Secrecy and Knowledge Production

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CORNELL UNIVERSITY

PEACE STUDIES PROGRAM

OCCASIONAL PAPER #23

ISSN 1075-4857

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PREFACE

Secrecy surrounds us. It binds people together while simultaneously erecting boundaries secure from penetration by others. It is a potent force for social organization and a tool for social control. Individuals as well as institutions produce and keep secrets; indeed, secrecy and its cousin privacy are at the core of current debates over national security, intellectual property regimes in an age of digital information, and the relationship between knowledge and social context. If secrecy is the opposite of openness and truthtelling, as night is to day, then secrecy has few advocates outside the realm of military security. However, dusk and dawn separate night and day; secrecy and openness catch their own image in each other's eyes. Our goal in the following papers is to capture that twinkling image and make readers aware of the multiplicity of secrets and the ways in which we might understand their many ways of working.

The origins of this publication lie in a workshop on "Secrecy and Knowledge Production" sponsored by Cornell's Peace Studies Program and held in Ithaca, New York on 18-19 April 1998. The workshop brought together interested scholars and practitioners from the worlds of national security and business to discuss the relationship of secrecy to the production of scientific and technical knowledge, the practice and consequences of secrecy in the national security arena, and the ways in which secrecy operates in private corporate settings. In addition to the invited papers that are collected here, our discussion was informed by the insightful comments of Sheila Jasanoff, Stefan Senders, and Susan Christopherson.

In the workshop we were interested in exploring how insights from the field of science and technology studies (S&TS) could be used to analyze public policy issues. S&TS scholarship on issues such as tacit knowledge, the labor of producing credible and reliable knowledge, and the mutual interactions of context and content in knowledge production has immediate ramifications for the study of secrecy in defense research. For example, John Cloud and Keith Clarke's chapter on the Corona satellite program describes the work that went into maintaining a wall of secrecy and the ways in which information from the program nevertheless passed to the civilian sector.

Given the growing importance of intellectual property issues in everyday life, we also sought to investigate the similarities and differences between corporate and military secrecy, although the sanctions for industrial espionage pale next to the provisions for capital punishment for military spying. The chapters by Steven Aftergood and Frank Kapper offer analyses of government secrecy practices from two different perspectives. Secrecy appears as pervasive in the boardrooms as it has been in the situation room. Mark Fruin's chapter on Japanese business practices highlights international differences in how corporations manage information dissemination, while Alec Shuldiner's study of secrecy practices at Corning, Inc. and Susan Wright and David Wallace's investigation of growing secrecy in both academe and corporations in the field of biotechnology reveal the variation among U.S. industries and institutions.

With the recent allegations over the transmission of nuclear secrets to China, our publication is remarkably timely. At the same time, the arguments offered here in chapters by Michael Dennis and Hugh Gusterson rule out the possibility of a quick, surgical fix for whatever problems plague the nation's weapons laboratories. The designs of the W-88 warhead or neutron bomb are not the only loss, if published allegations are true; what is lost is the credibility and culture of the national laboratories. How stricter security will affect researchers is a question we cannot answer. We can, however, observe that the claim of openness as a prerequisite for scientific growth and change appears highly problematic, given the science done under totalitarian regimes in the twentieth century.

The workshop was organized by Judith Reppy and Michael Dennis with funding from the John D. and Catherine T. MacArthur Foundation's institutional training grant to the Peace Studies Program. Elaine Scott and Sandra Kisner provided essential administrative support for the workshop, and Sandra Kisner contributed significantly to the task of turning the workshop papers into an edited publication.

SECRECY AND SCIENCE REVISITED: FROM POLITICS TO HISTORICAL PRACTICE AND BACK

Michael Aaron Dennis

If conventional understandings of science were accurate representations of our world, the conjunction of science and secrecy might serve as a powerful example of an oxymoron. Writing recently in *Scientific American*, Jeffrey Richelson, a student of secret government intelligence programs, explained that the major source of difficulty in having scientists cooperate with the U.S. intelligence establishment was that such

> cooperation will require an accommodation between two cultures, those of science and of intelligence, that have essentially opposite methods of handling information. In science, the unrestricted dissemination of data is accepted as being necessary for progress, whereas in intelligence, the flow of information is tightly restricted by a "need to know" policy; only those who have the proper security clearances and who cannot carry out their assigned responsibilities without certain knowledge or information are given access to it.[1]

For Richelson and countless others, the distinctive character of science is manifested in its openness, that is, the unrestricted exchange of information and knowledge without regard for the race, creed, sex, or national origin of those involved in the exchange. Secrecy is, however, far from unknown within the world of science. All of us are familiar with the existence of a classified world of research, containing its own journals, meetings, and professional organizations. That world exists both within and apart from the world we experience on a daily basis. Even the materials Richelson is addressing—the use of national intelligence databases to understand global environmental change, Project Medea—is predicated on the existence of a secret world where researchers, more often than not academics, produced the knowledge that we might now harvest.

Science and secrecy were not, and are not, the polar opposites of common understanding. Timothy Ferris, a regular *New Yorker* science writer, declared that

> real science is a white hole that gushes information; scientists (astronomers especially) prefer to tell one another almost everything, because if they don't they can't

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[1] Jeffrey T. Richelson, "Scientists in Black," *Scientific American* 278, 2 (1998): 48-55, 48.

> build on each other's results. (The gravest concern of those who do classified work is that if they are cut off from such constant exchange their careers will wither).[2]

Given that the history of science is littered with examples of willful and deliberate secrecy, whether on the part of individuals or institutions, including states, such a claim is patently false. Furthermore, despite his invocation of Soviet science as an example of what happens when science is kept secret, Ferris does not address David Holloway's remarkable claim: that researchers in the secret cities of the Soviet atomic bomb project, such as Sakharov, were the bearers of democratic values and practices during the long Cold War.[3] If one accepts Holloway's claim, secrecy isn't simply part of science but essential for democracy.

What then is the relation between science and secrecy? Is there a single, necessary relationship between the production of knowledge and the technologies through which that knowledge is made and disseminated?[4] This paper is more assay than essay—an attempt to chart the terrain of understanding secrecy and/in the production of knowledge. What follows is a discussion of the foundations of much of the existing work on secrecy. I argue that much, if not all, of this work views secrecy as being identical to questions of access; that is, questions of who can know specific pieces of information.[5] In this literature arguments against secrecy are cast in the language of economic rationality—it is inefficient to keep knowledge from others who might needlessly duplicate work already done. Almost all discussions of secrecy and science take place in a context where secrecy is viewed as obviously necessary—a nuclear weapons laboratory, for example—or where such restrictions are viewed as absurd and hence inimical to the "advancement of science." In response to this literature I suggest that we might read some accounts of secrecy like Edward

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[2] T. Ferris, "Not Rocket Science," *New Yorker* 74 (20 July 1998): 4-5.

[3] David Holloway, *Stalin and the Bomb* (New Haven: Yale University Press, 1994).

[4] Technologies in this sense also include the systems of classification and secrecy that surround much contemporary knowledge, whether for reasons of national security or corporate market position.

[5] Some examples of this work are Sissela Bok, *Secrets: On the Ethics of Concealment and Revelation* (New York: Vintage, 1989 [1983]); Herbert Foerstel, *Secret Science: Federal Control of American Science and Technology* (Westport: Praeger, 1993); and the collection edited by Marcel La Follette, "Secrecy in University-based Research: Who Controls? Who Tells?" *Science, Technology and Human Values* 10, 2 (1985): 3-119.

Shils' 1956 text, *The Torment of Secrecy*, or Norbert Wiener's autobiographical writings as steps towards the development of a radically different view of secrecy.[6] Specifically, such works observe that access is but one aspect of understanding secrecy and science; another often-ignored dimension is the effect of such practices upon the content of knowledge developed under particular secrecy regimes. Such a perspective might draw upon much work in science and technology studies to render secrecy comprehensible, if not transparent.

Normal Science?

Robert K. Merton's famous norms of science—communism, universalism, disinterestedness, and organized skepticism (CUDOS)—are the *locus classicus* for most understandings of the inimical and unnatural relation of science and secrecy. Drawing upon his pioneering study of Puritanism and the rise of the "new science" of the 17th century, Merton extracted what he identified as the guiding norms of the scientific community. In an influential 1942 article, "Science and Technology in a Democratic Order," Merton articulated his famous norms as a direct defense of the necessary relation of progress in science with democratic politics.[7] As David Hollinger has observed, Merton made it clear that science could only flourish under a democratic regime, not the fascist regime of Nazi Germany.[8] Merton clearly stated that secrecy was the antithesis of his norm

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[6] Edward A. Shils, *The Torment of Secrecy* (Chicago: Ivan R. Dee, [1956]; reprinted 1996); Norbert Wiener, *I am a Mathematician: The Latter Life of a Prodigy* (Cambridge: MIT Press, 1956); Norbert Wiener, *Invention: The Care and Feeding of Ideas* (Cambridge: MIT Press, 1993).

[7] Reprinted as "The Normative Structure of Science," in Robert K. Merton, *The Sociology of Science: Theoretical and Empirical Investigations*, ed. Norman W. Storer (Chicago: University of Chicago Press, 1973), pp. 267-78. Merton's norms were subject to a powerful and devastating critique that is largely forgotten: Ian I. Mitroff, *The Subjective Side of Science: A Philosophical Inquiry into the Psychology of the Apollo Moon Scientists* (Amsterdam: Elsevier, 1974). Mitroff convincingly demonstrated that whatever activity might be explained by a set of norms might also be explained by a set of counter-norms. Hence, it is possible to understand the entire process described by Merton with a set of norms articulating the opposite set of values—private property, local understanding, interestedness, and organized credulity. Unfortunately, it does not lend itself to a neat acronym.

[8] David A. Hollinger, "The Defence of Democracy and Robert K. Merton's Formulation of the Scientific Ethos," pp.1-15 in *Knowledge and Society*, ed. Robert Alun Jones and Henrika Kuklick (Greenwich, CT: JAI Press, 1983). Also of interest here is Everett Mendelsohn, "Robert K. Merton: The Celebration and Defense of Science" *Science in Context* 3 (1989): 269-90.

of communism, the belief that scientific knowledge was the common property of all people. My point here is not to claim that Merton invented the idea that science and secrecy are anathema. After all, his claim was that he had identified this practice through his study of the history of science. Central figures in the so-called Scientific Revolution distinguished themselves from other knowledge producers because of their emphasis on the public, and published, character of their knowledge claims. He was merely making clear to social scientists what natural scientists took as a self-evident truth, one that was visible from the emergence of the Royal Society in 17th century England.

For Merton the problem with secrecy in science was two-fold. First, secret science could not provide the researcher with the appropriate credit for their discoveries. Given that the only recognition in Merton's universe came to those who established their priority in making discoveries or breakthroughs, secrecy was clearly not in a researcher's self-interest. While working on a particular problem, researchers might choose not to communicate with others about their work, but when the work was completed they would race to publish their findings. Priority was the means to a reputation, to greater credibility, and to the rewards of science—prizes, grants, and status.[9] Second, secret knowledge was not open to the scrutiny of others who might point out errors and problems related to both the production and interpretation of the knowledge claims. If, as Merton and others believed, science "worked" through the rigorous self-policing of knowledge claims, then secrecy or restricting the dissemination of information might lead to the production of false knowledge. Finally, note that Merton's norms also created an autonomous social space for science, since only other scientists could credibly discuss the veracity of specific technical knowledge claims. Those untrained in the ways of science were incapable of adjudicating intellectual matters.

If Merton and his students, especially Bernard Barber,[10] were among the prime intellectual sources for the post World War II understanding of the relationship between science and secrecy, then we must look to the war itself and the subsequent militarization of American science for the institutional context in which such discussions began. Here we must make a historical point. We

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[9] Certainly I don't mean this to be an exhaustive list, merely evocative. It is altogether too easy to translate Merton's norms into a framework for the acquisition of social capital. If we do that secrecy might become both an asset and a liability.

[10] Bernard Barber, *Science and the Social Order* (New York: Collier Books, 1962 [1952]) is an especially good source for the antithetical relationship of science and secrecy.

may think of the war, especially the Manhattan Project, as the modern occasion for our discussions of science and secrecy, but that would be a profound mistake. Discussions about secrecy were endemic with the establishment of the first industrial research laboratories in early twentieth century America and the great expansion of such laboratories in the post World War I context, what one observer called "a fever of commercial science."[11] Similarly, the fear that corporate monopolies might control the production of scientific and technological knowledge, as presented in the Temporary National Economic Condition (TNEC) Hearings of 1939, was an early analogue of postwar fears of the military control of science.[12] To an extent we are largely unaware of, wartime discussions of secrecy drew upon these earlier debates as well as the recognition that for many industry had not affected science in a negative manner. On the contrary, many began to conceive of industrial research laboratories as universities in exile, a view that had little relation to corporate reality. With this caveat, let us turn to the war.

Pick up any memoir of the Manhattan Project and one will find ringing denunciation of General Leslie Groves and his policy of compartmentalization. Even Richard Rhodes, our contemporary chronicler of nuclear history, accepts the seemingly universal condemnation of Groves' apparent obsession with security and restricting the flow of information.[13] Oppenheimer's creation of the Los Alamos seminar series is viewed by both participants and historians as a triumph of the values of science over military paranoia. Los Alamos might have been isolated, but on the Mesa science ruled. Alas, such a perspective is seriously defective. First, while some researchers, such as Szilard clearly fought the classification and compartmentalization system, others accepted security

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[11] I am embarrassed to do this, but some discussion of this issue can be found in Michael Aaron Dennis, "Accounting for Research: New Histories of Corporate Laboratories and the Social History of American Science," *Social Studies of Science* 17 (1987): 479-518.

[12] On this point, see Larry Owens, "Patents, the 'Frontiers' of American Invention, and the Monopoly Committee of 1939: Anatomy of a Discourse," *Technology and Culture* 32,4 (1991): 1076-93. For a specific example of the fear of industrial control, see Peter Galison, Bruce Hevly, and Rebecca Lowen, "Controlling the Monster: Stanford and the Growth of Physics Research, 1935-1962," pp. 46-77 in *Big Science: The Growth of Large Scale Research*, ed. Peter Galison and Bruce Hevly (Stanford: Stanford University Press, 1992).

[13] Given that so much information went to the Soviet Union, one might wonder if Groves' obsession was really so unwarranted. For Rhodes, see Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon and Schuster, 1986).

as a necessary wartime evil. Far from chafing under the demands of security, these researchers flourished and relished knowing that they were responsible for only one aspect of a larger project. Second, all such accounts view secrecy and the military as the "enemy." Unfortunately, this ignores another view of secrecy that is quite important. Secrecy and the ability to keep secrets were an important way in which the researchers might gain the confidence of their military colleagues and paymasters. Vannevar Bush, the leader of the wartime research and development establishment made this clear when he told his colleague, Karl T. Compton, the president of MIT, that

> you and I are responsible for rather serious things, and the maintenance of our relations with the Army and Navy depends upon an orderly handling that inspires confidence.[14]

Keeping secrets was essential to establishing and maintaining the credibility of the civilian researchers. This is a definition or function of secrecy that we often forget. The relationship of academic researchers and the armed forces was new; building the connections that we accept as a historical given was an accomplishment in its own time. Undergirding Bush's statement was his recognition that only by properly handling the security issues would he and his organization acquire the trust of the military officers actually planning and fighting the war. Those who, like Szilard, bridled under the security regulations became individuals whom the military effectively ignored. Playing by the military's rules about information distribution allowed one the possibility of actually having an effect upon their actions.

Another problem with our over-reliance upon the Manhattan project for our understanding of wartime secrecy is that we seldom look at the other research and development programs. Take the case of the proximity fuze, which Bush believed even more difficult than the atomic bomb. In this case, the development of a sophisticated electronic device demanded the creation of new laboratories and new forms of industrial-military-academic cooperation. Merle Tuve, the leader of the project, instituted a compartmentalization policy that extended into the worker's eating habits. Researchers often ate lunch at a local "Hot Shoppes." At one lunch, Tuve overheard laboratory workers discussing their work. This led to a wonderful memo which was posted throughout the laboratory explaining that the Hot Shoppes was not a secure site and hence any discussion of the fuze project inside the restaurant would result in the arrest of all the members of a conversational

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[14] See 1 April 1941, VB to KTC, Box 26, Folder 609 (KTC '39-'42), Vannevar Bush Papers, LC.

group. Tuve's staff got the message, loud and clear, but they did not understand Tuve's intentions. Of course, Tuve was concerned that enemy agents might be serving the meat-loaf, but more pressing was the possibility that staff members might learn about work unrelated to their own specific job assignments. Compartmentalization was a form of management as well as a security precaution. For Tuve, controlling the flow of information among the researchers was as important, if not more important than controlling the possible loss of information to an enemy.[15] Localized secrecy was the means to an end, but not an end in itself.

Secrecy might also be considered an essential element of the design process regardless of whether a nation is at war. The design and development of new technologies is marked by initial periods of contestation and struggle over goals, methods, and even the very possibility of the goal. Hence, if one is developing a new technology—such as a proximity fuse, an atomic bomb, or an inertial guidance system—it might prove beneficial to restrict the sheer number of voices until the group working on the project has produced what they believe is a stable vision or version of the technology. In other words, secrecy might reduce the stress of interpretive flexibility—the inherent plastic meaning of any technology. Take the case of inertial guidance for aircraft and ballistic missiles. For this technology to 'work' it was essential that the inertial apparatus separate the acceleration of the plane from the acceleration of gravity. For many people, including George Gamow, the famous physicist, such a separation was impossible since it would violate Einstein's relativity theory. Those involved in developing the technology were of a rather different opinion, but the multiplicity of groups working on the problem aggravated the task of responding to Gamow's criticism since there was far from one solution to his objection.[16] Had the managers of the inertial projects

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[15] On these points, see Michael Aaron Dennis, "Technologies of War: The Proximity Fuze and the Applied Physics Laboratory," in *A Change of State: Political Culture and Technical Practice in Cold War America* (monograph in process).

[16] For this specific example, see Donald MacKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge: MIT Press, 1990); and Michael Aaron Dennis, "'Our First Line of Defense': Two University Laboratories in the Postwar American State," *Isis* 85, no. 3 (1994): 427-55. Given the complexity of this particular example, it may be a poor choice. Gamow probably came to his knowledge of inertial techniques through his membership on the Air Force Science Advisory Board. Conceivably, one might argue that as a board member Gamow was only doing his job by expressing his beliefs about the untenable character of the research. What is striking is that Gamow does not appear to have visited or contacted any of the groups trying to develop this technology before he produced his critique.

kept their work a better secret they might not had to deal with Gamow's critique until after they had stabilized their devices and methods. Once again, secrecy acts as a management technique, one that is quite powerful but easily abused. One can easily imagine researchers working on a device that shows little promise, but where the secret status of the project allows the work to persist. While we have several examples of this, including the Navy's canceled A-12 stealth attack aircraft, secrecy need not necessarily breed corruption.[17]

Understanding the range of ways secrecy was part of the wartime research effort is important, but we are forced to return to the atomic bomb. Certainly the bomb was among the best kept secrets of the war: on 5 August 1945 less than 100 people knew the full scale and scope of the project.[18] Furthermore, all knowledge relating to the bomb was secret; any public discussion required an active decision to declassify particular pieces of information. Even the Smythe Report, perhaps the oddest press release in American history, did not present technical details, only a general discussion of the project and its work.[19] However, the report's final paragraph contains the fundamental idea behind the report: an informed citizenry, with the tutelage of physicists, can make an informed set of decisions about the future of nuclear weapons. The interesting point here was that the government censors were the adjudicators of what the American people needed to know about the Manhattan Project—the autonomy of science had already been breached.

The postwar debate over the legislation establishing the Atomic Energy Commission dealt extensively with the issue of secrecy, but largely in terms of the punishments for revealing America's atomic secrets. Central to the congressional discussion was a gradual shift from an emphasis on the dissemination of Manhattan's knowledge to one of restricting and finally controlling the flow of information. Just as Vannevar Bush attempted to create a new taxonomy of knowledge centered upon the elusive idea of basic research, so did the Congress create a new taxonomy of secret, the category of "restricted data" defined as

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[17] See Robert Holzer, "DOD Secrecy Drives Up Weapons Cost, Development Time," *Defense News*, 21 October 1992, p. 10.

[18] Richard Hewlett, "'Born Classified' in the AEC: A Historian's View," *Bulletin of the Atomic Scientists* 37 (December 1981): 20-27.

[19] Henry DeWolfe Smythe, *Atomic Energy for Military Purposes* (Washington, DC: GPO, 1945; Stanford: Stanford University Press, 1989).

> all data concerning the manufacture or utilization of atomic weapons, the production of fissionable material, or the use of fissionable material in the production of power, but shall not include any data which the commission from time to time determines may be published without adversely affecting the common defense and security.[20]

What does the invention of a new level of secrecy do? First, it creates an additional class of individuals who have access to restricted data. Although this might be of interest to those studying the mixing of individuals with different clearances, or how particular organizations work, it is unclear how the taxonomy affects the issues with which we are concerned.[21] Is this not simply another example of access being the rationale and meaning of secrecy? Second, the invention of restricted data reminds us that during the immediate postwar period many people spoke and acted as if the revelation of a particular piece of information might "give away" the "secret" of the bomb.[22]

For students of this period, the growth of restricted data is both a problem and a blessing. If we view secrecy as a problem in access, then we are mainly concerned with acquiring that access for ourselves. In other words, we operate under the belief that whatever is classified should be declassified or removed from the penumbra of secrecy; in turn, we will have a better idea of what actually happened. Among the many assumptions present in our call for access is the belief that the classified and the unclassified are linked in some direct and unmediated fashion; as if the light of inquiry would make the past clearer. More than likely the opposite is true—the relation of the classified and unclassified is problematic and highly mediated. Knowing the contents of restricted data might not help us reconstruct events and processes; if I learn that Beryllium is an important ingredient in thermonuclear weapons have I learned something important? Only if I am attempting to understand the growth and development of the Beryllium machining industry or the growth in

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[20] Hewlett, "'Born Classified', p. 21.

[21] For an interesting discussion of these very issues, see Hugh Gusterson, *Nuclear Rites: A Weapons Laboratory at the End of the Cold War* (Berkeley: University of California Press, 1996), pp. 68-100.

[22] On the idea that there was a single secret and its consequences see Gregg Herken, *The Winning Weapon: The Atomic Bomb in the Cold War, 1945-1950* (New York: Vintage, 1981). Shils' work, cited above, also addresses this particular conception of an "atomic secret."

incidences of complaints of Beryllium poisoning or a related inquiry.[23] In other words, restricted data in and of itself might prove more meaningless than meaningful. Hence, if access is why we are interested in secrecy we really don't have much to say other than on a case-by-case basis. It is one thing to know what actually took place at the Gulf of Tonkin by reading the previously classified cables from the region; it is another thing to know that element X is used in technology Y. Knowing secrets may be exciting, but it may not be intellectually interesting.

So, what is interesting about secrecy?

Open the newspaper nearly any day of the week: secrecy is on display. New products, like Gillette's new three-blade razor, are the result of industrial processes so guarded that they make the Manhattan Project look like a sieve.[24] Secrets are only known when they are no longer secrets, but the power to unveil and display a secret is what makes secrets useful and dangerous. These types of events and practices don't figure in our understandings of secrecy and science, despite the way in which the atomic bomb's use at Hiroshima might be likened to the unveiling of a new and powerful product.

Return to our earlier ideas about why access is not what is interesting about secrecy. What is interesting is how researchers discuss secrecy. The most common belief appears to be that secrecy is a necessary evil, but one that ultimately undermines the development of science. It is one thing to keep secrets in wartime, another to do so under the conditions of peace. Yet researchers keep secrets all the time, sometimes quite inadvertently. In his study of Toshiba's management of intellectual capital, Mark Fruin tells us that Toshiba had a great deal of trouble setting up Knowledge Works factories overseas; indeed, the skills and knowledge necessary to make a Knowledge Works factory operate are so site and person specific that there is no way to capture this know-how short of exporting the people from a successful factory. As Fruin makes clear "the nature of

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[23] Or if I am trying to build my own bomb. However, even if I learn this particular fact and others, I still need to do a great deal of work if I want my own nuke. As recent events make clear, even impoverished nations are willing to use scarce resources to build the infrastructure necessary for a nuclear arsenal. My point is simply that individual factoids are not going to teach anyone how to build a bomb.

[24] See the *Wall Street Journal*, front page, left column, 14 April 1998.

factory know-how is not contained in manuals but is found instead in practice and experience."[25] For students of science and technology studies, it is clear that Fruin is talking about tacit knowledge—that knowledge which is practice-specific and often incapable of being articulated in any formal way.[26] Unlike restricted data, tacit knowledge is not intentionally secret but it has a similar effect. Restrictions on data are about slowing the spread of a technology; similarly, an inability to transmit tacit knowledge slows the ability of Toshiba to grow and compete with other Japanese and American firms. Clearly, however, tacit knowledge doesn't count as secrecy; rather it is part of the "tricks of the trade."

Another reason researchers argue against secrecy is the claim embodied in the Smythe report: secrecy denies the public the ability to learn about issues vital to the survival of the polity. There is an element of truth here, but not very much. Recall that during the debate over the H-bomb Leo Szilard believed the American public incapable of making the right decision with respect to the weapons' development.[27] More information was not going to help the public; the decision had to be made by those who knew best: physicists. Restricted data created a community of inquirers capable of making the best possible decision.

Szilard's world was far from democratic. Accountability was a problem for everyone but scientists. Despite his obsession with secrecy, Szilard accepted a political ideal that was a pure technocracy; a point made clear in his seminal story, "The Voice of the Dolphins."[28] Readers will recall that the story's underlying narrative, that intelligent dolphins rather than politicians were capable of ending the nuclear arms race, rested upon keeping the dolphins' actual work practices secret. In turn, after the story's happy ending, Szilard reveals the possibility that the dolphins were simply a cover for scientists imposing their rational vision upon international politics. In Szilard's

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[25] W. Mark Fruin, *Knowledge Works: Managing Intellectual Capital at Toshiba* (New York: Oxford University Press, 1997), p. 162.

[26] On tacit knowledge, see H.M. Collins and R.G. Harrison. "Building a TEA Laser: The Caprices of Communication," *Social Studies of Science* 5 (1975): 441-50.

[27] On Szilard's undemocratic perspective, see Peter Galison and Barton Bernstein, "In Any Light: Scientists and the Decision to Build the Superbomb, 1952-1954," *Historical Studies in the Physical and Biological Sciences* 19 (1989): 267-347.

[28] Leo Szilard, *The Voice of the Dolphins and Other Stories*, exp. ed. (Stanford: Stanford University Press, 1961, 1992).

universe secrecy prevented the uninformed from playing an authoritative role in politics. Ignorance was more than bliss, it was the basis upon which one might erect a rational political order.

If, as Yaron Ezrahi argues, science plays an authoritative and constitutive role in liberal democratic polities because it is transparent, then secrecy might undermine democracy.[29] Transparency refers to the public's ability to see the process through which authoritative claims are made; conceivably, anyone with enough time and patience might gather "the facts" and understand how a decision was made or a policy developed. Diane Vaughan's account of the Challenger disaster is an example of the belief in transparency; Vaughan's meticulous reconstruction of the cultures of NASA and Morton Thiokol as well as the conversations leading to the launch decision exemplify transparency's political value.[30] Vaughan as both scholar and citizen wades through the documents and pieces together what she believes is the actual story. The alleged transparency of technical processes, the belief that with enough time and resources we might understand any given decision, appears at odds with secrecy. Alternatively, transparency might rest upon the credibility of researchers who vouch for the truth of what takes place in the classified world. Individual researchers become spokespeople for the government's massive investment in secret research. In turn, the credibility of individuals becomes a surrogate for the credibility of the state. In this sense, secrecy and democratic politics don't appear as diametrically opposed as researchers and analysts might believe.

Reading accounts of secrecy in science from the postwar era written by researchers or those involved in the loyalty and security programs reveals a common strand: a belief that secrecy was a new evil. That is, whether it is Shils' *The Torment of Secrecy* or Wiener's *Invention* or his autobiography *I am a Mathematician*, one is struck by the overwhelming sense of nostalgia for a time when secrecy did not affect science. Read as Wiener discusses the state of science in 1956:

> There is not doubt that the present age, particularly in America, is one in which more men and women are devoting themselves to a formally scientific career than ever before in history. This does not mean that the intellectual environment of science received a proportionate increment. Many of today's American scientists are

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[29] Yaron Ezrahi, *The Descent of Icarus: Science and the Transformation of Contemporary Democracy* (Cambridge: Harvard University Press, 1990).

[30] Diane Vaughan, *The Challenger Launch Decision: Risky Technology, Culture, and Deviance at NASA* (Chicago: University of Chicago Press, 1996). I owe this insight, even in this mangled form, to my colleague, Sheila Jasanoff.

> working in government laboratories, where secrecy is the order of the day, and they are protected by the deliberate subdivision of problems to the extent that no man can be fully aware of the bearing of his own work. These laboratories, as well as the great industrial laboratories are so aware of the importance of the scientist that he is forced to punch the time clock and to give an accounting of the last minute of his research. Vacations are cut down to a dead minimum, but consultations and reports and visits to other plants are encouraged without limit, so that the scientist, and the young scientist in particular, has not the leisure to ripen his own ideas.[31]

The poignant character of Wiener's lament should not be lost on us, but it is important that this is a complaint about two different issues. First, losing control over the direction of research. Second, losing control over the actual content of the knowledge produced by the researcher. Secrecy was an imposition from those who did not understand the Mertonian ethos that scientists took for granted. In other words, the scientist always possessed dual citizenship: first, in what Michael Polanyi called the "republic of science" and next in a particular nation-state.[32] Implicit in the Mertonian formulation that Wiener and researchers embraced was the very possibility of divided loyalties. Choosing between science and country became something akin to choosing between a friend and country. Research problem choice could be seen as a way of assessing loyalty to a government; even if a researcher did not find the work interesting s/he would have to work on the project or risk being labeled as disloyal. The norms of science and the norms of secrecy were not merely antithetical, they were mutually exclusive.

Wiener's recognition that secrecy, citizenship, and knowledge-production were of a piece implied that secrecy affected the very content of knowledge. This is certainly a far more controversial point since we are leaving the realm of access behind. Wiener's point, and that of Edward Shils', was not simply the question of economic rationality. That is, secret science forced the unnecessary duplication of work that had already been completed. Rather, it was a qualitative point more difficult to address. Put simply, Wiener is arguing that one gets a certain type of knowledge from a particular social organization, in this case a secret organization or research that is secret. This knowledge is different than what might be produced in a more open space. The argument is not that secrecy allows "bad" or incompetent science to flourish, although that was certainly a possibility if one believed in the scientific community's homeostatic propensities. Instead, it

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[31] Wiener, *I am a Mathematician*, p. 361.

[32] Michael Polanyi, *Science, Faith and Society* (Chicago: University of Chicago Press, 1946).

was an argument about the constraints and conditioning of the imagination. Secret knowledge produced a different map of intellectual geography, a different sense of the horizons of possibility. Pursued over time, such knowledge would produce an entirely different and separate world, one in which access would be the least of an outsider's problems. Even with access, the outsider would find themselves as visitors in a foreign country without any sense of the nation's language or grammar. Obviously, translation would prove possible over time, but such a scheme undermined the possibility of claims to universalism, let alone the claim that scientific knowledge was public property. Secrecy eroded the extent to which scientific knowledge, and concomitantly the world explained with that knowledge, might serve as a common currency for culture across boundaries.[33]

We might also read these discussions of secrecy as versions of Paul Forman's belief that knowledge is made to order; you get what you pay for.[34] That is, secrecy is at one with the idea that scientists are employees following orders. As employees why should we expect that they would control the content and direction of their research? While such a perspective is attractive, it does not appear to connect with the ways that scientists present themselves; indeed, we might read Forman as being more like Wiener and Shils insofar as he laments the transformation of physics into its secret and corporate present.

On the Matter of Conclusions?

Far from being straightforward, the relationship of secrecy and the production of knowledge opens up a hermeneutic can of worms that science and technology studies must address. Part of the problem is that conventional understandings of science are inadequate to the task because they are implicated in the problem. In her work on research subpoenas, Sheila Jasanoff makes it clear that simply acquiring access to the raw materials that an investigator uses to write a scientific paper does not provide one with a road map to the construction of any particular paper.

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[33] I think that this constraining of possibilities is what Ian Hacking is going on about in Ian Hacking, "Weapons Research and the Form of Scientific Knowledge," in *Nuclear Weapons, Deterrence, and Disarmament*, ed. David Copp (Calgary: University of Calgary Press, 1986).

[34] See Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," *Historical Studies in the Physical Sciences* 18 (1987): 149-229; and Paul Forman, "Inventing the Maser in Postwar America," *Osiris (2nd ser.)* 7 (1992): 105-34.

Instead, such access transforms those demanding the data into interpreters who must provide their own story about the materials or explain why the materials cannot be used to make the claims that are at issue.[35] Lawyers have an advantage generally not available to historians or sociologists: the discovery process. More recently, discovery has acquired a new meaning. At MIT students working for startup companies established by individual professors are required to sign non-disclosure agreements, i.e., contracts that forbid the student from discussing the product under development; Professors working on related products have allegedly designed homework assignment to determine the nature and status of a competitor's work. Student employees are caught in a bind: violate their non-disclosure agreement or fail the homework assignment.[36] Industrial espionage masked as pedagogy has brought the marketplace squarely into the classroom, but it also raises the issues of secrecy in a powerful and palpable form.

We can not acquire all the relevant materials, no matter how much we desire to do so. At the same time we need to think of ways to discuss how the classified world relates to the world to which we do have access. How are we to imagine the relations between realms that have very different reciprocal relations. Once again, we are back to questions of access, but with a difference. The question is not how to access this world, but how to assess that world's impact on what is visible.[37] How is the hand that stamps the security seal on a document linked to the hands that write the document? Is our situation reminiscent of the physicist studying a black hole: how can we find out what happens in a black hole if nothing can escape from it? Or, is it that some things do move

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[35] Sheila Jasanoff, "Research Subpoenas and the Sociology of Knowledge," *Law and Contemporary Problems* 59, Summer (1996): 95-118. Obviously this point is also related to the historian's use of laboratory notebooks in reconstructing scientific and technical practices. How is what is in the notebook related to what is in the published document? A fascinating example of this is found in Gerald L Geison, *The Private Science of Louis Pasteur* (Princeton: Princeton University Press, 1995). Note that I have not discussed what Merton and others take for granted—the need for some secrecy in the quest for priority—since such claims rest on an assumption of openness.

[36] See Amy Decker Marcus, "MIT Students, Lured to New Tech Firms, Get Caught in a Bind," *Wall Street Journal*, 24 June, 1999, A1.

[37] Ron Doel is getting at a related idea near the end of his essay, "Scientists as Policymakers, Advisors, and Intelligence Agents: Linking Contemporary Diplomatic History with the History of Contemporary Science," pp. 215-44 in *The Historiography of Contemporary Science and Technology*, ed. Thomas Söderqvist (Amsterdam: Harwood Academic Publishers, 1997).

from the classified to the unclassified worlds—people, for example, and information. By studying the shape and form of what we can see, might we not make inferences about the secret world?[38] Or is it, as Wiener suggested, utterly outside the scope of our imaginations?

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[38] For example, could we not argue that the International Geophysical Year (IGY) of 1957 was simply arms control by other means? That is, by measuring the earth's gravitational field and producing sophisticated maps of the Arctic, Russia and the U.S. acquired the information necessary to allow inertial guidance systems to fly to their targets with a greater degree of accuracy. That is, more information allowed for greater claims of inevitable destruction.

GOVERNMENT SECRECY AND KNOWLEDGE PRODUCTION: A SURVEY OF SOME GENERAL ISSUES

Steven Aftergood

Introduction

Secrecy and the production of knowledge are, to all appearances, in conflict. Certainly the self-understanding of the scientific enterprise asserts the essential importance of the open exchange of information, which is the very opposite of secrecy. According to one of the nation's leading scientific societies, "The basic function of the scientific community is the advancement of knowledge, including its clarification, interpretation, diffusion, and evaluation."[1]

If science pursues the advancement of knowledge generally, including the diffusion of that knowledge, secrecy emphasizes the value of *differential* knowledge: If I can prevent you from knowing something that I know, I may be able to derive benefits in terms of military or economic advantage from the secret knowledge that I hold. By doing so, however, I may at some point inhibit my own ability to gain new knowledge. This paper briefly surveys the national security classification system, and considers several instances where official secrecy has intersected with the production of technical knowledge—for good or ill.[2]

An Overview of the National Security Classification System

> Our democratic principles require that the American people be informed of the activities of their Government. Also, our Nation's progress depends on the free flow of information. Nevertheless, throughout our history, the national interest has

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[1] John T. Edsall, *Scientific Freedom and Responsibility* A Report of the AAAS Committee on Scientific Freedom and Responsibility (Washington, DC.: American Association for the Advancement of Science, 1975), p. x.

[2] There is a sizable literature on the conflict between science and national security that I will not even attempt to summarize. See, for example: Harold C. Relyea, *Silencing Science: National Security Controls and Scientific Communication* (Norwood, NJ: Ablex Publishing, 1994); and Herbert Foerstel, *Secret Science: Federal Control of American Science and Technology* (Westport, CT: Praeger Publishers, 1993). On national security secrecy generally, see Sen. Daniel P. Moynihan (chair), *Report of the Commission on Protecting and Reducing Government Secrecy*, U.S. Government Printing Office, March 1997 <http://www.fas.org/sgp/library/moynihan/index.html>.

> required that certain information be maintained in confidence in order to protect our citizens, our democratic institutions, and our participation within the community of nations.[3]

Government imposes restrictions on information for a variety of reasons—to protect personal privacy, to preserve the confidentiality of law enforcement investigations and diplomatic initiatives, and to prevent "damage to national security," an objective whose definition is fluid and to a certain degree subjective. This latter function, the use of controls on information in order to protect national security, is the purpose of the national security classification system. The current classification system is governed by Executive Order 12958, issued by President Clinton in April 1995. (A separate, but parallel, classification system is rooted in the Atomic Energy Act of 1954 and applies solely to "atomic energy information.")

Information that is owned by, produced for, or otherwise controlled by the U.S. government may be "classified" (i.e., withheld from disclosure) if it concerns one of the following categories:[4]

- military plans, weapons systems, or operations;
- foreign government information;
- intelligence activities (including special activities), intelligence sources or methods, or cryptology;
- foreign relations or foreign activities of the United States, including confidential sources;
- scientific, technological, or economic matters relating to the national security;
- United States Government programs for safeguarding nuclear materials or facilities; or
- vulnerabilities or capabilities of systems, installations, projects or plans relating to the national security.

Even information that does fall into one of these categories is not supposed to be classified unless a responsible official determines that its disclosure "reasonably could be expected to result in damage to the national security" and the official can identify or describe that damage. Further-

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[3] Executive Order 12958, "Classified National Security Information," 60 *Federal Register 19825*, April 20, 1995.

[4] "Classified National Security Information," section 1.5.

more, “Basic scientific research information not clearly related to the national security may not be classified,” the Order directs.[5]

That is the theory; the actual practice is considerably more complex.

One degree of complexity arises from the enormous size and volume of the secrecy system. The number of government officials who are authorized to designate information classified was most recently reported to be 4,420.[6] Inevitably, the expectation of what might result in damage to national security will vary considerably among these thousands of individuals, and it is possible to find startling discrepancies in the classification and declassification practices of various agencies.[7] The total number of classification actions reported in the most recent year alone was over 5.7 million. “How much classified information is contained in the total universe of classified information?” That is a question that “we cannot definitively answer,” the Information Security Oversight Office reported to the President. Nevertheless, it is clear that there are well in excess of one billion pages of classified documents that are over 25 years old which have been deemed historically valuable.

Three Categories of Secrecy

A different sort of complexity has to do with the subjective aspect of the classification system and its resulting susceptibility to abuse. In the actual practice of national security classification, it is possible to discern three general categories: genuine national security secrecy, political secrecy, and bureaucratic secrecy.[8]

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[5] “Classified National Security Information,” section 1.8b

[6] Information Security Oversight Office, “1996 Report to the President,” National Archives and Records Administration, 1997 <http://www.fas.org/sgp/isoo/isoo96.html>.

[7] The essentially arbitrary, or at least subjective, nature of the classification process has encouraged one research strategy sometimes used by historians and others, i.e., requesting the declassification of the same document from multiple agencies, since different agencies will often release (and withhold) different portions of a particular classified document.

[8] This discussion is borrowed from an earlier paper: “Secrecy and Accountability in U.S. Intelligence,” prepared for the Center for International Policy, October 1996 <http://www.fas.org/sgp/cipsecr.html>.

Genuine national security secrecy pertains to that information which, if disclosed, could actually damage national security in some identifiable way. Without attempting to conclusively define "national security" or "damage," common sense suggests that this category would include things like design details for weapons of mass destruction and other advanced military technologies, as well as those types of information that must remain secret in order for authorized diplomatic and intelligence functions to be performed.[9] This, of course, is the only legitimate form of national security secrecy.

Political secrecy refers to the deliberate and conscious abuse of classification authority for political advantage, irrespective of any threat to the national security. This is the least common of the three categories, but the most dangerous to the political health of the nation. Perhaps the most extreme example of political secrecy historically was the classification of CIA behavior modification experiments on unknowing human subjects, as in the MKULTRA program. To guarantee the permanent secrecy of this activity, most MKULTRA records were destroyed in the early 1970s.[10]

An exceptionally blunt expression of political secrecy is contained in a 1947 Atomic Energy Commission memorandum which instructs that

> It is desired that no document be released which refers to experiments with humans and might have adverse effect on public opinion or result in legal suits. Documents covering such work . . . should be classified "secret."

This memorandum itself remained classified Secret until its declassification in 1994.[11]

The third category is what may be called *bureaucratic secrecy*. As classically described by Max Weber, this has to do with the tendency of all organizations to limit the information that they release to outsiders so as to control perceptions of the organization. Bureaucratic secrecy appears

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[9] President Nixon's Executive Order 11652 gave the following examples of what would constitute "exceptionally grave damage" to national security: armed hostilities against the United States or its allies; disruption of foreign relations vitally affecting the national security; the compromise of vital defense plans or complex cryptologic and communications intelligence systems; the revelation of sensitive intelligence operations; and the disclosure of scientific or technological developments vital to national security.

[10] See, generally, John Marks, *The Search for the 'Manchurian Candidate': The CIA and Mind Control* (New York: Times Books, 1979).

[11] "Medical Experiments on Humans," Memorandum from O.G. Haywood, Jr. to Dr. Fidler, Atomic Energy Commission, April 17, 1947, attached herewith (Appendix A).

to be the predominant factor in current classification practice, accounting, in my opinion, for the majority of the billions of pages of classified records throughout government.

There is inevitably a subjective factor involved in assigning a particular unit of information to one of these three categories of secrecy. The borders of the three categories may sometimes be blurred in practice. Furthermore, information that falls in one category at one moment will often belong in another category at some later date. Responsible classification management—i.e., the elimination of all but genuine national security secrecy—therefore depends to a large degree on the good judgment and the good will of the classification officials themselves.

When responsible classification management fails, or when classification authority is abused, the result is . . . pathological secrecy.

Pathological Secrecy

In the best of cases, secrecy undercuts the possibility of peer review and oversight. In the worst of cases, secrecy will be applied far out of proportion to any requirements of national security and will lead to bad policy, sometimes on a large and expensive scale. There are several instances in the last decade in which secrecy has caused or contributed to the failure of multi-billion dollar technology programs.

The Navy's A-12 attack aircraft program is something of a paradigm of a secret program run amok. The A-12 was a "special access" program, which means that access to information about the program was strictly limited using controls above and beyond those applied to other classified information. Because of these stringent controls on access, oversight was inhibited and officials were slow to learn that the program could not possibly accomplish its goals, resulting in its cancellation in 1991 after the expenditure of some $2.7 billion dollars. "The fact that it was a special access program, and the fact that there were limited clearances granted to oversight individuals to look at the program certainly were contributing factors" in the program failure, according to the Department of Defense Inspector General.[12] Secrecy was likewise a contributing

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[12] House Armed Services Committee, hearing on "The Navy's A-12 Aircraft Program," 101 Congress, December 10, 1990 [HASC No. 101-84], p. 88.

factor in the failure of several other large special access programs including the $3.9 billion Tri-Service Standoff Attack Missile (TSSAM)[13] and the Tacit Rainbow anti-radar missile.[14]

Abuses of classification authority on a smaller scale are even more common. The decision to classify the TIMBER WIND nuclear rocket propulsion program as an unacknowledged special access program "was not adequately justified," according to a 1992 Department of Defense Inspector General audit.[15] The Strategic Defense Initiative Organization "continued to safeguard its association with the technology for reasons that were not related to national security." The program was terminated within two years after its existence was disclosed (without authorization) to the public.[16]

Alert members of Congress eventually began to detect a pattern and a common thread in such failures. As the House Armed Services Committee put it:

> The Committee believes that the Special Access classification system has progressed beyond its original intent, and that *it is now adversely affecting the national security* it is intended to support.[17]

While oversight of the most highly classified special access programs seems to have improved in last few years, anecdotal reports indicate a continuing problem with pathological secrecy.

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[13] Bradley Graham, "Missile Project Became a $3.9 Billion Misfire," *The Washington Post*, April 3, 1995, page A1. "Inhibiting wider scrutiny of TSSAM was its highly classified nature . . . Northrop's Kresa said the secrecy surrounding this and other cruise missile projects complicated his company's attempts to hire qualified people . . . ."

[14] On so-called "black programs" generally, see Tim Weiner, *Blank Check: The Pentagon's Black Budget* (New York: Warner Books, 1990).

[15] Department of Defense Inspector General, "The TIMBER WIND Special Access Program," Report Number 93-033, December 16, 1992. "Pentagon Audit Blasts SDI Nuclear Rocket Classification," by Joseph Lovece, *Defense Week*, January 11, 1993.

[16] William J. Broad, "Rocket Run by Nuclear Power Being Developed for 'Star Wars'," *New York Times*, April 3, 1991; R. Jeffrey Smith, "U.S. Developing Atom-Powered Rocket," *Washington Post*, April 3, 1991; "DoD Cancels Plans for Nuclear Rocket," by Vincent Kiernan, *Space News*, May 17-23, 1993.

[17] House Armed Services Committee, "National Defense Authorization Act for Fiscal Years 1992 and 1993," Report No. 102-60, May 1991, p. 101.

> [Philip] Odeen [chairman of the 1997 National Defense Panel] confirmed that a number of secret weapons were not used in the Persian Gulf war either because their capabilities couldn't be revealed to commanders—or because they were offered too late in the conflict. "Guys came to us saying they had something that would win the war," one wartime commander told us. "When I asked what it was, they'd say, 'I can't tell you,' or 'I can't reveal the effects,' or 'I can't tell you how it would work with other systems.' We told them to get the hell out."[18]

Of course, not all secret programs are failures. In some important cases, secrecy may actually have contributed to success.

CORONA: A Secret Success Story

Secrecy is not absolutely incompatible with the advancement of scientific and technical knowledge. Some of the most dramatic technological breakthroughs have been achieved under a rigorous framework of official controls on information. The development of the atomic bomb is one example. The United States' first satellite reconnaissance program, codenamed CORONA, is another.[19]

CORONA, which began in 1960 and continued until 1972, was a joint effort of the Central Intelligence Agency, the Advanced Research Projects Agency, and the Air Force. To say that CORONA revolutionized intelligence and space exploration would be no exaggeration. According to an official history of the program:

> The totality of CORONA's contribution to U.S. intelligence holdings on denied areas and to the U.S. space program in general is virtually unmeasurable. Its progress was marked by a series of notable firsts: the first to recover objects from orbit, the first to deliver intelligence information from a satellite, the first to produce stereoscopic satellite photography, the first to employ multiple reentry

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[18] James R. Asker, ed., "Washington Outlook," *Aviation Week & Space Technology* 147 (October 13, 1997): 21. See also a discussion of the emergence of the Stealth Fighter from classified status, which posed the question: "If the very existence of the aircraft is to be protected at the expense of using it, what is the purpose for having such a weapon?" Jim Cunningham, "Cracks in the Black Dike: Secrecy, The Media, and the F-117A", *Airpower Journal* (Fall 1991): 32. <http://www.cdsar.af.mil/apj/cunn.html>.

[19] See also the chapter by John Cloud, this volume.

> vehicles, and the first satellite reconnaissance program to pass the 100-mission mark.[20]

Most important of all, CORONA permitted an empirical assessment of Soviet military capabilities—a field previously dominated by worst-case thinking.

On its way to ultimate success, however, CORONA suffered a series of daunting setbacks that would have doomed another program. The first dozen launches were all failures. Of the first 30 missions, only 12 were productive.[21] Although several of the launch failures (and some of the successes) were noted in the press at the time, the overall secrecy of the program, together with the urgent need for its success, helped shield CORONA from the political consequences of its recurring failures and nurtured the program to a successful conclusion.

A View from Industry

One might suppose that defense contractors would enthusiastically support the secrecy system, since they are the beneficiaries of several billion dollars of secret government largesse each year. But that is not necessarily the case.

The legendary Lockheed Skunk Works, the most famous of the defense contractors specializing in classified programs, has also offered outspoken criticism of secrecy policies. Ben R. Rich, who participated in the trailblazing Skunk Works projects to develop the U-2 spy plane, the SR-71 Blackbird, and the F-117 Stealth Fighter, wrote:

> A classified program increases a manufacturer's costs up to 25 percent . . . In the past, the government has slapped on way too many security restrictions in my view. Once a program is classified secret it takes an act of God to declassify it . . . What was secret in 1964 often is probably not even worth knowing about in 1994. I would strongly advocate reviews every two years of existing so-called black programs either to declassify them or eliminate them entirely. . . .
>
> Secrecy classifications are not inconsequential but a burden to all and horrendously expensive and time-consuming. If necessarily in the national interest, these expenses and inconveniences are worthwhile. But we ought to make damned

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[20] Kenneth E. Greer, "CORONA," *Studies in Intelligence*, Spring 1973; reprinted in *CORONA: America's First Satellite Program*, Kevin C. Ruffner, ed. (Washington, DC: Center for the Study of Intelligence, Central Intelligence Agency, 1995), p. 37.

[21] Greer, "CORONA," p. 1.

> sure that the secrecy stamp is absolutely appropriate before sealing up an operation inside the security cocoon.[22]

Mr. J.S. Gordon, the current President of Lockheed Martin Skunk Works, elaborated further on some of industry's concerns about secrecy policy:

C In original classification, the government has often relied on outdated perceptions concerning the value of the information, the whims of an overzealous classification official or, if all else fails, the status quo.
C Overclassifying technology inhibits information exchange between programs and leads to "reinventing the wheel."
C Classifying contractual and financial data within a corporation, which in today's environment should rarely be classified, inhibits accurate forecasting, limits oversight, and could eventually lead to an erosion in shareholder value based on unavailability of information for analysis.
C From a legal standpoint, classifying unnecessary paperwork can put the company and the customer in jeopardy of union actions and lawsuits.[23]

It appears, then, that official secrecy often exceeds the identifiable requirements of national security. If secrecy provides political "cover" and shields certain programs from the prying eyes of overseers, it also imposes an unwelcome burden on the "knowledge producers" themselves.

An Official Critique: the 1970 Defense Science Board Report

The disadvantages that secrecy imposes on knowledge production have not gone unnoticed by the government agencies that are the authors of that secrecy.

These disadvantages were described with unusual clarity by a 1970 Defense Science Board Task Force on Secrecy, created by the Director of Defense Research and Engineering and submitted to the Secretary of Defense. The Task Force, chaired by Dr. Frederick Seitz, concluded notably that "more might be gained than lost if our nation were to adopt—unilaterally, if necessary—a policy of complete openness in all areas of information."[24] Further:

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[22] Ben R. Rich and Leo Janos*, Skunk Works* (Boston: Little, Brown & Company, 1994), pp. 333-34.

[23] J.S. Gordon, Point Paper, "Response to Commission on Protecting and Reducing Government Secrecy Request for Information," Lockheed Martin Skunk Works, 13 September 1995, available at <http://www.fas.org/sgp/othergov/skunkworks.html>.

[24] The Task Force quickly added, however, that "in spite of the great advantages that might accrue from such a policy, it is not a practical proposal at the present time." *Report of the Defense Science*

> With respect to technical information, it is understandable that our society would turn to secrecy in an attempt to optimize the advantage to national security that may be gained from new discoveries or innovations associated with science and engineering.
>
> However, it must be recognized, first, that certain kinds of technical information are easily discovered independently, or regenerated, once a reasonably sophisticated group decides it is worthwhile to do so. In spite of very elaborate and costly measures taken independently by the US and the USSR to preserve technical secrecy, neither the United Kingdom nor China was long delayed in developing hydrogen weapons.
>
> Also, classification of technical information impedes its flow within our own system, and may easily do far more harm than good by stifling critical discussion and review or by engendering frustration. There are many cases in which the declassification of technical information within our system probably had a beneficial effect and its classification has had a deleterious one:
>
> (1) The U.S. lead in microwave electronics and in computer technology was uniformly and greatly raised after the decision in 1946 to release the results of wartime research in these fields.
>
> (2) Research and development on the peaceful uses of nuclear reactors accelerated remarkably within our country, as well as internationally, once a decision was made in the mid-1950s to declassify the field.
>
> (3) It is highly questionable whether transistor technology would have developed as successfully as it has in the past 20 years had it not been the object of essentially open research.[25]

The Task Force also offered the following "sociological" observation:

> it was noted that the laboratories in which highly classified work is carried out have been encountering more and more difficulty in recruiting the most brilliant and technical minds. One member of the Task Force made the pessimistic prediction that, if present trends continue for another decade, our national effort in weapons research will become little better than mediocre.[26]

As if to confirm this latter prediction, U.S. Army General (ret.) William E. Odom wrote recently that most military laboratories have become worse than useless:

> Major savings could be achieved by abolishing virtually all the Defense Department and military service laboratories. Few of them have invented anything of note

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Board Task Force on Secrecy, Office of the Director of Defense Research and Engineering, 1 July 1970. <http://www.fas.org/sgp/othergov/dsbrep.html>.

[25] *Report of the Defense Science Board Task Force on Secrecy*, p. 9.

[26] *Report of the Defense Science Board Task Force on Secrecy*, p. 11.

> in several decades, and many of the things they are striving to develop are already available in the commercial sector . . . Because they are generally so far behind the leading edges in some areas, they cause more than duplication; they also induce retardation and sustain obsolescence.[27]

Conclusion

There is a remarkable consensus among all concerned that secrecy has an adverse effect on the production of technical knowledge. At a minimum, secrecy increases costs and diverts precious resources into the large security infrastructure.[28] At a maximum, secrecy produces intellectual stultification and shields corruption or mismanagement.

Against this view, it can be argued that secrecy is nevertheless sometimes necessary to protect a sensitive technology from adversaries who would seek to duplicate it or negate its value. Though not strictly a legitimate function, secrecy can also protect a fragile program from domestic political interference or opposition.

There is a further consensus among all concerned that there is “too much” secrecy. It would be difficult or impossible to find any official spokesman who would claim that official secrecy is already at its essential minimum level and must not be reduced further. Unfortunately, however, this consensus exists only on a general plane. As soon as the secrecy of a particular program or category of information is called into question, the consensus breaks down. Many a classified program manager will doubt the need for secrecy in someone else’s program, but is certain that his own program must remain secret.

As a result, it has proved difficult to substantially reduce the scope of official secrecy in technology, although some notable steps have been accomplished in the last several years by the Department of Energy, the Air Force and other agencies, due to agency leadership at senior levels.

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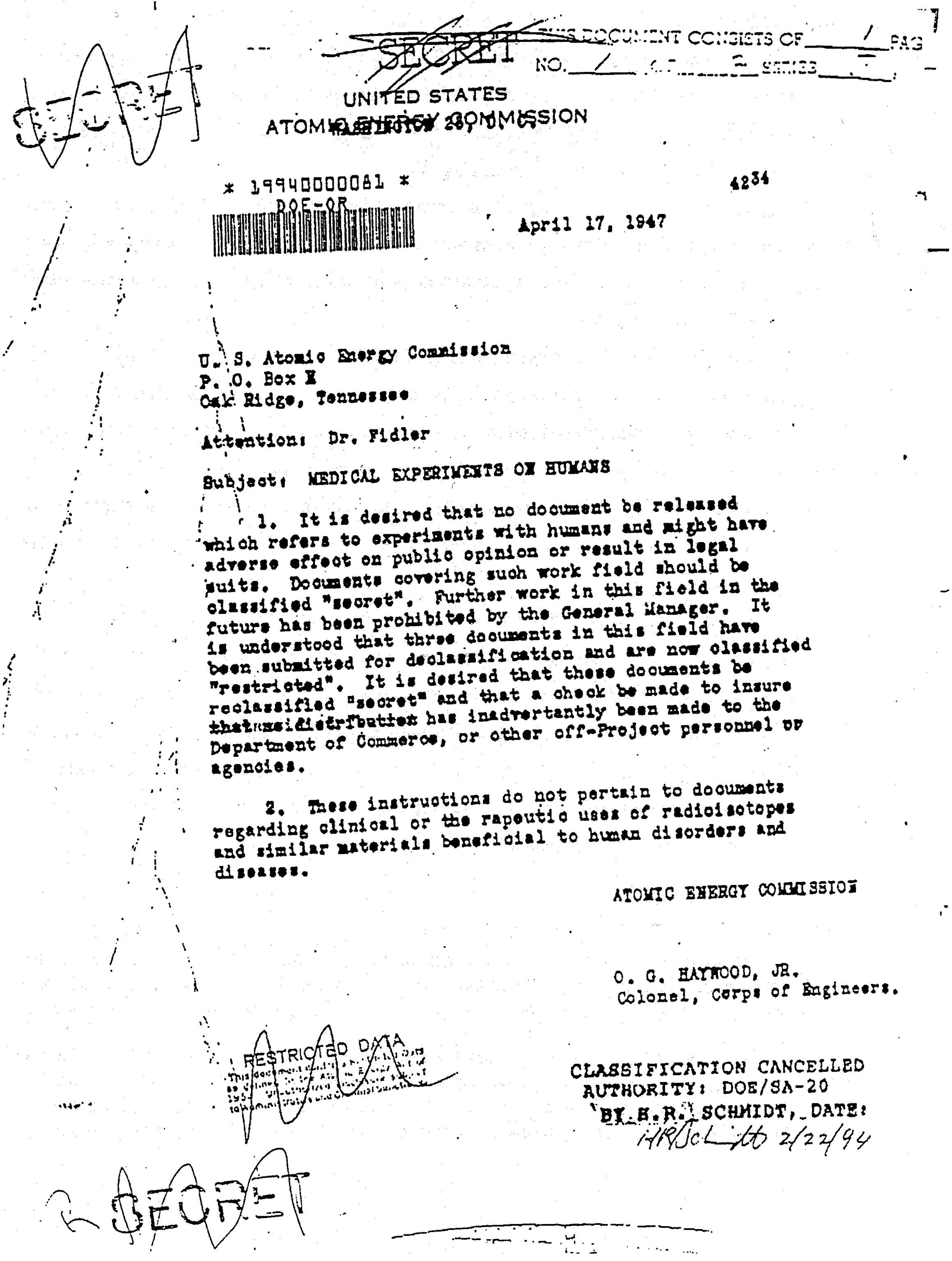
[27] Lt. Gen. (ret.) William E. Odom, *America’s Military Revolution: Strategy and Structure After the Cold War* (Washington, DC: The American University Press, 1993), p. 159. For a more nuanced appraisal of the problems of a particular laboratory, including its “culture of insularity,” see Commission on Physical Sciences, Mathematics, and Applications, National Research Council, *1997 Assessment of the Army Research Laboratory* (Washington, DC: National Academy Press, 1998).

[28] The total classification-related security costs in government and industry reached $5.2 billion in FY 1996, according to the Information Security Oversight Office “1996 Report.” This includes the costs of information security, physical security, and personnel security. Some three million citizens hold security clearances for access to classified information, which must be periodically reviewed.

But if it is true that secrecy is incompatible with knowledge production, this may turn out to be a self-correcting problem over the long term. To the extent that secrecy fosters inefficiency and stifles creativity, innovation will increasingly be found outside of the secret laboratories, which may eventually suffocate in their own splendid isolation.

Appendix A



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4234

April 17, 1947

U. S. Atomic Energy Commission
P. O. Box E
Oak Ridge, Tennessee

Attention: Dr. Fidler

Subject: MEDICAL EXPERIMENTS ON HUMANS

1. It is desired that no document be released which refers to experiments with humans and might have adverse effect on public opinion or result in legal suits. Documents covering such work field should be classified "secret". Further work in this field in the future has been prohibited by the General Manager. It is understood that three documents in this field have been submitted for declassification and are now classified "restricted". It is desired that these documents be reclassified "secret" and that a check be made to insure that no distribution has inadvertantly been made to the Department of Commerce, or other off-Project personnel or agencies.

2. These instructions do not pertain to documents regarding clinical or therapeutic uses of radioisotopes and similar materials beneficial to human disorders and diseases.

ATOMIC ENERGY COMMISSION

O. G. HAYWOOD, JR.
Colonel, Corps of Engineers.

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AUTHORITY: DOE/SA-20
BY H. R. SCHMIDT, DATE:
HRSchmidt 2/22/94

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THE ROLE OF GOVERNMENT IN THE PRODUCTION AND CONTROL OF SCIENTIFIC AND TECHNICAL KNOWLEDGE

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Introduction

This paper addresses the federal government's role in the production and control of scientific knowledge and technical know-how. It describes some of the positive and negative aspects of national security (secrecy) controls on both knowledge production and its export to other countries. I conclude by posing a few questions for further consideration.

The Government's Role in Knowledge Production and Control

The job of government is to protect and serve its people in an efficient and effective manner.[1] By contrast, the bottom line of business is to make a profit. This difference leads to different considerations with respect to secrecy.

Our government has a practical as well as a statutory role in the production of scientific and technical knowledge and its export. A major concern of the government in these activities stems from its primary mission of protecting its people, institutions, and lands from destruction by external aggressors, both declared and potential.

The intelligence and weapons systems, which we rely upon to detect hostile actions as well as to defend the United States, are the intellectual property of our nation's scientists, engineers, and many others in government, academe, and industry. Considering what was actually and vividly at stake during the Cold War years—namely the survival of our population and institutions—it was reasonable to expect that our government would protect such militarily critical technology and goods from the former Soviet Union and from any other potential adversary's acquisition for as long as possible. For this reason and others, the government imposes certain restrictions on who has a valid "need to know." These restrictions are implemented in a variety of ways, the security

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[1] It should be noted that when the term "government" is used in this paper, it generally refers to the Executive Branch of the federal government. In the discussion which follows, the primary role of the Legislative Branch is to provide the money needed to fund the research proposed by the Executive Branch.

classification system being most prominent. Other mechanisms are used as well, including, among other things, the National Disclosure Policy (NDP) and the Armed Forces Patent Review Board (AFPRB). The NDP is used to determine the level of access to classified information that each country worldwide gets for a number of information categories. One of the categories is scientific and technical information and research. The AFPRB makes decisions on which patents pending should be covered by a patent secrecy order.

A nation’s science and technology achievements gives it both real and perceived power, in a military, economic, political, and diplomatic sense. From the national perspective, scientific and technological leadership in militarily critical areas can give a nation capabilities it could not possess otherwise. Again, in a national security context, it can provide a country a special edge, or competitive position relative to actual or potential adversaries, both in real as well as in perceived terms. Our nation’s capabilities in nuclear weapons design, computer technology, space reconnaissance, electronic micro-miniaturization, and stealth technology are obvious examples of such leadership.

The objective of “secrecy” classifications in these cases is simple. It is to preserve the lead time of the United States as long as possible. Experienced government professionals know you can’t keep scientific knowledge or technology secret forever. Their goal is to make the time it takes for a potential (or real) enemy to acquire the technology as long as possible, and to make its acquisition as costly as possible. I assure you that this was my goal when I had responsibility for making decisions on technology export cases for the U.S. Defense Department.

Do such secrecy precautions cost? Of course they do, sometimes inordinately so. Should some things be unclassified? Definitely! In a society where you may get severely punished for not classifying at the proper level but are not punished for over-classifying, you get the result you would expect: over-classification. But let me propose a rhetorical question: who among you has had to make decisions to classify or not to classify? If yes, did you ever classify anything Top Secret? Did you ever classify anything too low? Too high? In your discussions did you ever use the lowest classification to discuss a subject you knew probably should be at a higher level? If you had the CNWDI, SIOP or other compartmental clearances,[2] would you chance making a mistake, particularly with someone you didn't know very well? Do you know the potential consequences?

The bottom line here is that there is no substitute for experience and good judgment. The inexperienced need to be counseled to ask someone with solid experience for guidance. Unfortunately they usually aren't counseled, and they normally don't ask anyone for help. The key question that the classifier must ask, and answer as honestly as possible, is this: What would the operational and financial consequences be to the United States and its allies, if this information/product/technical know-how got into the hands of the enemy? With this as a reference point, it is easy to see how someone might be overly cautious. If you haven't had to make these type of decisions consider yourselves lucky. In any case, please be gentle in your judgment of the honest and conscientious folks who have.

There is a class of people who are an exception to the honest and conscientious individuals noted above. These are the individuals who classify (or make "Privileged" or "Business Confidential") anything that might bring them embarrassment or censure for poor or biased judgments, waste, fraud, or abuse of authority. There are many examples of such behavior. I have small sympathy for such people. Unfortunately, they abound not only in government, but in industry as well.

The government can do things that individual companies or institutions cannot. One of the most important is the ability to provide massive amounts of money over long periods of time. It can also provide a focus, unity, and national vision that transcend parochial interests. The government has the authority and ability to organize scientific and technological efforts on a scale no one can come close to matching. It has access to resources, intelligence, and facilities available nowhere else in the world. It has the option of bringing in similar resources and commitments from other nations, which, again, no single company or institution can do. These are pretty impressive capabilities in anyone's book, and numerous examples of them in action abound—e.g., the Manhattan Project, the goal of a man on the moon in our lifetime, the Space Telescope, and the Global Positioning Satellite System.

Do secrecy and security restrictions make scientific and technical progress less efficient? I think the answer is an emphatic and definite yes. Is progress slower than it might be if there were

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[2] CNWDI: Critical Nuclear Weapon Design Information. SIOP: Single Integrated Operational Plan (for use of nuclear weapons).

no secrecy restrictions? The answer here is less definitive, but is still yes. Should all science and technology developments occur without the protective veil of secrecy? In my opinion, no. The powerful capabilities some militarily critical technologies provide demand responsible care and use, and not all national leaders have another nation's best interests at heart. Neither are all individuals without malice towards others. Most of us would not like to see certain world leaders with the capabilities inherent in weapons of mass destruction. Hostile intentions do matter, but it is an enemy's capabilities that can kill you.

Some Current Issues of Concern

Beyond the control of military information, there are other areas in which secrecy raises policy issues for the government. The encryption of financial data, for example, is not just an issue for the United States. It is a valid global concern for everyone. The difficulty in exporting encryption technology that software companies and other businesses have is just the tip of the iceberg. The problem is more pervasive, and a global solution is desperately needed, and soon. National solutions are nice, but other nations may not wish to trust another nation not to eavesdrop on their communications or to tamper with their financial well being.

Could a multinational effort to develop a global encryption algorithm and technology be successful? Sometimes a prudent sharing of selected and crucial technical knowledge, even with a real or potential adversary, can lead to greater regional or global stability. Take the example of the concept "Fail-Safe," which was developed at the RAND Corporation and released to the public by the government on purpose so that the Soviet Union would learn how to use the "Fail-Safe" method for their own operational nuclear forces. While this knowledge gave the Soviets much greater operational capabilities, it also made for a nuclear environment with greater inherent stability, one that was less likely to lead to an "accidental" nuclear war. The U.S. gave away a technical advantage in order to achieve greater nuclear stability. Might this approach work for other issues such as encryption? What assumptions might we need to make? What should the trade-offs be for each side?

The related issue of computer piracy or of computer information system security is one of concern to all computer users. It is of even greater concern to business and government agencies. Financial losses, seldom reported publicly, are estimated to be enormous. Here is another case in which a national effort might be appropriate. The combined capabilities of academe, industry and government could probably solve this problem.

We generally speak of national security in strictly military terms, and that term is frequently invoked when justifying certain governmental actions. The reality is that economic security in today's global and interdependent economy is as important as military security and sometimes takes precedence for limited periods of time. It is time we re-think and redefine more broadly the term "national security" and how this broader conception applies not only to key federal statutes (such as the Export Administration Act and the Arms Export Control Act) but also to decisions and discussions of U.S. national defense plans and policies.

The recent loss of highly classified nuclear weapons design information from our National Laboratories to the People's Republic of China should have surprised no one. The relatively free access Chinese scientists had to their American counterparts, the apparent highly cooperative attitude of the lab's senior staff, and the conscious Chinese tactic of using "friendship" to acquire what they want, greatly facilitated the transfer of scientific know-how to the Chinese. It is naive at best, and criminally irresponsible at worst to assume that foreign nationals from a potential adversary country (which the PRC is), will not try to obtain (read steal) highly classified nuclear weapons design information if given the opportunity to do so. The intelligence gathering objectives of visiting PRC scientists has been well known to the U.S. intelligence community for years.

What happened at the National Labs is to be expected under the circumstances given. It is apparent that proper information security procedures were not followed, and just as important, close personal relationships were allowed to exist. It is a basic principle of technology transfer that the more intense the personal contact, the more quickly and completely the transfer of technical know-how will occur. Will the nuclear weapon design information lost have potentially serious consequences for the United States and the free world? The answer is undoubtedly yes. In my view, it will permit the PRC to develop better nuclear weapons more quickly and at a vastly cheaper cost. Strategically, it could permit them to field smaller, more accurate nuclear and thermonuclear weapons more quickly, and consequently provide them greater diplomatic leverage in world politics.

What are the relevant lessons here? There are several. Key among these is the reaffirmation of the need to aggressively protect that information and technology which is truly vital to our nation's security, and Critical Nuclear Weapon Design Information (CNWDI) is information of a vital nature. A second lesson is that the nuclear information acquired will in time give the PRC an increase in both real and perceived power in a military, political and diplomatic sense. If our monitoring of PRC nuclear tests subsequently verifies unexpected advances, the international perception will reflect itself in military, political and diplomatic terms. A third lesson is that this loss of CNWDI information will ultimately exact a price, not yet determined. A fourth lesson, perhaps obvious but worth reflection upon, is that once technology is transferred, it is gone. You can't get it back. Another, though not final, lesson is that everyone engaged in critical areas of military research should be extra sensitive to their own potential for compromise by people who have no real "need to know" the information they possess. They should be especially alert to foreign nationals from nations with competing international interests which are significantly different from our own. Should we continue to be friendly and cooperative with future scientific visitors from the PRC? Definitely yes, but we should be circumspect and realistic about what we share, and we should be security conscious at all times.

THROUGH A SHUTTER DARKLY: THE TANGLED RELATIONSHIPS BETWEEN CIVILIAN, MILITARY AND INTELLIGENCE REMOTE SENSING IN THE EARLY U.S. SPACE PROGRAM

John G. Cloud and Keith C. Clarke

Introduction

On September 12, 1962, on the eve of the Cuban Missile Crisis and 15 years into the Cold War, President Kennedy gave a celebrated speech accelerating the U.S. space program. One paragraph, in particular, resonates with a workshop on "Secrecy and Knowledge Production":

> We choose to go to the Moon. We choose to go to the Moon in this decade and do *the other things*, not because they are easy but because they are hard, because that goal will serve to organize and measure the best of our energies and skills, because that challenge is one that we are willing to accept, one we are unwilling to postpone, and one that we intend to win, and *the others too* (Kennedy 1962, emphasis added).

The *other things* to which the President repeatedly referred may now be considered a tacit acknowledgment that, less than five years after the launch of Sputnik I, the United States had created an extraordinary series of reconnaissance satellites, and that the program, called CORONA, had already moved from experimental to operational status. Operational space-borne reconnaissance completely transformed the context and progress of the Cold War—but it was conducted at the highest and most compartmentalized levels of secrecy in the history of the nation.

The very possibility of reaching the Moon publicly was inevitably linked to the technological innovations that allowed secret observation of the Soviet Union and the rest of the world, but the nature of that linkage remained hidden for the next third of a century. In late 1995 the CORONA program was declassified. Public release of previously deeply classified data now makes it clear that the coupling of open and secret, as in the Apollo program and CORONA, was not unusual, and was in fact the general case. Such a coupling—now referred to as the "Dual Use" policy—extends through U.S. space history. Since 1968, for example, the Civilian Applications Committee (CAC), a federal interagency committee, has provided federal civil agencies access to classified reconnaissance information. The roots of such contemporary programs as Medea, which provides top U.S. scientists access to classified space-borne intelligence data for tackling global environmental problems (Richelson 1998), may be found in the secret relationships between ARPA, NASA, and the Intelligence Community forged in the very earliest days of the U.S. space program.

The goal of this paper is to describe those early secret relationships and to present a model, the "Shuttered Box," which organizes a great deal of the history of the U.S. space program and the U.S. engagement in the Cold War. The Shuttered Box model may prove useful in the design of future dual-use systems in the still dangerous post-Cold War world.

The Convoluted Path to "Open Skies"

The Cold War lasted so long, and was so pervasive, that most of us retain a common belief that it was inevitable. However, as Pamela Laird notes, the singular power of committed historians of science, technology, and power comes from their ability to make real the experiential contingencies of the past that actually gave rise to the structures that only now appear inevitable (Laird 1998).

Contrast the layers of secrecy that already cloaked "the other things" to which Kennedy obliquely referred in 1962 to the overture for public multilateral aerial and space-borne reconnaissance made only 7 years earlier by Col. Richard Leghorn, the architect of President Eisenhower's aborted "Open Skies" policy. In 1955, he published an article in *U.S. News & World Report* advocating a "peace offensive" explicitly linking cooperative high resolution reconnaissance to major steps towards effective nuclear disarmament.

> We could simultaneously press harder for our aerial-inspection proposal, perhaps by advocating "free international air" above a three-mile or even a 12-mile limit, as now practiced at sea.
>
> And we might announce a start on construction of a reconnaissance earth satellite, the transmitted results from which we would be willing to turn over to a U.N. inspection agency (Leghorn 1955, p. 70).

Soon after this paper was written, President Eisenhower disclosed the U.S. plan to build a small satellite, a first step towards a reconnaissance version. There are some indications that Eisenhower's "Open Skies" policy was both a serious proposal and a clever negotiating ploy to counter the Soviet Bloc by proposing a policy it would never accept anyway (Hall 1998). Leghorn's plan could be considered a deeply prescient model for eventual bilateral accords between the U.S. and the now-former Soviet Union for mutually acceptable nuclear weapons treaty verification. The two powers finally agreed to cooperative observation by the other, but only in 1998, 43 years after Leghorn proposed it.

Leghorn's proposal for reconnaissance data to be turned over to the U.N., which essentially would establish high-resolution space-borne remote sensing as a global public utility, has yet to be realized. Perhaps the closest equivalent to his proposal is the new MEDEA program, in which selected U.S. scientists are cleared into access to high-level intelligence data, the redacted findings from which can be revealed to the public and the larger scientific community. The first published paper based on such data, a study of tree abundance over time in the African Sahel, reveals a problematic relationship to non-classified scholarship. The paper was initially rejected by *Science* because normal peer-review was completely precluded by the nature of the data used. It was eventually published in *Global Change Biology*, which noted that:

> Many of the data for this paper are in classified intelligence archives. As a consequence, the options for evaluating the paper and for ensuring that other scientists can reproduce the analysis is constrained . . . [and] Limitations on access to the data make it impossible for the journal's usual review process to assess all aspects of data quality, selection, or interpretation (Schlesinger and Gramenopoulos 1996).

Despite restrictions in access and movement between civilian and classified realms, scientific and technological discoveries made on the dark side have been for decades transmitted to the other side, allowing NASA to send astronauts to the Moon and to explore the solar system. Materials developed for programs that were once among the most secret assets of the United States—such as mylar and videotape—now suffuse popular culture around the world. How did such complex interchange develop, and what mechanisms were devised that could provide the requisite separation between the civilian and classified worlds, yet could provide and even encourage cooperative uses between those worlds?

Our attempt to define and describe these mechanisms is rooted in ongoing research on the history of space-borne reconnaissance and observation in the U.S. space program. It may be considered a small and modest part of the recently revitalized history of the Cold War, triggered by the declassification of the CORONA program, the hidden pioneer of the U.S. occupation of space.

CORONA Fundamentals

CORONA was the very first U.S. satellite program to be successfully deployed, but it and its fruits remained highly classified until years after the breakup of the Soviet Union and the nominal end to the Cold War. The full history of CORONA is coterminous with the entire U.S. space program, and it links all elements of the civilian, military, and intelligence community involved in space. With the recent declassification of CORONA, a profusion of histories have appeared, rich in detail on the entire Cold War (Ruffner 1995; McDonald 1997; Peebles 1997; Day, Logsdon, and Latell 1998). Interestingly, the recent histories based on access to the now-declassified data provide important validation to earlier speculative histories written while the real story was still deeply black (see especially Burrows 1986, and McDougall 1985).

In previous work (Cloud 1997a) we have suggested that the period of postwar collaboration between the CIA and the military on the one hand and the civilian mapping agencies on the other went through five phases. In the first phase, all overhead reconnaissance was entirely conducted by military and intelligence agencies. In the second phase, the differentiation between a non-military (e.g., NASA) and a military/intelligence space component began, one that remains in place today. The third phase, of covert cooperation between these realms, began immediately with the second phase. In the fourth phase, the covert collaboration reached a maximum of virtually complete integration between intelligence, military and civilian operations. This integration continues to date, but only in recent years (phase five) has it been openly acknowledged (Cloud 1997b).

Conventional historical explanations for the organization of the U.S. enterprise in space have often emphasized developments as responses to crises, particularly those histories written by participants in the crises at the time. In these versions, the "early" development of thermonuclear weapons by the Soviet Union, the sudden appearance of Sputnik, the discovery of missiles in Soviet Cuba, and other dramatic events provoked over-arching responses that both substantially ordered and significantly changed programs and priorities. There is, however, mounting evidence of a deeper symmetry to developments, and a more coherent ordering and continuity of effort in the enterprise of space.

The subject of space-borne reconnaissance may be ordered by reference to constituencies that developed at the very beginning of the Cold War. The V-2 photography trials performed by Clyde Holliday and other staff members of the Applied Physics Laboratory of Johns Hopkins University mark the primordial beginnings of space-borne observation at White Sands Proving Grounds, New Mexico, starting in 1946. All the V-2 science experiments, while nominally civilian, were actually created as extensions of the interests of the U.S. armed forces scrambling to position themselves in the new nuclear world (Devorkin 1996). The RAND Corporation, the prototypical “think-tank,” was established by contract to the brand-new U.S. Air Force, which was explicitly differentiating itself from its former parent, the U.S. Army. RAND pioneered spaceship design—and space-borne reconnaissance methods. A preliminary step to space was high-altitude balloon reconnaissance, which RAND began in 1947 with experimental and highly classified balloon trials staged at Holloway Air Force base, adjacent to Roswell, New Mexico—coincidentally enough, precisely at the same time that the first mysterious alien sightings appeared near Roswell. Balloon reconnaissance trials in the U.S. soon led to top-secret deployment of balloons over the Soviet Union. In Project GENETRIX about 560 balloons were launched upwind from the Soviet Union in 1956, although most were shot down or lost and only 44 camera payloads were recovered (Hall 1998). Promising photography was recovered from those payloads, creating and reinforcing an emerging constituency devoted to photographic intelligence, as opposed to the more traditional constituencies organized around spies, for example. Problems with the balloons impelled plans for high-altitude aircraft to substitute for them. In a sense, balloons begat the U-2, which was seen as a stop-gap technology from the beginning, ultimately vulnerable to Soviet aircraft or missiles. It was hoped the U-2 would buy time to perfect a reconnaissance satellite. Thus the U-2 begat CORONA (Harris and Davies 1988).

An organizational and financial model developed for reconnaissance balloons, the U-2, and CORONA, with profound implications for the ordering of U.S. society during the Cold War. In all three cases the technology was designed, constructed, and maintained by sole-source contracts with carefully selected U.S. corporations, administered from the highest levels of the Directorate of Central Intelligence (DCI). The programs had untraceable and unreported budgets, and cover programs to divert attention or serve as plausible explanations for any inadvertent attention received by the secret efforts. In 1958 the DCI, in collaboration with the U.S. Air Force and the newly-founded DOD Advanced Research Projects Agency (ARPA), organized a new security system far more secret than anything ever attempted in the history of the United States. Overhead reconnaissance from aircraft (TALENT) and from spacecraft (KEYHOLE) were combined in a new security class, TALENT-KEYHOLE, with resultant implications that have suffused U.S. society ever since, although rarely recognized (Burrows 1986).

The cover story program for CORONA was the U.S.A.F. “Discoverer” satellite program, which began launching rockets in 1958. CORONA/Discoverer, like the rest of the U.S. space effort, was extremely problematical at first, with a long series of launch failures and technical difficulties. The first successful CORONA mission, which returned exposed film to earth by parachute snagged by aircraft in the central Pacific near Hawaii, did not occur until August, 1960. The very first film roll had captured more imagery of the Soviet Union than all the previous balloon and U-2 flights combined (Wheelon 1995).

CORONA returned film successfully from August 1960 to May 1972. The program featured three different series of cameras: the KEYHOLE reconnaissance camera series; the ARGON geodetic and mapping camera, which flew between 1962 and 1964; and the experimental high-resolution LANYARD camera. Instrument resolution was better than 2.8 m at all times after 1963, and achieved 0.6 m in the single KH–6 LANYARD mission, a next-generation prototype flown in 1963. LANYARD was an attempt to gain higher spatial and spectral resolution imagery of particular use for technical reconnaissance. Post–1973 higher-resolution reconnaissance sensors, such as the Air Force's GAMBIT containing the KH–7 camera, remain classified. ARGON was a panoramic geodetic camera system, supported by the U.S. Army, that was used within the CORONA program for mapping purposes; seven of the twelve missions between May 1962 to August 1964 were successful. These missions were almost entirely for cartography and geodesy. The KEYHOLE reconnaissance camera series evolved continually during the life of the project: with non-stereo panchromatic photography from cameras KH–1, KH–2, and KH–3, ground resolutions improved from around 12 meters to 4 meters, and with multi-spectral stereo photography from the KH–4 cameras (KH–4, KH–4A, and KH–4B) ground resolutions decreased from 3 meters to 2 meters.

In addition to the down-looking reconnaissance cameras, CORONA missions included stellar cameras for positioning and navigation; lower resolution, broader field-of-view index cameras for positioning and rectification; and horizon cameras for determining spacecraft attitude. In 12 years, CORONA acquired 800,000 images taken from space, covering 750 million square nautical miles and filling 39,000 film cans containing 2.1 million feet of film. In late 1960, the National Reconnaissance Office (NRO) was organized by the DCI and DOD to administer the program, launch the rockets, archive the film, and direct its many intelligence and other applications. So successful was the endeavor that by 1962, when President Kennedy made his offhand reference to "the other things," a program considered experimental and unsuccessful only two years earlier had become almost routine, with a rocket launched successfully from Vandenberg Air Force Base about once a month until the end of the program in 1972. It was 31 years before the U.S. government officially acknowledged that the NRO existed.

CORONA in its Sociotechnical Ensemble

Both popular and scholarly examination of CORONA have generally focused on its application to Cold War strategic reconnaissance. In a celebrated and often-quoted informal aside made by President Johnson on March 15, 1967, he noted that:

> I wouldn't want to be quoted on this, but we've spent $35-40 billion on the space program. And if nothing else had come out of it except the knowledge we've gained from space photography, it would be worth ten times what the whole program has cost. Because, tonight, we know how many missiles the enemy has. And, it turns out, our [previous] guesses were way off. We were doing things we didn't need to. We were building things we didn't need to build. We were harboring fears we didn't need to harbor (Klass 1971, pp. xv-xvi).

Pursuing knowledge production along these lines is quite problematic, for reasons that extend in at least two very different directions. First, the earliest CORONA-led recognition that the Soviet Union's ICBM missile arsenal was significantly smaller than had been previously assumed occurred before, and not after, the mid-1960s significant expansion in the U.S. ICBM fleet (MacKenzie 1990). Cold War realities were driven by much more complex calculations, and profit margins, than first appear. Second, with the passage of time and the complete erosion of the strategic significance of the CORONA photography, it can now be appreciated that the intellectual exercise of identifying a Soviet missile site pales in comparison to the exercise of determining the missile site's position in the vast Eurasian landmass, across the Pacific from North America. Ultimately, the U.S. geo-referencing system, the World Geodetic System, which was devised precisely to manage that feat of positioning, will be recognized as perhaps the most significant and lasting intellectual achievement of the Cold War. CORONA's applications to global mapping will be recognized as the trigger mechanism for many innovations in cartography and geographic information science. The CORONA archives will come to be valued principally as the world's first global remote sensing data set from space.

A substantial literature places science, including its actors and theories, within a context of the three elements that underlie research in science and technology studies. These elements are the continuum between science and technology, the relationship between the macro and microlevel of activity within science, and the system and environment in which the science takes place. In Forman's model, developed in the context of radar technology during World War II and its following period, technological breakthrough came first, and the science followed only when the actors (in this case the scientists) took their new understanding back to purely scientific problems (Forman 1987). Forman has called this the "overwhelming of science by its own techniques." Key elements are the personal or single laboratory basis of the technology (microlevel), the social promotion of programs of knowledge production (especially by the military), and the compartmentalization of the ideology, creating a "friendly-hostile" cooperation between scientists. With CORONA, technology clearly preceded science. The environment, however, was extremely complex.

In addition, research has concentrated on characterizing the nature of technology as it relates to science. Approaches have included examining links (personal, intellectual, formal) between the key players or "actors" in the form of a network that explains connectivity between events and accomplishments. The list of key contributors to CORONA on the NRO web site at http://www.nro.odci.gov/corona.html is an example. From another perspective, scientific progress comes not from small sequential improvements of technology but from focusing on "reverse salients" that generate critical problems for a technology. Examples abound in CORONA, from the 13 launch failures, to lost film capsules, to the problems of exposed and snapped film (McDonald 1995).

The interaction between secret and open science has permeated studies of Cold War science and technologies, and has been analyzed in depth by MacKenzie (1990) in the case of missile guidance and in Forman's work on the Maser (1995). This technological systems approach was pioneered by Hughes (1983), and involves consideration of the technological, economic, and political context of scientific and technological change (Bijker 1995). The participants in the process often played the role of creating and maintaining compartmentalized organizational units within the technology, such as individual laboratories. Forman (1995) has called compartmentalization the "committed refusal to become consciously aware of this far-reaching social integration and to face the daunting problem of reconciling the conflicting values underlying a scientific enterprise so integrated," unlike those cases when scientists were participants in the societal debates their work created. CORONA is an excellent case study of bipolar compartmentalization. In spite of an extreme effort to compartmentalize the science and technology that introduced a level of secrecy higher than "Top Secret," the power of the CIA/NRO to keep CORONA hidden was both intentionally and unintentionally diminished as the Cold War mission gave way to a realization of the powerful dual-use nature of the science, the data, and the technologies themselves.

Complex relationships and transactions between the non-classified and classified realms evolved in major stages, as we have noted. We will now consider the engine or mechanism by which these changes were made. Our concepts and terminology expand upon the “black box,” as used by Bruno Latour (1979, 1987) and Donald MacKenzie (1990), the latter in reference to inertial guidance systems for nuclear missiles. According to MacKenzie,

> I use it in two closely related ways. The first is to refer to a guidance or navigation system that does not require input from the outside world to operate. This, for example, is the sense in which the term was used in the first extant paper on the topic by inertial guidance pioneer Charles Stark Draper . . . in the other meaning, a black box is opaque in a slightly different sense. It is a technical artifact—or more loosely, any process or program—that is regarded as just performing its function, without any need for, or perhaps any possibility of, awareness of its internal workings on the part of users (p. 26).

MacKenzie’s twinned uses of the black box metaphor ordered his insightful history of the evolution of inertial and non-inertial missile guidance. The obvious parallels between an enclosed missile guidance system and an extraordinarily secret reconnaissance camera system induced me to apply his metaphor to CORONA and its applications. The black box, however, immediately proved too limiting: it cleaves the world in two, inside and out, which parallels the division between the non-classified and classified realms—but the black box allows only two states of relationship between the realms—open or shut, connected or divided. The reality of CORONA applications has proved to be much more complicated, as the cases of its role in the civilian re-mapping of the United States and creation of the World Geodetic System demonstrate.

The Shuttered Box

Some months prior to the announcement of the declassification of CORONA in November 1995, a person with long experience within the Intelligence Community drew our attention to the unclassified version of the Nixon Administration’s Office of Management and Budget Federal Mapping Task Force on Mapping, Charting, Geodesy and Surveying (OMB 1973). By reading judiciously between the lines, one could glean the general rationale by which CORONA photography was applied to remapping the entire United States from the mid-1960s on, and why.

The OMB report identified two major impediments to the effective integration of federal geographic efforts. The first was "the disarray of military [mapping, charting and geodesy] with two, and then three, voices speaking simultaneously to civilian agencies, often working at cross purposes with them and with each other" (OMB 1973, p. 7). The report noted that the consolidation of most Defense Department and allied intelligence efforts into the Defense Mapping Agency "corrected the problem of civilian-military coordination which perplexed the earlier study groups." The other major problem the study identified was "an inability to identify and implement surefire innovative improvements that would bring about stepped-up delivery of surveys and maps to using agencies when and where needed." The study had answers for this problem too—but they revolved around civilian applications for imagery and data derived from the most highly classified satellite systems in the U.S. space program. Necessarily, the unclassified recommendations were and remain discreet, but the implications are clear:

> The second impediment can now be resolved by applying DOD advanced technology against civilian requirements...The lack of civilian [mapping, charting, and geodesy] involvement has been accompanied by the development of *expensive systems for civilian use that cannot compete in any meaningful way with DOD-developed techniques*. Failing to adapt to new technology will mean continued pressure for redundant and less efficient systems . . . We believe that federal civilian MC & G resources can be made more productive by a community reorganization based on establishing a comprehensive and integrated program to provide multipurpose products (OMB 1973, p.10) (emphasis added).

The report contains a number of very interesting figures, notably a diagram correlating sensor system ground resolution to the percentage of mapping and geographic applications that can be satisfied by imagery captured at that spatial resolution (see Figure 1). With the sensor information available following the declassification of CORONA, it is possible to situate the spatial resolutions of then-extant and future civilian sensor systems, such as LANDSAT, along with counterpart resolutions for the various CORONA camera series systems KH–1 through KH–6 (see Figure 2).

Two conclusions can be made. First, as the OMB report states, the classified systems, from very early on, were capable of much more productive geographic applications than the civilian sensor systems, then and now (and in the future). Second, the two suites of sensor resolutions are separated by a "resolution gap"–there is no significant overlap between U.S. civilian and classified sensor spatial resolutions. Although it is unmarked, one can identify a wall or barrier between them. Indeed, Mack's landmark history of the LANDSAT system makes clear that the Intelligence

Community specifically vetoed a higher resolution film-return system proposed for the early LANDSAT program, precisely because it feared any unclassified sensor that had capabilities approaching those of classified sensors would inevitably undermine national security (Mack 1990).

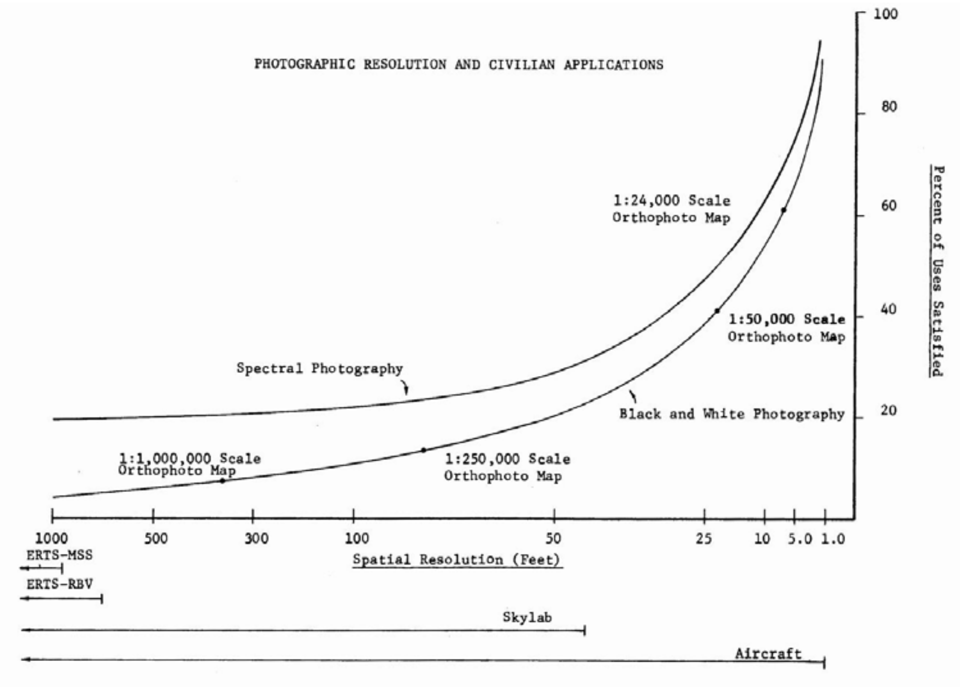
The OMB report limits its delineation of the differences between civilian and classified realms to spatial resolution. We might develop other critical axes, such as frequency of imaging repeat coverage, and the relative ability to cover areas on demand, or any other highly significant criteria of system performance. In effect, we can graph sensor system capabilities in multi-dimensional space, although, for clarity, we restrict the illustration to three dimensions (see Figure 3).

The implicit recognition of the multiple walls between realms creates—a box. Not a solid black box, though—all walls of the box communicate between outside and in by "shutters," which one might envision as camera diaphragms, or as the venetian blinds of a hard-boiled *film noire* detective (see Figure 4). On one side is the classified world populated by those with clearance. On the other is the open world of civilian science. The Shuttered Box works in this manner: by coordinating the opening and closing of shutters on all sides, the view through the box is precluded at all times—there is an absolute separation between entities on either side of the box—but by opening and closing shutters in tandem, materials and people can pass securely *in either direction* back and forth through the box. That which can and has passed through the box includes: (1) funding; (2) people and their experience; (3) tools and techniques; (4) findings and redacted data; and (5) knowledge and science.

The Shuttered Box in Action

Cloud's dissertation research is devoted to recovering the previously disguised and secret advances in Cold War geography which have reordered and transformed our world. Perhaps the two most dramatic advances with reference to contemporary U.S. society are the creation of the World Geodetic System (WGS) and the secret remapping of the United States, which was done based on intermediate imagery derived from CORONA photography. Both episodes are exemplary illustrations of the Shuttered Box in action.

Figure 4.1



Source: OMB *Report of the Federal Mapping Taskforce on Mapping, Charting, Geodesy and Surveying* (1973), p. 140.

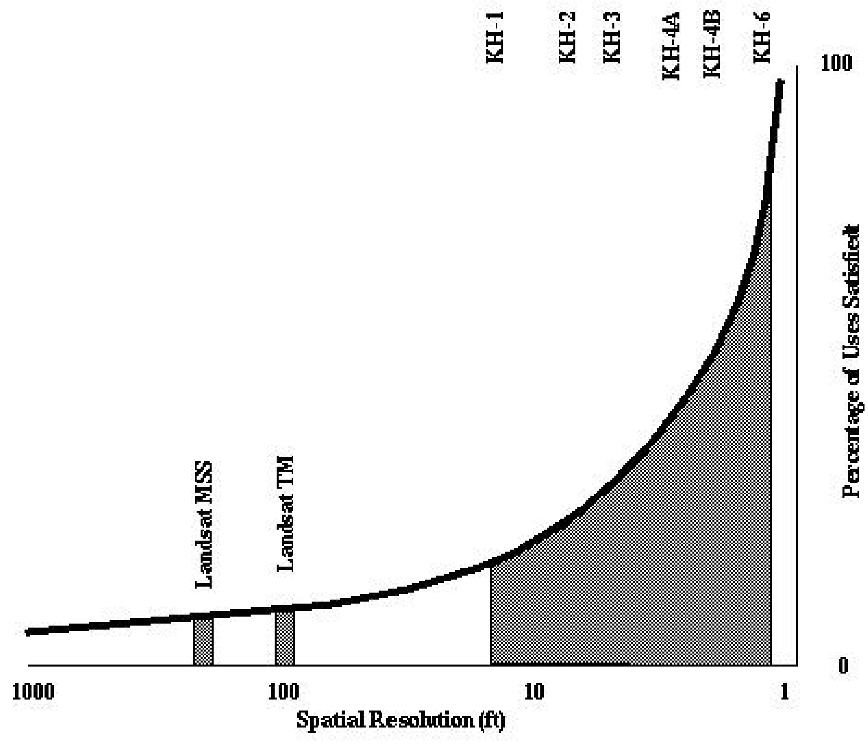


Figure 2: Photographic Resolution and Civilian applications during the Corona era.

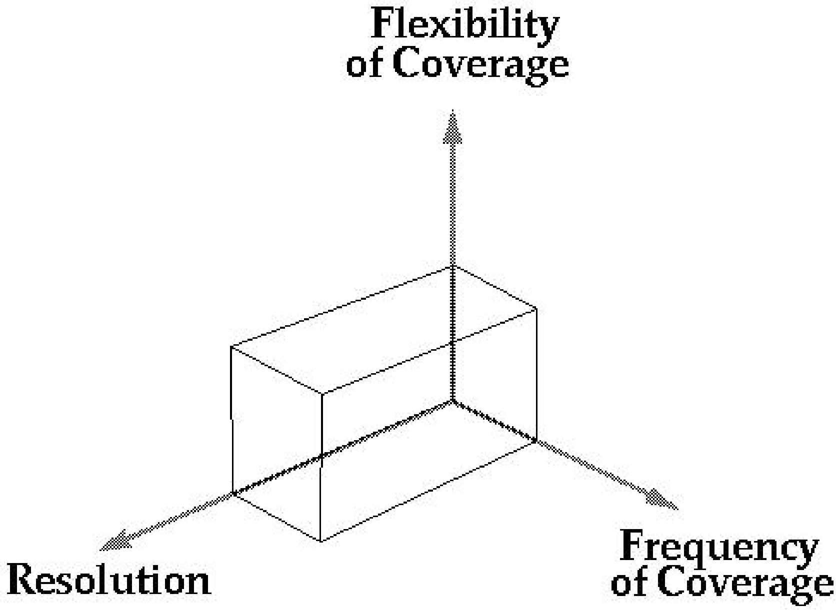


Figure 3: Sensor system capabilities

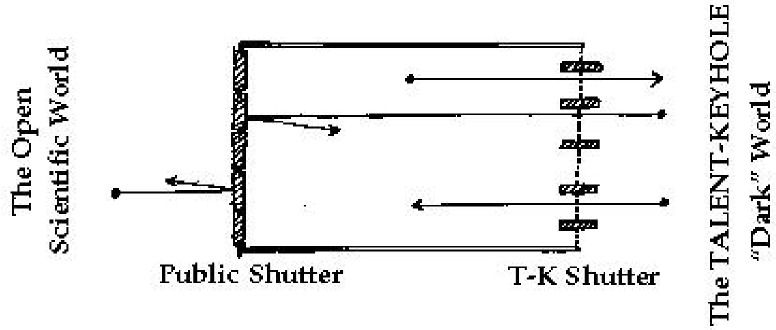


Figure 4: The Shuttered Box in Action

The World Geodetic System

Knowledge of position has always carried strategic value, but during the early Cold War knowledge of bomber and missile launch site positions and target site locations assumed a paramount significance. The U.S. enterprise to position the planet is incorporated in the World Geodetic System (WGS), which was and is absolutely critical to aiming ICBMs—and is also one of the most beneficial and permanent intellectual achievements of the entire Cold War.

The WGS has two sets of components, related but quite distinct. Horizontal position on the planet—such as latitude and longitude—is defined in relation to a reference ellipsoid, a geometrical figure that approximates the true shape of the planet. Vertical position—height above or below sea level—is defined by height relative to an equipotential surface of gravitational attraction, which is defined as the geoid. Accurate vertical positioning, therefore, is precisely equivalent to knowledge of the planet's gravitational field. Characterizations of position are essentially complex mathematical models fitting shapes to approximate the true contours and gravity fields of the earth. These are called datums, the singular of data, because they are generally best characterized by their unique initial points, the places where the mathematical models are affixed to the ground. For most of the 20th century, the U.S. planet has begun at Meades Ranch, Kansas, the initial point of the North American Datum of 1923. In the pre-Cold War era, all datums were continentally based and nationally centered. The critical gaps between national-level datums was recognized as early as World War II, triggered by the enormous expansion in weapons ranging represented by the V-2 rocket (von Braun 1951).

At the beginning of the satellite era, geodetic work performed by the Army Map Service and the Air Force Aeronautical Charting and Information Center confirmed both the mismatch between continentally-based datums and the inadequacies of global gravity models (DMA 1983). Geodetic progress developed through carefully monitored contacts between classified and unclassified geodetic players, involving the establishment of overt and covert geodesy education programs, initially at Ohio State University. The Defense Intelligence Agency (DIA) Mapping and Charting Directorate became the controlling authority on geodetic progress, using budget-wielding power to induce increased cooperation between Army and Air Force efforts and integrate gravity field data produced through the Navy's Transit navigational satellite system (Daugherty interview, 21 January 1998). These efforts were necessarily concentrated on areas of the planet outside the Soviet bloc. Captured German geodetic materials seized by Allied intelligence at the close of

World War II included surveys through interior Eurasia, produced originally to map the route of the Trans-Siberian Railroad. CORONA photography was used to relocate the remains of survey towers from the original surveys, allowing geodetic corrections of immense strategic value for "locking in" the positions of Soviet facilities in interior Eurasia. All these efforts culminated in the consolidation of many classified geodetic and mapping enterprises in the Defense Mapping Agency (DMA) and the still-classified World Geodetic System of 1972—the world's first truly global datum. Degraded versions of the data set were pushed through the Shuttered Box to create the civilian USGS WGS of 1972. Refined gravity field data allowed the completion of a corrected classified datum, WGS 1984, which, in degraded form, is the basis for the North American Datum of 1983 (NAD 1983), used for all mapping of the United States and also used as the geodetic foundation for the Global Positioning System (GPS), the Universal Transverse Mercator (UTM) map coordinate system (Synder 1987).

The relationship between classified and unclassified programs was complex: the unclassified players were not just receivers in the relationship. For example, a civilian gravity researcher might collect gravity measurements, which he or she copied and pooled and gave to DOD. The data were absorbed into major computations on the dark side, adding to what became the WGS (classified). Civilian researchers got unrestricted access only to the degraded version of WGS released publicly by U.S. Geological Survey (USGS). In cartographic applications, however, civilian users were often given access to versions of CORONA photography, up to and including full-blown undegraded CORONA, but the origins of the imagery were completely disguised, and/or the fact that CORONA was a data source for the resultant civilian map or whatever was also completely disguised.

In general, the whole point of the Shuttered Box was to facilitate selected exchanges between classified and unclassified constituencies, but to do so in such a way that certain vital parameters of "security," as self-defined by the classified community, weren't compromised or threatened, again in their terms.

The Secret Remapping of the United States

As discussed earlier, after CORONA moved from experimental to operational mode, the superiority of CORONA imagery for many traditional mapping and geodetic efforts was quickly recognized. Extraordinary mechanisms were invented to take advantage of CORONA—but not to compromise its security, nor reveal its use for non-classified applications.

While most CORONA imagery covered Asia and Russia, about six percent of the imagery covered the United States in a systematic way to assist in civilian mapping, disaster planning and relief, pollution monitoring, and planning. Mapping with CORONA imagery was advocated by Presidential Science Advisor Eugene Fubini under the Johnson administration (Day et al. 1998). The U.S. Geological Survey, the Environmental Protection Agency, the National Oceanographic and Atmospheric Administration, and the U.S. Forest Service participated in the applications. The structures created by the USGS for CORONA applications are a paradigmatic example of the Shuttered Box in action. After testing for feasibility, a location was selected in Reston, VA for construction of a map production facility that could be entirely secured at TALENT-KEYHOLE standards. Funding for the unit was placed into the USGS budget, and the "Special Mapping Center" was opened in late 1968 (Baclawski 1997). In Baclawski's words "the Geological Survey became the largest civil agency user of the CORONA imagery." While mapping the United States differed from mapping the USSR and China in that an existing datum and base set of maps were available, in fact in 1973 there was not even a complete 1:250,000 map series. Thousands of maps needed revision and updating, and coverage gaps at larger scales needed to be filled. In addition, with now superior geodetic control available, the control framework "needed to be refined, up-dated and better integrated."

The first use of CORONA imagery was as a supplementary source for updating the 1:250,000 national coverage. Next, attention was turned to updating the 1:24,000 series maps, using a purple overprint of revisions. No imagery needed to come out of the Shuttered Box, only the derived map products. Nevertheless, the compilation images became part of the CORONA imagery archive. While this effort was under way, a new national land use and land cover series was completed. Based on the 1:250,000 series, but clearly having a more detailed map base, the polygonal outlines of land use and land cover as classified in the Anderson set of categories was traced by hand. The Anderson classification system (Anderson et al. 1976) shows a remarkable degree of similarity to feature identification and image interpretation guides in use by the DMA, now finding their own way into the unclassified realm, such as the Civil National Imagery Interpretability Rating Scale (http://www.fas.org/irp/imint/niirs_c/guide.htm). The national land use and land cover maps were among the first digital mapping/GIS ventures undertaken by the USGS

(Mitchell et al. 1977), another example of technology and perhaps expertise finding its way through the shutters.

Conclusion

CORONA and its constituencies were just one of a number of remarkable and highly secret major projects that were initiated during the administration of President Eisenhower. These projects share two major identifying characteristics, one of which seems particularly attributable to Eisenhower and his administration, the other more general. Eisenhower had been both a warrior and a college president, and he brought both realms to bear on the science and technology of national security. He made unparalleled use of scientific advisors at the highest levels of policy, and he insisted on broad implementation of compartmentalized security measures consistent with those implemented for Operation Overlord in World War II (R.C. Hall 1995).

A comparison between the principal super-systems initiated during Eisenhower's administration is instructive, as is a comparison between the approaches and trajectories of the major scholarly analyses of these systems, and their place in advancing theory and practice in science studies. The parallel super-systems commensurate with CORONA include SAGE and other systems of enormous inter-networked computers for guidance and control, as analyzed by Paul Edwards (1996). The evolution of ICBM inertial guidance systems, the proverbial "black box of black boxes," has been addressed by Donald MacKenzie (1990). Nuclear-powered submarines, and allied technologies of the deep seas, were recently described by Sontag and Drew (1998). CORONA as a system of overhead reconnaissance is situated within a larger set of such systems, including such vehicles as the U–2, the A–12 or SR–71, and other aircraft and spacecraft. There have been many analyses of aspects of this history, particularly the landmark history by William Burrows (1986).

These systems and the institutions that built them can be analyzed in many ways. Traditional approaches analyze policy issues at the highest levels of the organizations in question. These approaches are particularly useful for analysis of the bridging institutions that were created in response to limitations and failures in existing organizational structures; many of these problems were only identified in the process of attempting the super-systems. The rather notorious inter-service rivalries within the Department of Defense, for example, were addressed by consolidations of enterprise such as the creation of the Advanced Research Projects Agency (ARPA), and the consolidation of all military and intelligence photographic analysis facilities under the National Photographic Interpretation Center (NPIC). To meld military and intelligence access to CORONA, specifically, the bridging institution of the National Reconnaissance Office (NRO) was devised. It has recently been revealed that there is an aquatic counterpart, the National Underwater Reconnaissance Office (NURO) (Sontag and Drew 1998).

Newer approaches to the subject in the field of science studies concentrate on the socio-technical ensemble of the project or on its social organization. These studies pay particular attention to the structures and mechanisms developed in the process of realizing the objectives, and the methods by which these structures are codified into regulations and procedures that become long-term or permanent constituents of what may be considered the culture of the project's organizations (Johnson, 1998). The analyses generally concentrate on a single specific product or program, with less attention to larger-scale systems integration. Common to both major strands of analysis is a focus on issues and actors within the projects and systems, with much less attention to relationships and impacts extending outside and beyond.[1]

The concept of the Shuttered Box, however, necessarily embraces the structures of exchange between the unclassified and classified world to a larger degree than the analyses of its counterpart super-systems, in large part because of the unanticipated, multiplicative consequences of overhead reconnaissance. At the time of the super-systems' creation the civilian world had little need for massive computer control systems, or superbly accurate inertial guidance systems—although it would soon be transformed by the smaller computer systems that SAGE induced, built with the computer chips that had been designed for ICBM guidance. Overhead reconnaissance as a source for geo-referenced information, however, was and is remarkably different. The civilian world already possessed resources critical to CORONA's successful applications, particularly the legacies of cartographic institutions and practices, and academic resources in geographic and geodetic theory such as the geodetic sciences department at Ohio State University. More important, CORONA photography from the outset was recognized as having dual uses, with civilian and classified applications alike, a development that had little parallel among the other super-systems.

The twinned utility of CORONA triggered the evolution of the Shuttered Box. The disparities between the civilian and classified realms were not bridged, but accommodated by the mech-

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[1] The exception is MacKenzie (1990).

anisms of the Shuttered Box. The solutions devised were suboptimal, and remain so. But all parties were and are served successfully enough. And all parties continue to require even more georeferenced information. Thus, the dual use nature of overhead reconnaissance is the reason that CORONA was much more carefully concealed than the other super-systems, and remained little known for over three decades. As a result, the contemporary maps that hang on our walls today are, like Edgar Allan Poe's "Purloined Letter," our deepest secrets, hidden in plain view.

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SECRECY, AUTHORSHIP AND NUCLEAR WEAPONS SCIENTISTS

Hugh Gusterson

Introduction

In a recent talk at MIT the scientist and "science-in-fiction" novelist Carl Djerassi[1] pointed out that, whereas novelists often eschew personal fame by writing under pseudonyms, it is usually vitally important to scientists to win recognition for their work under their own names. In the words of the narrator of his novel *The Bourbaki Gambit*:

> There is one character trait . . . which is an intrinsic part of a scientist's culture, and which the public image doesn't often include: his extreme egocentricity, expressed chiefly in his overmastering desire for recognition by his peers. No other recognition matters. And that recognition comes in only one way. It doesn't really matter who you are or whom you know. You may not even know those other scientists personally, but *they* know *you*—through your publications. (Djerassi 1994, 18-19)

Djerassi was intrigued by a group of distinguished French mathematicians who, playing the exception to the rule, refused science's cult of individual fame by publishing, starting in 1934, under the collective nom de plume Nicolas Bourbaki. (Their aim was, in part, to demonstrate that the truth status of knowledge was independent of the authority of its authors.) The identities of the mathematicians who made up Bourbaki were kept secret and, in Djerassi's narrator's words, "now people refer to *him*, not *them*" (Djerassi 1994, 18). In Djerassi's novel the "Bourbaki gambit" of anonymous collectivization is repeated by a group of contemporary elderly scientists who together invent PCR.

I want to suggest here that the conditions of bureaucratic secrecy under which American nuclear weapons research has been conducted have created a phenomenon we might refer to as the "Bourbakification" of science. This phenomenon is by no means unique to the world of nuclear weapons science, but we can discern there the starkest shape of a mode of scientific production that is, in weaker forms, more widely dispersed. In the process of Bourbakification the distinctive

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[1] Djerassi, the inventor of the birth-control pill, has now completed a trilogy of what he calls "science-in-fiction" novels: novels that take scientists as their principal characters and explain the workings of science to the reader.

contributions of individual scientists have been repressed or gathered together under the sign of sacralized individuals standing for groups. Unlike the original Bourbaki experiment, this has not been a ruse entered into voluntarily. It has been enforced by the conjoint workings of military secrecy and "big science," both working together to produce the phenomenological death of the scientific author.

The Death of the Authors of Death

The Livermore Laboratory, America's second weapons laboratory, was founded in 1952 in order to intensify work on the atomic and hydrogen bombs as the cold war escalated. Most parts of the laboratory are off-limits to the public, and access to spaces and to information for its 8,000 employees (almost 3,000 of them scientists and engineers with Ph.D.s) is regulated by an elaborate system of rules and taboos. The laboratory is divided into zones of greater or lesser exclusion related to the system for classifying information and people. A few areas on the perimeter of the laboratory are "white areas" accessible to the public. (These areas include two cafeterias, the Public Affairs office, the Visitors' Center, etc.) Large parts of the laboratory are "red areas" that are off-limits to the public, although only open research is done there. (Since I did fieldwork in the late 1980s and early 1990s the proportion of red areas has increased, although weapons work remains the primary focus of the laboratory.) These red areas serve as a buffer zone around the "green areas," constituting roughly half of the laboratory in the 1980s, where secret research is done. Only those with green badges (bestowed at the end of a lengthy investigation by the federal government) can enter these areas unescorted. Within the green areas, there are also special exclusion areas, set apart by barbed wire fences and guard booths, accessible only to a few. The plutonium facility, for example, is in an exclusion area, as is the facility where intelligence reports are handled. The laboratory, then, is a grid of tabooed spaces and knowledges segregated not only from the outside world but, to some degree, from each other as well. Red areas, for example, although they are located inside the laboratory's perimeter fence are, in terms of informational flow, functionally a part of the outside world that is separated by informational shielding from the laboratory's green areas—some of which are, in turn, shielded from others (Gusterson 1996, ch. 4).

Unlike academic scientists, Livermore scientists in the green areas are not under pressure to publish in order to keep their jobs. The system of a multi-year probationary period followed by either ejection or permanent tenure that organizes scientific careers in the academy does not apply at the Livermore Laboratory. Here, at least until recent financial pressures caused by the end of the cold war, scientists had near-guaranteed job security as long as they worked conscientiously and kept their security clearances in order, and the laboratory's work ethic, especially in comparison with that of research universities, emphasized teamwork over individual distinction.

Up to the end of the cold war at least, nuclear weapons science was principally organized around the design and production of prototype devices for nuclear tests at the Nevada Nuclear Test Site, and around the measurement of these tests.[2] This design and production work was undertaken by enormous multi-disciplinary teams of physicists, engineers, chemists, and technicians, with small teams of physicists playing the lead design role and overseeing the tests. The work of these physicists involved calculating the expected performance of the device, often by refining the enormous supercomputer codes used to model nuclear explosions; checking predictions against data from previous tests, and, in the process, flagging anomalies that might be resolved by further research; making serial presentations to design review committees; consulting with colleagues whose expertise might improve the experiment; consulting with representatives of the Department of Energy and the armed forces about military requirements; and overseeing the machining of parts and the final assembly of the device and the diagnostic equipment.

One weapons scientist, Peter,[3] mentioned in a recent email message to me that, "while the design activity is genuinely a group effort, neither the contribution to the effort nor the acknowledged credit for the result is evenly distributed. One person may be thought of as the principal architect, while others are given credit for significant components." In particular, the lead designer would get special credit. In the localized face-to-face community of weapons designers, this credit would be established and circulated as much by word of mouth—in gossip and in formal presentations—as through the written documentation of individual contributions and achievements. And, in any case, the final result was as much the test itself as any written distillation of it. It was the test that ultimately clarified the validity of the designers' theories and design approaches, and if we ask

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[2] Measurement was a challenge, since the devices, buried underground with the measuring instruments, destroyed the measuring equipment a few nanoseconds after the commencement of the experiment.

[3] "Peter" is a pseudonym. Ironically, anthropology's conventional practice of shielding interviewees by giving them pseudonyms in this case becomes another way of killing the authors behind the barbed wire fence.

what it is that nuclear weapons designers were authoring all those years, we might have to say that it was not ultimately written texts so much as devices and “events”—the weapons scientists’ term for nuclear tests.

The world of nuclear weapons science behind the fence is, though not completely informationally imporous to the outside world, fundamentally autarchic. (One weapons designer told me that her first few years at the laboratory felt like the equivalent of a second physics Ph.D. in fields not taught at the university.) In some ways the national security state has created a national intellectual economy analogous to the traditional unmonetarized African economies described by Paul Bohannon in which there were separate spheres of exchange that could not be integrated so that, for example, the beads of one family could be exchanged for the cloth but not the food of another family, since beads and food, circulating in different spheres, were untradeable and non-convertible. Thus, although it is sometimes possible to transform information produced in the laboratory’s weapons programs into knowledge that can be traded on the open market outside the laboratory, often this is not the case. Peter described one end of the spectrum in his email message:

> As you know, the people involved in weapons work range from someone like Forest Rogers[4] (who calculates wonderful opacities, but would have little practical understanding of a W or B anything [finished nuclear weapons], to Dan Patterson (who lives and breathes weapons). People at Forest’s end of the spectrum can publish the bulk of their work in regular scientific journals. As an example, the first publications of OPAL opacities (OPAL is the code that calculates the opacity) resulted in a paper that for some years was the most cited in astrophysics (fortunately uranium is not important in calculating astrophysical mixtures).

At the other extreme are scientists the very titles of whose publications are secret, so that their resumés are, to the outside world, surrealistically blank after years of labor. One of these joked during a layoff scare, “If I made a resumé there’d be nothing on it.” Another physicist, reflecting on current fears of downsizing with some bitterness, characterized the government’s attitude to its scientists as: “Thanks for defending the country. It’s too bad you don’t have a resumé, but we don’t need you now.” And, indeed, when scientists retire, they are not allowed even to keep copies of their own work if it is classified—a “death of the author” of a particularly poignant kind, as his (or her) lifetime’s creative work is confiscated and swallowed up by the state at the exact moment it releases his or her aged body. This reminds us that weapons designers do not own the

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[4] See Iglesias and Rogers (1996) and Rogers, Swenson, and Iglesias (1996).

knowledge they produce—do not even have a guaranteed right of access to it after they have produced it—since it belongs to the state and the bureaucratic organizations that have commissioned it. In other words, weapons scientists, despite their Ph.D.s, are wage laborers for the state—albeit well paid ones—and, in the final analysis, they have little control over the intellectual wealth they build.[5]

This intellectual wealth is often well shielded from the knowledge markets of the outside world. "There was this complete disconnect with the outside world," one scientist told me. Peter's email message says:

> Many [weapons designers] have given up outside publication entirely. Any good academic paper begins by offering a context to show why the particular detail being investigated is of interest. For example, the detailed processes of lithium production in a particular class of stars is pretty boring to most astronomers who are not nucleosynthesis afficionados. It becomes of interest when framed in the context of determining the original baryon density of the universe. The context for much weapons work cannot be provided, and thank the gods that there is no suitable academic journal for the material that they investigate.

Another scientist recalled a colleague who told him he had not been to the library in years because the outside world knew nothing of him and therefore probably had nothing of interest to say to him in its publications. This can induce a twofold sense of erasure: first, one's achievements and hence one's professional person may be completely invisible to the larger scientific community (or even to one's colleagues within the laboratory: one scientist told me that one of his colleagues won the prestigious Lawrence Award for his work, but he was never able to find out what his colleague had done). Second, one's work may be literally written over by the scientific community outside the fence which, in an inversion of the Soviet nuclear scientists' repetition that established itself as original, publishes original work that is unknowingly a repetition. Peter's email message describes the predicament of Livermore researchers in Inertial Confinement Fusion —until recently a highly classified technology because of its applications to thermonuclear design:

> I went to a conference in 1983 at which an academic researcher was discussing hohlraums as a means of smoothing the laser pulse and converting it to X-rays. The

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[5] The picture is, in fact, more complicated than this thumbnail sketch allows. Some weapons scientists lead a double life, finding ways to publish in the open literature at the same time as they do their weapons work. This enables them to build intellectual capital and authorial profiles outside the laboratory perimeter in a way that makes them potentially mobile in the scientific job and knowledge markets.

> lab people had to sit in silence as a colleague re-discovered territory that they had crossed years before.[6]

Until much of the laboratory's work on inertial confinement fusion was declassified and published after the end of the cold war, it did not publically exist.

But the predicament of nuclear weapons scientists as authors extends beyond their inability to trade their knowledge, and thus to establish their reputations, outside the laboratory. Even within the laboratory establishing their reputations via written authorship can be complicated. This is because the laboratory's knowledge economy mixes the characteristics of a common market with those of a medieval kingdom with many separate zones of barter, currency, and taxation. Traditionally nuclear weapons knowledge was recorded not so much in standardized and refereed articles, as it would be in conventional academic settings, but in reports detailing the results of nuclear tests, new ways of calculating opacities, and so on. These reports, instead of being codified into a uniformly accessible grid of knowledge, were often stored eccentrically. As one scientist described it:

> There was a mill for publishing the results of test shots, the latest methods for calculating opacities and so on. But there was no serious library for these reports in the early days. The reports would get thrown in a room, then someone would take one and hold on to it and that article would now be officially "misplaced." (That's why the GAO found that 10,000 secret documents were missing at Livermore. They're not exactly lost. They're not floating around outside the lab. They're in people's offices somewhere.) Old-timers would have safes full of documents inherited from someone else who retired ten years earlier. So, when they retired, you'd get those documents transferred to you, and that was a sort of library.

In other words, even within the laboratory, knowledge could be stored and exchanged in highly localized ways. The circulation of knowledge might be restricted by the semi-forgotten nature of a written report languishing in a colleague's safe, by networks of friendship, or by the assumption

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[6] The Soviets did not classify Inertial Confinement Fusion research to the same degree as the Americans. This could lead to curious situations such as one at a conference in the 1980s where Livermore fusion researchers were embarrassed that Russian scientists were openly presenting the results of their fusion experiments to an audience that included many Americans without security clearances—even though the rationale for hiding such knowledge from the uncleared was that they might share it with the Russians!

that weapons scientists, for national security reasons, should not have access to too much secret information unless it was directly relevant to their work.[7]

At its most extreme, the laboratory environment can unmake the very form of writing itself as a means of storing information, creating within one of the most high-tech environments in the world a partial return to the orality that preceded literacy and hence removing the very possibility of authorship in the modern writing-based conception of the term. Many scientists' reputations rest not on written reports,[8] but rather on oral presentations they have given; on insightful questions in design review meetings; on huge craters their devices have inscribed upon the surface of the Nevada Desert; on an inventive idea they are locally remembered to have suggested and worked through; on a beautiful component they designed which was instantly vaporized by the very test whose success it enabled; and on a socially recognized knack for judgement—a feeling for the devices and how they will behave. Because so much weapons design knowledge is practical knowledge that is unwritten or is thought to be hermeneutic rather than purely factual in nature, it is seen as residing in the designers themselves. (For this reason the laboratory prohibits groups of designers from traveling together on the same plane, in case it crashes.) One of the older designers, Seymour Sack, was described to me as "a walking repository of 500 experiments [nuclear tests]." Some scientists worry that, as Peter put it:

> There are so few people genuinely involved in design, you efficiently communicated by other means [than formal writing] . . . And the formal record suffers from this deficiency. While we have vaults containing the measured results of tests [as well as cutaways of nuclear devices showing their internal "anatomy"], the reason that certain choices were made are not obvious from the materials stored there. This information still exists as oral histories, but the content of this reservoir diminishes as the experience base drops.

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[7] It was widely believed in the 1980s by weapons designers in A and B Divisions, the two main weapons design divisions, that O Group, a breakaway group of designers ultimately protected by Edward Teller's patronage, manipulated secrecy regulations to protect its work from peer review. O Group was working on, among other things, a nuclear bomb-pumped X-ray laser that was highly controversial both technically and politically. Many weapons scientists complained that they suspected O Group's science was not rigorous, but could not evaluate it because of special levels of classification placed on its reports and briefings.

[8] One interesting example here is Bruce Tartar, the current director of the laboratory. One scientist told me that, curious to know more about his director's scientific career before he became director of the laboratory, he had tried to find what he had written about, but was unable to find a single report or article by him listed anywhere.

Ironically, if Plato worried that the transition to literacy and the written documentation of information would destroy memory, contemporary weapons scientists worry, inversely, that their high-tech oral culture will prove the enemy of memory, as aging designers retire and die. They worry that not just their individual contributions but substantial parts of their science itself will die out in the absence—now that the testing ranges of the world have fallen silent—of the nuclear tests which, more than written documentation, have enabled the reproduction and transmission of their science. This science has been passed on by means that, in some ways, have more in common with medieval craft apprenticeships than the computerized bibliocentric mazeways of most scientific disciplines at the end of the twentieth century.[9]

The years since the end of the cold war have seen increasing attempts to codify and document what the weapons scientists know and to bring the means by which their information is recorded into greater conformity with the practices of the outside world. This is a form of nuclear salvage work, thought it differs from the efforts of Rhodes and Holloway (discussed below) in that it is more interested in the formal codification of knowledge than in the individualization of its authors. Thus, in recent years, Livermore scientists have invested time in cataloguing reports and installing them in a central library and in making written or videotaped records of the reasons for specific design decisions. In 1989, the laboratory also started a peer-reviewed classified journal, modeled on those published by university scientists. This journal has not, however, done very well, partly because it runs counter to the comfortable orality of knowledge circulation long established among the weapons scientists. One scientist said the journal was “of little consequence.” Another described it as “a strung-out, thin sort of a thing, not conveniently available.” He said, “I never tried to publish in the journal because I thought it was pointless. Three people would read it, and then it would disappear forever.” He added (echoing the sentiments in the Djerassi quote with which this paper began) that the point of publishing is to have people who have not met you read about your work but, since his research can only be discussed within a small face-to-face community that already knows about his work, publication would be a futile waste of time.

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[9] This has led MacKenzie and Spinardi (1995) to argue that, in the absence of nuclear testing, advanced nuclear weapons design knowledge might more or less fade away.

Nuclear Salvage History

In 1945, after the revelation of the Atomic Bomb, it was Oppenheimer, the Director of the Los Alamos Laboratory and *Life* magazine's Man of the Year, who received the credit for the bomb. This was despite the fact that the bomb was originally conceived by Leo Szilard, and the implosion mechanism—crucial in making the plutonium bomb work—was thought of by Seth Neddermeyer (a scientist who has long since disappeared into the oblivion of anonymity) and refined by Teller, Von Neumann and Kistiakowsky (Rhodes 1988).

Seven years later, after the first hydrogen bomb was tested, the media erroneously gave the credit to Edward Teller's new laboratory at Livermore, and scientists at Los Alamos, furious to find their entire institution stripped of credit for its work, were prevented by national security regulations from correcting the error (York 1975, 13).

Edward Teller himself has been known for years as "the father of the H-Bomb," even though the key design breakthrough is now widely credited to Stan Ulam,[10] and Teller largely withdrew from the project as it entered the engineering phase (Rhodes 1995). Disquiet among former colleagues at Teller's popular identification as *the* inventor of the hydrogen bomb eventually impelled him, in 1955, to publish his *Science* article, "The Work of Many People," in which he described the H-Bomb as "the work of many excellent people who had to give their best abilities for years and who were all essential for the final outcome." He protested that "the story that is often presented to the public is quite different. One hears of a brilliant idea and only too often the name of a single individual is mentioned" (Teller 1955, 267). That individual was, of course, Teller himself and, although in his article he named the other people who were vital to the project, he was not permitted by security regulations to say what any of them actually did. Thus the article, paradoxically, has the effect of reinforcing the appearance of Teller's singularity since, as lone author, he is arbitrator and custodian of others' unknown contributions, which he authorizes.

We see in these examples how the secrecy characteristic of nuclear weapons research makes it difficult to certify the distinctive contributions of individuals, creating a situation where

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[10] Ulam thought of making the hydrogen bomb a two-stage device in which the first stage (a fission bomb) would be used to compress, not just ignite, fuel in the secondary. Teller later thought of using radiation rather than neutrons from the atomic bomb to achieve compression (Rhodes 1995, ch. 23). Some weapons scientists have joked that Ulam "inseminated" Teller with the idea and that Teller is in fact the "mother of the H-Bomb" (Easlea 1983).

credit tends to gravitate towards those, such as Teller and Oppenheimer, who already have established scientific reputations or bureaucratic positions of authority. This gravitational tendency has been reinforced by the organizational dynamics of "big science" laboratories, such as Los Alamos and Livermore, where weapons science has been undertaken. In these large hierarchical science institutions intellectual value, or capital, tends to behave in the same way as material value in large capitalist institutions: it is extracted from those on the bottom, who create it through labor, accruing as wealth to those on the top. Thus in the large science laboratories the labor of a Seth Neddermeyer is transmuted into the reputation of a Robert Oppenheimer.[11]

The last ten years have seen accelerating attempts by historians and other chroniclers of nuclear history to undo the Bourbakification of the inventors of the atomic and hydrogen bombs and to bestow secure identities and lines of credit on those scientists who, as their generation dies, stand between anonymity and immortality. I call this nuclear salvage history. Nuclear salvage history seeks to reverse the phenomenological death of the scientific authors of the first decade of the nuclear era just at the moment when their physical bodies are expiring. This project has been aided by the progressive declassification of the basic weapons design information and by the increasingly urgent desire of the pioneers of nuclear weapons science, now in their twilight years, to record their labors.

The leading practitioner of nuclear salvage history is the indefatigable Richard Rhodes, whose books *The Making of the Atomic Bomb* and *Dark Sun: The Making of the Hydrogen Bomb* have exhaustively catalogued the personalities and contributions of the principal scientists in the first decade of nuclear weapons science. Rhodes's history is resolutely middlebrow in the sense that it is the story, vividly told, of great men, each a miniature portrait in his own right, acting on the world to change history.[12]

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[11] For more on the dynamics of big science, see Galison and Hevly (1992) and Galison (1997).

[12] This approach also characterizes the biographies of two of the great Manhattan Project scientists: Lanouette's (1992) biography of Leo Szilard and Gleick's (1992) biography of Richard Feynman which, even in their titles (*Genius* and *Genius in the Shadows*) focus on the creativity and uniqueness of their subjects. As the literary theorist David Lodge has observed, commenting on the imperviousness of biography to new literary theories that decenter the subject, "literary biography thus constitutes the most conservative branch of academic literary scholarship today. By the same token, it is the one that remains most accessible to the 'general reader'" (Lodge 1996, 99).

Rhodes's books about weapons scientists are epics of invention in which he is deeply concerned with the documentation and demarcation of individual originality and creativity. Martha Woodmansee points out that the modern conception of authorship is "a by-product of the Romantic notion that significant writers break altogether with tradition to create something utterly new, unique—in a word, 'original'" (Woodmansee 1994a, 16). This essentially Romantic trope of originality as an individual gift that strikes in world-changing flashes of inspiration is common in middlebrow science writing, where it resonates with high school textbook accounts of Archimedes' and Newton's discoveries, and it figures prominently in Rhodes's accounts. Some of the most compelling passages in his books describe the exact moment of creative inspiration, which he hunts down with extraordinary determination. Take, for example, the cinematically vivid opening paragraph of *The Making of the Atomic Bomb*, in which he describes Leo Szilard's sudden realization that it might be possible to construct an atomic bomb powered by a nuclear chain reaction:

> In London, where Southampton Row passes Russell Square, across from the British Museum in Bloomsbury, Leo Szilard waited irritably one gray Depression morning for the stoplight to change. A trace of rain had fallen during the night; Tuesday, September 12, 1933 dawned cool, humid and dull. Drizzling rain would begin again in early afternoon. When Szilard told the story later he never mentioned his destination that morning. He may have had none; he often walked to think. In any case another destination intervened. The stoplight changed to green. Szilard stepped off the curb. As he crossed the street time cracked open before him and he saw a way to the future, death unto the world and all our woe, the shape of things to come.[13] (Rhodes 1988, 13)

The same trope recurs in *Dark Sun: The Making of the Hydrogen Bomb*, where Rhodes records Françoise Ulam's memory of her husband's breakthrough in the design of the hydrogen bomb with the same dramatic emphasis on one man's destiny to change history:

> Engraved on my memory is the day when I found him at noon staring intensely out of a window in our living room with a very strange expression on his face. Peering unseeing into the garden, he said, "I found a way to make it work." "What work?"

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[13] Rhodes subsequently revealed the extraordinary labor that went into the research and writing of this paragraph. He had to visit London to see the intersection for himself, and he research London weather records so that he could evoke the physical setting for Szilard's inspiration as precisely as possible.

> I asked. "The Super,"[14] he replied. "It is a totally different scheme and it will change the course of history." (Rhodes 1995, 463)[15]

Juxtaposing such dramatic moments of inspiration with all the other contributions that brought nuclear weapons into being, Rhodes's writing is also encyclopedic in impulse. Michel Foucault (1977, 147) has observed that the modern individualist idea of the author has a "classificatory function," since the author's "name permits one to group together a certain number of texts, define them, differentiate them from and contrast them with others." We see this classificatory function clearly in Rhodes's books, which seek to demarcate the exact contribution made by each of the leading weapons scientists and to rank them. (He spends several pages, for example, discussing whether Ulam or Teller should get more credit for the hydrogen bomb.) In the process of this enormous accounting operation he salvages the contributions, formerly known to few, of those like Neddermeyer, saving them from their own premature authorial deaths, and he redefines the

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[14] The "Super" was the hydrogen bomb.

[15] Rhodes tries to trace the exact moment of Neddermeyer's conception of implosion in the same way, but ultimately has to content himself with a more speculative discussion of the exact origin of the idea:

> Neddermeyer could not quite remember after the war the complex integrations by which he came to it [implosion]. An ordnance expert had been lecturing. The expert had quibbled at the physicists' use of the word "explosion" to describe firing bomb parts together. The proper word, the expert said, was "implosion." During Serber's lectures Neddermeyer had already been thinking about what must happen when a heavy cylinder of metal is fired into a blind hole in an even heavier metal sphere. Spheres and shock waves made him think about spherically symmetrical shock waves, whatever those might be. "I remember thinking of trying to push in a shell of material against a plastic flow," Neddermeyer told an interviewer later, "and I calculated the minimum pressures that would have to be applied. Then I happened to recall a crazy thing somebody had published about firing bullets against each other. It may have been a picture of two bullets liquefied on impact. That is what I was thinking when the ballistics man mentioned implosion." (Rhodes 1988, 466)

If Rhodes's books use, wherever possible, the trope of sudden inspiration to narrate the origins of America's first and second generation nuclear weapons, it is interesting that William Broad's (1985) account of the stillborn genesis of third generation nuclear weapons at the Livermore Laboratory in the 1980s contains exactly the same literary device in its description of Peter Hagelstein's sudden envisioning of a design for the X-ray laser at a review meeting where he was in a mystical state induced by sleep-deprivation. For a playwright's use of exactly the same literary device, this time to evoke Alan Turing's breakthrough in cracking the Nazi Enigma code during World War II, see Whitemore (1996).

contributions of the manager-scientists, of whom Oppenheimer is the obvious exemplar. Oppenheimer's brilliance is displaced in Rhodes's account from scientific invention to recruitment, synthesis, and leadership. For example, Oppenheimer may not have thought of implosion, but he had, in Bethe's words, "created the greatest school of theoretical physics the United States has ever known" (Rhodes 1988, 447), where Neddermeyer, who did think of implosion, was trained. But above all Oppenheimer managed and led. Rhodes summarizes his contribution to the Manhattan Project thus:

> Robert Oppenheimer oversaw all this activity with self-evident competence and an outward composure that almost everyone came to depend upon. "Oppenheimer was probably the best lab director I have ever seen," Teller repeats, "because of the great mobility of his mind, because of his successful effort to know about practically everything important invented in the laboratory, and also because of his unusual psychological insight into other people which, in the company of physicists was very much the exception." "He knew and understood everything that went on in the laboratory," Bethe concurs, "whether it was chemistry or theoretical physics or machine shop. He could keep it all in his head and coordinate it. It was clear also at Los Alamos that he was intellectually superior to us." (Rhodes 1988, 570)

This evocation of the role of the manager in the big physics laboratories that emerged in mid-century is, incidentally, echoed in Zel'dovich's comment about Oppenheimer's Soviet counterpart, Yuli Khariton, who oversaw the construction of his country's first atomic bomb. Zel'dovich told the young Sakharov, "There are secrets everywhere, and the less you know that doesn't concern you, the better off you'll be. Khariton has taken on the burden of knowing it all" (Holloway 1994, 202).

The Soviet bomb project has produced its own nuclear salvage history, the finest example of which is David Holloway's *Stalin and the Bomb*. Unlike Rhodes, Holloway is a highbrow historian who situates his narrative in a broader historical context and uses it to illuminate the dynamics of Soviet society and of the cold war international system. However, like Rhodes, Holloway also seeks to discern the contributions made by specific individuals, to rank and compare them, and to mark what was original—though this turns out to be a troubling category. In producing this history Holloway faced two special problems. The first was the intense secretiveness of the Soviet state, which had rendered its own nuclear scientists even more anonymous and mysterious, more Bourbakified, than their counterparts in America. Thus, if Rhodes's writing derives much of its power from his ability to show us vivid individual characters and richly textured narratives of scientific work behind Los Alamos' veil of secrecy—to salvage the details of authorship from the well of anonymity—Holloway's accomplishment in salvaging the details of the Russian nuclear story in a much more closed society must be judged still more extraordinary.[16]

Holloway's second difficulty was, in writing his own version of the nuclear epic, to establish the authority of scientists condemned to a repetition. The Soviet scientists were, after all, not only doing something that had already been done; they were, in the case of the atomic bomb at least, doing it with the aid of design information purloined from Los Alamos by the spy Klaus Fuchs.[17] As Martha Woodmansee (1994a) argues, while copying and embellishing the work of others used to be seen as a form of authorship in its own right in mediaeval Europe, in the context of contemporary copyright law and current ideologies of authorial individualism, copying is now seen as a highly degraded form of creativity. Thus the enterprise of establishing scientific authority in Holloway's nuclear salvage history is enacted in circumstances that call for different, at times more defensive, narrative strategies than Rhodes's. In Holloway's account it is also clear that, given the discursive conjoining of science and nation-building in Soviet nationalist ideology, what is at stake in establishing the authorship of these weapons is not only the reputation of individual scientists but also the reputation of the nation these scientists represent.

As far as the atomic bomb is concerned, Holloway's strategy is to remind us that Khariton could not be sure the purloined information was accurate, so that "Soviet scientists and engineers had to do all the same calculations and experiments" as their American counterparts (Holloway 1994, 199). He then details who did what where. As regards the hydrogen bomb, Holloway shows that the information Fuchs gave the Soviets about design efforts in the United States would have misled them since Los Alamos at this time was, under Teller's guidance, pursuing a design strategy that turned out to be a blind alley. Holloway demonstrates that Sakharov and Zel'dovich fol-

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[16] This is to speak as if Holloway wrote only about the Soviet scientists and Rhodes only about the Americans. In fact, substantial portions of Rhodes's *Dark Sun* narrate the Soviet bomb project as well. However, Rhodes, who does not speak Russian, was at a disadvantage researching the Russian side of the story, and these parts of the book are generally considered to be weak, even misleading in parts.

[17] In the early 1990s this became a matter of some controversy in Russia as the intelligence services and veteran scientists of the original Soviet atomic bomb project feuded over who should get most credit for the first Soviet nuclear test: the spies who obtained the design for America's first plutonium bomb or the scientists who figured out how to build it.

lowed their own design path, in many ways making quicker progress than their American counterparts and that, although the Americans were slightly ahead of the Soviets in creating a full-blown thermonuclear explosion, the Soviets were ahead in learning to use lithium deuteride—the key in making a deliverable bomb rather than an enormously unwieldy thermonuclear firecracker (Holloway 1994, ch. 14).

The stakes attached to originality (even if only the originality of a repetition) here are high, for individuals and nations. When Hirsh and Mathews published an article in 1990 in a fairly obscure American journal alleging that the Soviets had used fallout from the first American H-Bomb test in 1952 to deduce the design breakthrough made by Teller and Ulam,

> . . . it caused some consternation among scientists who had taken part in the Soviet project. Khariton asked that a search be done of the files of those scientists who had been engaged in the detection and analysis of foreign nuclear tests. Nothing was found in those files to indicate that that useful information had been obtained from analysis of the Mike test. This was not because of self-denial. Sakharov and Viktor Davidenko collected cardboard boxes of new snow several days after the Mike test in the hope of analyzing the radioactive isotopes it contained for clues about the nature of the Mike test. One of the chemists at Arzamas-16 unfortunately poured the concentrate down the drain by mistake, before it could be analyzed. (Holloway 1994, 312)

Thus did the carelessness of a chemist save the honor of a nation.

The nuclear salvage history of Holloway and others has given names to the scientists behind the Soviet bomb, bestowed epic status on their labors, and enabled them to take their place as individuals in the Pantheon of science. In other words, it has saved them from Bourbakification in a way that is nicely evoked by the English physicist Stephen Hawking’s quip when he finally met Zel’dovich: “I’m surprised to see that you are one man, and not like Bourbaki” (Holloway 1994, 198).[18]

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[18] Hawking meant by this that Zel’dovich seemed to have accomplished too much for one man. The admiration for Zel’dovich, and the sense of him as a great scientist, is also conveyed in a story told to me by a scientist at the Livermore Laboratory: when the Princeton physicist John Wheeler, who had worked on the American hydrogen bomb, finally met Zel’dovich, he presented him with a salt and pepper shaker, one male and one female in shape. Alluding to the greater elegance of the first Soviet H-Bomb design compared to its American counterpart, he said that the male represented Zel’dovich and the female Teller.

Conclusion

Michel Foucault (1979) and Roland Barthes (1988) have both argued that what we recognize as authorship is a social institution that emerged at a particular historical moment defined by social individualism, scientific rationalism and, we might add, commodification. Over the last two centuries the ideology of authorship has tended to privilege written texts. These have been construed, through the lens of Romantic assumptions about individual creativity, as the products of unique individuals. Especially in the sciences, which Robert Merton (1942) long ago defined precisely in terms of their commitment to the universal circulation and accessibility of texts, these texts have circulated freely and have been collected in libraries that facilitated widespread access to them.

The Livermore Laboratory has developed a mode of scientific production partly at odds with these conventional notions of authorship. Although some knowledge circulates in formally authored texts, much of it circulates orally and informally. This knowledge is often produced in collaborative teams, so that individual intellectual production is not so highly fetishized as it is in academic circles where lead authorship and quantity of authorship is so vital a metric in tenure and promotion decisions. And, far from circulating freely, the written knowledge produced within the laboratory often cannot leave the laboratory (unless it is going to Los Alamos) and, even within the laboratory, may lie dormant in safes or travel eccentric routes of exchange marked by chains of friendship rather than being universally available.

In terms of the politics of authorship, it is hard to know what to make of this. Martha Woodmansee (1994b) has argued that the conventional ideology of authorship, which fetishizes the individual and commodifies texts through copyright laws, is a prison-house that inhibits collaborative creativity and forces us to misrecognize the degree to which all intellectual production is, no matter what the copyright lawyers say, inherently social and collaborative. In some ways scientists at Livermore might be said to have escaped this prison-house, liberated by the barbed wire fence around them. The knowledge they have produced largely circulates outside the commodified sphere of exchange regulated and constrained by copyright laws and the academic promotions treadmill. And many Livermore scientists, in a critique of academic culture that is increasingly resonant for this author, criticize the cult of individual assessment in the university and the emphasis in academia on stockpiling refereed articles as commodities, even if hardly anyone reads many of them. Many scientists told me they were attracted to work at Livermore precisely because it emphasized collaborative teamwork and did not force its scientists to publish or perish. As one weapons designer put it:

> I find writing hard, and I don't like the publish or perish business. It's not that I don't like pressure or hard work; I just like to impose my own deadlines rather than jump through other people's hoops. The university is like the military the way it confines you and arranges everyone in hierarchies . . . I have more freedom at the lab (quoted in Gusterson 1996, 47-48).

On the other hand, these freedoms come at a price, since scientists may lose individual control over the products of their intellectual labor. These scientists may not be allowed to own copies of their own writings once they retire, may not be allowed to circulate their papers—even to name them—to friends, family, and colleagues beyond the barbed wire fence. Indeed, they could be prosecuted for discussing their own ideas with the wrong people, since their ideas belong to the state. Hence they cannot use their writings to build a public persona as authors conventionally do. Nor, until recently, could they earn royalties if they designed something patentable since the patent was awarded to the Department of Energy.

There are now signs, however, that, the end of the cold war is forcing a revision of authorship practices at the Livermore Laboratory. Just at the moment when it has lost nuclear testing, traditionally a means of consolidating and transmitting weapons design knowledge, the Laboratory is increasingly moving to formalize and codify its knowledge, cataloguing and centralizing reports, trying to transcribe oral knowledge, and establishing a peer-reviewed journal for weapons designers. In some ways the laboratory seems to be trying to bring about the (re)birth of the author.

But what are the limits of this (re)birth? Can it rupture the isolation of the laboratory and restore its weapons scientists to history, as Rhodes and Holloway have done for Ulam, Neddermeyer, Zel'dovich, and Altschuler? It may be that, unlike the contributions of Neddermeyer and Ulam, the work of today's American weapons scientists lies beyond the retrieval techniques of nuclear salvage history. Working in teams on design tasks seen as routine rather than charismatic, their work shrouded in secrecy and only partly documented, these scientists, known as unique individuals by one another, may be condemned in the knowledge of the outside world to live outside history.

Acknowledgements

I am indebted to Babak Ashrafi, Mario Biagioli, Roberta Brawer, and Allison Macfarlane for directing me to reading and for clarifying in discussion some of the ideas in this paper. My thanks also to the four weapons scientists who answered my urgent email appeals for information on this subject.

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LEARNING TO KEEP SECRETS: THE MILITARY AND A HIGH-TECH COMPANY

Alec Shuldiner

Introduction

All companies have secrets—employee data are often proscribed by law, the hierarchical structure of most business organizations entails unequal access to information internally, a superior market position may be achieved by keeping a competitor in the dark, and so on—but high-tech companies, are particularly interested in secrets, most notably the technical secrets that are the ideal product of their R&D. If these secrets can be held as the exclusive preserve of that organization, it will likely be able to reap a far larger percentage of their fruits. This effort to contain knowledge may be aided by legal constructions such as patents, but as any student of intellectual property (or S&TS for that matter) knows, in practice there is no such thing as perfect disclosure in a patent application or elsewhere, and secrecy (not just tacit knowledge) remains an important part of high-tech industrial practice, even in companies that make their knowledge available to the public or their competitors.

How an organization of any size can keep a secret is not, however, immediately obvious. A company must create protocols for defining secrets and for deciding when and with whom they should be shared; it must also find a way to judge how well those procedures are working and to detect when a secret has been improperly divulged. Secrecy is, simply stated, a serious organizational challenge for commercial enterprises of this sort. This challenge is shared by any organization, commercial or otherwise, that wishes to manage the flow of information within and through its boundaries.

An obvious example of this latter sort of organization is the United States government, and in particular the U.S. military, which is centrally concerned with controlling information and preserving secrets. As recent scholarship has shown, the development of American industry has been significantly influenced by military examples, from interchangeable parts manufacture (the "American System") and the logistics of railroad management to machine tool development and the creation of modern business planning systems (most famously PERT).[1] In light of this, and of the military's expertise in secrecy, it seems not unreasonable to suppose that here, too, managers have taken their cues from majors. The question then—and the subject of this paper—is to what extent, in the American context, have secrecy protocols and practices transferred from the military to high-tech commercial enterprises?

My answer falls within certain limits. Most important, I rely exclusively on a single example of a high-tech company: Corning, Inc., a maker of specialty materials located in Corning, NY; this paper is a case study with the usual strengths and weaknesses that implies.[2] Furthermore, I do not explore the distinctions between NASA, the AEC/DOE, and the DOD, as a more complete analysis would require. Last, I do not consider the power of the military's example as an indirect factor in the formation of corporate secrecy practices (e.g., how widespread use of classification by the military has made the keeping of secrets in general a more acceptable practice among American businesses); though important, such historical work is beyond the scope of this project.

These qualifications not withstanding, my findings may be broadly stated thus: commercial high-tech companies (as opposed to companies engaged primarily in military contracting, the inner circle of the military-industrial complex) are not particularly likely to alter existing corporate secrecy practices in general as a result of exposure to military secrecy requirements and procedures. Though any high-tech company accepting military contracts is expected to adopt certain specific secrecy practices as part of its contractual agreement with the military, Corning at least has intentionally worked to limit the impact of military secrecy requirements and practices within the firm as a whole, and, to some extent, has been abetted in this effort by the nature of military

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[1] See Merritt Roe Smith, *Harpers Ferry Armory and the New Technology: The Challenge of Change* (Ithaca: Cornell University Press, 1980); Alfred D. Chandler, Jr., *The Visible Hand: The Managerial Revolution in American Business* (Cambridge: Belknap Press, 1977); David F. Noble, *Forces of Production: A Social History of Industrial Automation* (New York: Knopf, 1984); and Harvey M. Sapolsky, *The Polaris System Development: Bureaucratic and Programmatic Success in Government* (Cambridge: Harvard University Press, 1972), respectively.

[2] This paper is drawn from a chapter of a book on the history of R&D at Corning that I am co-authoring with Margaret Graham, a partner at the Winthrop Group (see the Postscript for more information on this project). Though I have reworked all of the material in this paper, many points of analysis were originally, and in essence remain, the product of our collaboration. My thanks to her here, as well as to all those at Corning who have made this project possible.

secrecy requirements themselves. This, I argue, is likely to have been the case at other companies of this sort.

I begin with a short history of secrecy at Corning. Two episodes from that account are then more closely considered: Corning's experience with the military during World War II, and the case of Edward U. Condon (Corning's Director of R&D from 1951 to 1954). I conclude with a necessarily brief discussion of Corning's Canton facility—its most extreme instantiation of military secrecy requirements—and the assertion that a misfit between military and commercial secrecy practices did exist at Corning, and is likely to exist at similar companies, such that relatively little practical exchange normally occurs between the two.

A Brief History of Secrecy at Corning

Corning has never been a major defense contractor (with the important exception of its work for the government during World War II), but it has maintained a variety of military and commercial product lines simultaneously since the early 1940s. Corning has also long thought of itself as an industry leader in R&D, and considers its intellectual property holdings an essential part of its competitive capabilities. In terms of glass technology in particular, it is arguably the pre-eminent research-producing organization in the world, and is notable as having founded one of the very first industrial research laboratories in the United States.

Placing glass production on a modern industrial basis involved the divulgence and subsequent codification of very closely-held secrets. Neither were easy tasks: the knowledge of the gaffer (or master glassworker) was not casually shared nor were the practices of such craftsmen often directly translatable into either laboratory terminology or mechanical operation. Corning, from its founding in the mid-1800s, was forced to be conscious of the importance and complexity of secrecy in the industrial context.

Corning did not, however, have any notable contact with the military until World War I. The British naval blockade at the beginning of that war halted the flow of German glassware into the United States and gave Corning its opportunity to begin production of scientific glassware for the American market (the beginning of its famous Pyrex line). This same blockade also cost the United States access to high-quality optical glass, and without such glass, production of binoculars, fire control equipment, range-finding instruments, and other crucial military goods was halted. The Department of War decided to help establish domestic production in order to command sufficient stocks of optical glass during the coming conflict.

That effort, which from 1917-1919 closely managed upwards of ninety percent of America's optical glass production, was led by Arthur L. Day, at the time the Director of the Carnegie Institution of Washington's Geophysical Laboratory. Day initially turned to Corning for assistance in this undertaking: he knew the company's capabilities well, having helped establish its first laboratory in 1908 with a former colleague of his, Eugene Sullivan, as that lab's director. Sullivan, however, felt that the job would be an unprofitable use of scarce resources. "We are not in a position to spend money on optical glass experimentation at the present time," he wrote to Corning president A.B. Houghton, "unless the Government is willing to take its share of the outlay. . . ."[3] The government was not, and absent either opportunities or obligations, Corning chose not to act.

World War II was a very different story. The lessons of the Great War having been ignored, in 1939 the U.S. found its optical glass supplies once again threatened. With German sources foreclosed, the American military was forced to turn to domestic suppliers, who, it was discovered, had once again largely exited the market in the face of German competition. This time Corning led the effort to meet Allied military demand for optical glass and contributed crucially to radar and other important wartime projects. In some cases, in particular that of radar, strict secrecy regimes were imposed on Corning and other companies engaged in military work.

World War II provided a precedent for the Korean War's rearming and the subsequent creation of the Cold War's military-industrial complex. Both of these later developments were accompanied by an increasingly strict regime for the determination and maintenance of secrecy on the part of the government in general and the military in specific. Companies working on defense contracts were required to subscribe to carefully defined and sometimes onerous secrecy requirements. Corning, busy with projects for the Department of Defense, the Atomic Energy Commission, and the National Aeronautics and Space Administration, had a more than casual acquaintance with these procedures. The company was, however, careful to limit its exposure to the rapidly forming military economy.

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[3] Eugene C. Sullivan, letter to A.B. Houghton, 29 March 1917 (Department of Archives and Records Management, Corning, Inc. [hereafter DARM], box P-4, folder "Optical Glass").

Even absent military work, however, Corning's management remained deeply concerned with keeping secrets. The infamous Glass Trust, of which Corning was a key member, was based in large part on interlocking intellectual property [IP] agreements. Like many such arrangements, it was characterized by the gentlemanly sharing of proprietary R&D information within the trust and the jealous withholding of that same information from those unfortunate enough to be excluded. The busting of the Glass Trust in 1946 revealed that the sharing of personnel and practices between the members of that trust—secrecy as tacit knowledge—was in some respects an even greater obstruction to competitors than were exclusive IP agreements. In any case, both intellectual property and the hoarding of tacit knowledge within the company continued to play a key role in management's strategic planning. Corning engaged in extremely expensive efforts to protect its IP position in glass-ceramics and optical waveguides in the 1960s and 1970s, for example, and both projects, as well as others, were at various points considered highly secret by the company while under development.

Still, the most carefully hidden of Corning's secrets was the direct product of government contracts: the Canton cell. Located in Canton, NY, government contracts requiring "Top Secret" clearance were largely relegated to this R&D facility. Radar delay lines, satellite optics, and similar undertakings dear to the Cold War heart were the lifeblood and justification for Canton, which was intentionally sited deep in the woods of upstate New York. Yet throughout this all, Corning remained a relatively small company, in comparison both to other glass manufacturers and to other major industrial performers of R&D. As a result, with the only partial exception of the Canton cell, personnel involved in military work were generally also occupied in part with commercial projects, often along similar lines. The company has never had either the resources or the taste for a duplication of research efforts.

Corning, the Military, and World War II

As noted above, Corning's relationship with the military effectively began with World War II. Indeed, that war represented not only Corning's introduction to military contracts, but also the most intensive encounter the company would ever have with the peculiar needs and requirements of the military. In retrospect the degree of Corning's involvement in the war effort seemed inevitable:

> If anyone doubted 'Corning Means Research in Glass,' such doubt has been erased by the wartime requirements placed on our laboratory and production organizations. Countless projects have been solved for the various agencies of the Government. . . . Corning had built its reputation on doing in glass that which others could not do. Naturally then, in this most scientific of all wars, the Government turned to us for vital and new products.[4]

Yet previous to the war Corning had thought of itself (not unhappily) as too small and specialized to be of much concern to the authorities in Washington. This self-image, and the relative ignorance of military imperatives that it implied, was to change rapidly.

In 1939, Corning's chairman, Amory Houghton, was tapped to be a member of the War Production Board, one of a growing army of Dollar-A-Year men called to Washington to help the national mobilization effort. Among other things, this gave him access to large amounts of data about his and related industries, as well as knowledge of projected military needs. Ironically, Houghton's position did not immediately translate into a high priority ranking for Corning, and the company was left painfully vulnerable to materials shortages and manpower losses.

This situation was eventually rectified, even while Corning embarked on an intensive and almost all-consuming research program at the government's behest. A 1945 list of R&D projects actively connected to the war effort shows that the company was working on improved products or processes in optical glasses, filter glasses, lighting ware, electric lamps, electronic devices, atomic energy, triggering devices, projectiles, landmines, chemical warfare, silicone products, and more. The list includes projects undertaken for all branches of the military as well as many companies and organizations, including instrument makers, oil companies, and university research laboratories, that subcontracted work to Corning.[5]

Corning's ability to respond to a surge in manufacturing demand was also tested. Its role in producing cathode ray tubes, the large vacuum bulbs that lay at the heart of radar display systems, was critical to the war effort and accounted for something like two-thirds of the company's wartime production capacity. A second wartime production role considered to be vital to defense needs was the production of optical glass, for which the government built Corning a plant in

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[4] N.a., no title, n.d., p. 1 (DARM, box G-8, folder "Postwar Planning").

[5] W.W. Shaver, "Corning Glass Works Research and Development Projects Actively Connected with the War Effort," 1945 [DARM, box 9, folder "War Products (Priorities)"].

Parkersburg, West Virginia. With the addition of this 100,000-pound-capacity plant, Corning became one of the two largest producers of optical glass in the country and unquestionably its most efficient. The *Saturday Evening Post* reported in 1944 that "Corning is tied into war jobs by 75 per cent of its capacity," a figure that did not include indirect work for other government contractors.[6]

Yet despite its absorption in the war effort, Corning worked to maintain some degree of independence, especially in its research division. It differed during the war from most other research-performing companies in that it took no money in support of R&D from the Office of Scientific Research and Development, and very little from the various branches of the military that sought its help.[7] This was in marked contrast to many of its major customers in the radio industry: Westinghouse, RCA, General Electric, and Zenith all received between five and ten million dollars in OSRD funding alone. Corning eschewed direct funding intentionally as a precaution against future claims that it had any obligation to share its proprietary technology with other government suppliers.

This independence was emphasized by Corning's postwar behavior, itself based on decisions made as early as 1942. By any measure, the company's most important wartime efforts had been the development of mass production techniques for the manufacture of CRT bulbs for radar sets and the greatly improved melting processes for optical glasses used in all manner of military products. Both eventually led to postwar military contracts (Corning's work on massive optical elements for the Air Force's wind tunnel and geodetic survey projects was particularly successful), yet radar also grew into television, and military optical glass demand came to be dwarfed by Corning's ophthalmic business. For Corning, wartime work would prove to be first and foremost a source of new commercial opportunity in the civilian sector, in distinction to other companies that used the war as a chance to become permanent defense contractors.

Though marked by and largely remembered for its string of successes, Corning's wartime experience was not entirely positive. While petitioning to have its priority rating raised, Corning lost 3,000 of its employees, many of them technicians, supervisors, and people with special skills.

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[6] Arthur W. Baum, "They Live in a Glass House and Like It," *Saturday Evening Post* (19 August 1944), p. 26.

[7] W.W. Shaver, "War Problems for Which Financial Assistance from the Government Might be Justified," 2 September 1942 [DARM, box 9, folder "War Products (Priorities)"].

When new plants came on line and production requirements increased at old ones the company scrambled to find 7,000 new employees, 4,000 to staff new positions and the remainder to fill existing ones. Finding workers with appropriate skills was impossible. Many of the replacements and virtually all of those staffing new plants were women who had never had exposure to the typically all-male preserve of the glass industry, much less training in glass production techniques.[8]

In retrospect it is evident that there were cross-fertilization benefits that resulted from bringing in new people, especially in a company which had enjoyed the sort of employment stability that Corning had. Many of the recruits came from other major companies with greater or different production experience, others came from college and university engineering departments. However, at the time the loss of know-how at so many levels was keenly felt, and the benefits of outsider knowledge and new forms of expertise did not begin to compensate for the loss sustained. This was especially true because wartime contracts placed a much higher emphasis on high volume production than Corning had previously encountered. In view of the need to produce to tight military specifications, and to schedule, there was no chance to do the kind of work with new compositions where much of the company's research expertise had previously focused.

Lastly, and in some respects most painfully, the war necessitated unfortunate sacrifices of intellectual property:

> War needs have meant, too, that much of our know-how has been given to competitors or to other firms engaged in war activities. Examples of giving away methods to other manufacturers include formulae and manufacturing methods for radar, electric sealing, method [sic] of strengthening tumblers.[9]

Sullivan, who continued to direct Corning's labs throughout the war, was torn between satisfaction with the company's accomplishments and resentment at the government's power to disrupt his carefully planned research agenda. The military, he recalled,

> . . . insisted on Corning undertaking their glass problems, and these problems in many cases were such that it seemed almost fantastic to expect glass to meet them, yet in general some sort of solution was worked out. Glass bullets were an example.

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[8] Interview with John Munier conducted by Meg Graham, March 1997 (Winthrop Group transcripts).

[9] N.a., no title, n.d., p. 1 (DARM, box G-8, folder "Postwar Planning").

> Optical glass was forced upon us although we had never made a pound while others had been in the business for years.[10]

Sullivan was, of course, writing from the perspective of the lab, but it is clear that his sentiments were shared elsewhere in the company. Certainly it is the case that Corning, conscious of the mixed blessings of government contracts, decided to keep the military at arm's length following the war. This decision was represented and reinforced by Corning's hiring of Edward U. Condon as its research director in 1951.

Condon

Nowhere would the consequences of Corning's independent post-war stance, or the difficulties of maintaining it, be as clearly spelled out as in its experience employing this controversial new Director of Research and Development. Condon had spent the war on loan from Westinghouse to the government, first at MIT's Radiation Laboratory and then at the Manhattan Project. For a short time he had been Oppenheimer's second in command at Los Alamos. Like his more celebrated colleague, he had later been branded a security threat by rightist elements in the Congress. Following the war, Condon accepted the position of Director of the National Bureau of Standards. As an outspoken advocate of internationalism and the sharing of nuclear "secrets," Condon became the target of the House Un-American Activities Committee, which declared him "one of the weakest links in our atomic security" in 1948. Condon weathered this initial charge, but a second such attack decided him against further public service. He announced both his resignation from the NBS and his intention to join Corning on 10 August 1951. The atmosphere in the nation's capital had grown increasingly ugly since the 1948 atomic explosion in Russia, and Corning's offer suggested a welcome change of scene.

Corning, long accustomed to adopting government scientists and more than comfortable with mavericks, welcomed Condon with open arms. His experiences at the heart of the new military/scientific order, his standing in the research community, and his own background as a nuclear physicist all recommended him to the job. It was one he was to perform brilliantly. Condon swiftly demonstrated his ability to articulate a research philosophy to upper management, to represent and

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[10] E.C. Sullivan, no title, 4 May 1951 [DARM, box G-9, folder "War Products (Priorities)"]. Note that Sullivan is not being particularly careful with his language here; by any definition Corning had been engaged in optical glass production previous to World War II, though hardly extensively.

link Corning to the broader research environment, to analyze and react to developments in the political, scientific, and industrial spheres, and to monitor and contribute to the day-to-day work of his research colleagues. But though Corning treasured him, ultimately it was not able to provide him with a safe haven.

Condon had come to Corning with his governmental security clearance intact (HUAC's charges had been mostly innuendo; their "investigations" had never uncovered material sufficient to warrant revocation of Condon's clearance), but the company's management was well aware that their decision to hire Condon was at best a neutral one vis à vis the maintenance of military connections. Condon was a popular figure in the scientific community and had been recently elected President of the American Association for the Advancement of Science. Furthermore, President Truman had publicly exonerated him following HUAC's attacks, taking the opportunity to warn of the evil that irresponsible charges could do to a valuable reputation and career. Nevertheless, Condon was publicly skeptical of the growing Cold War hysteria and of the military's increasingly central role in American politics and the U.S. economy. This stance made him an enemy of the architects of the postwar military-industrial complex, and, potentially, Corning with him.

Two years after leaving the NBS, Condon lost his military security clearance automatically and, as Corning was involved with classified research, applied to have it reinstated. He was cleared for access by the Eastern Industrial Security Board in June of 1954, but when news of the EISB's action reached the Washington newspapers in October, 1954, the Secretary of the Navy personally revoked Condon's "Q" clearance. Vice President Richard Nixon claimed credit for the Secretary's action, which gives some sense of the forces aligned against Corning's chief scientist. Initially, Condon appeared ready to contest these charges as he had the others, but late in 1954 he declared that he was, after all, unwilling to fight this battle yet again.

The matter of Condon's revoked security clearance left Corning in an awkward spot: their Director of R&D was no longer able to direct, or even to know of, some of his own research projects. Though classified research was a small part of the company's total R&D activity, the position was clearly untenable. The issue was resolved by Condon's resignation—likely both his suggestion and decision—late in 1954.

Though Condon was with Corning for only a few years, those years were critical ones, and the policies that he established were maintained and strengthened by his hand-picked successor, William Armistead. Furthermore, though no longer Director of Research and Development, Condon remained a consultant with Corning for several decades, and continued to advise the company on a wide range of R&D-related decisions. His commitment to openness among researchers had been the source of his troubles; that same commitment was reflected in his management of Corning's R&D efforts, and was his most important legacy to the company.

Canton

Given then that Corning's World War II experience led the company to hold the military at arm's length, a decision institutionalized by its choice of Condon as head of its R&D program, it is hardly surprising to discover that Corning has not applied secrecy practices learned from postwar military contracts to commercial R&D efforts. In order to see this absence of links in practice, one must begin with the history of Corning's Canton plant, which since its construction in 1966 has housed most of the company's classified work, both R&D and production.[11]

In 1965 Corning received a sizable order for mirror blanks, one too large to meet with existing capacity. It was decided to construct a new plant and to dedicate it to this sort of work. Corning was at the time also involved in producing mirror optics for satellites, highly secret work that demanded its own set of security precautions, including a research and production area physically separated from the rest of the company. The construction of the Canton facility thus solved two problems at once: it supplied needed capacity and allowed the company to build an appropriately isolated working area from scratch.

The plant was intentionally sited in an isolated part of New York State, far to the north, bordering on the Adirondack State Park, and separated by several hours' drive from the main body of the company. The main justification for this rural location, however, was not enhanced security, but the fact that the fused silica production process used for this sort of work generated clouds of hydrochloric acid fumes. Construction began in 1966, and by 1969 all of the company's fused silica work had been shifted to Canton, along with most of its telescope mirror blank production.[12]

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[11] Material for this section was found in DARM. It has not, to date, been cataloged, but consists of a few brief chronologies for the Canton plant.

[12] Fused silica is a form of glass that exhibits virtually no expansion across a wide range of temperatures, and optical elements formed from fused silica will accordingly not distort images as the temperature of the observing chamber changes. It is, however, an extremely expensive material; most telescope blanks are made out of one of several other ultra-low-expansion glass

The plant's existence was guaranteed in 1973 when upper management decided to transfer most of the rest of the company's government contracts to Canton, including orders for radomes, aircraft windshields, and instrumentation tape reels. Later that year the plant obtained a multi-million dollar contract for the Space Shuttle windows.

This range of products suggests the depth of technical capability necessary to do this work, and it is no surprise, therefore, to learn that Corning's military production drew on a great deal of commercially developed know-how. Research knowledge transferred from the central labs to this northern outpost and, to a lesser extent, vice versa. Corning in the 1960s and 1970s was simply too small a company to be duplicating its own research; indeed, given that it was valuable to the military precisely because of its specialized knowledge it would have made no sense at all to establish divisions between its military and commercial work that prevented the one from benefitting from the other altogether.

Nevertheless, Canton did contain within it a secure cell, and if information made its way from that cell to the plant's other facilities it did so only in a highly partial, sporadic, and circumstantial fashion, a fashion quite in contrast to the flow of information within the company as a whole. Similarly, the transfer of knowledge from Canton to the rest of the company was inhibited by its physical and cultural isolation. Such knowledge would, of course, also include information concerning security practices themselves. The plant became an amalgam of Corning's corporate culture and Canton's local one, the one reinforcing the other in that most plant employees were already long native to the area.

Conclusion

While the example provided by Corning may be unique in its details, it is hardly unreplicated in its broader outline, and there are good theoretical reasons why this should be so. A great many fundamental differences exist between military and commercial secrecy at its most general. There are differences in what is at stake in each instance, in what the common default (secrecy or openness) is, and in when and how exceptions to that default may be made. Furthermore, in any company that creates a secure cell for military work, one will note important features in how that cell is managed and connected to the larger organization. A list of who has access to the cell and,

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formulae instead.

within it, to the work that is being done looks quite different than does a similar list of people who commonly have access to even the commercially secret parts of the main body of the organization. (See diagram, next page.)

But more important than any of these differences is the fact that secrecy practices are not self-contained protocols that work regardless of their context; to the contrary, the adoption of such practices necessitates an ancillary commitment to entire complexes of behavior, or, put another way, the assumption of a culture in which such practices can function efficiently and reliably. Corning's research tradition had long been opposed to secrecy of any sort within the corporate walls. The company's earliest R&D efforts were directed at setting aside individually-held craft secrets. Arthur Day had been an outspoken advocate of this program: "secret processes," he claimed, "are generally a cloak to cover ignorance rather than great wisdom." Glassmaking had been too long "dominated by secret formulas and tricks of personal experience which followed no law and formed a part of no system of generalization," and it was up to science, and Corning scientists in particular, to make that tacit knowledge explicit.[13]

From that day R&D at Corning has generally been carried out in an atmosphere of openness, one marked by persistent efforts by those managing research to encourage, and even to require the sharing of ideas, methods, and discoveries, both within the lab and between it and the shop floor workers, the patent lawyers, the marketing executives, and others. Information generated by the R&D process is routinely gathered, centralized, and redisseminated via internally circulated publications, regular lecture series, and occasional conferences; such practices have been the norm at Corning ever since its lab grew too large for daily contact to serve this unifying purpose (that is to say at least since the 1930s). This culture conflicts with a culture of research secrecy of the sort that contractual military work demands, and it is for this reason, as well as to satisfy military requirements, that Corning encapsulated and isolated such work at a remote facility in Canton.

One might comment at this point that a similar atmosphere of freedom characterizes some of the national laboratories performing highly classified work for the military (Los Alamos or Lawrence Livermore, for example), but note that the contact that companies like Corning have

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[13] Arthur L. Day, "Natural and Artificial Ceramic Products," *Bulletin of the American Ceramic Society* 13 (April 1934), p. 91.

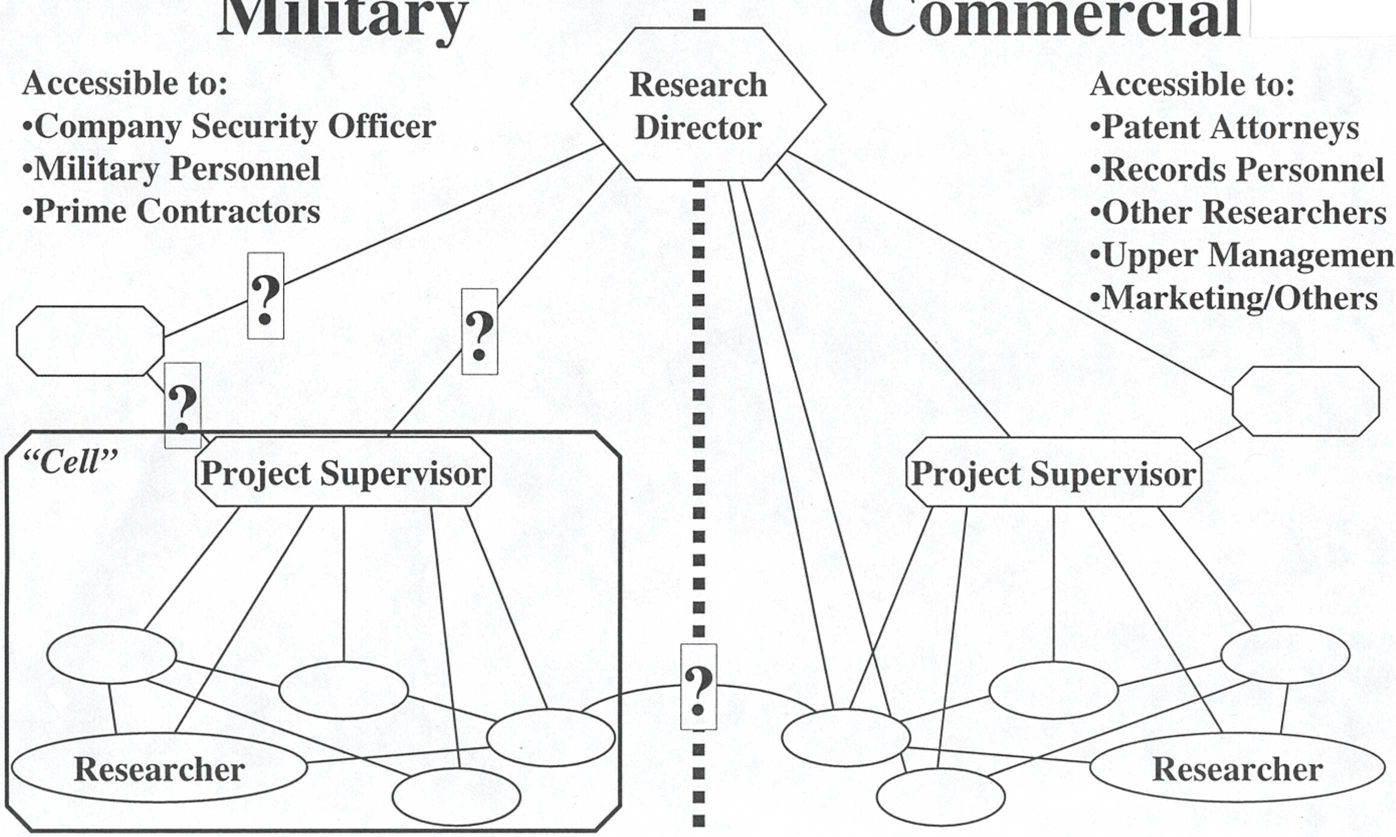
Organizational Structures

Military

Accessible to:
- Company Security Officer
- Military Personnel
- Prime Contractors

Commercial

Accessible to:
- Patent Attorneys
- Records Personnel
- Other Researchers
- Upper Management
- Marketing/Others



"Secrecy & Knowledge Production"

Cornell University, April 1998

with the military is not, as a rule, with the military's research centers, but with individual contract officers demanding adherence to certain protocols. This secrecy regime is likely all that such a company will ever learn of military practices of this sort, even if within military labs themselves a different secrecy regime exists. The protocols that the contract officer bears in hand are more easily encysted than absorbed, and the lessons learned in fulfilling those contracts, in the end, are likely to be ones of isolation not integration.

Postscript: Secrecy and the Scholar

The data upon which this paper is based are the product of an on-going research project between the Winthrop Group, Inc.—a company offering professional historical services—and Corning, Inc. which is focusing on the history of R&D at Corning. As one of the historians working on this project I have signed a non-disclosure agreement with the Winthrop Group that could potentially limit my ability to speak on this subject. In exchange, I have been given remarkably free access to Corning's archives, research centers, and personnel. This paper was written with the knowledge that Corning would have the final say as to what may be divulged in a factual sense, which lends a certain irony to the entire undertaking.

I mention this not so much as a warning—my material has not, in fact, caused the company any concern, though the reader should always suspect self-censorship under such circumstances, especially when confronted with a paucity of footnotes and a lack of specific details—but rather to highlight a meta-analytical problem that must at the very least provide a subtext for a conference of this nature. Some secrets lie beyond the analyst's reach, others may be discovered and published with impunity, and between these two extremes lie a very great many secrets that may be explored only partially, or perhaps solely, under certain restrictive conditions.

All scholars are familiar with the necessity of choosing what to say and what to leave silent. Such choices are commonly made in accordance with professional standards, personal taste, and the stylistic demands of the forum in which publication is sought; the knowledgeable reader will have some sense of what choices have been made and thus of what got cut but never again pasted. Secrecy requirements—whether a product of corporate non-disclosure agreements or military classification—force the scholar to make yet another set of such choices, but in this case the reader is far less likely to be able to reconstruct a more complete story. Lacunae may remain unbridged and unbridgeable by the reader, a fact that testifies to the tenacity of secrets.

GOOD FENCES MAKE GOOD NEIGHBORS: COOPERATION BETWEEN FIRMS AND PROPERTY RIGHTS IN JAPAN

W. Mark Fruin

In Robert Frost's turn-of-the-century day, Yankee ingenuity held that borders, boundaries, and fences were essential for managing human activity and private property. Without them, Frost believed that the desire to own property and to profit therein are frustrated. In *Mending Wall*, Frost asserts a primacy of property rights in relations characterized by specialized assets, writing:

> And on a day we meet to walk the line
> And set the wall between us once again,
> We keep the wall between us as we go.[1]

Today, Frost's commonsense runs counter to a groundswell of writing that trumpets an imminent arrival of borderless national economies and effortless interfirm cooperation. Property rights are rarely mentioned in this global call-to-arms although they are regarded as a *sine qua non* of economic development (North 1990). Japan's model of widespread cooperation between firms, especially among assemblers and suppliers in Toyota-like production systems, is a touchstone of this new age philosophy.

Japan's property rights regime is unusual because property rights are frequently not assigned to property owners and originators, thus contradicting Frost's "Good Fences" rule. Property rights embody the normal expectations and legal guarantees that encourage investment, without which economies will not grow, enterprises will not profit, and entrepreneurs will not take risks. Such expectations and guarantees are termed "good fences" in this essay.

Good fences require that the sources of good ideas are identifiable and that rewards accrue in proportion to value added. In short, property rights are needed for cooperation or, as Robert Frost puts it, "good fences make good neighbors." However, property can be appropriated and expropriated, thereby breaking the chain between property origination, ownership, and profit-making. In less developed economies where low levels of legal protection are associated with high

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[1] I am using the metaphor of fences for firm borders, and I admit to borrowing Robert Frost's mastery of the metaphor in "Mending Wall," *North of Boston* (New York: H. Holt & Co., 1915).

levels of appropriation and expropriation, property rights claims and the rents that flow from them are weakly supported. Identifying where good ideas come from and profiting from them are not one and the same thing (Teece 1986, 1998).

Japan's low legal protection and high appropriation of intellectual property is surprising, given Japan's high level of economic development and reputed status as a developmental model. Low property rights protection appears part and parcel of Japan's late industrialization when firms raced pell-mell to catch up to and surpass the leading firms of the West. Property rights were overlooked, neglected, and ignored in this long march toward industrialization, so much so that low property rights protection has become an institutionalized feature of Japan's economy and business system (Aoki 1988; Fruin 1992; Gerlach 1992; Odagiri 1992).

In partial response, firms clustered together to generate and protect property, and this introduces us to the appropriation part of the story. Within clusters, property rights are shared or at least made available as means for developing common practices and standards. Access to clusters and, thus, to property and community practices is tightly controlled. In this sense property rights are organizational, in that their recognition, protection, and promotion are group-based. This well describes the functioning of technology-based clusters, like Toyota Motor's group of companies. This essay, using examples from Toyota's and Toshiba's groups of companies, identifies distinctive features of Japan's property rights system and discusses various mechanisms that have arisen to recognize and assign property rights among Japanese firms.

Firm Boundaries and Property Rights in Japan

Japan industrialized late. Industrial catch-up required firms to focus on selective transfer of technology and to specialize by function and product. Without focus and specialization, the century to half-century gap in technical knowledge and production experience between Japan's fledgling firms and leading Western firms would not be bridged. Focus and specialization pushed firms into coalitions with other firms to secure needed resources outside their areas of concentration. Instead of internalizing resources and capabilities in ever-larger, Western-style, M-Form firms (Chandler 1962, 1990), widespread co-specialization of assets resulted in an institutional environment and property rights regime remarkably different from those of the West (Kester 1991; Fruin 1995; Gerlach 1992).

An undeveloped market for corporate control may be another consequence of Japan's late development. The relative infrequency of mergers and acquisitions prevents individuals from cashing in on their good ideas (Kester 1991; Gerlach 1992). Although business lines are often spun-off from core firms within business groups (*kigyo shudan* or *busunesu gurupu*), broad diversification by single firms is discouraged by the interlocking nature of groups (Fruin 1992). The group effect can be seen in R&D as well. Seventy-five percent of industrial R&D funding and 80 percent of R&D activity are undertaken by large private firms, all of which are enmeshed in business groups, so government-backed big-science and high-technology projects are relatively fewer than in the West (Fransman 1996).

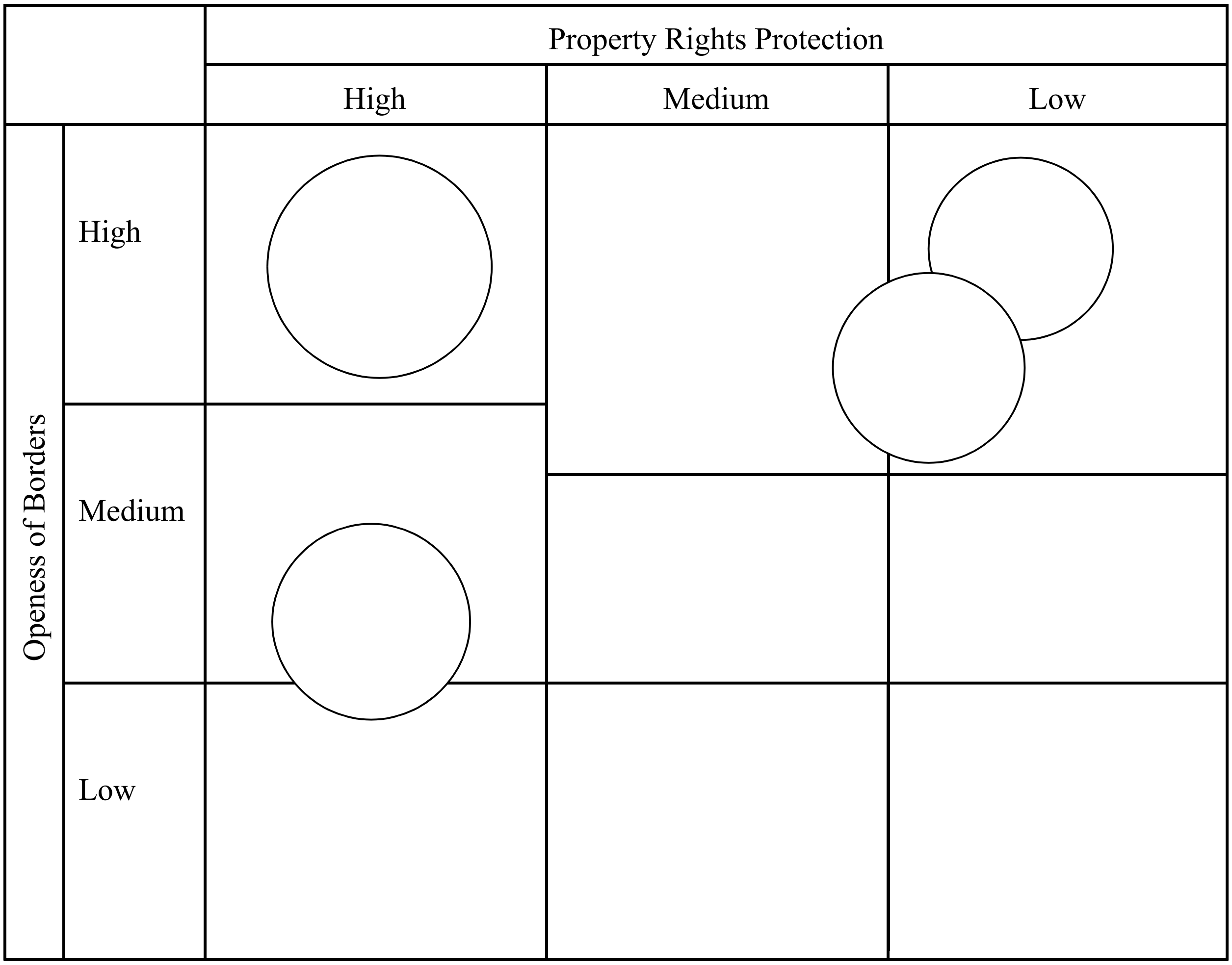
Long-term or relational contracting is prized, not unexpectedly in a country so geographically limited and demographically dense. Relational contracting assumes a degree of long-term, asset co-specialization on the part of transacting parties. In addition, the institutionalized personnel practices of large firms, such as lifetime employment and seniority-weighted reward, encourage low levels of occupational mobility and turnover (Abegglen 1958). Given the closed market for corporate control and the relatively low levels of government R&D funding, firms either generate intellectual property themselves or create circumstances that allow for co-generation and sharing of intellectual property. Such circumstances are seemingly threefold: first, interfirm relations are punctuated by long-term reciprocity; second, job-hopping between firms is constrained; third, unfriendly efforts to take over proprietary resources are eschewed.

All of which contributes to the distinctiveness of Japan's property rights regime. Valuable ideas are mostly generated and paid for privately, either by single firms or by firms clustered in groups, alliances, and coalitions. Within groupings, firms cooperate frequently without too much concern for property rights. The Toyota Production System (TPS) is based on a combination of open firm borders and low levels of property rights protection with other Toyota group members, as depicted Figure 1.

Toyota's lean production model relies on transaction-specific as opposed to residual property rights. Transaction-specific property rights (TSPR) are a means of recognizing the value of intellectual property in transfer pricing between firms. TSPR parcel out rewards and benefits on a transaction-by-transaction basis. In most cases, this is a straight, fixed cost calculation: x number of people times y number of hours. However, another part of the reward is more directly tied to the firm-specific, self-developed technologies of suppliers. Fair valuation of this variable cost is a function of assembler-supplier experience and interfirm governance arrangements, as illustrated in the following auto and electronics industry examples (Clark and Fujimoto 1991; Fruin and Nishiguchi 1994; Fruin 1998a).

Figure 7.1

Open Borders and Property Rights Protection



The Auto Industry and Property Rights in Japan

The excellent work of Kim Clark and Takahiro Fujimoto on product development performance in Japan's motor vehicle industry finds that effective product development hinges in large measure on what they call "heavyweight product managers" and "heavyweight product development organizations" (Clark and Fujimoto 1991). Heavyweight means organizations are well led and provisioned, sitting amidst divisionalized and matricized firms.

What is fenced-in is more important than what is not because speed, quality, and efficiency are *interdependent qualities* of effective product development. Fencing in projects with sufficient authority, autonomy, and team-specific assets is critical because if these are borrowed extensively from "under the table" or "over the wall," speed, quality, and efficiency suffer. Also fencing in avoids resource "hold-up" and allows for integrated problem solving, multifunctional coordination, and intensive product/process communications. Everything and everyone necessary to product development effectiveness are closely coupled.

Not fencing in key resources at the start or along the way blurs functional, technical and organizational requisites of product development effectiveness. Overlap of functions is more easily achieved *within* product development organizations, and in this sense intramural coordination is different in any number of ways from interdepartmental coordination. The former economize on time and resources by creating project teams that are typically small, polyvalent, and well experienced while the latter often aggregate functional specialists without emphasizing their previous experience and time-to-market performance.[2] Intramural and interdepartmental coordination suggest different strategies: one of self-contained tasks in the former and good lateral relations in the latter (Galbraith 1974).

Project Team Size

Clark and Fujimoto find that—beyond a certain critical mass—the larger the team, the lower product development performance. Size seems related to bureaucratization (DiMaggio and Powell 1983) and to problems of appropriation because the larger the team, the more sharing of value added is problematic. The sharing of value added is critical because in Toyota's case, Japanese auto components, sub-assemblies and sub-systems are about 70 percent "black box" parts. Black box parts are where suppliers provide parts and components of their own proprietary design to meet assemblers' functional requirements. That is, about 70 percent of the parts,

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[2] My emphasis on product and production specialization as opposed to functional specialization differs from other accounts. See Paul Adler (1991).

components, and sub-assemblies that go into Toyota cars are based on suppliers' self-developed technologies.

The high proportion of parts, components, sub-assemblies, and sub-systems that are "black box" means that the function, performance, and integrity of Toyota's lean production system depend heavily on supplier capabilities. The high reliance mirrors both the comparatively high systems engineering capabilities of suppliers *and* the quality of transactional relations imbuing supplier networks (Clark and Fujimoto 1991). Asanuma calls these "relation-specific" skills (Asanuma 1989).

Such capabilities and relations are embedded in frequent communications and information exchange across firm borders. These depend on good fences because a Denso employee (Denso is one of Toyota's largest, most critical, and independent suppliers), no matter how much time and effort s/he expends on behalf of Toyota, is a Denso employee. Long-term or "lifetime" employment is the rule in large firms and compensation is heavily weighted in favor of experience coupled with individual contributions to firm welfare. Because such employment practices are coupled with firm-specific internal labor markets, borders between firms can be open and intellectual property freely traded.

Product development team members benefit more from continuing their current employment than they do by seeking greener pastures elsewhere. This helps explain the high ratio of black box parts (newly designed parts compared to in-house engineering) and Aoki's characterization of the purchasing manager's role in auto assembly firms (Aoki 1986). Or, in the words of this essay, good fences make good neighbors. Expectations of reward are tied to transaction-specific property rights and personnel policies of long-term employment and seniority-based compensation.

The Electronics Industry and Property Rights in Japan

Autos and electronic goods are quite different products. In Japan's motor vehicle industry product development projects are relatively long-lived, some forty odd months or so. But forty-month long development cycles are unheard of in the electronics industry, except for heavy power generation and transmission equipment segments of the industry (Fruin 1998b).

Industry-specific features may frustrate a simple carry-over of lean production and heavyweight product development models from the auto industry. A wide reading of the literature and months of fieldwork investigation and observation suggest that two variables and not one are important in electronics. First, product development and product life cycles have to be appraised. These are positively correlated in Clark and Fujimoto's study, meaning the shorter the life cycle, the shorter the product development cycle.

Second, the degree of *intergenerational* differences between products must be assayed. Intergenerational product differences are typically not significant in the case of the autos and in some electronic industry products. For example, at Toshiba's main photocopier (PPC) factory in Japan, PPC development activities amount to little more than a kind of set-aside from normal production activities (Fruin 1997). However, in the case of laser printers (LP) at the same plant, technical discontinuity between generations is sufficiently great that design engineers re-consider LP systems and sub-systems in terms of functions, features, performance, design for manufacturability, and operating costs. They also review hardware and software specifications in light of the latest microchip and semiconductor devices on the market.

For LP development projects, much like heavyweight development projects in the auto industry, projects are well stocked with their own resources and raison d'etre. If most electronic products are like LPS rather than PPCs, heavyweight product development organizations may be needed when either product development cycles are short or intergenerational product differences great.

The autonomy of product development organizations in the electronics industry, however, hinges on two additional variables: organizational slack and the breadth and depth of team members' skills. When resources are not fully committed within firms, and hence slack resources are available, openness between development projects and the rest of the firm allows for an easy pass through of resources. When project assets are borrowed in this way and as long as problems of appropriation do not arise, the authority of product development managers is not diminished by relying on resources outside of his/her control.

Borrowing resources is likely under three conditions: first, when property rights disputes are unlikely, the best available know-how and knowledge within a firm, its affiliates, and suppliers will be secured; second, when development skills are highly specialized, they cannot be easily substituted for and hence resources outside the immediate control of development projects may be sought; third, when development cycles are short, employees are more easily loanable. Along these lines, Clark and Fujimoto report that product development organizations in North America and Western Europe are typically more specialized in design and engineering skills and that product development cycles are typically longer (Clark and Fujimoto 1991). Or, larger-sized product development teams are the rule when product development teams are more highly specialized. Larger and more specialized teams result in longer product development cycles and, significantly for our purposes, more property rights claims.

But in Japan, team polyvalence, *kaizen* activities, and an emphasis on minimizing organizational slack may keep a lid on team size. Teams make up in breadth what they lack in depth. Development teams are not large, several dozens at best, although some recent research has emphasized the importance of redundancy or slack in product development activities (Imai, Nonaka, and Takeuchi 1985; Nonaka and Takeuchi 1995).[3] If resources can be borrowed easily, breadth can be finessed by openness. However, the effectiveness of this solution depends on the quality of cooperation in the technology transfer processes among firms in the same group.

Presumably this is why assemblers rank the quality of their relations with suppliers as highly as the quality of supplied parts and components in Japan (Asanuma 1989; Fruin 1998a). Where relations are good, resources are available and loanable, and property rights claims are not generally prosecuted. The degree of cooperation within groups also hinges on the degree of intergenerational product differences and the length of product development cycles. Where intergenerational differences are low and development cycles long, borrowing is an alternative to stockpiling resources (Clark and Fujimoto 1991). In fact, smaller numbers of more widely skilled development team members are preferable, given transaction-specific property rights and lifetime employment norms (Hashimoto 1979; Fruin 1997). But ultimately, this hinges on the nature of the property rights regime and cooperation within business groups.

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[3] Japan's firms are reputedly better at interorganizational coordination, yet a top American executive told me, "no matter how I spoke to my Japanese counterparts about 'synchronous engineering,' they could not understand my point. Either they were feigning ignorance or the problem was not framed sensibly to them." Conversation with J.B. in Fontainebleau, France on May 25, 1991.

But practices like synchronous engineering are not exclusively Japanese. As Mr. William Reed, President of Semiconductor Equipment & Materials International wrote to Senator Bensen: "I find it plausible that in some cases suppliers will work more closely with their local customers in the development stages of their equipment. This practice is understandable, given proximity, cultural similarities and traditional customer/vendor relationships. An increasing number of US equipment suppliers are working closely with their American customers," *Financial Times*, 17 May 1991: 18.

Cooperation and Property Rights: Japan versus Silicon Valley

The necessity of interfirm coordination and cooperation are acute today. Product numbers and varieties are growing as product life cycles are shortening. It is increasingly difficult for single firms to manage the development, production, and distribution of complex products worldwide. As a result, cooperation between firms is growing.

Models of interfirm cooperation differ significantly in the degree to which they recognize and protect property rights. Japan and Silicon Valley are quite different in these respects. Japan's firms are highly praised for a strong learning orientation based on open borders with stakeholders, such as suppliers, labor unions, group (*keiretsu*) affiliates, and banks. Learning and cooperation in Japan's case centers around a core firm or firms clustered in a well defined business group. Defining which firms are in and which are not underlies learning and cooperation in Japan. Perhaps for this reason Japan's firms are hardly ever touted for spillover effects or, as we have seen, property rights protection (Cole 1989; Porter 1990; Fruin and Nishiguchi 1993; Liker et al. 1999).[4]

Two explanations for the cooperation of Japan's firms have been offered. The first, more or less a cultural explanation, relies on trading experience, proximity, and transactional frequency to build up "trust" between parties. A business ethic infused with "goodwill" rather than "opportunism" is the result (Dore 1983; Fruin 1983; Williamson 1985). The second comes from classical game theory, especially non-cooperative Nash equilibrium games, in which neither player (company) is motivated to change, nor agrees not to change (Morrow 1994). Such games allow for intensive information exchange and emergent norms of fair governance that are like the non-contractual, co-specialized activities of Toyota assemblers and suppliers (Womack et al. 1990; Clark and Fujimoto 1991). In either case, cooperation is particularistic, not a general outcome.

In Silicon Valley cooperation between firms brings not only firm profits but also regional prosperity with spillover effects (Piore and Sabel 1984; Helpman and Krugman 1985; Porter 1990;

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[4] There are parallels between Toyota's system of knowledge production and the defense contracting system in the United States. Both are closed systems and, as a result, knowledge spillover benefits are limited. Both have strong supplier/contractor qualifying requirements, which are more stringent than public standards, and in general decisions are made in-house without reference to public standards. Recently the Department of Defense has engaged in a serious effort to move to commercial standards in order to remove some of the barriers inhibiting product and knowledge flows between the two sectors, but thousands of military-specific standards and requirements remain.

Saxenian 1994). Cooperation is based on open standards combined with strong property rights protection. While the costs of legal protection are high, the benefits of cooperation coupled with strong property rights protection go far in explaining Silicon Valley's wealth-generating cornucopia. However, it is worth noting that this combination may be more exceptional than normative, even in the United States (Saxenian 1994; Bratton 1989).

Japan and Silicon Valley's models of cooperation are very different. Toyota's lean production model requires co-evolutionary experience as a prerequisite for the tight cooperation that involves sharing co-specialized and proprietary information. Silicon Valley networks are less particularistic, with low entry costs for joining transactional networks but with substantial legal and opportunity costs associated with living and working in the San Francisco Bay Region. (Perhaps these costs help explain why Silicon Valley is still a regional, and not a national, model). Cooperation is concerned with the setting of open standards, such as Sun Microsystem's Java software that works with almost any hardware and operating system.

Japan's cooperation is particularistic with a business group while Silicon Valley's is more general, such as setting industry standards. For such reasons, Toyota's lean production system and associated property rights pivot on three elements:

1) flexible transfer prices between assemblers and suppliers;
2) multilateral exchange of know-how mediated by governance arrangements;
3) pay for performance in rewarding project team members.

These interactions and behaviors occur within a particular group of companies, and as such, they are a recognition and adaptation of the universality of Frost's property rights concerns. But the rule of law, especially as it applies to property rights protection, is institutionally embedded in business practices that cut across industries in Japan—witness the similarity in Toyota and Toshiba's assembler-supplier practices.

Good fences make good neighbors, even in Toyota's and Toshiba's worlds, by allowing for adjustments in how property ownership is recognized and rewarded, thereby minimizing transaction costs in spite of high levels of interfirm resource dependency. Expectations of reward based on suppliers' product/process innovations are built-into human resource policies, and research suggests that such expectations powerfully affect productivity and innovation (DeAlessi 1983; Rosenberg and Frischtak 1983). The payoffs for cooperation are great in spite of a property rights regime characterized by high appropriation and low protection.

Black-box suppliers, who supply most of the intellectual property generated with the Toyota Production System, are especially unlikely to defect. They enjoy a privileged position as "systems suppliers," providing entire, integrated solutions to Toyota Motor. They often control the flows of proprietary technology and are in strong positions to be well paid for their contributions in the form of transaction-specific property rights, goodwill transfer prices, and flexible wage payments. As a result, transaction costs, based in part on transactional frequency and on multilateral bargaining arrangements in supplier associations, appear low (Williamson 1985; Nishiguchi 1995; Dyer 1998; Fruin 1998a).

Toyota's lean production system arose in a particular historical and institutional setting when, at first, Japan needed to catch-up and more recently when, for the most part, applied research and knowledge creation were generated privately within firm clusters. Such circumstances were part and parcel of Japan's late industrialization as are other features of Japan's institutional environment, such as low labor mobility, interfirm governance arrangements (like supplier associations), and the absence of a market for corporate control. The rule of law, especially property rights law, did not develop as an independent feature of the institutional environment outside the rough and rumble of corporate practice.

For such reasons, the nature of cooperation between firms in Japan is distinctive, and the institutional features that underlie cooperation there are unlikely to be repeated elsewhere. The singularity of Japan's institutional response among advanced industrial economies seems likely to hinder the worldwide spread of Japanese production and product development systems. So global best practices, like the Toyota Production System, are not necessarily global because they cannot be transferred intact. Adjustments, adaptations, and transformations are to be expected (Liker et al. 1999).

There are advocates for the worldwide spread of Japanese production and development systems without significant changes (Womack et al. 1991; Nishiguchi 1995). However, our own view is that the transfer of technology and resources between countries, even within the same company, always requires some degree of adaptation and change. In this respect, Silicon Valley rather than Toyota's lean production model appears to "have legs" or greater international currency. The Silicon Valley model is grounded in a rule of law where property rights are not dependent on particularized institutional or organizations settings, as in Japan's clustered model. Given that property rights are publicly acknowledged and enforced in the Silicon Valley model, switching, monitoring and enforcement costs are lower than would otherwise be the case (Bratton 1989). The rule of law in this public sense appears to be a more efficient institutional setting for property rights development and protection than Japan's closed corporate system.

Japan's national strategy of late industrialization and low property rights protection is unlikely to be repeated elsewhere, at least not in the same ways that it unfolded in Japan. Today, property rights protection is a widely recognized condition, if not precondition, of international technology transfer and economic development. Laws and institutions that facilitate the creation of common standards across corporate, national, and international borders are the norm. They seemingly excel at eliciting and ensuring cooperation.

Robert Frost was actually ambivalent about fences, walls, and boundaries. He celebrated them as indispensable for good relations but decried them as contrary to the human spirit. "Something there is that doesn't love a wall," he lamented, even while tumbled stones and fallen fruit seem to make good boundaries a neighborly necessity. Without fences, what's mine and yours are unclear. Ambiguity in property rights as in most everything else makes for poor performance.

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VARIETIES OF SECRETS AND SECRET VARIETIES: THE CASE OF BIOTECHNOLOGY

Susan Wright and David A. Wallace

Secrets do not develop in a social vacuum. Rather, the construction of a web of secrecy is a social process that defines relationships between those inside and those outside the web, the conditions under secrets are wholly or partially revealed, and the conditions of access and denial. Probably more often than not, those conditions are formed and perpetuated through extended overt or covert political conflict. To fully understand the social construction of secrets, we must ask how these relations are formed and by whom, how contests of secrecy develop, by what means, in what settings, with what effects.

The evolution of biotechnology is particularly interesting in this respect because its origins were remarkably transparent. The field evolved from what was once a purely academic discipline, molecular biology. Although actual behavior of individual scientists did not always measure up to the traditional norms of scientific inquiry, nevertheless, those norms were influential, supporting not only the (more-or-less) free exchange of research results but also broad public discussion of the social implications of the field.

After the commercial potential of genetic engineering, gene sequencing, and other techniques that provided the basis for biotechnology became apparent in the late 1970s, however, several developments combined to veil the new field in secrecy: first, the transformation of biotechnology from a field with largely academic connections to one with strong corporate connections; second, the U.S. Supreme Court's establishment, in *Diamond v. Chakrabarty*, of intellectual property rights for life forms and the subsequent increase in secrecy within academic biotechnology research; third, the limiting of public access to information concerning controls for research and development in genetic engineering.

The first three parts of this paper examine these developments and the ways in which they have supported the formation of a new norm of secrecy for biotechnology. The fourth and final part addresses the implications of secrecy in the biotechnology industry for an important area of public policy, namely, the present negotiations aimed at strengthening the 1972 Biological Weapons Convention through measures designed to increase confidence in compliance.

The Social Transformation of Biotechnology

The early development of genetic engineering (a key technique of biotechnology) is unusual for a new technology because it took place in sites to which the public had considerable access—university research laboratories supported by government grants. As a result, the interests and goals of genetic engineering's pioneers—Peter Lobban, the graduate student at Stanford University who was the first to conceive of a form of genetic engineering that worked effectively, Paul Berg, Stanley Cohen, Herbert Boyer, and Robert Helling—are known through documents that are public, such as a thesis proposal, grant proposals to the National Institutes of Health, and a proposal to the University of Michigan for a sabbatical.[1]

This norm of transparency continued for some years as development of the techniques of genetic engineering proceeded. One expression of the persistence of traditional academic norms of research was the willingness of leading researchers to present their proposals for future research to the committee appointed by the National Institutes of Health to advise on possible hazards of genetic engineering. Detailed protocols specifying the genes to be transferred, the means for transferring them, and the recipient organisms were widely circulated not only to peers in the field but also to the larger public.[2]

At the same time, industrial applications were widely anticipated and efforts were pursued to demonstrate the potential for using genetic engineering as the basis for a new industry in which microbes would be used as "factories" for making novel proteins. By 1976, two genetic engineering companies—Cetus and Genentech—were starting up and embracing a vision of a commercial future for gene splicing. "We are proposing to create an entire new industry, with the ambitious aim of manufacturing a vast and important spectrum of wholly new microbial products using industrial micro-organisms," proclaimed a Cetus report circulated to potential investors in 1975.[3] That this vision was not entirely an effect of public relations hype is suggested by other events in

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[1] Susan Wright, *Molecular Politics: Developing American and British Regulatory Policy for Genetic Engineering, 1972-1982* (Chicago: University of Chicago Press, 1994), ch. 2.

[2] These practices continued until the late 1970s when controls for genetic engineering were progressively weakened. By 1982, the responsibility for most decisions on genetic engineering precautions was delegated to local biosafety committees and public circulation of protocols for new research was, therefore, restricted: see Wright, *Molecular Politics*, chs. 9-10.

[3] Cetus Corporation, "Special Report," (unpub. c.1975).

this period. Stanford University applied for a patent for the method of inserting foreign DNA into a bacterium developed by two of the pioneers of the field.[4] And by the fall of 1976, at least six transnational corporations—Hoffman-La Roche, Upjohn, Eli Lilly, SmithKline, Merck, and Miles Laboratories—had initiated small research programs in genetic engineering.[5]

Nevertheless, at this stage, industrial investments in the field were small. While the pharmaceutical industry was certainly alert to the potential of the new field, a key technique of genetic engineering was missing. From an industry standpoint, it was not enough to be able to transfer DNA from a higher organism into a bacterium. In addition, it was deemed essential that the foreign DNA could reprogram the bacteria to synthesize the products encoded by the DNA. As late as the mid-1970s, it was not clear that this was feasible.[6] Consequently, investors were wary. Conceivably, Cetus's vision could turn out to be nothing but hype. In any case, for the moment, large corporations were content to watch developments in the universities and start-up companies like Cetus from the side-lines.[7]

A turning point in industry perceptions of genetic engineering occurred in the fall of 1977 when Herbert Boyer at the University of California, San Francisco and vice-president for research at Genentech and Keiichi Itakura at the City of Hope Medical Center in Duarte, California, demonstrated that the DNA encoding a small human brain hormone could be used to program bacteria to make the hormone.[8]

This achievement, proclaimed by the president of the National Academy of Sciences as "a scientific triumph of the first order," was announced at a congressional hearing and attended by substantial publicity. From that point on, the technique was used repeatedly to demonstrate the bacterial synthesis of insulin, growth hormone, interferon, and other proteins normally made only by higher organisms. The trickle of investments in genetic engineering turned into a torrent as ven-

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[4] U.S. patent no. 4,237,224, granted to Stanley Cohen and Herbert Boyer and assigned to Stanford University, December 1980.

[5] Nicholas Wade, "Guidelines Extended but EPA Balks," *Science* 194 (1976): 304.

[6] J. Atkins, "Expression of a Eucaryotic Gene in *Escherichia coli*," *Nature* 262 (1976): 256-57.

[7] For details, see Wright, *Molecular Politics*, p. 83.

[8] Keiichi Itakura, "Expression in *Escherichia coli* of a Chemically Synthesized Gene for the Hormone Somatostatin," *Science* 198 (1977): 1056-63.

ture capitalists and transnational corporations raced to position themselves in the field. The transformation of genetic engineering from an area of academic research to an industrial technology was under way. Investments climbed steeply after 1977. By 1980, equity investments in small genetic engineering firms had reached $600 million. They would grow even more rapidly as front runners like Genentech and Cetus entered the stock market in the early 1980s.[9]

Start-up genetic engineering companies moved quickly to lure scientists from universities with competitive salaries and stock options. Transnational corporations began to complement their investments in start-up firms with investments in university research. Between 1981 and 1982 alone, they invested some $250 million in biological research in universities and research institutes. These investments were supported by a most congenial economic and political climate shaped by legislation passed by the Carter and Reagan administrations that fostered university-industry cooperation, provided substantial tax credits for research and development, and allowed universities and small businesses rights to patents arising from federally supported research.[10]

The torrent of investments in genetic engineering from the late 1970s onwards encouraged practitioners to form a variety of new affiliations with the private sector. Scientists, formerly cloistered in academe, became equity owners, corporate executives, members of scientific advisory boards, and industry consultants. By the early 1980s, it was said to be difficult to find a genetic engineer who did not have a corporate connection.

Considerable evidence shows that these roles introduced new norms for the practice of science. Following the Supreme Court decision on *Diamond vs. Chakrabarty* in 1980 (see below), the interest of genetic engineering firms and transnational corporations in securing patent coverage for their inventions produced confidentiality arrangements under which employees agreed not to disclose proprietary information or share materials. The start-up Biogen informed investors in 1983 that “in its relations with universities, Biogen seeks to maintain the maximum degree of openness consistent with reasonable protection of proprietary information,” and the company also noted that “trade secrets and confidential know-how may be important to Biogen’s scientific and commercial

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[9] For details, see Wright, *Molecular Politics*, pp. 83-105.

[10] David Dickson and David Noble, “By Force of Reason,” in Thomas Ferguson and Joel Rogers, eds., *The Hidden Election: Politics and Economics in the 1980 Presidential Election Campaign* (New York: Pantheon, 1981), pp. 260-312.

success."[11] Universities implicitly supported this new norm by encouraging researchers to seek patent protection for their results. Symptomatic of these changes were the contradictions that began to embroil university research and teaching from the late 1970s onwards. Complaints of researchers' unwillingness to share ideas and materials were aired. As genetic engineering pioneer Paul Berg, himself a member of the scientific advisory board to the company DNAX, told *Newsweek* in 1979: "No longer do you have this free flow of ideas. You go to scientific meetings and people whisper to each other about their companies' products. It's like a secret society."[12] Legal struggles over ownership of cell lines flared up. While some universities issued guidelines to minimize conflicts of interest, these measures neither hindered the formation of corporate links with university research nor affected the basic conditions under which these links were formed. As Donald Kennedy, president of Stanford University summarized the social relations of molecular biology and its commercial offspring in 1980: "What is surprising and unique in the annals of scientific innovation so far is the extent to which the commercial push involves the scientists who are themselves responsible for the basic discoveries—and often the academic institutions to which they belong."[13]

In the 1980s a survey of university-industry research relationships in biotechnology by researchers at Harvard University confirmed what a growing body of anecdotal evidence suggested: that corporate linkages in biotechnology were growing and that these linkages were affecting the norms and practices of research in this field.[14] Most notable was the extent of the practice of secrecy of biotechnology, not only in corporations but also in universities. In 1986 the Harvard researchers concluded that "biotechnology faculty with industry support were four times as likely as other biotechnology faculty to report that trade secrets had resulted from their university research." Furthermore, 68 percent of biotechnology faculty who did not receive industry

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[11] Biogen, N.V., *Prospectus* (October 14, 1982): 14.

[12] Paul Berg, quoted in Sharon Begley, "The DNA Industry," *Newsweek* (20 August 1979): 53.

[13] Donald Kennedy, "Health Research: Can Utility and Quality Co-exist?" Speech given at the University of Pennsylvania, December 1980.

[14] David Blumenthal et al., "Industrial Support of University Research in Biotechnology," *Science* 231 (17 January 1986): 242-46; David Blumenthal et al., "University-Industry Research Relationships in Biotechnology: Implications for the University," *Science* 232 (13 June 1986): 1361-66.

support and 44 percent of those who did considered that university-industry linkages ran a risk of undermining intellectual exchange and cooperation.[15] Follow-up studies in the 1990s indicated that secrecy in this field continued to grow.[16]

If the extent of the industry linkages with university researchers was low, such results might be of minor interest. However, a further study by researchers at Tufts University in 1985-88 demonstrated that the percentage of faculty members with industry affiliations in university departments pursuing research in areas related to biotechnology was high, peaking at 31 percent for MIT's department of biology.[17] Taken together, the Harvard and Tufts studies indicate a major shift in the social relations of biotechnology, specifically, the formation of strong linkages between academic research in biotechnology and industry. The significance of this shift is discussed further in the following section.

The Establishment and Impact of Intellectual Property Rights for Life Forms

Despite claims that the issue of patenting life is solely one of law and technology, it also invokes a deep interplay of economics, social values, and access to information.[18] In 1980 the U.S. Supreme Court very narrowly (5-4) ruled in Diamond v. Chakrabarty that a patent could be obtained under section 101 of the U.S. patent law for a laboratory-created genetically engineered bacterium—that a "live, human made micro-organism is patentable . . . [as it] constitutes a 'manu-

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[15] Blumenthal et al., "University-Industry Research Relationships," p. 1364.

[16] David Blumenthal et al., "Participation of Life Science Faculty in Research Relationships with Industry," *New England Journal of Medicine* 335, 23 (5 December 1996): 1734-39; David Blumenthal et al., "Relationships Between Academic Institutions and Industry in the Life Sciences—An Industry Survey," *New England Journal of Medicine* 334, 6 (8 February 1996): 368-73; David Blumenthal et al., "Withholding Research Results in Academic Life Science: Evidence from a National Survey of Faculty," *Journal of the American Medical Association* 277, 15 (16 April 1997): 1224-28.

[17] Sheldon Krimsky et al., "Academic-Corporate Ties in Biotechnology: A Quantitative Study," *Science, Technology, and Human Values* 16, 3 (Summer 1991): 275-86.

[18] For a discussion of some of these issues see: Daniel J. Kevles, "Ananda Chakrabarty Wins a Patent: Biotechnology, Law, and Society, 1972-80," *HSPS: Historical Studies in the Physical and Biological Sciences* 25, 1 (1994): 111-36.

facture' or 'composition of matter."[19] The court argued here that the genetically engineered bacterium under dispute qualified for patent protection as it was not "nature's handiwork" which produced the organism, but rather it was a "non-naturally occurring . . . product of human ingenuity," which fell within the wide scope of patentability contemplated by the Congress.[20] Prior to this decision all that could have been obtained was a patent for the process that used the microorganism but for not the organism itself, the established norm at the time being that life was not patentable.

The Court received ten amicus curiae briefs in advance of their decision on this case—nine in favor of the patent and one opposed. A sample of four of these briefs [three pro-patent: Pharmaceutical Manufacturers Association (PMA), Genentech, Inc., and the American Society for Microbiology (ASM); and one anti-patent: The Peoples Business Commission (PBC)] reveals alternative perspectives on the patent's consequences for openness of information.

Pro-patent briefs argued that patents would increase public knowledge and the exchange of scientific information because the Patent Act was in part an information disclosure statute.[21] Meeting the public reporting requirements for biotechnological inventions, however, is more complex than for other types of patents. Microorganisms and other patentable life forms cannot always be adequately represented by written documents alone. To ameliorate this potentially negative consequence of patented biological entities, one pro-patent brief argued that the depositing of organisms within authorized national culture repositories would help satisfy U.S. Patent and Trademark Office public reporting requirements.[22]

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[19] Diamond v. Chakrabarty, 447 U.S. Slip Opinion, pp. I-II. (1980). Inventions patentable under 35 U.S.C. 35 § 101 include discoveries of any "new and useful process, machine, manufacture, or composition of matter, or any new and useful improvement thereof. . . ."

[20] Diamond v. Chakrabarty, 447 U.S. Slip Opinion, pp. 4-7.

[21] 35 U.S.C. § 112 states that the patent "specification shall contain a written description of the invention, and of the manner and process of making and using it, in such full, clear, concise, and exact terms as to enable any person skilled in the art to which it pertains, or with which it is most nearly connected, to make and use the same, and shall set forth the best mode contemplated by the inventor of carrying out his invention."

[22] Diamond v. Chakrabarty, Brief on Behalf of the American Society for Microbiology, Amicus Curiae, n.d., pp. 9-11.

More specifically to the point of secrecy, two pro-patent briefs claimed that in the absence of patent protection commercialization of biotechnological inventions would instead be shielded by trade secrecy, which had no public reporting requirement.[23] The anti-patent brief argued that the quest for patent rights to life forms had already inhibited the creation of Federal safety standards to regulate genetic engineering experimentation and implied that oversight of any such standards would be further hampered by corporate claims of protection of proprietary information.[24]

The degree to which patenting life invoked a public interest produced an interesting split in the Supreme Court's thinking at that time. The majority stated that the public interest was not an issue appropriately related to the *legal* question on whether microorganisms were patentable. They argued that the Court was not the proper arena for challenging the patentability of life forms on the grounds that genetically altered life forms posed "potential hazards."[25] The dissenting minority held the opposite position. They believed that in this instance it was Congress's and not the Court's role to determine "whether and how far to extend the patent privilege into areas where the common understanding has been that patents are not available." This was deemed especially so when the subject "uniquely implicates matters of public concern."[26]

The decade following the Court's ruling saw a broad expansion of the scope of patentable subject matter. By 1987, PTO considered "nonnaturally occurring nonhuman multicellular living organisms, including animals, to be patentable subject matter."[27] Currently, patentable subject matter includes natural, recombinant and synthetic genes and other DNA, cells and cell lines, gene and

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[23] Diamond v. Chakrabarty, Brief on Behalf of Genentech, Inc., Amicus Curiae, January 23, 1980, pp. 5-6, 14-15; Diamond v. Chakrabarty, Brief on Behalf of the Pharmaceutical Manufacturers Association, Amicus Curiae, n.d., pp. 12-14.

[24] Parker v. Bergy and Parker v. Chakrabarty, Brief on Behalf of the Peoples Business Commission, Amicus Curiae, December 13, 1979, p. 20.

[25] Diamond v. Chakrabarty, 447 U.S. Slip Opinion, pp. 2, 11. (1980).

[26] Diamond v. Chakrabarty, 447 U.S. Dissent, pp. 1-2, 4 (1980).

[27] U.S. Congress. Office of Technology Assessment, *New Developments in Biotechnology: Patenting Life—Special Report* (1989), p. 93.

cell products like proteins and antibodies, as well as novel and preexisting biological "agents" such as plants and animals, and specific parts of plants and animals.[28]

Since the Court's decision in 1979, the growth and scope of the biotechnology industry has been impressive. At present, there are nearly 1,300 biotechnology companies in the U.S., employing over 150,000 workers. In 1998 these companies spent over $9.9 billion on research and development (R&D). The industry relies heavily on private investment seeking high returns, and believes that "patents are among the first and most important benchmarks of progress in developing a new biotechnology medicine."[29] The successful commercialization of a biotechnology patent requires years of development and an average $300 million investment.[30] Between FY 1994 and FY 1997 the biotechnology industry entered over 48,000 patent applications (12,000 per annum). This is startling when compared to 1978, when only 30 biotechnology patents were requested, and 1988, when the number was just 500. As the biotechnology industry matured, the availability of patent information to the public began to evidence tensions in two areas: depository requirements and researcher secrecy.

In the Supreme Court case discussed above, one of the pro-patent briefs suggested that depository requirements would help biotechnology patents meet the law's public reporting requirements. However, granting patents on life forms raises thorny questions regarding how and under

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[28] Ned Hettinger, "Patenting Life: Biotechnology, Intellectual Property, and Environmental Ethics," *Environmental Affairs* 22, (1995): 277-78.

[29] Reported by the Biotechnology Industry Organization (BIO), *Introductory Guide to Biotechnology*. Available September 5, 1999 at <http://www.bio.org/aboutbio/guidetoc.html>; *1997-1998 BIO Editor's and Reporter's Guide to Biotechnology*. Available April 1, 1998 at <http://www.bio.org/library/welcome.dgw>. BIO derived these statistics from Kenneth B. Lee, Jr. and G. Stephen Burrill, *Biotech '97 Alignment: An Industry Annual Report*, 11th ed. (Ernst & Young, 1997). BIO is the biotechnology industry's most important trade and lobbying organization, representing over 700 biotechnology companies, academic institutions, state biotechnology centers, and other entities in over 47 states and 20 countries. BIO states that it "supports efforts of eliminate excessive, irrelevant regulatory burdens that inhibit safe and effective products from reaching the public as quickly as possible."

[30] James Nurton, "Biotechnology Patents: Biotechnology's Winning Formulas," *Managing Intellectual Property*, June 1997. Available March 27, 1998 at <www.lawmoney.com/public/contents/publications/MIP/mip9706/mip9706.7.html>

what circumstances actual biological specimens should be handled in the patenting process and what role authorized bioculture repositories could play in storing the items.

The Patent Act states that reporting requirements for a specification must contain a written description of the invention and the process for making and using it. It must describe the "best mode contemplated by the inventor of carrying out his invention."[31] It is the specifics as to what exactly satisfies the "best mode" requirement that has proved to be problematic. The law does not aggressively require deposits and the PTO makes determinations on a case by case basis, the argument against them being that deposited cultures are easy prey to infringement given that they are self-replicating entities.[32]

In a 1992 symposium on legislative and legal issues in biotechnology patent attorney Albert P. Halluin reviewed recent legal decisions that depositing a bioculture in a registered and authorized culture depository was not necessary to fulfill the "best mode requirement of a patent specification. In one specific case, Amgen, Inc. v. Chugai Pharmaceutical Co., a federal circuit court determined that Amgen did not violate the "best mode" disclosure requirement when it did not deposit cells it had created.[33] Halluin, for one, argued that such a decision "breaks the patent bargain" whereby inventors get exclusive monopoly rights to their inventions for seventeen years in exchange for public reporting of the details of that invention into the flow of scientific information, and that, by not having to make deposits, inventors will receive the benefits of both trade secrecy and patent protection simultaneously.[34]

While the issue of researcher secrecy did not receive attention from either the Supreme Court of the amicus curiae briefs in 1980, it has developed into a major issue. Privatization of biological knowledge engendered by the patent development process has hindered the sharing of such knowledge. Such withholding can actually undermine innovations in biotechnology because it limits reporting of research results. Some university-based researchers have become averse to

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[31] 35 U.S.C. § 112.

[32] U.S. Patent and Trademark Office, "Deposit of Biological Materials for Patent Purposes: Final Rule," 37 *Code of Federal Regulations*, Part I, Section 1801, January 1, 1990.

[33] Amgen, Inc. v. Chugai Pharmaceutical Co. Ltd., 18 USPQ2d 1016 (Fed. Cir. 1991).

[34] Rudy Baum, "Knotty Biotech Issues Receive Attention," *Chemical and Engineering News* (27 April 1992): 30-31.

freely sharing samples and delay publication of the research findings until after their patents are awarded.[35]

A 1994 survey by Blumenthal found that 90 percent of 210 life-science companies, including biotechnology firms, conducting life-science research had a relationship with an academic institution and that over half of these relationships resulted in “patents, products, and sales” as a direct result of this relationship. An overwhelming majority of these companies sometimes require academics to maintain the confidentiality of information during and after the filing of a patent application, often at rates three times longer than that recommended by the National Institutes of Health. Withholding information in this manner was seen by Blumenthal and his co-authors as potentially denying other researchers the opportunity to conduct peer review that repeats and confirms/disconfirms prior work. Blumenthal concludes that the previous decade’s interaction between universities and industries “may pose greater threats to the openness of scientific communication than universities generally acknowledge.”[36]

A related 1997 survey, also authored by Blumenthal, of over 2,000 life science faculty found that nearly 1 out of every 5 faculty reported that they delayed the publication of their results for at least six months; half of this group reported doing so because of patent applications. Faculty who were engaged in the commercialization of their research were found to be more likely to deny access to their research results and were three times more likely to delay publication for at least six months than those whose research was not targeted towards commercialization.[37] A more recent study by Blumenthal found that over half of some 1,000 university scientists who admitted

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[35] A.J. Lemin, “Patenting Microorganisms: Threats to Openness,” in Vivian Weill and John Snapper, eds. *Owning Scientific and Technical Information : Values and Ethical Issues* (New Brunswick: Rutgers University Press, 1989). Cited in Hettinger, “Patenting Life,” (1995) p. 293.

[36] David Blumenthal, Nancyanne Causino, Eric Campbell, and Nancy Seashore Louis, “Relationship Between Academic Institutions and Industry in the Life Sciences—An Industry Survey,” *New England Journal of Medicine* 334, 6 (8 February 1996): 368-73.

[37] David Blumenthal, Eric Campbell, Melissa Anderson, Nancyanne Causino, and Karen Seashore Louis, “Withholding Research Results in Academic Life Science: Evidence From a National Survey of Faculty,” *JAMA: Journal of the American Medical Association* 277, 15 (16 April 1997): 1224-28.

receiving gifts from drug or biotechnology companies stated that these donors expected some influence over their work, ranging from patent rights to pre-publication review.[38]

While unforeseen at the time of the Supreme Court ruling, the patenting of life has generally negatively impacted openness in terms of both the scope of patent reporting and the dissemination of research results. The largely unforeseen complications associated with depository requirements, and the increases in academic reluctance to share research results in a timely fashion, are shifting norms away from the traditional transparency that has long been associated with scientific inquiry.

Restriction of Public Access to Information Concerning the Development of Genetic Engineering

The early development of genetic engineering is unusual not only because the public had access to knowledge about the new field itself but also because it also had access to the processes through which policies for control of the new field were formed. The U.S. National Institutes of Health (NIH), which assumed responsibility for developing genetic engineering controls, was not a regulatory agency but rather the leading sponsor of biomedical research. The traditional norms of scientific inquiry encouraged openness in the NIH arena and the sunshine laws passed by Congress in the 1960s and 1970s reinforced those norms. Consequently, the meetings of the Recombinant DNA Advisory Committee (RAC) established by the Department of Health, Education, and Welfare to advise the NIH director on the safety of genetic engineering were open. Indeed it was said at the time that one of the best ways to get a sense of the cutting edge of this new field was to attend those meetings, which generated thousands of pages of information about future experiments.

This public face of government policy making for genetic engineering was widely registered in the press coverage of the time and has been the focus of much academic analysis since. There is, however, a less visible, but arguably more influential dimension of the formation of genetic engineering policy. The evidence comes from a series of meetings that took place between government officials and representatives of the pharmaceutical and emerging biotechnology indus-

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[38] "Corporations Swap Gifts for Influence Over Scholars," *New York Times*, April 1, 1998. Two out of every three recipients of these gifts, which ranged from pieces of DNA to lab equipment to money, stated that the gift was important or very important to their research.

tries in the late 1970s. These meetings were held out of the glare of the public spotlight on this controversial field. Consequently, they were much less registered in the press and in academic analysis. These meetings were, in general, unannounced, and information about them emerged long after they were held and mainly as a result of requests for records under the Freedom of Information Act.[39] They had little of the drama of the clashes that happened among members of the RAC and between the RAC and members of the public.

While the principal concern of academic scientists involved in genetic engineering was to get on with their research and not to be held back in relation to work in other countries, the principal concern of industry representatives who discussed their concerns with U.S. government officials in this period was quite different: the central theme of all of the meetings examined was the protection of trade secrets. Industry interest in maintaining secrets focused on the openness of the NIH procedures: The NIH controls promulgated in 1976 classified the large-scale culture and the release into the environment of genetically engineered organisms as “prohibited experiments.” This category did not mean that experiments were absolutely prohibited but that permission for an

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[39] Industry meetings revealed by FOIA requests are as follows:

(1) Meeting of the NIH director, Donald Fredrickson, with representatives of the pharmaceutical and chemical industries, June 2, 1976, National Institutes of Health. This meeting was attended by representatives of Eli Lilly, Dow, General Electric, W.R. Grace, Pfizer, Monsanto, Smith Kline & French, Merck, and other large transnational corporations.

(2) Meeting of the Assistant Secretary for Science and Technology, Department of Commerce, Dr. Betsy Ancker-Johnson, with representatives of 17 firms including Abbott, Cetus, CIBA-Geigy, Dupont, General Electric, Eli Lilly, Merck, Monsanto, Upjohn, Wyeth, Searle, and Pfizer, November 19, 1976.

(3) Meeting of the NIH director, Donald Fredrickson, with representatives of the Pharmaceutical Manufacturers’ Association, November 29, 1976.

(4) Meeting between representatives of the National Institutes of Health, private industry, and the Department of Commerce, November 17, 1977, at the Pharmaceutical Manufacturers’ Association. No records available.

(5) Meeting representatives of the Department of Commerce, the National Institutes of Health, the Office of Science and Technology Policy, and the pharmaceutical and biotechnology industries, December 18, 1977, at the Pharmaceutical Manufacturers’ Association.

(6) Meeting with the General Counsel of the Department of Health, Education, and Welfare, Peter Libassi, and representatives of the pharmaceutical industry, October 13, 1978, Department of Health, Education, and Welfare.

(7) Meeting of DHEW General Counsel Peter Libassi, representatives of NIH and FDA, and representatives of the pharmaceutical industry, May 25, 1979, Department of Health, Education, and Welfare.

exception could only be granted after full disclosure of technical details and a review, held in public, by the RAC. From the first recorded meeting of pharmaceutical industry representatives with NIH director Donald Fredrickson in June 1976 onwards, industry representatives pressed for a major modification of this requirement. What the industry wanted, and eventually achieved in the 1980s, was review of their projects not by the RAC but only by a local "biosafety committee" appointed by the company pursuing the project.

The ideal policy-making procedure the industry desired was described in some detail at a meeting between Department of Commerce officials and representatives of the pharmaceutical industry in December 1977.[40] Protection of trade secrets was the paramount concern. The industry representatives proposed a system of "voluntary compliance" with the NIH controls, with the responsibility for monitoring the safety of industrial processes transferred to the local level, to biohazard committees appointed by the industry in question. A representative of the Upjohn Company gave as an example of an "apparently successful committee" a group established by Upjohn at its headquarters in Kalamazoo, Michigan, composed of six Upjohn executives and three prominent members of the local community. These people were "the highest type of person who would make sure that the public interest [was] properly served."[41]

From 1976 onwards, representatives of the pharmaceutical and biotechnology industries pressured the NIH director to devise means to protect corporate secrets by threatening to ignore the NIH controls whenever these secrets were at risk. For example, shortly before the NIH controls were issued in June 1976, the executive vice president of Eli Lilly, Cornelius Pettinga, informed the NIH director, Donald Fredrickson, that Lilly would not feel obliged to provide NIH with information about the organisms used in its genetic engineering work; nor would the minutes of its biosafety meetings be necessarily available for public inspection. If convinced of the safety of a genetic engineering process, Lilly would have "no hesitation in conducting" genetic engineering at

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[40] Meeting no. 5, previous note.

[41] Dr. George S. Gordon, Department of Commerce, Memorandum for the Record, on meeting with representatives of the pharmaceutical industry, December 19, 1977, held at the headquarters of the Pharmaceutical Manufacturers Association.

industrial-scale volumes. Pettinga reminded Fredrickson, some of Lilly's work would be "proprietary."[42]

Two years later, in October 1978, Genentech, with whom Lilly had contracted to do the development work for production of human insulin, made good on this threat. A front-runner in the race among biotechnology startups for dominance in the field, Genentech informed the NIH that its biosafety committee had approved large-scale production of human insulin with genetically engineered microbes at a containment level that violated the NIH guidelines. Despite NIH insistence to Genentech that large-scale production required prior review and approval by the RAC, the company continued to flaunt the NIH controls. In March 1979 the company informed the NIH that "due to problems of proprietary information, Genentech would make most of the decisions assigned by the . . . Guidelines . . . [by itself]." To the *New York Times* Genentech justified its action on the grounds that to submit data to the NIH would be to "risk divulging information to Genentech rivals who might force it from the Government under the Freedom of Information Act."[43]

The NIH responded to the Genentech rebellion not by disciplining the company, as it did the occasional unruly scientist—indeed, as a non-regulatory agency, it had no legal authority to do so. Rather, the NIH responded by adjusting its procedures to conform to industry requirements for secrecy. In May 1979, with the Genentech rebellion in full gear, the NIH director proposed to the RAC a "voluntary compliance" scheme in which industry proposals for large-scale work would be reviewed by the committee in closed session, with criminal penalties for committee members who divulged corporate secrets.[44]

The largely academic RAC resisted this idea. The committee voted to recommend mandatory controls for the private sector—a signal to the U.S. Congress to take up the industry problem. As the Director of the National Institute of Allergy and Immune Diseases, Richard Krause, observed at a further private meeting with industry representatives a few days later, "this [procedure]

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[42] Cornelius W. Pettinga to Donald S. Fredrickson, 4 June 1976, Recombinant DNA History Collection, MC100, Institute Archives and Special Collections, MIT Libraries, Cambridge, MA.

[43] Details of the exchanges between Genentech and the National Institutes of Health are given in Wright, *Molecular Politics*, pp. 324-44.

[44] Wright, *Molecular Politics*, pp. 292-93.

represents a significant departure from traditional NIH procedures" and that "some [RAC] members might wish to resign when all of these considerations are brought to their attention."[45] Krause had correctly read the committee's response. It took a shrewd personal campaign for over a year on the part of the NIH director to persuade the RAC to accept the idea of keeping industry information secret. The practice of secrecy did not sit well with academics used to the freedom to share ideas—especially when jail terms for divulgence of corporate secrets were part of the bargain. Ironically, the only criminal penalty for violation of the NIH controls was not for the unauthorized release of genetically engineered organisms but for the unauthorized release of information concerning such organisms.[46]

In 1982 these issues about public exposure of industry secrets began to disappear when a further major revision of the NIH controls transferred responsibility for industrial-scale uses of genetically engineered organisms to local biohazard committees—the model industry representatives had pressed for all along. A further issue of concern to industry—review of release of genetically engineered organisms into the environment—took several more years to settle. In the early 1980s, release of genetically engineered organisms into the environment was still seen as a significant concern. (After all, release negated one of the basic premises of the NIH controls, containment.) This was an issue on which technical opinion was seriously divided, as indeed it remains to this day. In this case, the White House Office of Science and Technology Policy intervened and dealt with the problem of protecting industry secrets by taking the problem out of the NIH and putting it in the Department of Agriculture and the Environmental Protection Agency to regulate under existing statutes.[47] That move was hardly an ideal solution but it was no doubt satisfactory from an industry point of view since it served to take industry proposals out of the public spotlight.

In summary, the early NIH controls for genetic engineering were an anomaly in the history of regulation of private industry. The response of the emerging genetic engineering industry to the

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[45] Department of Health, Education, and Welfare, Minutes of Meeting of Pharmaceutical Manufacturers' Association with HEW General Counsel, Peter Libassi, p. 3.

[46] For a detailed account, see Wright, *Molecular Politics*, ch. 10.

[47] See, e.g., Valerie Fogleman, "Regulating Science: An Evaluation of the Regulation of Biotechnology Research," *Environmental Law* 17, 2 (1987): 229-64.

norm of openness the controls assumed reveals not only the drive towards secrecy by this industry but also the responsiveness of government institutions: when it came down to a choice between protecting traditional academic norms of open review or developing closed procedures, the National Institutes of Health chose the second course, even though a majority of the members of the NIH advisory committee opposed it. It was industry, not academic science, which won the temporary battle for introducing secrecy into the NIH procedures.

Effects of Secrecy in the Biotechnology Industry on Public Policy: Negotiations to Strengthen 1972 Biological Weapons Convention

The Biological Weapons Convention (BWC), which bans the development, production, stockpiling, and transfer of biological and toxin weapons, was negotiated in 1969-1972. With the important exception of the high levels of secrecy attached to research and development within biological warfare programs, this was a period when biological research was generally governed by traditional norms of openness, at least in the civilian sector.[48] This is not to say that the pharmaceutical industry at that time was not interested in intellectual property. Even in 1966, a British Foreign Office report referred to "the commercial secrecy with which so much microbiological work in the West is tied up," dismissing the calls for openness at that time from non-governmental organizations such as Pugwash as "based on exceedingly frail assumptions about the cosmopolitanisms of scientists."[49] At that point, however, the interests of pharmaceutical corporations focused on products and processes, not genes, cells, and organisms.

Furthermore, molecular biology in the 1960s was an academic field. Attempts to patent the results of "basic" research in molecular biology would have been seen as anachronistic and prob-

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[48] Activities in military contexts were an entirely different matter. The U.S. terminated its highly secret biological weapons program in 1969, but the policy guiding its continuing biological defense program (National Decision Memorandum 35, November 25, 1969) was silent on the question of secrecy. The former Soviet Union also conducted a secret biological weapons program which began in the 1920s and underwent a substantial expansion in the 1970s. For a detailed account of the latter, see Anthony Rimmington, "Invisible Weapons of Mass Destruction: The Soviet Union's Biological Weapons Programme, 1918 to 1991," in Susan Wright, ed., *Meeting the Challenges of Biological Warfare and Disarmament in the 21st Century* (forthcoming).

[49] U.K. Foreign Office, Arms Control and Disarmament Research Unit, "Arms Control Implications of Chemical and Biological Warfare: Analysis and Proposals," ACDRU(66)2 (2nd draft, 4 July 1966), p. 57.

ably also as a barrier to the "freedom" of scientific inquiry. Harvard molecular biologist Matthew Meselson, who is often credited as an influence on President Richard Nixon's decision to dismantle the U.S. biological weapons program and to support negotiations leading towards a universal ban on such weapons, has been, over the past three decades, a constant advocate of transparency with respect to biological research, of openness as the route towards strengthening the Convention.[50] And so, during the BWC negotiations, when the Soviet Union and other members of the eastern bloc proposed in March 1971 a draft convention that included an article committing parties to the "fullest possible exchange of equipment, materials, and scientific and technological information for the use of bacteriological (biological) agents and toxins for peaceful purposes,"[51] not a single country objected. Indeed, the proposal was so uncontroversial that the chief American negotiator, James Leonard, recalled that it provoked no discussion at all.[52]

Today, some twenty-seven years after the completion of the BWC, the emergence of strong norms of secrecy in the civilian sector is having a significant impact on the further elaboration of the Biological Weapons Convention, and particularly on the efforts now under way to strengthen the Convention by negotiating a legally binding protocol with compliance and verification provisions. At the end of the cold war such an instrument was seen, particularly by some western states and by some non-governmental organizations, as a promising route to "strengthening" the Convention. This view also gained momentum from the progress being made at that time towards completion of the Chemical Weapons Convention and the Soviet Union's general reversal of its previous opposition to on-site inspections. Despite reservations aired by the United States in particular, development of a verification Protocol received qualified support at the Third Review Conference in 1991, and following the work of an expert group and a special conference of the states

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[50] See, e.g., Matthew Meselson, Martin Kaplan, and Mark Mokulsky, "Verification of Biological and Toxin Weapons Disarmament," *Science and Global Security* 2 (1991): 235-52; Matthew Meselson, "Implementing the Biological Weapons Convention of 1972," *UNIDIR Newsletter* 4, 2 (June 1991): 10-13.

[51] Bulgaria, Czechoslovakia, Hungary, Mongolia, Poland, Romania, Union of Soviet Socialist Republics, "Draft Convention on the Prohibition of the development, production and stockpiling of bacteriological (biological) weapons and toxins and on their destruction," 30 March 1971 (CCD/325).

[52] Susan Wright, interview with James Leonard, August 1996.

parties in 1994, the negotiation of a Protocol by an Ad Hoc Group comprising delegations from the States Parties began in 1995.

From the outset, it was recognized by many States Parties as well as by leaders of the biotechnology and pharmaceutical industries that verification in the BWC context posed particularly difficult technical problems. Unlike chemical warfare agents, biological agents can be relatively easily produced and also easily destroyed. Quantities of biological agents, therefore, are not significant markers of the presence or absence of a bioweapons program. They may also occur naturally in the environment. Consequently biological verification poses difficult problems of interpreting both false positives and false negatives. Furthermore, both equipment and agents are largely dual-purpose in nature and cannot therefore be used as unambiguous indicators of the presence or absence of a bioweapons program.[53]

Beyond these technical problems, the boundaries between permitted and prohibited activities defined by the Biological Weapons Convention itself introduce a further and serious ambiguity. The treaty as written does not draw a sharp boundary between defensive and offensive research and development, or even, in limited quantities, production.[54] Furthermore, by the fall of 1995 the experience of the UN Special Commission on Iraq (UNSCOM) had underscored the point that even highly intrusive, no-notice inspections might raise strong suspicions but were unlikely to produce definitive evidence of violations if the inspected party was intent on hiding evidence of bioweapons activities.

In response to these problems, proponents of verification proposed high levels of transparency in the biological sciences and biotechnology. According to an early and influential proponent of verification, "Full disclosure is the only guarantee of defensive intent . . . If a verification regime is to provide security, it must require and enforce total openness; at the same time, it will obviate the need for secrecy by constituting a better deterrent than any secret defense program."[55]

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[53] United Kingdom, "The Role and Objectives of Information Visits," 13 July 1995 (BWC/AD HOC GROUP/21).

[54] Susan Wright, "Complexity, Ambiguity, Secrecy: The Problem of 'Strengthening' the 1972 Biological Weapons Convention," in Susan Wright, ed., *Meeting the Challenges of Biological Warfare and Disarmament in the 21st Century* (forthcoming).

[55] Barbara Rosenberg and Gordon Burck, "Verification of Compliance with the Biological Weapons Convention," in S. Wright, ed., *Preventing a Biological Arms Race* (Cambridge: MIT

It is doubtful than any of the states parties would have endorsed such a call for complete openness. Nevertheless, the U.K. and several other states parties (including Australia, Canada, New Zealand, South Africa, the Netherlands, and Sweden), recognizing the major challenges of BWC verification, initially called for high degrees of transparency. In the words of a U.K. working paper, what was needed was "an integrated and balanced package of measures" comprising wide-ranging declarations, on-site inspections (known in this context as "visits"), challenge inspections and investigations of alleged use designed to uncover violations, and implementation by a professional inspectorate. Certainly this early vision of verification suggested that the regime would need to be even more intrusive than that of the Chemical Weapons Convention if it were to function effectively in deterring violations and in enabling states to provide reassurance about their biological defense activities.[56]

From the beginning of the negotiations for the BWC Protocol, however, the U.S. biotechnology and pharmaceutical trade associations have opposed development of an intrusive verification regime and have pressed the U.S. Department of Commerce and the U.S. State Department to support their position. At the forefront of this effort have been the Pharmaceutical Research and Manufacturers of America (PhRMA), representing the country's leading research-based pharmaceutical and biotechnology companies, and the Biotechnology Industry Organization (BIO), representing some 1400 biotechnology firms. Foremost among the industry's concerns is the risk of loss of intellectual property through information acquired by international inspectors during visits to industrial facilities.[57]

Loss of intellectual property was also an important concern for the chemical industry during the negotiations leading up to the Chemical Weapons Convention. However, the growth of the biotechnology industry is currently extremely dynamic, with a ten-fold increase in the global

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Press, 1990), p. 304.

[56] United Kingdom, "The Role and Objectives of Information Visits," 13 July 1995 (BWC/AD HOC GROUP/21). For further analysis of the U.K.'s position, see Oliver Thranert, "Issues in the Ad Hoc Group to the BWC: How did the Three Depositary States—the United States, Russia, and the United Kingdom—Approach the Compliance Problem?" in Susan Wright, ed., *Meeting the Challenges of Biological Warfare and Disarmament in the 21st Century* (forthcoming).

[57] The documents supporting this view were obtained by one of the authors (David Wallace) through a request under the Freedom of Information Act filed in 1998.

market predicted for the 1990s,[58] and industry leaders have argued that it is more vulnerable to loss of proprietary information than the chemical industry. In a detailed paper sent to the U.S. State Department in 1995, the trade association BIO argued that "the sensitivity to loss of proprietary information is much greater in the pharmaceutical and biotechnology industries than in the basic and fine chemical production industries where numerous non-proprietary intermediates and catalysts are often used. Any implementation of a declaration and verification protocol under the BWC must protect proprietary information for the pharmaceutical and biotechnology industries where the U.S. is the undisputed world-leader." In an analysis of the various off-site and on-site measures being considered at that time in Geneva as part of a verification package, the paper argued that all on-site measures, such as sampling, interviewing, identification of key equipment, and continuous monitoring as well as auditing off-site, were of greatest concern to the industry.[59]

PhRMA and BIO have repeatedly pressed the U.S. government to respond to their interests in protecting their proprietary information. In June 1996, the president of PhRMA, Gerald Mossinghoff, wrote to then-Secretary of Commerce Michael Kantor expressing concern that "the U.S. may not be able to take a forceful leadership role in formulating a protocol that achieves the objectives of strengthening the BWC while protecting U.S. businesses' legitimate proprietary interests." The U.S. government was urged to "play a positive role in these negotiations and not stand by while other countries develop an international norm that could prove inimical to our national interests." And it was also reminded that "the pharmaceutical industry is one of the few remaining U.S. industries with a positive trade balance that has been maintained for over ten years. We are relying on the U.S. Government to help us maintain this position as the BWC is negotiated."[60]

Rather than an extensive and intrusive regime aimed at transparency, the U.S. trade associations have pressed for drastically limiting the reach of such a regime with respect to

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[58] For discussion of these points, see Biswajit Dhar, "The Patent Regime and Implementing Article X of the Biological Weapons Convention: Some Reflections," in Susan Wright, ed., *Meeting the Challenges of Biological Warfare and Disarmament in the 21st Century* (forthcoming).

[59] U.S. Pharmaceutical and Biotechnology Industries White Paper on Strengthening the Biological Weapons Convention, (n.d.; sent by A. Goldhammer, BIO, to U.S. State Department, 23 June 1995), p. 2 and Appendix 2.

[60] Gerald Mossinghoff to Michael Kantor, 12 June 1996.

information concerning industrial processes, equipment, and facilities. In a policy statement circulated in 1996, PhRMA proposed the following conditions:

- No routine inspections of any kind.
- On-site inspections limited to investigations of non-compliance.
- Allegations aimed at an investigation of non-compliance to be subjected to a strong "green-light" filter requiring a vote of three-quarters of the members of an Executive Council of representatives of the States Parties to a Protocol in order to proceed.
- Non-governmental inspected facilities to have the right to make the final determination of materials and equipment to be shielded from inspectors because of their proprietary nature.[61]

Similar positions were advocated by BIO and by the Material Technical Advisory Committee, a group of senior executives drawn from U.S. industry and academia.[62] The positions taken by the European trade associations, the European Federation of Pharmaceutical Industries and Associations (EFPIA) and the Forum for European Bioindustry Coordination (FEBC) in 1998 were less specific and somewhat more flexible than that of their American counterparts but nevertheless aired the same concerns. In a position paper circulated in 1998, EFPIA resisted the idea of site visits other than investigations of non-compliance and similarly urged that proprietary information

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[61] Pharmaceutical Research and Manufacturers Association, "Reducing the Threat of Biological Weapons—a PhRMA Perspective," 25 November 1996; circulated at the Fourth Review Conference of the Biological Weapons Convention, 25 November-6 December, 1996. For a detailed discussion of these requirements, see William Muth, "The Role of the Pharmaceutical and Biotech Industries in Strengthening the Biological Disarmament Regime," in Susan Wright and Richard Falk, eds., *Responding to the Challenge of Biological Warfare—A Matter of Contending Paradigms of Thought and Action, Politics and the Life Sciences*, symposium proceedings, *Politics and the Life Sciences* (March, 1999).

[62] Alan Goldhammer, BIO, to William Reinsch, Under Secretary for Export Administration, U.S. Department of Commerce, 3 July 1997; Alan Hart, Chairman, Materials Technical Advisory Committee and R&D Director, Advanced Materials, Dow Chemical Company, to Steven Goldman, Office of Chemical and Biological Controls and Treaty Compliance, U.S. Department of Commerce, June 27, 1997.

remain under the full control of an inspected company.[63] FEBC specifically rejected routine inspections.[64]

These positions contrasted with the support of the chemical industry for the CWC regime. The chemical industry, like its biotechnology counterpart, was certainly sensitive to the need to protect proprietary information.[65] Nevertheless industry leaders accepted such measures as routine visits to declared sites, sampling, and investigations of charges of non-compliance with a “red-light” filter. With a red-light filter, challenge investigations are carried out unless three-quarters of the members of the Executive Council vote against proceeding. They are therefore more likely to take place than with a green-light filter. Industry leaders were also, apparently, satisfied with the procedures for protection of confidential information provided in the “Annex on the Protection of Confidential Information” to the Chemical Weapons Convention. In contrast, measures to protect proprietary information proposed for the BWC Protocol have not so far reassured leaders of the biotechnology industry. The reasons for the differences in the behaviors of the two industries are beyond the scope of this paper to analyze in depth and they are no doubt complex. The Chemical Weapons Convention was completed at the end of the cold war, in a different negotiating climate; the chemical industry is an older, more established, less dynamic industry, and the patent data suggest that it is less dependent on “cutting edge” techniques; industry representatives also claimed that they were concerned about the negative public image that resistance to the CWC might yield; and so forth.

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[63] European Federation of Pharmaceutical Industries and Associations (EFPIA), Statement on the Biological and Toxin Weapons Convention (n.d. c. March 1998).

[64] Forum for European Bioindustry Coordination, Position on a Compliance Protocol to the BTWC, Draft, June 30, 1998, cited in W. Muth, “The Role of the Pharmaceutical and Biotech Industries.”

[65] See, e.g., Detlef Mannig, “At the Conclusion of the Chemical Weapons Convention: Some Recent Issues Concerning the Chemical Industry,” in Benoit Morel and Kyle Olsen, eds., *Shadows and Substance: The Chemical Weapons Convention* (Boulder: Westview Press, 1993), pp. 145-46; John Gee, “A Strengthened BWC: Lessons to be Learned from the Chemical Weapons Convention,” *UNIDIR Newsletter* No.33/96 (1996): 75-80; Ettore Greco, “Protection of Confidential Information and the Chemical Weapons Convention,” in M. Bothe et al., eds., *The New Chemical Weapons Convention—Implementation and Prospects* (The Hague: Kluwer Law International, 1998), pp. 365-70.

What is clear is that, in the absence of other, over-riding factors, concerns with protection of trade secrets have so far haunted the collective consciousness of the biotechnology industry, and have influenced national policy, perhaps particularly that of the United States. The effects of industry pressure on the U.S. position were evident in a brief White House statement issued in January 1998 that adopted a "green-light" filter for investigations of non-compliance. In addition, the White House paper dropped any requirement for routine inspections aimed at confirming the accuracy of declarations, proposing only "voluntary" visits where access as well as the visit itself would be controlled by the visited party, and "non-challenge clarification visits" designed to clarify ambiguities in declarations.[66] Since a "green-light" filter requires such a large majority vote to be pursued, it is likely to be very difficult to achieve in practice except in the most extreme circumstances. Thus the Clinton proposal amounted to not much more than a system of declarations plus a few clarifying visits.

Even so, the U.S. pharmaceutical and biotechnology industry was not satisfied. In March 1998, PhRMA chairman Sidney Taurel of the huge pharmaceutical corporation Eli Lilly wrote to National Security adviser Samuel Berger and Secretary of Commerce William Daley to express the continuing concern of the industry about "possible adverse impacts on biomedical innovation through harm to our companies' intellectual property, reputations, and confidential business information." Specifically, Taurel cited the industry's "[worries about] non-challenge inspections and our skepticism whether any 'voluntary' visit will truly be voluntary."[67] A United States working paper tabled in Geneva in July 1998 appeared designed to meet PhRMA's concerns halfway. The paper proposed that clarification visits would be undertaken only after stringent efforts to address issues in other ways and only under conditions that allowed the visited party to protect proprietary information and to decide on access to samples. Furthermore, when the Director of the U.S. Arms Control and Disarmament Agency addressed the Ad Hoc Group in October 1998, his statement

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[66] United States, Office of the Press Secretary, the White House, "Fact Sheet: The Biological Weapons Convention," 27 January 1998.

[67] Sidney Taurel (Eli Lilly), Chairman, PhRMA to Samuel Berger (Assistant to the President for National Security Affairs) and William Daley (Secretary, Department of Commerce), 9 March 1998; see also Jonathan B. Tucker, "Strengthening the BWC: Moving Toward a Compliance Protocol," *Arms Control Today* (January/February 1998): 20-27.

was remarkable for its complete silence on the question of visits.[68] In summary, the influence exerted by the U.S. pharmaceutical and biotechnology industries has had the effect of denying the United States a leadership role in Geneva in supporting a Protocol that provides transparency concerning intentions.

Over twenty years ago, before the change in the norms of biological research addressed in this paper had taken place, the Swedish diplomat Alva Myrdal wrote: "Openness is the primary tool for verification of disarmament . . . Immediately accessible to verification by the international community are scientific and technological data available through publications and other media." Myrdal called for even greater openness, arguing that "the key to control of disarmament is the construction of universal confidence based on the cumulative process of shared information."[69] The work of the Ad Hoc Group is premised on a similar view. The U.S. biotechnology industry's desire for protection of industry secrets appears to be on a collision course with the needs of a compliance or verification regime for high levels of transparency. PhRMA and BIO do not represent every single biotechnology company and pharmaceutical corporation. (To this point, one or two have dissented from the trade association position.) But they represent some of the most influential members of a huge industry. It is doubtful that a verification system with the kinds of restrictions proposed by PhRMA could provide either reassurance about a country's intentions or evidence of a violation, since a prohibited activity could be hidden under the guise of protection of trade secrets. But the negotiations are not yet over, and the industry's position may yet evolve if the industry can be persuaded that support for a strong verification regime is in its best interests.[70] To this point, however, the evidence suggests that the change in norms of transparency in biotech-

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[68] United States, Working Paper: Proposed Elements of Clarification Visits, 9 July 1998 (BWC/AD HOC GROUP/WP.294); United States, Statement of John Holum to the Biological Weapons Convention Ad Hoc Group Session XII, 6 October 1998. For a more detailed analysis of the evolution of the U.S. negotiating position, see Oliver Thranert, "Issues in the Ad Hoc Group to the BWC: How did the Three Depositary States—the United States, Russia, and the United Kingdom—Approach the Compliance Problem?" in Susan Wright, ed., *Meeting the Challenges of Biological Warfare and Disarmament in the 21st Century* (forthcoming).

[69] Alva Myrdal, *The Game of Disarmament* (New York: Pantheon Books, 1976), pp. 302-04.

[70] If such a reversal were to happen, we might then learn more concerning the positions of a further sector interested in secrecy—the military agencies around the world responsible for biological warfare programs.

nology has had the effect of seriously diluting present efforts on the part of governments and non-governmental organizations to strengthen the verification regime for biological weapons.

Conclusion

In the post-Cold War world there has been a general trend towards increased transparency by governmental bodies: classified archives are being opened and scholars and the public are developing a richer understanding of our shared recent past. Such initiatives will enable the world's societies to obtain a clearer sense of the reasons behind the ebbs and flows of the Cold War era. Ironically, at the same time that the public sector is generally making more information available about itself, both private industry and academia have witnessed increases in secrecy. The allowance of patents for biotechnology discoveries has had a negative impact on traditional norms of scientific inquiry, typified by openness of research and timely access to the results of research. The quite expensive race to obtain patents in the highly competitive biotechnology industry has led to a narrowing public access not only to the contents of actual patents, but also to the research undergirding the patents. While intellectual property rights serve as an incentive to investments in and commitments to scientific innovation, reducing scientific investigations to largely commercial endeavors whose rewards are largely contingent on obtaining patents will continue to erode informed public and academic discourse. Concerns over patentability have and will continue to drive researchers into non-disclosure and other secrecy commitments with private firms, thus severely limiting timely access to emerging scientific knowledge.

In conclusion, secrets are political creatures, not only because they define relations in which knowledge is withheld from a person or group or country but also because they articulate a set of relative gains and losses to the actors involved. The secrets of the biotechnology industry are no exception. Since its inception in the mid-1970s, this study shows that the industry has exerted considerable influence to close routes of access to knowledge concerning the nature of the organisms in use, the genes they carry, the techniques of modification, and the industry’s intentions for the future of the field. Such a trend poses substantial barriers to informed public policy discussion on the advisability and safety associated with life forms that are appropriated as “intellectual property.” Furthermore, as the case study on the Biological Weapons Convention shows, the secrecy now veiling the biotechnology industry may well impact policies in areas that appear remote from the initial sphere of action.

Acknowledgements

The authors thank the participants in the Cornell University Conference on Secrecy for a stimulating discussion of the issues and Judith Reppy for generative editorial suggestions.

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