The Commercialization of Science, and the Response of STS*

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1. Money Can't Buy Me Truth?

> Claims about the proper method for writing the history of science are simultaneously claims about the relations between the producers and consumers of scientific knowledge.—Michael Dennis (1997, p.1)

It is not hard nowadays to find people who harbor strong opinions about the contemporary commercialization of science, primed and willing with very little prompting to recount some anecdote about the travails or triumphs of Viridiana Jones in the Temple of Mammon. First off, there are the motley ranks of Cassandras, who, significantly enough, tend to have a soft spot for the Good Old Virtues of the Mertonian Norms, and bewail the prospect of expulsion from the prelapsarian Garden.[1] They lament that once there may have been an invisible college, chorused sweetly in consort in the quest for truth, but now there are only feckless individual entrepreneurs scrabbling for the next short-term contract. 'Who will now defend the virtue and purity of science?' they wail. By contrast, there also stand the massed phalanx of neoclassical economists, science policy specialists and their bureaucratic allies, who by and large tend to reverse the valences, but nevertheless engage in much the same forms of discourse. For them, most scientists in the Bad Old Days had been operating without sufficient guidance from their ultimate patrons, the corporate pillars of the economy; but luckily, with a bit of prodding from the government, a friendly nudge from their university's intellectual property officer, plus a few dollars more waved in their directions, scientists have been ushered into an era which appreciates the compelling logic of "technology transfer." At the risk of caricature, one might summarize their central task as the gathering of empirical data in order to argue that the expanding modern commercialization of scientific research has turned out to be 'inevitable,' with the corollary that there exists little evidence that it has "significantly changed the allocation of university research efforts" (Nelson, 2001, p.14).[2]

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[1] See for instance: (Brown, 2000; Hollinger, 2000; Miyoshi, 2000; Newfield, 2003; Krimsky, 2003; Monbiot, 2003; Washburn, 2005). Editor: Please cite here Handbook entry that covers Mertonian norms.

[2] For similar assessments: the "growing commercial engagement has not, thus far, altered the research culture of universities, so as to privilege applied orientations at the expense of basic science" (Owen-Smith & Powell, 2003, p.1696); "Science today is only a short way down the path to becoming a toady of corporate power" (Greenberg, 2001, p.3); Do we see "universities compromising their core values [?]... at least at the major research universities, their revenue-enhancing activities have not seriously distorted such values" (Baltimore, 2003, p.1050); "There is evidence to suggest that university licensing facilitates technology transfer with minimal effects on the research environment" (Thursby & Thursby,

Admittedly, many of these purveyors of glad tidings would still regard themselves as arguing in favor of the preservation of an 'optimal' sphere of research reserved for open public science and pure unfocused curiosity (a 'separate but equal' doctrine applied to unspecified portions of the university), however much they would also avow that the economy must constitute the ultimate arbiter of scientific success in this more rational regime of organization. The history of science for them is simply divided into an Age of Confusion when 'open science' had unaccountably been mistakenly conflated with the whole of science, fostering a lack of understanding of the efficient organization of systems of innovation, and our own current Age of Free Enterprise, when we see the true situation of pervasive ownership with clarity. This kind of crude "before and after" discourse has also come to dominate much of the contemporary science policy literature, which is filled with euphemisms like 'technology transfer' and 'democratically responsive science' which seek to reconcile the harsh authority of the almighty dollar with the delicate sensibilities of those otherwise inclined to resist the advent of the End of History. It has become fashionable of late to pillory Vannevar Bush for his invention of the notion of the pipeline 'linear model' which situated 'applied science' as the downstream result of 'basic science'; now we are all supposed to know better.[3]

This rather superficial Stage 1/Stage 2 narrative, be it upbeat or downbeat, has very little to do with the actual histories of the sciences. Sometimes this has become a problem in some sectors of STS as well, as we discuss below in Section 3. Part of the problem arises because STS has only very recently begun to come to grips with the phenomenon of commercialization, lagging the Cassandras and the science policy bureaucrats by perhaps a decade or more. The 'commercialization of science' turns out to be a very heterogeneous phenomenon, resisting simple definition. Consequently many contemporary discussions of the commercialization of science have proven deeply unsatisfying, tethered as they are to totemic monolithic abstractions of Science and The Market pushing each other around in Platonic hyperspace. Indeed, some historians have long sought to remind their readers of what one collection (Gaudilliere & Lowy, 1998)

2003, p.1052). Richard Nelson has tended to become more skeptical of this position over time, however; see (Nelson, 2004).

[3] Some recent evidence begins to suggest that Bush may not have been as directly responsible for these ideas as has been previously thought, but that they might instead be traced to the economist Paul Samuelson, who helped draft *Science, the Endless Frontier*. See Samuelson (2004, p.531).

calls "The Invisible Industrialist" who occupied the interstices of numerous laboratories and frequented the hallways of universities since the middle of the 19th century. Yet, in rejecting the false polarities of the neo-Mertonians on the one hand and the economic apologists for the modern era on the other, it would appear that the denizens of science studies have of late run a very different risk of denying that there has been any significant change whatsoever in scientific protocols; hence important structural differences are overlooked that might be traced to alterations in the ways in which science has been paid for and accommodated within the economy over long stretches of time. One recent instance of this sort of attitude has been expressed by Steven Shapin (2003, p.19):

> Throughout history, all sorts of universities have 'served society' in all sorts of ways, and, while market opportunities are relatively novel, they do not compromise academic freedom in a way that is qualitatively distinct from the religious and political obligations that the ivory tower universities of the past owed to the powers in their societies.

A cruder version of this orientation was captured in interview transcripts with the chair of an electrical engineering department (in Slaughter, Archerd & Campbell, 2004, p.135):

> You have to accept the fact that it [research] is going to be driven by the people who give you the money. [If] the state gives us money, they tell us what to do. [If] NSF gives us the money, they tell us what research they want done. [If] DoD gives us the money, [its] the government… Why is it any different with industry? I see no difference whatsoever.

Yet another manifestation is the attempt by the Paris School of Bruno Latour and Michel Callon to reduce the economy to just another instance of the laboratory, as a prelude to erasing all ontological differences between scientific and economic activity, while chanting, "we have never been modern!"[4] Strangely, this widespread ahistorical insistence upon 'the way things have always been' in science in its co-existence with the economy dates back to the supposed godfather of social studies of science, Thomas

[4] See Callon (1998; in Mirowski & Sent, 2002; in Barry & Slater, 2003), and for critique, (Mirowski and Nik-Khah, forthcoming). One might also include Capshew & Rader (1992) in this tendency to see "Big Science' back to the 17th century, although they do have the good grace to notice: "Aside from a surge of interest in the rise of industrial research, surprisingly little attention has been directed towards the economics of science… The organization and production of knowledge in such contexts bears more than a passing resemblance to Big Science" (p. 24).

Kuhn.[5] In a little-read set of comments upon a pivotal conference upon the relationship of industrial R&D to science held at Minnesota in 1960, he insisted that "the two activities, science and technology, have very often been almost entirely distinct," and indeed, that "historically, science and technology have been relatively independent enterprises," going back as far as classical Greece and Imperial Rome! As an historian, Kuhn felt impelled to admit that,

> "Since 1860… one finds that characteristic 20th century institution, the industrial research laboratory… Nevertheless, I see no reason to suppose that the entanglements, which have evolved over the last hundred years, have at all done away with the differences between the scientific and technological enterprises or with their potential conflicts." [6]

The indisputable fact that scientists and their institutions have always and everywhere been compelled to 'sing the prince's tune when taking the prince's coin' in one form or another does not imply that the evident modern trend towards the escalated and enhanced commercialization of science need not or will not alter the very makeup of the supposedly invariant 'scientific community,' not to mention the nature of the 'outputs' of the research process. Furthermore, the under-appreciated fact that the political economy of the sciences in America has been transformed from top to bottom at least twice over the past century has yet to be correlated with the types of science that have been performed in the manner which has become the trademark of science studies – that is, fine-grained studies of the interaction of forms of organization with the stabilization of knowledge claims—or indeed, the ways we tend to think about the successful operation (or conversely, the pathologies) of the 'scientific community'. This sort of agenda was called for in the perceptive paper of Michael Aaron Dennis in 1987; but his entreaty has yet to be sufficiently heeded.

Close on the heels of the enunciation of the 'Hessen Thesis' in the 1930s [7] and the subsequent Cold War anti-Marxian backlash against it, most appeals to economic

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[5] In this respect, as in many others, we owe our awareness to the politically retrograde influence of Kuhn upon the questions posed within science studies to Steve Fuller. See (Fuller, 2000; Mirowski, 2004a, chap. 4).

[6] The first two quotes can be found in (NBER, 1962, p.452); the third comes from pp.454-5.

[7] Named after Boris Hessen, this was an argument that the content of Newtonian mechanics owed much to the artisan traditions and economic structures of the 17th century. For modern research on the subsequent suppression of Hessen's work and attendant economic considerations, see (McGuckin, 1984; Chilvers, 2003; Mayer, 2004). Editor: Please cite here Handbook Entry that may deal with Hessen.

structures as conditioning factors in the production of science simply dropped out of postwar theoretical discourse within science studies. As Dennis has written about American historians, the manner of "solving the problem of providing for the support of the material foundations of science – salaries, labs, instruments – effectively eviscerated the possibility of anything even remotely resembling the materialist historiographies of science that had developed between the wars" (1997, p.16). Something similar seems to have happened in Europe as well. The postwar political shift in the philosophy of science also played a part in repressing such questions (Mirowski, 2004a; 2004b). Consequently, as the next Great Transformation of research was taking place in the 1980s, science studies was instead turning its attention to micro-scale studies of laboratory life, ignoring how the laboratory's macro-scale relationship to society was being re-engineered all around them, not to mention the shift in those paying for all those DNA sequencers and inscription devices.[8] The qualitative effects of the panoply of market activities upon scientific research thus remain very much an open issue.

Curiously, expressions of concern over the potential impact of economic incentives upon science have instead become the province of groups who have tended to set themselves up in opposition to STS. Predictably, they frequently wind up their exercises by concluding that commercialization has not drastically changed contemporary science. Positing the invariance of the end-state from the mode of production of knowledge has itself become a veritable industry amongst those anxious to provide reassurance that their 'social epistemology' underwrites an invisible hand story in the sphere of scientific research: as they phrase it, that epistemically sullied motives (which are then abruptly conflated with 'social influences') do not threaten the goals of science.[9] These attitudes have taken root in the science policy community and a segment of the

[8] This problem of this blind spot of science studies approaches to the laboratory has recently been noticed by (Kleinman, 2003, chapter 5). There is the further complication that early works such as (Latour & Woolgar, 1979; Bourdieu, 2004) made extensive use of economic metaphors, while essentially ignoring any substantive economic structures. The curiosum of resort to economic metaphors while avoiding the economy is nicely discussed in (Pels, 2005; Hands, 2001). Nevertheless, the range of reasons behind the neglect of economic factors by STS in the 1970s-90s cannot be covered in this venue. The few exceptions to the general disinterest will be covered in section 3 below.

[9] The most prominent advocate of such a position in philosophy is Philip Kitcher (1993), but it can also be found as far back as the work of Friedrich Hayek in the 1930s, which gives some idea of its neo-liberal origins. It is also showing up in the analysis of higher education: see (Geiger, 2004; Kirp, 2003; Apple, 2005). For further contemplation and critique on this issue, see (Mirowski, 2004a; 2004b; forthcoming).

philosophy of science (Mirowski, 2004b; 2006b), and pervade discussion of commercial research in business schools.[10]

A different approach to the "new economics of science" explores the possibility that alternative forms of the commercialization of science actually have indelibly shaped both the practice of research and the contours of whatever it is that we encounter at the end of the process (Mirowski & Sent, 2002). One key variable turns out to be the ways in which that protean entity "the laboratory" was appropriated and reconstructed by higher education, corporations and the government over the long 20th century, a point first made by (Dennis, 1987). This will be the topic of Section 2 below. Another crucial variable is the way in which the divide between 'public' and 'private' conceptions of knowledge have shifted in the recent past, and how that has fed back upon the rationales for various actors in their exercise of the governance of science (Slaughter & Rhoades, 2004). Section 3 is an overview of this problem. A third variable concerns the modern phenomenon of globalization, and the ways in which it tends to undermine earlier nationalist and parochial approaches to the problem of the economics of science and the notion that there might persist 'national systems of innovation' (Drahos & Braithwaite, 2002; Drori et al, 2003). In one sense, it poses an immanent critique of the previous sections, which is explored further in Section 4.

Many different groups have entered the fray in asserting their expertise to frame discussions of the modern commercialization of science. One can find examples in such far-flung enterprises as literary criticism (Newfield, 2003; Miyoshi, 2000), medical schools (Angell, 2004), library science (Scheiding, forthcoming), education schools (Apple, 2005; Slaughter & Rhoades, 2004), and popular journalism (Press & Washburn, 2000; Shreeve, 2004; Dillon, 2004; Judson, 2004; Washburn, 2005). Some political theorists have attempted to adapt the 'social contract' literature in politics to discussions of regime change (Guston & Kenniston, 1994; Hart, 1998). Some fields (say, for instance, 'knowledge management' specialists in business schools, intellectual property lawyers in law schools, or political economists in science policy units) highlight certain

[10] See, for instance, (Cohen & Merrill, 2003; Leydesdorff & Etzkowitz, 2005; Tijssen, 2004; Fuller, 2002) and recent issues of the *Journal of Technology Transfer*. Few are aware that such neoliberal attitudes towards science date back to the 1950s, when members of the Mont Pelerin Society first conceived of the ambition to "get the state out of the knowledge industry." For discussion, consult (Friedman, 1962, chap.6; Walpen, 2004).

facts about the changing status of science, but neglect other equally salient facts, say, from legal history, the politics of education, the annals of military procurement, or international trade policy. Other scholars, by suggesting that advanced economies were becoming increasingly 'weightless', would graduate to a third stage of capitalism consisting almost exclusively of the service sector, or indeed disengage from gross physical production processes altogether. Of course, most people recognized that much of that talk bordered on delusional, but nevertheless managed to appear sensible (or at least fashionable) by engaging in locutions such as "The Information Society" or "The New Knowledge Economy".[11] Frequently, appeal to this supposed novel entity served as a prelude to promote subsumption of science under a more general theory of the 'marketplace of ideas' (Foray, 2004; Feldman et al, 2002; Mirowski, forthcoming).

One might justifiably wonder if the cacophony of voices adds up to much more than a generalized atmosphere of anxiety. If STS is to claim to stake out a distinctive approach to the phenomenon of the modern commercialization of science, then it will need to make a fateful choice between casting the 'constructivist' stance as one treating the entirety of science as just another form of marketing (Woolgar, 2004), or else one stressing the essential historical instability of the commercial/communal binary as instantiated in actual concrete practice. In this chapter, we stand as advocates of the latter position. Hence we outline *one version* of an STS approach to commercialization in Section 2; and then contrast it to some other versions in Section 3 of this chapter.

Once the ground has been prepared in section 2 by an analytical scheme of temporal periodization (albeit one grounded primarily in the American context), we shall then point out the differing meanings of the commercialization of science under each individual regime. While market considerations were never absent from the laboratory or the classroom, the modern commercialization movement can in no way be considered a 'return' to anything like the interwar science promoted by Jazz Age captains of industry.[12] Modern science has turned out to be a qualitatively different phenomenon

[11] For some economic examples, see (Danny Quah in Vaitilingham, 1999; Shapiro & Varian, 1999; Powell & Snellman, 2004). A Google search on the term "Knowledge Economy" in October 2005 produced an amazing 1,690,000 hits.

[12] We believe it is important to counter such claims as: "the weakening of university-industry research linkages during a significant proportion of the postwar [WWII] period was the real departure from the historical trend" (Mowery et al, 2004, p.195 fn15), and "The so-called Mode 2 is not new; it is the

because it has been grounded in profound historical transformations in the corporation, the university, and government, with consequences for their respective initiatives to exercise control in the organization and funding of science. We offer our limited exercise in this chapter more as a preliminary exemplar than a definitive template for research into other countries in other eras: one future task of STS might be to report similar species of watersheds in other disparate culture areas.[13] Whether or not that comes to pass, the other question raised by this chapter is: To what extent does modern globalization force the multiplicity of social trajectories of the provisioning of science to converge to a single, worldwide model of commercialized science in the 21st century? If the response to that question is in the affirmative, then should we also expect the intellectual rationales for a particular mode of commercialized science to similarly be winnowed down to a few simplified narratives of 'scientific success'? If that is the case, then one begins to appreciate the challenge that a neoliberal "new economics of science" poses to the future of STS. If broad generalizations indeed become possible, then they will exist because corporations and governments and INGOs have been engaged in a concerted project of standardization spanning national and cultural and disciplinary boundaries.

2. Three Regimes of 20th Century Science Organization

STS scholars have been wary of reifying the concept of "Science" as a trans-cultural trans-historical category, and for good reason. The more one learns about scientists and their livelihoods, the more one comes to appreciate the sheer diversity of their activities, the vast compass of their societal locations, and the multitude of ways their findings have become stabilized and accredited as knowledge. What keeps this daunting multiplicity from defeating analysis for STS scholars is the dominance of

original of science before its academic institutionalization in the 19th century" (Etzkowitz & Leydesdorff, 1998, p.116). STS should be devoted to explanation of what is especially characteristic of the new social structures of science.

[13] Indeed, some of the best modern work in STS exploring the interface of science and political economy takes an explicitly comparative perspective. See, for instance, (Daemmrich, 2004; Wright, 1994; Jasanoff, 2005). We do not subject the European laboratory to the same regimes analysis as the American situation, due to space and research constraints. However, we do try and indicate some of the modern implications of the analysis for the European context in Section 4.

certain identifiable institutional structures involved in organizing scientific inquiry in the modern period. Scientists have not subsisted as a purely self-organized discourse community, contrary to the rhetoric dominant during the Cold War era. Rather, they have always been enmeshed in complicated alliances with and exclusions from some of the dominants institutions of our era: primarily, the commercial corporation, the state, and the university.[14]

The story of the quotidian activities of the scientist always presumes some social scaffolding of communal support, which in the modern epoch has been most frequently built up from corporate, governmental and educational [CGE] elements. Furthermore, various individual scientific fields will be experiencing relative growth or stagnation, depending upon the particular historical configurations of their own intellectual trajectories, in combination with the levels of encouragement provided by the CGE sectors. To render this set of propositions more concrete, we provide below in Table 1 a schematic outline of the three regimes of science funding and organization in the United States in the 20th century, based upon our reading of the relevant economic and social history, as well as the contributions of historians of science. To keep the historical sketch from becoming unwieldy, we have restricted the table to indications of CGE developments that would have direct bearing upon the constitution of the "laboratory" in scientific research; considerations of length preclude extension of the CGE analysis to, say, clinical medicine, field sciences, or purely abstract mathematical endeavors (although we believe these would be amenable to similar periodization). The purpose is to elevate to consciousness the fact that the corporation, the legal framework and the university have not been static over time, and that their alterations can be directly related to the ways in which scientists have made their livelihoods and pursued research agendas promoted by their immediate patrons. Thus, contrary to the prognostications of social scientists, there has been no single 'Market' that governed the evolution of science in America; rather, there have been multiple formats of provisioning, embedded within larger structures.

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[14] We signal here our awareness of the literature that seeks to acknowledge their importance under the rubric of the "Triple Helix". We shall briefly evaluate its contribution in Section 3 below, showing how the regimes analysis differs from their narrative. Section 4 complicates the analysis by adding a fourth actor, viz., the international agency. Editor: please cite here Handbook Entry dealing with definitions of Science.

Table 1

American Regimes of Science Organization
In the 20th Century

eriod, Regime	Corporation evolving	Government Corp. policy	Government Science policy	Science managers	Higher education	Pivotal disciplinary science
890- WWII aptains of rudition egime	1895-1904 great merger movement: Chandlerian firm of 'Visible Hand'. Innovation of in-house R&D labs to control competition.	Massive expansion of corporate prerogatives. Corps become legal agents; patents a major strategic tool. Beginning of anti-trust. Employers own research of employees.	Almost non-existent. NRC formed as trade assoc. lobby for natural sciences. General suspicion of government involvement. NRE fails. Wartime patent bounty.	Charismatic PhD directs corp labs. Foundation officers run few elite univ. grant programs (on corporate principles).	Mostly Elite liberal arts. Research subordinate to pedagogy. Science not a major priority. Foundations attempt reform. Labs founded.	Chemistry, Electrical engineering
WII- 1980 old War egime	M-form, conglomerate diversification. R&D units as semi-autonomous revenue earners (due to military contracts). Regulatory capture.	Corporate powers augmented; antitrust strengthened. IP weakened. Military contracts as industrial policy.	Huge expansion Federal military funding and control. Military promotes basic science to defeat enemies. National labs. NSF as non-military face of 'pure' science.	Military primary sci managers for: research univs,, think tanks, nat'l labs, corporate contract research 'Peer review' a secondary inst.	Mass education at expanded research univs. Integrated teaching/research. Produce dem. citizens: academic freedom as Political statement	Physics, Operations research, Formal logic.
980- ? lobalized rivatization egime	Breakdown of Chandlerian model. Retreat from vertical integration, diversification. Corps outsource R&D, spin off in-house labs.	Transnational trade agreements expand corp powers to circumvent national control. Antitrust weakened; IP vastly expanded.	Privatize publicly funded research: Bayh-Dole, etc. Kill OTA. Science just one political resource amongst many.	Globalized corp officers control: univs, hybrids, CROs, startups.	Stock up human capital for those who can pay. Only entrepreneurs are free. Sever the teaching/research connection.	Biomedicine, Genetics, Computer science, Economics.

The designations we have provided for the various regimes are predicated upon popular characterizations found in the existing historical literature. The 'Captains of Erudition' regime is so designated in honor of Thorstein Veblen (1918), who wrote one of the earliest descriptions of the American research university as becoming subject to specific corporate organizational principles; it also bows in the direction of the dominant American school of business history based on the work of Alfred Chandler.[15] The label indicates a very elitist and closed corporate model of the organization of science. The 'Cold War' regime is a label now regularly used to designate what many now portray as a fleeting interlude of military dominance over science management in the period beginning in WWII.[16] And the terminology of "globalization" is not so much an appeal to a fashionable concept in contemporary social theory, as it is an insistence upon a set of factors indispensable for an understanding of the forces that drive the current wave of commercialization of science.

The Genealogy of the American Laboratory

Laboratories were not something that just naturally appeared in the American landscape: they had to be built, and to be able to subsist as more than ephemeral entities, they had to be integrated into some sector of the economic infrastructure. Unlike the situation in Europe, large-scale laboratory science did not originate in the university sector in America. Rather, from the outset, it was very much a commercial initiative.

The broad outlines of the rise of the industrial research laboratory are now well known.[17] Everyone concedes that its origins are to be found in continental Europe, primarily but not exclusively in Germany, and that it was initially located in large firms engaged predominantly in what has become known as the "second industrial revolution": chemicals, electrical machinery, railways, and pharmaceuticals. An earlier vintage of historiography tended to assert that the "science-based industries" simply summoned an implicit exigency to incorporate research activities within their ambit, in both Germany and the US; but modern historians have since grown much more cautious, realizing that the ingredients to explain the appropriation of what had previously been a specialized

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[15] "The organizational approach to understanding big business developed by Alfred Chandler, Jr. has given historians a framework within which to place the research laboratory" (Smith, 1990, p.121). The Chandlerian approach to business history is best represented in (Chandler, 1977; 2005b).

[16] "The postwar R&D system, with its large well-funded universities and Federal contracts with industry, had little or no precedent in the pre-1940 era and contrasted with the structure of research systems in other postwar industrial countries. In a very real sense, the US developed a postwar R&D system that was internationally unique" (Mowery & Rosenberg, 1998, p.12).

[17] See, for instance, (Fox & Guagnini, 1999; Shinn, 2003; Hounshell, 1996; Budieri, 2000, chap. 2; Mowery, 1981, 1990; Swann, 1988; Smith, 1990).

pedagogical device for industrial purposes would be found in a strange brew of state policy towards advanced education, ideologies of state-building and political rectitude, the rise of various notions of intellectual property, the conditions which gave rise to large and powerful corporations in particular political settings, and the ambition to exert control over burgeoning transnational mass markets in clothing, transport and communications, electrical equipment and patent medicines. Whereas most manufacturing firms had long made provisions for internal quality control, routine testing, and incremental process improvement, an innovation arose around the 1870s to expand the purview of these specialized corporate arms into patent protection, the bureaucratization of trade secrets and the generation of novel processes and products. It resembled a phase transition between the periodic use of the sciences for corporate purposes to something approaching the institution of bureaus dedicated to *doing* science for corporate purposes. The distinction was not always sharp, the results were not often that immediately striking, and the transition was not always conscious.

The rise of the industrial laboratory was the consequence of an American pincers movement: on the one hand, a push to bureaucratize and industrialize (or vertically integrate backwards, as economists might say) something which heretofore had been conceived as the ineffable capacity of the individual genius, and on the other, a pull to adapt a purpose-built academic social formation to corporate imperatives which itself had only recently been stabilized in specialized educational settings for pedagogical purposes. Michael Dennis correctly points out that when later 19th century American figures made their pleas for "pure science", they did not refer to some notion of disembodied science carried on for its own sake, nor to an imaginary autarkic scientific community defending its prerogatives, but rather to a pedagogical ideal for a species of hands-on higher education where teaching and research were combined in a setting relatively sheltered from commercial considerations. Pace Bruno Latour, the issue was not whether the denizens of laboratories or their proxies 'circulated' in the wider world, but rather whether laboratories themselves were a robust phenomenon that could be severed from the nascent research university and successfully grafted onto the multidivisional corporation. The wrenching estrangement of the laboratory from its teaching functions constituted so dramatic a departure from its conceptual origins in the later 19th century that it was not hard to find any number of academics expressing scorn for the new-fangled industrial laboratories and their spiritually debased inhabitants, disparaging the public confusion of untutored tinkerer-inventors with real 'scientists'. Yet it would be an anachronism to read these as indicative of some transcendental incompatibility of science and commerce, as Kuhn did. Rather, it makes more sense to approach them as symptoms of conflicts attendant upon institutional innovations in the construction of both the public and private spheres, still in their early stages.

A] The Captains of Erudition Regime

One of the most salient differences between the German situation and its American counterpart circa 1900 was that, by and large, the academic research laboratory did not substantially predate the rise of the industrial laboratory in the United States.[18] Higher education in the natural sciences and the social sciences was acknowledged to have been superior in the German setting at the beginning of the 20th century; it was also recognized as having attained an unprecedented level of state-sponsored centralization. The German university had pioneered the research seminar and the research laboratory; the pedagogical research laboratory had not yet become solidly established in American universities, which were predominantly devoted to moral uplift and liberal arts education for a narrow elite, although the forms this assumed were widely decentralized and diverse.[19] As David Noble put it, in the 19th century 'shop culture' was deemed opposed to 'school culture' (1979, p.27); if anything, the universities lagged behind firms when it came to building and staffing labs. Indeed, far from being transplanted bodily from an academic to a corporate context in the US as it had been in Germany, the American scientific laboratory was built up almost from scratch, modulo some Germanic inspiration, more or less simultaneously at both sites. For instance, as early as 1881, American Bell Telephone experimented with the location of a new physics laboratory,

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[18] According to (Smith, 1990, p.124), "broad-ranging research in German dye companies began in 1890… yet these research programs do not appear to have been a model that American companies emulated."

[19] A partial exception to this generalization is the agricultural experiment station attached to state land-grant universities from the 1870s onwards. Their indeterminate status as scientific research poles is discussed by Charles Rosenberg in Reingold (1979). An attempt to project the extension service onto the industrial sector in the form of a bill to establish 'engineering experimental stations' at land grant colleges in 1916 was easily defeated (Tobey, 1971, p.40; Noble, 1979, p.132).

offering Harvard University the money to build it, as long as “professors could use university laboratories in work for private companies” (Guralnick in Reingold, 1979, p.133). MIT’s fabled ‘Research Lab for Applied Chemistry,’ originally intended to carry out industrial research, dated from 1908. Since dedicated university laboratories were rare, the academic/commercial distinction was less than distinct. Yet the siting of industrial research on college campuses often proved less than satisfactory for its patrons, mostly due to a perceived insufficiency of corporate control (Lecuyer, 1995, p.64), redoubling the formation of in-house laboratories. This made for an unusual political economy of science in early 20th century America, going some distance towards explaining a certain impression of ‘exceptionalism’ in the culture of science which one encounters among many commentators (Wright, 1999), and one that contributed to the fact that American scientific research achieved an advanced level of commercialization far more quickly than any other country by the 1930s. It also coincided with the successful elevation of a subset of the natural sciences to world-class status for the first time in the United States, thereby raising the intriguing prospect of the existence of multiple institutional paths to the fortification of a research base in the course of economic development of national systems of research.

Science in the American university system had gained a foothold comparatively late, around the beginning of the Erudition regime.[20] The highly decentralized character of the American higher education sector at first posed an obstacle to the development of a scientific curriculum, although it would later prove a boon. While later historians might point with pride to the earlier founding of Harvard’s Lawrence School, the Yale Sheffield School or the Massachusetts Institute of Technology, the impact of these and other educational institutions upon actual practices of research and the shape of American science were slim to negligible prior to the 1890s. The impetus for the change in regimes originated instead from within the corporate sector, initially in the creation of a new kind of in-house laboratory for commercialized science, but later in the export of corporate protocols and funding structures to some handpicked research universities, by way of the instrumentality of a few activist foundations. Hence our brief overview necessarily begins with a flyover of the relevant background history of the corporation.

[20] See, for instance, Stanley Guralnick in (Reingold, 1979).

American historians of technology have tended to lean on the work of Alfred Chandler, and in particular his book *The Visible Hand* (1977), to provide the framework within which they situate their understandings of the rise of commercialized science. This turn of events has been slightly incongruous, partly because Chandler devotes very little explicit discussion to the role of industrial laboratories in his history, but also because it is predicated upon a fairly old-fashioned technological determinism (Chandler, 2005a). Set against an earlier literature that approached the corporation as a nexus of power growing dangerously out of control, Chandler portrayed the rise of the large American corporation around 1900 as a rational organizational response to technological imperatives of high-throughput capital intensive patterns of production, found primarily in the newer science-based industries, which could only be made viable through the parallel construction and organization of mass markets on an unprecedented scale. Chandler praised the Jazz Age mega-corporation for adopting centralized bureaucratic managerial structures and vertically integrating backwards into inputs and forwards into sales, advertising and market research. Although he did lightly touch upon the rise of the industrial laboratory (eg., 1977, pp.425-33), it is treated as just another exemplar of the line-and-division managerial structure to which Chandler sought to attribute the success of firms such as Standard Oil, General Electric and DuPont. Hence, Chandler did not so much proffer an explanation of the rise of the industrial research laboratory, as mutely point to one necessary bureaucratic prerequisite for its coming into existence. Some industries could have sought to 'integrate backward' into research, except for the inconvenient fact that in most cases there were no pre-existent stable structures for them to integrate backwards into.

The Chandlerian narrative in science studies (Smith, 1990) should therefore be supplemented by legal and political considerations, which Chandler largely shunned. The limited liability corporation, far from being an established fixture on the American scene, had just undergone a period of substantial judicial fortification at the end of the 19th century, due to the infamous Santa Clara non-decision extending 14th Amendment rights to corporations (Nace, 2003), the race to the bottom of states to liberalize corporate charters, and the unprecedented merger movement of 1895-1904. This sudden arrogation and consolidation of power had not gone unnoticed, and had begun to provoke a counter-movement beginning with the Sherman Antitrust Act of 1890 and continuing with the Clayton Act of 1914, and provoked political movements hostile to corporate dominance of the economy in the Progressive Era. The rise of the American industrial laboratory should be situated within this context, in order to appreciate some of its more distinctive characteristics, as well as its impact upon academic science.

The standard popular account portrays the *fin de siècle* industrial lab as a sort of factory of innovation, churning out gadgets that became new products or improved production processes on demand for the corporate hierarchy. This was the image promoted by the Scripps Science News Service, the very first corporate-backed 'public relations of science' initiative, which began in 1921 (Tobey, 1971, chap.3). But the more recent literature resists this tendency to frame the lab either as a straightforward invention factory or as some university-science-department-in-exile[21], and for good reasons. The prime directive behind many of the innovations growing out of the large corporation was the drive to control markets, to render unforeseen events manageable, and to stifle competition. As the government began to block direct attempts at market control such as explicit cartels, pools and other tied arrangements through its initiatives such as anti-trust prosecutions, the locus of corporate control began to shift to indirect arenas such as intellectual property, the imposition of technical standards, and the like. One primary reason that large corporations turned their attention to bringing scientific research within their walls in this period is that "invention and innovation were effective defenses against antitrust suits" (Hart, 2001, p.926), and that patents in particular but intellectual property in general were conceived as the best and most effective means of controlling competition in the early 20th century (Noble, 1979, p.89). This trend was actively promoted by certain US government policy moves, such as the seizure by the Alien Property Administration of German patents in 1919, and their licensure to American firms under very favorable terms (Mowery, 1981, p.52; Steen, 2001). As both case law and legislation were slanted in the direction of integrated corporate organization instead of inter-firm cartels (or other features of the German model[22]), "legal doctrine

[21] See (Wise, 1980; Reich, 1985, Dennis, 1987; Hounshell, 1996)

[22] The early 20th century American stress on suppression of inter-firm governance of markets may account for the substantial differences in the German and American climate, which were treated as a puzzle by (Mowery, 1990, p.346).

inadvertently spurred corporate consolidation, and the consolidated corporations in turn, enhanced their investments in R&D… The birth of the central corporate laboratories in this period…. are therefore in part the product of antitrust law" (Hart, 2001, p.927).

Legal redefinitions of intellectual property and clearer stipulations as to who might assert claims over the fruits in the case of scientific research were heavily conditioned by the shifting needs of the fortified corporation. In a move with untold consequences for the future organization of science, corporations managed to have the case law with respect to employee inventions shifted away from older labor-theoretic notions of the fruits of individual genius and towards a presumption of employers' ownership of *anything* an employee might do or invent. Prior to the 1880s, the standard default rule was that rights to inventions were vested in employees; but first, through the creation of the doctrine of 'shop right' in the 1880s-1910s, and afterwards, through a series of judicial decisions which made direct reference to corporate research laboratories, the presumption of ownership was shifted decisively to the firm itself (Fish, 1998). Corporate initiatives then fed back upon general cultural images: by the early 1920s, American court decisions began appealing to the apparently commonly accepted notion that invention and science was a 'collective' and not an individual phenomenon.[23] Nobelist Robert Millikan began to complain in the 1920s that the German research university did not sufficiently respect the collective character of scientific research (Tobey, 1971, p.219). However, the convenient notion of the 'collectivity' was not to be allowed to exude too far outside of the firm's boundaries (as in the writings of Thorstein Veblen), for that might bring back the dreaded world of cartels, patent pools, plunderbunds and trusts. The legal bias against cross-firm combinations and joint ventures bore direct consequences for the existence and viability of corporate labs that might try and escape from the tentacles of corporate bureaucracy. While free-standing independent industrial labs were also founded in this period, they never caught on or expanded to the extent that in-house industrial research did; unlike some of the largest in-

[23] See, for instance, the 1921 *Wireless Specialty* case (Fish, 1998, p.1176). Further, the metaphor which compared exploration of the laws of nature to exploration for minerals or gas deposits, which would dominate Kenneth Arrow's Cold War 'economics of innovation' (in NBER, 1962) was already present in the 1911 *National Wire* decision (Fish, 1998, p.1194). The legal acceptance of the ideal of collective science in the 1920s then dovetails quite nicely with recent claims that the 'theoretical' treatments of the scientific community as a distinctly social entity in philosophy and sociology find their origins in the 1930s (Jacobs, 2002; Mirowski, 2004b).

house labs, they never conducted any world-class science; and moreover, they undertook contract work that did not mimic the big corporate labs, but was most often subordinate and supplementary to them.[24] Thus, even though the research process was clearly becoming commercialized, it was not rendered so thoroughly fungible to the extent of being freely outsourced by its corporate sponsors. (The modular 'marketplace of ideas' turns out to be a much more recent phenomenon.) Hence, the particular form assumed by contract research in America was (and continued to be) heavily conditioned by industrial policy and intellectual property conventions.

After the first generation of the Captains of Industry who had built or consolidated the massive industrial corporations had retired, or otherwise cashed out some of their gains, they or their family members decided to devote some funds to philanthropy (or perhaps merely engage in tax avoidance) through the creation of various foundations: the Russell Sage Foundation (1907), the Carnegie Corporation (1911), the Rockefeller Foundation (1913) are some of the better known. Assistance to higher education had become part of their agenda, but there arose serious questions as to the most appropriate way to pursue this goal. At first, grants were patterned upon other philanthropic practices, and when it came to academic recipients, they were pitched to essentially provide temporary individual outdoor relief to indigent or otherwise needy scholars. However, just as in the case of intellectual property, by the 1920s the focus on the isolated individual as the monad of science funding had gone out of fashion, and attention turned to the targeted application of funds to provide research endowments for continuing programs, reorient whole disciplines, and build new institutions. It was consistent with this vision that the grants were overwhelmingly channeled to private universities, and structured so as to concentrate 'excellence' in a few powerful institutions. As Robert Kohler put it most succinctly: "The large foundations were… carrying business methods and managerial values from the world of large corporations into academic science" (1991, p.396). In everything from recasting the research grant as a contract that imposed certain standards of bureaucratic accountability, to imposing the line-and-division managerial structure upon university administrations and departments,

[24] See (Mowery, 1981), and the claim by (Mowery, 1990, p.347): "independent research organizations do not appear to have substituted for in-house research."

to encouraging the creation of teams of researchers, the corporate officers who staffed the large foundations tended to foster the standards and practices of the large American corporation within their flagship research universities. As E.B. Croft of Bell Labs put it,

> It might appear that it would tend to destroy the initiative of the individual; that it would make it difficult to properly assign the credit and give the reward to the individual worker. These are all problems of administration that have had to be worked out. First of all we must establish in the individuals a state of mind, which leads them to really believe that their best results are attained through cooperation with others (Noble, 1979, p.119).

Harvard and Chicago would be coaxed and inspired to become the AT&T and Standard Oil of American higher education, surrounded by smaller and relatively insignificant rivals who had not learned the lessons of building a permanent and successful managerial hierarchy, and not inconsequentially, a strong research capacity. Colleges would either emphasize liberal-arts pedagogy, or would aspire to technical expertise in research. Consequently, the scientific research laboratory was propagated throughout the academic landscape as the necessary accessory to the mature corporate business plan. “Foundation managers allied themselves with the small but growing numbers of academics… who realized that [corporate] organization and management were good ways to keep ahead of the pack in the increasingly crowded and competitive world of basic research” (Kohler, 1991, p.400).

The fact that much of the structure of the American academic science laboratory was inspired by that of the industrial research lab did not imply that academic scientists uniformly sought to mimic their industrial brethren, however. Even as the social structure of laboratories was becoming patterned upon corporate social structures, the academic scientists lauded the university laboratory as a pedagogical ideal existing separate and apart from commercial pressures, but also from government subsidy. Yet this quest for ‘purity’ only exacerbated the problem of who precisely would fund and manage the research carried on under that banner. The nagging tension between science beholden to special interests versus science in pursuit of the public interest proved a challenge to those who apprehended the “Erudition” dynamic as a danger to democracy, such as Walter Lippmann, Thorstein Veblen and John Dewey (Mirowski, 2004b). The Foundations were increasingly targeting their funds to support very specific research projects in a limited portfolio, or else professionalized arenas of higher education such as medical schools, and could not be expected to bear the burden of the health of the whole gamut of sciences, much less the careers of the next generation of scientists. The National Research Council [NRC], established in 1916 as a sort of trade association to lobby for the support of the natural sciences, actually opposed direct government subvention of researchers (Noble, 1979, p.155). The NRC-backed drive to institute a National Research Fund, which would derive its endowment from corporate subscriptions, failed miserably in the period 1926-32 (Tobey, 1971, chap.7). Robert Millikan was denouncing federal support for the sciences at private universities as late as 1937 (Lowen, 1997, p.33); it remained miniscule. Outside of a few private universities favored by the foundations, the problem of sustained privatized care and maintenance of a diversified academic research capacity was not solved by the supposedly collectivized community of researchers, nor by its corporate patrons. It would not be solved until World War II.

Nevertheless, American laboratories for the first time in their history were able to produce some world-class science under the Erudition regime. Whether the Nobel Prizes were for work originated in the academic sector, as Theodore Richards' chemistry prize in 1914 or Robert Millikan's physics prize of 1923, or from within the burgeoning industrial sector, as that of Irwin Langmuir of GE in 1932 or C.J. Davisson of Bell Labs in 1937, there was a certain American style of research which traced a part of its lineage to the corporate inspiration of the laboratories. European commentators noted a certain empiricist temper regnant, a kind of phenomenological exploration well-suited to teams of researchers, infused with an experimental and accounting mentality as contrasted with a rationalist orientation. German dominance in both physics and chemistry were still widely acknowledged in this period. Electrical engineering, however, found its center of gravity shifting westward by the 1930s. Nevertheless, America's deficiencies with regard to theoretical imagination were a common theme of opprobrium emanating from the older and cultured precincts of Continental Europe. Chemistry, probably the most lavishly supported of the natural sciences in America in this era, itself produced no radical changes in fundamental doctrines (Mowery, 1981, p.104). One might therefore conclude that the corporate orientation of American science did indeed influence the types of research performed in this era, as well as some of the results produced. More to the point, when larger cultural movements felt impelled to come to terms with the world-historical significance of the advancement of science, most frequently it was European science that served as their reference point.[25]

B] The Cold War Regime

The fact that American science was utterly transformed in World War II, and then persisted in that novel economic format throughout the Cold War, is a widespread conviction hardly requiring defense at this late date;[26] but it does tend to get confused with another notion, that mostly this was due to the rise of 'Big Science': the idea that postwar science organization was driven by scale effects, in much the same way that Chandler asserted that the structure of the modern corporation has also been driven by scale effects.[27] But concentration upon abstract size and its quantification, a tendency often associated with Derek de Solla Price and the scientometric movement, serves in a way conformable with Cold War trends to lend itself to technological determinism. There is no doubt that the constitution of huge teams devoted to the production of a particular weapon or device, such as the MIT Radiation Lab, the Manhattan Project or Lawrence's cyclotron, could not help but provoke revisions in the way American culture would apprehend the nature of the 'laboratory' in the postwar period. Science seemed increasingly to be organized around 'gadgets', as the denizens of Los Alamos called the Bomb, and the devices were Big along almost any dimension one would care to assess: reactors, accelerators, space vehicles, von Neumann's room-sized computers, and so forth.

Yet, before we become blinded by the shiny surfaces, blinking lights and phalanxes of bench scientists, it will become necessary to direct our attention to some rather more pedestrian aspects of the quotidian prosecution of postwar science, namely, the myriad of ways in which the government, primarily but not exclusively in the guise of

[25] A good example of this is the way that various philosophical/cultural attempts to reconcile science with democracy or science with industry tended to struggle with Einstein's theory of relativity. For a nice account of these struggles, see (Tobey, 1971, chap. 4).

[26] See (Mowery & Rosenberg, 1998; Kevles, 1995; Leslie, 1993; Kleinman, 1995; Lowen, 1997; Morin, 1993).

[27] Examples of these sorts of arguments can be found in (Capshew & Reder, 1992; Galison & Hevly, 1992) and in the otherwise insufficiently appreciated pioneer researcher in economic themes of science organization, (Ravetz, 1971).

the military, transposed and inverted the previous understanding of the relationship between science and industry characteristic of the interwar period. The military, responding to a relative vacuum in science policy in the immediate wake of World War II, moved to retain access to the scientists whom had done so much in helping them to win the last conflict; and then when other governmental agencies were eventually brought into play, the political situation dictated that military innovations and military funding would remain the dominant consideration in science organization. The American government had destabilized the presumptions which ruled prior to 1940, and in altering its stance towards both industrial and science policy, it compelled both the corporation and the university to revise the ways in which science would be carried on within their precincts. This was the era of the now derided "linear model": the assertion that innovations in 'pure science' were necessary formal prerequisites for advances in 'applied science,' and that both made their way in an orderly fashion down the pipeline until 'technological development' resulted in the new products that drove capitalist expansion.[28] Under the triple imperatives of classification, rationalization and projection of ideological superiority, the military refined the 'purity' of the laboratory in a different crucible. As an unintended consequence, the change in regime underwrote a conviction, almost a dogma that science and commerce should never mix, even though this flew in the face of a previous generation's experience. Gaining a better perspective on the Cold War regime will go some distance in dispelling the fruitless standoff between the neo-Mertonians and the economic enthusiasts, and the Stage 1/ Stage 2 mindset with which we began this paper.

The wartime experience of the OSRD/NDRC and the immediate postwar debates over civilian vs. military control of science have been superbly covered by the present generation of historians, so it need not be recapitulated here. What has been perhaps missing from these accounts is the ways in which the militarization of science had an impact on the previous regime of corporate science, as well as the ways in which the

[28] For a description of how the linear model was precipitated out of the Cold War context, see (Kline, 1995; Mirowski & Sent, 2002). For a brief history of its existence as a statistical category, consult (Godin, 2003). For the vicissitudes of its modern manifestations, see Calvert (2004). For a new perspective upon its provenance as the brainchild of the economist who also promoted the economic concept of the "public good", see (Samuelson, 2004, p. 531; Mirowski, forthcoming). Editor: Please insert cite to any Handbook entry that deals with basic/applied distinction.

American university was forced to reorient itself in order to occupy the space cleared for it within the postwar settlement. The most obvious alteration was the intrusion of the government as the third, and now largest player in the funding and management of science; but this implied something more than slinging largesse at a few favored natural sciences. It involved subscribing to a tenet that politicians were often loathe to admit, given their redoubled allegiance to the virtues of market organization: that the federal government was in the business of picking winners and losers in the realm of technological development by running a *sub rosa* industrial policy under the auspices of the military, which included promotion of a very different set of practices than had held sway before the war regarding intellectual property and antitrust. Meanwhile, the corporation was growing in its power and reach, given that many of its European competitors had been hobbled by the war. Both the government and the corporations were impressed by the efficacy of science in winning the late war; it was taken as given that it would also play a pivotal role in winning the Cold War.

The Cold War is now regarded as the Golden Age of the Chandlerian firm. The line-and-division mode of management had proven its mettle during the war; through the 1970s the roster of the hundred largest American corporations displayed amazing stability; since a certain equilibrium had been reached in the control of their core markets, the new watchword became 'diversification'. Dominant firms in mature industries sought to grow by buying up new product lines and moving into newer industries, and the M-form or multidivisional bureaucratic managerial structure spread throughout the corporate sector (Lamoreaux et al, 2003). As corporations became less tied to single product lines or nominally related competencies, the role of the corporate laboratory began to shift. Industrial science still assumed many of the functions it had done prior to WWII, such as routine testing and product improvement. Yet the increasingly multidivisional or conglomerate nature of the firm dictated that each division should become its own profit center, and that funds would be allocated within the firm according to criteria applicable across all divisions. Here is where the military takeover of science policy came into play. Not only did military funding come to dominate academic science, but it also re-orchestrated a major portion of industrial or commercial science (Graham, 1985).

Because the American military did not set out with deliberate forethought and intention to become commander-in-chief of science policy in America, but rather found itself backing into the commitment fitfully and by degrees, it had to be very flexible about experimenting with various methods to fund and manage the scientists whom it wished to keep on retainer, and in the process, invented many new configurations of laboratories. Many point to the Manhattan Project as the first decisive military experiment with science organization. Although the original OSRD contracts were run through universities as the research entities, soon it was decided that the industrial-scale centrifuges and uranium enrichment research at Clinton Tennessee and the Hanford Works site would be contracted out to private firms—in that case, DuPont. The postwar legacy institutions at Oak Ridge, Los Alamos, Argonne and Brookhaven were set up as something else which had been resisted throughout the previous regime: government-run "national labs" funded directly by the Atomic Energy Commission (Westbrook, 2003). Other sorts of research was deemed to require something other than a university or corporate setting, and so the Air Force and the Ford Foundation concocted a university campus without students or faculty combined with a non-profit Santa Monica beachfront resort at RAND in 1948, and thereby innovated the think tank. Finally, in the critical areas of aerospace, electronics and missile development, it was decided that R&D had best be done on a strictly commercial basis, and there the military took the fateful step down the road of subsidizing corporate R&D in areas where it believed there was a compelling national interest in maintaining supremacy at the forefront of research.[29]

The dramatic reorientation of the in-house corporate lab from an internally-oriented product development agency to an external research contractor had profound implications. First, and most significantly, the ability to attract military funds reconciled the corporate lab with the M-form corporation, in that the lab could (and often did) justify its divisional status by capturing its own streams of external revenue. But in order for this to happen, the corporate science lab had to be brought into line with the rather different notions of accounting, control and intellectual property propounded by their military

[29] See, for instance, (Forman, 1987; Graham, 1985; Hounshell, 1996, pp.47-50). As Colonel Norair Lulejian said in a 1962 speech: "Can we for example plan and actually schedule inventions? I believe this can be done in most instances, provided we are willing to pay the price, and make no mistake about it, the price is high" (quoted in Johnson, 2002, p.19).

patrons. Recently, Glen Asner has made the very interesting argument that a series of accounting, tax code and procurement regulations imposed by the military over the 1950s "provided incentives for the corporations to restructure their research programs on the basis of the linear model" (2002, p.4). For example, the Procurement Act of 1947 effectively perpetuated the wartime innovation of the cost-plus contract in the realm of military R&D. The Department of Defense did not mind funding what would be dubbed "basic research" in the aftermath of WWII, because their regulations concerning overhead would putatively allow them to control the mix of basic and applied as they saw fit, and the 1954 tax code revisions allowed accelerated write-offs of new investments in research infrastructure, which Department of Defense sought to encourage. Here we observe that the basic/applied distinction, far from mapping preset divisions between universities and industry, was inscribed in the very contracts that propagated it, largely through a myriad of nearly invisible stipulations concerning the economic provisioning of research.[30] Far from mere boondoggles, these practices had the dual effects of allowing a greater degree of disjuncture of the research of the corporate lab from the activities of other divisions of the same corporation, while at the same time allowing the lab to be structured more along the lines of the university. (The fact that the model had historically come full circle undoubtedly rendered the transition easier.) Corporate labs were consolidated at locations remote from production facilities on campus-style settings, often justified by levels of secrecy and classification also demanded by the military. Scarce postwar research personnel were often courted with promises of university lifestyles, and a fair amount of autonomy with regard to research agendas. Bell Labs, Xerox Parc, IBM Yorktown Heights[31], RCA Sarnoff, Westinghouse Pittsburgh, Merck Rahway and others became powerhouses of basic research, often enjoying substantial autonomy in setting their own research agendas. "A two-class system (military and nonmilitary) developed, with the best and brightest concentrated in the military class" (Hounshell, 1996, p.49). And the investment began to pay off in a more 'academic'

[30] The irony of this story is that what was rendered more 'real' in the industrial sector was simultaneously further eroded in the academic sector. See, for instance, (Lowen, 1997, p.140): "By the mid-1950s the claims that the programs of basic and applied research [at Stanford] were entirely distinct and that the applied research program was not affecting the academic program were largely rhetorical."

[31] IBM stands out from these other corporations as forming its in-house research capacity rather late, creating a "Pure Science" Department only in 1945. On this unusual history, see (Akera, 2002). The Yorktown Heights facility was only opened in 1960.

modality: between 1956 and 1987 twelve corporate scientists won Nobel Prizes (Buderi, 2000, p.110). Was it therefore so very odd that even the community of corporate scientists came to subscribe to the linear model, since everything seemed inclined to ratify its existence?

While it was not the intention of the American military to render the industrial research lab transformed so that it would more closely resemble the university science facility, it was their intent to channel research in such a manner so as to conduct what has been sometimes called a 'stealth industrial policy'.[32] Specialists in funding agencies like the ONR, the AEC and DARPA thought they could predict which industries were making use of cutting-edge science to produce the technologies of the future; and under the imperative of national security, they could justify their interventions to make their own predictions come true. Their successes in the areas of quantum electronics, solid state physics, and computers are well known; but there were also significant initiatives in pharmaceuticals, radiobiology, meteorology, and catalysis. Not only did the government back select horses in the derby; they dabbled in equine husbandry as well. Through a combination of weakened intellectual property rules and fortified antitrust practices, they sought to breed a corporation better suited to withstand the chill winds of the Cold War.

The American military had publicly pledged its troth to the magic of the market, but generally were not willing to entrust mission-critical aspects of weapons development or considerations of national security to the vagaries of the free market. The postwar innovation of systems management was constructed to *plan* invention (Johnson, 2002). In particular, the Cold War regime witnessed a policy of striking mitigation of intellectual property rights in areas where the military was directly involved in science management. Starting with the Atomic Energy Act of 1946, the government asserted a policy to retain patent rights deriving from military-funded research, but only to make any such inventions that arose available to American firms on a non-exclusive royalty free basis.[33] The policy was both chauvinistic, in the sense that national security dictated the subsidy of American firms, but also anti-monopolistic, in the sense that national security would

[32] See (Hart, 1998, pp.227-9; Teske & Johnson, 1994).

[33] See (Westbrook, 2003, p.51). The policy was not uniformly applied to industrial contractors, but the second-source rule often mitigated any commercial advantage that the firm might enjoy from keeping patent rights. Interestingly enough, the supposedly public-spirited University of California resisted the AEC rule, and was defeated.

be compromised if the military were to become inordinately dependent upon any single firm. Such considerations also governed the "second source rule" promulgated by the Department of Defense, which conveyed the intellectual property surrounding critical weapons systems or military technologies to a second competitor firm, so that the fortunes of no single producer would constitute a bottleneck.

Not only was the military skeptical of the virtues of strong protection of intellectual property in frontier science, but so too were the economic experts that (for a time) dominated antitrust policy in the US. In the 1940s the Department of Justice adopted the position that one of the more deleterious effects of monopoly was the suppression of technological innovation, and filed suits against some of the nation's most high-technology companies of the time, such as DuPont, Alcoa, IBM and General Electric. Compulsory licensing of patents became for the first time a common element in antitrust settlements (Hart, 2001, p.928). The effect of these policies, in consort with military regulations, was to induce firms to pull back to some extent from acquiring the promising technologies of would-be competitors, or to play down the aggressive pursuit of patent infringement cases against major rivals, and to pour more of their resources into their own in-house labs. The result, under the banner of national security, was an oxymoronic regime of relatively open science hedged round by classification and secrecy.

It is through this Cold War lens that we can come to better understand the ways in which academic scientists could come to believe in the independence and isolation of their ivory tower. The military was convinced that encouragement of a certain format of higher education was an indispensable complement to the protection of national security. In stark contrast with the Erudition regime, postwar public policy was aimed at sustained subsidy of academic science beyond the narrow scope of few private universities, although those fortunate institutions also benefited immeasurably under the new regime. Indeed, one might suggest that it was only during the Cold War that the totality of economic sectors embraced higher education as an exercise in American nation-building, with all that might imply: mass education, a diversified research base, a democratic ideology, open science, and the open propagation of research results. The military played a major role in fostering this system, primarily through the innovation of overhead payments on research grants, but also through more fleeting initiatives like the GI Bill and generous fellowships. The objective was to fuse teaching and research into a single symbiotic system, held together by the glue of generous funding.

It was a fateful decision early on at the OSRD to keep most contract research tied to university settings, and to reconcile university administrators to that fact with lavish subsidy. Vannevar Bush arbitrarily proposed overhead payments of 50% of labor costs for university research grants (although his real allegiance was demonstrated by the 100% rate proposed for corporations); and although the magnitudes of the subsidy were the subject of some controversy during the war, universities learned to deal with the inconveniences of having to subject these payments to bureaucratic accountability and oversight (Gruber, 1995). Although some university administrators were convinced that the postwar period would return rather quickly to the Erudition regime's dependence upon industrial contract research, other more visionary captains were impressed by the sheer magnitude of military largesse. As Robert Hutchins of the University of Chicago admitted in a memo in June 1946, "It seems likely that within the next five years the Government will become, directly and indirectly, the principal donor of the University."[34] Those who were willing to go along with the drastic shift in patronage thereby stood a chance of stealing up on their rather more hallowed and prestige-laden competitors. MIT notoriously took advantage of the opportunity to climb the league tables (Leslie, 1993). In 1946, Stanford managed to accumulate military contracts that were twice the value of its contract research during the entirety of WWII (Lowen, 1997, p.99).

It may seem that the saga of the Cold War regime could be sketched entirely without consideration of the role of the private foundations; but this would not be altogether valid. Older foundations continued programs of academic subsidy, and a few new players, like the gargantuan Ford Foundation, came upon the scene (Raynor, 2000). However, a government crackdown on the use of foundations as tax shelters in 1950, combined with the fact that even the largest foundations could not begin to match the magnitude of impact of the federal government on higher education and science, meant that most foundations scaled back their ambitions concerning the management of science. For instance, in 1960 the Ford Foundation was channeling more support to American

[34] Quoted in (Gruber, 1995, p.265).

universities than the NSF; but by 1970 it had all but withdrawn from the support of academic science (Geiger, 1997, p.171). Foundations became notorious for their fickle initiatives, which could disappear with each executive change; they were no longer participants in science management for the long haul.[35]

Hence, the American Cold War regime was largely structured as a concertedly nationalized system of science, but one whose ideological significance was so highly charged that it had to be presented as an autonomous and autarkic invisible college of stalwart stateless individuals who need pay no heed to where the funding and institutional support for all their pure research was coming from. 'Purity' had become conflated with 'freedom' and 'democracy'; "science" stood as the embodiment of all three states of virtue; and American science organization was promoted as a rebuke to the Soviet machine, but equally it was thought to stand as reproof to anyone who sought to make science submit to an imperious political master.[36] It was only within the Cold War regime that 'academic freedom' really seemed to possess sufficient gravitas to actually be used in an effective defense of academic tenure—something we can now appreciate in the era of its disappearance. The researcher had only to answer to his disciplinary peers, or in the last instance, to his individual conscience, and feel an enlightened disdain for the hurly-burly of the marketplace – at least until the DARPA grants officer came to call.

C] The Globalized Privatization Regime

The advent of the globalized regime of privatized science was not heralded in such an unmistakable way by war or depression, as were the previous regimes. A superficial perspective might seek the watershed at the Fall of the Berlin Wall, since, after all, it was the most dramatic event that signaled the cessation of the Cold War. However, if we triangulate between corporate evolution, educational transformation and

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[35] This is not to say that foundations had no lasting effects in the Cold War regime. The Ford Foundation established the dominant model for American business schools in this era, while Rockefeller played a crucial role in the academic development of molecular biology (Kay, 1993). The relationship of science to state power in this era is discussed in Ezrahi (1990).

[36] These issues are discussed further in (Hollinger, 1990; Mirowski, 2004b).

government policy, the inauguration of the privatization regime in America would have to be located a decade or so earlier.

Economic historians, legal scholars and science studies researchers all tell the story in somewhat different ways, but it is significant that they all trace the metamorphosis back to roughly 1980.[37] The trigger seemed to be the widespread conviction that the United States had lost ground to international competitors during the oil crisis and economic slowdown of the later 1970s. Although there was substantial disagreement over the causes of the supposed sclerosis, an array of initiatives were launched in order to defeat the diverse culprits sapping America's economic dominance. One major candidate for economic reform was the organizational structure of the Chandlerian corporation (Lamoreaux et al, 2003; 2004; Langlois, 2004). Various participants had become convinced that the huge managerial conglomerate had become too unwieldy to effectively compete in the world market in the 1970s, and the 1980s were the era of hostile takeovers, leveraged buyouts and shareholder attacks on the top management of large corporations. In response, there was a significant retreat from diversification within firms, with one calculation suggesting that by 1989 firms had divested themselves of as much as 60% of acquisitions made outside of their core business between 1970-82 (Bhagat et al, 1990). There was also a retreat from previous levels of vertical integration in industries like automobiles, computers, telecommunications and retail. Consequently, corporations began to equate agility and nimbleness with repudiation of hierarchical managerial control of process, and with it the M-form paradigm, and thus sought to re-engineer the supply chain to depend to a greater extent on market co-ordination.[38] Networks of sub-contracts began to displace ownership ties as modes of organization; venture capital began to channel investment into startup firms. Labor-intensive heavy manufacturing was outsourced to low-wage countries. Moreover, the roster of America's largest corporations underwent severe shakedown, after having enjoyed relative stability for the previous sixty years. The lumbering giants

[37] See, for economic historians (Lamoreaux et al, 2003, p.405); for legal scholars (Boyle, 2000; Lessig, 2001;2004; McSherry, 2001); for historians of education, (Geiger, 2004, p.3; Matkin, 1990, p.22; Slaughter & Rhoades, 2004; Kirp, 2003; Apple, 2005); for politics see (Krimsky, 2003, pp.30-1; Mirowski & van Horn, 2004c). Washburn (2005, chap.3) documents the political maneuvers leading up to the passage of the Bayh-Dole Act.

[38] Interestingly enough, Chandler himself does not entirely agree with this assessment. See (Chandler, 2005a).

were prodded into defensive action, which was widely interpreted as a return to market methods of co-ordination (Langlois, 2004).

Another important initiative was deployed in the arena of organization and control of international trade. In a far-sighted mobilization, a handful of representatives of corporations located in high-tech industries such as pharmaceuticals, semiconductors, computers and entertainment formed the International Intellectual Property Alliance in 1984, for the purpose of linking issues of intellectual property to larger trade negotiations.[39] They succeeded beyond their wildest ambitions, using the Uruguay Round of negotiations over the General Agreement on Tariffs and Trade to impose US standards and levels of intellectual property protection upon developed and developing countries alike, and to enforce them with trade sanctions through the World Trade Organization. TRIPS [Trade-Related Intellectual Property System] came into force on January 1, 1995, and has implanted the basic legal premises of the globalization regime to all corners of world, refashioning academic and corporate activity in the interim.[40] Although focused on the seemingly narrow legal playing field of intellectual property, one might regard TRIPS as one facet of an even larger concerted political movement to weaken the prerogative of national governments to exert regulatory control over the corporate entities within their boundaries, all in the name of a liberalization of trade and the protection of foreign investment. In any event, manufacturing capacity was shifted to lower-wage countries in search of a quick productivity boost.

These major restructurings of the corporate sector coincided with a crisis in the sphere of higher education. After 1975, enrollments in US higher education ceased to grow for the first time in US history, while cash-strapped states began to contract their funding. (Geiger, 2004, pp.22 et seq.). The military, under pressure to reduce funding of projects not immediately relevant to its mission, had been attempting to withdraw from many of its commitments to the funding of academic science in the 1970s, so universities suffered a double deficit, with no end in sight. In order to maintain graduate enrollments, many departments in the sciences began to admit rising proportions of foreign students

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[39] The identity of these vanguard industries is crucial for our narrative, because it is widely recognized that "in most industries, university research results play little if any role in triggering new industrial R&D projects" (Mowery et al, 2004, p.31). The activist firms were located in that small group of industries that did make extensive use of academic research.

[40] See the discussions in (Drahos & Braithwaite, 2002; Sell, 2003).

(NSB, 2004, p.5-25). While this had salutary effect upon the rather parochial atmospheres of American university towns, it also had the deleterious effect of revealing the essential bankruptcy of the Cold War justification of education as serving the objectives of state-building. Many of the students in technical areas were not citizens of the US, and periodically some politician would ask what universities were doing training the work force of potential competitors at American expense. But more to the point, the whole idea of an informed citizenry and skilled workforce began to lose salience as more and more production activity was shifted overseas, and corporate managerial cadres became more international. The university was losing its grip on its previous social *raison d'être* , even as it remained the preferred path for individual economic advancement. It also, in an ironic twist, was revamped in a Chandlerian direction, even as many corporations were fleeing that organizational model in droves. Significant aspects of faculty governance were diminished or dismantled altogether (Geiger, 2004, p.25), and were replaced with top-heavy managerial hierarchies who multiplied divisions, institutes and other offices, often in the name of rationalization and cost-saving. Costs were more directly addressed by replacing tenured faculty with temp labor and part-time teachers; but this reversed the Cold War tendency to unite teaching and research as mutually reinforcing activities.

And then there was the overt political attempt to bring the hobbled universities more into line with the re-engineered corporation. It has become *de rigueur* for commentators noting the commercialization of science to bow in the direction of the Bayh-Dole Act of 1980 as a major turning point in the treatment of intellectual property in the US, because it allowed universities and small businesses to retain title to inventions made with federal R&D funding, and to negotiate exclusive licenses.[41] Actually, the historical situation with regard to intellectual property was much more complex, and yet, the end result was almost a complete reversal of practices under the Cold War regime. First off, universities had been permitted on a piecemeal basis to patent federally funded research via individual Institutional Patent Agreements since 1968 (Mowery et al, 2004, p.88). Only in 1983 was Bayh-Dole style permission extended to large corporations, their

[41] See, for instance, (Slaughter & Rhodes, 2002; Geiger, 2004; Miyoshi, 2000; Krimsky, 2003; Washburn, 2005; Mowery et al, 2004). Mowery et al (2004, p.94 et seq) do us the service of pointing out the fallacies behind the rhetoric one finds in European contexts, to the effect that if those countries would only institute their own versions of Bayh-Dole, they would automatically reap untold benefits of escalated technology transfer.

real intended beneficiaries, by a Ronald Reagan Executive Memo—the better to fly under journalistic radar (Washburn, 2005, p.69). Second, Bayh-Dole was only one bill in a sequence of legislation throughout the 1980s which expanded the capacity of corporations to engage in novel forms of collaborative research while capturing and controlling their products (Slaughter & Rhoades, 2002, p.86). For instance, the Stevenson-Wydler Act of 1980 opened the door to commercialization of research performed at the national laboratories. The National Cooperative Research Act (NCRA) of 1984 shielded corporations from antitrust prosecution when engaged in joint research projects. The National Technologies Transfer Act of 1989 allowed federally-sponsored research facilities to spin-off previously classified research to private firms. Over the same period corporations sought and won numerous laws to strengthen both patent and copyright, and in 1982 managed to have a special Court of Appeals in the Federal Circuit dedicated to patent cases. The scope of what is susceptible to patent in America has been progressively broadened, and challenges to the legitimacy of patents have become less successful.[42] The very notion of a public sphere of codified knowledge has been rolled back at every point along its perimeter, initially by blurring the lines between public and private property. This hyper-restrictive system of intellectual property has then been exported to the rest of the world under the aegis of the WTO and the World Intellectual Property Organization, as outlined above.

The concerted fortification of intellectual property was accompanied by the weakening of antitrust policy, in exact reversal of the Cold War regime. Absolution was not just granted in the specific case of the NCRA, but more generally under the influence of the Chicago School of law and economics, monopoly was increasingly downgraded as a source of inefficiency or political danger in the viewpoint of the Justice Department (Hart, 2001; Hemphill, 2003; van Horn, forthcoming). The doctrine was propounded that monopoly was not necessarily harmful to innovation (even in the case of US vs Microsoft), that size of R&D budget was not correlated with demonstrated ability to innovate, and that good products win out in the end, no matter what the industry structure. In any event, defenders could point to the increasing resort to cross-licensing

[42] For the degradation of the patent system, see (Kahin, 2001; Jaffe & Lerner, 2004). For more general consideration of the extension of intellectual property, see (Lessig, 2001; Drahos & Braithwaite, 2002; Sell, 2003; Mirowski, 2004a, chap.6).

and joint ventures to suggest that there was no return to the bad old days of trusts and patent pools (Caloghirou et al, 2003). Rather, a fortified and unfettered corporate sector free to contract for research when and where it saw fit was thought to be one of the best prophylactics against upstart foreign producers, and looming national economic decline.

The cumulative consequence of all these convergent vectors was a fateful restructuring of the American corporation and the most important revision in the organization of science within the regime of globalized privatization: the relative demise of the in-house corporate research labs, and the spreading practice of the outsourcing of corporate research.[43] It is here, and not in any vague shift in the *Zeitgeist* or narrative of the rationalization of technology transfer, that we find the root cause of the new model of commercialization of science in the 21st century. While each of the trends we have identified above was not deliberately attuned by itself to bring about the destruction of the in-house corporate lab, each contributed to its demise. It is important to understand the ways in which the withdrawal of the military from science management, the perceived failure of the Chandlerian firm, the push to globalize the neo-liberal Washington consensus, and the crisis of higher education all converged upon the corporate lab.

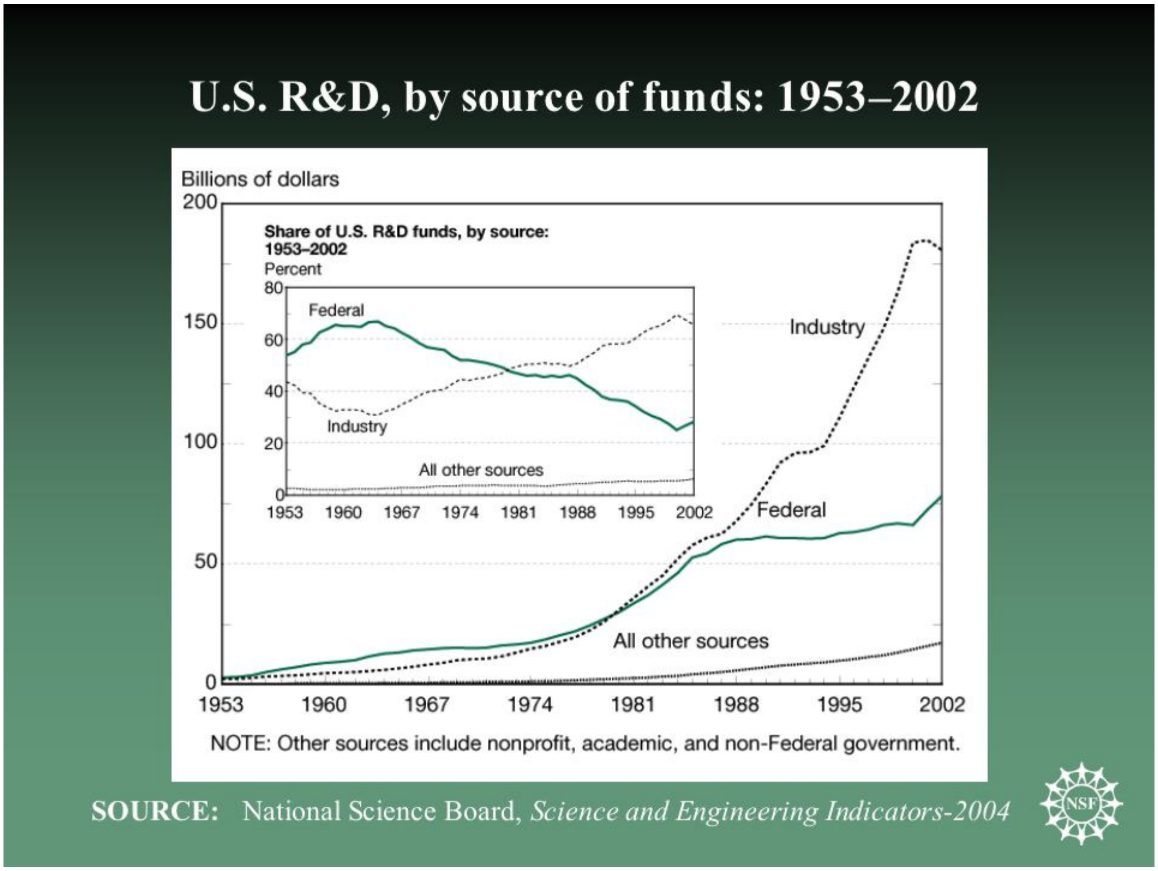
Pundits in business schools often attribute the passing of the large corporate lab to the supposed empirical observation that big in-house research labs don't deliver the goods (Anderson, 2004), usually accompanied by reference to some neo-liberal doctrine that in the long run healthy science resists being planned, but this superficial analysis ignores the fact that the labs had been weaned from their internalist parochial commercial orientations by military contracts during the Cold War (Graham, 1985). The corporate labs had been permitted to maintain their external orientation and unfettered curiosity and campus ambiance as long as they were revenue centers for the firm, but when the military withdrew from the organization and funding of basic research, then the semi-autonomous corporate lab became a liability. In a more forgiving environment, perhaps they might have been reoriented back more concertedly to the development side of R&D, and persuaded to renounce the linear model of technological change, but by the 1990s they

[43] This sea change is documented and discussed in (Anderson, 2004; Buderi, 2000, 2002; Economist Intelligence Unit, 2004; Reddy, 2000; Chesbrough, 2001; Berman, 2003; Markoff, 2003).

ran up against the anti-Chandlerian movement to divest the firm of its extraneous product lines and scale back on vertical integration. In many corporations, the research division was a prime candidate for downsizing or spin-off, and that is precisely what happened throughout the 1990s. RCA Sarnoff was first sold off to SRI International, and soon thereafter spun off as Sarnoff Corporation in 1987. AT&T slashed research at Bell Labs starting in 1989, only to spin off the remnant as Lucent in 1996 (Endlich, 2004). Westinghouse Pittsburgh was first decimated, and then sold off to Siemens. Research divisions disappeared altogether at firms such as US Steel and Gulf Chevron. By 1995 IBM had eliminated a third of its research budget, essentially gutting its flagship Yorktown Heights facility; other units, such as its Zurich laser group, were spun off as separate firms. After the merger of Hewlett-Packard and Compaq and the spin-off of Agilent, the renowned HP Labs were slated for reorganization and downsizing (Markoff, 2003). The historian Robert Buderi, who has been most concerned to document this phenomenon, admits that research directors regarded it as a "research bloodbath" in the late 1980s and 1990s (Buderi, 2000, p.22), but has sought to paint the bloodletting as a proscription for both corporate and scientific health. The problem with this diagnosis is that it is too narrowly focused upon the individual firm in isolation, and ignores the larger system of the funding and organization of science. Buderi writes, "We now see less basic research going on. IBM does not chase magnetic monopoles anymore, but should it have done so in the first place?" (2002, p.249) This presumes someone somewhere else will take up the chase for magnetic monopoles, and someone else will worry about where and how that will happen. But this question of who organizes which science to what ends is precisely the debate that is glaring in its absence in the Globalized privatization regime.

The downsizing and expulsion of in-house corporate labs has not implied a corresponding contraction of private funding of research and development in America; quite the contrary. In a pattern that has been mimicked with a lag in other countries, in the US, federal R&D expenditures as a proportion of the total R&D has declined continuously since the late 1960s, while the proportion of R&D expenditure originating in the industrial sector has increased from the same period, surpassing the federal proportion around 1980 (NSB, 2004, p.O-11).

[Insert Figure 1 Here]



If corporate labs are being slashed, how could this be? The resolution of these seemingly contrary trends is that the increased volume of research is being performed outside of the boundaries of the corporations funding it. Some of it is being performed in other corporations purpose-built for research under the new regime, while the rest is increasingly performed in academic and hybrid settings. It is precisely at this juncture that the other historical trends described above of the globalization of corporate trade and investment, and the crisis of the research universities, come into their own.

The breakdown of the Chandlerian model of the hierarchical integrated firm has prompted the nagging question: Why integrate R&D into the firm when you can buy it externally, and reduce costs by doing so? But that question presumes that R&D is a distinct fungible commodity in a well-developed market, one so competitive that it can lower the costs relative to doing it yourself. One major thesis of this chapter is that, no matter how 'commercialized' science may or may not have been in the previous

American science regimes, until recently this state of affairs that was uniformly absent. The strengthening of intellectual property, the weakening of both domestic antitrust and the ability of foreign governments to counter corporate policies, the capacity to shift research contracts to lower-wage and easier regulatory environments and therefore engage in regulatory arbitrage, the availability of low-cost real-time communication technologies, and the presence of an academic sector which was willing to be restructured to surrender control of research to its corporate paymasters: all of these were necessary prerequisites to seriously countenance the corporate outsourcing of research on a mass scale.

The globalization of corporate R&D is one of the characteristic hallmarks of the new regime. Of course, multinational companies headquartered in smaller countries like the Netherlands and Switzerland have internationalized their R&D activities essentially from their inception; but the more striking trend is the international outsourcing of research across the board since the 1980s (Reddy, 2000, p.52). Just as with recourse to academic capacity, global outsourcing tends to be concentrated in a few industries, such as pharmaceuticals, electrical machinery, computer software and telecommunications equipment. Nevertheless, surveys within these industries reveal a sharp increase in research carried out beyond the home country's boundaries from the 1960s to the 1990s (Kuemmerle, 1999). A more recent survey by the Economist Intelligence Unit reveals the globalization of R&D gathering pace over the 1990s, with over half the respondents indication they would expand their overseas R&D investment in the next three years. When queried as to the major considerations governing their decision, the most popular responses cited strong protection of intellectual property, lower costs and the tapping of indigenous research capacities. It is the access to lower wage labor in the context of an academic infrastructure, which *is disengaged from any corporate obligations to provide ongoing structural support for local educational infrastructure*, which explains the shift in research funding to countries like China, India, Brazil and the Czech Republic (EIU, 2004, p.9). Another way to cut costs is to disengage the firm from nationalist appeals to help support scientific infrastructure, accompanied by improved opportunities to further reduce or avoid corporate taxation.

Approaching the commercialization of science from this angle profoundly revises the usual narrative of the privatization of modern academic science as a straightforward case of cash-strapped universities following the money, albeit with a few nagging qualms concerning the propriety of telling corporations only what they want to hear.[44] Rather, a new STS historiography might be proposed where many of the novel institutions of globalized privatized research were first pioneered *outside* of the academic sector *per se*, as adjuncts to the modification and re-engineering of the modern corporation, and only then have been foisted on universities forced to react to these benchmark citadels of the new globalization regime in their own internal restructuring of scientific research.[45] Government revisions of policies with regard to intellectual property or educational subsidy may have constituted incentives, but could not unilaterally impose the structure of the new regime. While legislation such as the Bayh-Dole Act were enabling, they should not be confused with the *cause* of the privatization of science, which was instead attributable to the larger shift in the nexus of science management and funding.[46]

Indeed, one of the great unspoken presuppositions of modern commentators on the commercialization of science is that the either the scientist or the community at large is still capable of choosing 'how much' public open science one wants to preserve, while leaving the remainder to be covered by the private sector.[47] Contrary to this presumption that one can rationally choose a menu in any combination from Column A and Column B, once the institutional structures of the globalized privatization regime have been put in place, then the very character and nature of public science is irreversibly transformed. One can observe this in the recent rivalry between Celera and the public Human Genome

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[44] Examples of this narrative can be found in (Bok, 2003; Geiger, 2004; Krimsky, 2003; Nelson, 2001; Owen-Smith & Powell, 2003; Thursby & Thursby, 2003; Kirp, 2003). .

[45] One signal characteristic of each of the three regimes discussed in this paper is the insistence on the extent to which American higher education has been responding to innovations originating outside their purview, and the relative absence of first-mover advantage when it comes to the organization of science. Here is one place where the history of science and the history of education need to be brought into closer dialogue.

[46] This is the major drawback in the premises governing (Mowery et al, 2004; Krimsky, 2003; Nelson, 2004), and the literature that restricts itself narrowly to considerations of intellectual property such as (McSherry, 2001).

[47] One major representative of this position in the science policy literature has been the work of Paul David and Partha Dasgupta's paper on their version of a 'new economics of science'. See the summary in (Mirowski & Sent, 2002), and (David, 2003; 2004). For an incisive critique of their distinction between 'tacit' and 'codified' knowledge, which they correlate with open vs. commercial science, see (Nightingale, 2003).

Project. In a fascinating journalistic account of the race, James Shreeve clearly finds Craig Venter a more compelling protagonist than Francis Collins, and equally clearly subscribes to modern neo-liberal doctrine that the free market produces better research more cheaply than does the hierarchical managerial model, and yet almost in spite of himself, demonstrates in numerous ways that once the commercialized Celera entered the arena, then the public genome project found itself buffeted and transformed in mutations beyond its control. For instance, because it was committed to open science, the faster the Human Genome Project went, the more it ended up helping Celera to beat it to the finish line (Shreeve, 2004, p.198). In another instance, no matter how loudly Venter trumpeted that Celera was not costing the taxpayer a single dollar, if anything his project depended on public subsidy in ways more elaborate but more dubious than the public genome project, in that at least the latter was subject to some forms of public accountability that Celera could effortlessly flout. Knowledge was secondary for Celera; it was fencing off the genome that trumped everything else. "The key was for Celera to be proactive, to grab as much potential intellectual property as possible and sort out later who really owns what. Celera was getting a late start" (Shreeve, 2004, p.231). One might also worry that the quality of the 'finished' genome was substantially degraded by the various stratagems induced by the public/private rivalry carried on in the glare of journalistic scrutiny. The ultimate irony was that Craig Venter the consummate entrepreneur still conceded too much of his proprietary information to other scientists to suit his own paymasters, and therefore was unceremoniously ejected from Celera in January 2002, after the news spotlight had moved on.

3. Alternative Market Models of the Conduct of Scientific Research

Ever since the field of science studies broke away from Mertonian sociology of science, its adherents have proven reticent when it comes to discussing macro-scale structural attributes of science, which of course includes the economics of science. This made it difficult for STS to coherently discuss the framework of the Cold War regime during that era, although interest has revived in that topic in more recent times. Nevertheless, back then, the task of providing an overview of the Cold War regime devolved instead to the science policy community, and thus to a great extent, to neoclassical economists. The major proponents of market models of science from the 1950s to the 1980s were a group of economists primarily associated with RAND during that era. It was they who introduced the now-pervasive habit of treating knowledge as if it were production of a "thing", on a par with any other commodity in their analytical framework. Yet these analysts did not think of themselves as market fundamentalists, but rather styled themselves left-leaning defenders of the necessity of sustaining public science through public subsidy, and maintaining academic science as effectively removed from industrial R&D. The achieved this by means of the analytical construct of the "public good".[48]

The artifice of the 'public good' was one conceptual attempt within the tradition of neoclassical economics to justify the intrusion by the government into the marketplace by insisting that there were a few anomalous 'commodities' that did not posses the standard attributes expressed in orthodox economic models. In particular, these goods would be produced at "zero marginal cost", which would suggest that standard equilibrium pricing (where price=marginal cost) would lead to the underprovision of the good, or worse, it not getting produced at all. Often the public good was saddled with further anomalous characteristics, such as 'non-rival consumption' (the condition that my consumption of the good would not diminish or otherwise hamper your consumption of the same good) and 'non-excludability' (the producer could not prevent you from also using the good through standard property rights), which were cited to buttress the stipulation that markets would fail in providing adequate levels of the good. One still encounters extended rhapsodies on the special character of that market item called "information" or "knowledge", which are little more than unwitting repetitions of the original Cold War doctrine.[49] Although there is no necessary analytical connection, the terminology of 'public good' has been frequently contrasted with 'private knowledge,'

[48] The neoclassical approach to science, along with some of the founding documents, can be found in (Mirowski & Sent, 2002), especially pp. 38-43. The economists' role in creating a new political identity for the university is mentioned in (Godin, 2003, p.67). Their more specific concerns when it came to weapons procurement are outlined in (Hounshell, 2000). On the economists' role in the Bush report, see fn.3 *supra*.

[49] See, for instance, (Foray, 2004, chap.5; Guena et al, 2003; David, 2003; Washburn, 2005, p.62; Shi, 2001). The genealogy of the "economics of information" is covered in (Mirowski, forthcoming).

and thus used to suggest that the 'public commons' or our scientific birthright is being violated and encroached upon by a nefarious enclosure movement (Boyle, 2000; Lessig, 2001, 2004; Nelson, 2004).

There are plenty of reasons to think that the concept of the 'public good' was never a very useful or effective tool with which to understand the economics of science in any era, much less the current one. Although it was often cited to justify the lavish public subsidies of scientific research in America during the Cold War, it mainly served to distract attention from the military and chauvinistic motives for science funding, not to mention the ways in which corporate organization and academic science were intermeshed, as described in the previous section. The treatment of knowledge as a fungible thing was also the thin end of the wedge of the neoliberal attack upon putative distortions in the 'marketplace of ideas', a thrust which ran counter to the prevailing portrayal of science as an activity that transcended mundane political economy. After all, public good theory only maintained that it was 'inconvenient' or 'inefficient' to privatize some portion of knowledge production, *not* that the institutions of scientific research would be fundamentally undermined or corrupted in crucial respects by commodification. And then one could only maintain the fiction that science produced a tangible 'thing' by not looking too closely at the actual practice of research in its social context: the idea of 'scientific method' as a free-standing technology indifferently portable to any situation was the obverse side of this image. But the irony which we intend to highlight here is that, just as the "public good" concept was losing its prior rationale – both because of the transition from the Cold War to the Globalized Privatization regime, and because of a trend within neoclassical economics away from treating knowledge as a thing and towards treating the agent as information processor (Mirowski, 2002) -- some segments of the science studies community began to pick it up and adapt it for their own purposes.

Science studies scholars were not particularly quick off the mark to notice that the funding and organization of science had been undergoing profound transformation.[50] By the mid-90s, it had become commonplace to observe that the average scientific career was experiencing deformations – lengthening of the period (through postdocs) after the

[50] One notable exception was (Dickson & Noble, 1981). An early wake-up call was (Slaughter & Rhoades, 2002), first published in 1996.

doctorate but prior to first academic position, greater bureaucratic surveillance, more soft money positions, increased incidence of joint authorship (or no credit at all), a 'productivist' ethos – and that this might tend to undermine the Mertonian portrait of scientific norms (Ziman, 1994). At that juncture, at least two groups of scholars began to write about changes in the 'mode of production' of knowledge leading to a postacademic or revolutionary kind of science. One group has become known as proponents of "Mode 1/ Mode 2" analysis, while the other is retailed under the rubric of the "Triple Helix."

The first appearance of the Mode 1/Mode 2 characterization of modern science was the multi-authored *New Production of Knowledge* (Gibbons et al, 1994). The book did not contain a systematic empirical survey of concrete science in any particular culture area, but rather a discursive set of observations about what it felt like to pursue a research career in the present in what was clearly assumed to be an American or European setting (Mode 2), while comparing it to a past situation (Mode 1) which the authors clearly thought would be fresh in the memories of most of their readers. That book pointed to phenomena such as a weakened university structure, the general erosion of the power of scientific disciplines, the atrophy of peer control as internal guidance system, the rise of interdisciplinary research teams, and the demise of the self-sufficient laboratory. This first book did not focus attention upon the commercialization of the university *per se*, but instead portrayed research in general as becoming forced to be more responsive to external interests and concerns. A second volume by a subset of the previous authors, *Re-thinking Science* (Nowotny et al, 2001), ventured further in the direction of casting Mode 2 as a change in the epistemological presumptions of the actors. This time, they were prompted by critics to acknowledge some events like the demise of the Cold War and the passage of the Bayh-Dole Act, but was cast in cultural categories, such as the existence of a "new form of economic rationality"(p.37), or postulating that "the rising tide of individualism in society has now reached scientific communities" (p.103), rather than dealing with any specific concrete economic institutions or practices. A later contribution to a symposium on their work (Nowotny et al, 2003, pp.186-7) led to the following condensed characterization of 'Mode 2':

- "Mode 2 knowledge is generated within a context of application… [which] is different from the process of application by which 'pure' science generated in theoretical/experimental environments is applied"

- Mode 2 is marked by "transdisciplinarity, by which is meant the mobilization of a range of theoretical perspectives and practical methodologies"
- "Much greater diversity of the sites at which knowledge is produced, and the types of knowledge produced."
- "The research process can no longer be characterized as an 'objective' investigation of the natural (or social) world… traditional notions of 'accountability' have had to be radically revised."
- "Clear and unchallengeable criteria, by which to determine quality, may no longer be available."

In this overview, they also assert that their scheme provided "a more nuanced account than either of the two standard [alternatives] – characterizing commercialization as a threat to scientific autonomy (and so, ultimately, to scientific quality); and as the means by which research is revitalized in both priorities and uses" (2003, p.188). Elsewhere, one of the authors maintained Mode 2 "does not represent yet another attempt to cajole universities into behaving more like businesses" (Gibbons, 2003, p.107). While the later emendations did indeed complicate the earlier versions of the argument, there was no denying the fact that "knowledge" was still being treated as a thing and a product, and that the authors maintained a mildly positive stance towards the modern developments.

The "Triple helix" [3H] thematic has not been as extensively codified in any particular text, by contrast with the "Mode 1/Mode 2" doctrine, but its themes were spread throughout numerous special journal issues and edited volumes which tended to derive from NATO-funded conferences convened by Henry Etzkowitz and Loet Leydesdorff. The 'triple' in the 3H referred to the insistence that one must look simultaneously at the three sectors of industry, government and academia and their interactions—a precept at first resembling our own STS analysis above.[51] However, and more to the point, Etzkowitz in particular argued that universities were experiencing a "second academic revolution": the first was the incorporation of the research function along side the teaching function, and the second is purportedly the reconciliation of

[51] . The invocation of the 'helix' appears to have been a mere rhetorical figure which allows the authors to introduce what they consider to be evolutionary considerations, as well as a spate of terminology found more frequently at the Santa Fe Institute, such as "co-evolution", "lock-in", and other forms of complex nonlinear dynamics. More significantly, the tradition seems to avoid discussion of explicit economic concepts and modalities, as we do in section 2 above.

economic development with those prior two functions. "The organizing principle of the Triple Helix is the expectation that the university will play a greater role in society as entrepreneur" (Etzkowitz, 2003, p.300). He envisions the genesis of an "entrepreneurial university" which is capable of carrying out all the requisites of commercialization without in any way impugning teaching or research, and has repeatedly pointed to MIT as the exemplar of this novel form (Etzkowitz, 2002). In 3H, sheer entrepreneurial zeal is proposed to overcome many of the scruples which have dogged the commercialization of science: "In this information-based economy, knowledge can be a public and a private good in one and the same time" (Etzkowitz et al, 2000, p.327). It appears that the new regime blurs even the institutional distinctions that would have seemed central to 3H, such as corporation/university: "the university and the firm are each assuming tasks that were once largely the province of the other" (Etzkowitz & Leydesdorff, 1998, p.203). This permits adherents of 3H to be coy about whether universities are urged to simply adapt to new demands, or instead, universities and corporations as structures are converging to some single new institutional entrepreneurial entity (Shinn, 2002). By contrast to our account, the metaphorical language of 'dynamics' in fact absolves most proponents of 3H from delving in detail into what sets the educational sector apart from the government or the firm, either in structure or in functions.

Both Mode 2 and 3H authors have acknowledged that their 'paradigms' are effectively pitted in analytical competition with one another; and in one or two places, they also admit that they intend their work to "pose a challenge to STS." [52] Some science studies scholars have acknowledged the challenge, and in so doing they have not been happy with what they encountered. In a series of sharply critical commentaries, they have found both Mode 2 and 3H wanting as both history and contemporary science policy, and have asked themselves what therefore accounts for the widespread attention paid to these literatures.[53] While most have indicted the Mode 2 literature as lacking any demonstrable empirical component, the situation within 3H is more complex.

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[52] The "pose a challenge" quote comes from (Etzkowitz et al, 1998, p.xii). "Another purpose of Triple Helix discourse is to turn science studies from a constructivism narrowly focused on micro-processes" (Etzkowitz, 2003, p.332). The critical agenda of one of the Mode 2 authors can be gleaned from (Guggenheim & Nowotny, 2003).

[53] The critiques summarized in this paragraph are taken from the following sources: (Delanty, 2001; Elzinga, 2002; Shinn, 2002; Pestre, 2003; Bassett, 2003; Ziedonis, 2004).

The Mode 2 authors argue in ways more reminiscent of philosophers of science, identifying an altered 'epistemology' without paying too much attention to which specific agents may experience this epiphany, much less dissecting the institutions that might foster it. The 3H authors by contrast have encouraged much specific research into questions of science policy and education in a wide array of countries and culture areas (often presented at their biannual conferences); but the sense of the critics is that it still does not add up to a coherent analysis. Instead, Globalization is treated as a benign diffusion of an entrepreneurial spirit to universities in the periphery (Etzkowitz, 2003, 297). For instance, when Etzkowitz writes specifically about MIT, it is "less a history than a brief for the university as an engine of economic development" (Bassett, 2003, p.769). Elsewhere, 3H authors briefly note that corporate R&D is increasingly outsourced, but reveal little curiosity about the forms that it takes or what causes may be behind it (Etzkowitz et al, 1998, p.55). Intellectual property issues insufficiently explored.

But what then has proven so attractive about Mode 2 and 3H? In our opinion, and that of the critics, they both provide a convenient big tent for authors who seek to "legitimate a neo-corporatist vision of the world" (Shinn, 2002, p.608). In the case of Mode 2, this tends to be addressed to higher education bureaucrats and scholars in the humanities located in the hegemonic developed nations, whose fears need to be assuaged:

> Can the universities enter into this new closer relationship with industry and still maintain their status as independent autonomous institutions dedicated to the public good? The answer must be in the affirmative. (Gibbons, 2003, p.115)

In the case of 3H, in our experience it tends to appeal more to scholars located in developing countries, or else scholars located in peripheral areas of the developed world. They tend to be much more directly active in local science policy, and cannot take acceptance of the neoliberal dogma of free market globalization for granted to quite the degree that their counterparts in America can manage. Their research infrastructure does not enjoy the self-confident reputation of the major world universities, and therefore those individuals find they must pay much closer attention to the manner in which local governmental units and multinational corporations openly impact their attempts to provide some adequate level of high-quality education and research. Nonetheless, their activities require a generic analysis that does not appear to be too closely shackled to any specific local conditions, be they legal niceties, local educational customs, or distinct nationalist aspirations for alternative development paths. The generic character of the analysis and lack of legal specificity will help facilitate publication in hegemonic (often English language) journals or other outlets that might otherwise remain closed to the foreign scholars. While they often must appear responsive to their local constituencies who regard globalization with great suspicion, in the final analysis they are put in the difficult position of expressing qualified endorsement of commercialization initiatives which are often imposed from without: for example, WTO mandated changes in intellectual property, government-mandated cuts in public education expenditures, or multinationals contracting for research with a limited cadre of entrepreneurial scientists in their targeted areas of interest.[54] In this manner, 3H has become just another symptom of the globalization that has made itself felt in every university, corporate lab and government research facility throughout the world over the past two decades.

Both Mode 1/Mode 2 and 3H exhibit the same drawback we have identified in the introduction to this chapter: they adopt a stark before/after approach to modern developments in the funding and organization of science, and then inflate them up into all-purpose doctrines which ultimately provide a generic imposition of the neoliberal mindset upon their local higher education infrastructure, and, if pertinent, any government-organized scientific research capacity. They serve up palatable versions of a neoclassical economics of science as a public good, leached of all the actual technical content, which if more openly espoused might both repulse and dismay the sorts of clientele whom they are pledged to serve. In a caricature of the neoliberal economist, they end up simply presuming that any marketized science whatsoever inevitably enhances freedom, encourages expanded participation, and improves overall welfare.

This is not the place to summarize and critique the rise of neoliberal theories of the economy and the state since World War II.[55] It will suffice to suggest that neoliberalism differs from its classical predecessor through its transcendence of the

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[54] The use of 3H terminology to backtrack from previously skeptical analyses of the commercialization of research can be observed in (Campbell et al., 2004).

[55] See, however (Barry et al, 1996; Rose, 1999; Walpen, 2004; Apple, 2005; Van Horn & Mirowski, forthcoming). As mentioned in footnote 9 above, the neoliberal push to privatize knowledge production and knowledge conveyance dates back to the 1950s.

classical liberal tension between the self-interested agent and the state by reducing both state and market to the identical flat ontology of the neoclassical model of the economy. "Freedom" is thus conflated with entrepreneurial activity, and state functions are 'rationalized' by reducing them to market relationships. Hence the blurring of distinctions between university and corporation, or 'public vs. private' science found in both Mode 2 and 3H are derivative representations of the larger neoliberal agenda. This dictated that education should no longer serve Humboldtian ideals of creating a solid citizenry and fostering cultural development, but rather should be treated as just another fungible commodity (Friedman, 1962). Since poor people would never be able to afford much of it, and certainly very little "higher education," it follows that they would be relegated to role of passive consumer, while a corporate class of experts would effectively define and steer scientific research.[56] The kind of science that would sustain a research infrastructure is that science which would be one responsive to the needs of corporate customers (who, conveniently, had in the interim become more interested in outsourcing their R&D). Neoliberalism is very much a top-down project, while under its sway 'democracy' has been redefined to encompass pro-corporate "free market" policies interspersed with highly stylized and commercialized "elections".

The new economics of science that we believe is better oriented to conform with STS research in general accepts that some form of economic underpinnings have always shaped the organization and management of scientific research, but that because there is no such thing as a generic market, there has never anywhere existed a fully constituted "marketplace of ideas".[57] Since markets are plural and do not produce identical results either over time or between various cultural areas (much as the nature of commercialized science in America differed dramatically between the three regimes identified in Section 2 above), it becomes all the more imperative to specify in detail the fine structure of operation of each of the major players in the course of the organization of science: universities, corporations, and governments. The disaggregation of science into its component structures (laboratories, clinics, field stations, classrooms, libraries, conferences) and the disaggregation of its managers into diverse agents (academics,

[56] The segment of the STS community concerned to critique the "Public Relations of Science" movement will recognize here one of their major *betes noirs*.

[57] The types of economic theory which support this vision are discussed in (Mirowski, 2006).

corporate officers, government representatives, corporate trustees) is the first step towards constructing a sociologically-aware account of the economics of science, and not, as has been the tendency of Mode 2 and 3H, to blur them all together into a homogeneous entrepreneurial agent. Who pays whom, and who answers what to whom has consequences for the sorts of knowledge fostered. It behooves analysts to pay closer attention to who performs the labor in the laboratory under which diverse circumstances; to ask how findings are published or otherwise promulgated; to trace the flows of physical items between laboratories and corporations; to itemize the forms of attribution and audit which are brought to bear; and to inquire what institutions and customs constitute and sustain 'the author'. Where and in what manner the various components of the university become commercialized (Slaughter & Rhoades, 2004; Kirp, 2003) matter as much for the health of science as do other more obvious variables, such as the identities of the various fields presumed to hold the greatest promise for conceptual advance and commercial development. The recent innovation of the commodification of "research tools" in particular bears profound implications for nearly every aspect of scientific research, most of which have not been adequately explored by neoliberal-influenced science policy analysts.[58] It does not further analysis to simply presume that science as a whole is a production process that extrudes a thing-like entity called "knowledge"; indeed, the attempt to reify and commoditize information is itself an artifact of the modern privatization regime: a process that can never succeed in its entirety, because complete codification and control of a reified information would paralyze scientific inquiry.

This statement is not hyperbole. Take for instance, the assertion found in 3H and elsewhere that the university and the corporation are converging to a single commercial entity. While it is certainly the case that universities may be observed to behave like corporations in progressively more elaborate ways, ranging from the exploitation of trademarks to outsourcing wage-intensive functions, very few universities are willing to altogether relinquish their special non-profit status and the range of perquisites which attach to their educational location in the national infrastructure. The very few that do,

[58] See the discussion of research tools by (Walsh et al, 2003; 2005), and the critique of that work in (David, 2003). Further important work on the commercialization of research tools is cited in (Eisenberg, 2001; Streitz & Bennett, 2003).

such as the University of Phoenix, have very clearly opted out of maintaining in a serious research capacity; and therefore they are little better than digital diploma mills. Most of the commercialization of existing universities in the 1990s was due to a relatively small cadre of natural science faculty in alliance with some entrepreneurial academic administrators who wanted to exploit commercial opportunities while still enjoying all the fruits of their non-profit status. Their bonanza has been the Trojan Horse which opened up the rest of the university to a whole range of neoliberal "reforms". If, contrary to all present evidence, universities eventually really did become corporations, then one might anticipate that the myriad functions now combined on one campus would fragment, spun off due to centrifugal forces. Libraries would disappear (Kirp, 2003, p.114); expensive vocational schools (like teaching hospitals) would devolve as separate units; downmarket research for hire would migrate to contract research organizations located in separate research parks, along with 'technology transfer offices'; theatres and concert halls would go it alone; low-cost 'distance education' would decamp in search of cheap foreign labor; and dormitories would be sold off as public housing.

The fundamental crux of the modern economics of science is that, contrary to boosters such as (Bok, 2003; Baltimore, 2003; Thursby & Thursby, 2003), it may very well be that the current configuration of the commercialization of science is neither stable nor viable. Corporations are interested in academic science as long as it cuts costs – that is, as long it still receives the panoply of subsides which accrue to it in its separate non-profit status. As Slaughter & Rhoades (2004, p.308) put it, "Academic capitalism in the new economy involves a shift, not a reduction, in public subsidy." But the mere fact of commercializing university research puts that status and those cost advantages in jeopardy. Already, state legislators in the US expect their flagship universities to "float on their own bottoms"; universities in the UK are expected to attract foreign students. The imposition of "Revenue Center Management" to units within the university begins the process of restructuring there that has already occurred in the post-Chandlerian corporation (Kirp, 2003, pp.115-128). The more that teaching is disengaged from research and farmed out to non-faculty and other migrant labor, the less of a political case can be made that integrated expensive specialized research facilities should be maintained. And whyever would anyone want to leave a large cash legacy as a bequest to a private university that was a profitable corporation? The more that natural science faculty become enfolded in their corporate roles, holding down two or more jobs, the less they will be willing to cross-subsidize their poorer 'colleagues' in the social sciences and humanities. How much longer can an increasingly privatized and balkanized educational sector expect to receive any state or philanthropic subsidy on an ongoing basis?

The current beneficiaries of the commercialization of academic science may very well be destroying the goose that laid their golden egg.

4. Globalization of Science in the Modern World System

If there is no simple sense in which it can be asserted that corporations and universities are converging to a single institutional model, that does not therefore imply that there is not underway some larger convergence of diverse 'national systems of innovation' to a relatively uniform 'advanced' transnational model of the commercialization of science. The logic of the spread of multinational corporations would seem to suggest the possibility of something like this situation coming to obtain, especially since the barriers between differing economic systems collapsed after the fall of the Wall. Various forms of anecdotal and narrative evidence equally suggest a trend towards convergence in university systems over the last two decades.

In response to a common perception that the United States had been outpacing Europe in science and technology, Europe has been on the forefront of fostering educational convergence. On the teaching front, the aim has been to make higher education in Europe converge towards a more transparent system that involves the constituent countries using a common framework based on three degrees: Bachelor, Master, and doctorate.[59] These efforts started with the so-called Sorbonne Declaration signed in May 1998 by the ministers in charge of higher education of France, Italy, the United Kingdom, and Germany. They thereby pledged to harmonize the architecture of the European higher education system. The next step involved the signing of the Bologna Declaration in June 1999 by 29 European ministers in charge of higher education. This lay the foundation for establishing a European Higher Education Area by 2010 in an

[59] See http://europa.eu.int/comm/education.

effort to promote the European system of higher education worldwide. These declarations were followed by the Prague Communiqué in 2001 and the Berlin Communiqué in 2003, which further emphasized the need to make the European Higher Education Area attractive to the rest of the world. Critics have argued that this Area has no coherent pedagogical or intellectual basis. Instead, the framework is mostly a money saving measure that reduces overcrowding by getting students out of the classroom and into the work force.

The so-called Bologna process fits into the broader framework of the "Lisbon objectives," which constitute the research equivalent of the European Higher Education Area.[60] With regard to scientific research, the aim of the European Union has been to become the most competitive and dynamic knowledge-based economy in the world. These efforts originated with the Lisbon European Council of 2000, which sought to bridge the gap with the United States and Japan by coordinating research activities and laying the foundation for a common science and technology policy across the European Union. It was followed by the setting of specific targets at the European Summit of Barcelona in 2002. This set the stage for establishing a European Research Area by 2010, which is referred to as "an internal knowledge market," considered a research and innovation equivalent of the "common market" for goods and services, and meant to establish "European added value." In the process, the European Union is to increase its global expenditure on research to 3% of GDP — one and a half times the current level — by 2010 and the share of research funded by business is to rise at the same time.

Evidence of convergence to some model of the commercialization of science also abounds in the individual member states of the European Union. For instance, after two decades that witnessed an exodus of top students and scholars as well as a decrease in government support per student by 15%, Germany now plans to form a group of ten American-style elite universities in 2006 and award almost $30 million a year for five years to increase their competitiveness and quality (Bernstein, 2004; Hochstettler, 2004). Its attempts at American-style reform further involve the establishment of alumni organizations to raise money, the selection of students, and the payment of faculty salary based on performance. These efforts run counter to Germany's long-held egalitarian

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[60] See http://europa.eu.int/comm/research.

ideal. Additional culture clashes include the fact that German universities have no infrastructure of alumni nurturing, no tradition of alumni bequests, no endowment, and no fees. Critics warn of dangers and unintended consequences due to the difference in cultural context between the United States and Germany and the distinct social roles of universities in the two countries. In their opinion, these plans have more to do with past problems such as the burden of bureaucracy and lack of financing than with an effort to regain the luster German universities lost some time ago.[61]

In the United Kingdom, recent reforms include the Research Assessment Exercise (RAE) and the Teaching Quality Assessment (TQA) (Hargreaves Heap, 2002).[62] The former was introduced in 1986 as a mechanism of control. Its main purpose is to enable UK higher education funding bodies to distribute their public funds for research selectively on the basis of quality by making universities accountable for their use of public money. It is conducted roughly every four years and since 1992 about £5 billion of research funds, which constitutes the bulk of the research component of the so-called Block Grant, has been allocated using the RAE. The effect of the RAE has been to concentrate funds at top institutions as well as to increase research activity. A rather worrisome response has been an increase in private-like research. The teaching equivalent of the RAE, the TQA, involved visitors coming into universities for a week. Though nominally a teaching evaluation, it was mostly a check on bureaucratic procedures. Moreover, it was unbelievably costly and so onerous that it was abandoned and replaced by institutional audits. Since the survival of universities now depends on their RAE score, most effort at British universities goes into augmenting scores, while teaching gets neglected.

Wrenching experiments in privatization can be found in, for instance, Japan (Brender, 2004; Miyake, 2004), where national universities are being transformed into independent administrative agencies in what is being billed as the biggest higher-education reform in more than 100 years, thereby forcing them to seek funding from companies and other outside sources. The result has been an effort on the part of the

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[61] See (Hochstettler. 2004): "But the elements that work so well in America simply cannot be transplanted into the German context and expected to solve the problems now confronting German universities. In the final analysis, Germany must find a way to re-establish its academic pre-eminence on its own terms."

[62] See http://www.hero.ac.uk.

national universities to make more discernable scientific contributions to industry. Going further, many Chinese universities that have rushed to nurture start-up firms under pressure of the country's open-market policy, strained national budget, and drastic cuts in university funding. And some Chinese universities are operating companies on their own. Critics feel that the education reform in Japan, while presented as a way to give universities more autonomy, is mostly an excuse to reduce financial support. They fear that the changing governance structure plunges universities into an uncharted and ill-conceived era of competition, where government tries to have it both ways in retaining control by setting up evaluation committees and advisory boards. Of the latter, the faculty advisory board oversees academic matters and an advisory board comprised of industry, government and education leaders oversees management and finances.

In short, there appears to be a global effort to reduce the putative advantage of the United States in science and technology, while at the same time limiting the amount of government support and reducing the accompanying bureaucracy. Universities have responded to these trends by raising money from the private sector, conducting more privatized research, and shifting their attention away from teaching. In the process, they have encountered cultural barriers due to the fact that non-American universities perform a different societal role than those in the United States, and exist in a different CGE environment.

The neoliberal perspective would suggest that the national research systems were merely responding to a uniform market pressure to render their academic sectors more efficient, but just as in the national case, this analysis would miss too much of the concerted activity which produces such epoch-making departures. In particular, we would insist that it is necessary to expand our previous CGE analysis to take into account a fourth class of actors in the modern world system. Science has not only been promoted by firms, governments and universities, but increasingly in the 20th century it has also been organized and funded by international agencies who have propagated commercialization and standardization of research practices and institutions. These agencies may be divided into three classes: {A} the World Trade Organization, which has spread and enforced standardized rules of intellectual property and trade in services under the guise of providing stable platform for international trade (Drahos & Braithwaite, 2002; Sell, 2003); {B} the United Nations, which through UNESCO and WIPO has promoted international science policy; and {C} a raft of International Non-Governmental Organizations (INGOs), which play a crucial role in the spread of the globalized privatization regime (Drori et al, 2003).

An earlier Mertonian approach tended to treat science as subsisting beyond or outside of politics, but nothing reveals the obsolescence of this belief better than an inventory of the means by which particular scientific institutions have been spread by these international organizations. Some, of course, are merely the international arms of linkages of national professional organizations of scientists, and as such conform to prior images of the self-organization of science. But increasingly after WWII, these have been politically oriented INGOs that combine both scientists and laypeople into activist groups seeking to spread a model of 'best-practice science' in the name of economic and political development.

Figure 2 goes here

Cumulative Foundation of Science INGOs, 1870-1990

[Drori etal, 2003, p.84]

The activities of these INGOs goes some distance in explaining how it is possible that corporations can begin to take advantage of a globalized standardized research capacity in such a wide range of cross-cultural settings, as suggested above. The pervasive similarities of science policies in almost all of the developed world, and now, increasingly, those parts of the developing world where corporate R&D is moving in the near future (Economist Intelligence Unit, 2004) is due in large part to the work of INGOs in propagating a generic culture of commercialized research in parochial national education systems and government bureaus of science policy. The implications of this push to standardization extend far beyond the simple spread of something like a "scientific world view." For instance, standardization of scientific institutions and the de-legitimation of local knowledge is a necessary prerequisite for the globalization of for-profit higher education (Morey, 2004) as well as the harbinger of the outsourcing of much routine scientific labor to low-wage countries. The university sector is the next major economic area slated for a serious round of downsizing, cost-cutting and outsourcing in the advanced metropolitan countries, and STS scholars will appreciate this this will have unprecedented effects on the content of research just over the horizon.

5. Conclusion

The analysis of the new regime of globalized privatization will depend upon the theoretical orientation which is accessed in order to understand what is becoming a very wide-ranging and pervasive phenomenon. Most existing work, based upon neoclassical economics, has ignored or misunderstood many of the phenomena covered in this chapter. In its place, we advocate a new political economy of science, which joins up with recent developments in STS, to produce an independent analysis of the effects of commercialization upon the practice of modern science.

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