# A Road Not Taken: economists, historians of science and the making of the Bowman report

Backhouse, Roger; Maas, Harro

*DOI:*
10.1086/691421

# A Road Not Taken: Economists, Historians of Science, and the Making of the Bowman Report

**Roger E. Backhouse,** *University of Birmingham and Erasmus University Rotterdam*
**Harro Maas,** *University of Lausanne*

**Abstract:** This essay investigates a hitherto-unexamined collaboration between two of the founders of modern history of science, Henry Guerlac and I. Bernard Cohen, and two economists, Paul Samuelson and Rupert Maclaurin. The arena in which these two disciplines came together was the Bowman Committee, one of the committees that prepared material for Vannevar Bush's *Science—The Endless Frontier*. The essay shows how their collaboration helped to shape the committee's recommendations, in which different models of science confronted each other. It then shows how, despite this success, the basis for long-term collaboration of economists and historians of science disappeared, because the resulting linear model of science and technology separated the study of scientific and economic progress into noncommunicating boxes.

On 20 September 1944 Rupert Maclaurin, Professor of Economics at MIT, wrote to Henry Guerlac, the official historian of the Radiation Laboratory, about his future.

> I would like, therefore, to explore with you the possibilities of your ultimately joining our group here. . . . I am under the impression that you were planning to go back to Wisconsin to finish out your term there, anyway, but my interest is of longer range than that. I believe that if you wanted to switch your field to history of modern science and engineer-

---

Harro Maas is Professor at the Centre Walras-Pareto for the History of Economic and Political Sciences at the University of Lausanne. He has published widely in the history of economics, especially on Victorian political economy. His current interest is in economics as a public science and the intrusion of economic tools in the private sphere. Among his recent publications are *Observing the Economy*, edited with Mary S. Morgan (Duke, 2012), *Economic Methodology: A Historical Introduction* (Routledge, 2014), and *The Making of Experimental Economics: Witness Seminar on the Emergence of a Field*, edited with Andrej Svorenčík (Springer, 2016). CWP-IEPHI, UNIL, 1015 Lausanne, Switzerland; harro.maas@unil.ch.
Roger E. Backhouse is Professor of the History and Philosophy of Economics at Birmingham University and Erasmus University Rotterdam. He is writing an intellectual biography of the economist Paul Samuelson, the first volume of which is scheduled to appear in 2017. Other current projects include editing volumes on the recent turn to applied economics and on liberal economists and the welfare state. With Philippe Fontaine he has edited *The History of the Social Sciences since 1945* (Cambridge, 2010) and *A Historiography of the Modern Social Sciences* (Cambridge, 2014). Department of Economics, University of Birmingham, Birmingham, B15 2TT, United Kingdom; reb@bhouse.org.uk.
*Acknowledgments.* We are grateful to Michael Dennis and Béatrice Cherrier for invaluable assistance. Neither is implicated in the conclusions we have drawn. We would also like to thank Yann Giraud, Michael Gordin, Verena Halsmayer, Tiago Mata, Jessica Wang, participants in seminars at Duke University, the London School of Economics, and Harvard University, three anonymous referees, and the Editor of this journal for helpful comments on earlier versions.

*Isis*, volume 108, number 1.  0021-1753/2017/0108-0005$10.00.

> ing there might be a very interesting career opportunity at M.I.T. In any event, wherever you are after the War, I would like to see now if we couldn't work out some kind of cooperative plan for developing the field. I would like to see a setup which would attract some really able graduate students into the field and would include a live program of research in which the skills and background of the economist and the historian of science would be combined in some way or would at least benefit by cross fertilization of ideas.[1]

This long-term collaboration never happened: economics and the history of science developed largely independently after the war, with significant implications for the study of the linkages between science, technology, and economic change. Guerlac returned to Cornell, where he was instrumental in establishing its history of science program, while the economists at MIT developed approaches to innovation and economic growth that were not informed by history of science. Before their separation, however, a brief window of collaboration between economists and historians opened up when Maclaurin, Paul Samuelson, Guerlac, and I. Bernard Cohen came together in the first four months of 1945 to work on the Bowman Report.

On 21 November 1944, President Roosevelt asked Office of Scientific Research and Development (OSRD) director Vannevar Bush to prepare a report on what the American government could do to support science after the war in order to ensure the long-term safety and prosperity of the American people.[2] One element of Bush's response was to invite the Johns Hopkins geographer Isaiah Bowman, a veteran of such work, to chair a committee that would prepare a report on the third of four questions posed in Roosevelt's letter: "What can the government do now and in the future to aid research activities by public and private organizations?"[3] Maclaurin was invited to act as the committee's secretary. Samuelson was named assistant secretary, and Guerlac was appointed to head up the secretariat, which included Cohen as one of its members, that would undertake research and prepare materials for the committee.[4]

Despite the extensive literature on the Bush Report, the operation of the Bowman Committee, the arena in which our protagonists came together and in which different conceptions of science confronted each other, has not been analyzed. Historians have examined the role of this report in the political processes leading to the establishment of the National Science Foundation (NSF), the authorship of Roosevelt's letter to Bush, and whether or not it affirmed the linear model of the relationship between science and economic growth.[5] This literature on

---

[1] Rupert Maclaurin to Henry Guerlac, 20 Sept. 1944, Henry Guerlac Papers, Cornell University, Ithaca, N.Y., Box 25, Folder 9 (such references hereafter take the form **HGP 25/9**).

[2] Vannevar Bush, *Science—The Endless Frontier: A Report to the President* (1945; Washington, D.C.: National Science Foundation, 1960).

[3] Roosevelt added the rider, "The proper roles of public and private research, and their interrelation, should be carefully considered": quoted *ibid.*, p. 73. Bowman had taken the initiative for the short-lived National Science Advisory Board, headed by MIT president Karl Compton. It had reported on the same question that was now under consideration. See Carroll W. Pursell, Jr., "The Anatomy of a Failure: The Science Advisory Board, 1933–1935," *Proceedings of the American Philosophical Society*, 1965, *109*:342–351.

[4] The secretariat also included the chemist John Edsall and Robert Morison, a biologist and Rockefeller Foundation officer. Remarkably, Samuelson's later account mentioned only Edsall, Morison, and himself as writing the report. See Paul Samuelson, "Three Moles," *Bulletin of the American Academy*, 2009, 58(2):83–89, esp. pp. 83–84. Papers relating to the Bowman Committee are archived with the OSRD Papers, National Archives and Records Administration, College Park, Maryland. Guerlac kept copies of some of these papers, along with notes and minutes not found with the OSRD Papers, and these are now part of the Guerlac archives at Cornell.

[5] On the Bush Report and the NSF see Daniel J. Kevles, "The National Science Foundation and the Debate over Postwar Research Policy, 1942–1945: A Political Interpretation of *Science—The Endless Frontier*," *Isis*, 1977, 68:4–26; Michael A. Dennis, "Historiography of Science: An American Perspective," in *Science in the Twentieth Century*, ed. John Krige and Dominique Pestre (Amsterdam: Harwood Academic, 1997), pp. 1–26; Jessica Wang, "Liberals, the Progressive Left, and the Political

the origins of the report has to a large extent focused on Bush's crafting of his text so as to convey the desired message to his target audiences. The four committees whose reports he drew on are generally depicted as following Bush's preconceived plans or bending to his wishes. For example, in his biography of Bush, G. Pascal Zachary opined that, with the exception of that on health research, the committees were made up of Bush's friends, who were, by and large, already in agreement with his ideas or were willing to go along with them.[6] This was not the case.[7] Important features of the Bowman Report were determined by the scientists on the committee; many of them were indeed Bush's friends and shared his views, but this did not prevent the secretariat, supported by some committee members, from pushing a different line. Even though the secretariat did not get exactly the report it wanted, the final document was not one that the committee would have produced if left to its own devices.

This essay reconstructs the deliberations of the Bowman Committee through the eyes of its secretariat to show how these young scholars—who, with the exception of Maclaurin, were to become stars in their respective disciplines—sought to reconcile competing perspectives on science policy. We then assess the consequences of their work, both for the Bowman Report and for what happened to the two disciplines afterward. First, however, we need to establish the main issues in play in discussions of science policy in the years leading up to the Bush Report and what our four protagonists brought to the table.

## THE POLITICS OF SCIENCE POLICY

Roosevelt's question to Bush—how to safeguard the security and prosperity of the American public through science—has to be seen against general fears among policy makers and economists of a sustained period of stagnation due to a lack of industrial innovation and low population growth. Clearly, the research and development departments of corporate America had contributed substantially to economic growth in the interwar period, but it was generally held that industrial innovation relied on discoveries made by scientists with university positions, even though high-profile scientists were increasingly hired by industrial laboratories.[8] If innovation was sought through science, this raised important questions about its supporting infrastructure and about how to regulate the relations between science and industry. In addition,

---

Economy of Postwar American Science: The National Science Foundation Debate Revisited," *Historical Studies in the Physical and Biological Sciences*, 1995, *26*:139–166; Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War* (Chapel Hill: Univ. North Carolina Press, 1999); and Johanna Bockman and Michael A. Bernstein, "Scientific Community in a Divided World: Economists, Planning, and Research Priority during the Cold War," *Comparative Studies in Society and History*, 2008, 50:581–613. On the authorship of Roosevelt's letter see Kevles, "FDR's Science Policy," *Science*, 1974, *183*(4127):798–800; and G. Pascal Zachary, *Endless Frontier: Vannevar Bush, Engineer of the American Century* (New York: Free Press, 1997). On exclusion of the social sciences from the NSF see Mark Solovey, "Senator Fred Harris's National Social Science Foundation Proposal: Reconsidering Federal Science Policy, Natural Science–Social Science Relations, and American Liberalism during the 1960s," *Isis*, 2012, *103*:54–82; and Solovey, *Shaky Foundations: The Politics–Patronage–Social Science Nexus in Cold War America* (New Brunswick, N.J.: Rutgers Univ. Press, 2013). On the Bush Report and the linear model of innovation see David Edgerton, "'The Linear Model' Did Not Exist: Reflections on the History and Historiography of Science and Research in Industry in the Twentieth Century," in *The Science–Industry Nexus: History, Policy, Implications*, ed. Karl Grandin, Nina Wormbs, and Sven Widmalm (New York: Watson, 2004), pp. 31–57; and Benoit Godin, "The Linear Model of Innovation: The Historical Construction of an Analytical Framework," *Science, Technology, and Human Values*, 2006, 31:639–667.

[6] Zachary, *Endless Frontier*, p. 222.

[7] Bush himself denied that he sought to control the committee, except on the matter of patents, which he reserved for himself. On 23 Mar. 1945 Bush wrote E. B. Wilson saying he left the committee's work "practically entirely in their hands," because "the formulations and studies and thoughts should come from them and not from me," on the grounds that he had no time to "think through the broad matters that are involved": Report to President, OSRD 4/Committee No. 4.

[8] Michael Dennis, "Accounting for Research: New Histories of Corporate Laboratories and the Social History of American Science," *Social Studies of Science*, 1987, *17*:479–518.

the contributions of science to the Great War had raised doubts about science itself. When hostility to science waned in the 1930s, questioning the benefits of science morphed into questioning the merits of its independence from political control, the most notorious statement of this view being *The Social Function of Science* (1939), by J. D. Bernal, a British chemist and member of the Communist Party. This book asked why public money should support the private activities of scientists who were not subject to political control. Bernal aroused much opposition from scientists across the political spectrum, especially in view of the state interference in science in the Soviet Union and Nazi Germany. In 1940 the chemist Michael Polanyi and others established the Society for Freedom of Science in Britain to counter what they considered an embrace of totalitarian control of science. Robert Merton's "A Note on Science and Democracy," in which he originally articulated the famous "scientific ethos," echoed such concerns.[9]

Even before the appearance of Bernal's book, and in response to the changing relations between science and industry, James Conant, president of Harvard, and Karl Compton, president of MIT, each began to promote a different model of science. They both used Henry Rowland's famous distinction between "pure" and "applied" science, which legitimized the scientist's independence from practical goals, they both saw scientists as motivated primarily by curiosity about the world, and they both feared the consequences of political control—but they held contrasting views on the institutional context in which science was best undertaken.[10] Conant focused on the conditions that enabled the individual creative genius to flourish. Harvard created interstitial spaces, such as J. L. Henderson's Fatigue Laboratory and the Society of Fellows, that allowed dissent and debate. Conant's model was the Harvard scholar who, as Jamie Cohen-Cole recently put it, conversed in "a learning environment with 'pleasant rooms' and 'comfortable chairs.'"[11] In contrast, Compton's interest was in the output side of the chain of discovery. Though he too embraced the rhetoric of creative genius, his model of the scientist was that of the laboratory researcher who worked on a long-run collaborative experiment that inevitably needed the hand of an intelligent planner. Creative individuals could function very well in a planned environment. Compton's model scientific setting resembled

---

[9] J. D. Bernal, *The Social Function of Science* (New York: Macmillan, 1939); and Robert Merton, "A Note on Science and Democracy," *Journal of Legal and Political Sociology*, 1942, *1*:115–126. On opposition to Bernal's book, including the formation of the Society for the Freedom of Science, see Mary Jo Nye, *Michael Polanyi and His Generation: Origins of the Social Construction of Science* (Chicago: Univ. Chicago Press, 2011). Polanyi would increasingly move to an interpretation of the scientific process that saw scientists coordinating as if guided by the invisible hand of the market, an interpretation congenial to Friedrich Hayek. In his *Road to Serfdom* (Chicago: Univ. Chicago Press, 1944), Hayek would similarly argue against state interference with science. On resistance of American scientists against Senator Harley Kilgore's proposals see the texts cited in note 5, above. On the different notions of planning see Marcia Balisciano, "Hope for America: American Notions of Economic Planning between Pluralism and Neoclassicism, 1930–1950," in *From Interwar Pluralism to Postwar Neoclassicism*, ed. Mary S. Morgan and Malcolm Rutherford (*History of Political Economy*, Suppl., 30) (Durham, N.C.: Duke Univ. Press, 1998), pp. 153–178.

[10] For nineteenth-century discussions of the distinction between "pure" and "applied" science see the Focus section "Applied Science," *Isis*, 2012, *103*(3). On Rowland's distinction see esp. Paul Lucier, "The Origins of Pure and Applied Science in Gilded Age America," *ibid.*, pp. 527–536.

[11] Jamie Cohen-Cole, "The Creative American: Cold War Salons, Social Science, and the Cure for Modern Society," *Isis*, 2009, *100*:219–262, on p. 254. On the Harvard model promoted by Conant see Joel Isaac, *Working Knowledge: Making the Human Sciences from Parsons to Kuhn* (Cambridge, Mass.: Harvard Univ. Press, 2012); and Cohen-Cole, *The Open Mind: Cold War Politics and the Sciences of Human Nature* (Chicago: Univ. Chicago Press, 2014). For Conant's remarks on the matter see James B. Conant, "President's Address," in *The Tercentenary of Harvard College* (Cambridge, Mass.: Harvard Univ. Press, 1937), p. 70 (the address was delivered on 20 Mar. 1936); and Conant, "The Advancement of Learning in the United States in the Post-War World," *Proc. Amer. Phil. Soc.*, 1944, 87:291–298. On Conant's vision regarding the role of a general science education (and on the relation of the history of science to the sciences) see Christopher Hamlin, "The Pedagogical Roots of the History of Science: Revisiting the Vision of James Bryant Conant," *Isis*, 2016, *107*:282–308. These very different accounts agree on Conant's efforts to create the conditions to enhance the creativity of the individual scientist, as necessary for scientific progress.

that of the industrial research laboratory, which had grown spectacularly in importance in the interwar period, and fits with his and Bush's attempts to enhance the research profile of MIT by strengthening its bonds with government and industry.[12] In line with this, Compton envisioned an important role for the Industrial Relations Section within the recently established Department of Economics and Social Science in reaching an understanding of the chain from scientific discovery to new technologies and products. Patents were of central importance in organizing this chain.

The immediate context of the Bowman Committee's discussions was the introduction, by Senator Harley Kilgore, of a series of bills to establish a national foundation in support of science. Seeing the problem as the monopolistic behavior of large firms, he proposed measures to spread support across the country under a board comprising representatives of different interest groups and to make the fruits of science a national resource instead of privately patentable goods. His proposals were seen by the OSRD scientists Bush, Conant, and Compton as bureaucratic interference that would stifle research by limiting universities' ability to set their own research agendas and to patent their research results.[13] They were agreed on the need for federal science funding, but there was no agreement on the form that funding should take. The OSRD, with its large research laboratories, might have been acceptable in wartime, but there was no consensus that a similar institution would work in peacetime. Moreover, if a government funding body were established, that raised the issue of political control and interference. Also canvassed was the idea of using the National Academy of Sciences (NAS) to distribute funds, but that raised further issues. The problem was seemingly intractable. Inevitably, given that Kilgore was making concrete proposals that involved an unacceptable degree of political control, Bush "seized the opportunity" given by Roosevelt's letter to articulate an appropriate response.[14] Kilgore thus became the bogeyman against which the committees formulated their reports.

## THE SECRETARIAT MEMBERS

### The Economists

All of our protagonists, like many of the scientists involved in the Bowman Committee, had strong links to Harvard and MIT. Apart from their availability at a time when many were

---

[12] See Christophe Lécuyer, "Patrons and a Plan," in *Becoming MIT: Moments of Decision*, ed. David Kaiser (Cambridge, Mass.: MIT Press, 2010), pp. 59–80; Lécuyer, "Academic Science and Technology in the Service of Industry: MIT Creates a 'Permeable' Engineering School," *American Economic Review*, 1998, 88(2):28–33, esp. p. 31; and John W. Servos, "The Industrial Relations of Science: Chemical Engineering at MIT, 1900–1939," *Isis*, 1980, *71*:531–549. See also David Noble, *America by Design: Science, Technology, and the Rise of Corporate Capitalism* (New York: Knopf, 1977); Dennis, "Accounting for Research" (cit. n. 8); Henry Etzkowitz, *MIT and the Rise of Entrepreneurial Science* (New York: Routledge, 2002); and Steven Shapin, *The Scientific Life: The Moral History of a Late Twentieth-Century Vocation* (Chicago: Univ. Chicago Press, 2009), esp. Chs. 4 and 5. Etzkowitz investigates the model of an entrepreneurial university that, he argues, was conceived and developed by Bush and Compton. Noble's book has served as an important reference for work that discredited the linear model that is commonly considered a result of the Bush Report.

[13] On Kilgore's initiatives see Robert Franklin Maddox, "The Politics of World War II Science: Senator Harley M. Kilgore and the Legislative Origins of the National Science Foundation," *West Virginia History*, Fall 1979, *41*:20–39. On the OSRD scientists' worries see Kevles, "National Science Foundation and the Debate over Postwar Research Policy" (cit. n. 5), p. 7; Wang, "Liberals, the Progressive Left, and the Political Economy of Postwar American Science" (cit. n. 5), p. 142; and Maddox, "Politics of World War II Science." Kilgore's proposals did not even enable his allies in the War Production Board to create a strong research arm within their own agency. See David M. Hart, *Forged Consensus: Science, Technology, and Economic Policy in the United States, 1921–1953* (Princeton, N.J.: Princeton Univ. Press, 2010), p. 159. The stance of Thurman Arnold, Roosevelt's chief "trust buster," explained to a Congressional committee in 1943, was shared by Vice President Henry Wallace, who similarly held that patents were misused by the large firms that controlled research. See Kevles, "National Science Foundation and the Debate over Postwar Research Policy," pp. 6–7.

[14] Maddox, "Politics of World War II Science," p. 31.

serving in the armed forces, these men were obvious choices for the secretariat. Maclaurin, the son of former MIT President Richard C. Maclaurin and a family friend of Compton, had studied at Harvard with Joseph Schumpeter, famous for his theory that the process of capitalist development was driven by waves of innovation.[15] As head of the Industrial Relations Section in MIT's Department of Economics and Social Science, he had been conducting research, supported by Compton and funded by the Rockefeller Foundation, on the economics of technological change.[16]

Compton had repeatedly emphasized the connection between pure research and the public good. Pure research, not driven by self-interest and uncertain in its results, had far-reaching consequences for a country's prosperity. In his address "Science and Prosperity," presented to the American Association for the Advancement of Science in Berkeley on 21 June 1934, he had quoted approvingly Faraday's prediction that "electricity will pay taxes," providing a rudimentary specification of the causalities of this connection:

> Most of the basic discoveries have been made by professors in educational institutes, spurred on to their investigations by insatiable scientific curiosity. Most of the rest has been done in industrial laboratories, especially recently, and governmental laboratories have played an important rôle, especially in fixing standards of measurement. The industrial laboratories have taken the lead in developing useful applications of the discoveries. Here and there have sprung up inventive geniuses. All this work has brought enormous returns to the public.[17]

Using electricity and the engineering arts to exemplify how investigations driven by curiosity ultimately raised the quality of life and provided employment to "millions of people," Compton had emphasized that the trajectory from basic discovery to application could be much improved if properly understood. Comparing the American federal government with Maxwell's famous demon, Compton argued that the former, "by intelligent operation of economic and legislative controls," could "bring order and power out of the chaos into which the country would necessarily drift" in its absence. Even if he agreed with Conant that fundamental breakthroughs in science were "almost impossible to plan," this did not mean that the ensuing process to application could not be managed and controlled.[18]

Maclaurin's project was to detail this process, thereby helping engineering students at MIT understand the institutional conditions necessary to turn light into fruit. He had outlined this vision at a session organized for the American Economic Association (AEA) in December 1941. At that session, Roosevelt's economic advisor, Lauchlin Currie, taking up an argument made by the Harvard economist Alvin Hansen before the war, had warned that the closing of the frontier and the resulting demographic transition made it necessary for the American economy to develop new industries. Other participants suggested that "many new openings could be developed if we spent a significant portion of our national income on scientific research

---

[15] The family lineage goes back to Colin Maclaurin, the eighteenth-century Scottish mathematician who became famous for his geometric rendering of Newton's *Principia*. After Richard Maclaurin's premature death, Compton, Jerome Hunsaker, and E. B. Wilson took care of his family.

[16] See Roger E. Backhouse and Harro Maas, "Marginalizing Maclaurin: The Attempt to Develop an Economics of Technological Progress at MIT, 1940–1950," *History of Political Economy*, 2016, 48:423–447. Compton's interest in setting up a program that would combine research and education in the process of innovation fit with David Hart's larger story treating the context of American science and technology policy; see Hart, *Forged Consensus* (cit. n. 13).

[17] Karl T. Compton, "Science and Prosperity," *Science*, 1934, *80*(2079):387–394, on p. 389.

[18] James B. Conant, "Role of Science in Our Unique Society," *Science*, 23 Jan. 1948, N.S., *107*(2769): 77–83, on p. 80.

and the training of scientists." Maclaurin concluded that "it was agreed that considerably more basic research was needed," for which industry was unlike to pay the bill. To enhance basic research, the federal government would have to step in.[19] He understood planning in terms of generic government support of fundamental research rather than targeted support for specific projects or sectors of industry. Clearly, federal support for fundamental research at industrial laboratories was out of the question if industry alone would reap the benefits from patents funded with public money.

Concerns about the public organization of scientific research were also prominent in Samuelson's thinking on the role of science in a nation's prosperity. Samuelson was a young mathematical economist that MIT had recently recruited from Harvard, where he had overlapped with Guerlac at the Society of Fellows. He had initially participated in Maclaurin's project and acted as a consultant for the discontinued National Resources Planning Board, where he studied the economy's prospects after the war, but by 1944 he was working as a mathematician in the Radiation Laboratory. Earlier that year, he took an active interest in the committee chaired by the president of General Electric (GE), Charles Wilson, to report on the funding of military research after the war. In a letter to a contact at the National Planning Association that clearly reflected discussions inside the Radiation Laboratory, Samuelson expressed concern that the Wilson Committee might come up with a proposal that was too modest. Samuelson believed that science needed greater organization than the scientists on the committee, who prioritized freedom from government interference, were prepared to admit. He ended up expressing his view in an unsigned editorial in the *New Republic*: "A good many well known scientists . . . take their coloration from the conservative business men who are their associates, and seem terribly alarmed lest government aid to scientific research should interfere with the sacred fetish of 'private enterprise.' There is a real danger that they may cripple scientific research by turning it over to the same auspices which helped bring us to such a perilous condition in 1941."[20]

A temporary board, of the sort suggested by the Wilson Committee, with members appointed by the National Academy of Sciences, was like suggesting "that the carpenters' union should elect members of a board which is to plan public works." Science was a public good and, paraphrasing Clemenceau, "too important to be left to scientists." If scientists did not come up with an adequate program because they were scared it might lead to unwelcome government interference, there was the danger that others who did not properly understand the nexus between science and industry (presumably he had Kilgore in mind) might do so, with detrimental results. For Samuelson, the main issue was not to preserve individual freedom as a necessary condition for fundamental research but, rather, to acknowledge the relation between planned military research and its spin-offs in the economy at large: "military research should be simply the opening wedge in a forward program of encouraging technical progress." Drawing on his own experience at the Radiation Laboratory, he claimed that "centralized research in OSRD laboratories" was much more effective than research funded by federal grants-in-aid to universities. Managed science could be successful. When challenged that the government should not operate its own laboratories, instead contracting research to existing organizations, Samuelson

---

[19] W. Rupert Maclaurin, "Economics of Industrial Research," *Amer. Econ. Rev.*, 1942, 32(1[2]):231–232, on p. 232. See also Alvin H. Hansen, "Economic Progress and Declining Population Growth," *ibid.*, 1939, 29(1):1–15.

[20] Paul Samuelson to John Coil, 6 Aug. 1944, Paul A. Samuelson Papers, Rubenstein Library, Duke University, Durham, N.C., Box 19, Folder C(1941–51) (hereafter abbreviated as **PASP 19/C[1941–51]**); and [Paul A. Samuelson,] "Science and the National Defense," *New Republic*, 1 Jan. 1945, pp. 7–8. On Samuelson more generally see Harro Maas, "Making Things Technical: Samuelson at MIT," *Hist. Polit. Econ.*, 2014, 46(Suppl.):272–294; and Roger E. Backhouse, "Paul Samuelson's Move to MIT," *ibid.*, pp. 60–77. See also Backhouse, *Founder of Modern Economics: An Intellectual Biography of Paul A. Samuelson*, Vol. 1: *Becoming Samuelson, 1915–48* (Oxford: Oxford Univ. Press, 2017).

responded that the Radiation Laboratory's being at MIT was "a mere formality." It "has little to do with M.I.T. and it *is* the OSRD." He continued, "Ask any producing scientist whether grants in aid to the Applied Mathematics Panel or to, say, the Radio Research Laboratory at Harvard are more productive. He is sure to select the latter and this is always the case."[21]

Shortly after Samuelson had raised the issue of the Wilson Committee giving away too much to private enterprise, Maclaurin told Guerlac that he had had "a number of discussions recently with Dr. Jewett and Dr. Compton concerning the post-war organization of science in this country." In consultation with Compton and probably with Samuelson, Maclaurin drafted a proposal to investigate the claim that universities were important for fundamental research. His memorandum proposed a survey of research programs in the natural sciences and engineering that were carried out at premier American universities before the war. Echoing Compton's remarks on the relation between science and economic growth and comments by men of business at his own AEA session, it claimed that fundamental research was of prime importance "to advance the higher standards of living" in the United States, for the creation of new investment opportunities, and for the development of new products. There were, however, important reasons to fear that fundamental research at universities had come under pressure in the prewar era because of lowered income flows from university endowments and decreased bequests from individuals. Even though new funds had been found through collaboration with industry—with the additional "healthy effect" that scientists were forced to pay closer attention to "actual industrial problems" and the achievement of "practical results"—this could have come at the cost of fundamental research.[22]

The sting came in the second part of the proposal. It was not clear that universities were using their funds efficiently, and there was, to date, no assessment showing that universities did in fact support the "long range" programs in which they claimed a "comparative advantage" over industry. Neither was there any assessment of "the nature and adequacy of [the universities'] total research contributions in different fields." The memorandum outlined a survey of major university research centers, focusing on the adequacy of their teaching and (long-term) research programs in the natural and life sciences and engineering, in order to determine the "nature of the university research problem in its relation to future industrial needs" and possible remedial action.[23] The similarities between these questions and those that would be tackled by the Bowman Committee are striking.

## The Historians

We have seen that for the economists the emphasis was not on the freedom of the individual scientist to pursue his or her own curiosity, the issue that was central to the different visions of the relationship between science and the state that was brought to the secretariat by Guerlac and Cohen.[24] They had come to know one another in the late 1930s through taking classes on the history of science in the Renaissance with the Italian émigré scholar Giorgio Diaz de Santillana, then a visiting lecturer at Harvard. Cohen, taken by Santillana's ideas about the

---

[21] [Samuelson,] "Science and the National Defense," p. 8; and Samuelson to Bruce Bliven, 30 Nov. 1944, PASP (*New Republic*) (emphasis in the original). The Radio Research Laboratory (RRL) was a spin-off from MIT's Radiation Laboratory. Samuelson wrongly seems to imply that the Applied Mathematics Panel did not fall under the OSRD. On the RRL see Roger L. Geiger, *Research and Relevant Knowledge: American Research Universities since World War II* (Oxford: Oxford Univ. Press, 1993). On the effectiveness of OSRD research see Larry Owens, "The Counterproductive Management of Science in the Second World War: Vannevar Bush and the Office of Scientific Research and Development," *Business History Review*, 1994, *68*:515–576.

[22] Maclaurin to Guerlac, 20 Sept. 1944, HGP 25/9; and "Memorandum," HGP 25/9.

[23] "Sample accompanying letter" and "Memorandum," HGP 25/9.

[24] Dennis, "Historiography of Science" (cit. n. 5), discusses the different attitudes of Cohen and Guerlac to history.

perpetual brotherhood of scientists that could thrive only when they were granted freedom of thought, had been the first to enroll in the history of science graduate program supported by Conant.[25] He used historical case studies to teach students what Conant would later call the unchanging "tactics and strategies" of science. In 1941 he took on the task of writing a primer in the history of science that was commissioned by the NAS to promote its newly established private fundraising program, the National Science Fund, by convincing the general public of the benefits of science. This was eventually published as *Science: Servant of Man* (1949). Cohen sought to reclaim science's independence and neutrality, challenged by arguments about the relations between science and war, by arguing that what mattered was "pure science"—even when the distinction between pure and applied was gradual and not clear-cut. He agreed with Conant that real progress in science—genuine breakthroughs—could be made only if the scientist was not forced to think about practical applications of his work—that is, when he was free to determine his own research agenda. Cohen could therefore only "sigh" at science writers such as J. B. S. Haldane and Bernal, who maintained that scientists could not expect to be paid "merely to amuse themselves." The "glorious pages of the history of science" showed that pure science—the unplanned research activities of the bright and curious—would eventually bear fruit in useful inventions.[26] This was also the message of a book-length manuscript, "American Science and War," on which Cohen had been working with a student of Robert Merton, Bernard Barber, in the early 1940s.[27] Work on this manuscript would strain relations with Guerlac, whose thesis was precisely about the interrelations between war and science and who came to know about Cohen and Barber's investigations only when Cohen asked for clarifications on the involvement of French engineers in the American War of Independence.[28] The outline of its contents showed the authors' concern with the flurry of publications before the war, by Bernal, Haldane, Merton, and others, that problematized the social function of science in a modern society, especially scientists' claims for freedom and independence from political control.

Guerlac's interests turned to history of science while he was a Junior Fellow at Harvard, from 1935 to 1938. In his thesis, "Science and War in the Old Regime: The Development of Science in an Armed Society," submitted in 1941, on the establishment of the École de Mésières and the education of the French corps of engineers, Guerlac analyzed the historically intricate relations between science, war, and the state in an era in which war was considered an inevitable part of everyday life. Referring to the work of Leonardo, Stevin, Galileo, and earlier writers on engineering problems, he argued that the link between science and warfare

[25] For this history see I. B. Cohen Papers, Harvard University, Cambridge, Mass., Box 3, Folder: History of Science at Harvard, undated; and Cohen, "A Harvard Education," *Isis*, 1984, 75:13–21.

[26] I. B. Cohen, *Science: Servant of Man* (London: Sigma, 1949), p. 312 (quoting Bernal). See Dennis, "Historiography of Science" (cit. n. 5), p. 9, for details on the project.

[27] A manuscript of that proposed book, dedicated to George Sarton, circulated in 1942 among a Washington audience. Cohen later published parts of it. See I. B. Cohen, "Science and the Revolution," *Technology Review*, 1945, 47:367–368, 374–378; "Science and the Civil War," *ibid.*, 1946, 48:167–170, 192–193; "American Physicists at War: From the Revolution to the World Wars," *American Journal of Physics*, 1945, 13:223–235; and "American Physicists at War: From the First World War to 1942," *ibid.*, pp. 333–346. Bernard Barber published his own sociologically informed *Science and the Social Order* (New York: Free Press) in 1952; we have used the 1953 British edition.

[28] This is clear from a furious letter from Guerlac to Cohen: "But then you have the audacity, not only to tackle a general job on the science and war business, on which notably I have been doing a certain amount of work, and do it collaboratively without even mentioning it to me, let alone asking me to participate (which again is your privilege), but you have the indescribable *toupée* to ask me to let you pillage my thesis for background material. What a nerve!" Cohen quoting Guerlac in an appeasing reply dated "Friday night," Nov. 1941, HGP 25/23–24. Cohen destroyed his copy of Guerlac's letter.

had deep roots. For example, he thought it no exaggeration to consider Galileo's *Dialogues concerning Two New Sciences* (1638), a landmark in the so-called scientific revolution, a work on warfare: "The purpose of the treatise is revealed when, in the last section, he applies his carefully developed laws of motion to the study of projectiles."[29]

Guerlac noted approvingly how writers such as Lancelot Hogben, Joseph Needham, Merton, and Bernal had started to approach their subject "with interpretive purpose, a sociological curiosity, and the resolve to explore the religious, social and economic forces which have influenced the rise of science." This literature was important in that it signaled "the need for a broad sociological and historical approach to the problem of the development of science." He contended that Bernal's "ambitious survey" of the social function of science had presented "for the first time . . . a realistic attitude towards science and war." The reason this "realistic attitude" was not more widely held was that "under the spell of the Idea of Progress," which Guerlac attributed to the "relatively peaceful" nineteenth century, science had come to be seen to promise "the elimination of war and the creation of the perfect society." Even after the devastations of the Great War of 1914–1918, the idea of science's beneficial progress quickly caught on again—even though, as Guerlac showed through looking at the case of France, "the tradition of military engineering in Western Europe is centrally important for the history of the interaction of science and war."[30]

On these issues Guerlac had come to conclusions that differed dramatically from Cohen's. His main thesis concerned the intricate relations between scientific progress, state planning, and war in seventeenth- and eighteenth-century France. In contrast to the received view, cherished by Cohen and Conant, of the Enlightenment as having established scientists' independence from the state, Guerlac showed how the organization of science in France under Colbert became a closely administered process, from which emerged large collaborative war-related projects, such as Cassini's famous map of France and Lavoisier's *régie des poudres et salpêtres*.[31] Guerlac followed the centralized organization of science in France through its publication practices and the educational reform of its engineering schools, especially the École de Mézières, the predecessor of the École Polytechnique. Science was planned and war related, and scientists—even in the Age of Enlightenment—had no problem taking part in such collaborative, planned, and highly consequential research programs. They were creative in the service of the state.

In an undated and unsigned lecture he must have delivered around or after Conant's Franklin Medal Lecture of 1943, in which Conant used the example of Nazi science to justify the need to keep science free from government interference, Guerlac examined the sea change that had taken place in American science between the creation of the NAS in 1916 and America's involvement in World War II. Exploiting Conant's painful comparison of American science with that of Nazi Germany, Guerlac explained how American scientists had *de facto*

---

[29] Henry Guerlac, "Science and War in the Old Regime: The Development of Science in an Armed Society" (Ph.D. diss., Harvard Univ., 1941), p. 61. Guerlac chose to write a thesis in European history "to see if I could meet the standards of a regularly established field" and because he thought the Department of History would provide better technical and methodological training. Guerlac to Edward Mead Earle, 20 Oct. 1952, HGP 26/11–12. A third reason was that he believed Sarton to be too busy to give much time to graduate students. See also Marie Boas Hall, "Eloge: Henry Guerlac, 10 June 1910–29 May 1985," *Isis*, 1985, 77:504–506.

[30] Guerlac, "Science and War in the Old Regime," pp. i–ii, vi, iii–iv, 2. Barber's *Science and the Social Order* (cit. n. 27) would contribute to the literature reflecting the need for a sociological and historical approach.

[31] On Lavoisier's *régie des poudres* see Patrice Bret, "Lavoisier à la régie des poudres: Le savant, le financier, l'administrateur et le pédagogue," 1994, halshs-00002883.

been "marshalled and mobilized to help fight this war" and how, under the pressure of circumstances, the organization of science had moved in the same direction as in Nazi Germany, where there had been a shift from (state-funded) theoretical research toward research into practical problems that could find application in the near future. Thus the Nazis developed research programs that were "rigidly controlled" and "subject to a highly centralized supervision." It would be wrong, however, to conclude from this that Nazi Germany held science in low esteem. Implicitly referring to Conant, Guerlac contended that, though Americans "cried out" at what they saw as Nazi betrayal of the values of science, it would not take long for American science to reorganize itself in similar fashion. This reorganization would not be easy, for—and here Guerlac agreed with Barber and Cohen—America had no tradition of planned science, decentralization being "invariably the rule."[32] However, after the start made during Roosevelt's New Deal, science was ready to mobilize by 1940—even before America had been drawn into the war.

In June 1940 Roosevelt created the National Defense Research Council (NDRC)—headed by a strong opponent of peacetime planning of science, Conant—which "farmed" contract research to research centers, "much as production contracts are let to various industries." The traditional distinction, if it ever existed, between research in universities and research in government and industrial laboratories had vanished. Science was being organized and planned in collaborative efforts of groups of scientists for specific purposes, rather than being dependent on the independent mind of the independent scholar. The organization of science increased further with the creation of the OSRD, headed by Bush. Guerlac's additional message was that, well before Pearl Harbor, the wartime organization had betrayed the alleged open culture cherished by the proponents of free science by beginning to function in a climate of secrecy. Possibly referring to his inside knowledge of the Los Alamos project, acquired during his work as the Radiation Laboratory's official historian, Guerlac closed by saying that just enough was known about the achievements of organized science "to prove that great things are in store for the Axis."[33]

Guerlac, therefore, considered it a mistake to see the OSRD as the exception instead of the rule: the wartime experience should radically change thinking about the postwar organization of science in America. The difference between those who took this position, which Guerlac shared with Samuelson, and those who stressed the need for scientists to be free of any constraints on where their curiosity might lead was to be a major preoccupation of the Bowman Committee.

## THE SECRETARIAT AND THE COMMITTEE

### The Operation of the Committee

In addition to its chairman and secretary, the Bowman Committee numbered fifteen. There were two physicists: Vice Chairman John Tate and Isaac Isidor Rabi, heavily involved in the Manhattan Project and winner of the 1944 Nobel Prize for Physics. Industry was strongly represented—for example, by Oliver Buckley of Bell Telephone Laboratories and Edwin Land of Polaroid. Also involved were Warren Weaver from the Rockefeller Foundation, William Wrather from the U.S. Geological Survey, several university administrators, a former White House lawyer, and the economist Harold Moulton, president of the Brookings Institution.

---

[32] Undated and unsigned lecture with notes and corrections in Guerlac's longhand, HGP Box 25/3. For Conant's Franklin Medal Lecture see Conant, "Advancement of Learning in the United States in the Post-War World" (cit. n. 11).

[33] Undated and unsigned lecture with notes and corrections in Guerlac's longhand, HGP Box 25/3.

**Table 1.** Subcommittees and Chair

| | |
|---|---|
| 1. *Government assistance for research in universities and nonprofit research institutions* | Rabi |
| 2. *Government assistance to government research* | Wrather |
| 3. *Government assistance to research in industry* | MacQuigg |
| 4. *Tax stimuli to industrial research* | Dewey |
| 5. *Change of patent system* | (See below) |
| 6. *Economic relationships between scientific research and industrial development* | Maclaurin |
| 7. *Proper roles of public and private research* | Wrather |
| 8. *Instrumentality problem* | Cox |
| 9. *Government policy to further international exchange of science and engineering knowledge* | Haskins |

Adapted from HGP 25/6-11, pp. 3–4. The table shows nine subcommittees, of which Subcommittee 5 *de facto* consisted only of Bush. He wrote a memorandum for the Bowman Committee that was, not to his discontent, only marginally integrated in the final report. There were some so-called additional problems identified that were not assigned to specific committees, to which the secretariat predominantly contributed.

Much of the work was done by a steering committee, the main committee meeting only twice. At its first meeting, on 26 December 1944, the steering committee, comprising Bowman, Tate, Maclaurin, Buckley, Rabi, and Weaver, broke the president's question into three: What should government do to encourage fundamental research in natural science and engineering in universities? What research should government do itself? What should government do to stimulate research in private industry? They took for granted Compton's claim that "a study of the history of scientific development" clearly indicated "that many important new industries have had their origins in fundamental research" by individual scientists at universities. However, the distinction between fundamental and applied research was disputed, as was the question of whether fundamental research was best undertaken at universities or in government-financed laboratories.[34]

Samuelson, presumably in consultation with Maclaurin, set the agenda for the second meeting of the steering committee, on 3 January 1945, which discussed a memorandum he had prepared. It was decided that subcommittees, listed in Table 1, would prepare reports on different parts of the problem, which a working group would then combine into a draft report to be considered by the whole committee. Though the full committee met on 13 January, Samuelson's memorandum had come close to defining the topics that the subcommittees would work on.[35]

Committee members contributed working documents to these subcommittees and were assisted by members of the secretariat and external experts. For example, Maclaurin's subcommittee, investigating the economic relationships between scientific research and industrial development, turned to the Harvard economist Wassily Leontief. It is not known which members of the secretariat participated in the subcommittee meetings, but the minutes indicate that Guerlac and possibly Samuelson were present at several of them. Cohen was consulted by

[34] Minutes of committee meeting, 18 Jan. 1945, HGP 25/10.
[35] Untitled typescript, 1 Jan. 1945, HGP 25/6. Samuelson's name is handwritten with the date at the end of the document.

Guerlac, especially on the history of science in America, but he did not remember having been present in any of the meetings.[36] He assisted Rabi in a survey of prewar research activities in universities and collaborated with the chemist John Edsall and Leontief on an assessment of the organization of science in Europe. Guerlac wrote a memorandum on earlier proposals to support science in which he did not conceal his enthusiasm for the recommendations of Compton's Science Advisory Board of 1933–1935. Bush considered the issue of patents too important to entrust to others and kept this for himself.

A draft report was discussed at the second meeting of the full committee on 26–27 March. Between that meeting and the second week of April, drafts went back and forth between the secretariat and Bush's secretary Carroll Wilson and between committee members, who wrote memoranda on specific issues (see Figure 1). A revised draft was sent to Bush and members of the full committee on 20 April, and then, after further revisions, a final version was sent to Bush on 9 May.[37]

### The Issues at Stake

Terminology was clearly important, for it echoed conflicting views on what sort of research was in need of government support, the appropriate institutional setup, and whether certain types of research could be planned. At its first meeting, the steering committee considered the notion of "fundamental" science, which was directed at the discovery and understanding of "new properties and attributes of nature." However, it quickly settled on the distinction between "pure" and "applied," where "pure" also entailed the freedom of the individual scientist to pursue his own interests, resisting the guiding hand of state or industry. A suggestion, possibly by Samuelson, that Guerlac would be a good person to define the distinction between the pure and the applied was not taken up, even though it was agreed that the secretariat would prepare a memorandum and search for additional documentation and data on the "pure science problem." Instead, Weaver and Rabi each prepared a memorandum on pure science, while Buckley prepared notes on applied research. Weaver's distinction between the pure and the applied would make it to the final report, though the document occasionally referred to "fundamental" and "basic" research as well, reflecting the lack of agreement within the committee and the secretariat. When a committee member criticized the distinction between pure and applied science in the penultimate draft report, Guerlac explained in a letter copied to Samuelson that unfortunately, apart from minor tweaking of the text, little could be done because the steering committee "had already pronounced *ex cathedra* on terms and definitions."[38]

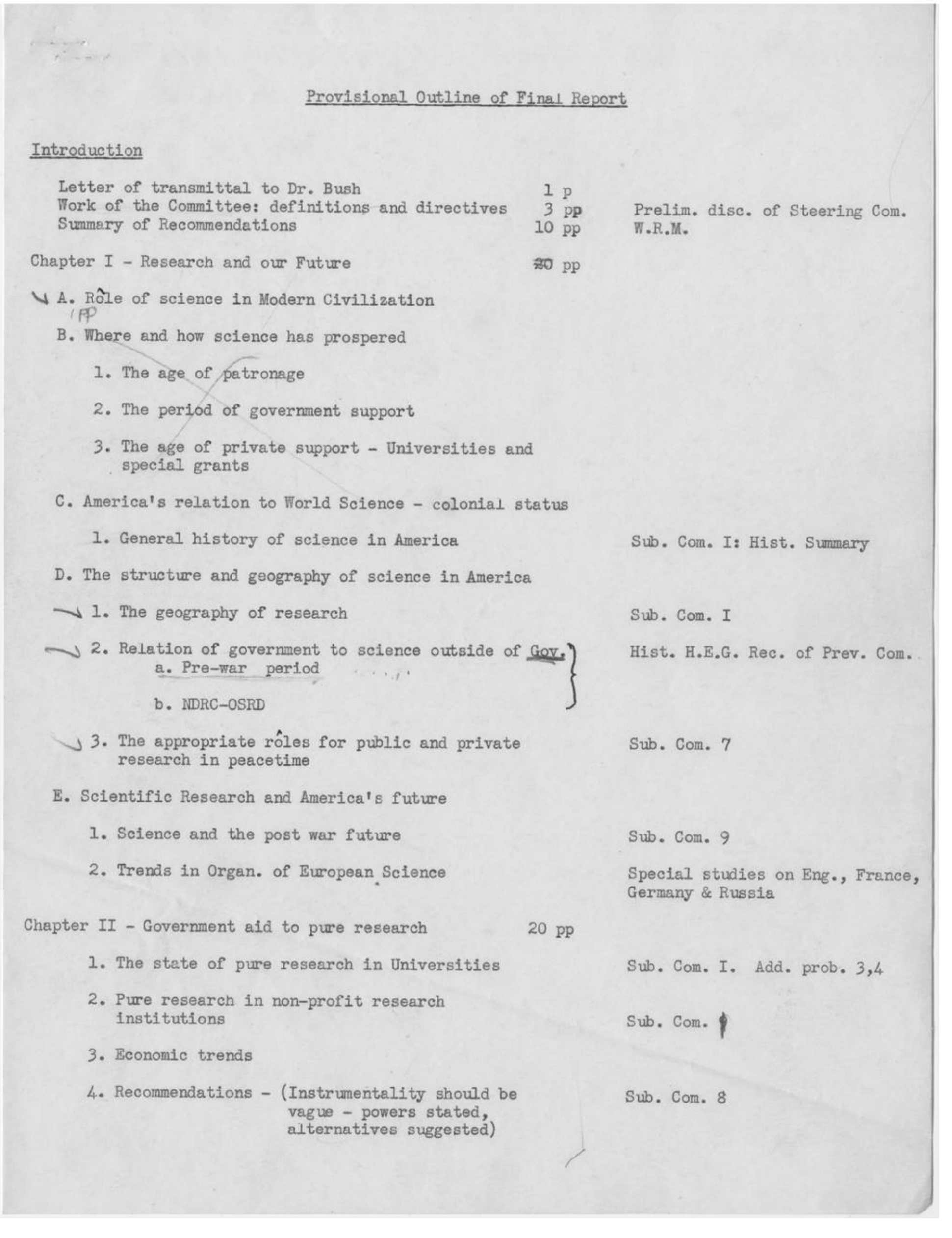
Even though the pure/applied distinction was non-negotiable, it was modified in the final report by the addition of a third category, that of "background" research: standardization, geological maps, meteorological data, physical and chemical constants. The matter of background

---

[36] J. Cole, D. E. Stokes, I. B. Cohen, and G. Holton, *Science, The Endless Frontier, 1945–1995: Learning from the Past, Designing for the Future*, 1994, http://cpt.phys.utk.edu/~th/Physics490/VBushConference/complete94.pdf. Cohen figured on Guerlac's list of people to speak to for his memorandum on earlier science policy initiatives: handwritten notes, HGP 25/5.

[37] It cannot be established unambiguously who wrote which part of the final report. Samuelson remembered writing most of the report together with Edsall and Morison: Samuelson, "Three Moles" (cit. n. 4), p. 83. Guerlac wrote in 1952 that he and Samuelson had written most of the draft: Guerlac to Earle, 20 Oct. 1952, HGP 26/11–12. In the early 1990s Cohen remembered having written sections on the history of science in France and the United Kingdom and the history of science in the United States: Cole *et al.*, *Science—The Endless Frontier, 1945–1995: Learning from the Past, Designing for the Future* (cit. n. 36), p. 22. The Guerlac Papers contain several drafts and memoranda in Guerlac's longhand, covering the history of science in the United States and Europe, earlier committees that had advised on government support for science, and parts of the report of Subcommittee 1, but we cannot unambiguously conclude that the text was his.

[38] Detailed minutes of meeting, Steering Committee, 3 Jan. 1945, HGP 25/9, p. 6 ("background" research); and Guerlac to William R. Rubey, 5 Apr. 1945, HGP 25/3.



Provisional Outline of Final Report

| | | |
|---|---|---|
| **Introduction** | | |
| Letter of transmittal to Dr. Bush | 1 p | |
| Work of the Committee: definitions and directives | 3 pp | Prelim. disc. of Steering Com. |
| Summary of Recommendations | 10 pp | W.R.M. |
| Chapter I - Research and our Future | 20 pp | |
| A. Rôle of science in Modern Civilization | | |
| B. Where and how science has prospered | | |
| 1. The age of patronage | | |
| 2. The period of government support | | |
| 3. The age of private support - Universities and special grants | | |
| C. America's relation to World Science - colonial status | | |
| 1. General history of science in America | | Sub. Com. I: Hist. Summary |
| D. The structure and geography of science in America | | |
| 1. The geography of research | | Sub. Com. I |
| 2. Relation of government to science outside of Gov. a. Pre-war period b. NDRC-OSRD | | Hist. H.E.G. Rec. of Prev. Com. |
| 3. The appropriate rôles for public and private research in peacetime | | Sub. Com. 7 |
| E. Scientific Research and America's future | | |
| 1. Science and the post war future | | Sub. Com. 9 |
| 2. Trends in Organ. of European Science | | Special studies on Eng., France, Germany & Russia |
| Chapter II - Government aid to pure research | 20 pp | |
| 1. The state of pure research in Universities | | Sub. Com. I. Add. prob. 3,4 |
| 2. Pure research in non-profit research institutions | | Sub. Com. |
| 3. Economic trends | | |
| 4. Recommendations - (Instrumentality should be vague - powers stated, alternatives suggested) | | Sub. Com. 8 |

**Figure 1.** Fragment of the draft content list, undated, probably early March 1945. Source: "Provisional outline of final report," HGP 25/6. The outline shows how the reports of the subcommittees of the Bowman report contributed to the report and, for some sections, if they were the responsibility of Guerlac (H.E.G.) or Maclaurin (W.R.M.). Courtesy Cornell University.

research was raised, understandably, in the subcommittee chaired by Wrather, of the U.S. Geological Survey.[39] Such research is fundamental and is not likely to be funded by indus-

[39] Bush, *Science—The Endless Frontier* (cit. n. 2), p. 82; and Report of Subcommittee 7, 6 Mar. 1945, HGP 25/10.

try, but it has a practical objective; equally important, because it lies in the public domain, the government was well placed to handle it. Background research consisted of long-term projects, usually involving large teams and requiring complex planning and administration.[40] A note in Guerlac's hand gave the example of studies in heredity, which would need long-term collaboration between statisticians and geneticists and careful planning to get to any meaningful results.[41]

The committee argued that universities provided the preferred "historically acquired" "habitat" for pure research. The corollary of this institutional and conceptual focus was that postwar support for fundamental research would increasingly go to (private) universities, strengthening their position against governmental and industrial research laboratories and showing the importance of a reconsideration of patent regulation.

Referring extensively to Conant's Franklin Medal Lecture, Subcommittee 1 agreed that although some types of scientific activity required planning, pure research, inherently "unpredictable," did not, implying that support should be "flexible." Just as the scholar was motivated by neither "financial reward" nor "the love of practical accomplishment," so the scientist was driven by a "complex of motives," including "a curiosity which leads from the better known to the lesser known." They quoted Conant approvingly: "The scholar must be free. . . . He must inquire and speculate with as few restraints as possible." Because pure research also required "its followers" to look at "familiar facts from unfamiliar viewpoints," there were many scientists (the text mentioned Pasteur) whose ideas had met with strong resistance.[42] The alignment of pure science with free inquiry made it almost impossible to conceive of a role for the state other than as a patron to private universities, enabling scientists—and universities—to pursue their own preconceived goals.

Haldane's and Bernal's prewar questions about taxpayers' money targeted exactly this issue and were echoed by Rabi in one of the first meetings of the steering committee. Rabi expressed his concerns about the likely recommendation of the committee to seek sustained funding of pure research in private universities. This was highly unusual and politically sensitive; moreover, he noted, even "under optimal conditions" university spending had been far from efficient. He challenged the need for unrestrained scientific freedom, claiming that "at least 75 percent" of the six hundred people working in the Radiation Laboratory "would like to stay on in a similar setting after the war. These men have learned the value of adequate financial support and the intellectual pleasures of working together." Working as a team on a project did not mean working under "centralized" or "government dictation."[43]

---

[40] "Ch. IV: Scientific Research in the Government Service," HGP 25/6, p. 8. Parts of this manuscript are in Guerlac's longhand. See also Bush, *Science—The Endless Frontier*, p. 99.

[41] Handwritten note, "Kinds of research which may need govt support to permit them to develop," undated and unsigned, HGP 25/6.

[42] "Report of subcommittee no. 1, March 13, 1945," HGP 25/10; there are quotations from and implicit references to Conant, "Advancement of Learning in the United States in the Post-War World" (cit. n. 11). Important parts of this report remained unchanged from their first appearance in the preamble, possibly drafted by Weaver or Rabi, on 13 Jan. 1945; see Minutes of committee meeting, 18 Jan. 1945, HGP 25/10. Significantly, an earlier version of this report shows a question mark in the margin in Guerlac's hand. Conceiving of Pasteur's work in terms of the pure scientist searching for nature's hidden secrets was difficult to square with his own analysis of the entanglements between science and the state, in which teamwork and the state planning of research played such an important role. Guerlac's question mark can be found in a draft for Subcommittee 1, entitled "Government Assistance to Universities and non-profit research institutes," 5 Feb. 1945, HGP 25/8. The question mark did not lead to a change of text in the March report submitted to the full committee. Cohen used the case of Pasteur to argue for the importance of "happy accidents," unplanned fundamental breakthroughs, in his science primer. The image of Pasteur as pursuing "pure science" became obsolete, of course, after Bruno Latour's *The Pasteurization of France* (Cambridge, Mass.: Harvard Univ. Press, 1993).

[43] Subcommittee draft, "The Federal government's role in post-war scientific research," 16 Feb. 1945, HGP 25/8, pp. 5, 10; and "Minutes of the meeting of the Bowman Committee held on March 26–7, 1945," HGP 25/1–2, p. 6.

Not only did some committee members not accept that decisions about university spending on pure research were best left to the universities, but the idea had also been challenged by Samuelson, Guerlac, and Maclaurin. In an undated note Guerlac reiterated his conviction that it was a mistake to locate pure research only in the universities, for Bush's OSRD had unmistakably changed the prewar scientific landscape by attracting the best and the finest, immigrants as well as American citizens, into a collaborative and long-term research effort. The committee "would be deceiving" itself if it ignored wartime experience and the fact that the OSRD was "perhaps the most representative," "if not the strongest[,] voice in American science."[44]

In view of this, it is paradoxical that government funding of pure research in universities was supported by information provided by the secretariat. The secretariat produced a comparison of the historical relations between science and the state in Europe and the United States to which Guerlac, Cohen, and Leontief (covering the Soviet Union) contributed, as well as statistics on the widening gap between company and university spending on research.

The report of Subcommittee 6, which was chaired by Maclaurin, argued that the "introduction of a technical innovation" was only the "final stage of a highly complex chain of interrelated activities which begin with the discovery of new scientific principles and ultimately lead through all the various stages of invention, development and testing, to the introduction of a new product, piece of machinery or process." An example was the radio industry, a major source of employment that had not existed as recently as 1919, which was the subject of Maclaurin's own research. Innovations such as radio depended crucially on progress in pure science: "pure research is the pacemaker of modern technological progress." Even after the "unprecedented research effort after Pearl Harbor," the United States still fell short in "pure research" compared with Germany and England. However, the report also argued that "technical invention" was no longer "an individual venture" but instead was increasingly the result of "a coordinated research enterprise requiring the cooperation of a large number of highly trained workers—pure scientists, engineers, technicians and designers"—whose capital requirements matched or surpassed those of industry.[45] In line with Maclaurin's prewar proposal on fundamental research in universities, there was no emphasis on the freedom of the (individual) scientist; instead, there was a complex chain of planned processes in which individuals with different motivations and skills collaborated. Clearly Maclaurin wavered between "pure" research that resisted organization and fundamental research that could combine scientists and technicians with different interests into a team.

Based on drafts by Guerlac and others, the report of Subcommittee 1, chaired by Rabi, used the secretariat's materials to argue that this complex chain started with pure research undertaken in European universities, compared with which American universities traditionally had fallen short. On the other hand, "Yankee engineering ingenuity," a popular expression in Barber and Cohen's manuscript on American science and war, had translated the results of European pure science into useful products. With the collapse of science in Europe, there was therefore an economic need to stimulate pure science in American universities. The historical relations between science and the state in Europe that Guerlac had highlighted in his own thesis work were sidestepped, as was the larger picture that would have included the spectacular growth of fundamental research in industrial research laboratories. But the conceptual distinction between the pure and the applied automatically classified fundamental research in industrial research laboratories as applied research, reaffirming the prewar idea that industry depended on universities for fundamental innovations.

---

[44] Handwritten notes, HGP 25/3/6, p. 4.

[45] "Preliminary report of sub-committee #6," HGP 25/10, pp. 3, 4, 2, 4.

The need to support research at universities followed also from statistics on research expenditures that showed the ratio of total expenditures on pure and applied research declining from 1:5 before the war to a projected 1:7 in 1947. This widening gap was taken to support a variant of Gresham's law that "applied research invariably tends to drive out pure research." (See Figure 2.) Rather than concluding that there was no evidence for the importance of pure research at American universities, and that fundamental research in government and industry made up for this lack, the subcommittee drew the opposite conclusion: government support was needed because private universities could not be expected to fill the gap.[46]

## THE BOWMAN REPORT

A draft report prepared by the secretariat, drawing on the subcommittees' reports, was considered by the full committee on 26–27 March. Its recommendations proved contentious, the meeting addressing three questions:

(1) Is substantial federal aid for scientific research necessary and desirable?
(2) Can such support and political control be effectively separated?
(3) Is there any conceivable way to ensure flexibility in appropriation of such funds and do we recognize any agency now in existence as capable of administering them?[47]

On all three questions Rabi was pitted against those who feared that government funding would reduce private support for science and lead to political control of science. Other committee members spoke of "the danger of the encroaching federal octopus on the internal affairs of universities" and claimed that "conditions tolerated by investigators during the war . . . would not be tolerable in peacetime." Rabi argued against this, using Guerlac's review of the history of science in America and repeating his January statement that the majority of the scientists in his own laboratory would like to remain in a similar position after the war, albeit without claiming that research at the Radiation Laboratory was as fundamental as pure research in universities.[48] Such pragmatism did not satisfy other committee members. When Bowman raised the issue of government support for applied research, the quantum theory of photosensitivity (useful for photography) and the Servo Laboratory at MIT were cited as examples where government support for applied research would be legitimate.

When instructing the secretariat on how to write the final report, the committee finessed the main points of disagreement by agreeing that the report should begin with the highly qualified statement, "An increased measure of federal aid for scientific research is desirable if such aid can be implemented without restriction of scientific freedom, having in view the desirability of continuing private support." They then turned to the possible establishment of a National Research Board; there was a long discussion, described by the minute takers as "unproductive," about the merits of using the NAS to distribute funds.[49]

Similar divisions were revealed when attention turned to support for industrial research. The major disagreement concerned whether long-term research planning was compatible with freedom in the market. Buckley, from an industrial research laboratory, argued that it was not, Rabi that it was. The compromise was for the report to summarize the issues without expressing a clear opinion on what should be done. Similarly, it was agreed that the report would state that a new instrumentality "could *conceivably* be within the Academy" before outlining the

---

[46] *Ibid.*, p. 13.
[47] "Minutes of the meeting of the Bowman Committee held on March 26–7, 1945," HGP 25/1–2, p. 1.
[48] *Ibid.*, pp. 2, 5 (quotations), 6.
[49] *Ibid.*, pp. 13, 14.

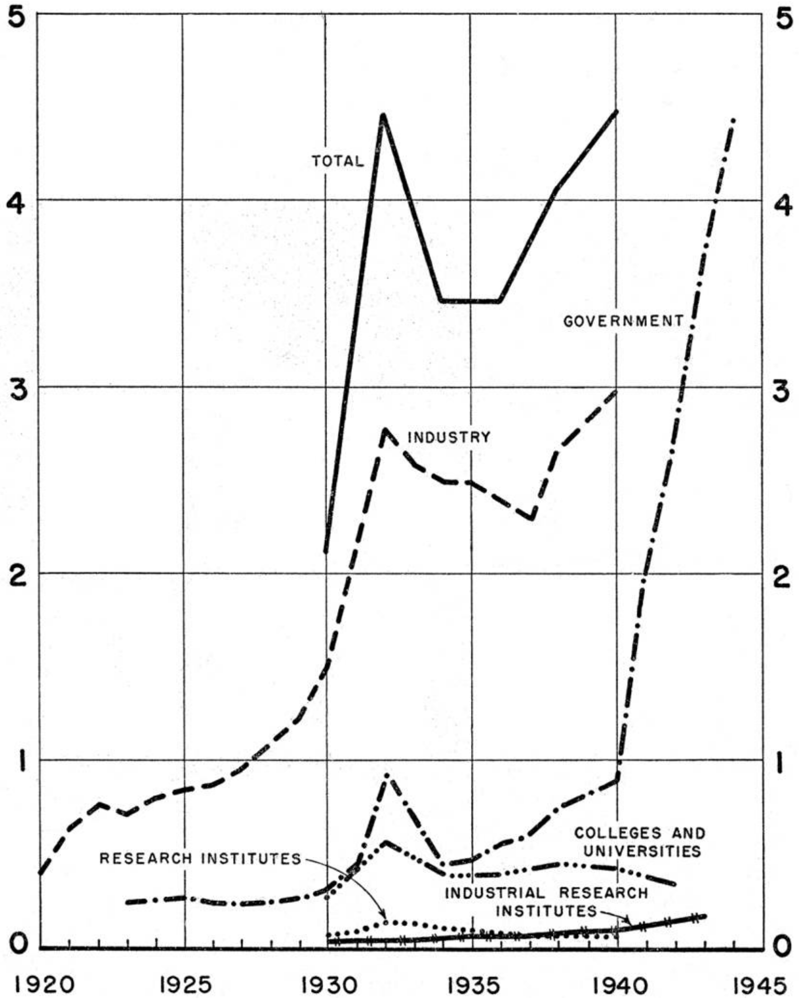



**Figure 2.** Expenditures for scientific research in the United States, in U.S. dollars per 1,000 national income. Source: Vannevar Bush, *Science—The Endless Frontier: A Report to the President* (1945; Washington, D.C.: National Science Foundation, 1960), p. 88. The surge of expenditures on government research from 1940 shows research performed under the auspices of the OSRD. The labels "Government," "Industry," "Colleges and Universities," "Research Institutes," and "Industrial Research Institutes" conceal the tripartite distinction used in the discussions within the Bowman Committee among "pure," "background," and "applied" research, in which "pure research" became identical with research at colleges and universities.

anticipated advantages and disadvantages. The secretariat was advised to stay "vague" in the final wording of the instrumentality problem. Bowman conceded that "the committee had not completely matured its thought." After the meeting, in a memorandum to Samuelson, Robert Morison did not conceal his disappointment with the outcome.[50]

[50] *Ibid.*, pp. 26, 27; and Robert Morison to Samuelson, 28 Mar. 1945, HGP 25/1–2.

The proposal for a National Research Board was the most sensitive part of the draft report, for it was close to what Kilgore was proposing as well. This raised the seemingly impossible task of keeping sufficient distance from Kilgore's plans without alienating him completely, a goal that was important because Kilgore had shown a willingness to work with Bush and the Bowman Committee on a newly worded initiative.[51] Guerlac's papers contain extensive notes on how different constituencies could be kept on board.[52]

A note in Guerlac's hand, shown here as Figure 3, recapitulated the issues discussed over the preceding months. It listed instances of wartime experience of planning, including the U.S. Army Air Force test and research base at Wright Field, the NDRC, and the OSRD, concluding that "industry" had "genuinely converted" the "research space" and researchers themselves. This supported the message conveyed in propaganda movies on research spaces such as Wright Field, which told the public that long-term state-planned military research was crucial to winning the war.[53] Yet despite this, Guerlac observed that the "inside group" of scientists involved in the NDRC and the OSRD entertained a "philosophy of compromise and appeasement" vis-à-vis industry and had a "chronic fear" of a "lack of industrial cooperation." This philosophy was grounded in Bush's assessment of "the situation" and the attempt to bypass "crackpots" like Kilgore and others who supported his initiative. "The situation" was that the OSRD could have been as successful as it was only in the exceptional circumstances of the war; in peacetime it could not be expected that research at government institutes would be a match for fundamental research at universities or in industry. As industry could very well take care of itself, the reality was that universities should be supported and be enabled to capitalize on their findings through patents, a vision that found expression in the passing remark in the final report that "patents are the life of research."[54] Guerlac's criticism went even further in notes on the way the word "Freedom" was abused:

> Liberty and Science —
> Conant and Compton's false hist. Ideas. — Laissez-faire
> Buckley — Misuse of word liberty {doesn't mean "no government aid"
> Philosophy merely justifies kow-towing to industry.

He concluded that they were "Begging basic question," "Refusing to be objective and scientific—prejudice— . . . ."[55]

A second, undated note, clearly not intended for wider distribution because of its characterization of Conant, Compton, and Frank B. Jewett (who was "on the extreme right"), was more specific in its analysis of Bush's original intentions in forming the Bowman Committee and his attitude toward Kilgore: "The appointment of the committee was intended to short-circuit congress, to keep the proposal out of Congress and to face the Kilgore planners with a fait accompli." It discussed the issue of a "scientific high command" in even sharper terms.[56] The tone and conclusions of this note reflect the position taken in Samuelson's editorial in the *New Republic*, suggesting that he might have been the author.

---

[51] Harley Kilgore to Vannevar Bush, 5 Feb. 1945; and Bush to Kilgore, 10 Feb. 1945: OSRD 2/Committee No. 3 (2-1-45 to 5-15-45). In the end, however, Kilgore would feel double-crossed by Bush when the Bush Report was used to stall his own initiative; see Maddox, "Politics of World War II Science" (cit. n. 13), p. 32.

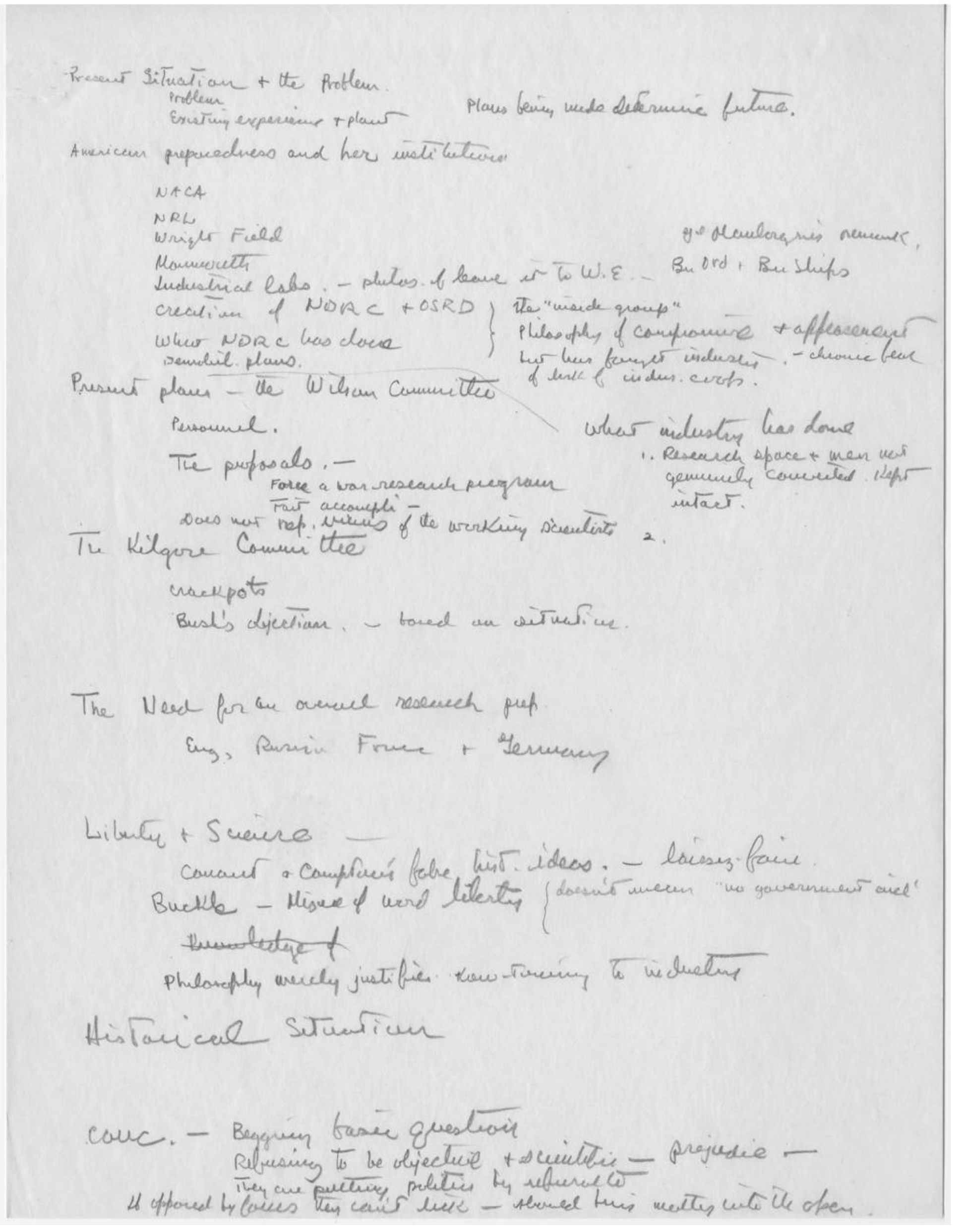
[52] See esp. "A scientific high command," undated, HGP 25/3.

[53] Handwritten notes, HGP 25/3. For a pertinent propaganda film see https://www.youtube.com/watch?v=thZJGVWN7fY.

[54] Bush, *Science—The Endless Frontier* (cit. n. 2), p. 103.

[55] Handwritten notes, HGP 25/3. The remainder is hard to decipher, though the final words are "should bring matters into the open."

[56] "A scientific high command," HGP 25/3/4.



Present Situation + the Problem.
Problem
Existing experience + plans
Plans being made determine future.

American preparedness and her institutions

NACA
NRL
Wright Field
Monmouth
Industrial labs. – philos. of leave it to W.E. –
Creation of NDRC + OSRD
What NDRC has done
Remedial plans.

Bu Ord + Bu Ships
the "inside group"
Philosophy of compromise + appeasement

Present plans – the Wilson Committee
Personnel.
The proposals. –
Force a war research program
Fait accompli –
Does not rep. views of the working scientists

what industry has done
1. Research space + men not genuinely converted. kept intact.
2.

The Kilgore Committee
crackpots
Bush's objection. – based on situation.

The Need for an overall research prog.
Eng, Russia France + Germany

Liberty + Science —
Conant + Compton's false hist. ideas. – laissez-faire
Buckle – Misuse of word liberty { doesn't mean "no government aid"
Philosophy merely justifies turn-turning to industry

Historical Situation

Conc. – Begging basic question
Refusing to be objective + scientific — prejudice —
They are putting politics by refusal to
opposed by forces they can't lick — should bring matter into the open.

**Figure 3.** Guerlac's handwritten notes. Source: HGP 25/3, p. 1. Guerlac's rough notes provide a perspective on the secretariat's thinking that was not visible in either drafts or the published version of the report. Following in part the list of contents of the report, they also critically comment on the deliberations within the Bowman Committee, as well as on the *partis pris* of some of its members or the views of those "East-coast scientists" whose opinions could not be sidestepped, such as Bush, Conant, and Compton. Courtesy Cornell University.

There had been, so this note argued, a "gradual realization" even among the more conservative voices (such as Jewett) that "some planning" was necessary. There was a "small number of scientists" who would remain in "key positions" regardless of what happened: a "close inner gang" of "Eastern seaboard and MIT" Republicans who were antibureaucratic and conservative but "patriotic" rather than "reactionary." They had "stood up to business" and "slugged it out in the public interest during the war." This was the background to their discussion of organizational forms.[57]

Owing to the sensitivity of the issue, redrafting the proposal for the creation of a National Science Research Board was done in close consultation with Carroll Wilson, if not with Bush himself.[58] In this process, differences between committee members were fudged, the final draft promising "*financial support without control*" and recommending "a largely autonomous board" with a staff of scientists. Even though it was much weaker than Guerlac and Samuelson would have liked, it was too strong for much of the committee. However, the committee was prepared to support it when prefaced with what Bowman called a "statement of social philosophy." This detailed arguments against federal control over science but stated that the need for federal funding was a conclusion to which the committee had been "forced." Because scientific research had to be free, the board should not attempt to guide the normal growth processes of science in detail. In a letter to Bush, Bowman pointed out the importance of this part of the report: "Without these few pages on social philosophy about half of the committee would be unwilling to sign our report. I would be among that half."[59] The statement of social philosophy, added at the last minute, was much more than a preamble.

## SCIENCE POLICY, ECONOMICS, AND THE HISTORY OF SCIENCE

The analysis of science policy offered a unique opportunity to forge a long-term collaboration between economists and historians of science aiming to understand the relations between science, innovation, and economic progress. Months before the Bowman Committee was formed, Maclaurin had asked Guerlac to join his department at MIT for this purpose. Maclaurin had started a project on the process of innovation in which Samuelson had briefly been involved, and it is likely that he brought Maclaurin into contact with Guerlac when he and Guerlac found themselves at the Radiation Laboratory. Teaming up with Cohen and others in the Bowman Committee's secretariat might have speeded up the collaboration envisioned by Maclaurin.

But this did not happen. In part, the reasons for this road not taken can be found in fault lines within the Bowman Committee and its secretariat, especially between Guerlac and Samuelson on one hand and Cohen and, increasingly, Maclaurin on the other. These fault lines became even more apparent after the war. We have seen that Guerlac and Samuelson came to a shared understanding of science and of the need for postwar federal funding of science. They considered the functioning of the OSRD as the new "upper bound" on how scientists working in collaboration on planned research projects could produce useful outcomes, not only for the military but also as spin-offs to industry at large. This did not mean that they were in favor of political control of science, but it led them to downplay the emphasis on the unconstrained freedom of the scientist. This position was originally shared by Maclaurin: in his proposal to investigate research spending on universities he noted that it was all to the good that

---

[57] *Ibid.*

[58] "Recommendations of Bowman Committee," OSRD 2/Committee No. 3 (2-1-45 to 5-15-45). See also "Bowman Committee: Basic Recommendations," *ibid.*, p. 7; and "Draft prologue," *ibid.*, p. 7.

[59] Isaiah Bowman to Bush, 11 Apr. 1945, OSRD 2/Committee No. 3 (1 of 3).

scarce resources had forced university scientists to pay closer attention to practical applications. Within the Bowman Committee only Rabi shared this view, emphasizing repeatedly that the fact that members of the staff at the Radiation Laboratory were asked to work on set problems did not mean that their modes of proceeding were prescribed as well. The problem of planned research was no different from that of a research institute with a professor and an assemblage of scientists working on a predefined problem, and there was no reason to be dismissive about federal institutes' capacity to produce efficient outcomes compared with universities.

However, this was not the message of the Bowman Report, which emphasized the importance of private research universities and the freedom of individual scientists to pursue their own research agendas. The idea that the progress of science crucially depended on the complete freedom of the scientist, endowed with sufficient means to follow his intuitions, had been consistently pushed by Conant under the banner of the scientist's need for freedom in a democratic society and would find its way into his *Understanding Science*, the published version of his Terry Lectures at Yale University, in which he thanked Cohen for preparing the case studies. It could equally be found in Cohen's own *Science: Servant of Man* and in the manuscript on American science and war he had previously been working on with Barber. Choosing his examples from the seventeenth and eighteenth centuries, Conant laid out a scheme for "general education" in science that would use historical cases to develop the "tactics and strategies" followed by some of the geniuses of experimental inquiry. For reasons of simplicity, these case studies (the air-pump, phlogiston, and electricity), which brilliantly tackled issues that still resonate today—such as the structuring role of conceptual schemes, science as a social endeavor, and the larger sociopolitical context—were chosen from a period in which "pure" research was performed in newly established learned societies and in the private sphere, rather than at universities. However, the nub of his argument was that the "tactics and strategies" of early modern scientists could be seamlessly applied to twentieth-century science, despite the development of large-scale industrial laboratories and planned science under the OSRD, to which Guerlac and Samuelson attached such importance and in which Conant himself had played no minor role. Separating these "tactics and strategies" from the larger socioeconomic context, Conant went on to argue that the inclusion of materials drawn from the "political, social, and cultural" context was "quite irrelevant" to the general principles underlying scientific progress. These general principles concentrated on the conceptual choices made by individual scientists who were left free to pursue their intuitions without a view to practical results. Commenting on J. L. Henderson's quip that the steam engine had done more for science than science for the steam engine, Conant argued that "very little could be learned" about the "Tactics and Strategies" of science from studying advances such as the steam engine, "because they do not belong to science."[60] In his primer Cohen confronted head-on the charge that socioeconomic changes had changed the landscape of science; he sought to isolate the individual scientist and his conceptual struggles as the premier subject of the historian of science. Science became defined as pure science, to which the study of socioeconomic history and the history of technological change would not add anything substantial.

Maclaurin's analysis of the process of innovation followed on from this view of scientific progress. In his 1949 book on innovation in the radio industry, a book that was enthusiastically endorsed by Compton, Maclaurin detailed how the original and unplanned discoveries of scientists such as Faraday and Maxwell had been translated into marketable products via entrepreneur-inventors like Marconi. In his foreword Compton emphasized Maclaurin's care

---

[60] James Bryant Conant, *On Understanding Science: An Historical Approach* (New Haven, Conn.: Yale Univ. Press, 1947), pp. 108, 23. On the case studies see Hamlin, "Pedagogical Roots of the History of Science" (cit. n. 11).

in pointing out "the important distinction between advances in fundamental science and their practical applications in new or improved products." His great contribution was to draw a clear distinction between "the scientist, the inventor, and the business innovator," which was of "major significance to an understanding of the process of technological change."[61]

*Science—The Endless Frontier* was prominent in the book. Maclaurin placed a quotation from Bush about the importance of "pure research" at the beginning of the preface, and Compton cited Maclaurin's experience with the Bush Report as his reason for encouraging Maclaurin to engage in his studies. This was, of course, misleading, because those studies had begun three years earlier. Compton had consistently attached importance to the relation between fundamental research, industrial innovation, and economic progress and had hoped that Maclaurin, taking advantage of his proximity to MIT's engineers, would investigate these relations in detail. Maclaurin's book met Compton's expectations. It traced the development of the radio industry from its roots in pure research, via the work of inventors and innovators (whom he carefully distinguished), to commercial applications that would have surprised even Faraday. In retrospect Cohen remembered that at the time most scientists considered this process, now known as the linear model of innovation, as "axiomatic."[62] But as with any axiom, it was the outcome of choices. Within the secretariat it had even been described as Conant and Compton's "false" history of ideas, catering to corporate America and misrepresenting the history of scientific progress. These choices made economic history and the history of technology an add-on to the history of science and fixed the primacy of the university as the site for pure research and the necessary starting point in the process of innovation.

## CODA

The widely held view that the Bowman Committee was of one mind in its attitude toward the postwar organization of science is not correct. Though there was unanimity on the importance of support for basic research, there were deep divisions over the role to be taken by the state, and the secretariat played an important part in drafting a report that would be acceptable to all members of the committee, supporting those members, such as Rabi, who favored planned science. The Bowman Committee's distinction between pure, applied, and background research was part of its attempt to pick out the university as the premier site for fundamental research and to secure the freedom of university scientists to follow their own research agendas, a conclusion that was supported by historical and statistical materials produced by the secretariat. This conclusion was premised on the idea that new discoveries in science would, somehow, translate into new industries. Though vague on details, the committee agreed that support for pure research should rest in the hands of scientists, minimizing government control. The reactions to the draft report at the 25–26 March meeting, where many committee members expressed their fears of federal control, show that the report's recommendations were not a foregone conclusion. Samuelson's view was that they had achieved as much as they could have hoped for in the final document: despite the finessing of key problems, the report made a case for federal funding of science that was more than the majority of committee members would have accepted without Bowman's carefully worded statement of social philosophy. Yet despite this success, the basis for any future collaboration between economists and historians of science was slipping away.

Maclaurin, who continued to study technical change and the relation between scientific progress and innovation, became a spokesman for the consensus position of the Bowman

[61] Rupert Maclaurin, *Invention and Innovation in the Radio Industry* (New York: Macmillan, 1949), pp. ix, xi.
[62] Cole *et al.*, *Science—The Endless Frontier, 1945–1995: Learning from the Past, Designing for the Future* (cit. n. 36).

Committee. In a short article in the *Harvard Business Review*, a venue directed at the audience Bush, Conant, and Compton had been most anxious to keep on board, he took up Bush's request to spread the report's message. He argued that although Langmuir's Nobel Prize–winning research with GE and Land's research at Polaroid had bridged "the gap between fundamental research and its practical applications in industry," this was "impractical" for "industry generally." He used the example of Faraday (soon to be displaced by more recent examples of radar, penicillin, and the atomic bomb) to show how unlikely it was that scientists could interest "stockholders and investors" in his kind of research.[63] This almost dictated the conclusion that the government had to step in to support research at universities, rather than elsewhere. He failed to draw the opposite conclusion: that the new conditions for fundamental research had changed with the emergence of large-scale industrial laboratories in which the distinction between the pure and the applied or between science and technology made little sense to begin with.[64]

Guerlac had no wish to become the "historian of modern science and industry" that Maclaurin wanted him to be. The history of nineteenth-century science could quite unjustifiably give the impression of science's peaceful progress and of scientists who were completely free to pursue their own curiosity. But this image of the scientist, cherished and carefully nurtured by Conant and Cohen, was modeled on the humanist scholar, on the ideal of a universal brotherhood of independent minds, rather than on the experimentalist, who increasingly worked under conditions not so different from the modern factory research laboratory; their model was closer to the Harvard scholar than the MIT engineer. His thesis work and his work as the official historian of the Radiation Laboratory had shown Guerlac the intricate relations between science, war, and the state and had demonstrated that scientists could voluntarily engage in long-term collaborative and planned projects—a view that was shared by Rabi.

Guerlac therefore had strong personal and intellectual reasons for preferring to accept Cornell's offer to establish a program in the history of science rather than Maclaurin's invitation to move to MIT, and George Sarton warned him that "at the MIT" he would always be "an outsider among technicians, somewhat like a professor of English in a conservatory of music."[65] His appetite for such collaboration must have been further diminished when he saw the direction Maclaurin's ideas were taking. His opposition to a 1957 proposal to bring the history of technology within the history of science may well have been motivated by his resistance to the then-common distinction between the scientist and the engineer and the alleged neutrality of scientific and engineering knowledge.[66] Cohen remained at Harvard, physically close to Samuelson and Maclaurin, but he saw no need for the history of science to be informed by economic history.

Samuelson committed himself to MIT, which was not only a congenial environment free of Harvard's anti-Semitism but also a place where economics could be developed as a science that favored mathematical modeling over the institutional, historical approach that Maclaurin

---

[63] W. Rupert Maclaurin, "Federal Support for Scientific Research," *Harvard Business Review*, Spring 1947, pp. 385–396.

[64] See esp. Leonard S. Reich, *The Making of American Industrial Research: Science and Business at GE and Bell, 1876–1926* (Cambridge: Cambridge Univ. Press, 2002); and George Wise, *Willis R. Whitney, General Electric, and the Origins of U.S. Industrial Research* (New York: Columbia Univ. Press, 1985).

[65] Michael Aaron Dennis, "Echoes of the Past: Henry Guerlac and Radar's Historiographic Problem," in *Tracking the History of Radar*, ed. Oskar Blumtritt (Piscataway, N.J.: IEEE, 1994), pp. 285–298, on p. 294 (quoting Sarton).

[66] See Jennifer Karns Alexander, "Thinking Again about Science in Technology," *Isis*, 2012, *103*:518–526, esp. p. 523. Alexander does not elaborate on the reasons for Guerlac's opposition, which *prima facie* does not square with his own work on the French school of engineering and his work on the Radiation Laboratory. On Cohen's position see also Hamlin, "Pedagogical Roots of the History of Science" (cit. n. 11).

had learned from Schumpeter at the Harvard Business School. Samuelson did not become an advocate of the linear model, but the mathematical models developed at MIT mostly black-boxed science and technology, leading to a similar outcome. There is an irony in that although he had sided with Guerlac, Rabi, and the advocates of planned science, Samuelson's career prospered because Compton's successor as president of MIT, James Rhyne Killian, followed Conant's first rule for enabling science to flourish, warmly recommended by Cohen in his own primer in the history of science: "There is only one proved method of assisting the advancement of pure science—that of picking men of genius, backing them heavily, and leaving them to direct themselves."[67]

[67] Cohen, *Science: Servant of Man* (cit. n. 26), p. 307 (quoting Conant).