourdieu 2004, pp. 72-3; emphasis mine)

[20] Bourdieu's sociology combines this objectivist reading with an analysis of agents' subjective experiences, but the objectivist mode has epistemological priority over the subjectivist mode (Bourdieu and Wacquant 1992, p. 11).

The next section reconstructs this argument in some detail.

2. Science is Social: The Scientific Field

Bourdieu argues that the scientific world should be theorized as a field. To say that the scientific world is a field is to say that it is "a system of objective relations … and the locus of a competitive struggle in which the specific issue at stake is the monopoly of scientific authority … in the sense of a particular agent's socially recognized capacity to speak and act legitimately in scientific matters" (Bourdieu 1975, p. 19).[21] The objective relationships between the various agents, i.e., "isolated scientists, teams or laboratories, defined by the volume and structure of the specific capital they possess," determine the structure of the field, i.e., "the state of the forces that are exerted on scientific production, on the scientist's practices" (Bourdieu 2004, 33). Agents enter the field by acquiring competence (scientific capital and a sense of the game) and the propensity to take part in the game (*illusio*, the belief not only in the stakes but also in the game itself). Once in the field, they engage in a struggle to attain a monopoly of scientific authority/competence, or at least the recognition of their competitor-peers (Bourdieu 1975, p. 23).

By approaching the scientific world as a field, Bourdieu seeks to overcome what he sees as two main problems of the Mertonian sociology of science. First, Bourdieu (1975, p. 21) objects to the tendency to differentiate "political" and "intellectual" dimensions of scientific practice. For Bourdieu, all stakes, strategies and practices are inseparably scientific and social:

> Every scientific choice – the area of research, the methods used, the place of publication, the choice of rapid publication or partially verified findings or later publication of fully checked findings – is also a social strategy of investment oriented towards maximization of the specific, inseparably social and scientific profit offered by the field and determined by the relationship between position and dispositions. (Bourdieu 2004, p. 59)

Second, Bourdieu rejects the Mertonian view of science as an integrated and irenic world where the internalization of scientific norms guarantees the conditions within which science unfolds according to its own logic. But this does not mean science is "a world of war against all." Instead, it is a field of struggles where agents compete to attain monopoly of scientific authority or at least, the recognition of their competitor-peers (Bourdieu 1975, p. 23); but do so according to a set of shared rules. It is these shared rules and never-ending struggles that enable science to rectify errors. In place of Merton's (1973, p. 277) concept of "organized skepticism" as one of

[21] In line with his emphasis on breaking with the readily perceived objects of analysis, Bourdieu posits that various organizations found in the scientific world, such as laboratories, learned societies and disciplinary associations, become proper objects of sociological knowledge only to the extent that they yield to an analysis that uncovers the relations that structure actors' strategies. For example, the laboratory, which has been the focus of many studies in the new sociology of science, can be a scientific object to the extent it can be viewed as a field (Bourdieu 2004, p. 36). Likewise, scientific organizations can be studied by taking into account the positions of their members within the relevant scientific field (Bourdieu 2004, p. 46). Even though Bourdieu generally talks about the scientific field as if it were a single, unified entity, his empirical illustrations indicate that he views the scientific world as made up of multiple fields overlapping or embedded within one another, and variously inhabited by not only individual researchers but also labs or disciplines (Bourdieu 2004, p. 65). For example, a scientific discipline is possibly the prime example of a scientific field, and it is also situated within a broader field of disciplines (Bourdieu 2004, pp. 65-66). See Camic (2011) for a similar observation.

the norms governing scientific practice, Bourdieu proposes a mechanism inherent in the definition of the field: the struggle for scientific authority.

3. Relative Autonomy

Bourdieu rejects the possibility of distinguishing between political and scientific dimensions of scientific practice but still retains a boundary at the level of the field. While scientific stakes, strategies and practices are all inseparably scientific and social, the scientific field is, like all fields, "relatively autonomous with respect to the encompassing social field" (Bourdieu 2004, p. 47).

The relative autonomy of the scientific field is not a given but a historical conquest, as illustrated by the creation of scientific disciplines. It is linked with the cost of entry into the field, which comprises scientific competence (scientific capital and a sense of the game) and the propensity to take part in the game (illusio, the belief not only in the stakes but also in the game itself). The higher the cost of entry, the more autonomous the scientific field,; the more "the system of forces that are constitutive of [its] structure is… independent of the forces exerted on the field" and the more the field "has the 'freedom' it needs to develop its own necessity, its own logic, its own *nomos*" (Bourdieu 2004, p. 47). In a relatively autonomous scientific field, agents pursue the forms of scientific capital specific to that field.[22] With this formulation, Bourdieu opposes individualist notions of disinterestedness: individual scientists are of course interested in accumulating capital, but a relatively autonomous field with its own *nomos* orients agents to the scientific capital specific to that field and supports an "interest in disinterestedness" (Bourdieu 2004, p. 52).

4. Production of Trans-Historical Truths

According to Bourdieu, the scientific field is "a field like others, but one which obeys a specific logic," which is why it is "the historical site where trans-historical truths are produced" (Bourdieu 2004, p. 69). What are the specificities of the scientific field? The first is "its more or less total closure," which means that in a sufficiently autonomous scientific field, scientists' audience comprises only other scientists who are perfectly capable of understanding, criticizing and refuting scientific claims (Bourdieu 2004, p. 69). The second is "the fact that the scientific struggle, unlike the artistic struggle, is aimed at the monopoly of the scientifically legitimate representation of the 'real' … (as produced by the theoretical and experimental equipment actually available at the moment in question)" (Bourdieu 2004, p. 69). The third is that competitors in the scientific struggle have "an immense collective stock of equipment for theoretical construction and empirical verification or falsification which all participants in the competition are required to master" (Bourdieu 2004, p. 69).

How do these specificities (closure, specific aim and collective equipment) lead to the production of trans-historical truths? First, the closure of the scientific field gives rise to "specific laws of dialogue and argument" (Bourdieu 2004, p. 70). As they enter the scientific field, agents tacitly agree to the epistemological rules that "govern the confrontation of the

[22] Bourdieu (2004, p. 57) does recognize that sciences always need two types of resources: "strictly scientific ones, which are for the most part incorporated, and the financial resources needed to buy or build equipment (such as the Berkeley cyclotron) or to pay staff, or administrative resources, such as posts." Accordingly, he posits that researchers struggle for two types of scientific capital: "a capital of strictly scientific authority and a capital of power over the scientific world which can be accumulated through channels that are not purely scientific and which is the bureaucratic principle of temporal powers over the scientific field such as those of ministers and ministries, deans and vice-chancellors or scientific administrators" (Bourdieu 2004, p. 57).

scientist with the external world … [and] with other scientists" (Bourdieu 2004, p. 83). Hence, the scientific field is the site of the "conflictual but regulated" competition and cooperation among the scientists best able and most inclined to expose particularities (Bourdieu 2004, p. 84). The peer group that objects to propositions is also the source of the validity of the propositions that survive the objections. Through "the work of deparicularization," marks linked to the particular conditions of emergence are obliterated and facts are universalized (Bourdieu 2004, p. 83). In sum, "objectivity is the intersubjective product of the field," and scientific truths present themselves as transcendent (Bourdieu 2004, p. 83).

The specificities of the scientific field are closely intertwined with its autonomy. As the price of entry increases, the scientific field "becomes restricted to competitors who are increasingly well equipped to criticize scientific statements rationally and to discredit its author" (Bourdieu 1975, p. 33). The antagonism in the field "leaves less and less room for the unthought assumptions of doxa" (Bourdieu 1975, p. 33). Hence, the more autonomous the scientific field, the stronger its specificities, and the more capable it is of producing trans-historical truths.

Latour: The Actor-Network as the Basis of Relative Certainty

1. The Construction of Scientific Facts: "A More Realistic Realism"

Latour's first study in the sociology of science sought to respond to the Mertonians' lack of attention to the content of science. *Laboratory Life* ([1979] 1986), co-authored with Steve Woolgar, drew on Latour's ethnography at the Salk Institute (San Diego, CA) to examine the construction of a scientific fact. Specifically, Latour and Woolgar ([1979] 1986, p. 105) set out to understand "how a fact takes on a quality which appears to place it beyond the scope of some kinds of social and historical explanation." They argued that "'reality' cannot be used to explain why a statement becomes a fact since it is only after it has become a fact that the effect of reality is obtained" (Latour and Woolgar [1979] 1986, p. 180). Instead, they proposed that a statement becomes a fact because it corresponds to the entities produced through scientific practice (Latour and Woolgar [1979] 1986, p. 129). Thus, they concluded that "facts are thoroughly understandable in terms of their social construction" (Latour and Woolgar [1979] 1986, p. 107).

This account of the social construction of scientific facts was criticized as a denial of the existence of outside reality, science's ability to represent reality and the truth of scientific findings (Latour 2005, pp. 88-93). Latour and Woolgar ([1979] 1986, p. 182) foresaw these criticisms and qualified their argument: "[We do not mean to say] 'there is no such thing as reality' or 'facts do not exist'; but rather that 'out-thereness' is the consequence of science work rather than its cause." Yet Latour's full response to these critiques came in his subsequent work.[23] In *Science in Action* (1987), he observed that the phrase "scientific facts are socially constructed" was taken to mean that they cannot be true. Employing the actor-network approach, he called for discarding the boundary between science as a social practice and reality as a non-social entity and to analyze how both facts and reality are constituted simultaneously through scientific work. Accordingly, Latour and Woolgar ([1979] 1986) removed the term "social" from the title of the second edition of *Laboratory Life*. In later works, Latour went on to employ actor-network theory to question the science/society boundary and the notion of pure science (Latour 1987; [1984]1988; 1996). Ultimately, he combined these moves into an actor-network theory of truth (Latour 1999).

[23] See Latour (1993, pp. 25-27; 2000; 2005, pp. 88-106) for retrospective accounts of his trajectory, including his break from other new SSK scholars and move toward questioning the notion of "social explanation."

2. *Actor-Network Theory/Sociology of Associations*[24]

Actor-network theory criticizes sociologists' definition of a domain called "the social," which is populated with entities deemed social (such as classes, institutions, social forces etc.), and the sociological practice of invoking only these social entities to explain social outcomes. According to Latour, sociologists should discard the social/non-social boundary and refrain from explaining outcomes with reference to entities invisible to the actors. Rather, they should follow and describe the *actor-network*, that is, the chain of associations that assemble various kinds of outcomes (Latour 2005, pp. 10-11). An actor-network connects various *actants*; that is, social or nonsocial entities do things when included in the network.[25] Tracing the actor-network means tracing the associations between different kinds of actants and recognizing their contribution to the assembly of an outcome.

In *Science in Action* (1987), Latour uses the actor-network approach to overcome the criticisms to *Laboratory Life*. Specifically, he focuses on how scientists come to decide what a scientific object really is. He notes that it is often taken for granted that a scientific term such as "enzyme" refers to a well-known object, but at the time of its emergence, this object is basically "a list of written answers to trials" (Latour 1987, p. 87). In other words, before scientists agree on what an enzyme is, they observe what this new object does in certain experimental conditions and offer competing ideas of what it might be. "The list of trials [became] a thing" only gradually (Latour 1987, p. 92). On these grounds, Latour argues that "reality is what resists… trials of strength" (Latour 1987, p. 93) With this shift, Latour discards the idea that reality is fixed and out there in such a way that there could be only one possible true representation of it and develops the idea that objects have a *relative existence* that is established in part through scientific work.

Yet this should not be taken to mean that truth is made up of thin air. In viewing scientific experiments as trials where scientists try to *enroll* yet-to-be-defined scientific objects to behave as their hypotheses predict, Latour recognizes that these objects may or may not do so. In other words, nonhumans may or may not validate scientific claims. He refers to this as nonhumans' "agency." In short, Latour discards the social/nonsocial boundary and redefines "social construction" as referring to an assembly that includes nonsocial actants.

3. *Against Boundaries: Translation*

In his second book, *Science in Action*, Latour (1987; [1984] 1988; 1996; 1999) ventures beyond the laboratory and is troubled by what he considers an imagined boundary between science and society. He observes that this boundary is rooted in the idea that society pollutes

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[24] Latour (2005, p. 10) traces the origins of actor-network theory to an article that he wrote with Michel Callon in 1981, where they argued that sociologists should refrain from deciding a priori who the macro actors and the micro actors are and instead focus on following how these actors are assembled (Callon and Latour 1981). Latour's first full study using actor-network theory, *Les Microbes: guerre et paix suivi de Irréductions* (1984) – the French original of *The Pasteurization of France* (1988) – followed this advice and raised the question of whether Pasteur could have caused all the changes in microbiology, medicine and hygiene for which he is often credited. Three years later, Latour published *Science in Action* (1987), where he employed actor-network principles to work out a theory of the production of scientific facts that responded to the criticisms of *Laboratory Life*. In recent years, Latour (2005) has come to argue that actor-network theory constitutes an alternative to traditional sociology and to advocate the name "sociology of associations."

[25] An actor-network is a network not in the sense of a structure but in the sense of a chain that connects various actants. It is an actor in the sense that each actant along the network is an active mediator that makes things happen.

science and once again turns to actor-network concepts to discard it. Latour posits that to understand science, we need to follow all of the associations through which science is achieved. In his view, the question of whether the actants connected in actor-networks pursue purely political or scientific interests is not important. Instead, what matters is how these actants associate.

To analyze these associations, he proposes the concept of translation, which refers to "the work through which actors modify, displace and translate their various and contradictory interests" (Latour 1999, p. 311). In *Pandora's Hope*, Latour offers a succinct illustration of this concept. Focusing on a short episode beginning in 1939, he describes the operation of translation that aligned the interests or goals of Frédéric Joliot, the scientist, and Raoul Dautry, the French Minister of Armaments. This description is worth quoting at length:

> Dautry wants to ensure France's military strength and the self-sufficiency of its energy production. … Joliot wants to be the first in the world to produce controlled artificial nuclear fission in the laboratory; this is his goal. To call the first ambition "purely political" and the second "purely scientific" is completely pointless, because it is the impurity alone that will allow both goals to be attained. … The **operation of translation** consists of combining two hitherto different interests (waging war, slowing down neutrons) to form a single composite goal. Of course, there is no guarantee that one or the other of the parties isn't cheating. … [In any case] neither of the parties … will be able to arrive at exactly his original goal. There is a drift, a slippage, a displacement, which, depending on the case, may be tiny or infinitely large. … What is important in such an operation of translation is not only the fusion of interests that it allows but the creation of a new mixture, the laboratory. … The laboratory's walls, its equipment, its staff, and its resources were brought into existence by both Dautry and Joliot. It was no longer possible to tell, among the complex forces mobilized around the copper sphere filled with uranium and paraffin, what belonged to Joliot and what to Dautry. (Latour 1999, pp. 88-9; emphasis mine)

In contrast to Bourdieu, who retains the idea of a boundary between the scientific field and other fields through the notion relative autonomy, Latour insists on dispensing with such boundaries through the notion of translation. While for Bourdieu, the relative autonomy of the scientific field is the basis of a specific kind of interest, for Latour, what matters are the translations that reshape and combine interests.

4. An Actor-Network Account of Scientific Truths

If Bourdieu's theory of scientific truths is grounded in the specificities of the scientific field, Latour's theory is informed by a specific assertion; namely, that there is no need to distinguish between science and technology. In *Science in Action*, Latour postulates that the making of facts and artifacts should be studied in the same way:

> The problem of the builder of 'facts' is the same as that of the builder of 'objects': how to convince others, how to control their behavior, how to gather sufficient resources in one place, how to have the claim or the object spread out in time and space. In both cases, it is others who have the power to transform the claim or the object into a durable whole. (Latour 1987, p.131)

In *The Pasteurization of France* ([1984] 1988), his insights regarding technological change clearly inform his theories of the production of truth. By the time *Pandora's Hope* (1999) is published, this progression is complete: Latour argues that the truth of a scientific statement depends on chains of associations that extend not only within but also beyond the scientific community. For example, in the Joliot story, research on the potential of a nuclear chain reaction in uranium involves numerous associations, such as obtaining support from the French Ministry of War, making an agreement with a Belgian company to procure uranium, acquiring the necessary heavy water from Norway during the war and working to coordinate the behavior of neutrons. In this account, Latour does not differentiate between mobilizing nonhumans versus humans or between mobilizing scientists versus non-scientific actants. Rather, he argues that all associations are equally crucial for building scientific facts and artifacts.

Further, Latour contends that associations increase science's ability to construct and uphold truths. He states, "The truth of what scientists say [comes] … from the safety provided by the circulating references that cascade through a great number of transformations and translations" (Latour 1999, p. 97). While the traditional view of science had the motto "the more disconnected a science, the better," Latour (1999, p. 97) argues "the more connected a science, the more accurate it may become." Table 1 summarizes the two sociologies of truth as they have been described thus far.

TABLE 1 Bourdieu's and Latour's Sociologies of Truth

	Bourdieu	**Latour**
Early work on the construction of scientific facts	*Craft of Sociology* ([1968/73] 1991) Applied Rationalism	*Laboratory Life* ([1979/1986]) Realistic Realism
What does it mean to say "science is social"?	The scientific field is a social field.	Facts and artefacts are assembled by actor-networks.
Theory of the production of scientific truths	Relative Autonomy //Specificities → Truth	Associations // Translations → Truth

Comparative Discussion

As the preceding sections show, Bourdieu's and Latour's sociologies of truth are quite different. This is because the two theorists work with different ontological and epistemological assumptions. This section will elaborate these differences in greater detail.

Different Theories of Sociological Knowledge

Bourdieu and Latour work with very different theories of sociological knowledge. Bourdieu ([1989]1996, p. 7) holds that the goal of sociology is "to uncover the most profoundly

buried structures of the various social worlds… as well as the 'mechanisms' which tend to ensure their reproduction or their transformation." He criticizes interactionist approaches to the study of science and insists on the need for an objectivist view of the scientific world as a field. He calls for examining the distribution of the different types of scientific capital and the operation of the scientific *habitus* and for examining how these objective and subjective structures shape agent's choices, strategies and trajectories within the scientific field. For Bourdieu, "science is social" in the sense that the scientific world is a social field.

In contrast, Latour (2000) argues against the practice of defining a domain called "the social" and theorizing what goes on there by positing entities that are invisible to the actors (such as classes, structures and forces). He proposes instead to follow and describe the associations through which various outcomes are assembled. This move raises the question of whether actor-network theory is a "sociological" theory. Latour's (2005) response is positive: he does not give up the terrain of sociology and calls actor-network theory *the sociology of associations*. Latour's approach to studying science leads him to discard the boundaries between the social and the natural and the boundaries around the scientific world. For Latour, "science is social" in the sense that scientific facts, their referents and artifacts and all related outcomes are assembled through associations that cut across such imagined boundaries.

Contrasting Positions on Rationalism

Bourdieu and Latour take opposite positions on questions of epistemology. Bourdieu rejects idealist and realist epistemologies and embraces rationalism. Latour rejects all epistemologies. To trace how these epistemological positions inform their theories, it is useful to focus on two hypotheses that theories of truth often engage with. The first concerns the relation between knowledge and reality, positing that the truth of a scientific fact derives from its "correspondence" to an outside/objective reality. The second concerns the relation between scientists and the rest of society. Theorists have postulated that the truth of a scientific fact derives from the "purity" of the scientific institution, community or methods, defining purity in different ways. For example, Merton ignored the question of "correspondence" and offered a theory of "purity" asserting that scientists can unravel nature's laws because science is a bounded institution where the scientific ethos governs scientific work. When the new SSKers objected to the Mertonian solution, they questioned both of his settlements: Can correspondence really resolve scientific debates? Can the scientific ethos really ensure objectivity and truth? Bourdieu's and Latour's sociologies of truth also engage with these two hypotheses, and it is their positions on rationalism that have shaped their responses.

In *The Craft*, Bourdieu counters idealist and realist epistemologies of science, but still takes an epistemological position, that of applied rationalism. According to the latter, propositions are *won* through a *break* with everyday or commonsense appearances, *constructed* through what is always a theoretic act and *confirmed* by the criterion of "fidelity to the real" (Bourdieu et al. ([1968/1971] 1991, p. 66). In *Science of Science and Reflexivity*, Bourdieu revises the final element, arguing that confirmation depends on "the theoretical and experimental equipment actually available at the moment in question" (Bourdieu 2004, p. 69) and is always with "reference to the *constructed* 'real'" (Bourdieu 2004, p. 88; emphasis mine). In this later formulation, both scientific fact and the reality it refers to are constructed. In any case, throughout his writings, Bourdieu is not troubled by the correspondence problem because in his view, what makes a constructed fact true is not any direct correspondence to an outside reality but the specific conditions under which it is constructed and validated (which decide the criteria

and the necessary degree of correspondence). Beginning with "The Specificity of the Scientific Field" (1975), Bourdieu argues that his concept of the field allows one to theorize the specific conditions necessary for the production of scientific truths. Hence, his theory of the scientific field is a version of the purity hypothesis.

In "The Peculiar History" (1999), Bourdieu links his theory of the scientific field with Kantian rationalism. He states that in place of the Kantian project, one may substitute the sociological analysis of the scientific field. In *Science of Science and Reflexivity* (2004), Bourdieu elaborates on this move. He states that his project is to ground Kantian rationalism in a sociological account of the scientific field and build a "historicist rationalism" (Bourdieu 2004). According to Bourdieu (2004, p. 83), Kant views objectivity as "intersubjective validation" arising from "the agreement of the transcendental consciousnesses which… are universally attuned to the same universal." The sociological analysis of the scientific field "introduce[s] into Kantian intersubjectivity the social conditions which underlie it and which give it its specifically scientific efficacy" (Bourdieu 2004, p. 83). In sum, Bourdieu's sociology of truth is clearly anchored in an a priori epistemological position, i.e., Kantian rationalism.[26]

Latour, on the other hand, responds to the correspondence problem by rejecting the ontological divide between knowledge and reality. In *Laboratory Life*, Latour and Woolgar argue that the truth of a scientific statement cannot come from its correspondence to reality because the reality is constituted at the end of the scientific validation process. Latour's response to the criticism that he denies the existence of reality is to reject the boundary between science as a social endeavour and reality as a non-social entity and to redefine "social construction" as an assembly that involves humans and nonhumans. If scientific facts and reality are constituted through the same set of associations, the correspondence problem disappears.

Soon after writing *Laboratory Life*, Latour turns to the purity hypothesis. Unlike Bourdieu, who claims that rationalist epistemology can be grounded in the functioning of the scientific field, Latour rejects all epistemologies and so has no interest in postulating any boundaries between the scientific world and the rest of society. Instead, he extends actor-network ideas (specifically, the postulate that sociologists should refrain from defining entities) to abolish such boundaries. Latour does refer to autonomy. In *Pandora's Hope*, he defines autonomization as the process by which "a discipline, a profession, a clique or an 'invisible college' becomes independent and forms its own criteria of evaluation and relevance" (Latour 1999, p. 102). Yet, for Latour, all that autonomization does for scientists is to delineate a group of colleagues that they need to enroll behind their statements. The techniques for enrolling colleagues may be somewhat different from those employed to enroll nonhumans or funders, but they are not so different as to form the main foundation for scientific truths (Latour 1999, p. 104).

In fact, Latour not only avoids taking an epistemological position but also seeks to problematize the rationalism that Bourdieu attempts to historicize. In the postcript to the second edition of *Laboratory Life,* Latour (1986, p. 274) recounts how his work in Ivory Coast and his frustration with the tendency of the development literature to describe natives' abilities as cognitive rather than social led him problematize so-called "scientific thinking." In *We Have Never Been Modern*, he argues that modernity has instituted an artificial separation between knowledge and politics and that the epistemological arguments for distinguishing between

[26] Turnbull and Antalffy (2009) argue that Bourdieu calls for distinguishing philosophical and sociological views of science, putting aside epistemological arguments and proceed with the sociological analysis of science. This reading neglects how he tries to overcome the epistemology/sociology divide and ground epistemological arguments in the workings of the scientific field.

science and ideology have led to a form of rationality that is no longer adequate (Latour 1993, p. 28, 42, 92). In *Pandora's Hope*, he takes up the issue in more detail and argues that the assumption of Reason has been employed to justify the power of experts over the presumably "irrational" mob (Latour 1999, pp. 10-11). In fact, Latour argues that epistemological ideas about the rationality of Western Science as well as the ontological assumptions about the reality of Nature and Society are all part of the modernist settlement that needs to be left behind. In short, Latour's way of overcoming relativism is to reject all the terms through which relativism is defined (Latour 1993, p. 24)

In sum, Bourdieu and Latour have similar responses to the correspondence question in that both reject any notion of direct correspondence to an outside reality as the basis of truth. Yet they are at odds on the purity question. Bourdieu retains a sense of the threat of pollution – not in the Mertonian manner of imagining the scientific community to be governed by a peculiar ethos, but in the manner of insisting on the relative autonomy of the scientific field as the necessary condition for the production of scientific truths. In contrast, Latour discards any notion of purity or pollution; for Latour, the boundaries that demarcate science from the rest of society are part of the modernist settlement and need to be discarded. For Bourdieu, truth depends on autonomy; for Latour, truth depends on associations.

TABLE 2 Bourdieu's and Latour's Sociologies of Truth – Divergences

	Bourdieu	**Latour**
Different theories of sociological knowledge	Objectivist	Non-objectivist
Different positions on rationalism	Argues for a historicist rationalism	Argues for discarding rationalism/relativism dichotomy
Relation between knowledge and reality (Correspondence)	"There is no objective reality independent of the conditions of its observation" (Bourdieu 2004, p. 74)	Objective reality coalesces only at the closing of a scientific controversy.
Relation between science and society (Purity)	Relative autonomy	Associations

Whither Sociology of Truth?

This paper began with the observation that both Bourdieu and Latour attempt to build a sociology of science that overcomes relativism. This section will evaluate what each has attempted and achieved in this respect.

Bourdieu (1991, p. 23) begins by asserting that it is possible and necessary to dissolve the boundary between epistemology and sociology and seeks to ground Kantian rationalism in the workings of relatively autonomous scientific fields. In other words, he begins from an a priori epistemological position and then supports this position with the argument that relatively autonomous scientific fields produce "trans-historical truths." Let us suppose we figured out how to identify a relatively autonomous scientific field and that the agents in that field came to agree on the truth of a scientific statement. How can we verify that the ensuing product "trans-historical"? Such a verification is not possible because Bourdieu never provides a definition of "trans-historical" independent of his embrace of rationalism. What is a "trans-historical truth?" It is a statement produced under the specific conditions that manifest in relatively autonomous scientific fields. How are "trans-historical truths" produced? They are produced under the conditions that manifest in relatively autonomous scientific fields. In short, Bourdieu's epistemological position, definition of truth and stipulations regarding the necessary conditions for the production of "trans-historical" truths are all interdependent. If Bourdieu's sociology of truth avoids relativism, it is only because it is anchored in rationalism.[27]

Given that Bourdieu's assertion that scientific facts established in relatively autonomous scientific fields are trans-historical is an epistemological (rationalist) assertion, his project may be described as one of social epistemology. Yet social epistemology tries to reconcile the idea that science is social with the idea that it may embody rationality and proposes to conduct empirical studies to answer the question of how science should be organized to fulfill the promise of reason (Fuller [1988] 2002, p. ix). In contrast, Bourdieu's sociology of truth as social epistemology answers this question once and for all: only relatively autonomous fields can produce trans-historical truths. The latter assertion is not verifiable, and it does not invite empirical investigations

Latour's initial position on the epistemology/sociology boundary was similar to that of most new SSKers; that is, he left epistemology aside and examined how scientific statements are rendered credible. But Latour later parted with the new SSKers to reject the label of relativism. New SSKers said they did not seek to judge the truth or falsity of a statement but to explain how a statement becomes credible. In contrast, Latour argued that there can be no realist or rationalist foundations to verify or falsify a statement; credibility assembled through associations is all that exists. In other words, Latour sought to overcome relativism by rejecting the terms by which it is defined. There may be multiple networks and multiple truths. Truths assembled by lengthier actor-networks are truer simply because they are assembled by lengthier networks. As in Bourdieu's theory, Latour's definition of truth and his stipulations for the production of truths are interdependent. What is truth? It is an assembly produced through associations. How do truths get produced? They are produced through associations. Yet, in Latour's case, this interdependence is less problematic. Because Latour does not attempt to define truth as something more than credibility, he is able to dispense with an a priori epistemological position.

The problem with Latour's approach arises not from his epistemology or definition of truth but from his sociology, which rejects ontological boundaries and focuses only on

[27] Kim (2009) argues that his study of the biometry-Mendelism controversy provides empirical substance for Bourdieu's sociology of truth. He identifies the closing of the controversy as the outcome variable to be explained. He argues scientists' conversion to Mendelism must have been "due to the operation of the sense of judgment … which enabled them to distinguish what was 'scientifically reasonable' from what was not" (Kim 2009, p. 75). Yet Kim's argument cannot resolve the circularity of Bourdieu's theory of truth. When a set of scientists reach consensus, what makes the consensus a "transhistorical" truth? Kim's answer follows Bourdieu: the rationality produced by the field.

associations and translations. Because his sociology of associations is built on blurring kinds boundaries and distinctions, it provides few tools to perceive and track the effects of different resources and alliances on different kinds of technoscientific output. If there are boundaries between scientists and non-scientists, Latour's sociology cannot perceive them. If different actants have differential abilities to enroll others, Latour's sociology does not seek to register the differences. Furthermore, his theory posits that boundaries cannot have anything to do with the resulting truth of scientific statements. In the end, Latour's sociology of truth, even as it dispenses with epistemological assumptions, also verges on providing an answer to the question of how science should be organized: more associations equal more truth!

The normative moments of both theories become clear when the theorists comment on recent developments in contemporary science. In the foreword to *Science of Science and Reflexivity*, Bourdieu laments the loss of science's autonomy:

> [T]he world of science is threatened by a serious regression. The autonomy that science had gradually won against the religious, political or even economic powers, and partially at least, against the state bureaucracies which ensured the minimum conditions for its independence, has greatly weakened. … In short, science is in danger, and for that reason it is becoming dangerous. (Bourdieu 2004, p. vii)

In contrast, Latour celebrates increasing associations:

> How different are the connections nowadays between research and society! Consider the group of patients who created a French association for the treatment of muscular dystrophy (AFM)…. [M]any decisions are now made by the patients, their families and their representatives. Patients now routinely generate their own science policy. (Latour 1998a, p. 209)

What is remarkable about these comments is not only the two theorists' contrasting emphases (loss of autonomy versus increasing associations) but also their selective attention as to who partakes in science. While Bourdieu (2004, p.vii) highlights the increasing "control of large industrial companies" Latour foregrounds patients' involvement in the governance of science. Thus, we see how Bourdieu's and Latour's epistemological and ontological positions are linked to their empirical diagnoses. It is precisely because they push two directly oppositional epistemological and ontological points to the extreme that their comparison constitutes useful groundwork for further research on the questions of resources, alliances, boundaries and products in science.

Conclusion

After the rationalism/relativism debates of the 1980s and the science wars of the 1990s, discussions on the objectivity of science and scientific knowledge have subsided. Yet, given the constitutive role of science and technology in society and the increasing controversies around technoscience, the question of how and under what conditions science produces reliable knowledge is difficult to leave behind. In recent decades, growth in university-industry research relations and in the commercialization of science has brought back the question of how science's embeddedness shapes its agendas, practice and products. The comparison presented above constitutes useful groundwork for further research on this question.

A side-by-side comparison of Bourdieu and Latour proves to very interesting and instructive. Both attempt to go beyond the epistemology/sociology boundary and to build a sociology of science that overcomes relativism. Each develops a very different sociology of truth. And most strikingly, these two theorists stipulate opposite conditions for the production of scientific truths: Bourdieu calls for autonomy while Latour calls for associations. My reconstruction of their theories sought to show how their ontological and epistemological assumptions informed their theories of the production of scientific truths. Bourdieu and Latour have different ideas about what a sociological account should achieve. More fundamentally, they have different relations to epistemology in general and rationalism in particular. Bourdieu embraces rationalism, sets out with the notion of "trans-historical" truths and argues that the production of such truths depends on the conditions that manifest in relatively autonomous scientific fields. Latour rejects all epistemologies, defines truth as merely an assembly and argues that the production of truths depends on associations. In both theories, the theorist's epistemological position, definition of truth and stipulations for the production of scientific truths are all interdependent.

In the final analysis, both theories are weakened by their *a priori* assumptions . Bourdieu's sociology of truth is too dependent on his epistemology – the argument that knowledge produced in relatively autonomous scientific fields is trans-historical is still an epistemological assumption. Latour's sociology of truth is too dependent on his ontology, which does not allow us to perceive or track any distinctions, boundaries or power differentials. In addition, these *a priori* assumptions inform the normative moment of each theory. Most succinctly, each theorist's position on the rationality of science was correlated with his stipulation for the production of truths (autonomy versus associations). Finally, I hope to have shown that it was precisely because they embraced oppositional ontological and epistemological assumptions, and because they pushed these assumptions as far as possible that comparing Bourdieu and Latour was worthwhile. Compared side-by-side, these two sociologies of truth produce a useful map for thinking about how two better theorize the embeddedness of science.

The existing literature on the funding, organization, conduct and implications of scientific and technological research is expanding. At the same time, there is a lack of theorization of how the relations within which technoscientific work is undertaken shapes the ensuing products.[28] How should we conceptualize boundaries and associations? Are they material or only discursive? What are the trends? What are the implications of these trends for the resulting technoscientific products? What are the political implications? These questions cannot be answered once and for all, but should fruitfully inform future empirical studies.

[28] See Mirowski and Sent (2008) and Panofsky (2010) for similar observations.

CHAPTER THREE

TOWARDS A NEW POLITICAL SOCIOLOGY OF SCIENCE

In 1938 and 1942, Robert K. Merton published two brief articles that became known as the loci of his defense of science and democracy. "Science and the Social Order" (1938) began by quoting Weber's observation that "the belief in the value of scientific truth is … a product of definite cultures," followed by a slightly revised statement of a key premise that Merton had formulated at the end of his dissertation completed a couple years earlier: "the persistent development of science occurs only in societies of a certain order, subject to a peculiar complex of tacit presuppositions and institutional constraints" (Merton 1938, p. 321). Merton went on to observe that science was coming under threat in many contexts and turned to the situation in Germany. He recounted the Nazi attacks on science, the removal non-Aryans from universities and interpreted these developments as resulting from "an incompatibility between the ethics of science and the new political code" (Merton 1938, p. 326). This account was enmeshed with comments on how a liberal order supports science by giving it a sphere of autonomy.

A few years later, Merton published "A Note on Science and Democracy" (1942), the *locus classicus* of his concept of the "scientific ethos." This article again began by commenting on the shifting fortunes of science, and noting that the study of the scientific ethos is part of the larger problem of understanding the contexts that support or hamper science. Acknowledging the lack of comparative studies, Merton offered "the provisional assumption" that democracy offers the best context for the development of modern science. In discussing the four norms that make up the scientific ethos, Merton argued that democracy supports science because both are governed by the same principles.

As Hollinger (1996, p. 85) has explained in detail, Merton's writings on science and democracy were part of a broader intellectual discourse on the affinity between the two, but also constituted a distinctive contribution in that they defined science as an institution and suggested that "democratic values [institutionalized] in a concrete social constituency." Yet, after 1942, Merton and his collaborators turned the internal workings of the scientific community, and the question of the relation between science and its societal context, or democracy for that matter, was largely pushed aside.[29] This paper begins with the proposition that reconstructing and reassessing Merton's thinking on science and its societal context can contribute to a better understanding of the perils and promises of science in contemporary democracies. The first part reconstructs Merton's main tenets. The second part reevaluates them in relation to current developments and debates.

Merton's Sociology of Science

How Science is Social: Two Premises

Merton's dissertation, completed in 1935 and published in 1938 as *Science, Technology and Society in Seventeenth Century England* ([1935] 1938/2001), examines the emergence of modern science. Merton ([1935] 1938/2001, p. 3) begins by categorizing "science and

[29] Tellingly, the 1942 article was renamed twice in subsequent publications. Mendelsohn (1989, p. 279) interprets Norman Storer's renaming of this article as "The Normative Structure of Science" in *The Sociology of Science: Theoretical and Empirical Investigations* (1973), as an indication that it was "decontextualized, removed from its historical setting and made to see a disembodied element in the corpus of sociological literature."

technology" as a field of culture, and proposing that an account of the growth of science and technology in modern times requires a consideration of not only the internal history of the field, but also "the social and cultural conditions." Focusing on seventeenth-century England, the study is concerned with "the sociological factors involved in the rise of modern science and technology;" more specifically, (i) the rise of interest in science, and (ii) the shifts of interest from one science or technological field to another (Merton [1935] 1938/2001, pp. 3-4). After providing some data pertaining to these explananda, Merton builds two main theses. Chapters four through six argue that the puritan ethic provided support for the rise of modern science. Chapters seven through ten detail how economic and military problems of the day influences scientists' foci of research.

In Chapter eleven, Merton begins by noting that "any attempt to formulate a comprehensive sociological theory of scientific development… [would be] premature" (Merton [1935] 1938/2001, p. 209), and limits himself to offering a preliminary framework. Drawing on Alfred Weber's distinction between three orders of sociological phenomena (society, culture and civilization), Merton locates science and technology under civilization, and proposes to organize pertinent factors under two other two headings: societal and cultural. The discussion of societal factors in this chapter addresses population density and social interaction and the discussion of cultural factors revisits utilitarianism and belief in progress. In introducing the *cultural* factors, Merton provides a key passage on how to understand science as "a social activity" that is worth quoting at length:

> It has become increasingly fashionable to observe that science is, after all, a social activity. Science demands the interplay of many minds, of present thinkers and past thought; it likewise entails a more or less formally organized division of labor; **it presupposes disinterestedness, integrity, and honesty of the scientists and is thus oriented toward moral norms**; and finally, **verification of scientific conceptions is itself a fundamentally social process. But** the dependence of science upon society may be traced to even **more basic considerations** than these. Science, as all large-scale activity which involved the continued interaction of many persons, must above all be countenanced by society, if it is to find any systematic development. Otherwise put, the very existence of science and scientists presupposed that they occupy some positive level in the social scale of values which is the final arbiter of the prestige attached to various pursuits…. **The persistent development of physical science occurs only in societies of a definite order, subject to a peculiar complex of tacit presuppositions and institutional constraints**. … Scientific pursuits may continue only when the drift of interested and capable persons toward the various scientific disciplines is continuous and unfailing; and this vocational espousal of science is assured only through regulated and canalizing forces operating in certain directions rather than by the haphazard proclivities of individuals striking out to satisfy their several interests. (Merton [1935] 1938/2001, pp. 225-6; emphases mine)

This passage presents two key premises informing Merton's early work: (i) "science is oriented toward moral norms"; and (ii) science develops only in certain societies. The first is an early formulation of the Merton's model of science as an institution governed by the scientific ethos. In his introduction to the 2001 edition of *Science, Technology and Society*, Merton ([1935] 1938/2001, pp. xi-xiii) notes that while working on his dissertation, he had not yet begun to think

of science as an institution, and that the first premise found its full expression only in the 1942 paper. Relatedly, in his preface to the 1970 edition of *Science, Technology and Society*, Merton ([1935] 1938/2001, pp. xl-xli) notes that the latter is mainly about the second of these two premises, the one stating that science develops only in certain orders.

Science in the 1930s

Merton's two subsequent articles, "Science and the Social Order" (1938) and "A Note on Science and Democracy" (1942), represent not a very thorough but nonetheless coherent attempt on his part to rework and expand the two key premises formulated at the end of this dissertation this time in relation of the developments of the 1930s. In other words, having just completed his study of the emergence of modern science, Merton turns to theorizing the state of science in his own time.[30] Implicit in both pieces is a narrative that figures modern science that emerged in the seventeenth century and flourished thereafter, as having come back under threat. Taken together, these two articles provide a good sketch of Merton's theoretical perspective and substantive arguments about science. Briefly put, Merton posits that (i) science aims to extend certified knowledge and scientific norms help science achieve this goal; and (ii) that democracy provides the best context for scientific progress. Let me reconstruct each in turn.

1. Science/Norms/Truth

In the 1942 article, Merton defines science as an **institution with a specific goal**, namely, certified knowledge; and posited that science could reach this goal to the extent it was governed by a set of "institutional imperatives," that is, "the "scientific ethos."

> The institutional goal of science is the extension of certified knowledge. … The entire structure of **technical and moral norms implements the final objective.** (Merton 1942, pp. 117-118)

In this section, Merton mentions two technical norms[31], but proceeds to discuss the institutional imperatives that comprise the ethos of modern science: universalism, communism, disinterestedness and organized skepticism. Merton acknowledges that his focus on norms is a limited focus:

> We are here concerned here in a preliminary fashion with the **cultural structure of science**, that is, with one limited aspect of science as an institution. Thus, we shall consider, not the methods of science, but **the mores** with which they are hedged about… similarly we shall not deal with the substantive findings of science (hypotheses, uniformities, laws) except as these are pertinent to standardized social sentiments toward science. (Merton 1942, p. 116)

In defining the goal of science, Merton relies on two boundaries. The first is a boundary between truth and utility, which favors the pursuit of truth for its own sake. Hence, in his dissertation, Merton observes that science in the seventeenth century had not yet become "widely accepted as a value in its own right," and thus was "required to justify itself to men in terms of

[30] This reading is important especially in light of the fact that Merton's subsequent work did not continue to engage with real-world developments in the same way (Mendelsohn 1989).
[31] These are: empirical evidence and logical consistency (Merton 1942, p. 118).

values other than that of knowledge itself (Merton [1935] 1938/2001, p. xl-xli). In writing about developments in Germany, Merton begins by quoting Weber's observation that "the belief in the value of scientific truth … is a product of definite cultures," and elaborates how extrascientific criteria compromise "the purity of science" and undermines its progress:

> [I]f such extrascientific criteria of the value of science as presumable consonance with religious doctrines or economic utility or political appropriateness are adopted, science becomes acceptable only insofar as it meets this criteria. In other words, as the 'pure science' sentiment is eliminated, science becomes subject to the direct control of other institutional agencies and its place in society becomes increasingly uncertain. (Merton 1938, p. 328)

Merton (1938, pp. 329-30) recognizes "the technological criterion of scientific achievement" helps increase "the social support" for science, but notes that it also brings the possibility that "the antipathy toward technological products [would be] projected toward science itself." Relatedly, he observes that scientists at times responded by asserting that "the social effects of science must be beneficial in the long run, but argues that this response was more an article of faith than a statement of fact" (Merton 1938, p. 333).

Secondly, Merton's view of science relies on a boundary between truth and values pursued in other realms of social life. He argues that "there is no logical necessity for a conflict between skepticism within the sphere of science, and the emotional adherence demanded by other institutions" (Merton 1938, p. 335). Problems emerge when the unsophisticated scientist "permit[s] his skepticism to spill over into the area of value generally…"(Merton 1938, p. 334).

2. Science/Social Order/Democracy

The preceding ideas about the goals and norms of science inform Merton's discussion of the relations between science and the social order. In the 1938 article, Merton begins with a very slightly paraphrased version of the passage from Chapter eleven of the dissertation ("the persistent development of science occurs in societies of a certain order, subject to a peculiar complex of tacit presuppositions and institutional constraints"); observes that science has come under attack in many societies; and examines Nazi attacks on science:

> An analysis of the role of science in the Nazi state uncovers the following elements and processes. The spread of domination by one segment of the social structure, the State, involves a demand for primary loyalty to it. Scientists, as well as others, are called upon to relinquish adherence to all institutional norms which conflict with those of the State. The norms of the scientific ethos must be sacrificed, in so far as they demand a repudiation of the politically imposed criteria of scientific validity or of scientific worth. The expansion of political control thus introduces conflicting loyalties. (Merton 1938, p. 325)

This passage provides a good glimpse of Merton's mode of reasoning. Given that his definition of science as an institution foregrounds the cultural dimension, the question of whether the societal context supports science becomes whether it allows the scientific norms to function. Hence, when he turns to the situation in Germany, he interprets the conflict between the totalitarian state and the scientist as deriving in part from "an incompatibility between the **ethic**

of science and **the new political code** imposed on all irrespective of occupational creed" (Merton 1938, p. 326).

Merton's discussion of how the Nazi regime undermines science is enmeshed with comments on how a liberal order supports science. According to Merton, in contrast to a totalitarian order, a liberal order gives non-political institutions, including science, a substantial sphere of autonomy. 1942 article expands these comments. After offering the "provisional assumption" regarding the affinity of science and democracy, Merton's discussion of scientific norms touches upon how a democratic order supports modern science. For example, universalism in science means "truth-claims … are to be subjected to preestablished impersonal criteria," and that scientific careers should be open to all talents (Merton 1942, pp. 118-9). According to Merton, democracy supports this scientific norm, because it also includes universalism as a main principle, however inadequately it may be put in practice (Merton 1942, p. 120).

Underlying Premises

According to Hollinger (1996, p. 89), Merton's writings in the 1930s drew on "two loosely connected traditions in the public discussion of science:" one consisting of characterizations of the scientific "spirit" or "habit of mind," and the other consisting of explicit descriptions of the relation between science and democracy. Participants in the second tradition commonly wrote that "democracy and science … shared such qualities as freedom of thought and respect for the dignity of human race" (Hollinger 1996, p. 85). These qualities were commonly attributed to scientists or to democrats as individuals, or to the general "spirit" of science or of democracy. What made Merton's contribution distinctive and influential was his sociological outlook. In place of individual scientists or the "spirit" of science, Merton put forth a view of science as an institution and community (Hollinger 1996, p. 86). In other words, Merton took the key sociological step, but two main factors consequentially shaped his sociology of science: (i) a focus on "the cultural," and (ii) a set of *a priori* beliefs about both science and democracy, which he shared with some of the intellectuals of his time.

As many commentators have pointed out, Merton's dissertation had two parts: on focusing on the role of Puritan values and the second, on the role economic and military problems.[32] It is notable how Merton basically considered most possible culprits (religion, economy and the state) in accounting for the emergence of modern science. Yet, his arguments about how these culprits contributed to the outcome are very careful, confined and *cultural*.[33] To say this for the first part of the study is not contentious. Most commentators agree that Merton's argument about how Puritan values helped to make science a respected pursuit is a cultural argument. Yet, Merton's argument about the role of economic and military problems in shaping scientists' foci of research is also a cultural argument is not as universally accepted. Zuckerman (1989) maps Merton's two theses (one about the role of religion/Puritanism; and the other about the role of military and economic problems) onto three interrelated dichotomies: religion *versus* state/economy; culture (values, norms) *versus* society (social structure); idealistic *versus* materialistic. In her view, Merton's analysis of economic and military influences stands for

[32] See Merton ([1970] 2001, p. xxxv), Zuckerman (1989), Enebakk (2007) on how the differential reception of the two parts.

[33] Much water has flowed under the bridge, and it is awkward to describe arguments as "cultural", invoking the cultural/material divide in sociology. I use the term "cultural" to specify Merton's explanatory move without implying that the cultural/material divide holds.

"society," and so, for Merton, "society *and* culture contributed to the institutionalization of science" (Zuckerman 1989, p. 239). Yet, there are reasons to question this reading. In Chapter eleven of the dissertation, Merton includes economic and military influences not under societal factors but under cultural context. His retrospective account in the introduction to the 2001 edition also suggests that "economic and military utilities" helped science "to justify itself":

> This brings us directly to a principal assumption underlying the entire book. The substantial and persistent development of science occurs only in societies of a certain kind, which provide **both cultural and material conditions** for that development. This becomes particularly evident in the early days of modern science before it was established as a major institution with its own, presumably manifest value. Before it became widely accepted as a value in its own right, **science was required to justify itself to men in terms of values other than that of knowledge itself**. This underlying idea unites the several themes of the monograph, the one dealing with the role of Puritanism, and the other with **the role of economic and military utilities** in the institutionalization of science (Merton [1935] 1938/2001, p. xl-xli; emphases mine).

In this passage, Merton starts out by restating the "persistent development thesis" with reference to cultural *and material conditions*. Yet, economic and material factors are reduced to arguments about the utilities of science.

Zuckerman's (1989, p.254) argument that Merton's second thesis (about the role of economy and military) received less attention was because it was "insufficiently materialist for the Marxists and too materialist for the non-Marxists" offers a window onto the politics of knowledge in the 1930s. In contrast, I contend that Merton's second thesis was not even materialist. In chapters 7-10 of the study, Merton paints an impressively succinct picture of the economic and military developments in seventeenth century England, but the question he poses and answers is only about how economic and military needs may have had mostly an indirect influence on scientists' choice of topics. If Merton had *really* pursued the question "What are the *material* conditions that helped the emergence of modern science and technology in seventeenth century England," that would probably have required quite a different response.

When Merton turned to the 1930s, he proposed that the problem of the emergence of science and the persistence of science were different sorts of problems, but also remarked how the attacks on science on the 1930s obliged scientists to defend science as if it was just emerging! In any case, now Merton (1942, p. 116) proposed to theorize science as an institution, and to explicitly focus on its cultural structure. Focusing on the cultural (norms of science) was defined in opposition to focusing on the content or findings of science. In the two articles published in 1938 and 1942, Merton's discussion of scientific norms was closely coupled a discussion of the relations of between science and societal context in terms of normative congruence. When Merton discussed whether the societal context supports science or not, he focused on whether the societal context allows scientific norms to prevail in the institutional sphere of science. In this framework, a totalitarian order hampered science because it made demands on scientists at odds with the scientific norms. His statements about why science and democracy fit well together took two forms. On the one hand, he posited that a democratic order offered autonomy to different institutional spheres, including science, which meant these spheres could operate on their own terms. On the other hand, he suggested that science and democracy fit well together because they, in fact, cherished more or less the same norms!

Examining the structural-functionalist strains in Merton's model of science is beyond the confines of the current discussion. Yet, the following passage provides a glimpse of the underlying theoretical framework:

> A principal sociological idea governing this empirical inquiry holds that the socially patterned interests, motivations and behavior established in one institutional sphere – say that of religion or economy – are interdependent with the socially patterned interests, motivations and behavior obtaining in other institutional spheres – say that of science. **There are various kinds of such interdependence, but we need touch upon only one of these here**. The same individuals have multiple social statuses and roles: scientific and religious and economic and political. This fundamental linkage in social structure in itself makes for some interplay between otherwise distinct institutional spheres even when they are segregated into seemingly autonomous departments of life. … Separate institutional spheres are only partially autonomous, not completely so. It is only after a typically prolonged development that social institutions, including the institutions of science, acquire a significant degree of autonomy. (Merton [1938] 1970, p. xxxi-xxxii; emphasis mine)

In this framework, social world is divided into partially autonomous institutional spheres, and one of the reasons why institutional spheres are interdependent is because one individual may show up in more than one sphere. This is why a political order demanding values other than cherished by the scientific institution creates strains on the scientists.

In concluding the 1938 paper, Merton (1938, p. 336) stated that "anti-science movement derives from the conflict between the ethos of science and of other institutions," and offered a list of conditions under which such conflict arises:

> Conflict arises when the social effects of applying scientific knowledge are deemed undesirable, when the scientist's skepticism is directed toward the basic values of other institutions, when the expansion of political or religious or economic authority limits the autonomy of the scientist, when anti-intellectualism questions the value and integrity of science and when non-scientific criteria of eligibility for scientific research are introduced. (Merton 1938, p. 336)

This list appears like a mixed bag but is actually informed by very clear presumptions on Merton's part. The first condition is based on the assumption that science and its applications can be demarcated. The second condition is linked to Merton's belief that scientists' skepticism, in principle, need not clash with values of other institutions. The following three conditions are basically derived from the situation in Germany.

In sum, Merton's step to sociologize science was a big step in his time, but was very much informed by his theoretical framework and a priori commitments to science and democracy. His focus on the cultural structure of science had to do with his choice to side with structuralist-functionalism despite his early interest in British Marxist historians' approaches to science. The distinctions between truth and utility, and between truth and values, were derived from an implicit belief in science as the pursuit of knowledge for knowledge's sake independent of utility and value implications. Merton's provisional suggestion about the fit between modern science and democracy was similarly derived from an a priori commitment to liberalism.

After 1942

After 1942, study of the relations between science and its societal context was neglected, as Merton and his students focused mostly on the internal workings of the scientific community (how norms worked). Zuckerman's (1988) comprehensive review of the development of the field between the late 1950s and the late 1980s attests to this shift. Zuckerman (1988, p. 513) states that the agenda of the sociology of science comprises three parts: "social and cognitive organization of scientific work, problems in the sociology of scientific knowledge, and the reciprocal connections between science and its social contexts." Yet, the content of the review shows that Mertonian school focused on the first two parts and not at all on the third part. In one of the appendices to the review, Zuckerman (1988) lists the lines of research she did not examine: and these are (1) comparative analysis of science in different societies and (2) the relations between science and politics, economy, the academy and technology. One major exception was Bernard Barber's book, *Science and the Social Order* (1952), which took up Merton's initial questions about the links between science and societal structures and values, and extended his contrasts between liberal and democratic societies.

In his foreword to Barber's book, Merton touches upon this neglect. He observed that the more a phenomenon became "defined as a social problem", the more it became "a proper object of study," and suggested that science may be reaching that threshold (Merton 1952, p. xx). In the 1970 preface to his own *Science, Technology and Society*, Merton ([1970] 2001, p. xxxi) provides a *post facto* overview of the ideas of institutional sphere, autonomy and interdependence that underlay his approach to science. Next, Merton [1970] 2001, p. xxxiii) observes that the nature of the interdependence among science, economy, and government changes historically; and that the relations in seventeenth century England are very different from the relations in twentieth century United States or Soviet Union, "where science has long been institutionalized, where scientific research requires vast support and where it has acquired new magnitudes of consequence for technologies of production and destruction." This observation, made in 1970, in a way, lays out the *third installment* of the Mertonian program, which was never really pursued (the first being about the emergence of modern science in seventeenth century, and the second about science in the 1930s). The question is, however, is the Mertonian model of science and its relations to the societal context adequate for grasping the promises and perils of science since the 1930s to the present?

Towards a New Political Sociology of Science

Since the late 1970s, many have responded to the latter question in the negative. Some have questioned the assertion that scientific norms helped science produce valid knowledge, and argued that this account of science can be better viewed as ideology (Barnes and Dolby 1970; Mulkay 1976). The new sociology of scientific knowledge (SSK) argued for going beyond the Mertonian focus on professional norms and mechanisms to examine how social factors influence scientific *knowledge* (Barnes 1974; Bloor [1976] 1991; Lynch 1993; Mulkay 1979).[34] The Strong

[34] A number of scholars have observed how Merton did recognize that sociology of knowledge required the analysis of the impact of social factors on intellectual products, including scientific truths (Gieryn 1982a; Bourdieu 1990; Meja and Stehr 1999). In his foundational review of the field of sociology of knowledge, he stated that "[t]he 'Copernican revolution' in this area of inquiry consisted in the hypothesis that not only error or illusion or unauthenticated belief, but also the discovery of truth was socially (historically) conditioned" (Merton [1945] 1973, p. 11). It is also notable that the key passage I quoted earlier from Chapter eleven of his dissertation includes the

Programme insisted that true and false claims need to be explained symmetrically; that is, with reference to the same set of causes (Barnes and Bloor 1982; Bloor [1976] 1991). Around the same time, a number of scholars began to adopt ethnographic, ethnomethodological and interactionist approaches to studying scientific laboratories, work and practices (Latour and Woolgar [1979] 1986; Knorr-Cetina 1981; Lynch 1993). These turns to focus on the content of science and on the everyday processes of knowledge production sought to break the link Merton stipulated between the normative structure of science and the production of truths. Yet, critics of Merton never really focused on his work on the relations between science and its societal context. The next round of work in the field of science and technology studies moved quickly to emphasize (i) the constructedness of the boundary between science and non-science (Gieryn 1982, 1999); and (ii) the mutually constitutive role of science, technology, society and nature (Latour 1987, [1984] 1988, 1999; Jasanoff 1987, 2004).

I propose another line of critique so as to work towards a better theorization of the relations between science and the political order in the current era. Merton's sociology of science had an important thrust: to theorize science as an institution in relation to its context. Yet, his theory of science shaped by his focus on the "cultural," and his implicit assumptions about science and democracy. Juxtaposing these assumptions with the developments in science since the 1930s allows us to rethink what theorizing science as an institution may mean, and what the institutional attributes of science mean for the relation between science and democracy.

In the very decade Merton put together his model of science and democracy, big science was born (Price 1963; Galison and Hevly 1992). While the term "big science" is often growth of large-scale research during and World War II, and especially the Manhattan project, Seidel (1992) traces its beginnings to the period between the two world wars, and particularly to the work in high-energy physics led by Ernest Lawrence at the Radiation Lab at UC Berkeley. Big science involves costly machinery, teamwork and hierarchy, blurring of the boundary between science and technology and interdisciplinarity. From those origins in the 1930s, big science spread to other states, drawing major amounts federal funding through the Cold War until the present.

With these developments, whatever descriptive and critical gains Merton's model of science provided prior to the 1930s were seriously diminished. As scientific research became increasingly dependent on costly technologies and materials, only research centers with the necessary resources and technology can achieve scientific breakthroughs. The role of scientific research in defense and in war-making in effect imploded the distinctions Merton sought to maintain between truth, utility and values. With increasing federal funding for science in accordance with national defense goals, the notion of science for science's sake or of science as value-free became untenable. Let me elaborate these points in a little more detail.

1. Theorizing Science as an Institution

"Institution" is a much-contested term in sociology. Without going into the existing definitions, let us ask the questions Merton asked. If science is an institution, what is its institutional goal and what are the necessary conditions for the institution to reach that goal? Merton proposed that the goal of science is to extend certified knowledge and that the cultural structure of science, the scientific ethos that governed scientists' behavior was necessary for achieving this goal. Critics have questioned these assertions, asking if scientific norm really

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statement: "verification of scientific conceptions is itself a fundamentally social process" (Merton [1935] 1938/2001, p. 225). Yet, he and his colleagues did not study the production or the products of science.

govern scientists' behavior, or if that is why science produced reliable knowledge, and so on. If Merton's answers were wrong, what is the institutional goal of science and the necessary conditions for its attainment? Before outlining an alternative answer, I will review two related lines of scholarship that have addressed related questions: Bourdieu and the new political sociology of science.

Bourdieu, beginning at the same time as some of the new sociologists of scientific knowledge, similarly objected to Merton's model of science as an integrated and irenic world where the internalization of scientific norms guarantees the unraveling of truths, and proposed to approach science as a field. The scientific field is one where agents compete for "the monopoly of *scientific authority* … in the sense of a particular agent's socially recognized capacity to speak and act legitimately in scientific matters" (Bourdieu 1975, p. 19). Hence, while in Merton's (1973, p. 277) view, scientists are to be socialized to abide by the norm of "organized skepticism", in Bourdieu's view, it is the contentious struggles for scientific capital that allows to constant agonistic scrutiny of scientific claims. In replacing Merton's irenic view with an agonistic view, Bourdieu highlighted how the struggle for scientific authority depended on not only embodied but also material resources:

> [I]n order to be pursued, the sciences require two types of resources, strictly scientific ones, which are for the most paert incorporated, and the financial resources needed to buy or build equipment (such as the Berkeley cyclotron) or to pay staff, or administrative resources, such as posts. (Bourdieu 2004, p. 57)

Hence, in scientific fields, researchers struggle for two kinds of scientific capital: "a capital of strictly scientific authority and a capital of power over the scientific world which can be accumulated through channels that are not purely scientific (Bourdieu 2004, p. 57).

Bourdieu's approach to the scientific world does help overcome Merton's culturalist/normative bias and almost total neglect of the material conditions of production of science. Yet, even as he recognizes that science depends on material resources, Bourdieu posits that scientific advance depends on the relative autonomy of the scientific field. In Bourdieu's framework, the scientific field is relatively autonomous in the sense that it has developed its own *nomos*. This in turn means that it is the site of struggle for scientific capital (of both kinds of scientific capital), and other forms of capital (economic, political) can be effective only if they are converted into scientific capital. Further, it means that in adequately autonomous scientific field, the pursuit of scientific capital is at odds with the pursuit of the political or economic forms of capital that reign in the space beyond the scientific field.

In recent years, a number of sociologists have called for renewed attention to the relations among science, politics and economy. In the introductory chapter to *The New Political Sociology of Science*, Frickel and Moore (2006, p. 4-6) state that while "the interdependence of science, politics and economy has become virtually axiomatic in contemporary science studies scholarship," understanding how these interconnections have changed since the 1970s requires focusing on "the political and institutional dynamics that shape the funding, administration and practice of science," and "a deeper appreciation for the institutional bases of power in knowledge production" (Frickel and Moore 2006, pp. 4-6). Yet, both the authors contributing to *The New Political Sociology of Science* (NPSS), and a number of other organizational sociologists who have worked on growing university-industry research alliances in biotechnology tend to use the

concepts organization, field, institution and network eclectically[35] and tend to focus on "local" convergences, conflicts and contradictions. More recently, Moore and Frickel were joined by Kleinman and Hess in another overview piece expanding the "political sociology" approach. Moore et al. (2011, p. 506), drawing on Bourdieu, propose to approach science as "a quasi-autonomous field of power that is subject to influence from other fields but also possesses a degree of self-governance" and seek "to develop an historical sociology of recent changes in the scientific field that does not underplay the importance of structural change, including the increasing power of multinational corporations in global economics and politics, and recognizes the role of scientists, political and industrial reformers, and the organized citizenry in charting directions for the scientific field."

Considering Bourdieu's revisions to Merton, and how the NPSS draws on Bourdieu is revealing. Both efforts take into account science's dependence on material resources and raise questions about science's relation with economy and politics, but both are limited by the assumption of relative autonomy. In Bourdieu's case, the idea that the scientific field is relatively autonomous is linked with both his way of defining fields are relatively autonomous, and his implicit vision of science as the pursuit of truth. In the case of the NPSS, the term "field" is used more as an organizational term: science is a field in the sense that it "possesses some degree of self-governance." In contrast, I contend that theorizing science as an institution requires full recognition of science's dependence on economic, political and material resources, and discarding the assumption of relative autonomy.[36] This need not mean all boundaries are constructed or there are only associations. Rather, it means that economic, political and material resources make scientific advances possible and shape facts and artefacts in structured ways.

2. Truth, Utility, Values

In his discussion of the emergence of modern science in seventeenth century England, Merton ([1938] 2001 pp. 191-3) suggested that in its period of emergence, science had justify itself by utility arguments. In discussing the attacks on science in the 1930s, he argued that "there is no logical necessity for a conflict between skepticism within the sphere of science, and the emotional adherences demanded by other institution" (Merton 1938, p. 335). Problems emerged only when the unsophisticated scientist "permit[ted] his skepticism to spill over into the area of value generally"(Merton 1938, p. 334). With these arguments, Merton retained his implicit vision of science as the pursuit of knowledge for knowledge's sake removed from values governing other institutional spheres.

Merton's vision of science fit well with the discursive consensus that underlay the Cold War era science regime: "the idea of science as a public good." This idea was rooted in and reflected a belief in a kind of autonomous science that could be beyond political interests,[37] and did a lot of things for the different actors involved. It helped overcome the fights over the question of "whom the federal funding was subsidizing", and legitimate the substantial amounts of federally funded R&D. It also helped the recipients, that is, the universities construct a specific definition of autonomy vis-à-vis the government (Leslie 1993; Lowen 1997). The basic/applied distinction was a key tool for both scientists and universities to respond to the

[35] See Owen-Smith and Powell (2008). See Emirbayer and Johnson (2008) for a discussion on the use of the concepts "organization" and "field" in organizational studies.

[36] See Panosfky (2014) discussion of "misbehaving science" for another fruitful departure from common ways of employing Bourdieu's concept of the field to study science.

[37] Dennis (2004) argues that a crucial key to understanding Vannevar Bush's efforts is to note that he embodied this belief.

objections raised to the contribution of science to war-making (Smith 1971; Kleinman and Solovey 1995; Moore 1996).

Since the late 1970s, pressures of economic competition, federal policies aimed to encourage technology transfer (Berman 2012a), the growth of the biotechnology industry (Kenney 1986; Krimsky, Ennis and Weissman 1991; Rabinow 1996), and increasing connections between scientific research and technological innovation, have undermined the preceding consensus. Scholars and policy-makers have questioned the distinction between basic and applied science (Nelkin 1978; Slaughter 1993; Stokes 1997; Calvert 2006). Tellingly, NSF's project evaluation criteria were changed to include implications for society (Jasanoff 2005, p. 229). Today, there is much more recognition that science and technology are closely intertwined, and thus, scientific research cannot be examined separately from the uses it is put into and the values it embodies.

3. Science and Democracy

While Merton's writings on science and democracy were clearly informed by an implicit commitment to both, his focus on the cultural and his theorizing the relations between the scientific institution and the societal order in terms of normative congruence severely limited his diagnoses. Merton did touch upon two ways in which the liberal order may constrain the flourishing of science in the 1942 article. First, he noted that while democracy includes universalism as a dominant principle, laissez-faire democracy permitting the accumulation of differential advantages can clash with universalism in science (Merton 1942, p. 121). Second, he noted that the norm of communism, that common ownership of scientific products, is "incompatible with the definition of technology as 'private property' in a capitalistic economy" (Merton 1942, p.123).[38]

In an article titled "Challenges for the Democratic Management of Technoscience," Pestre (2008) asks: "how [can we] understand the various links between science, the political order and democracy?" Pestre's is a useful way of posing the question for it introduces a distinction between the political order that exists and democracy as an ideal. I contend that the key move needed in grasping the relations between science and the politico-economic order is to part with the notion of "relative autonomy" of science without discarding institutional analysis. Put another way, tracing the institutional dependence of the production of technoscience on economic capital and technology goes a long way to illuminate the ways in which technoscience is used and the kinds of values that are reinforced.

The EBI case can help illustrate this point. From the perspective developed above, the changes in the American science regime since the 1970s have for the most part reinforced the trends that began in the 1930s, and the Energy Biosciences Institute stands as a signpost for these trends. Contemporary science is big science in the sense that it requires major funding, depends on expensive technologies, is increasingly conducted in teams and is increasingly interdisciplinary. Scholars have used the term "big science" not only to describe the cost and scale of research projects, but also to specify the peculiarities of organizing research within teams using large-scale machinery. As such, some have argued that molecular biology is not "big science" in the full sense of the term (Knorr-Cetina 1999, pp. 81-83). I argue otherwise. Research conducted under the EBI roof is also heavily dependent on technology, funding and characterized by teamwork and interdisciplinarity. Further, the EBI is an organizational node in a

[38] Observers have linked these comments to Merton's relation to the British Marxist historians of science, who at the time wrote about the "frustration of science by capitalism" (Mendelsohn 1989; Enebakk 2007).

broader field of academic and private industry labs pursuing related scientific questions and technologies. The monetary resources required for entry into this field are raised not only in traditional ways but also from capital markets. The reward structure has also become more mixed in that scientific breakthroughs bring not only scientific prestige but also additional funding for research or personal profit in the form of patent income or gains in stocks.

Further, the EBI case actually showcases the untenability of the distinctions between science, utility and values. As its advocates emphasized throughout the controversy, the EBI is a research *and development* organization that aims not only to conduct fundamental research, but also to ensure speedy technology transfer. In a related manner, it also recognizes the risks associated with emerging technologies and attempts to address their economic, legal and social implications. Hence, the EBI's research program comprises two substantially different components. The first focuses on developing economically viable second-generation biofuels. The second component focuses on environmental and socio-economic impacts, seeking to assess how the growth of a biofuels industry might impact land, water, air quality; land use patterns and food supply; climate change; and geopolitical conflicts. Once again, this research aims to chart the "the right way" to develop the biofuels industry. In short, in contemporary science, not only has the boundary between science and technology (truth and utility) been largely dissolved, the interdependence between economic resources and scientific advance translates into the dominance of the possessors of economic capital over the definition of good science, good technology and green energy.

CHAPTER FOUR

ACADEMIC FREEDOM IN THE ENTREPRENEURIAL UNIVERSITY

In recent decades, the growth of university-industry research relations have led to debates on how these relations impact university autonomy and academic freedom (Nelkin and Nelson 1987; Busch et al. 2004; Washburn 2005; Slaughter 2011; Calhoun 2009a, 2009b). In 2014, American Association of University Professors (AAUP) responded to these debates by proposing a set of recommended principles to guide academy-industry relations. One of these principles states that "the university must preserve its academic autonomy—including the academic freedom rights of faculty, students, postdoctoral fellows, and academic professionals— in all its relationships with industry and other funding sources" (AAUP 2014, p. 4). Yet, the question of how universities can achieve this principle has led to contentions.

On November 14th, 2007, University of California, Berkeley (UCB) signed a contract with British Petroleum (BP) to found a new research and development organization: the Energy Biosciences Institute (EBI).[39] With $500 millions of BP funding provided over ten years, the EBI brings together researchers from UCB, Lawrence Berkeley National Laboratory (LBNL), the University of Illinois at Urbana Champaign (UIUC), and BP for the development of biofuels.[40] The founding of the EBI got mired in a controversy on the Berkeley campus and the media, including a polarizing conflict among the faculty.[41] Critics, mainly from the humanities and the social sciences, faulted the administration for lack of transparency and debate, and invoked the principle of shared governance to institute faculty senate oversight in the making and the execution of the partnership. In response, faculty from science and engineering departments, along with members of the administration, described the critics' move as an attempt to block the partnership and a threat to academic freedom. The EBI controversy was not the first of its kind. Over its duration, the contending sides frequently drew on other related controversies, mainly, the Novartis controversy surrounding the five-year (1998-2003), $25 million agreement between Novartis Agricultural Discovery Institute (NADI) and UCB's Department of Plant and Microbial Biology (PMB), and the Tobacco Funding Debate (2002-2007) that was ongoing at the time.

How are contemporary definitions and defenses of academic freedom shaped by the ongoing transformation of universities? Writings on academic freedom often focus on reiterating the centrality of academic freedom to the idea of the university at the expense of examining how both are being transformed in the context of growing university-industry relations (Nelson 2010; Evans 2014). Sociology of the academic profession has long analyzed the divisions among the professoriate, yet much of this work relies on the notion of "academic subcultures," and fails to link the content and influence of these subcultures to structural forces acting on universities (Ladd and Lipset 1975; Schuster and Finkelstein 2006; Rothman et al. 2011; Gross 2011). Organizational scholars working on the growth of university-industry research relations have paid less attention to the ensuing divisions and struggles within universities (Powell and Owen-

[39] http://www.berkeley.edu/news/media/releases/2007/11/14_ebisigning.shtml

[40] It is the largest-to-date example of the university-industry research partnerships that have proliferated in recent decades. It is also one of a series of research agreements between major oil companies and leading research universities in the United States (Goozner and Gable 2008; Washburn 2010).

[41] http://stopbp-berkeley.org/ (Last accessed February 19, 2009).
http://webcast.berkeley.edu/event_details.php?webcastid=19207
http://academic-senate.berkeley.edu/ebi.html

Smith 1998; Vallas and Kleinman 2008; Berman 2012a, 2012b). In this paper, I argue for a theoretical framework linking (i) the ongoing debates on academic freedom and university autonomy, (ii) the intensifying divisions within the academic field, and (iii) the broader transformation of universities amidst growing relations with the economy. The faculty conflict on the EBI provides an extremely fruitful focus for examining these links, and for revealing the necessity and the yields of doing so.

As I will recount in detail below, the two faculty groups that clashed over the EBI agreement pursued two fundamentally distinct projects. The critics of the EBI were concerned about the impact of the agreement on the university, and employed the principle of "shared governance" to fight for institutional autonomy. In response, the advocates of the EBI advanced an individualist definition of academic freedom to defend their freedom to receive funding from any source, subject to university regulations, and argued that the critics' efforts constituted a threat to academic freedom. Why did the advocates come to perceive critics' struggle for institutional autonomy as a threat to academic freedom? The key to answering this question lies in recognizing that the university is a divided field. In the entrepreneurial science and engineering disciplines, where research requires substantial amounts of funding, growing relations with industry has meant increased opportunities for gaining scientific authority and administrative influence. More strikingly, the blurring the boundary between basic and applied science in several disciplines has led to convergence in the epistemological goals pursued in academia and industry. Hence, faculty in entrepreneurial sciences and engineering posit that industry funding increases their academic freedom. In the humanities, social sciences and the conservationist disciplines, researchers pursue knowledge that is not readily funded by private industry, and are concerned that the relative scarcity of extramural funding constrains the livelihood of alternative research paradigms and the influence that these disciplines have in university governance. Hence, they come together behind the cause of defending university independence through administrative and faculty senate mechanisms. In the conflict that is the focus of this paper, the two factions came to a clash at the special meeting of the Academic Senate held on April 19th, 2007. Drawing on previous or ongoing debates on industry funding, they proposed competing resolutions. In the meeting hall, the advocates of the EBI had the majority and won the vote. Some faculty senate oversight was instituted, but oversight processes continued to define and defend academic freedom in individualist terms.

While focusing on a single episode of contention around how to defend academic freedom and university independence, the paper aims to make the case for and contribute to two broader and much needed lines of research. The modern idea of academic freedom has emerged and developed along with the modern university, and continues to be a key notion in discussions on universities. My analysis of the faculty conflict on the EBI highlights that we need a historical sociology of "academic freedom" that will uncover how it is been defined and defended in different socio-historical contexts, and offers one piece of the larger account. At the same time, I argue for a power-sensitive and dynamic understanding of the academic field in its relations with the state and the economy. Such an approach reveals the specificity of the contemporary American research university, where the dominance of science and engineering faculty is translated into specific definitions of "valuable knowledge" and "academic freedom."

I begin with a review of existing studies of the American professoriate, and studies of the growing university-industry research relations to highlight the gap that this paper aims to address, and outline how Bourdieu's work on the French academic field provides a useful point of departure. The second section provides a historical sketch of the development of the modern

university and the evolving definitions of academic freedom. The empirical bulk of the paper recounts the faculty conflict on the EBI (with short incursions into Novartis and Tobacco Funding debates), and shows how professors' oppositional stances were rooted in oppositional views and sensibilities on a number of related issues. The discussion and conclusion aims to specify the yields of an interlinked analysis of debates on academic freedom, epistemic cultures and the transformation of the academic field.

Professions, Organizations and Ideas

Studies of the Professoriate

The faculty conflict around the EBI immediately brought to the minds of many C. P. Snow's much debated lecture, *The Two Cultures and the Scientific Revolution*, delivered in 1959 at the University of Cambridge. On this occasion, Snow ([1959] 1963, p. 3) observed that "the intellectual life of the whole of western society [was] increasingly being split into two polar groups … literary intellectuals [and] scientists." He characterized each group as "a culture …in an anthropological sense," sharing "common attitudes, standards and patterns of behaviour" (Snow ([1959]1963, p. 9).[42] Studies of the American professoriate have followed in the footsteps of C. P. Snow and have made considerable use of the term "culture." In one of the early studies, *The Academic Mind* (1958), Lazarsfeld and Thielens had an explicit concern about academic freedom, and used data from 2451 interviews with social scientists at 165 colleges to analyze professors' perceptions of and reactions to McCarthyism. Ladd and Lipset's (1975) landmark study, *The Divided Academy*, examined survey and interview data on professors' views on campus and national politics of the 1960s. Ladd and Lispet (1975, p. 68, 70-74) discussed two ways of differentiating "academic subcultures" (liberal versus applied, and natural versus social science); and suggested that disciplines house different degrees of liberalism versus conservatism because they differ in "subject matter; and the concerns, orientations, identifications and associations the subject nourishes."

Since the 1980s, there have been a series of studies documenting the growth and diversification of the professoriate (Finkelstein 1984; Bowen and Schuster 1986; Clark 1987a, pp. 134-8, 1987b; Becher [1989] 2001; Finkelstein et al. 1998; Schuster and Finkelstein 2006). These studies have also linked professors' views to disciplinary, departmental or institutional cultures, but have not addressed the relation between these academic subcultures and the changing political economy of universities.[43] For example, Rothman et al.'s (2011), *The Still Divided Academy*, a follow-up survey to Ladd and Lipset (1975), analyzes professors' responses on the mission of the university and the state of academic freedom merely as components of an interactively produced institutional culture. Gross (2011, p. 115) argues that work on professors' views on academic freedom is part of a broader agenda of examining how epistemic cultures deal with the "knowledge-politics problem," (the problem of "formulat[ing] stances on how political views should factor into knowledge work"), but also focuses on explicit cultural understandings at the expense of the structural context of knowledge work.

[42] Snow's focus was less on the causes of this split but more on its consequences. He argued that the two cultures not only failed to understand, but had become hostile towards each other; and that this was a great loss. He was particularly critical of the literary intellectuals for failing to appreciate the industrial and the scientific revolutions, which he found to be not only undoable but greatly desirable from the point of view of the poor.

[43] See Hackett (1990) for an exception.

Organizational Studies

Since the late 1990s, organizational sociologists have examined how developments in the life sciences impact knowledge production, innovation and universities. Powell and Owen-Smith (1998, p. 267, 270) highlighted the "crumbling" of the boundaries between universities and firms in the life sciences, and the dearth of work on "the consequences of these developments on the internal culture of universities." To address this gap, Owen-Smith and Powell (2001, p. 33-34) examined how changes in the life sciences were shaping academic careers and work practices through in-depth interviews with scientists, and argued that "the growing salience of networks" was leading to "new fault lines among faculty, between faculty and students, and between scientists' interests and those of their universities." Berman (2012a, 2012b) has used organizational field theory to analyze how the field of academic science in the United States has changed since the later 1970s. While these studies have shed much light on the structural changes affecting knowledge production in American universities, they have focused on either parts of the university (scientists, scientific laboratories, licensing offices) in relation to the economy, or the broader organizational field (field of research universities or field of U.S. academic science), and have not addressed the intensifying divisions and struggles *within* the academic field.

Towards an Analysis of the American Academic Field

For an interlinked analysis of professors' views on academic freedom and the ongoing transformation of American universities, Bourdieu's work on the French academic field provides a good starting point in that it offers both a fruitful theoretical framework and a comparative vantage point to examine the American case.[44] In *Homo Academicus* ([1984] 1988), Bourdieu theorizes the French academic world as a set of nested fields that came into existence over the nineteenth century by gaining autonomy from the political and intellectual fields, and where *academic capital* constitutes the legitimate source of power and prestige. He identifies *two kinds of academic capital*: temporal and scientific. Temporal capital consists in "control of instruments of reproduction of the professorial body," and scientific capital refers to scientific authority among academics and reknown beyond academia (Bourdieu [1984] 1988, p. 78).[45] In the first part of the book, Bourdieu focuses on Parisian professors in the faculties of science, arts, law and medicine circa 1968, and finds that while professors of medicine and law held more temporal capital, professors of arts and sciences held more scientific capital. Further, Bourdieu ([1984]1988, p. 74) postulates that while the former are oriented towards exercising their temporal power to reproduce the dominance of temporal capital in the field, and the latter are oriented towards accumulating scientific capital through research. More strikingly, he argues that the two types of academic capital are at odds: "the acquisition and exercise of administrative

[44] See Albert (2003) for an attempt to use Bourdieu's framework to examine how relations to the market shapes knowledge production. See Emirbayer and Johnson (2008) for a review of use of Bourdieusian concepts in organizational studies, and Albert and Kleinman (2011) for a discussion on how to bring Bourdieu to science and technology studies.

[45] The concept of "scientific capital" takes on its meaning *within* the field under examination. When examining the scientific field, Bourdieu (2004, p. 57) distinguishes between the scientific and temporal types of scientific capital. When examining the academic field, he employs a less differentiated concept of scientific capital defined in relation to the temporal type of academic capital. In *Homo Academicus*, indicators of scientific capital include positions in CNRS committees and labs, teaching in intellectual schools, number of scientific conferences attended, and translations of work. Indicators of the temporal type of academic capital include academic positions less related to research (Bourdieu [1984]1988, p. 46).

power in the university field tends in fact to compromise the accumulation of a capital of scientific authority and vice versa" (Bourdieu ([1984]1988, p. 96). In the second part of the book, he focuses on the field of arts and social sciences faculty, and finds that this smaller field is also characterized by an opposition between temporal capital and scientific capital.[46]

Bourdieu's findings in *Homo Academicus* reflect the specificities of the French academic field in the 1960s.[47] First, the Parisian universities that Bourdieu focuses on were all state institutions. Second, academic careers in these universities were not necessarily and as heavily dependent on research as they have later become both in France and the United States. The faculties of law and medicine were engaged mainly in teaching the new cadres for legal and medical practice. In contrast, the teachings of the faculties of arts and sciences had less to do with such functions, and therefore, were allowed more lattitude to analyze the given order.

In comparison to French academic field of the 1960s, the American academic field differs in several respects. First, American universities are funded through many sources, faculty positions are funded mainly by state allocations (at decreasing levels), and the research funding comes in the form of grants from foundations, corporations and the government. Second, in the elite institutions, the Cold War era growth of federal funding for research has brought the research function to a position of unparalleled dominance. The prestige of universities, departments and faculty members are measured by research output, and it is this prestige that determines the prospects for more research funding, more applicants for undergrad admissions, more achieving faculty and more prestige. In other words, in elite American universities, the pursuit of extramural funding for research, scientific capital and temporal capital are all interdependent. Of course, the degree to which a discipline or department contributes to the teaching mission of the university constitutes a basis of temporal capital. That is, a discipline or department might not be attracting major research funding but nonetheless retained. Yet, in elite research universities like Berkeley, research distinction is a *sine qua non* of viability; teaching is a weak basis for temporal capital.[48]

In addition, two interlinked developments of the recent decades, the blurring of the boundary between basic and applied research in the biosciences, and the growth of university-industry research relations in biotechnology, has had two important consequences. The boundary between the pursuit of scientific discoveries and of opportunities of commercialization has blurred. Relatedly, the pursuit of scientific prestige in biosciences, and the seeking of monetary resources and returns in the biotechnology industry have become interdependent. In *Homo Academicus*, Bourdieu measures scientific capital in terms of research positions and scientific prestige, and assumes that more scientific capital (in the possession of a professor or in one area of the academic field) is an indicator for the pursuit of pure science or knowledge for knowledge's sake). Yet, in American research universities, scientific capital, defined as scientific authority and reknown, appears in multiple forms, some of which are highly dependent on funding, and increasingly convertible to monetary gain.

[46] In fact, he observes that it is homologous to the opposition in the field of power and proposes that its origin lies in the division of the labor of domination.
[47] See Wacquant (1989) for Bourdieu's thoughts on the American academic field.
[48] According to an administrator at UC Berkeley, departments are expected to be ranked in the top five among their competitors (Busch et al. 2004, p. 175).

Data and Methods

The data for the following analysis comes from public records of the EBI controversy, observations at public meetings and interviews with faculty members. While large-scale surveys have their advantages, focusing on this particular real-life conflict also yields important benefits. First, over the duration of the EBI controversy, faculty members could be observed in action: they offered statements on issues that they believed were important at public meetings; they wrote letters to the administration; they initiated and/or signed petitions; and they took action in the bodies of the Faculty Senate. Second, the EBI controversy occurred soon after two other controversies on relations with industry at the University of California, Berkeley: the Novartis Controversy (1998-2003) and the Tobacco Funding Debate (2002-2007). As the clashing faculty drew on statements made in these preceding cases, the EBI case offered a wide lens on the discursive terrain on which the conflict was fought.

It may be argued that the EBI controversy reveals more about the peculiarities of UC Berkeley, particularly its position as a node of protest over the decades. Yet, the following analysis is not predicated on the assumption that UC Berkeley is a microcosm of the larger field. Rather, I focus on the oppositions that the controversy revealed, and aim to link these oppositions to the structural trends affecting the field a as a whole. From another perspective, UC Berkeley is arguably the best choice for thinking about the substantive problems raised in the faculty conflict on the EBI. It is one of the top public research universities that have been in competition with the well-endowed elite institutions like Harvard, Yale and Princeton. It has also been under major budgetary difficulties due to declines in state funding. UC's relations to the Manhattan Project and the growing biotechnology industry in the Bay Area have attracted attention in debates about what it means for public research universities to serve the public good.

The Modern University and the Idea of Academic Freedom

Early Nineteenth Century Germany

By most accounts, the University of Berlin, founded in 1810, was the first modern university (Hofstadter and Metzger 1955; Howard 2006). In the preceding century, European universities modeled after the University of Paris had gone into decline. Over the nineteenth century, the University of Berlin became the model across Germany and the Western world. The rise of the University of Berlin involved two interrelated reversals: in sponsorship/mission and in internal hierarchy. Prior to the late eighteenth century, German universities had a religious mission. Notwithstanding some proto-modern characteristics, "all faculty members were members of religious orders or had received some form of clerical training," and theology had the reigning position among faculties (Howard 2006, p. 50). Medieval understandings of knowledge emphasized Aristotelian logic as the single correct method of thinking on all subjects and assumed that the domain of knowledge was finite (Howard 2006, p. 55).

Beginning in the eighteenth century, members of the philosophical faculties began to challenge the supremacy of theology with a new conception of *wissenschaft*. Universities of Halle (1694) and Gottingen (1737) has showed some signs of reform, but it was during the Prussian Reform Era (1807-1815) following Napoleon's defeat of Prussia that the great transformation of the German university came about. The philosophers' challenge to theology was supported by the new Prussian State, a progressive and paternalistic bureaucracy that took on the task of educating its citizens to serve the emergent nation. With state support, the philosophical faculty, which formerly had the task of preparing students for the higher faculties

of medicine, law and theology, gained the upper hand in the hierarchy and the definition of the university.

One of the foundational statements of the modern idea of academic freedom was offered in this context by Kant in a set of essays, titled, interestingly for our purposes, *The Conflict of the Faculties* ([1798] 1979).[49] In a work published a few years prior, *Religion within the Bounds of Bare Reason* ([1793] 2009), Kant had put forth his philosophy of religion, advancing the criterion of reason to evaluate the truth of religious doctrine. This claim to authority over biblical teachings was met with theologians' rebuke and government censorship. In *The Conflict*, Kant defended the freedom of the philosophical faculty with an argument regarding the epistemic basis and the societal value of philosophical knowledge.

Kant's argument drew on a distinction between two types of compliance with a teaching: complying because one is ordered to versus complying through autonomous judgment, i.e. reason. Based on this distinction, Kant argued that university faculties had different relations to the government. He posited that the task of the higher faculties of theology, law and medicine was to train the officials (clergy, legal officials and doctors) who would guide the people towards securing their welfare. The government had entrusted this task to these faculties, and therefore "reserve[d] the right to sanction [their] teachings" (Kant ([1798] 1979, p. 27). In contrast, the task of the philosophical faculty was to test the truth of the teachings of the higher faculties. Hence, "the philosophical faculty… [had to] be conceived as free and subject only to laws given by reason, not by the government" (Kant [1798] 1979, p. 43). Further, *The Conflict* argued that this task of truth, and hence, the philosophical faculty, were the defining core of the university.[50]

Hence, in early nineteenth century Germany, academic freedom was mainly freedom from religious orthodoxy. It was achieved thanks to increased state control over university affairs, including faculty appointments. This configuration worked mainly because the state supported the ideal of scientific reason and scholarship that challenged the religious orthodoxy. Yet, faculty with politically subversive views was rarely tolerated. Yet, the period over which the original conception of *wissenschaft* prevailed in German in universities was relatively short. Already in the 1830s, with the rise of positivism, and the demands of industrialization and technology, philosophy began to be overshadowed by the natural and empirical sciences.

[49] For a brief overview of the precursors of the term, see Sutton (1953).

[50] Kant sought to reconcile the tension between the philosophical pursuit of truth and the imperatives of government in two ways. First, he emphasized that the goal of the philosophical faculty is not incite people to rebel, but to help lead the higher faculties and the officials they train (clergy, legal officials and doctors) onto the way of truth (Kant [1798]1979, p. 47). Hence, the philosophical faculty was to make its objections to the teachings of the higher faculties only in scholarly communication, and not in public. Relatedly, the nineteenth century German concept of academic freedom applied mainly within the university; outside the university and in public affairs, professors, like other state employees, were required to be loyal to the state. Second, Kant expected that the government and the higher faculties would recognize that they also had an interest in truth; that, in the long run, governing by truth would strengthen their influence. This prospect that truth might serve the government did not bother him as he believed government by truth to be inherently good. See Derrida ([1984]1992) for a critical review of Kant's *Conflict of Faculties*.

Early Twentieth Century United States

From the founding of Harvard (1636) until the mid-nineteenth century, American colleges were mostly denominational and focused on teaching (Rudolph 1962; Hofstadter and Metzger 1955; Veysey 1965). Circa 1865, the word "university" was used but still did not have a clear definition. As the American university emerged in the following three decades of reform, three specific goals were advanced: practical public service, pure science and diffusion of liberal culture (Veysey 1965, p. 11). In 1876, the founding of the Johns Hopkins University after the German model by Americans who had studied in Germany marked the beginning of the research university in the United States. Disciplinary societies formed both in the natural and the social sciences. The American Association of Universities was founded in 1900 limiting its membership to institutions with graduate schools (Veysey 1965, p. 176; Geiger 1986).

The initial formulation of academic freedom in the United States was put forth in response to the conflict that emerged between the newly rising social and political scientists interested in social reform, and the representatives of big business in trustee boards or foundations (Hofstadter and Metzger 1955, p. 418; Veysey 1965, p. 73).[51] In 1900, in the most famous episode of this conflict, Edward Ross, an economist at Stanford University defending economic policies against big business interests, was forced to resign at the request of Mrs. Stanford. In 1913, members from American Economic Association, American Sociological Society and American Political Science Association convened a committee to look into similar cases. In 1914, eighteen full professors of Johns Hopkins University made a call for a national association. In 1915, Committee A of the newly founded AAUP (1915) published its much-cited document, "1915 Declaration of Principles on Academic Freedom and Academic Tenure."

In the struggle against university presidents and trustees, the AAUP that the latter were obliged to govern the university in the public interest. This meant that they had to allow scholars "to impart the results of their own and colleagues' investigations and reflection … without fear or favor" (AAUP 1915, p. 294). Only then could the university fulfill its functions of advancing knowledge, providing instruction and offering disinterested expertise. This call for limiting the authority of the university administration came with a commitment to professional self-regulation. Professors were obliged to conduct academic work according to "the requirements of the scientific spirit and method" (AAUP 1915, p. 298). Whether they met these requirements or not was to be judged solely by other academics.

The "Declaration" posited that academic freedom has three components: freedom of inquiry and research, freedom of teaching within the university or college, and freedom of extramural utterance and action.[52] Then, remarkably from the point of view of contemporary debates on academic freedom in research, the AAUP "Declaration" (1915, p. 292) argued that the first of these could just be disregarded. Focusing on the freedom of teaching and freedom of extramural expression and action, the "Declaration" argued that these two were "closely related." There was apparently some division among the authors on the question of defending the freedom of extramural expression and action (Hofstadter and Metzger 1955, p. 411; Metzger 1988, p. 6). Some argued, along the German model, that only professional utterances and actions in one's area of expertise should be protected by academic freedom. Others did not favor such a

[51] One precedent formulation of academic freedom was put forth by Thomas Jefferson, who led the founding of the University of Virginia in 1825. Jefferson's conception was along the lines of the German formulation: he advanced the ideal of freedom from clergy, but insisted on the universal value of his own political position (Hofstadter and Metzger 1955). In 1860s-70s, the spread of Darwinian ideas caused some crises, but did not lead to organizing around academic freedom.

limitation. Hence, the final report tried to walk a fine line: it argued that limits on scholars' extramural speech and participation in politics would not be desirable, but that this did not mean that there should not be any limits whatsoever. It insisted, however, that any disciplinary action concerning these issues should be the prerogative of committees of scholars.

After these definitions, the "Declaration" offered some "practical proposals" that focused on how academic institutions should handle faculty appointments and dismissals. Specifically, it argued that decisions to reappoint or dismiss professors should be made in consultation with faculty boards, and stipulated the institution of permanent tenure for "positions above the grade of instructor after ten years" (AAUP 1915, p. 300). In short, AAUP's proposal for defending academic freedom consisted of a call for faculty involvement in appointments and dismissals, but did not invoke any wider notion of faculty governance (Metzger 1988, p. 7)

Cold War Era

Over the twentieth century, one of the key changes in American universities has been the expansion of research in the practice of the academic profession and in the operation of the university (Galison and Hevly 1992; Geiger 1986, 1993, 2004; Graham and Diamond 1997; Leslie 1994, Calhoun 2009b). The leading role of universities in research over independent research institutes was already established circa 1900 (Geiger 1986, p. 5). From then until the end of World War II, academic research was supported variably by foundations and industry. After World War II, the federal government became the major funder of academic science and triggered an exponential growth of the university research system. Federal funding expenditures were directed mainly to areas of national concern (namely, defense, scientific and technological progress and health) (Kerr [1963] 2001, p. 41).

While the growth of federally-funded research was transforming American universities[53], much of the public and scholarly debate on academic freedom focused on scholars' freedom of extramural expression (Gardner 1967; Schrecker 1986, 2010). In response to McCarthyism and the loyalty oaths, scholars and activists turned to the First Amendment right to freedom of expression, and led the development of a constitutional definition of academic freedom (Metzger 1988). Before this period, the term "academic freedom" had appeared in judicial literature only once in a rather vague way (Metzger 1988, p. 10). In 1952, a group of public schoolteachers in New York began fight against a state law (Fienberg Law) that made members of certain organizations listed by the Board of Regents ineligible for employment in public schools. They did not win this case, but Feinberg Law was eventually (in 1967) declared unconstitutional. The Loyalty Oath Controversy (1949-1952) and the Free Speech Movement (1964-65) that took place at the University of California were important episodes in the development of the constitutional definition of academic freedom (Gardner 1967; Schrecker 1986; Cohen and Zelnik 2002).

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[52] In contrast to the German tradition, the AAUP document focused only on the freedom of the teacher (ignored the freedom of the student).

[53] Interestingly from the point of view of current debates, universities met the growth of federal funding for research with some wariness (Lowen 1997; Geiger 1993). In a detailed case study of Stanford, Lowen (1997) recounts how Stanford administrators were initially concerned that accepting government funds would decrease their autonomy, but how, in seeking elite status for the university, had to discard such qualms.

Recent Debates

In recent years, a number of scholars have observed that academic freedom has come to be discussed increasingly as an individual right to free expression, linking this trend to the legacy of the Cold War era defense of academic freedom under the First Amendment (Metzger 1988; Haskell 1996; Post 2002, 2006; Butler 2006; Doumani 2006; Frydl 2006). As a remedy, Post (2006, p. 64) argues for a return to the professional definition of academic freedom, (i) which justifies scholarly freedoms on the grounds that scholars will be subject to professional regulation and the knowledge that they thereby produce will serve the public good, and (ii) cannot serve to protect freedom of extramural speech and action. Finkin and Post (2009) similarly argues the "Declaration" still offers the best formulation of academic freedom in the way it describes how academic freedom subject to scholarly norms serve the public.

Others argue that the classical academic freedom doctrine has become "less relevant" (Hollinger 2000; Butler 2006; Calhoun 2009b). For example, Butler (2006) objects to Post's assumptions both (i) of a clear demarcation between freedom in inquiry and teaching, and freedom of extramural expression; and (ii) of the unquestionable legitimacy of professional/disciplinary norms. She argues that professional/disciplinary norms are always in flux, and are at times invoked to limit substantively justifiable speech and action**.** Scott (1996) also discusses the role of and the tensions created by professional/disciplinary constraints on scholarly work. Others discuss the impact of skepticism regarding the foundations of knowledge and objectivity on the defense of academic freedom (Menand 1996; Haskell 1996; Rorty 1996).

In short, much of the recent discussions have focused on constitutional versus and professional definitions of academic freedom at the expense of addressing the changing structural conditions of knowledge production. Less frequently, observers have noted the need to understand the structural context. For example, Butler (2006, p. 107) observes that "if academic freedom is … a prerogative that emerges in a specific context, then [it is necesssary] to supply a full and adequate description of the institutional context." Similarly, Calhoun (2009b, p. 576) posits that "academic freedom requires an institutional location." In the analysis that follows, I seek to address this gap by theorizing the institutional context of knowledge production as a field so as to grasp the divisions within the university, and how the balance of power in the academic field shapes the discursive and institutional outcomes regarding academic freedom.

Case Study: Faculty Conflict on the EBI

On February 1, 2007, the CEO of BP America, Robert Malone, announced BP's decision to partner with UCB to found the Energy Biosciences Institute. In the context of increasing concerns about global warming and oil dependency, UCB Chancellor Robert Birgeneau presented the proposed research as "our generation's moonshot."[54] Advocates presented biofuels as a promising energy source and argued that partnering with industry was crucial to move the results of scientific research into use as quickly as possible. Critics, on the other hand, drew attention to the environmental and socio-economic risks associated with biofuels. They also criticized the university administration for rushing the agreement without adequate transparency and debate. In particular, the university's commitment to creating faculty positions related to the institute's agenda, and renting campus space for proprietary research by BP scientists attracted scrutiny.

[54] Energy Biosciences Institute Press Conference, 1 February 2007, http://berkeley.edu/news/media/releases/2007/02/01_EBI_press.shtml (Accessed April 19, 2007)

Two Perspectives on Academic Freedom

Soon after the press conference, faculty members mainly from the humanities and the social sciences began to voice concerns about the making and the implications of the agreement. In response, the leadership of the Berkeley Division of the Academic Senate organized a Faculty Forum (March 8th, 2007) to discuss the proposed partnership.[55] The Forum began with presentations by six panelists followed by brief remarks by UCB Chancellor Robert Birgeneau and LBNL Director Steve Chu, and a question-and-answer period.

Among the panelists, Robert Reich (Public Policy) was invited to specifically discuss academic freedom. Reich began by stating that academic freedom issues raised by corporate contracts fell in five areas. The first concerned "the academic freedom of researchers to contract with whomever they wish for funding" (Robert Reich, Forum Transcript; emphasis mine). In discussing this issue, Reich referred to the then ongoing discussions on the proposal to ban tobacco industry funding, and noted that the UC's Assembly of the Academic Senate had advised the president of the university that "no unit of the university [should have] the authority to prevent a faculty member from accepting external research funding based solely on the source of funds."[56] Yet, Reich continued, "individual researchers are still members of an academic community, and there are external consequences, community consequences to decisions about with whom and under what conditions research is going to be undertaken." Hence, he proposed there are four principles that the university should defend: free flow of information, adequate attention to society's problems, autonomy in personnel policies, and protection of the university's image and goodwill. Reich stated:

> These potential dangers to academic freedom do not and should not rule out corporate sponsored research, but they should inform any process of negotiation, the terms and conditions of any research contract and the university's own internal rules and guidelines for maintaining **the independence of the academy**. (Robert Reich, Forum Transcript; emphasis mine)

Thus, Reich's presentation touched on two principles, "academic freedom of individual researchers" and "independence of the academy," and suggested that these could be found in tension. When Chancellor Birgeneau took the floor, following the panelists, he responded to Reich's discussion by focusing only on the first principle:

> I do want to say something quite definitively and a little more definite than Bob Reich did in his multi-dimensional five point presentation. … I want to make my own position absolutely clear, which is that to me personally, who's worked for all of my career on academic freedom issues—I won't go through the list with you—the idea that any person in our community would try to inhibit or prevent Jay Keasling and his post docs and graduate students, or Dan Kammen or Steve Wong at the University of Illinois, to prevent them from doing their research because they don't like the source of their funding, I consider that abhorrent and to represent a violation of the most basic principles of academic freedom. And so **this is about academic freedom, but it's about the freedom of our faculty to pursue the research that they want to pursue**. (Forum Transcript; emphasis mine)

[55] http://academic-senate.berkeley.edu/meetings/forum

[56] See below for additional detail on the tobacco funding debate.

Reich's and Birgeneau's positions on how to address academic freedom issues raised by the UCB-BP partnership were succinct expressions of the arguments the two faculty groups advanced over the duration of the conflict. Like Reich, the critics emphasized that the EBI agreement was an institutional agreement and insisted on the need for meaningful faculty senate oversight for safeguarding the substantive principles of a public university. In contrast, the advocates followed Chancellor Birgeneau and described critics' efforts as attempts to block the partnership and an attack on the academic freedom.

Special Meeting the Academic Senate

The critics found the Faculty Forum to be disappointing and frustrating. Hence, they took action, invoking the principle of shared governance to demand faculty senate oversight in the making and execution of the partnership agreement with BP. One group initiated an online petition ("Faculty Shared Governance Petition", March 12th, 2007) addressed to the chair of the Berkeley Division of the Academic Senate, William J. Drummond. This petition was signed by 131 faculty members mainly from the humanities and the social sciences, and called for convening "a blue ribbon Ad Hoc committee to review those aspects of the partnership that impinge on the Senate's mandate."[57] Another group of seventeen faculty members called for a special meeting of the Academic Senate and demanded a comprehensive review of the EBI agreement before its signing.[58]

The concerns, criticisms and actions of the critics were met with varying degrees of disagreement and anger in the ranks of the advocates, who viewed the critics' motions as an attempt to block the partnership. In response to the call for a special meeting of the Academic Senate, the advocates put up their own petition and mobilized to draft a substitute resolution.[59] Their resolution argued that existing policies were sufficient to protect the standards and integrity of research, and that the critics' motion constituted an attack on academic freedom.

The two sides came to a clash at the special meeting of the Academic Senate held on April 19th, 2007. At the special meeting of the Academic Senate, Anne Wagner (Art History), who presented the critics' resolution, asserted that multi-national corporations and the university have different goals, and that their motion aimed to initiate a discussion about how to balance those goals.

> [T]his is **a debate about the balance that ought to be struck** between those massive organizations whose goal is profit, and the university, whose goals and responsibilities are different. It is about **the difference between** the university, or some of its members, entering into **productive dialogue** with this giant oil company or that network of genetic engineering firms, and the university, or more and more of it, becoming the corporation's **paid servant**, with more and more of the outcomes of hired research never entering the public realm, and less and less of the actual detail of university/industry agreement being

[57] http://facultysharedgovernance.org/index.php (Last accessed March 8, 2008). See Appendix B for the disciplinary affiliations of the signees to the Faculty Governance Petition.
[58] http://academic-senate.berkeley.edu/sites/default/files/committees/forum/meetings/div_specialmtg_0407.pdf See Appendix C for the disciplinary affiliations of the signees to the call for the Special Meeting.
[59] This petition was mentioned at the special meeting of the Faculty Senate, but I have not been able to locate the signee list.

> available for the university's normal oversight bodies to inspect and debate. (Special meeting transcript; emphasis mine)

In contrast, the advocates followed Chancellor Birgeneau and argued that the critical faculty (and not the industrial partner) threatened academic freedom. Randy Sheckman (Molecular and Cell Biology), who introduced the advocates' resolution at the special meeting of the Academic Senate, likened the critics' demand for senate review of the EBI contract to the opposition to stem-cell research:

> Make no mistake. The purpose of this motion is not to render transparent the procedures used to vet the BP Biofuels research program. Rather, it is **to stifle corporate sponsorship of research on the Berkeley campus by imposing a fail-safe bureaucratic blockade**. This is in spirit and tactic the same strategy that the opponents of stem-cell research in California have used to stall the implementation of Prop 71. Now two-and-a-half years on from its overwhelming approval by the voters of the state, our research and teaching enterprise is enriched by private and corporate support without which much of the scholarly and practical work done in the colleges of Engineering, Chemistry and Business school to cite several examples would not be possible. (Special meeting transcript; emphasis mine)

John Taylor (Plant and Microbial Biology) followed Randy Sheckman, recounting how the scientists perceived the HSSF motions as a threat to free inquiry:

> We first became aware of the action against, or worries, concerns about the BP agreement from the website and petition. And then, we learned a special meeting had been called. And so we drafted a resolution and developed our own website in hopes of rallying support among those in the scientists who felt … **completely threatened by the resolution and our ability to have free inquiry in our research.** (Special meeting transcript; emphasis mine)

In their resolutions, the two sides drew on two earlier controversies that took place at the University of California: the Novartis Controversy (1998-2003) [60] and the Tobacco Funding Debate (2002-2007)[61]. These earlier controversies were also centered on governing the university's relations with industry, but the relations under scrutiny were of different sorts. The Novartis agreement was signed in November 1998 between the Novartis Agricultural Discovery Institute (NADI) and UC Regents, and provided a total of $25 million to UC Berkeley's Department of Plant and Microbial Biology (PMB). It stood out among university-industry partnerships in that it involved not one or several faculty members, but the Department of PMB as a whole, and became a benchmark case in debates on university-industry research relations (Washburn 2000, 2005; Kerr 2002; Gumport 2002; Eaton 2004 Calhoun 2006b).[62] In response to concerns and criticisms, the UC administration sponsored two reviews of this partnership: one internal (Price and Goldman 2002), and one external (Busch et al. 2004). Notably, both reviews

[60] See Appendix D for a more detailed overview of the Novartis Controversy.
[61] See Appendix E for a more detailed overview of the Tobacco Funding Debate.
[62] See also "Faculty Statement of Principles and Best Practices Concerning Strategic Corporate Alliances" (2005) Available at http://theuniversityfaculty.cornell.edu/forums/sca.html

reported that PMB faculty found the Novartis agreement "as enhancing their academic freedom rather than restricting it" (Busch et al. 2004, p. 117; Price and Goldman 2002). However, the external review parted with the internal review and looked beyond the department. Describing how the College of Natural Resources at Berkeley had long been a site of struggle between researchers committed to agricultural biotechnology and those committed to research on agro-ecological sustainability, Busch et al. (2004) evaluated how the administration's overall commitment to certain areas of research impacts academic freedom:

> For a number of reasons, the UC system and UCB administration have committed themselves to certain areas of research more than other areas, and to greater relations with industry. While such commitments potentially increase the academic freedom of some, they may reduce the academic freedom of others. For many faculty, students and activists, a decreasing emphasis on agroecological sustainability is a notable **threat to the diversity of views** in academic research and teaching within the UC system. (Busch et al. 2004, p. 128; emphasis mine)

In other words, it highlighted how the availability of corporate funding for certain research areas and the administration's tendency to valorize entrepreneurialism affect the academic freedom of scholars working in areas that do not attract corporate funding as readily.

Hence, HSSF's first resolution cited the external review of the Novartis agreement regarding large-scale partnerships to demand (i) that the Chancellor take into account the recommendations of the external review and (ii) that the contract be made available [to] all members of the Senate:

> WHEREAS the **Michigan State review explicitly recommends** that the University "avoid industry agreements that involve complete academic units or large groups of researchers," and "encourage broad debate early in the process of developing new research agendas;" and furthermore warns that "There are indications that the overstatement of academic freedom mixed with interests in commercialization is simultaneously eroding the public trust in science and the genuine merits of scholarly autonomy," and WHEREAS the proposed BP contract establishing the Energy Biosciences Institute raises deep questions on all these grounds, be it therefore RESOLVED that the Academic Senate urges the Chancellor that **no contract with BP be entered into until a comprehensive review of its terms is executed, with that review taking full account of the recommendations of the Michigan State report**, and that **the renegotiated contract be made available in full for consideration by all members of the Senate**."[63] (emphasis mine)

In response, the SEF drew on the tobacco funding debate, which was initiated in 2002 by faculty groups at UCSF and at some other schools and centers that petitioned to ban tobacco funding in their respective units[64], and was still ongoing in 2007. The petitioners were focused

[63] http://academic-senate.berkeley.edu/sites/default/files/committees/forum/meetings/div_specialmtg_0407.pdf

[64] These were the UCLA School of Nursing, UCB School of Public Health, UCSD Cancer Center and UCSF Cancer Center. A few months prior, the Dean of Harvard School of Public Health (HSPH), had announced that the HSPH faculty had unanimously decided to ban tobacco funding. A number of other institutions, including Johns Hopkins School of Public Health, University of Arizona School of Public Health, Emory University School of Medicine,

on funding of individual or small groups of researchers on questions of the effects of tobacco on human health, and were concerned about the impact of funding on study design, data collection and interpretation of results.[65] In the first phase of the official debate, a report by the University Committee on Research Policy (UCORP)[66] had recognized that some research funding come with "requirements and restrictions that could compromise [UC's] research mission," and that such problematic clauses did appear in relation to tobacco funding, but had stipulated that such requirements and restrictions should be addressed via University policies on research funding. Hence, it had chosen to follow the AAUP position on the issue, and argued against bans based on the source of funding:

> Be it resolved that … individual faculty members be free to accept or refuse research support from any source, consistent with their individual judgment and conscience and with University policy. Therefore, no unit of the University should be directed (by faculty vote or administrative decision) to refuse to process, accept or administer a research award based on the source of the funds. (UCORP 2004)[67]

After some faculty review, the Assembly of the Academic Senate had adopted this position with some minor changes. The second phase had been initiated by the Board of Regents in response to the federal district court ruling (August 2006) that had found Phillip Morris "guilty of violating the Federal Rackeeter Influenced and Corrupt Organizations Act" (RE 89)[68]. The Regents, concerned about how receiving funds from a convicted corporation reflected on the university as a whole, had asked the Academic Senate to reconsider the issue. In response, the Assembly of the Academic Senate had reaffirmed its earlier position, but had also approved two additional resolutions expressing the critics' concerns.

> The Assembly declares its deep disapproval of funding arrangements in which an appearance of academic freedom belies an actual suppression of academic freedom. The Assembly asserts its conviction that past funding arrangements involving the tobacco industry have been shown to suppress academic freedom. (RE 89)

The third phase of the debate had begun in January 2007 with Regent Moores' motion to introduce restrictions on tobacco funding. In debating this resolution, the Board of Regents had noted the tension among the earlier resolutions passed by the Assembly of the Academic Senate, and had asked the latter to "provide a formal and unambiguous position" (RE 89:7) In the face opposition, Regent Moores had moved to propose a compromise resolution that did not ban tobacco funding but stipulated that (i) researchers must exercise utmost care; (ii) proposals to secure funding from tobacco industry "must be reviewed by a scientific review committee" and "must be approved by the chancellor"; (iii) the president will submit an annual report to the Regents. When the UC-BP partnership was announced in February 2007, this last resolution was

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Harvard Medical School, and Ohio State University School of Public Health, had also adopted similar policies. For a presentation of critics' concerns, see http://www.universityofcalifornia.edu/senate/committees/ucaf/afforum/bero.pdf

[65] For an overview of critics' concerns, see http://www.universityofcalifornia.edu/senate/committees/ucaf/afforum/bero.pdf

[66] UCORP is a committee of the System-Wide Academic Senate.

[67] Available at http://senate.universityofcalifornia.edu/underreview/ac.research.strings.rpt.0704.pdf

[68] Available at http://www.ucop.edu/research-policy-analysis-coordination/policies-guidance/tobacco-industry-funding/index.html.

pending before the Board of Regents. In September 2007, the Board of Regents approved this version and the formal debate came to an end.

The advocates' resolution at the special meeting of the Academic Senate on the EBI cited the resolutions passed by the Assembly of the Academic Senate in the second phase of the Tobacco Funding Debate. It argued that the attempt to institute senate oversight over UC's partnership with BP constituted a threat to academic freedom:

> WHEREAS the public mission of the University of California, Berkeley, mandates the highest standards of scholarly integrity and transparency, particularly in regard to its relationships with sources of private funding, and given the position of the Systemwide Academic Senate on this issue, be it RESOLVED, cognizant of the memorial passed and ratified by the Systemwide Academic Senate in 2006, that the Academic Senate instruct the Chair of the Berkeley Division to advise the Chancellor that **grave issues of academic freedom would be raised if the campus were to deviate from the principle that no unit of the University, whether by faculty vote or administrative decision, has the authority to prevent a faculty member from accepting external research funding based solely on the source of funds**. Policies such as the faculty code of conduct are already in place on our campus to uphold the highest standards and integrity of research. The Academic Senate believes that any intervention on the basis of assumptions about the moral or political standing of the donor is unwarranted.[69] (emphasis mine)

Critics of the EBI rejected this perception that their demand for senate oversight constituted an attack on academic freedom. Richard Walker (Geography) emphasized that their goal is not to block the agreement but attain adequate oversight:

> This idea of this vast schism being created by our motion is very misleading. In fact, it is the overreaction of the counter-petitioners as characterized in Professor Sheckman's comments that has created this vast wedge. The issue before us was not about stifling corporate sponsored research by a blockade of corporate sponsored research. That's completely inappropriate; has nothing to do with the original motion. It is a total red-herring to introduce the attack on stem-cell research by right-wing members of the California public. And this kind of overreaction has no place here. The problem with the substitute motion is it follows from that in **this wonderful-ringing declaration of academic freedom – which is fine, but which is also redundant because the state-wide Academic Senate has already passed this. But the issue before us, as stated by Professor Wagner, was about the terms under which this contract was negotiated and would be signed**. And getting an adequate review of that contract. And seeing that this whole process is open. and that the results in terms of the faculty code of conduct would be … open to the usual standards of academic inquiry carrying no baggage of patent law or other kinds of trade secrets, restrictions in that contract. (Richard Walker, Geography, Special meeting transcript; emphasis mine)

[69] http://academic-senate.berkeley.edu/sites/default/files/committees/forum/meetings/ebi_resolution.pdf

A faculty member from Department of City and Regional Planning sought to overcome the divide by arguing that academic freedom of individual faculty and collective governance need not be in conflict:

> I think that individual inquiry, as an interpretation of academic freedom, is completely consistent with collective governance as interpretation of academic freedom. I think that we need to get past this false dichotomy that we have to choose between either unfettered individual inquiry from any source with whatever restrictions there might be substantively upon the disclosure or the inquiry itself, [or] … the idea that we have to have some collective governance that necessarily involves restrictions upon individual inquiry. I don't think that's the choice we have here**. I think we actually have a set of procedural concerns about both the rights of individuals to be engaged in academic freedom in their inquiry, and the rights of the collective body of the faculty to govern the conditions under which a public university operates are also protected**. (Special meeting transcript; emphasis mine)

At the end of the meeting, thanks to the numerical majority of the advocates, the vote ratified advocates' resolution. The latter reaffirmed the principle of academic freedom of individual faculty to receive funding against collective or institutional interventions; and proposed a senate committee to take part in the EBI negotiations. A five-member advisory committee comprising chairs of relevant senate committees participated in the EBI negotiations. Yet, they did so under pledge of confidentiality until the contract was finalized and made public.

Two Cultures

During and after the EBI controversy, participants and observers frequently commented on the severity of the clash between the opposing factions with reference to Snow. For example, at a teach-in organized by the student group StopBP-Berkeley, Cori Hayden (Anthropology) expressed her dismay on how the polarization reproduced the two cultures model:

> The contract is in negotiation … But there has already been an incredibly destructive effect, which is that a real polarization and fragmentation of the faculty on the Berkeley campus in an absolutely demoralizing way such that certain folks in the sciences have been pitted and have pitted themselves against people in the humanities and the non-econometric social sciences and in the arts. I mean there has just been this repetition, this replication of a mythical "two cultures" model which does not actually map onto how people engage with each other and with research on this campus. That has actually been very painful to be a part of it and to witness. (Teach-in Transcript)

A systematic analysis shows that the faculty members who partook in the controversy held notably oppositional views and sensibilities on a number of questions such as the mission of the university, the meaning of public versus private funding, how funding impacts research, and the promise of science and technology. To begin with, the HSSF and the SEF were motivated by *very different visions of the university*. The HSSF's vision of the university highlighted the tensions between the university and the corporation. At the Faculty Forum, Tim Clark (Art History) stated this vision:

> I, like many other members of the Senate, have deep misgivings about the deal being struck between British Petroleum as I like to call it and the university. … It is really no answer to those of us with worries about the terms of cooperation in this case to tell us that "it happens all the time." Of course I know as a historian that the university as a form of life came into being and continues in being only through a constant negotiation of *its* interests against those of the dominant social and ideological powers in the world around it. … The conflict of interest isn't obscure. **The boardroom wants profits and products; the lab wants, or ought to want, the truth, however inconvenient that truth may prove to its real world sponsors. The tension between one imperative and the other ought to be explicit in any deal the university strikes.** (Tim Clark, Forum transcript; emphasis mine)

In contrast, the SEF's accounts of the university focused not on the tensions but on the potentials of engagements with industry. Shankar Sastry's (Electrical Engineering and Computer Science) panel presentation at the Faculty Forum offered a celebratory description of these engagements:

> I want to talk about the tradition in Berkeley of engagement with industrial research. I think this tradition was begun with the realization that **it's important to work with industry to maximize the impact of our work**. … We have a tradition of not just developing cool new technologies at Berkeley and throwing them over the wall expecting that they will transform society. … We need to engage the world of industry and societal stakeholders in solving, really, the pressing problems of the day in terms of clean energy and the environment. (Shankar Sastry, Forum transcript; emphasis mine)

In a related manner, the two sides held contrasting views on the possible *impact of public versus private funding for academic research***.** David Winickoff (Environmental Science, Policy and Management) highlighted that UCB was a public university, and asked how that might inform its relations with public versus private sources of funding:

> So I think we have to think about the roles that they play as officers of a university with a public mission. … So it's not just enough to say that they're rights-holders like lay people. In fact, when people play different kinds of public roles like, let's say, candidates for office, we place limitations on what kinds of funds people can receive because we as a society believe that there's a potential for skew, potential for some kind of abuse. Now, I only bring this up not to criticize the particularities of the agreement but just to underscore and to ask, **Is there a vision—can we begin to talk more about the vision of the mixture between private funding and public funding at Berkeley… Is there a threshold over which we might want to set? Could we talk about what a threshold might be? Maybe our threshold is different than Princeton because we *are* a public university and that might matter to us.** (Forum transcript: emphasis mine)

In contrast, Robert Tijan (Molecular and Cell Biology) underlined the scarcity of funding and argued that source of funding does not influence the research done:

> I'm a little bit surprised, perhaps shocked, to hear that rather than to be happy about the fact that there's finally some influx of funding coming into it, they seem to be rather

> upset about it. And I think that a little reflection on this might be useful. I mean, I come from a field in biology where 30 or 40 years ago—I've been on this campus now thirty years—where trying to scrape up a little bit of money was really difficult and all of the advances that have happened in molecular biology would not have occurred without some help from private, sometimes foundations, but a lot of the money does eventually come from pharmaceuticals. And none of my students, and I've trained probably a hundred-and-twenty over the last thirty years, would say that that money in any way influenced what they did. (Robert Tijan, Forum transcript; emphasis mine)

On the question of the impact of different sources of funding on research, Miguel Altieri (Environmental Science, Policy and Management) pointed out that the rise of corporate partnerships such as Novartis or EBI supports *the growth of only certain research areas.* In his view, the university administration by engaging in such institutional partnerships without sufficient faculty input, was in effect favoring some research fields over others.

> [M]y main point here is that with the seven FTEs—which they have authorized without consulting the Budget Committee plus the fifty scientists from BP that are going to be here with the same rights as professors, with access to students, and they're going to be skimming off two-hundred years of publicly-funded university—are going actually to distort the research agenda. **The fields that I represent and Professor Chapela represents, for example, on alternative agriculture, are going to die on this campus**. When I retire no more research is going to be done on agroecology, which actually is the alternative to climate change to industrial agriculture. (Forum transcript; emphasis mine)

In contrast, the SEF (and the administration) presented the whole process as one where a group of researchers had come together to solicit funding for the research they were interested in. To Altieri's concerns, Dan Rokhsar (Molecular and Cell Biology and Physics) responded as follows:

> I don't see why this is any different from any other research project that anybody puts together. This is basically a coalition of scientists and technologists on campus who have a joint vision for what they want to accomplish. … [I]n the end, it's a bunch of scientists and engineers who have a research agenda that they want to do and they've found some source of funding to accomplish it, and I think that's applaudable. **If people have other ideas that they want to study**, to looking at global climate change and other impacts, things that they think might be going wrong, **they should find those sources, and ideally they would be out there**—foundations and other sources of funding—to have those voices heard and have those research projects done because we all need to move forward. (Forum transcript; emphasis mine)

Of course, the two sides' contrasting reactions of the EBI and its research agenda were linked to their *views on the potentials of science and technology*. On this dimension, Ignacio Chapela (Environmental Science, Policy and Management) was the most outspoken critic:

> What would certainly come out of the BP-Berkeley facilities would be a large number of genetically altered, reproducing, LIVING organisms to be released in the public environment. … Genetically-modified (or "GMO") grasses, trees, algae, bacteria, viruses

> destined for intentional, large-scale release in the public environment. **These organisms do not represent science. If anything, they may represent our failure as scientists to assume the deep inadequacies of our understanding about living organisms and the ecology of our planet.** … Biofuels may be convenient because they shove the tragic aspects of our insatiable consumption to the invisible corners of the Third World, but they will not change the laws of thermodynamics… (Forum transcript; emphasis mine)

In contrast, Jay Keasling (Chemical Engineering) expressed great enthusiasm about research agenda:

> All along this process we intend to examine the human practices of energy biosciences, that is, looking at the environment, the health, the safety of doing this, reaching out to people in the community, people around the world and educating them. And we don't want to take just a U.S.-centric view. We want to look at this from the point of view of the entire world. We want to look at the public policy issues around this so that we are educating people who are in government about the possibilities of biofuels. … **I am extremely enthusiastic about being able to do this project**. … I really think this is a problem that we need to solve, and this is a very Berkeley-like problem. It's essentially a moon shot for all of us. And we were chosen because we have both the breadth and the depth here at Berkeley to accomplish a major project like this. The difference between this project and a moon-shot is that we can't afford to fail on this project. (Forum transcript; emphasis mine)

In *Homo Academicus,* Bourdieu ([1984]1988, p. 74) describes that the dominant and the subordinate poles of the French academic field are linked to two "profoundly differentiated lifestyles": while indices associated with the dominant pole (such as large family, right-wing voting) reveal "a taste for order," the other set (such as higher rate of divorce, left-wing opinions) evokes "a detachment" (Bourdieu ([1984]1988, p. 51). Similarly, the sensibilities of faculty members who partook in the EBI conflict were notably oppositional along several dimensions. While the critics took more idealist positions, the advocates took more pragmatic positions. While the critics pointed out structural factors shaping research, the advocates adopted an individualist idiom in describing how researchers decide on research agendas or find funding sources. Most strikingly, while the critics were generally pessimistic on questions of climate change, potentials of science and technology, and the future of the university, the advocates were mostly optimistic and celebratory. **It was the oppositional sensibilities with which professors inhabit their positions in the academic field that led to the severity of the clash around the EBI agreement.** Table 3 summarizes these views and sensibilities.

TABLE 3 Faculty Conflict on the EBI: Oppositional Stances and Sensibilities

Question	HSSF	SEF
framing of the debate	*need for oversight over the agreement* Wagner (History of Art) Langan (English)	*attack on academic freedom* Sheckman (MCB) Taylor (PMB)
definition of academic freedom	*institutional* Reich (Public Policy) Nader (Anthropology) Schwartz (Physics)	*individual* Birgeneau (Chancellor, Physics)
role of faculty governance	*need for faculty oversight* Walker (Geography) Briggs (Anthropology) Rabinow (Anthropology)	*it is the chancellor's job to negotiate* *existing regulations suffice* DuLong (Economics) Vogel (Law; good enough corporation)
public versus private funding	Winickoff (ESPM)	Tijan (MCB)
how funding impacts research	Altieri (ESPM)	Rokhsar (MCB)
vision of the university	Clark (History of Art)	Sastry (EECS)
views on the research agenda	*pessimistic* Chapela (EPSM) Walker (Geography)	*optimistic* Keasling (ChemE) Chu (LBNL, Physics)

Coda

After the EBI controversy, the Divisional Council (DIVCO) at Berkeley convened a task force to formulate principles to govern future university-industry partnerships. This task force, comprising five Senate members who had been involved in the EBI contract negotiations and three other members with relevant expertise, produced a report titled "Report by Senate Task Force on University-Industry Partnerships" (RSTF)[70] and presented it to the DIVCO in October 2008. DIVCO endorsed it and provided it to the Vice Chancellor for Research, Graham Fleming. The RSTF is important because it epitomizes the exact form the principle of shared governance takes in relation to university-industry research partnerships, and indicates what it can do to protect academic freedom. Yet, it is a document with tensions. It aims to define how the university should counter the demands and constraints that funding agencies may attempt to impose on the practice and products of research. At times, it tries to emphasize and apply principles that critics of university-industry partnerships advance. At other times, it tries to explain why certain compromises do not necessarily endanger the university's core commitments. For the most part, it tries to argue that existing university regulations are sufficient to defend these core values, but also touches upon how current policies fall short.

[70] http://academic-senate.berkeley.edu/sites/default/files/recommendations-reports/tf_uip_final_report.pdf

The preceding tensions are seen in RSTF's discussion of academic freedom as well. In principle, the report reiterates the definition of academic freedom advanced in the tobacco funding case and the special meeting of the Academic Senate. At the same time, it addresses the issue of safeguarding researchers' academic freedom vis-à-vis funders. Specifically, it posits that academic freedom comprises two principles: "freedom of publication and discussion, and informed consent by the researchers as a basis of research participation (and withdrawal from the collaboration without penalty)" (RSTF 2008, p. 17). Yet, the report's proposals for safeguarding these freedoms miss some of the key trends shaping scientific conduct; such as the informal pressures for secrecy, and the ways in which the availability of funding shapes research agendas (Nichols 2007).

Discussion

In the faculty conflict around the EBI, the critics and the advocates came from different regions of the academic field. Among the critics of the partnership, the most frequently represented disciplines were English, Anthropology, History of Art, and Environmental Science, Policy and Management (ESPM). Further, the critics included at least five faculty members from History, Sociology, Geography, and City Planning and Architecture, but one each from Philosophy, Political Science and Economics. The advocates included professors from Molecular and Cell Biology, Plant and Microbial Biology, Chemistry, Chemical Engineering, Material Science and Electrical Engineering. From an "academic cultures" framework, it is not very surprising to find that critics and advocates belonged to specific departments. Yet, a fuller understanding of the faculty conflict on the EBI requires attention to (i) the nature of knowledge sought in different disciplines; (ii) the conditions of possibility of research in different disciplines; and (iii) the changing conditions of knowledge production in American research universities.

Research in the entrepreneurial science and engineering disciplines requires major amounts of funding. This phenomenon can be traced back to the birth of big science in the period before World War II (Price 1963; Galison and Hevly 1992). More recently, the biotechnology revolution has led to a blurring of the distinction between basic and applied research, and the university-industry boundary in the biosciences (Kenney 1986; Krimsky, Ennis and Weissman 1991; Kleinman 2003). Academic and corporate labs compete with one another in this field, with researchers moving back and forth, or holding multiple positions concurrently. Relations with industry provide not only funding for research but also access to databases and patented technologies; and thus, have become crucial for conducting cutting-edge research. There is a convergence of work conditions and epistemic goals (Rabinow 1996; Powell and Owen-Smith 1998; Powell et al. 2005; Kleinman and Vallas 2001).

In contrast, in the humanities and the environmental policy fields, research requires relatively less substantial amounts of funding. In addition, the meaning of scientific is very different. In the humanities, the production and valuation of knowledge is informed by a sense of distance from utilitarian considerations. In the environmental science and policy fields, researchers are oriented to producing knowledge for policy-making, but work in paradigms of conservation and sustainability that take critical positions on agricultural biotechnology. In both cases, research goals are not so readily valued or funded by industry.

Given the interdependency of scientific and temporal capital in the American academic field, decreasing government funding for public research universities means that disciplines that can attract extramural research funding can expand more easily: scientific capital can turn into

academic capital and vice versa. For the entrepreneurial sciences and engineering, the growth of industry funding and the emphasis on the university's role in economic growth translate into scientific and temporal capital in the academic field. For the subordinate disciplines, the relative scarcity of extramural funding, and the relative devaluation of humanistic and conservationist paradigms lead to constrained opportunities for accruing scientific and temporal capital.

Combining the preceding considerations allows us to explain not only professors' stances, but also the content of their projects: academic freedom versus institutional autonomy. HSSF and SEF took diverging positions on the EBI because they need different kinds of freedoms to fulfill their epistemic goals. The SEF, by singling out the freedom to partner with any funding entity subject to university regulations, has advanced an *individualistic* interpretation of academic freedom. This is because SEF's epistemic goals have come to converge with the goals of industry partners. In fact, the availability of funding and access to databases, option of cooperation on technology transfer questions help achieve these researchers their goals. The HSSF, on the other hand, sought institutional autonomy to because autonomy from the economic field is crucial for their epistemological goals.

Conclusion

As my brief historical sketch of the development of the modern university and the struggles for "academic freedom" demonstrated, the idea of "academic freedom" has been defined and defended in historically-specific discursive regimes, by agents oriented to specific epistemic goals, and always on a structured terrain that informs the outcomes of these struggles. This paper focused on a recent, illustrative episode, where UC Berkeley faculty came to clash on how to defend academic freedom and university autonomy in relation to an impending research agreement with a major corporation. Through an in-depth case study, I have sought to explain the occurrence, the content, the severity and the outcome of the faculty conflict. I have argued for an interlinked analysis of debates on academic freedom, on divisions within the university, and how the university's relations with the state and the economy structure the opportunities for accumulating academic capital within the academic field. Further, I have shown how the dominant disciplines within the academic field tend to shape the discursive and institutional outcomes of intra-organizational struggles.

To conclude, I will highlight how struggles for academic freedom are linked to specific epistemic goals, and that the organizational and legal outcomes of such struggles give clues about the reigning view of valuable knowledge in specific historical conjunctures. A longer discussion of this historical relation would be beyond the limits of this paper, but a quick comparison of Kant's and SEF's arguments for academic freedom can be illuminating.

Kant's argument for freedom of the philosophical faculty was an attempt to raise the epistemic status of philosophical knowledge. It was based on two key Enlightenment premises: that autonomous judgment is possible, and that the exercise of such judgment leads to a superior form of knowledge (truth). Kant ([1798]1979, p. 25) posited that the higher faculties are called higher because they serve the government's interests. In his view, the philosophical faculty had the higher place and was the defining core of the university, because it pursued the interest of truth. This argument proved to be influential during the Prussian Reform Era (1807-1815). Wilhelm Humboldt, the Prussian Minister of Education who lead the founding of the University of Berlin, drew on Kant's ideas to argue that universities can fulfill their purpose only when they are oriented to the pure idea of science and scholarship, and that "their dominant principles should must be freedom and the absence of distraction" (Humboldt [1809]1976:243). With the

Prussian state moving to the side of the philosophical faculty against theology, the former gained the upper hand in the definition and the hierarchy of the modern university (Howard 2006). Yet, the dominance of philosophical methods in German universities did not last too long. Already 1830s, the growth of natural and empirical sciences and the demands of industrialization and technology began to change the balance of forces among faculties.

In contrast, the prevalence of the SEF's definition of academic freedom (as the freedom to partner with any funding entity subject to university regulations) is linked with the balance of power in the academic field, and expresses the valorization of a utilitarian view of knowledge. Since the Cold War, research in science and engineering disciplines has been defined as a public good because it leads to technological applications. The Cold War growth of basic science has at times been interpreted as the growth of the pursuit of knowledge for knowledge's sake. Yet, the federal government's valuation of basic science as a public good was a far cry from Kant's valuation of truth that can emerge only in the absence of considerations of utility and order. Lowen (2005, p. 442) also notes that "'basic' science should not be confused with 'pure' science, or the search for knowledge for its own sake." Since the late 1970s, the biotechnology revolution and government-led initiatives, such as the Bayh-Dole Act (1980) allowing universities to patent and license federally funded inventions, have increased the links between fundamental research and commercial applications. In the resulting regime, techno-scientific work with commercial applications has become the dominant form of academic capital.

CHAPTER FIVE

UNIVERSITIES AND THE PUBLIC GOOD IN THE AGE OF CLIMATE CHANGE

On February 1st, 2007, Robert Birgeneau, then Chancellor of the University of California, Berkeley, opened a press conference stating that it was "a historic day on the Berkeley campus," and invited the chairman and president of British Petroleum America Inc., Robert Malone, to the podium.[71] Standing between California Governor Arnold Schwarzenegger and Chancellor Birgeneau, Robert Malone announced that British Petroleum (BP) was joining with the University of California, Berkeley (UCB) in a partnership that he believed would have an "immense positive impact" on the world. With $500 million of BP funding to be provided over ten years, the Energy Biosciences Institute (EBI) was to bring together researchers from BP, UCB, Lawrence Berkeley National Laboratory (LBNL) and the University of Illinois at Urbana Champaign (UIUC) to conduct research in the biosciences for the development of biofuels. The prominent functionaries who spoke after Birgeneau and Malone shared their excitement, but the news led to a major public controversy that lasted until the signing of the agreement in November 2007. While advocates argued that the founding of the EBI constituted a historic step on the road to a clean energy future, and insisted on the need university-industry partnerships in addressing pressing issues such as global warming, critics raised concerns about the agreement's impact on university governance and research, and questioned the promise of biofuels. Contending sides agreed that public universities had the responsibility to serve the public good, but were at odds on whether or how the EBI would fit with this responsibility.

Since the late 1970s, federal policies, developments in the biosciences and the economy, and the growing emphasis on the role of universities in spurring economic growth has led to an increase in university-industry research relations (UIRRs) (Croissant and Restivo 2001; Geiger 2004; Croissant and Smith-Doerr 2008; Mirowski and Sent 2008). Scholars have examined this trend under several headings with varying degrees of celebration and criticism: mode 1/mode 2 knowledge (Gibbons et al., 1994), "academic capitalism" (Slaughter and Rhoades 2004), "asymmetrical convergence" (Kleinman and Vallas 2001); "triple helix" of university-industry-government relations (Etzkowitz 2008), "science-mart" (Mirowski 2011) or "the market university" (Berman 2012). Yet, much of this work has focused on organizational or policy questions, examining UIRR's impact (i) on academic work conditions and practices, (ii) on innovation and technology transfer outcomes (Mowery et al. 2004; Kenney and Mowery 2014), or (iii) on functions traditionally associated with universities, such as the production of reliable expertise and open dissemination of knowledge (Krimsky 2003; Slaughter and Rhoades 2004; Washburn 2005). In this paper, I aim to foreground and analyze the role of UIRRs in the politics and governance of technoscience. For this, I draw on recent work in science and technology studies highlighting the blurring of the boundary between science and technology, the hybridization of fact and value especially in the discussion and governance of risks, and the constitutive role of technoscience in the socio-political order. Given the centrality of research universities in the American science regime, and the long-standing consensus on their responsibilities to serve the public good, I approach the contentions over the meaning and terms of the UCB-BP partnership as part of the broader struggles over the terms "public," "public good," and "good science." I ask: How do new forms of university-industry research

[71] http://www.berkeley.edu/news/media/releases/2007/02/01_ebi.shtml .

partnerships inform the prevailing definitions of these terms, and what are the consequences for the democratic management of technoscience?

The UCB-BP alliance leading to the founding of the EBI constitutes a fruitful case to pursue this agenda. As has been amply documented, Cold War American science regime was founded on a relative consensus, which stated that universities conducted basic science as a public good, and thereby legitimized expanding federal support for academic science. Since the late 1970s, various actors (state officials, technology transfer professionals, university administrators) have come to describe universities' real or expected contribution to innovation and economic growth as part of their public mission. The EBI, however, showed up as an outlier on the expanding map of university-industry research relations, and its meaning could not be settled right away. Not only was it the largest-to-date research agreement between a private corporation and a public university; but, more importantly, it embodied a number of novelties that informed the controversy around it. Briefly put, it (i) was to pursue an explicitly applied mission, developing biofuels, which happened to lie at the intersection of several interrelated technoscientific controversies; (ii) through a close collaboration among industry and university scientists under the same roof; and (ii) promised to address the socio-economic and environmental implications at the same time as it would be developing the technology. These attributes of the partnership created a multi-faceted (discursive, organizational and political) problem that required significant work to resolve or dissipate (depending on how one views to outcome).

In this paper, I reconstruct and analyze this controversy with a focus on how contenders fought over whether and how the EBI fit with the university's public mission. Beginning with the press conference, advocates argued that partnering with British Petroleum did not threaten but supported the university's public mission. They pointed out the urgency of the need to address global warming, and posited that working closely with industry gave them the opportunity to check if their ideas were implementable, and if so, the partner that could quickly implement them. The critics, however, were concerned about the impact of the partnership on the research agendas of university scientists, the underfunding of less business-friendly approaches to problems of climate, energy and ecology, and the dwindling of departments or laboratories oriented to latter approaches. When faced with criticism, advocates employed several strategies to minimize the impact of industry on research agendas: they argued that (i) university scientists were already interested in these lines of research; and (ii) that research proposals submitted to the institute would go through "peer review." In the last instance, at least one scientist stated that if other researchers believed other ideas to be more promising, they should go seek their own funding, ignoring the ways in which the availability of research funding is multiply structured.

Given that the EBI had an explicit mission, developing biofuels, part of the debate focused on the potentials and risks of biofuels. Advocates argued that biofuels was a promising route and that the institute's plans to research socio-economic and environmental implications would help address risks and adversities. Critics, on the other hand, were mostly opposed to the release of genetically modified organisms and the expansion of industrial forms of agriculture. They were not convinced that the impact research could address these paradigmatic issues, especially the potential adverse impact of a growing biofuels industry on communities and ecologies in the Third world. To these paradigmatic and normative objections, advocates responded with a "scientism," comprising a "wait-and-see" discourse and trust in the possibility of governing adverse consequences; in effect, disparaging the critics as unscientific or dogmatic.

As I recount below, these debates over the impact and mission of the proposed partnership took place concurrently with real contentions over university governance. Faculty critics (mainly from the humanities, social sciences and conservationist departments) attempted to employ the principle of shared governance to demand faculty senate oversight on the negotiation and the execution of the partnership in line with the principles of a "public" university. While they were targeting the university administration, faculty from science and engineering departments mobilized to defeat their proposition in the Faculty Senate, describing it as an attack on the academic freedom, defined in this instance as the freedom of faculty members to receive funding from any source (subject to university regulations).

Based on this analysis, I argue that the EBI case signals a new turn in the co-production of technoscience, universities and prevailing definitions of public good. I highlight how advocates descriptions of EBI's novelties (embrace of an applied mission, close engagement with industry scientists, research on socio-economic and environmental implications) pushed the boundaries of the former assumptions on how universities serve the public good, and explicate how their strategies (invoking peer review, scientism, academic freedom), along with their organizational and material advantages, shaped the outcome of specific encounters. A brief comparison with a similarly controversial partnership that UC Berkeley signed approximately ten years prior to the EBI, the five-year, $25 million agreement between UCB's Department of Plant and Microbial Biology, and Novartis Inc. (UCB-N, 1998-2003) supports my argument. In the UCB-N Controversy, the university administrators argued that it did not derail academic researchers' from basic science, and that the university did not wish to enter into debates on genetic modification. In contrast, the university scientists defending the EBI fully embraced the applied mission and entered the debate on biofuels. As a result, the EBI controversy co-produced a particular response to global warming and a particular vision of the university as in line with the public good. While the definition of the latter cannot be settled once and for all, it is clear that UIRRs constitute a key a terrain, albeit an uneven one, for ongoing contentions.

Politics of Technoscience and Research Universities

Scientific knowledge is increasingly constitutive of life (and death) on the planet. It leads to ever new technologies with variably distributed risks and benefits, and not enough certainty to resolve the contentions around these implications. It is indispensable in addressing the complex socio-economic and environmental problems; yet, it is increasingly produced and mobilized in connection with politico-economic interests. In what is described as simultaneously the knowledge and the risk society, the governance of science and technology is an increasingly contentious affair (Beck 1992; Jasanoff 2005, 2011; Nowotny et al. 2001; Pestre 2008).

Scholars in STS have produced an expanding body of work on the political struggles over the direction, production, use and implications of emerging technologies. Studies employing the idiom of co-production have highlighted the ways in which knowledge, social and natural order, and forms of governance are co-produced (Reardon 2001; Jasanoff 2004). Others have argued that the formerly taken-for-granted distinctions between science and technology, and between facts and values, fail to hold (Latour 1987; Jasanoff 2005; Pestre 2008). Following Winner (1980) and Nelkin (1978), some have further elaborated how contentions over technoscience decisions are contentions about the social order. Jasanoff (2006, p. 758) posits that technology has become "a space [of] bitter political battles over competing visions of the good and the authority to define it." According to Pestre (2008, p. 106), management of technoscience is contentious because decisions on technoscientific questions involve choices about the kinds of

society, environment (sustainability) or humanity (ethics) desired. Kinchy (2010, 2012) illustrates these points by showing how struggles over genetically engineered crops, for example, are political struggles over the shape of the global agrifood system.

Given the preceding, scholars have turned to examining the factors supporting or hindering the democratic management of technoscience, focusing on how social movements have engaged with scientific expertise (Yearley 1992; Epstein 1996; McCormick 2007, 2009; Kinchy 2010), on the programmatic efforts to increase public participation in the governance of technoscience (Elam and Bertilsson 2003; Kleinman 2000), as well as on the institutions that structure public participation. Some of this work has highlighted how the "culture of scientism" (Wynne 2006), or the scientization of politics (Kinchy 2010; Jasanoff 2011) hinders the incorporation of lay, non-scientific or normative perspectives into technoscientific decisions preferably early in the decision-making process. A number of scholars have gone further to argue that public participation efforts are part of a new form of governmentality that favors unengaged citizens over active stakeholders, and consensus of conflict (Lezaun and Soneryd 2007; Pestre 2008). Thorpe and Gregory (2010, p. 288), for example, argue that public engagement is necessary precisely because the technologies are contentious; and that it constitutes the necessary labor that produces the technology and the public, and the product and the market. Last but not least, recent work has pointed out that focusing on citizens' participation in technoscience through social movements or formal exercises cannot suffice because many influential decisions are made in other domains: markets, regulatory institutions and courts (Pestre 2008; Jasanoff 2006, 2009)

In this paper, I build on preceding analyses but turn to public research universities as a key site for the co-production of technoscience and definitions of the "public good." Like other politically useful terms, the notion of "the public good" has multiple referents and meanings rooted in different socio-historical contexts[72], but one striking aspect of the notion is that the term "good" is polysemous. It may refer to a *product*, as in "goods and services", or to a *state*, namely, one of well-being. In discussions around universities, commentators may state that universities *produce* public goods or *contribute to* the public good. Glenna et al.'s (2007) study of university administrators' views on how UIRRs contribute to the public good describes the shift from the Cold War to the current era using both phrasings. According to this account, the "Bayh-Dole" model posits that "public goods must be converted to private goods to contribute to the public good," but assumes that the production of public goods will continue as before (Glenna et al., 2007, p. 145). Winickoff (2013), similarly, reviews the consolidation of the imaginary of the university as an entrepreneur, which assumed a perfect fit between universities' pursuit of technology transfer and public goals, and moves on to examine how technology licensing controversies have challenged this assumption and brought the "public" back in.

In focusing on the debates on how universities contribute to the public good, I seek to simultaneously analyze the multiple ways in which participants use the term "public good," but also highlight how the idea of the university has been associated rightly or wrongly with the well-being of the society, and how it therefore becomes a terrain of struggle over what constitutes "good science."[73] As the EBI sought to address global warming, it was coupled with

[72] Douglas (1980) describes how the concept of "the public interest" came to replace the concept of "the common good" even though the latter denoted something more than an aggregate of private interests. See also Calhoun (2006) and Offe (2012).

[73] While Thompson (2013) defines "good science" in the somewhat particular case of stem cell research, I build on her emphasis on the importance of alternative political visions.

even grander claims, namely, to contribute to the well-being of humanity and the planet. How were these claims stated, challenged and defended? What visions of science, the university and the future were put forward? Which arguments held sway and why?

The Energy Biosciences Institute: "For a World that Runs on Cleaner Energy"?

In announcing the UCB-BP partnership, the chairman and president of BP America Inc., Robert Malone, spoke with great enthusiasm:

> Today, it gives me great pleasure to announce a **groundbreaking** partnership, that we believe will have **an immense positive impact on our world**. BP is on a journey to not only provide the energy needed today, but also to find, develop, produce and manufacture **clean, sustainable energy for the future**. … The EBI will perform groundbreaking research aimed at the production of new and cleaner energy, initially focusing on renewable biofuels for road transport. … This is indeed an exciting day for those of us who hold great hope for a world that runs on cleaner energy. (Robert Malone, President of BP- America, February 1, 2007, EBI Press Conference; emphases mine)

Speakers following Robert Malone shared his enthusiasm.[74] President of the University of California, Robert Dynes said it was "a good day for California, and a tremendous day for mother Earth." UCB Chancellor Robert Birgeneau described the partnership as "our generation's moonshot." California Governor Arnold Schwarzenegger mentioned California's Global Warming Solutions Act of 2006 (AB 32) setting goals for greenhouse gas emissions reduction by 2020[75], and his executive order of January 2007 establishing a Low-Carbon Fuel Standard[76]; and stated that this partnership was the perfect complement to those moves for making the state "the leader in the clean energy revolution."

The process leading to the founding of the EBI had been initiated in June 2006 by BP's invitation to five research universities to submit proposals for housing a research institute focusing on biofuels.[77] Responding to the call, Beth Burnside, then Vice Chancellor for Research at UC Berkeley, had led the effort to put together UCB's proposal to partner with Lawrence Berkeley National Laboratory (LBNL) and the University of Illinois at Urbana Champaign (UIUC) to house the institute. The proposals of the two UC campuses that were competing for the grant, UC Berkeley and UC San Diego, included a letter from Governor Schwarzenegger to Lord Browne, BP's Group Chief executive, pledging "$40 million to support construction of appropriate facilities for the EBI, if one of the UC campuses [were] selected" (EBI proposal, pp. 90-91).

At the EBI press conference, more than one speaker celebrated the coming together of public and private institutions in tackling global warming. LBNL Director Steven Chu, and California Senators Don Perata and Richard Ackerman noted that BP's decision showed the excellence of public universities and national laboratories. UCB Chancellor Birgeneau posited that the grant would help these institutions fulfill their public missions:

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[74] These included in order of appearance: UC President Robert Dynes, California Governor Arnold Schwarzenegger, Illinois Governor Rod Blagojevic, UCB Chancellor Robert Birgeneau, UI Vice Chancellor for Research Charles Sakowski, LBNL Director Steven Chu, BP Chief Scientist Steven Koonin, and California Senators Don Perata and Richard Ackerman.

[75] http://www.arb.ca.gov/cc/ab32/ab32.htm

[76] http://www.energy.ca.gov/low_carbon_fuel_standard/

[77] These included, in addition to UC Berkeley, Cambridge, MIT, Imperial College London, and UC San Diego.

> We are especially pleased that BP has chosen to partner with two preeminent and leading public and I underline, public universities together with the LBNL. Addressing the major problems that face our society is our public mission. This energy research effort will continue our legacy of excellence in research, teaching and innovation in service to our state, our nation and our world. (Robert Birgeneau, UCB Chancellor, February 1, 2007, EBI Press Conference)

Yet, not all those who heard about the partnership and its research agenda were as excited and optimistic. Soon after the press conference, faculty members, students and environmental and public interest organizations began to voice concerns about the partnership through public statements, teach-ins and protests. These concerns clustered around two nodes: (i) the impact of the EBI on university governance and public mission, and (ii) the risks and benefits of biofuels. A newly formed student group, StopBP-Berkeley, set up an elaborate website that played an important role in voicing of the criticisms.[78] The leadership of the Berkeley Division of the Academic Senate responded to the initial objections by organizing a Faculty Forum (March 8th, 2007). Yet, faculty members disappointed by this forum proceeded to further actions. One group initiated an online petition ("Faculty Shared Governance Petition", March 12th, 2007). Another group with members, mainly from the social sciences and the humanities, went further and called for a special meeting of the Berkeley Division of the Academic Senate. These efforts and events became key sites for clashing views on the partnership. In what follows, I will reconstruct this controversy under three inter-related headings: (i) contentions over university governance, (ii) debates on the direction of research, and (iii) debates on biofuels.

Contentions over University Governance

In the controversy that erupted after the press conference, critics expressed surprise at hearing about the proposed partnership so late in the process. When Berkeley faculty and students requested access to the proposal submitted to BP, the administration's response was mixed: the proposal was first opened to review in the Academic Senate Office and only subsequently revealed in press. In the meantime, it transpired that the UCB had committed to allocating seven full-time faculty positions (FTEs) in areas in line with the new institute's agenda (EBI Proposal, p. 87) without full consultation with the Budget Committee of the Berkeley Division of the Academic Senate.[79] UCB Chancellor's mention of Chris Somerville, who at the time had an appointment at Stanford University, as the director of the institute, also added fuel to concerns regarding personnel decisions. That the proposal discussed renting campus space for proprietary research to be conducted by BP (EBI proposal, p. 11) was also a major bone of contention.

Highlighting these issues, faculty critics charged that the university administration had moved forward without adequate transparency and discussion. Invoking the principle of shared governance,[80] they sought to mobilize the faculty senate to institute oversight in the making and execution of the partnership in line with the public nature and mission of the university. The

[78] http://stopbpberkeley.org

[79] At the Faculty Forum, Victoria Kahn, Professor of English, asked VCR Beth Burnside whether these appointments had been approved by the Budget Committee. Burnside acknowledged that "Budget Committee was not contacted before the proposal was sent in" but explained that it was consulted afterwards (Forum Transcript).

[80] See Douglass (1997) for an overview of shared governance at the University of California.

online "Faculty Shared Governance Petition" (March 12th, 2007), addressed to William J. Drummond, Chair of the Berkeley Division of the Academic Senate, was signed by 112 faculty members (mainly from the humanities and the social sciences) and called for convening of a blue ribbon Ad Hoc committee.[81] Another smaller group of 17 faculty members (mainly from the humanities and the social sciences) organized to call a special meeting of the Academic Senate. While the critics were attempting to mobilize the faculty senate to ensure some discussion and oversight on the impending partnership, this led to a mobilization of faculty members from sciences and engineering favoring the partnership. The latter group also put together a petition and a set of substitute resolutions for the special meeting of the Academic Senate. In other words, what began as a faculty movement vis-à-vis the university administration turned into a conflict among the faculty body.

At the meeting of the Berkeley Division of the Academic Senate held on April 19, 2007, Anne Wagner (Professor, Art History) presented critics' resolutions by arguing that corporations and universities had different goals:

> The resolutions we have brought to this Senate are not born of some naïve or quixotic conviction that a great public university should have no truck with the dominant social powers in the world around it – including the great multi-national corporations. Instead, this is **a debate about the balance that ought to be struck** between those massive organizations whose goal is profit, and the university, whose goals and responsibilities are different. It is about the difference between the university, or some of its members, entering into productive dialogue with this giant oil company or that network of genetic engineering firms, and the university, or more and more of it, becoming the corporation's paid servant, with more and more of the outcomes of hired research never entering the public realm, and less and less of the actual detail of university/industry agreement being available for the university's normal oversight bodies to inspect and debate. (Anne Wagner, Special Meeting Transcript; emphasis mine)

The critics' resolutions highlighted the public mission of the University of California, Berkeley, cited the external review of the Novartis agreement, and made two demands: that the Chancellor takes into account the recommendations of the external review of the Novartis agreement regardint large-scale partnerships, and that the contract is made available [to] all members of the Senate.[82]

The defenders of the partnership were concerned not about BP's influence on university governance but the potential burden faculty senate oversight could bring to the making the agreement. Randy Schekman (Professor, Molecular and Cell Biology), who introduced the defenders' resolutions, likened the critics to the opponents of state funding for stem cell research:

> Make no mistake. The purpose of this motion is not to render transparent the procedures used to vet the BP Biofuels research program. Rather, it is **to stifle corporate**

[81] http://facultysharedgovernance.org/index.php (Last accessed March 8, 2008).
Academic Senate Chair William Drummond responded to this petition with a letter providing a detailed overview of the administration's consultations with the Academic Senate on personnel issues. This letter, dated March 21, 2012, acknowledged that the "failure to consult the Budget Committee was a mistake," but rejected the accusation that shared governance [had] been "'egregiously' violated." It also reported that VCR Beth Burnside had invited chairs of four senate committees to participate in the contract negotiations with BP.
[82] http://academic-senate.berkeley.edu/sites/default/files/committees/forum/meetings/div_specialmtg_0407.pdf

> **sponsorship of research on the Berkeley campus by imposing a fail-safe bureaucratic blockade**. This is in spirit and tactic the same strategy that the opponents of stem-cell research in California have used to stall the implementation of Prop 71. Now two-and-a-half years on from its overwhelming approval by the voters of the state, our research and teaching enterprise is enriched by private and corporate support without which much of the scholarly and practical work done in the colleges of Engineering, Chemistry and Business school to cite several examples would not be possible. (Special meeting transcript; emphasis mine)

Their substitute resolutions, in turn, drew on the position of the Academic Assembly in the then ongoing tobacco funding debate, and described the critics' efforts as an attack on academic freedom:

> … [G]rave issues of academic freedom would be raised if the campus were to deviate from the principle that no unit of the University, whether by faculty vote or administrative decision, has the authority to prevent a faculty member from accepting external research funding based solely on the source of funds.[83]

In short, the critics viewed the proposed agreement as an "institutional" agreement and demanded meaningful faculty senate oversight and public discussion to ensure that its content was in line with the principles of a public university. For the advocates, the proposed partnership did not constitute a threat to university's public mission, and the existing regulations would suffice. While the two faculty groups employed "shared governance" and "academic freedom" against one another, the conflict was rooted in their opposite visions of the university, and contrasting views on how a public university can serve the public good. While one sought "independence," the other sought "engagement."

The outcome of this faculty conflict was formalized by the vote that occurred at the end of the special meeting of the Academic Senate. The advocates constituted the majority at the meeting and ratified Professor Schekman's resolutions. The latter reaffirmed the principle of academic freedom of individual faculty to receive funding against collective or institutional interventions; and proposed the convention of a senate committee to take part in the EBI negotiations under pledge of confidentiality. A five-member advisory committee comprising chairs of relevant senate committees participated in the EBI negotiations. Yet, they did so under pledge of confidentiality until the contract was finalized and made public.

Debates on the Direction of Research

Another set of debates occurred around the question of how the industry partner would impact the direction of university scientists' research, and what the implications would be. Advocates of the EBI were for the most part not troubled by how the partnership would shape university researchers' agenda. First, they asserted that the research mission proposed by BP fit well with university researchers' ongoing interest in addressing energy and climate issues. Beth Burnside (UCB Vice Chancellor for Research in 2007), one of the main architects of the partnership, made this argument in her presentation at the Faculty Forum convened to discuss the EBI:

[83] http://academic-senate.berkeley.edu/sites/default/files/committees/forum/meetings/ebi_resolution.pdf

> This proposal actually was preceded by a couple of years by an enormous amount of work aimed at the similar kinds of objectives. It was really stimulated in part by Steve Chu's challenge to LBNL senior scientists and to Berkeley faculty to really engage the problem of global warming and start looking at what can be possibly done about capturing the energy of sunlight to address problems of CO^2 emissions. So there was already quite a lot going on. There was the development of the Helios project [with] twenty-five or more faculty involved in this project. The kinds of objectives of the EBI were a part of the objectives of the Helios project. … So there was quite a lot of activity going in the direction of the kinds of objectives that BP announced when BP announced in June of 2006 that it was going to fund an energy bio-sciences institute to the tune of about 500 million dollars. (Beth Burnside, UCB VCR in 2007, Faculty Forum, March 8, 2007)

More notably, some advocates welcomed the impact of the industry partner, because it would help move research into use. Steven Chu (Director of LBNL in 2007) argued that in addressing climate change, it was important to be working with industry:

> The great motivation of this work—I can speak for myself [and] many of the scientists that want to work on this project—is really this issue of climate change. … Because we want to help mitigate climate change, you don't want to just produce academic papers. You actually want to get it out there. You want it to be developed and you want to get it deployed, and so the motivation is that you have to start working with companies at the get-go. (Steven Chu, LBNL Director in 2007, Faculty Forum, March 8, 2007)

As objections intensified, advocates also made an effort to minimize the influence of BP on the research agenda by emphasizing that projects to be funded would be selected through peer review. In a radio broadcast at the heat of the controversy, Chris Somerville (EBI Director) highlighted that the EBI's research priorities were set in conversation with a Department of Energy study in the field of bioenergy, and described the first round of the proposal review process:

> A series of goals emerged from a large study by DOE - about 85 scientists worked together for six months to develop a set of research priorities in the general area of bioenergy. So we have adopted those priorities and asked the faculty to propose what it is they would like to do to address those goals. So we received about 250 pre-proposals from the Berkeley, LBL and UI faculty. … We initiated, first of all, a process where the faculty gets to propose what they would like to do (what kind of research), and we composed a committee of about 15 faculty members from campus to review all these pre-proposals. And about 85 of them have been identified or selected for full proposal. So right now there is a process in place where some faculty is proposing in some detail research proposals. ... And then these proposals will be, some of them will be selected for funding based on the peer review. … [The faculty] will be supported in much the same way as an NSF grant, an NIH grant is implemented. (Chris Somerville, EBI Director, KALW, August 20, 2007)

Yet, the critics were not convinced with the "peer review" argument. Calling the radio show where Chris Somerville explained the peer review process, an LBNL researcher argued that the EBI's mission statement meant that the peer reviewers evaluated the proposals as to whether they fit with the institute's mission:

> I am a researcher at the LBNL who has participated in the proposal submission process that Dr. Somerville is talking about. And I think, what I have seen in the process is that the BP agenda is incorporated in the research priorities of the selection committee. And what happens is that the proposals that take a more community-based approach solving the biofuels problem, those are rejected out of hand by the research committee as not a research priority. And the solutions that are acceptable to the committee that all have their sort of voluntary accountability to BP are those which can lead directly to the IP that is going to lead to royalties for the university, that is going to lead to royalties for the researchers and that's gonna lead to products for BP. And what's happened is by taking the half billion dollars, you've now converted the research model that exists in my lab and at UCB from one of public service to one of creating intellectual property that can be used for increasing the revenues and profits of an oil company. (Researcher at LBNL, KALW, August 20, 2007)

More fundamentally, critics raised doubts as to whether the industry's approach to addressing the problems of global warming and energy supply, and decisions about what areas of research to prioritize were aligned with the public good. At the Faculty Forum, Robert Reich (Professor of Public Policy) elaborated this concern. Reich noted that departmental and disciplinary research agendas can be "affected by the mere knowledge of what research corporations are funding," and that researchers may "avoid examining questions that could upset potential corporate donors." He then went on to propose that "the fundamental question [may be] not just whether researchers feel free to follow their own interests as opposed to the interests of these sponsors, but whether the problems of society are being adequately addressed" (Faculty Forum, March 8, 2007). Relatedly, a number of critics drew attention to the initial and potential impact of the agreement on the creation of full-time faculty positions in various departments. In the first weeks after the partnership was announced, a group of faculty members had criticized the administration for bypassing the Budget Committee of the Berkeley Division of the Academic Senate in creating a number faculty positions related to EBI's research agenda. In his comments at the Faculty Forum, Miguel Altieri (Professor of Agroecology) referred to this episode and pointed out how faculty positions in research fields without similar industry support was dwindling:

> [My] main point here is that with the 7 FTEs—which they have authorized without consulting the Budget Committee plus the 50 scientists from BP that are going to be here with the same rights as professors, with access to students, and they're going to be skimming off two-hundred years of public funded university—are going actually to distort the research agenda. The fields that I represent and professor Chapela represent, for example, on alternative agriculture are going to die on this campus. When I retire no more research is going to be done on agroecology which actually is the alternative to climate change to industrial agriculture. (Miguel Altieri, Professor of Agroecology, Faculty Forum, March 8, 2007)

In response, advocates posited that the advance of one agenda does not need undercut another. At the Faculty Forum, Dan Rokhsar's (Molecular and Cell Biology, and Physics) argued that those who wished to see other projects pursued should go out and look for their own sponsors:

> I don't see why this is any different from any other research project that anybody puts together. This is basically a coalition of scientists and technologists on campus who have a joint vision for what they want to accomplish. And as Beth said, there was a call for proposals, BP had money that was available to achieve those same kinds of goals, and a large project was put forward that is larger than most people in this room have been associated with, but in the end, it's a bunch of scientists and engineers who have a research agenda that they want to do and they've found some source of funding to accomplish it, and I think that's applaudable. **If people have other ideas that they want to study, to looking at global climate change and other impacts, things that they think might be going wrong, they should find those sources, and ideally they would be out there**—foundations and other sources of funding—to have those voices heard and have those research projects done because we all need to move forward. (Dan Rokhsar, Faculty Forum, March 8, 2007, emphasis mine)

As in other situations of structural inequality, those inhabiting more resourceful positions in the struggle for research funding tended to present the terrain as an even one.

In sum, contenders clashed in their responses to the question that Reich raised (whether societal problems were being addressed). For the advocates of the EBI, the answer was strongly positive. In their view, the partnership with BP constituted a great opportunity for addressing the problems of energy and global warming because of the extent of funding and the opportunity to work with the industry giant to move research findings into use. For the critics, however, corporate influence on deciding which technologies to pursue did not necessarily or automatically align with the public interest.

Debates on Biofuels

The EBI was founded with an explicit mission: to conduct research in the biosciences for the development of biofuels – a technoscientific object lying at the intersection of multiple controversies (global warming, industrial agriculture, food security, genetic modification, ecology).[84] Hence, a major portion of the EBI debate focused on the risks and benefits of biofuels as a response to climate change. Initially, critics drew on the broader debate on biofuels to criticize EBI's mission. They argued that while burning ethanol may emit less CO^2 than gasoline, producing ethanol with current processes causes emissions that offset the savings.[85] They also drew attention to the impact of ethanol production on food prices and agricultural

[84] See Appendix G for an overview of biofuels.

[85] In a perceptive article, Iles (2010) describes the recent developments in the debate on the GHG emissions of existing biofuels. In 2008, researchers reported that large-scale biofuels production would cause major land-use change especially in developing countries, and that if these changes are taken into account, biofuels could not help reduce GHG emissions. In response, Biotechnology Industry Organization (BIO) argued that land-use changes cannot be accurately modeled and hence, should not be included in the life-cycle analyses ("the quantitative analysis of all physical and economic processes involved in the life cycle of producing a given unit of biofuel"(Hendrickson 2006 quoted in Iles 2010)). In other words, the debate on the GHG emissions of biofuels turned into a debate on how to conduct LCAs and what can reliably be included in them

practices around the globe. In response, the architects and advocates of the EBI highlighted that the institute recognized the criticisms against existing biofuels (ethanol and sugar cane) and would be working to develop *second-generation* or *cellulosic biofuels*, where the idea is to use non-food crops (and overcome the food-fuel debate), and to find energy-efficient ways to access and process the more energy-dense (i.e. cellulosic) parts of these crops. Yet, the critics argued that a cellulosic biofuels industry would still bring more problems than it would solve. They questioned the use of genetic modification in the development of crops and of processing technologies. They also criticized the paradigm of industrialized agriculture that the large-scale biofuels production entails for its effects on land-use, environment and ecology, and economic and social justice.

As the debate on the potentials and risks of biofuels continued, advocates began to highlight the EBI's plans to research the socio-economic and environmental implications of biofuels. One of the prominent advocates of the partnership, Jay Keasling (Professor of Chemical Engineering and Bioengineering), drew on his work in genomics research to describe how the EBI would carry out "upstream evaluation":

> All along this process, we intend to examine the human practices of energy biosciences, that is, looking at the environment, the health, the safety of doing this, reaching out to people in the community, people around the world and educating them. … There's been a model, and the genome projects were a model for how not to do this, and that is they had—they talked about the social aspects after the fact and completely disengaged from the science. And we believe that that's not how it should happen, that in fact we should be talking about the social aspects of these projects right along with the fact that we're doing them. (Jay Keasling, Faculty Forum, March 8, 2007)

In these instances, advocates seemed to hold that EBI would produce consensual assessments of risk (assessments that would be consensual because they would be scientific). Relatedly, they displayed ample confidence in the possibility of effective risk management. In fact, they argued that conducting this research at Berkeley increased the chances that it would be done responsibly:

> Is it going to be scalable, is it going to be done in an environmentally mindful way, and in a way that brings along the whole social milieu of the United States? Monsanto was mentioned and they did not do this in a correct way. I think the University of California at Berkeley and the Berkeley Lab are a perfect place for this so you can do this in a responsible way and you can get largely CO^2-neutral fuels in a responsible way and in a way that's sustainable. (Steven Chu, LBNL Director in 2007, Faculty Forum, March 8, 2007)

Jay Keasling was particularly optimistic on the potential impact of the university on BP: "If we're successful and we turn an oil company into a renewable energy company, that's great" (*San Francisco Chronicle*, February 2, 2007).

Critics combined scientific and nonscientific arguments to oppose the proposed research agenda. For example, in the initial days of the controversy, Tad W. Patzek (then Professor, Civil and Environmental Engineering) circulated a comment on the EBI proposal drawing on his

research.[86] At the same, Patzek opposed the release of genetically modified organisms and the expansion of industrial forms of agriculture in paradigmatic terms, arguing they would have very negative consequences for the world's ecosystems:

> When everything is built, all these expensive buildings, all these hundreds and hundreds of million dollars … and we will engage dozens of faculty and even more students to work on all of this. In the end, the outcome will be in my mind, either zero or highly negative from the point of view of the world and its ecosystems. (Tad Patzek, KALW, August 20, 2007)

Similarly, Ignacio Chapela (Microbial Ecology), known for his work on the spread of genetically modified organisms and the controversy that surrounded his tenure review at UC Berkeley,[87] highlighted the risks of the proposed route:

> What would certainly come out of the BP-Berkeley facilities would be a large number of genetically altered, reproducing, living organisms to be released in the public environment, in Berkeley, in the Midwest and around the world. Genetically-modified (or "GMO") grasses, trees, algae, bacteria, viruses destined for intentional, large-scale release in the public environment. These organisms do not represent Science. If anything, they may represent our failure as scientists to assume the deep inadequacies of our understanding about living organisms and the ecology of our planet. (Ignacio Chapela, Faculty Forum, March 8, 2007)

The EBI's plans to research socio-economic and environmental dimensions of a future biofuels industry did not convince the critics. Miguel Altieri (Faculty Forum, March 8, 2007) argued that much of the crop production would have to take place in developing countries and lead to increased deforestation, carbon-dioxide emissions, and use of transgenic crops and fertilizers. Anne-Lise Francois (English and Comparative Literature) questioned the aims of the EBI program on environmental, social and economic impacts of biofuels. Quoting from UCB's proposal to BP, she asked how the EBI planned to work on "the public understanding of biofuels":

> I do have a question about something I read in the proposal which appears on page 55. "This research will profit from paying significant attention to the evolving regulatory framework and societal response to Genetically Modified Organisms at both the domestic and international level. Included will be work on the public understanding of biofuel technologies and the modeling of social adoption in different political contexts." Now it's unclear to me what work on the public understanding of biofuels technologies can possibly mean. Does this mean that Berkeley will now be in the business of addressing and mollifying public critiques of genetic engineering? I think it's worth noting that these critiques have always been scientifically sound as well as politically grounded and I just want to make a note of the fact that the dissidents here, Chapela and Altieri, seem to be the only ones who are actually addressing the political and social contexts in which GMOs and now biofuels will be developed. … If you could please also address this

[86] Patzek, Tad W. 2007. "First Comment on the UCB-UIUC Proposal to BP."
[87] For details on Chapela's work and tenure case, see Delborne (2008), Kinchy (2010).

> question of what do you mean by "working on the public understanding of GMOs"? (Anne-Lise Francois, Faculty Forum, March 8, 2007)

Advocates sought to overcome the political nature of the debate by "scientization." This involved advising a "wait-and-see" approach, highlighting researchers' expertise, and describing critics' positions as unscientific or dogmatic. At the radio broadcast referred to earlier, Chris Somerville (EBI Director) responded to Patzek's comments about the potential harm biofuels would bring to ecosystems as follows:

> Two-hundred and fifty faculty made proposals for research that they thought academically interesting and address the general problem. I don't think it is appropriate to sort of prejudge it and say what the outcome of that will be. I think we should, my own personal opinion is we should stand back and see what it is. (Chris Somerville, EBI Director, KALW, August 20, 2007)

These strategies of "scientization" were not simply tools conjured to win the debate; they mostly reflected a sincere belief in the potentials of biofuels research. Yet, they helped represent the critics as "anti-innovation," "anti-science" or "dogmatic," and empower experts and their sponsors in the politics of clean energy.

Discussion

Universities have long highlighted their service to the public as grounds for claiming tax-exemptions and various forms of support. Yet, the question of how they contribute to the public good has been debated and answered in different ways in different eras. In the early twentieth century, "1915 Declaration of Principles on Academic Freedom and Academic Tenure," published by the newly founded American Association of University Professors (AAUP), argued that the presidents and trustees were obliged to govern the university in the public interest. For the AAUP, this meant that they had to ensure the conditions where "what purports to be the conclusions of men trained for and dedicated to the **quest for truth** shall in fact be the conclusions of such men and not echoes of the opinions of the lay public or of the individuals who *endow or manage* universities" (AAUP 1915, p. 294). During the Cold War era, the rapidly expanding federal patronage for academic science was accompanied with and justified by idea that **basic science** was a public good. This idea, influentially formulated in Vannevar Bush's report, *Science: The Endless Frontier* (1945), helped overcome the fights over the question of "whom the federal funding was subsidizing" (Kevles 1977), and helped recipient universities construct a specific definition of autonomy vis-à-vis the government and its war-making (Smith 1971; Leslie 1993; Kleinman and Solovey 1995; Moore 1996, 2008; Lowen 1997). Arguably, it was also rooted in and reflected a belief in a kind of autonomous science that could be beyond political interests, embodied by both Vannevar Bush and Robert K. Merton (Dennis 2004; Hollinger 1996). Beginning in the late 1970s, the preceding settlement began to be undermined. The break-up of the bipartisan consensus in government about the funding of academic science (Kevles 1992; 2006), pressures of economic competition, federal policies aimed to encourage technology transfer (Berman 2012a), the growth of the biotechnology industry (Kenney 1986; Krimsky, Ennis and Weissman 1991; Rabinow 1996), and the success stories of Silicon Valley in California and Research Triangle Park in North Carolina led to a growing emphasis on universities' role in innovation and economic growth as contributions to the public good

(Slaughter 1993). The distinction between basic and applied science was increasingly questioned (Nelkin 1978; Stokes 1997; Calvert 2006); even NSF's project evaluation criteria were changed to include implications for society (Jasanoff 2005, p. 229).

I argue that the EBI case signals a new turn in the debates on how universities contribute to the public good, and simultaneously in the co-production of universities, definitions of "good science" and technoscientific futures. Throughout the controversy, advocates presented the EBI's novelties (embrace of an applied mission, close engagement with industry scientists, research on socio-economic and environmental implications) as crucial attributes helping university scientists address major difficult problems facing society. Thus, they put out a particular definition of "good science" that spilled former boundaries. When faced with criticism, they employed strategies (invoking "peer review," scientization) that other scholars have identified in similar or related technoscientific controversies (Jasanoff 1987; Kinchy 2010, 2012). They also employed strategies that were linked with their powerful positions in their respective fields, such as using an individualist definition of academic freedom, or minimizing the structural and mainly economic inequalities shaping the funding and development of research agendas.

A brief comparison with a similarly controversial agreement that UC Berkeley had signed in 1998, the five-year, $25 million dollar agreement between UCB's Department of Plant and Microbial Biology (PMB) and Novartis Inc. (1998-2003), supports the preceding argument. The agreement with Novartis had also stirred up considerable controversy. In response to criticisms, the UC administration had sponsored two reviews of the agreement (one internal and one external). The internal review was led by the incumbent Associate Vice Chancellor for Research, Robert M. Price, and focused on the impact of the agreement on the Department of PMB (Price and Goldman 2002). The external review was undertaken by a team of researchers from Michigan State University, and sought to discern the impact of the Novartis agreement not only on PMB but also on the College of Natural Resources and UC Berkeley, and discussed some of the broader issues that crystallized in the surrounding controversy (Busch et al. 2004; Rudy et al. 2007).

Both UCB-N and the EBI belong to a specific category of research agreements, increasingly referred to as "strategic corporate alliances (SCA)," that is agreements made by departments, colleges or the university as a whole, as opposed those made by individual researchers (AAUP 2014).[88] Both are arguably products of the same field-level forces that dissolve the boundaries between corporate and academic research in certain research fields. Yet, the differences in the making, organizational form and the defense of the two agreements highlight the turn that the EBI represents.

To begin with, the partnerships differed with respect to which party initiated the relation. In the UCB-N case, the Department of PMB had initiated the search for the industry partner, received several proposals and chosen Novartis. In the EBI case, BP initiated the search, invited proposals from a limited number of institutions, and chose UCB. Further, with the UCB-N

[88] Like other forms of university-industry research relations, SCAs are not entirely new, but growing to be more common (AAUP 2014, p. 36). Since the late 1990s, there has been more attention to how SCAs are formed and how they affect academic research and the universities involved. Most discussions cannot but refer to Novartis agreement signed in 1998. In 2005, when Cornell University developed and announced a Strategic Corporate Alliance Plan, its faculty put together a "Faculty Statement of Principles and Best Practices Concerning Strategic Corporate Alliances." In 2010, Center for American Progress published "Big Oil Goes to College: An Analysis of Ten Research Collaboration Contracts between Leading Energy Companies and Major U.S. Universities" authored by Jennifer Washburn. Recently, the AAUP's Recommended Principles to Guide Academy-Industry Relationships (2014) devotes a separate section to these kinds of agreements.

agreement, the Department of PMB joined with Novartis Agriculture Discovery Institute (NADI), a genomics research endeavor linked to Novartis Research Foundation and devoted to fundamental research. The NADI funds were provided to the department, where a departmental committee (including two Novartis representatives invited by the Department of PMB) decided its allocation (Busch et al. 2004, p. 87). The agreement did not specify a research agenda or mission. In contrast, UCB-BP agreement led to the founding of a stand-alone research institute with its own governance and building bringing together BP and university administrators and scientists. The EBI was founded with a definite research mission: to explore the potentials of bioscience to develop biofuels. In the context of debates on the pros and cons of biofuels, the EBI took on the task of exploring the socio-economic and environmental issues as well.

With regard to how the university administration and researchers responded to the criticisms of these partnerships, there were two notable differences. First, on the question of the industry partner's influence on scientists' research agenda, the internal review of the Novartis agreement argued that the partnership had not had a major impact on the direction of research in the Department of PMB by relying on the distinction between basic versus applied science. Citing interviews with participating scientists, it argued that the partnership had not caused a divergence from the department's mission to conduct basic science (Price and Goldman 2002, p. 25). In contrast, university advocates of the EBI embraced the goal of developing technology as a way of increasing their impact and argued that partnering with industry was crucial for this task. Second, in the UCB-Novartis case, the internal reviewers acknowledged that part of the criticism was rooted in objections to genetic modification, but stated that they would refrain from entering the debate on "the value of genomics and biotechnology" (Price and Goldman 2002, p. 28). In contrast, in the EBI controversy, university scientists embraced the task of developing biofuels, highlighted EBI's program to examine the socio-economic and environmental implications of the future biofuels industry, and entered the debate on the potentials and risks of biofuels. In other words, their break with the basic/applied science distinction *and* their willingness to enter into discussions of risks paved their way into the heart of a major technoscientific controversy.

Conclusion

In "Technology as a Site and Object of Politics," Jasanoff (2006) examines the politics of technology under four headings: politics of design, risk, standardization and ethical constraint. The foregoing analysis of the EBI controversy suggests that work under a fifth heading, namely, "the politics of public good," could contribute to the study of the politics of technoscience. In the EBI case, clashes around the definition of public good continually revolved around a simultaneously discursive, organizational and legal boundary: between public and private. EBI advocates tended to blur this boundary. For example, they put emphasis on the need for and benefits of public-private partnerships in addressing climate change and energy issues. The critics insisted that private interests do not always align with the "public good" and mobilize the "publicness" of the institutions, traced back to the land-grant heritage, in pursuit of their politico-scientific goals. As BP moved into the university, but continued to have an interest in the image of the university as pure so as to solidify the representation of its interests as the interests of all, the critics described this as pollution to fight against this purification of particular corporate technoscientific agendas.

CHAPTER SIX – CONCLUSION

STRONG OBJECTIVITY

Knowledge matters. How knowledge is produced, legitimized and used, matters. The aim of this study has been to work towards an understanding of not only how knowledge is produced but also how its conditions of production impact its content, legitimacy and implications. There are many ways to go about writing a thesis on knowledge production in the contemporary era. In charting the parameters of this thesis, I have been occupied by two concerns. First, all observations are theory-laden, and all theories have implicit assumptions and implications, and it is important to take these assumptions and implications into account. The question of how aware individuals or scientific communities can be of their assumptions and blindspots is of course a great question for another discussion. Second, while there is obviously a strong coalition of forces and arguments demanding that scientific accounts should be value-neutral, providing just facts for others to use in making their own value-judgments, there is also a strong case for a kind of social theorizing that aims not only to grasp the situation on the ground, but also to illuminate how it might be transcended, given the high likelihood that the situation on the ground is less than ideal. With these concerns, this study has pursued two main questions: (i) How to theorize knowledge production? (ii) What are conditions under which science can deliver boon rather bane?

I began with a comparison of Bourdieu's and Latour's theories of science. The comparison showed how each theorist sets out with different views on what a sociological account should achieve and accordingly, work with different concepts that describe the empirical world in starkly different ways. But this much could be said from a comparison of their work on a subject other than science. In fact, if we compare their conceptual toolbox without attending the object under study, it is probably not possible to adjudicate between them: they are just different languages. Yet, given my interest in theorizing science *as science*, I focused on comparing their theories of science as *theories of truth*. I foregrounded how they stipulated opposite conditions (autonomy versus associations) for the production of reliable knowledge, and showed how these stipulations are rooted in their a priori positions regarding science: Bourdieu believes science is the site of Reason, and Latour believes rationalism and other epistemologies are all discourse.

Given the preceding, the question became: how can we adjudicate between these two theories as *theories of truth*? This may sound like it amounts to adjudicating between rationalism and relativism. To some degree it is. But it is useful to approach the problem in two steps. The first step is to ask: How well do Bourdieu and Latour capture the conditions of possibility of reliable knowledge? I contend that the EBI case, or rather the broader set of relations of production of knowledge, science and technology, amidst which the EBI emerged and declined, highlight how scientific advance depends on technology and major amounts of economic capital. Bourdieu's theory of truth fails to capture this interdependence. Latour's account of how more associations produce more scientific advance sheds more light on the empirical reality.

The second step is to ask: What are the normative implications of the two theories? Chapter two argued that both theories verge on providing an answer to how science should be organized. Here, I wish to take one more step and point out how Bourdieu's and Latour's visions of science map on to the much-discussed expertise-democracy problematic. Bourdieu, by insisting that production of scientific truths require sufficiently autonomous scientific fields,

expresses a belief in the "value" of expert knowledge over lay forms of knowledge. Latour, by insisting on the need for associations, offers a "democratizing" move. In other words, rationalism-relativism dichotomy becomes expertise-democracy dichotomy.[89] Once again drawing on the EBI case, I contend that two theoretical/normative moves can help advance the expertise/democracy debates. One move is to seek a more meticulous understanding of the institutional conditions of the production of knowledge, science and technology. The second, corollary move is to investigate the necessary conditions for "strong objectivity." Let me elaborate these two moves in a little more detail.

The tension between expertise and democracy is real. Who, in their right mind, would advocate that non-experts take the place of experts in processes of knowledge production? Yet, current debates suffer from certain weaknesses. For example, take Steven Shapin's formulation of the tension:

> There is a tension that lies at the very heart of the relations between scientific expertise and the democratic American state. … On the one hand, we have the conviction that there are certain conditions in which scientific inquiry can best acquit its **intellectual goals** and thereby make its most effective **contributions to human well-being and national interests**. … If you want **reliable scientific knowledge**, the price is a significant chunk of public funding and relief from the accountability to which other recipients of public funding are routinely subject. …
> On the other hand … When the experts are "on top," as opposed to "on tap," political decisions are redefined as technical decisions and the cast of participants to decision-making is narrowed accordingly. …
> The tension is just irresolvable: if we propose to resolve it in favor of minute political accountability, we are likely to stunt scientific development and the free play of creativity; if we exempt the scientific community from normal patterns of accountability, we compromise democracy through a tyranny of the experts… (Shapin 1999, p. xvi-xvii; emphases mine)

Shapin begins by conflating the achievement of "intellectual goals" with "effective contributions to human well-being and national interests." Next, it is as if the only choices are (i) funding scientists and leaving them alone and (ii) minute "political accountability." Lastly, this version locates the tension between scientific experts and the democratic American state as the representatives of the people. Yet, both science and the state are complex institutions with undertheorized interests and biases, and with different relations to different parts of the citizenry.

A better formulation of the tension between science and democracy requires (i) a better theory of the goals, institutional structure, and products of scientific knowledge, and thus, (ii) a better account of whom the experts depend on, and how these dependencies shape the production of knowledge, science, and technology. To move in this direction, resuscitating Merton's proposal to theorize science as an institution in relation to its societal context is a good starting point. Yet, contrary to what Merton and others have assumed, theorizing science as an institution (or a field). Rather, it means that recognizing science needs resources and has its own internal

[89] The literature on expertise and democracy is wide and expanding: Collins (2009), Collins and Evans (2002), Durant (2011), Guston (2000), Hilgartner et al. (2015), Jasanoff (1990, 2003, 2005), Kleinman (2000), Lahsen (2005), Latour (2004a), Weingart (1999).

biases, which in turn means that scientific advances take particular routes because of the institutional need for resources and the ensuing institutional biases.

Once we grasp the institutional conditions and biases of science, the normative/political question becomes: What are the conditions necessary for the production of reliable knowledge that can help address the multi-faceted problems in the public interest? To answer this question, I propose to draw on Sandra Harding's (1992, 1993, 1995) response to the post-1960s crisis in philosophies of science by calling for "strong objectivity." Harding (1995, p. 336) argues that what is long recognized as neutral science is actually a particular science constituted (depoliticized) by a certain kind of politics: "the institutionalized, normalized politics of male supremacy, class exploitation, racism and Eurocentrism." Hence, rather than being caught up in the dichotomy of objectivism versus relativism, she calls for retaining the pursuit of rationality by pursuing "strong objectivity." For Harding, this requires setting out with the recognition of social inequalities and crafting knowledge projects that start outside the dominant locations – not to produce multiple truths but to advance the collective truth. If we leave behind the idea of relative autonomy of science as the basis of reliable knowledge, but remain concerned about what kinds of interdependencies shape the agendas, findings and implications of contemporary science, the solution may be to advocate the pursuit of a parallel form of "strong objectivity" through more associations with the less powerful.

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APPENDICES

APPENDIX A
Chronology of the EBI Controversy

1998	
Nov 23	UCB-Novartis (UCB-N) agreement signed
2000	
Sep 22	BP gives $20 million to UCB & CalTech to study methane
2001	
May 22	BP announces Helios re-branding
2004	
June 17	Steven Chu named Director of LBNL (from Stanford physics department)
July 13	External Review of UCB-N completed
July 27	Robert Birgeneau appointed Chancellor of UC Berkeley
2006	
June 14	BP Issues call for proposals
Jun -Nov	Beth Burnside, UC Berkeley's Vice Chancellor for Research, briefs Committee on Academic Planning and Resource Allocation (CAPRA) every other week
Early August	BP tours UC Berkeley & LBNL
August	Chu announces plan to build $90 million building for 'Helios Project'
Fall	Burnside begins briefing CAPRA about every 2 weeks
Sep 5	Burnside briefs Academic Senate President William Drummond and Vice Chair Sheldon Zedeck
Sep 9	Campus Administrative memo sent to deans, department chairs, and directors asking for research interests and capabilities relevant to the EBI
Sep 25	Burnside briefs Committee on Research (COR)
Early Sep	Administrative memo circulated regarding the EBI proposal
Oct 12	Amyris announces $20 million in funding and hiring of the president of BP's American fuels operation John G. Melo, who devised BP's Helios branding
Mid Oct	UCB partners with University of Illinois at Urbana-Champaign (UIUC)
Oct 23	Burnside briefs COR
Nov 16	Chair of Budget Committee (BC), Pat Kirch, is given copy of draft the EBI proposal & invited with CAPRA members to give feedback within a week
Nov 20	Chair of CAPRA briefs DIVCO with BC Chair present
Nov 20	California Governor Schwarzenegger pledges $40 million to the UC campus that is selected by BP
Nov 22	CAPRA (chaired by Calvin Moore) meeting Burnside informs committee that the proposal is in final stages and to be sent to BP next week; says CAPRA will be given the final version; notes that the Chancellor pledged 7 FTEs (BC chair who is a member of CAPRA absent in this meeting)

Nov 27	UCB submits proposal to BP
2007	
January 15	Chu speaks at special meeting of the California State Board of Food and Agriculture
January 23	LBNL releases Long Range Development Plan (LRDP)
Feb 1	EBI Press Conference BP announces UCB, UCIC & LBNL announced as winner UIUC Academic Senate learns of UIUC's involvement with BP
Feb 1-9	Burnside provides a non-redacted copy of the EBI proposal for review at the Academic Senate Office.
Feb 5-9	Daniel Kammen's informational meeting
Feb 12	Divisional Council (DIVCO) Regular Meeting BC Chair states that BC has not received written information about 7 FTE
Feb 13	Meeting between Burnside, Bresleauer, de Vries, and the Budget Committee – assure the BC that 7 FTE would be filled in normal procedures
Feb 14	Burnside meets with CAPRA
Feb 15	Meeting of President Drummond, Chancellor, and members of DIVCO
Feb 26	*StopBP-Berkeley* hosts teach-in featuring Iain Boal, Miguel Altieri, Tad Patzek, Ignacio Chapela and Jennifer Washburn
Feb 28	Dean of the College of Natural Resources (CNR) Paul Ludden holds a closed meeting with students The EBI proposal revealed in press
March 1	*Stop-BP Berkeley* stages "Oil Spill" protest at University Hall EBI website launched Graduate Assembly (GA) considers but tables a resolution on EBI
Mar 2 Fri	Berkeley Daily Planet (BDP) reports access to EBI proposal
Mar 5-8	Burnside places a redacted version of the EBI proposal online
March 8	Academic Senate Faculty Forum
March 12	Petition established at www.facultysharedgovernance.org
Mar 14	City of Berkeley Planning Commission Special Joint Meeting
March 15	Presentation of public comment at the UC Regents' meeting in Los Angeles; Regents approve building plans
March 19	UCB Informational Forum on EBI
March 20	Burnside advises Drummond that chairs of BC, COR, CAPRA, CAF invited to participate in negotiation of contract
April 2	AS Executive Committee discusses the deal
April 5	Graduate Assembly (GA) passes resolution on the EBI proposal *StopBP-Berkeley* obtains the appendices of the EBI proposal
Apr 12	Letter by John M. Simpson (FTCR) to Chancellor
April 16	CNR & Stop-BP Forum
April 19	Academic Senate Meeting

April 25	Associated Students of the University of California (ASUC) passes resolution on the EBI proposal
April 26	*StopBP-Berkeley* hosts public forum "Biofuels: A Conversation between Science, Engineering, Ecology, Politics and Urban Design"
May 1	Call for EBI Research Pre-Proposals issued
May 2	*StopBP-Berkeley* hosts public forum "The UCB-BP Deal – Implications for the Public University"
May 8	Protest march & petition submission
May 17	Criticisms raised against the partnership at UC Regents Meeting in San Francisco
May 18	International Low Carbon Fuel Symposium
Jun 1	Student Consultation Meeting I; Meeting with GA Liaison
Jun 8	EBI Deadline for pre-proposals
Jul 2	EBI Invitation for full proposals
Jul 3	Student Consultation Meeting II; Meeting with GA Liaison
July 12	LLNL signs 2-year agreement with BP to study underground coal gasification
July-August	Negotiations over the partnership contract
August 2	GA meeting
Aug 17	EBI Deadline for full proposals
Sep19-20	Regents Meeting in UC Davis
Oct 2	EBI awards research grants
October 4	Town Hall Meeting with President Dynes
October 4	*StopBP-Berkeley* organizes protest "Trojan Horse"
October 4-5	Energy and Agriculture Conference
Nov 15	EBI Contract Signed

APPENDIX B
Distribution of the Signees of the Faculty Shared Governance Petition

Department	**#**	**Department**	**#**	**Department**	**#**
English	20	ESPM	10	Integrative Biology	4
Anthropology	11			Math	2
History of Art	9	City Planning /Architecture	6	Molecular and Cell Biology	1
Comparative Literature	5	Law	3		
Rhetoric, Languages	5				
History	6				
Sociology	6	Journalism	1		
Geography	5	Social Welfare	1		
Gender & Women's Studies	3	Nutrition/Tox	2		
Psychology	3	Public Health	1	Civil & Environmental Engineering	2
Philosophy	1			Mechanical Engineering	2
Political Science	1			Electrical Engineering & Computer Science	1
Economics	1	Agricultural and Resource Economics	1	Business School	1
Total	**76**		**25**		**12**

APPENDIX C
Distribution of the Signees of the Call for the Special Meeting of the Academic Senate

Department	**#**
Anthropology	5
ESPM	3
History of Art	2
English	2
Comparative Literature	2
Geography	2
Rhetoric	1
Total	**17**

APPENDIX D
Novartis Controversy

In November 1998, the Novartis Agricultural Discovery Institute (NADI) and UC Regents signed a five-year research agreement whereby NADI committed to provide a total of $25 million to UC Berkeley's Department of Plant and Microbial Biology (PMB). This agreement was surrounded in an extended controversy and has been a benchmark case in debates on university-industry research relations. The Novartis alliance stood out among university-industry partnerships in that it involved not one or several faculty members, but the Department of PMB as a whole (Busch et al. 2004). In addition, the agreement contained a highly controversial provision: one that gave NADI first-look rights for inventions funded by other sources. In response to concerns and criticisms, the UC administration sponsored two reviews of this partnership. The internal review was led by the Associate Vice Chancellor for Research, Robert M. Price, and focused on the impact of the agreement on the Department of PMB (Price and Goldman 2002). The external review was undertaken by a team of researchers from Michigan State University (Busch et al. 2004). It sought to discern the impact of the Novartis agreement not only on PMB but also on the College of Natural Resources and UC Berkeley, and discussed some of the broader issues that crystallized in the surrounding controversy.

Somewhat remarkably, the two reviews reached parallel conclusions on the effect of the agreement on the department. According to the internal review, the Novartis agreement did not affect PMB researchers' freedom to set their research agenda and to discuss, interpret and publish their finding in any adverse ways. The faculty testified that the agreement did not require them to alter their research foci or methodology. The faculty publication record also showed that they continued to publish in the areas that they were established. There was no evident shift towards more applied or genomic research. The only formal limits to information sharing were those associated with the use of Novartis database. The researchers did not find that the requirement to send presentation and publication drafts for NADI review resulted in any "significant impact on the date of publication" (Price and Goldman 2002, p. 2). The review also found that the agreement did not pose a threat to the principle of faculty governance. Novartis representatives did not have any direct role in the governance of the department; their participation was "limited to matters related to the research program" that the company funded and was through minority representation on the two committees governing this program (Price and Goldman 2002, p. 14). In fact, the internal review found that, contrary to the critics' expectations, the agreement greatly benefited the department. The increase in resources greatly contributed to the research capabilities and achievements. Faculty members felt that the grant-making process imposed relatively little bureaucratic and substantive constraints, and hence, allowed them to pursue riskier projects. During the period agreement period, the department did not become reliant on Novartis funding and was in fact able to obtain funding from other sources as well. The external review reported similar findings. The PMB researchers did not think that the agreement had any adverse effects on the department. They viewed UCB-N "as enhancing their academic freedom rather than restricting it" (Busch et al. 2004, p. 117)

However, the external review parted with the internal review and looked beyond the department. It described how the College of Natural Resources at Berkeley had long been a site of struggle between researchers committed to agricultural biotechnology and those committed to research on agro-ecological sustainability (Busch et al. 2004, p. 126). It evaluated how the administration's overall commitment to certain areas of research impacts academic freedom:

> For a number of reasons, the UC system and UCB administration have committed themselves to certain areas of research more than other areas, and to greater relations with industry. While such commitments potentially increase the academic freedom of some, they may reduce the academic freedom of others. For many faculty, students and activists a decreasing emphasis on agroecological sustainability is a notable threat to the diversity of views in academic research and teaching within the UC system. (Busch et al. 2004, p. 128)

In other words, it highlighted how the availability of corporate funding for certain research areas and the administration's tendency to valorize entrepreneurialism affect the academic freedom scholars working in areas that do not attract corporate funding as readily. It was precisely this latter point that the critics of the EBI drew on in the EBI controversy.

APPENDIX E
Tobacco Funding Debate

The tobacco funding debate at the University of California occurred between 2002 and 2007 roughly in three phases. The first phase began when a number of individual units of the university (the UCLA School of Nursing, UCB School of Public Health, UCSD Cancer Center and UCSF Cancer Center) adopted bans against receiving research funding from tobacco companies (RE89). These units were following a set of other institutions that had already adopted such bans; namely, Johns Hopkins School of Public Health, University of Arizona School of Public Health, Emory University School of Medicine, Harvard School of Public Health, Harvard Medical School, and Ohio State University School of Public Health. In response, the University Committee on Research Policy (UCORP), a committee of the System-wide Academic Senate, took up the issue and drew up a report titled "Problematic Restrictive Clauses in Contracts, Grants and Gifts for Research." In this report, UCORP recognized that some research funding come with "requirements and restrictions that could compromise [UC's] research mission," and that such problematic clauses did appear in relation to tobacco funding. Yet, it stipulated that such requirements and restrictions should be addressed via University policies on research funding. Following the current AAUP position on the issue, it argued against bans based on the source of funding:

> Be it resolved that … individual faculty members be free to accept or refuse research support from any source, consistent with their individual judgment and conscience and with University policy. Therefore, no unit of the University should be directed (by faculty vote or administrative decision) to refuse to process, accept or administer a research award based on the source of the funds.

After some faculty review, the Assembly of the Academic Senate adopted this position with some minor changes. Hence, the first phase of the debate concluded with the largest faculty body at the University of California taking the position that no unit or group can be allowed to impose bans pertaining to funding sources of individual faculty.

The second phase was initiated by the Board of Regents in response to the federal district court ruling (August 2006) that found Phillip Morris "guilty of violating the Federal Rackeeter Influenced and Corrupt Organizations Act" (RE89). The Regents, concerned about how receiving funds from a convicted corporation reflected on the university as a whole, asked the Academic Senate to reconsider the issue. In response, the Academic Assembly reaffirmed its earlier position:

> The Academic Assembly instructs the Chair of the Assembly to advise the President that grave issues of academic freedom would be raised if The Regents were to deviate from the principle that no unit of the University, whether by faculty vote or administrative decision, has the authority to prevent a faculty member from accepting external research funding based solely on the source of funds. Policies such as the faculty code of conduct are already in place on all campuses to uphold the highest standards and integrity of research. The Academic Assembly believes that Regental intervention on the basis of assumptions about the moral or political standing of the donor is unwarranted.

This was the resolution that advocates of the EBI quoted in the special meeting of the Academic Senate convened to debate the partnership with BP. Yet, in the discussion period of the same meeting of the Assembly, two additional resolutions were proposed and approved:

> The Assembly declares its deep disapproval of funding arrangements in which an appearance of academic freedom belies an actual suppression of academic freedom.
> The Assembly asserts its conviction that past funding arrangements involving the tobacco industry have been shown to suppress academic freedom.

These two resolutions, when taken together with the preceding one, indicate that multiple definitions of academic freedom were invoked in these discussions.

The third phase of the debate began in January 2007 with Regent Moores' motion to introduce restrictions on tobacco funding. In debating this resolution, the Board of Regents noted the tensions among the earlier resolutions passed by the Academic Assembly and asked the latter to "provide a formal and *unambiguous* position" (RE89/Sep, p.7; emphasis mine). In May 2007, Assembly of the Academic Senate voted against Regent Moores' proposal. President Dynes and Council of Vice-Chancellors for Research also declared positions against it. In the face of this opposition, Regent Moores moved to propose a compromise resolution that did not ban tobacco funding but stipulated that (i) researchers must exercise utmost care; (ii) proposals to secure funding from tobacco industry "must be reviewed by a scientific review committee" and "must be approved by the chancellor"; (iii) the president will submit an annual report to the Regents. In September 2007, the Board of Regents approved this version and put an end to the formal debate, at least for the time being.

When the UC-BP partnership was announced in February 2007, the tobacco funding case was pending before the Board of Regents. Nonetheless, sciences and engineering faculty employed the Academic Assembly resolutions passed in the second phase to argue that the attempt to institute senate oversight over UC's partnership with BP constituted a threat to academic freedom.

APPENDIX F
Energy Biosciences Institute (EBI)

The Energy Biosciences Institute is housed mainly on the Berkeley campus, but also has affiliated research laboratories and fields at the University of Illinois at Urbana-Champaign. It is governed by two committees: the executive committee and the governance board. The executive committee is made up of ten scientists, two of which are from BP. It issues call for proposals, evaluates them through a peer-review process, and forms yearly research programs. The governance board comprises eight members, four of which are from BP. The yearly research programs are subject to approval by the governance board. These programs and projects are organized under five main divisions: four of which are devoted to the task of developing viable and clean biofuels: Feedstock Development; Biomass Depolymerization; Biofuel Production; and Environmental, Social and Economic dimensions. The fifth division is focused on developing biological processes for enhanced oil recovery and fossil fuel processing.

Scientists in the Feedstock Development Division work towards identifying and improving plants that might yield more biomass than plants used today. For this purpose, they use genomic tools to improve certain plants, assess pests and pathogens that could affect biomass yields, engage in field trials to obtain data on agronomic requirements and procedures, and develop engineering solutions to the growing, harvesting and transport and storage of biomass crops. Scientists working in the Biomass Depolymerization Division are engaged in finding ways to break the cell walls and free the sugar molecules in plants. Hence, they work on characterizing cell walls, and examine enzymes, catalysts and organisms that specialize on breaking cell walls (e.g. organisms found in grass-eating animals). The Biofuels Production Division comprises one main program to build a microbial characterization facility that will house experiments on improving bacteria that can take sugars and make them into biofuels. Scientists working in the Environmental, Social and Economic Dimensions Division aim to assess the environmental, social and economic impact of biofuels: how they affect land, water, air quality, how they affect land use patterns and food supply, how they might affect global warming. They also have projects on legal issues and intellectual property regulation, as well as a project examining the possible political effects of a transition from the current fossil fuels regime to a biofuels regime.

APPENDIX G
Biofuels

Biofuels are fuels made from biological materials. In principle, they include non-liquid fuels such wood and charcoal, but the term is often used to refer to liquid fuels for transport. Currently common types, known as first-generation biofuels, are ethanol (produced from corn or sugarcane) and biodiesel (produced from vegetable oils) (Delborne 2010). Around the turn of the twentieth century, ethanol was a major competitor to gasoline, but lost through a mix of factors not purely technological (Carolan 2009a, 2009b). The 1970s' oil crisis triggered a renewed interest in biofuels. In Brazil, it led to the founding of a major sugarcane-ethanol industry. In the United States, it spurred a major round of research. The take-off of the American ethanol industry occurred in the 2000s. By 2008, the United States had become the leader in the global ethanol production, putting out 9 of the 17 billion gallons produced globally.

Yet, the benefits and risks of biofuels are far from clear. The advocates of *corn-ethanol*, industry lobbying organizations in particular, argue that corn-ethanol leads to lower CO^2 emissions than gasoline, contributes to energy independence, and is a source of economic growth. Critics, however, argue that while burning ethanol may emit less CO^2 than gasoline, producing ethanol with current processes causes emissions that offset the savings. They also draw attention to the impact of ethanol production on food prices around the globe.

In the second part of the 2000s, certain sectors of the biofuel advocacy network turned their attention to *second-generation* or *cellulosic biofuels*, which are still in R&D stage. In this case, the idea is to use non-food crops, and to find ways to access and process the more energy-dense (i.e. cellulosic) parts of these crops. Advocates of cellulosic biofuels acknowledge the criticisms against corn- and sugarcane-ethanol. They propose that research can help develop production processes that will require less energy and therefore, less CO^2 emissions over the product life-cycle. They also argue that focusing on non-food crops will overcome the food prices issue. Yet, the critics hold that a cellulosic biofuels industry would bring more problems than it would solve. They question the use of genetic modification in the development of crops and of processing technologies. They also criticize the agricultural paradigm that the large-scale biofuels production entails for its effects on land-use, environment and ecology, and economic and social justice.

In a perceptive article, Iles (2010) describes the recent developments in the debate on the GHG emissions of biofuels. In 2008, some researchers reported that large-scale biofuels production would cause major land-use change especially in developing countries, and that if these changes are taken into account, biofuels could not help reduce GHG emissions (Fargione et al. 2008; Searchinger et al. 2008; quoted in Iles 2010). In response, Biotechnology Industry Organization (BIO) argued that land-use changes cannot be accurately modeled and hence, should not be included in the life-cycle analyses ("the quantitative analysis of all physical and economic processes involved in the life cycle of producing a given unit of biofuel"(Hendrickson 2006 quoted in Iles 2010)). In other words, the debate on the GHG missions of biofuels turned into a debate on how to conduct LCAs and what can reliably be included in them.