Big Science: Price to the Present

*By James H. Capshew and Karen A. Rader**

Let us not take it for granted that life exists more fully in what is commonly thought big than in what is commonly thought small.
—Virginia Woolf, *The Common Reader* (p. 190)

BIG IS AN ADJECTIVE easily attached to modern science. Whether measured by the number of scientific papers published each year, the amount of money expended on research, or the size of the scientific work force, science is an activity that commands enormous resources. It also represents a powerful and pervasive way of understanding the natural world and human society. Thus it is at once a broadly diffused mode of cognition and a concentrated form of organized labor.

The growth of science is perhaps its most notable historical characteristic, whether considered in terms of scope, scale, complexity, or impact. From the seventeenth century to the present, science has been transformed from the preoccupation of a few savants and natural philosophers in Europe to the occupation of several million scientific and technical specialists around the globe. By the middle of the twentieth century science had become so intensively and widely cultivated that some observers suggested that the era of "Big Science" had begun.

It is no accident that the existence of Big Science was first discerned in the United States, where growth is a way of life and bigger is often viewed as better.[1] In science, growth was especially visible in the increasing scale of research. Beginning in the 1930s Ernest Lawrence and his staff at the University of California made headlines with their ever-larger cyclotrons, huge machines designed to explore the atomic nucleus that required great amounts of money and personnel. For many the age of Big Science was ushered in by the Manhattan Project during World War II, when the building of the atomic bomb involved the mobilization of much of the U.S. community of physical scientists in an engineering project of

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[1] "What was distinctive in American science was the scale: the ability to bring large masses of men, money, and equipment together over great distances to achieve a result. Indeed, throughout all of American history, one of the leitmotifs has ever been the enlargement of scale": Charles C. Gillispie, *The Professionalization of Science: France, 1770–1830, Compared to the United States, 1910–1970* (The Third Neesima Lectures, 1981) (Kyoto: Doshisha Univ. Press, 1983), p. 13. A recent survey indicates that roughly half of the world's large research facilities and instruments costing more than $25 million exist in the United States, with the remainder split among sixteen other countries: House Committee on Science and Technology, Task Force on Science Policy, *World Inventory of "Big Science" Research Instruments and Facilities,* 99th Congress, 2nd session (Science Policy Study Background Report 4) (Washington: Government Printing Office, 1987).

An interior view of the support column of the sixty-five-foot pressurized electrostatic generator (Van de Graaff type) of Westinghouse Research Laboratories in Pittsburgh, Pennsylvania, which began operating in 1939. The first industrial "atom smasher" of its kind, it was designed to produce protons with energies up to five megavolts. Courtesy of the Westinghouse Historical Collection, Westinghouse Electric Corporation.

unprecedented magnitude. By the early 1960s, with the advent of NASA and the national space program, the term "Big Science" was firmly affixed as a label for projects that required large-scale organization, massive commitments of funds, and complex technological systems.

In addition to the basic ingredients of money, manpower, and machines, the media and the military were often seen as essential to the pursuit of Big Science. Print journalism and television were important in fostering public and political support for such projects. National security provided a useful rationale for research and development generally, and served as a justification even for projects that had no direct military application.

Although the litany of money, manpower, machines, media, and the military provides a convenient mnemonic for the chief features of Big Science, it also tends to obscure the process of growth by focusing on its end results. "Big," of course, is a relative term, and relies on some contrast with "small" or "little." In the case of science, this contrast is almost always implicit; small-scale or "Little Science" is usually defined as lacking one or another characteristic of Big Science, or as some vague historical predecessor. Thus Big Science has come to be identified almost entirely as a contemporary phenomenon, with the singularity of current large-scale research enterprises taken for granted. There can be little doubt that such projects as the Hubble Space Telescope or the Human Genome Initiative qualify as Big Science, but an exclusive concern with the attributes of "bigness" in science draws attention away from the more significant and interesting question of how science becomes larger.

This essay explores the origins of the concept of "Big Science" in the early 1960s and its subsequent discussion in historical, sociological, and policy studies. After reviewing the literature, we clarify the distinction between two major dramas of scale in science. One is concerned mainly with the contemporary scene and the consequences of science *being* big; the other takes an evolutionary approach and looks at how science *becomes* big(ger). We then argue that the latter perspective can be used to historicize Big Science, as well as to recast issues of scientific growth within a framework sensitive to relative changes in scale, scope, and significance. Along these lines, we sketch the beginnings of a new narrative of "bigger science" originating before the twentieth century. In conclusion we suggest some ways in which recent work in the history and social studies of science might contribute to this new historiographical approach to Big Science.

I. CONSTRUCTING THE WORLD OF BIG SCIENCE

> The large-scale character of modern science, new and shining and all-powerful, is so apparent that the happy term "Big Science" has been coined to describe it.
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> —Derek J. de Solla Price,
> *Little Science, Big Science* (p. 2)

In a rhetorical sense, the age of "Big Science" was already established by 1982, when the avant-garde performance artist Laurie Anderson produced a record album entitled *Big Science.* The transit of the term from scientific to popular culture suggests its linguistic attractiveness as well as its analytical intractability. Although its exact origins are obscure, the term was being used in the late 1950s and had achieved wide circulation by the early 1960s.[2] It articulated an awareness that the growth of science, for better or worse, had significant implications for modern society. Instead of taking growth for granted, observers began to explicate its various manifestations, whether their aims were expository, managerial, critical, or celebratory.

The resulting literature on Big Science is remarkably heterogeneous, and a critical mass of scholarship dealing with the subject is only starting to emerge. Accordingly, the review that follows is suggestive rather than definitive, and the themes we discuss do not by any means exhaust the analytic possibilities. Our goal is simply to offer a preliminary thematic map of the territory and to point out some important landmarks.

Big Science as a Pathology

In 1961 Alvin Weinberg used the term "Big Science" in a commentary written for *Science* on the "impact of large-scale science on the United States." For Weinberg, a physicist and the director of the Oak Ridge National Laboratory, the term's meaning was obvious: throughout the article "Big Science" (always capitalized) simply replaces the awkward epithet "large-scale science" from his title. The "symbols of our time," declared Weinberg, were to be found in "the monuments of Big Science" such as particle accelerators, rockets and space vehicles, and experimental nuclear reactors. Drawing comparisons with the pyramids of Egypt and the Cathedral of Notre Dame, Weinberg suggested provocatively that the "tools of giant science" play an analogous role in expressing the highest values and deepest aspirations of modern culture.[3]

Yet Weinberg's encomium was tempered by an overarching concern that Big Science might be "ruining science." Although he acknowledged that "Big Science is an inevitable stage in the development of science," he warned scientists and the public to be wary of its "triple diseases—journalitis, moneyitis and administratitis." According to him, journalitis resulted from Big Science's great need for public financial support and from the recent proliferation of specialized scientific writings; together they insured that "serious" scientific writing would be predigested in the popular press, "blurring the line between journalism and science." Moneyitis was simply "the rush to spend dollars instead of thought" in devising research strategies, such as "order[ing] a $10 million nuclear reactor instead of devising a crucial experiment with the reactors at hand." And administratitis was the increased number of scientist-administrators permeating the ranks of the scientific enterprise: more "chiefs with bellies to the mahogany desks" than "Indians with bellies to the bench."[4]

Weinberg, then, was responsible for the original rhetorical construction of Big Science as well as for championing a metaphorical view of Big Science as a pathological condition, afflicting the otherwise healthy organism of science proper. In

[2] Hans A. Bethe used the term in his review of Robert Jungk, *Brighter Than a Thousand Suns,* in *Bulletin of the Atomic Scientists,* 1958, *14*:426–428, on p. 428. We are grateful to Robert W. Smith for drawing this reference to our attention.

[3] Alvin M. Weinberg, "Impact of Large-Scale Science," *Science,* 1961, *134*:161–164, on p. 161.

[4] *Ibid.,* p. 162.

addition to identifying the disease he offered a prescription: "by confining Big Science" to huge national laboratories like Oak Ridge, we could potentially "prevent the contagion from spreading" and "taking over Little Science." Several years later, in his *Reflections on Big Science,* Weinberg still worried whether Big Science was "blunting science as an instrument for uncovering new knowledge." Yet he also argued persuasively for his belief that "the large mission-oriented laboratories and what they do are indispensable to the society that supports them." As a science policymaker, what Weinberg desired was not so much the reinstatement of Little Science in place of Big Science as a redistribution of financial resources within Big Science itself, largely according to each project's "social merit or relevance to human affairs and the values of man."[5]

While Weinberg maintained a faith in the fundamental healthiness of science, some of his colleagues were less optimistic. The launch of Sputnik in 1957 led to a massive infusion of federal funds for scientific research, reawakening fears among some scientists that large, mission-oriented projects were distorting the traditional moral economy of the scientific community. In 1959 the head of the Carnegie Institution of Washington, the physicist Merle Tuve, sharply criticized what he regarded as a disturbing trend toward the pursuit of technological goals rather than scientific understanding. His remarks at a symposium on basic research sponsored by the National Academy of Sciences and the American Association for the Advancement of Science were published in the *Saturday Review* under the headline "Is Science Too Big For the Scientist?"[6] In a similar vein the maverick mathematician Norbert Wiener criticized the advent of what he labeled the "megabuck era" in science and the inevitable corruption of researchers by the values of the marketplace.[7] For both Wiener and Tuve, Big Science appeared to be a terminal disease of mature science, and their criticisms were flavored with a certain nostalgia for an earlier, more innocent age.

Big Science as a Scientific Phenomenon

Although Weinberg helped to introduce the term Big Science to the scientific community, a physicist turned historian, Derek J. de Solla Price, gave it wider currency with his book *Little Science, Big Science,* published in 1963. His concern with "science-in-the-large" dated from the 1940s, and he had presented similar ideas in a previous essay entitled "Diseases of Science," a chapter in the volume *Science Since Babylon* (1961).[8]

[5] *Ibid.;* and Alvin M. Weinberg, *Reflections on Big Science* (Cambridge, Mass.: MIT Press, 1967), pp. v–vi, 75–76.

[6] Merle A. Tuve, "Is Science Too Big for the Scientist?" *Saturday Review,* 6 June 1959, pp. 48–52. The episode is described in Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940–1960," *Historical Studies in the Physical and Biological Sciences* (*HSPS*), 1987, *18*:149–229, on pp. 218–219. See also Allan A. Needell, "Lloyd Berkner, Merle Tuve, and the Federal Role in Radio Astronomy," *Osiris,* 1987, *3*:261–288.

[7] Norbert Wiener, "Science: The Megabuck Era," *New Republic,* 27 Jan. 1958, pp. 10–11. Wiener's ambivalent attitudes toward science are explored in Steve J. Heims, *John von Neumann and Norbert Wiener: From Mathematics to the Technologies of Life and Death* (Cambridge, Mass.: MIT Press, 1980).

[8] Derek J. de Solla Price, *Little Science, Big Science* (New York: Oxford Univ. Press, 1963), republished with other essays as *Little Science, Big Science . . . and Beyond* (New York: Columbia Univ. Press, 1986), see pp. xvi, xix–xx (1986 ed.); Price, "The Exponential Curve of Science," *Discovery,*

Even though he implied that growth could be pathological, Price took a radically different approach. He was less concerned with the state of contemporary scientific institutions than with charting the overall historical growth of science by means of a variety of statistical indicators. In his preface Price argued that the subject justified his quantitative approach: "Why should we not turn the tools of science on science itself," he asked, in an "attempt to develop a calculus of scientific manpower, literature, talent and expenditure on a national and on an international scale."[9]

Unlike Weinberg, Price did not take the meaning of Big Science for granted. In fact, he explicitly desired to elucidate what can "distinguish the present phase [of Big Science] as something new, something different" from the preceding age of Little Science. And although he acknowledged that Big Science had brought with it qualitative changes like "a great deal of administration, organization and politicking," Price argued that "we have not been sufficiently scientific in analyzing a whole set of regularities" in the growth of science.[10] Clearly, he envisioned Big Science primarily as a scientific phenomenon: science itself was a measurable entity and Big Science was simply a quantitatively different signal on the historical screen of scientific progress.

Price used statistical data on the increasing number of scientists and scientific papers to determine what he called the first "law" of scientific growth, namely that scientific activity has maintained a general exponential growth for the last three hundred years, doubling in size along the way about every fifteen years. Not only has science always been growing, but in absolute terms it had grown by five orders of magnitude since the seventeenth century. Price also noted that while this growth might be correlated with the appearance of bigger equipment and larger research groups, these were incidental rather than defining characteristics of the phenomenon of Big Science. Instead, in his general model Price defined Big Science as science at the saturation point of its logistic growth curve, that is, at the historical phase during which previously exponential growth begins to level off. Saturation did not imply the end of science but rather "the beginning of new and exciting tactics for science, operating with quite new ground rules." Thus Big Science could be understood as "an uncomfortably brief interlude between the traditional . . . Little Science and the impending period . . . [of] New Science." Price speculated that science had probably made this transition during the 1940s or 1950s, although he clearly rejected the implication that World War II was a major factor in the most recent growth of science.[11]

Price's original and highly quotable book did much to focus attention on issues of scientific growth, and helped to popularize the notion of Big Science, even though it left many important questions unanswered.[12] The research specialty

1956, *17*:240–243; and Price, *Science Since Babylon* (New Haven: Yale Univ. Press, 1961), Ch. 5.

[9] Price, *Little Science, Big Science . . . and Beyond* (1986), pp. xv, xvi.

[10] *Ibid.*, pp. 13, xv–xvi.

[11] *Ibid.*, pp. 29, 28, 15.

[12] E.g., Price's statistical approach allows him to avoid defining in concrete historical terms what "saturation" in the growth curve of science means or what cumulative effects exponential growth has wrought. For a general review and critique see Renate Breithecker-Amend, *Big Science und das Ende des exponentiellen Wachstums: Zur Wissenschaftsforschung de Solla Prices* (Frankfurt am Main: Verlag Peter Lang, 1988).

known as scientometrics was given impetus by his work, as was the quantitative analysis of science more generally.[13]

Big Science as an Instrument

Even early on, however, it was clear to observers that technology was a hallmark if not the main driving force behind Big Science. In his initial formulation Weinberg emphasized the monumental technologies (e.g., accelerators, space vehicles) characteristic of Big Science, which were made possible only through state sponsorship. In a more general vein Price stressed the importance of technological innovation (i.e., instruments, machines, automata) as an engine of scientific change. He challenged the prevailing assumption that technology derived from or was subordinate to scientific theorizing and argued that however brilliant thinkers such as Newton and Descartes were, in the long run techniques counted for as much as ideas. "In short, the scientific revolution, as we call it, was largely the improvement and invention and use of a series of instruments of revelation that expanded the reach of science in innumerable directions, and almost fortuitously."[14]

Along these lines, some studies have emphasized the instrumental dimensions of Big Science, either in the literal sense of its technological components or in the metaphorical sense of its use to serve conjoined political and cognitive goals. From the first perspective, the technology-intensive nature of Big Science has created new problems of practical engineering for scientists, for example, how to operate an orbiting satellite laboratory through remote control. In the case of particle physics, the "big machine," as the title of Robert Jungk's 1968 study of CERN (Conseil Européen pour la Recherche Nucléaire) suggests, served as a focus for research. Accelerators provide perhaps the best example of how bigger and bigger scientific instruments can evolve into complicated technological systems requiring industrial-scale inputs of capital and labor.[15]

Peter Galison has pointed out that even existing technology exploited in a new way can realign scientific practice and epistemological priorities according to the engineering ethos of Big Science. Luis Alvarez's team of researchers at Berkeley, Galison argues, "recreated" Donald Glaser's bubble chamber invention, using a computer to automate and routinize many tasks that were previously the reponsi-

[13] See G. Nigel Gilbert, "Measuring the Size of Science: A Review of Indicators of Scientific Growth," *Scientometrics,* 1978, *1*:9–34; and Yehuda Elkana, Joshua Lederberg, Robert K. Merton, Arnold Thackray, and Harriet Zuckerman, eds., *Toward a Metric of Science: The Advent of Science Indicators* (New York: Wiley, 1978); and more generally Paul Forman, John L. Heilbron, and Spencer Weart, "Physics *circa* 1900: Personnel, Funding, and Productivity of the Academic Establishments," *HSPS,* 1975, *5*:1–185; and Arnold Thackray, Jeffrey L. Sturchio, P. Thomas Carroll, and Robert Bud, *Chemistry in America, 1876–1976: Historical Indicators* (Dordrecht: Reidel, 1985).

[14] Derek J. de Solla Price, "Of Sealing Wax and String," *Natural History,* 1984, *93*:49–56, on p. 54. (A somewhat different version appears in *Little Science, Big Science . . . and Beyond* [cit. n. 8], p. 246.)

[15] Robert Jungk, *The Big Machine* (New York: Scribners, 1968). See also John L. Heilbron and Robert W. Seidel, *Lawrence and His Laboratory,* Vol. I of *A History of the Lawrence Berkeley Laboratory* (Berkeley: Univ. California Press, 1989); and Armin Hermann, John Krige, Ulrike Mersits, and Dominique Pestre, *Launching the European Organization for Nuclear Research,* Vol. I of *History of CERN* (Amsterdam: North-Holland, 1987). For a general overview see Philip H. Abelson, "Instrumentation and Computers," *American Scientist,* 1986, *74*:182–192.

bility of physicists. As a result, data reduction played a more prominent role in experimental demonstration, and an increased premium came to be placed on the managerial talent of the research team's leader.[16]

Beyond exemplifying the impact of instrumentation on the conduct of research, Big Science can also be characterized as a political instrumentality deployed for the purposes of its patrons. In space research, for example, much effort has been expended on modifying laboratory instruments so they will work in the environment of space, launching them into place, and providing reliable communications systems. But such feats of engineering and science take place within the context of NASA, which, as the philosopher Stephen Toulmin pointed out in the mid 1960s, is basically "a political agency taking political decisions . . . a state within a state." Similarly Walter McDougall argued in his recent history of the space program that "the advent of spaceflight in our time . . . is not just a tale of the gumption and luck of Russian, German or American rocketeers, but also of the progress of the idea of command technology as a tool and symbol of the modern state." The general point has been expressed well by the historian Charles Gillispie: "From science all the politicians and statesmen want are instrumentalities, powers but not power: weapons, techniques, information, communications, and so on."[17]

Big Science as Industrial Production

The technological thrust of Big Science, combined with heavy investments in facilities and infrastructure, had important organizational consequences that were often conceptualized as the industrialization of research. In 1964 Toulmin noted that "in the organization of scientific research, the natural unit is the *technique*—nuclear reactors, rockets, lasers, computers, games-theory, or whatever."[18] Around such structures grew differentiated groups of investigators, each performing a special technical function and operating under some form of management. The similarities to industrial production were inescapable.

In 1964 the physicist Paul Zilsel criticized the "mass production of knowledge" and argued that "the huge 'think factories' of our time are equivalent to the Lancashire cotton mills of the industrial revolution. The scientists are many, and they are very busy producing staggering quantities of 'knowledge.' Their product, however, is increasingly taking on the character of a mere commodity; and their work takes on the alienated character of assembly line production." Zilsel's lament reflected a sense that the industrialization of research represented the triumph of the values of big business in science. He and other writers in this thematic genre paint a picture of Big Science as a market-conscious, product-oriented, and capital-intensive activity which, unlike Little Science, has taken on

[16] Peter Galison, "Bubble Chambers and the Experimental Workplace," in *Observation, Experiment, and Hypothesis in Modern Physical Science,* ed. Peter Achinstein and Owen Hannaway (Cambridge, Mass.: MIT Press, 1985), pp. 309–373.

[17] Stephen Toulmin, "The Complexity of Scientific Choice: A Stocktaking," *Minerva,* 1964, *2*:343–359, on p. 355; Walter A. McDougall, *. . . the Heavens and the Earth: A Political History of the Space Age* (New York: Basic Books, 1985), p. 19 (see also Clayton R. Koppes, *JPL and the American Space Program: A History of the Jet Propulsion Laboratory* [New Haven: Yale Univ. Press, 1982]); and Gillispie, *Professionalization of Science,* (cit. n. 1), p. 18.

[18] Toulmin, "Complexity of Scientific Choice," p. 358.

the impersonal and inhumane nature of an industrial enterprise. Yet very few writers among those who hold this view of Big Science ever actually use the Weinbergian term in their work; they are much more likely to make generic references to the "commodification" of knowledge or to the historical process of "scaling-up" that science has undergone.[19]

Though others had begun to articulate this view of Big Science by the mid 1960s, the philosopher Jerome Ravetz was still remarkably prescient when in 1971 he wrote at length about it in *Scientific Knowledge and Its Social Problems.* He worried that "until very recently, there was no systematic appreciation of the fact that as science grows and penetrates industry it becomes industrialized. This is partly a matter of scale." Ravetz offered a twofold image in characterizing "industrialized science." He first noted that much of what the government considered pure or basic scientific research now involved capital investments on a "gigantic scale." This increasing capitalization necessitated a structural division of labor between bench scientists and their "industrial managers" and resulted in the concentration of power in the hands of science administrators. Ravetz also considered the industrialization of science to entail an ideological shift, "with the loss of boundaries which enabled different styles of work, with their appropriate codes of behavior and ideals, to coexist." This shift was especially insidious, given his belief that "research is a craft activity, of a very specialized and delicate sort" and that "scientific knowledge cannot be mass-produced by machines tended by semi-skilled labor."[20]

Other writers stressed the organizational aspects of Big Science, with its hierarchical structures and highly specialized divisions of labor. For instance, Gerald Swatez's sociological study of the bubble-chamber research group at the University of California's Radiation Laboratory (now the Lawrence Berkeley Laboratory) presented a model of "teamwork" in science that fell between egalitarian person-to-person collaboration and authoritarian hierarchical control. Lew Kowarski, the technical director of the French atomic energy commission, explored the social-psychological dimensions of large research establishments, particularly in relation to problems of morale and management. From NASA administrator James Webb's perspective, Big Science was defined by a management program: how to insure "the constructive use of great aggregations of resources and power." Webb was also quick to note that "endeavors like NASA would not be 'large-scale' unless the need they serve is urgent and important" from a societal standpoint.[21] Whether they viewed it with alarm or approbation, most writers agreed that a high degree of organization and coordination had become a permanent part of scientific life.[22]

[19] Paul R. Zilsel, "The Mass Production of Knowledge," *Bull. Atomic Sci.*, 1964, *20*:28–29, on p. 29; and, e.g., Michael Gibbons and Bjorn Wittrock, eds., *Science as a Commodity: Threats to the Open Community of Scholars* (Harlow, Essex: Longman, 1985).

[20] Jerome R. Ravetz, *Scientific Knowledge and Its Social Problems* (New York: Oxford Univ. Press, 1971), pp. 22, 23, 31. See also Ravetz, *The Merger of Knowledge with Power: Essays in Critical Science* (London: Mansell, 1990).

[21] Gerald Swatez, "The Social Organisation of a University Laboratory," *Minerva*, 1970, *8*:36–58; L. Kowarski, "Psychology and Structure of Large-Scale Physical Research," *Bull. Atomic Sci.*, 1949, *5*:186–191, 200, 204; and James E. Webb, *Space Age Management: The Large-scale Approach* (New York: McGraw Hill, 1969), pp. ix, 9.

[22] At least one prominent physicist held the view that cooperative research was a means to overcome

Big Science as an Ethical Problem

Ravetz's depiction of Big Science as industrialized research contains the moral admonition that a scientific enterprise governed by values of rationalized production was also likely to be ethically indifferent at best. Yet the conviction that Big Science posed an ethical problem for science and for society actually originated several decades earlier, in the wake of the atomic bombing of Japan in World War II. In general the problem was cast as one of accountability, but accountability meant different things to those who had a professional responsibility within the community and to those looking in from the outside.

Those within the community expressed a growing awareness that the new institutional norms of Big Science necessitated rethinking of the profession's internal ethical standards. The trend toward multiple-authored papers, for instance, raised questions about the traditional modes of attributing and distributing credit to individuals. In 1962 the physicist Melvin Schwartz argued that in such a "production-line" approach to scientific research "*nobody* need feel real responsibility for the accuracy of the results." As the Challenger space shuttle disaster would reveal, it was correspondingly difficult to lay blame when a large, expensive project went awry.[23]

In broader ethical terms, scientists often supposed themselves morally powerless and were less reflexive about their own agency in constructing the Big Science relationship between science and society. In the late 1950s Norbert Wiener, for example, lamented society's "degradation of the position of the scientist as an independent worker and thinker to that of a morally irresponsible stooge in a science-factory." Scientists who were just doing their job, Wiener argued, had become unwittingly caught in a stormy sea of political maneuverings but were simultaneously rendered powerless to pull themselves ashore—a situation "ruinous [not only] for the morale of the scientist [but also for] the objective scientific output of the country."[24]

During the 1960s and 1970s critics from within and without the scientific community saw the conduct of scientists as more deliberate. From within, *Science* editor Philip Abelson argued in 1966 that the current funding system, which allowed high-energy physicists to make decisions about their own support, was as blatantly self-serving as "asking a hungry cat to make recommendations about the disposition of some cream." Yet as one science policy expert, Harvey Brooks, complained in 1971, "the psychological and intellectual involvement of the public tends to occur only in relation to costly 'spectaculars' such as the Apollo pro-

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the inherent limitations of the individual human intellect: "We should not forget that while it is true that relativity theory could not have been conceived by teamwork, the structure of the George Washington Bridge, and probably even that of the Hanford nuclear reactors, could not have been thought out by a single individual." Eugene P. Wigner, "The Limits of Science," *Proceedings of the American Philosophical Society,* 1950, *94*:422–427, on p. 427.

[23] Melvin Schwartz, "The Conflict Between Productivity and Creativity in Modern Day Physics," *American Behavioral Scientist,* 1962, *6*(4):35–36; and Thomas F. Gieryn and Anne E. Figert, "Ingredients for a Theory of Science in Society: O-Rings, Ice Water, C-Clamp, Richard Feynman, and the Press," in *Theories of Science in Society,* ed. Susan E. Cozzens and Gieryn (Bloomington: Indiana Univ. Press, 1990), pp. 67–97.

[24] Norbert Wiener, "A Rebellious Scientist After Two Years," *Bull. Atomic Sci.,* 1948, *4*:338–339.

gram . . . and this public interest in turn tends to distort the values and goals of science itself."[25] From without, Friederich Dürrenmatt's 1962 play *The Physicists* told the allegorical story of a physicist who feigns insanity to avoid facing his responsibilities to society. Ironically, this physicist is pursued in his asylum refuge by two other physicists who have already sold their scientific souls to become spies for their national homelands. At the play's climax, all three agree that the only "rational solution" to their ethical problems is to remain consigned forever to the asylum: "Let us be mad, but wise. / Prisoners but free. / Physicists but innocent."[26]

More recent scholarly studies have pushed the ethical critique of Big Science further into the domain of epistemology. Ian Hacking and Paul Forman have both suggested that the nationalistic, weapons-oriented ideology of Big Science affects not only research priorities but also the supposedly pure or "basic" character of the scientific knowledge which this system produces. According to Hacking, the prevalence of weapons research places limitations on the "menu" of choices for what even counts as scientific knowledge or discourse.[27] Forman implies that allowing government patronage to define physics research is tantamount to letting the military-industrial complex determine the character of our knowledge about the physical world. For him Big Science is essentially a new social contract between scientists and those who provide large-scale support or patronage of their work. In a later piece Forman places this argument in the historical context of the changing moral economy in physics after World War II. The physics community, he argues, has traded a prewar professional ethos based on the physicist as upholder of moral values for one which defines the physicist as a "playboy," an amoral actor stripped of virtually all social responsibility, who is just having "fun."[28] This new ethos, encouraged by the compartmentalization of research in Big Science, enables scientists to rationalize the decision to trade some measure of control over their work for greater occupational security and access to resources.

Big Science as Politics

If the working conditions within the realm of Big Science raised ethical questions, then the external relations of such activities presented an equally compelling set of political issues. Few could deny that Big Science was inherently political, since the accumulation of the necessary resources required the exercise of power. Often this power came from the authority of the national government, as in the case of the United States, which enlisted science in service to the state for a variety of

[25] Philip Abelson, "Are the Tame Cats in Charge?" *Sat. Rev.*, 1 Jan. 1966, pp. 100–103, on p. 102; and Harvey Brooks, "Models for Science Planning," *Public Administration Review*, 1971, *31*:364–374, on pp. 372–373. See also Brooks, *The Government of Science* (Cambridge, Mass.: MIT Press, 1968).

[26] Friederich Dürrenmatt, *The Physicists: A Play*, trans. James Kirkup (New York: Grove Weidenfeld, 1964), pp. 78, 84.

[27] Ian Hacking, "Weapons Research and the Form of Scientific Knowledge," *Canadian Journal of Philosophy*, 1986, *12* (Suppl.):237–260. See also John A. Remington, "Beyond Big Science in America: The Binding of Inquiry," *Social Studies of Science*, 1988, *18*:45–72.

[28] Forman, "Behind Quantum Electronics" (cit. n. 6); and Paul Forman, "Social Niche and Self-Image of the American Physicist," in *The Restructuring of Physical Sciences in Europe and the United States, 1945-1960*, ed. Michelangelo de Maria, Mario Grilli, and Fabio Sebastiani (Singapore: World Scientific, 1989), pp. 96–104.

purposes. Because of their size and complexity, Big Science projects could not avoid becoming embroiled in institutional, bureaucratic, and national politics.

World War II forcefully underscored the links between science and politics and brought about a new era in the relations between the American scientific community and the federal government. Scientists, especially physicists, enjoyed the power and prestige that accompanied the perception that their efforts had done much to win the war. Eager to reap the benefits of their new position, scientists obtained unprecendented support for basic as well as applied research. The expansion of support and the ensuing growth of scientific activity brought policy issues to the fore.

In the jargon of the 1950s, the question was whether scientists should be "on top" or "on tap"—that is, whether they were specially qualified to play a leading role in the political arena or should be limited to providing technical expertise.[29] The allocation of funds in a democratic society meant that priorities had to be set and choices made among competing interests. Such decisions were inherently political and depended more on judgments of relative value than on scientific knowledge or technical feasibility. As a *Science* editorial in 1956 said, "to launch a satellite requires some knowledge about the laws of physics, but the decision to use that knowledge is not itself a matter of physics. The decision rests on a complex system of values which, although difficult to express, culminates in the judgment that available funds should be spent to further the IGY program rather than, say, to reduce the national debt."[30] For Price, Weinberg, and others who hoped to contribute to national policy debates, it was useful to study the nexus of science, values, and politics in modern life. Similar concerns led the sociologist Edward Shils to establish *Minerva: A Review of Science, Learning and Policy* in 1962 as a forum for discussions of the "governmentalization" of science. The journal quickly became a place where scholars, scientists, and policymakers addressed issues arising from the salience of science in modern society.[31]

Federal patronage of basic science was one such issue. In the mid 1960s Weinberg formulated its rationale as a question of whether governments should support basic science as a form of high culture or as an overhead charge on applied science and technology. Weinberg tended to side with the latter position, favoring a utilitarian rationale. In a similar vein, Philip Abelson pointed out the lack of practical benefits derived from the huge investment in particle physics: "Never, in the history of science, have so many fine minds been supported on such a grand scale, and worked so diligently, and returned so little to society for its patronage." Stephen Toulmin recast the question of culture versus overheads by arguing that scientific activity was in the process of becoming a "tertiary" industry in postindustrial society. Thus its value could not be computed by tangible measures of productivity of scientists or outputs of knowledge. The social returns had to be assessed in terms of its contribution to increased employment and the quality of life.[32]

[29] Mary M. Simpson, "The Scientist in Politics: On Top or on Tap?" *Bull. Atomic Sci.,* 1960, *16*:28–29.

[30] Joseph Turner, "Facts and Values," *Science,* 1956, *124*:1055.

[31] On the aims of the new journal see Edward Shils, "Editorial," *Minerva,* 1962, *1*:5–17.

[32] Alvin M. Weinberg, "Criteria for Scientific Choice II: The Two Cultures," *Minerva,* 1964, *3*:3–14; Abelson, "Are the Tame Cats in Charge?" (cit. n. 25), pp. 101–102; and Stephen Toulmin, "The Com-

In *The Politics of Pure Science* (1967) the journalist Daniel Greenberg provided the first sustained look at the power structure of postwar American science and the role of the government in funding basic research. His account brought another familiar dilemma of science policy into focus, asking how to preserve the intellectual autonomy of science while serving the public interest. By the mid 1960s, he asserted, the American scientific community "had become affluent, highly productive, and the *de facto* sovereign of its own most vital affairs."[33]

In *The New Politics of Science* (1984) another journalist, David Dickson, took a less sanguine view of scientists' independence. He attempted to demonstrate "how the patterns of control over science reinforce and reproduce basic patterns of political control that operate in society." He viewed science as the key to the technological innovations that provide economic and military power, and argued that control over the production and use of scientific knowledge has been "increasingly concentrated in the hands of a class of corporate, banking, and military leaders, assisted by those in other sectors, such as universities" since the mid 1970s. Dickson's aim was to document these developments and to offer a strategy for reasserting democratic control over science.[34]

In an analysis of the politics of support in the field of oceanography published in 1989, the sociologist Chandra Mukerji went well beyond simple formulations of science and the state to sketch a complex interdependency. She argued that scientists have become part of an "elite reserve labor force" established by the government in the wake of World War II as a safeguard against future military emergencies. In accepting federal support, scientists "have traded the politically powerful voice of science for a not-so-steady supply of funds and relatively great control over the intellectual life of science." The ideology of autonomy both reinforces scientists' belief in their intellectual independence and legitimates their role as detached advisors to the government.[35]

The definition and history of Big Science also plays a role in current science policy debates. With several very large projects in the planning stages, such as the U.S. space station, the Superconducting Super Collider, and the Human Genome Initiative, both advocates and critics use notions of Big Science as an interpretative resource. For instance, James Watson, the former head of the Human Genome Initiative, first argued that the project to map the human genetic structure was an appropriate extension of Big Science into the realm of biology. Physical science had had its turn; now it was time for "biology's moon shot." Recently, however, Watson has recognized the political liabilities in the analogy and has begun to speak of the genome project as taking a "Little Science" approach, partly because only the management and not the work itself will be centralized. In order to add some historical perspective, John Heilbron and Daniel Kevles have scrutinized the common practice of drawing analogies to the development of nuclear science. In their view, lessons from the history of particle physics suggest that the

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plexity of Scientific Choice II: Culture, Overheads or Tertiary Industry?" *Minerva,* 1966, *4*:155–169.

[33] Daniel S. Greenberg, *The Politics of Pure Science* (New York: New American Library, 1967), p. 272.

[34] David Dickson, *The New Politics of Science* (New York: Pantheon, 1984), p. 5.

[35] Chandra Mukerji, *A Fragile Power: Scientists and the State* (Princeton: Princeton Univ. Press, 1989), p. 21.

genome initiative would be better served by not concentrating the project in a few national laboratories.[36]

Big Science as an Institution

Since World War II the bulk of the capital for Big Science has come from governments interested in supporting science for national interests, especially national security. Much of the early money allotted to Big Science was used for the construction and equipment of laboratory facilities rather than for the direct costs of research. In the United States, federal laboratories provided a "home for big science" after the war when the facilities involved in the Manhattan Project came under the control of the Atomic Energy Commission. Between 1951 and 1959 the six AEC laboratories received over $900 million for research and development expenses and nearly $3.5 billion for facilities.[37] These federal laboratories were among the first Big Science institutions, representing an unprecedented coming together of material resources, technical personnel, scientific interests, and sociopolitical goals.

The physical, spatial, and political dimensions defining this form of Big Science were difficult to ignore. A reconnaissance of the new $370 million proton synchrotron laboratory at CERN in the mid 1960s inspired one contemporary observer, the journalist Robert Jungk, to describe such facilities as "new territories," and CERN as a place where "the family of nuclear physicists, scattered by politics and war, is again united."[38] More recently, as historians have delved into newly opened government archives pertaining to several high-profile Big Science projects and laboratories, the institutional case study has become a popular analytic genre. Such accounts may emphasize the "bricks-and-mortar" and organizational aspects of Big Science or detail the transformation of some projects into institutions by virtue of their size and longevity.[39]

One exemplary work combining both kinds of analysis is a history by Robert W. Smith of the building of the Hubble Space Telescope. Costing about two billion dollars before it was even launched, the Space Telescope ranks as the most expensive scientific instrument ever constructed. But according to Smith, the telescope itself and the program to construct it should be viewed as "the products of a greater range of forces: scientific, technical, social, institutional, economic and political." He argues that rather than being peripheral to the scientific process "the fundamental question of whether or not to approve the Space Telescope was constantly being reframed as the issues bearing on the decision were themselves reshuffled and repackaged, often because of the needs of coalition building. More-

[36] Tom Shoop, "Biology's Moon Shot," *Government Executive,* Feb. 1991, pp. 10–17; and J. L. Heilbron and Daniel J. Kevles, "Finding a Policy for Mapping and Sequencing the Human Genome: Lessons from the History of Particle Physics," *Minerva,* 1989, *27*:299–314. On a previous foray into the realm of big biology see Chunglin Kwa, "Representations of Nature Mediating Between Ecology and Science Policy: The Case of the International Biological Programme," *Soc. Stud. Sci.,* 1987, *17*:413–442.

[37] Robert W. Seidel, "A Home for Big Science: The Atomic Energy Commission's Laboratory System," *HSPS,* 1986, *16*:135–175, on p. 162. See also the article by Roger L. Geiger in this volume.

[38] Jungk, *The Big Machine* (cit. n. 15), pp. 3, 189.

[39] See, e.g., Allan A. Needell, "Nuclear Reactors and the Founding of Brookhaven National Laboratory," *HSPS,* 1983, *14*:92–122.

over, the telescope's design and associated programs to build it were central elements in these changes." Smith's narrative represents a new synthesis in the study of Big Science. It illustrates with clarity how Big Science reconstituted the institutional organization of the scientific enterprise and was itself reconstituted by its necessary financial, political, and ideological ties to society at large.[40]

Other institutional case studies that focus more narrowly on the creation of specific Big Science facilities include John Heilbron and Robert Seidel's account of scientific and institutional progress at Lawrence Berkeley Laboratory in California and a collaborative history of the CERN accelerator center near Geneva, Switzerland.[41] Both works mention the importance of local context, including geographical and cultural factors, in shaping how the laboratory under study was conceived and constructed, although neither extends such generalizations to cross-national or even cross-laboratory comparisons. In contrast, Joan Bromberg's history of the pursuit of magnetic fusion as an energy source focuses on the four governmentally supported research teams at Princeton, Los Alamos, Lawrence Livermore, and Oak Ridge that carried out the bulk of fusion research. She argues that it is primarily the centralization of the research decision-making process, rather than centralized facilities, which defines the institutional boundaries of Big Science.[42]

Taken together, these case studies supplement earlier accounts that treated the institutional development of Big Science as a natural and unproblematic consequence of growth. Clearly, Big Science institutions have a character all their own. The case study format, however, has discouraged analysts from making broader generalizations about how individual institutions connect with the process of growth in science at large, particularly in terms of the realignment of social priorities that have allowed such powerful yet insulated research institutions to exist.

Big Science as Culture

At least as early as the 1960s, many began to perceive that the institutionalization of Big Science was intimately connected with the great cultural significance that science had come to hold in Western society. The humanist scholar Jacques Barzun produced a sharp critique of scientism in *Science: The Glorious Entertainment* (1964), and Spencer Klaw surveyed scientists as a rising cultural elite in *The New Brahmins: Scientific Life in America* (1968). As their titles suggest, these books were directed to the social and cultural ramifications of scientific ideology rather than the internal dynamics of the scientific community. Barzun argued that such effects arose largely because society had allowed science to become big: "Science itself, through its increased prosperity and fact-breeding specialism, has contributed to . . . disturbance and distress." Like Derek Price, he was hopeful that cultural observation, analysis, and criticism would influence contemporary policy decisions. Yet from Barzun's perspective, most microcultural studies of

[40] Robert W. Smith, *The Space Telescope: A Study of NASA, Science, Technology, and Politics* (Cambridge: Cambridge Univ. Press, 1989), quoting from pp. 25–26.

[41] Heilbron and Seidel, *Lawrence and His Laboratory* (cit. n. 15); and Hermann *et al.*, *Launching the European Organization for Nuclear Research* (cit. n. 15).

[42] Joan Lisa Bromberg, *Fusion: Science, Politics and the Invention of a New Energy Source* (Cambridge, Mass.: MIT Press, 1982), esp. pp. 2–5 and Ch. 6.

Big Science simply presupposed the phenomenon of interest: a pervasive scientific culture in which dramatically larger and larger forms of science became socially sanctioned.[43]

More recently, Sharon Traweek pioneered a new cultural approach to the history of Big Science. Her book *Beamtimes and Lifetimes* testifies to the continued effectiveness of the case study in analyzing important themes of Big Science, but it innovatively trades the "bricks-and-mortar" perspective of the traditional institutional approach for a self-consciously anthropological orientation. Arriving at the site of the Stanford Linear Accelerator Center (SLAC) as a "naive observer" of high energy physics culture, Traweek focused her attention on the internal dynamics of the community, looking at the physicist's socialization, gender roles, behavioral norms, and the connection between epistemological and social values. While other institutional and political case studies presumed the existence of "Big Science," "scientific community," and "society," Traweek's asked the question: "Where do the social categories of physicist and physics community . . . exist?" For comparative purposes she contrasted the SLAC community with a similar one at the KEK laboratory, near Tokyo, Japan.[44]

Traweek's study of high energy physicists exemplifies the genre of work that deals with Big Science as a cultural phenomenon, constructed within both science proper and society at large. It is informed by feminist perspectives on gender and science and is clearly aligned with the laboratory ethnography approach pioneered by sociologists of science. Traweek is also one of the few analysts to make explicit cross-national comparisons, suggesting that there may be "subtle yet significant differences" between American- and Japanese-style Big Science.[45]

Big Science as a Form of Life

In what might be described as an integration of the cultural and institutional approaches to Big Science, the sociologist Andrew Pickering has recently characterized Big Science as a form of life. As such, it embodies a heterogeneous yet coherent set of conceptual, material, and social resources. Although he locates the origins of Big Science in World War II, Pickering discards traditional approaches to the impact of the war on physics that separate "internal" from "external" factors according to epistemological criteria. In their place he suggests a more encompassing view of "the emergence of a unitary big-science constellation of scientific-technical-political-institutional practice." In order to illustrate his thesis he contrasts the initial invention of the bubble chamber in the early 1950s by Donald Glaser in the context of small-scale physics and its later exploitation by Luis Alvarez at the Lawrence Berkeley Laboratory. In the process of scaling up Glaser's initial 1-inch chamber into a 6-foot long version, Alvarez arranged a

[43] Jacques Barzun, *Science: The Glorious Entertainment* (New York: Harper & Row, 1964), quoting from p. 5; and Spencer Klaw, *The New Brahmins: Scientific Life in America* (New York: William Morrow, 1968).

[44] Sharon Traweek, *Beamtimes and Lifetimes: The World of High Energy Physicists* (Cambridge, Mass.: Harvard Univ. Press, 1988), p. 162.

[45] *Ibid.*, p. 156. See also, e.g., Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (Beverly Hills, Calif.: Sage Publications, 1986; 1st ed., 1979); and Max Charlesworth, Lyndsay Farrall, Terry Stokes, and David Turnbull, *Life Among the Scientists: An Anthropological Study of an Australian Scientific Community* (Melbourne: Oxford Univ. Press, 1989).

different combination of conceptual and institutional elements than did Glaser, even though both men utilized the same kinds of resources, including "workstyles, dollar bills, reason, knowledge of elementary particles, engineering rules of thumb, friends, contacts, skills, secretaries, piles of equipment."[46]

The latest and perhaps most concerted attempt to come to grips with the historiographical issues posed by Big Science is the recent volume *Big Science: The Growth of Large-Scale Research,* edited by Peter Galison and Bruce Hevly.[47] The chapters derive from papers presented by individuals from a number of fields, including history, physics, anthropology, and geography, at a conference held in 1988 at Stanford University. Although the articles themselves vary in their approaches, taken together they implicitly convey the power of the holistic analysis proposed by Pickering. In their programmatic comments the editors clearly favor a multidimensional and integrated framework. Furthermore, as a pioneering effort to bring order to an emerging field, the collection's very existence suggests that scholars are moving beyond simply recounting the standard drama of scale associated with specific Big Science projects.

II. EXPLORING THE DRAMA OF SCALE: SCENES FROM THE PAST

> The sheer bigness of Big Science was enough to arouse questions.
>
> —Daniel J. Kevles, *The Physicists* (p. 395)

The drama of scale lies at the heart of our intuitions about Big Science, and, more generally, at the heart of scientific understanding and of our understanding of science. By various conceptual and technical means scientists bring natural phenomena into view, figuratively and literally, and enable us to assimilate them into the realm of human knowledge. In the words of Bruno Latour:

> The small and invisible are made large, the large and unencompassable are made small. The fast are made slow and the slow are speeded up. Everything from the largest galaxy to the smallest particle is processed in the laboratory so that it can be captured on paper. Unlike the scientific phenomena themselves, the paper representations or "inscriptions" can be "read," superimposed, synthesized, integrated and transmitted. Above all they can be *manipulated.*

In a similar fashion, those who analyze the nature of science itself, including historians and sociologists, create dramas of scale when they move their focus back and forth between smaller and larger contexts, and across longer or shorter periods of time.[48]

[46] Andrew Pickering, "Big Science as a Form of Life," in *The Restructuring of Physical Sciences,* ed. De Maria, Grilli, and Sebastiani (cit. n. 28), pp. 42–54, on pp. 43, 48. On the evolution of the bubble chamber see Galison, "Bubble Chambers and the Experimental Workplace" (cit. n. 16).

[47] Peter Galison and Bruce Hevly, eds., *Big Science: The Growth of Large-Scale Research* (Stanford: Stanford Univ. Press, in press). We are indebted to Bruce Hevly for a prepublication copy of the book manuscript.

[48] Bruno Latour, as quoted in Charlesworth *et al., Life Among the Scientists* (cit. n. 45), p. 151. For a graphic depiction of the way changes in scale define the realm of science see Philip Morrison, Phylis Morrison, and the Office of Charles and Ray Eames, *Powers of Ten: A Book about the Relative Size of Things in the Universe and the Effect of Adding Another Zero* (New York: Scientific American Books, 1982). For an attempt to explain the enormous consequences of science by reference to the use of

Perhaps not surprisingly, writings on Big Science reflect little thematic unity as they encounter the dramatic properties of their subject. Such analytic confusion is often present in discussions of major historical shifts. In the context of urban history, for example, Sam Warner, Jr., noted that because "size at close range is one of the signal qualities of a city," early attempts to comprehend American urbanization were hindered by "the continual confrontation of large objects."[49] Similarly, as concern over the size and direction of science crystallized around the notion of Big Science in the 1960s, analysts remained focused on its most obvious aspects. Although Price and Weinberg appreciated the fact that Big Science had deep and complex roots, they were often dazzled by the sheer bigness of it all.

The studies examined in the preceding section reveal the increasing domination since the 1960s of a view of Big Science that is closer to Weinberg's initial formulation than to Price's. It entails a drama of scale that juxtaposes the huge machines, large organizations, and massive expenditures found in some contemporary research projects with the stereotyped lone investigator of the past, working with the proverbial sealing wax and string in a private laboratory. It associates Big Science with the rise of the military-industrial-academic complex, locating its origins in World War II. In retrospect, the Manhattan Project is made the opening act in the drama.

This focus on the large-scale qualities of current science has drawn attention away from Price's diachronic perspective, with its emphasis on long-term trends in the production of knowledge and the reproduction of scientists. Ironically, this drama of scale derives some of its appeal from revealing a regular growth of science during the past three centuries that makes its remarkable expansion since the mid-twentieth century simply the continuation of an existing pattern. And despite its historical referents, this drama has actually had little to say about the historical forces associated with the growth of science.

Size, however, is a relative parameter, and judgments of "bigness" must be made with respect to some norm or standard. There are also important distinctions to be made among different senses of "big," including large scale, broad scope, and great significance. In an effort to gain perspective on how modern science came to be considered Big Science, it seems useful to suggest some examples of past scientific endeavors that were considered impressively large by some measure at the time.

Price was among the first to suggest that large-scale science was not solely a contemporary phenomenon. He noted that "the great observatories of Ulugh Beg in Samarkand in the fifteenth century, of Tycho Brahe on his island of Hven in the sixteenth century, and of Jai Singh in India in the seventeenth century each . . . absorbed sensibly large fractions of the available resources of their nations." Astronomy, with its dependence on successively larger telescopes and research facilities, provides the most obvious prototype for modern large-scale science. In the nineteenth century, according to Robert Smith, the field underwent "a large-scale

graphical representations see Bruno Latour, "Drawing Things Together," in *Representation in Scientific Practice,* ed. Michael Lynch and Steve Woolgar (Cambridge, Mass.: MIT Press, 1990), pp. 19–68.

[49] Sam B. Warner, Jr., *Streetcar Suburbs: The Process of Growth in Boston, 1870–1900* (Cambridge, Mass.: Harvard Univ. Press/MIT Press, 1962), p. vii.

social process of rationalization" in which astronomers embraced the factory system as their organizational model.[50] Leaders in the research community, such as George Airy (Greenwich Observatory) and Edward Pickering (Harvard College Observatory), instituted an increasingly differentiated and hierarchical division of labor within the astronomical work force and began to focus more attention on large-scale mission-oriented projects such as the *Carte du Ciel.* These developments coincided with a period of vastly increased philanthropic support, most often aimed at the construction of bigger and better telescopes. To contemporary astronomers and other observers such changes indicated the dramatic growth of science.[51]

Another less obvious instance of relatively massive size can be found in the construction of huge batteries for electrochemical research in the early nineteenth century. At the Royal Institution in 1808, Humphry Davy built a battery with 500 voltaic cells, each containing double plates of metal 6 inches square. The battery generated currents to test the properties of various metals and other materials. Davy raised more than £1,000 in private donations for battery development and continued to experiment with larger cells, including some with plates 2 feet by 4 feet. One of his colleagues and supporters, John George Children, constructed a battery in 1813 that contained 21 cells, each with plates 2 feet 8 inches by 6 feet, all covered with 945 gallons of diluted nitrous and sulphuric acids. The problems associated with the operation and upkeep of this apparatus were as impressive as its ability to ignite metal wires.[52]

Because of their link to later technological developments, instruments such as telescopes and batteries could be considered simply as historical precursors to contemporary Big Science. But indulging in this presentist approach avoids questions about how such relatively large-scale projects were accomplished at the time. Moreover, focusing on centralization and concentration of resources misses other kinds of scientific activity that were impressively broad in scope, such as natural history, particularly of the type associated with Alexander von Humboldt.[53] This mode, decentralized and extensive, can provide other examples of the relative growth of science.

Since at least the eighteenth century there have been projects in the pursuit of natural knowledge that were considered remarkable in size. Some were expansive and sweeping, such as Diderot's *Encyclopédie,* which represented a literary strat-

[50] Price, *Little Science, Big Science . . . and Beyond* (cit. n. 8), p. 3; and Robert W. Smith, "A National Observatory Transformed: Greenwich in the Nineteenth Century," *Journal of the History of Astronomy,* 1991, *22*:5–20, on p. 17.

[51] Karen A. Rader, "Thinking About Nineteenth-Century Astronomy as Big Science," paper presented at the annual meeting of the History of Science Society, Madison, Wisconsin, Nov. 1991. See also Simon Schaffer, "Astronomers Mark Time: Discipline and the Personal Equation," *Science in Context,* 1988, *2*:115–145.

[52] Humphry Davy, "Electrochemical Researches," *Philosophical Transactions,* 1808, *98*:333–370; Davy, "The Bakerian Lecture for 1809: On Some New Electrochemical Researches," *ibid.,* 1810, *100*:16–74; John George Children, "An Account of Some Experiments," *ibid.,* 1809, *99*:32–38; and Children, "An Account of Some Experiments with a Large Voltaic Battery," *ibid.,* 1815, *105*:363–374. We are indebted to Richard Sorrenson for these references.

[53] "A 'big science' of this kind since the eighteenth century [was] natural history with its teams of collectors transported in specially adapted ships, and its teams of taxonomists classifying the specimens sent home in the great museums": David Knight, *The Nature of Science* (London: A. Deutsch, 1976), p. 185. On Humboltian science see Susan Faye Cannon, *Science in Culture: The Early Victorian Period* (New York: Dawson/Science History Publications, 1978), Ch. 3.

egy to elevate the status of natural knowledge. In the words of one historian, the "Encyclopedists recognized that knowledge was power, and by mapping the world of knowledge, set out to conquer it."[54]

The eighteenth century also saw the formation of a "grand alliance" between science and exploration that constituted the beginning of a "Second Great Age of Discovery." In 1761 and again in 1769 scientists from England, France, and several other countries put together expeditions to observe the transit of Venus across the face of the sun in order to refine measures of solar parallax. These endeavors required major investments in equipment and logistical support and contributed to the evolution of an international community of scientists.[55]

Written correspondence helped to expand the linkages among scientists in different countries and enabled data to be forwarded to appropriate collection points, individuals as well as institutions. By having the equivalent of modern-day research assistants in the field, natural historians could acquire the huge amounts of information that provided a basis for their work. Such far-flung correspondence networks enabled Linnaeus in the eighteenth century and Darwin in the nineteenth to accomplish their scientific projects.

During the nineteenth century science became increasingly institutionalized and thereby gained improved access to resources and became more firmly integrated with modern society. Among projects that convey the enlarged ambition of professionalizing scientists as well as their enhanced ability to muster the support necessary to achieve their goals was the British "Magnetic Crusade" to create a worldwide network of geomagnetic observatories in the 1830s. At the time William Whewell called the effort "by far the greatest scientific undertaking the world has ever seen." It promised to contribute to practical navigation as well as terrestrial physics, and it enlisted the sponsorship of the British Association for the Advancement of Science and the Royal Society, the support of the British Navy, and the labor of scattered groups of observatory personnel. Toward the end of the project John Herschel articulated the rationale justifying the effort: "Great physical theories, with their chains of practical consequences, are pre-eminently national objects, whether for glory or utility."[56]

Herschel's remarks point to the coupling of science and the state that has done much to fuel the growth of the scientific enterprise. Particularly germane in this context are the great national expeditions and surveys that have included the increase of natural knowledge (i.e., scientific research) among their goals. In the United States, for instance, the Army Corps of Topographical Engineers served simultaneously as "a pawn of politics" and "an instrument of science" in exploring the West before the Civil War. To determine the best route for a transcontin-

[54] Robert Darnton, "Philosophers Trim the Tree of Knowledge: The Epistemological Strategy of the *Encyclopédie*," in *The Great Cat Massacre and Other Episodes in French Cultural History* (New York: Basic Books, 1984), pp. 191–213, on p. 209.

[55] See William Goetzmann, *New Lands, New Men: America and the Second Great Age of Discovery* (New York: Viking, 1986); Stephen J. Pyne, "Space: A Third Great Age of Discovery," *Space Policy*, 1988, *4*:187–199; and Harry Woolf, *The Transits of Venus: A Study of Eighteenth-Century Science* (Princeton: Princeton Univ. Press, 1959).

[56] John Cawood, "The Magnetic Crusade: Science and Politics in Early Victorian Britain," *Isis*, 1979, *70*:493–518, quoting Whewell on p. 493 and Herschel on p. 518. See also Jack Morrell and Arnold Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science* (Oxford/New York: Clarendon Press, 1981), pp. 353–370.

ental railroad, the Corps enlisted the aid of the scientific community. In his description of the enterprise William Goetzmann evokes an earlier nexus of size, science, and political ambition: "Not since Napoleon had taken his company of savants into Egypt had the world seen such an assemblage of scientists and technicians marshaled under one banner."[57]

Scattered sites of inquiry, programmatic research planning, and extensive networks connecting investigators are common features of the examples above. Such measures of horizontal integration can be viewed as part of the expansion of scope associated with the drama of "bigger science." They provide an analytically useful contrast to the typical emphasis on the vertical dimensions of "Big Science" and the common perception that large-scale science requires masses of people and material in one place.

To recognize the importance of these early attempts to extend the scope and scale of modern science is not, as Price once said, to "be distracted by history" in an effort "to show that Little Science was sometimes big, and Big Science little."[58] Instead, it is an essential step toward understanding present-day Big Science as a historically contingent process. From this perspective, instruments such as telescopes and "mega-batteries" are only the most obvious technologies that permit humans to extend the range of their senses. No less important are the literary and social innovations—such as encyclopedias and correspondence networks—that enable humans to transcend the limitations of direct personal experience and face-to-face communication.[59] Thus the drama of scale is recast: what becomes most interesting about the "bigness" of Big Science is not its remarkable size in relation to its context, but rather how it came to be viewed as somehow distinctive or apart from the historical processes that produced it.

III. TOWARD A NEW HISTORIOGRAPHY

> As the world of science has grown in size and in power, its deepest problems have changed from the epistemological to the social.
>
> —Jerome R. Ravetz, *Scientific Knowledge and Its Social Problems* (p. 10)

One way of sorting out the issues we have raised is to make an explicit analytical distinction between "Big Science" as a rhetorical construction that has pointed to certain features of contemporary science following World War II and "big science" as a generic label for the forces of growth that have propelled the scientific enterprise since the seventeenth century. Once this distinction is made, the question becomes how to relate these two perspectives in a meaningful and useful way. In simple terms, the challenge is to explain simultaneously how science has grown larger and why it seems especially large now.

Perhaps the best way to begin is to be more sensitive about the different qualities of "bigness" in science and the distinctions that can be made among scale,

[57] William H. Goetzmann, *Army Exploration in the American West, 1803–1863* (New Haven: Yale Univ. Press, 1959), pp. 5, 305.

[58] Price, *Little Science, Big Science . . . and Beyond.* (cit. n. 8), p. 3.

[59] See Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton Univ. Press, 1985).

scope, and significance. In traditional usage Big Science has carried the sense of "large masses in one place" and the industrial connotations associated with scaling up. Such large-scale science requires the vertical integration of people and material resources; it coincides with the centralization of control and the concentration of work processes within a circumscribed locale. In contrast, activities that are broad in scope, such as scientific exploration, are big in the sense that they require coordination among geographically dispersed investigators or facilities. They rely on extensive communications networks and decentralized work processes; horizontal integration is their hallmark. Almost by definition, if a scientific project is sufficiently broad in scope or large enough in scale, it will be considered "big" in terms of its significance or impact.

Recent scholarship in science studies offers several potentially fruitful concepts and methods to address these new historiographic concerns. Despite the diversity of the approaches highlighted below, they all share a common focus on science as a form of social practice rather than as an abstract structure of knowledge. In so doing, they stress the material conditions and organizational processes of scientific work.

Whatever their mode of organization, big scientific projects always incorporate a heterogeneous collection of human and material resources—in fact, it could be argued that it is the process of coordinating a relatively large collection of such elements that constitutes "big" science. Such "heterogeneous engineering" has been explored by the sociologist John Law in an effort to develop a model of technological change that privileges neither nature nor society, but sees them in dynamic interplay. Of fundamental importance in this approach is the idea of "networks," of people and things, becoming longer or stronger (i.e., more stable and enduring) that Michel Callon, Bruno Latour, and others have developed. This has obvious application in the study of the growth of science.[60]

Another way of conceptualizing the growth of big science is in terms of the systems approach developed by the historian of technology Thomas Hughes. His emphasis on the dynamics of technological change, including the notions of reverse salients and technological momentum, and on the pursuit of the values of system, order, and control provides a useful perspective in exploring the effects of size in science. Furthermore, given that technological factors, including techniques as well as instruments and infrastructure, often play a central role in research, the work of Hughes and others making the "technological turn" in science studies is especially relevant.[61] Indeed, scholars have seriously questioned a hard and fast distinction between science and technology as they come to appreciate

[60] John Law, "Technology and Heterogeneous Engineering: The Case of Portuguese Expansion," in *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology,* ed. Wiebe E. Bijker, Thomas P. Hughes, and Trevor Pinch (Cambridge, Mass.: MIT Press, 1987), pp. 111–134; and more generally, Michel Callon, John Law, and Arie Rip, eds., *Mapping the Dynamics of Science and Technology: Sociology of Science in the Real World* (London: Macmillan, 1986).

[61] Thomas P. Hughes, "The Evolution of Large Technological Systems," in *Social Construction of Technological Systems,* ed. Bijker, Hughes, and Pinch (cit. n. 60), pp. 51–82. See also Hughes, *Networks of Power: Electrification in Western Society, 1880–1930* (Baltimore: Johns Hopkins Univ. Press, 1983); Hughes, *American Genesis: A Century of Invention and Technological Enthusiasm, 1870–1970* (New York: Viking, 1989); Steve Woolgar, "The Turn to Technology in Social Studies of Science," *Science, Technology, & Human Values,* 1991, *16*:20–50; and E. J. Woodhouse, "The Turn toward Society? Social *Re*construction of Science," *ibid.,* pp. 390–404.

the interplay between theory and experiment, ideas and instruments that characterizes both modern science and modern engineering. In thinking about big science, it might be helpful to appropriate Latour's handy term "technoscience" to label the processes involved.[62]

Aside from a surge of interest in the rise of industrial research, surprisingly little attention has been directed toward the economics of science, particularly in historical perspective. Although there are signs that the "cost-of-research index" in terms of per capita expenditures on scientific personnel has increased significantly over the last century, only a few, scattered attempts have been made to determine investment and productivity trends in science. Such measures could provide a useful means of comparing the scale of scientific activities over time and across cultures.[63] However, the new literature on the history of U.S. industrial laboratories illustrates how science was directly harnessed for economic gain by several major corporations.[64] The organization and production of knowledge in such contexts bears more than a passing resemblance to Big Science; the genetic links deserve to be explored further. Another promising way to connect science and economic growth is through the study of "science regions," geographically defined areas with a heavy concentration of research and development activities.[65]

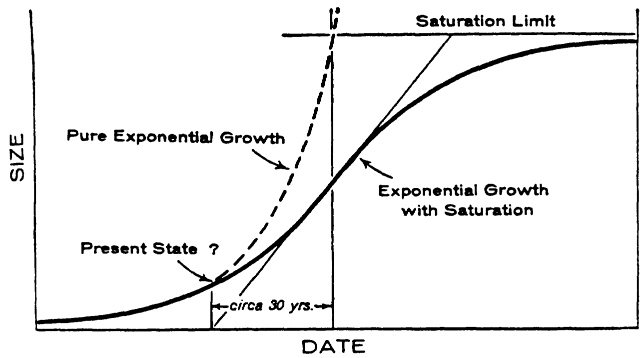
Recent work on the intellectual economy and social relations of the laboratory offers new insights concerning size in science. Peter Galison has made several case studies of research in physics that consider the effects of larger equipment and longer experiments on laboratory practice. He notes that big machines are difficult to modify, thus their design and construction represent compromises between "suitability" for current problems and "flexibility" for future ones. Like Galison, Latour is concerned with how knowledge is made in the laboratory, but he focuses more on the laboratory as a source of social power. He sees the laboratory as a means of exerting influence in the world because it destabilizes existing relations between knowledge and interests. If scientists can persuade other people

[62] Bruno Latour, *Science in Action* (Cambridge, Mass.: Harvard Univ. Press, 1987), pp. 29, 174–175.

[63] Data on the "cost-of-research index" suggest that between 1950 and 1960 U.S. spending on R and D increased approximately 4½ times while the amount of R and D performed doubled; Dael Wolfle, "How Much Research for a Dollar?" *Science,* 1960, *132*:517. See also H. S. Milton, "Cost of Research Index, 1920–1965," *Operations Research,* 1966, *15*:977–991. Forman, "Behind Quantum Electronics" (cit. n. 6), p. 171, presents a graph of the "bigness" of research in terms of support personnel and funding per researcher. In the laboratories of the Atomic Energy Commission the average investment in plant and R and D costs per employee grew from $16,415 in 1951 to $36,104 in 1959; calculated from Seidel, "A Home for Big Science" (cit. n. 37), p. 162. On research productivity in physics see Forman, Heilbron, and Weart, "Physics *circa* 1900" (cit. n. 13), pp. 115–128. Some economic indicators are presented in Thackray *et al., Chemistry in America, 1876–1976* (cit. n. 13).

[64] David A. Hounshell and John K. Smith Jr., *Science and Corporate Strategy: Du Pont R&D, 1902–1980* (Cambridge: Cambridge Univ. Press, 1988); Leonard S. Reich, *The Making of American Industrial Research: Science and Business at GE and Bell, 1876–1926* (Cambridge: Cambridge Univ. Press, 1985); and George Wise, *Willis R. Whitney, General Electric, and the Origins of U.S. Industrial Research* (New York: Columbia Univ. Press, 1985). See also Michael Aaron Dennis, "Accounting for Research: New Histories of Corporate Laboratories and the Social History of American Science," *Soc. Stud. Sci.,* 1987, *17*:479–518.

[65] See Robert Kargon, Stuart W. Leslie, and Erica Schoenberger, "Far Beyond Big Science: Science Regions and the Organization of Research and Development," in *Big Science,* ed. Galison and Hevly (cit. n. 47), Ch. 13. For a theoretical analysis of the economic justifications for R and D see Toulmin, "The Complexity of Scientific Choice II" (cit. n. 32).



Logistic growth curve of modern science. From Derek J. de Solla Price, Science Since Babylon *(New Haven: Yale Univ. Press, 1961), p. 116. Reproduced with permission.*

to detour through their laboratories, metaphorically speaking, to reach some desired goal (e.g., protection from disease), then they amplify their power.[66] Over the past several hundred years scientists have become increasingly successful at arranging such "detours," leading to the current "saturation" of society by science described by Derek Price.

Taking a new tack in the study of the Scientific Revolution, *Leviathan and the Air-Pump,* by Steven Shapin and Simon Schaffer, details the material, literary, and social technologies that enabled the establishment of modern science in the seventeenth century. These knowledge-producing tools—experimental apparatus, published communications, and community standards—were intertwined in the work of Robert Boyle in making natural knowledge. By thinking of the evolution of such inventions from that time to the present, we can explicate the growth of scientific knowledge without recourse to transcendental concepts of progress, and trace the proliferation and extension of the "experimental life" in and beyond Western society.[67]

Instead of further studies on how the scientific enterprise is shaped, influenced, and otherwise affected by forces arising from outside its boundaries, or more research on how the scientific enterprise has affected society, politics, and culture beyond its borders, we ought to be examining more carefully the boundaries and borders themselves. How are such lines negotiated by those who want to enforce them, by those who wish to abolish them, and by those who desire to move back and forth across them?[68] In addressing such questions we can unite the study of the social dimensions of science with the study of the scientific dimensions of society, and perhaps come to a better understanding of how science is made big and how Big Science is made.

[66] Peter Galison, *How Experiments End* (Chicago: Univ. Chicago Press, 1987), pp. 264–265; Bruno Latour, "Give Me a Laboratory and I Will Raise the World," in *Science Observed: Perspectives on the Social Study of Science,* ed. Karin Knorr-Cetina and Michael Mulkay (London: Sage, 1983), pp. 141–170; and Latour, *The Pasteurization of France* (Cambridge, Mass.: Harvard Univ. Press, 1988).

[67] Shapin & Schaffer, *Leviathan and the Air-Pump* (cit. n. 59), pp. 25–79.

[68] See Thomas F. Gieryn, "Boundary-Work and the Demarcation of Science from Non-science: Strains and Interests in Professional Ideologies of Scientists," *American Sociological Review,* 1983, *48*:781–795.