CHAPTER ONE

THE NATIONAL BUREAU OF STANDARDS AT MID-CENTURY

A NERVOUS NATION

In the year that marked the midpoint of the 20th century, the National Bureau of Standards (NBS) looked forward to its fiftieth anniversary. Formed in 1901 to maintain custody of the national standards of measurement and to develop new ones as needed by the burgeoning industry of the country, the Bureau had done this and more. It also had become a national corporate laboratory in the physical sciences, concerned with the problems of the Nation that had a technical base, from the propagation of radio waves to the underground corrosion of gas pipelines, from the development of more accurate methods of measuring length to calibrating master clinical thermometers, from providing extremely accurate time signals for the whole Nation and its territories to developing more rapid methods of analyzing the composition of steel, and many more, tedious in recitation but rich in accomplishment. The Bureau had served its Nation well in two world wars and its greatest depression. And now, in its forty-ninth year, the Nation that it served was not well; it was a decidedly nervous Nation.

Somewhat over four years earlier, the United States and its allies had won the largest war in history, although the feeling of the Nation had not been one of victory but of having completed an odious and bloody chore. And things—good and bad—had not turned out as expected. The dire depression that had been forecast by analogy with the period following the ending of the first world war had not materialized. The returning servicemen and servicewomen, and the home front workers flush with money saved during the war-year shortages, simply would not let a depression happen. Their demands, pent up for four stringent years, were too clamorous and pressing to permit a depression. Shortages—of housing, autos, appliances, meat—had occurred, and with them severe inflation despite price controls that were a relic of the war years, and which gradually were lifted. Strikes—some serious—had occurred when wage controls had not permitted wages to follow prices upward, but they had been settled, although not without some confrontation. But there was demanding work to be done. Families had to be started, educations completed, and the work of building an American civilization had to be continued. It was time to turn away from what was happening in foreign lands and concentrate on America, which, after all, was the main business of Americans.

And the war had raised aspirations among servicemen and servicewomen, and those on the home front. Throughout the New Deal and war years, minorities—Jews, African Americans, children of first generation immigrants of Southern and Eastern European lineage—had begun to taste the sweet fruits of social equality. They, and the rest of the population on the lower levels of the economic and social order, would not go back to the old days. As quoted by Goldman:

Paul L. Howard and Charles L. Faust of the Electrochemical Society presented a certificate of congratulations to Edward U. Condon on the Bureau's fiftieth anniversary in 1951.

> "Times have changed," Maurice O'Connell, of the CIO, notified a Los Angeles Chamber of Commerce meeting shortly after the Japanese surrender. "People have become accustomed to new conditions, new wage scales, new ways of being treated. . . . Rosie the Riveter . . . isn't going back to emptying slop jars."[1]

The Government stood ready to help with these aspirations. The GI bill of rights,[2] which had become law in 1944, began to weave its economic and sociological magic. Young entrepreneurs could obtain Government guarantees for loans to start new businesses, home loans at what now seem absurdly low rates of interest were guaranteed for veterans by the Federal Government and, most importantly, the Government stood ready to pay for their education. Millions of ex-servicemen from working-class families, who previously could not realistically dream beyond a blue-collar job, could now aspire to a BA, an MA, or even a doctorate degree. And millions of them took advantage of this with alacrity, swelling college enrollments to a then all-time high of 2 million in 1946.

---

[1] Eric F. Goldman, *The Crucial Decade—and After: America, 1945-1960* (New York: Vintage Books, 1960) : 13.

[2] *Servicemen's Readjustment Act of 1944, U.S. Statutes at Large*, 58 (1944): 284.

By 1950, times were good. New cars and household appliances, while not abundant, were more generally available, and the GI-bill homes were rapidly being filled with them. There was even a new toy, a new entertainment medium called television. While only 5000 homes had TV sets in 1945, their numbers rose rapidly. By 1948 there were 1 million, and 10 million by 1952. The Nation could not get enough, and new manufacturing and entertainment industries were born. Now news events, sports, variety shows, westerns, movies, and any other feature a producer could dream up to attract an audience were brought into the American living room. American society—and American politics—would never be the same again.

Yet despite these relatively good times, and the abundant feeling that they would get better, the Nation was nervous. It found itself in an unusual and unaccustomed position: it was the most powerful nation on earth and was—indeed had to be—deeply involved in foreign affairs. No longer could the national psyche be turned inward, concerned solely with improving the state of American civilization. In all discussions, foreign relations intruded, and foreign relations were not good. International communism had to be dealt with, and hanging over everything was the unbottled genie of the atomic bomb, demanding international attention. Isolationism was gone forever.

During the war there had been general support for the alliance with the Soviet Union among both liberals and conservatives, although the latter were always nervous about it, and concerned about what would happen after the war. Yet the Yalta agreement in February 1945 seemed to predict Allied unity, and promised self-determination for the nations of Eastern Europe. It was greeted by "almost unanimous praise."[3] But soon, headlines revealing a secret Yalta agreement "deemed favorable to the Soviet" increased restiveness among conservatives, and it soon became clear that the principal aim of the Soviet Union was the extension of her hegemony to all the nations of Eastern Europe. Goldman wrote: "The Russians made moves in flagrant violation of the Yalta provisions for free elections in the liberated countries of Eastern Europe," and treated the formation of the United Nations with disdain.[4] In his inimitable way, Britain's Winston Churchill, no longer speaking as the Prime Minister after the defeat of his party by Labor shortly before the surrender of Japan, added a new phrase to the English lexicon when, speaking at tiny Westminster College in Fulton, Missouri, he announced, "From Stettin in the Baltic to Trieste in the Adriatic an iron curtain has descended across the continent."[5] The outlines of what was later to be named the "Cold War" by Bernard Baruch were laid out.

And the problems of communism were not to be confined to the foreign theater. Canadian spy trials indicated that a systematic Soviet espionage effort, especially on atomic and military matters was, and had been for several years, in existence. In the United States, "American Communist leaders began deserting the Party. . . . The most publicized deserter, Louis Budenz, ex-editor of the *Daily Worker,* quit with a flat statement that Communist parties anywhere were . . . conspiracies which gave their loyalty

---

[3] Goldman, *The Crucial Decade—and After*: 10.

[4] Ibid., 11.

[5] Ibid., 37.

first and last to the Soviet Union."[6] Communism, in the public mind, had become not only an international menace, but an internal one as well.

The Nation had made its own moves, defensive and offensive, in response to Soviet actions. When, in a blatant attempt to drive the Western Allies from Berlin, the Soviet Union had imposed a blockade on that divided city, the Allies, in a mood of controlled fury, had carried out an airlift of supplies for the eleven months the blockade lasted and then for an additional four months. And when Communist guerrillas were in danger of overthrowing the non-Communist (though hardly democratic) government of Greece, President Harry S. Truman asked for, and received from Congress, $400 million for military aid to Greece. In doing so, he announced what would be called the Truman Doctrine: "[I]t must be the policy of the United States to support free peoples who are resisting attempted subjugation by armed minorities or by outside pressures."[7] Thus was the Cold War fully born.

But most important of all as a policy measure had been the Marshall Plan. Designed to halt the spread of communism in war-ravaged Europe, it promised to aid any nation "willing to assist in the task of recovery." The Soviets disrespectfully declined to join. The plan worked. France and Italy, which had been in danger of going Communist, were saved for the West, and aid was extended to Austria and China. And finally, the United States, Canada, and the nations of Western Europe created the North Atlantic Treaty Organization, which coordinated their military organizations and pledged mutual assistance in the event of an attack on any one of them. The adversaries in the Cold War had been defined.

Almost coincidentally, George Kennan, writing as "X" in an article in *Foreign Affairs* analyzing Soviet conduct and U.S. response, used the word "containment."[8] Almost immediately this became the term used to describe the emerging Truman Administration policy toward communism. This pragmatic realism was disturbing. Was communism to be legitimized in large parts of the world? Was the struggle to contain it to continue indefinitely? Would the struggle erupt into a war? It was an unsettling prospect.

Europe was not the only theater where communism needed to be contained. In the public mind, China was as important as Europe, and the policy of containment was not working there. Despite more than $2 billion in grants and aid, the corruption-riddled Chiang Kai-Shek regime had not been able to stem the southward spread of Mao communism. In January 1949, the Nationalist government fled to Formosa; China had fallen to the Reds. Communism had been contained in Europe; in China it had burst through any containment attempt. Now the combination of China and the Soviet Union ruled more than a quarter of the earth's surface and more than three-quarters of its people. The Nation, in early 1950, was shocked and fearful.

And it was not clear that communism was being contained on the domestic scene. While the trials of Communist Party leaders had proceeded and Government investigators traversed the country, the most important case occupying the public mind was that

---

[6] Ibid., 35.

[7] Ibid., 59-60.

[8] G. Kennan, "The Sources of Soviet Contact," *Foreign Affairs*, July 1947.

of Alger Hiss, president of the Carnegie Endowment for International Peace and a past employee of the State Department. Hiss, that symbol of urbane Eastern intellectualism, of Ivy League education and genteel upbringing, friend and darling of the intellectual establishment, was being accused of espionage by Whittaker Chambers, a brilliant but "uncouth" editor of *Time*, and an avowedly reformed Communist. If Hiss, the confidant of the Nation's leaders, could not be trusted, who could be? Was the Government riddled with Communist agents?

But above all, what contributed to the Nation's nervousness was the bomb—or rather, Soviet possession of the bomb. When in September 1949 President Truman announced that the Soviets had exploded an atomic bomb, the Nation was dumbstruck. Despite the Smyth report[9] which explained the basic physics of the bomb and gave an account of the U.S. effort to build it, despite the warnings of the atomic scientists that any dedicated industrial nation could build the bomb, despite the estimate of those same scientists that the Soviet Union could build one in about five years, many of the public (including the president) believed that the U.S.S.R. would not have the bomb for a long time, perhaps ten years. Now here was the intransigent Soviet Union, at least five years ahead of schedule, possessor of the most deadly weapon devised by man.

The three blows of 1949—China, Hiss, and the bomb—had shocked the Nation. Goldman wrote:

> [They] loosed within American life a vast impatience, a turbulent bitterness, a rancor akin to revolt. It was a strange rebelliousness, quite without parallel in the history of the United States. It came not from any groups that could be called the left, not particularly from the poor or the disadvantaged. It brought into rococo coalition bankers and charwomen, urban priests and the Protestant farmlands of the Midwest, longtime New Deal voters and Senator Robert A. Taft.[10]

And 1950 was to prove no anodyne. In January, Hiss was found guilty of perjury and led off to jail. When Secretary of State Dean Acheson avowed that he would not turn his back on Hiss, the public remembered that Truman had called the Hiss hearings a red herring. Was it really possible, as Congressman Richard Nixon averred, that "Traitors in high councils of our own government have made sure that the deck is stacked on the Soviet side . . . "?[11] The theme was picked up by an obscure junior senator from Wisconsin. Looking for a cause, he made several speeches charging that "there were 57 card-carrying members of the Communist Party in the State Department."[12] Soon a new word, "McCarthyism," was added to the English language.

---

[9] Henry D. Smyth, *Atomic Energy for Military Purposes, the Official Report on the Development of the Atomic Bomb Under the Auspices of the United States Government, 1940-1945* (Princeton, NJ: Princeton University Press, 1945).

[10] Goldman, *The Crucial Decade—and After*: 113.

[11] Ibid., 135.

[12] Ibid., 143.

On January 31, 1950, shortly after the Hiss conviction, President Truman announced almost laconically that he had "directed the Atomic Energy Commission to continue its work on all forms of atomic weapons, including the so-called hydrogen or superbomb."[13] A new and even deadlier arms race was under way, and scientists predicted that the Soviets would not be far behind. The specter of nuclear annihilation had become plainer.

Finally, on Saturday, June 24, while most of the Nation's policy makers were away from Washington, North Korea mounted an all-out invasion of South Korea. The policy of containment was to receive its greatest military threat. Following three hectic days of conferences and meetings at the United Nations, President Truman announced that the U.S. Navy and Air Force would provide cover and air support below the 38th parallel for the South Korean forces. An electric feeling of cohesion gripped the Government. In three more days, having in the meantime secured the passage of a resolution committing other members of the UN to "furnish such additional assistance to the Republic of Korea as may be necessary to repel the armed attack and to restore international peace and security in the area,"[14] and facing a worsening military situation, Truman announced on June 30 the commitment of U.S. troops in Korea. The Cold War had become hot.

THE STATE OF SCIENCE

It is tragically true that some persons, and some fields of endeavor, are beneficiaries of war. This was the case with science. It came out of the war bigger and more famous than ever before. The atomic bomb had focused the public's attention on science, and the Smyth report told that story with thriller-novel intensity. And the achievements of science went beyond the atomic bomb. Radar and sonar became household words. News of the proximity fuze, which enormously extended the effectiveness of bombs and artillery shells, became generally known and was considered by some to be a military development as important as the bomb. A whole new industry—synthetic rubber—had been built and was turned over to industry. Penicillin and DDT were viewed as boons to mankind. Advances in electronics helped to make TV and that new commercial rage, "hi-fi," better. The jet airplane, while having no decided effect during the war, was becoming of overriding military importance, and in 1952 the ill-starred Comet aircraft was to usher in the age of commercial jet air transportation. And atomic energy held the promise—or fantasy—of electricity so cheap that it would not need to be metered. Scientists—particularly physicists—were looked upon with awe.

In keeping with its successes, the support of science—both Federal and private — had increased dramatically. From a total in 1940 of $250 million, national research expenditures had risen to $1.1 billion in 1945, and in 1950 were estimated to become $1.75 billion.[15] Whole new laboratories—denoted as National laboratories after the

---

[13] Ibid., 135-136.

[14] Ibid., 160.

[15] Eric A. Walker, "Effects of Government Support on Scientific Research," *Bulletin of the Atomic Scientists* 7(4) (1951): 119. See also graphs in Appendix E.

control of atomic energy was transferred to civilian hands with the creation of the Atomic Energy Commission in 1946—had been formed: Livermore; the famous Los Alamos, now a household name; Sandia, for weapons research; Argonne; Brookhaven; Idaho; Berkeley; Oak Ridge; Hanford; and, later, Fermilab for basic research in nuclear and particle physics and more general programs. The mix of spending had, of course, changed dramatically. While in 1940 the portion of research and development expenditures devoted to the military was approximately 6 percent, by 1945 the portion had inevitably risen to somewhat more than 50 percent, and the Korean War promised a new spurt of military research expenditures. The military had discovered science, found it useful (indeed essential in these Cold-War days) and a new symbiosis had arisen. The point of view of the military was succinctly and accurately expressed by Major General Curtis E. LeMay—no desk-bound general—in a letter to Edward Uhler Condon, director of the National Bureau of Standards, on January 4, 1946:

> I have no doubt that many a scientist has breathed a sigh of relief that they may now return to "normal" pursuits. But the Army Air Forces have learned to depend on you people, and realize that we cannot get along without your continued assistance. I most sincerely hope that the partnership developed during the war will be continued in the days to come.[16]

During the war, Government activities in science—and that amounted to essentially the whole national scientific effort—were guided by a set of interlocking committees in the National Defense Research Committee (NDRC) and the Committee for Medical Research. These in turn reported to the Office of Scientific Research and Development (OSRD), essentially a holding company for all Federal research activities, and headed by Vannevar Bush. Two items in that structure were of critical importance. The system of interlocking committees was a collegial structure that scientists were accustomed to and comfortable with. This was a great inducement for scientists to come to work for the Government, although patriotism should not be discounted. Perhaps even more important, the OSRD was located in the Office of the President, and its director had direct access to the president. This gave Bush enormous clout when dealing "with the vast network of administrative relationships on which the success of a Government agency depends."[17]

Even well before the end of the war, it was clear that this structure, devised for the emergency, could not be extended into the postwar period and, in 1944, President Roosevelt had written to Bush asking him to address four questions which can be paraphrased as follows:

---

[16] NARA; RG 167; Director's Files; Box 2; Folder D/IDP (Part 1).

[17] Don K. Price, *Government and Science: Their Dynamic Relationship in American Democracy* (New York: New York University Press, 1954): 45.

1. Consistent with national security, what can be done to make known to the world the contributions made to scientific knowledge during the war effort?
2. What can be done for continuing into the future the war against disease?
3. What can the Government do now and in the future to aid research activities by public and private institutions?
4. Can an effective program be proposed for discovering and developing scientific talent?

After study by four committees, Bush submitted his report on July 15, 1945,[18] to President Truman, Roosevelt having died in the interim. He proposed the creation of a National Research Foundation consisting of five divisions: Medical Research, Natural Sciences, National Defense, Scientific Personnel, and Publications and Scientific Collaboration. The emphasis was to be primarily on basic research,[19] and work was to be supported in research outside the Government, primarily in universities. In its scope, the proposal that the Government support basic research for the health and economic well-being of the Nation rivals the decisions of the mid to late nineteenth century for Government support of agricultural research. A major turning point in the conduct of science in the United States was at hand.

Significantly, Bush's report proposed that the Foundation be headed by a board of members not otherwise connected with the Government, and that this board would choose its own executive director. This was recommended to assure "complete independence and freedom for the nature, scope, and methodology of research carried on in the institutions receiving public funds."[20] In retrospect this appears a naive recommendation. It is difficult to believe that any president would work on policy matters with a person he did not appoint. Thus, when Truman received a bill in 1947 proposing the establishment of this organization (now called the National Science Foundation) that contained this provision, he promptly vetoed it. "They offered a national science bill which eliminated the President from the Government of the United States, and I wouldn't sign it," said Truman speaking of his favorite whipping boy, the Eightieth Congress, when addressing the American Association for the Advancement of Science (AAAS) on September 13, 1948.[21] The veto was not unexpected. However, another bill giving the president power to appoint an executive director and a twenty-four-member board for the Foundation was passed by the Congress and signed into law in 1950.

---

[18] Vannevar Bush, *Science, The Endless Frontier; a Report to the President on a Program for Postwar Scientific Research, July 1945* (Washington: National Science Foundaation, 1960 reprint). This report was commissioned almost a year before the fall of Germany, was submitted a full two months before the Japanese surrender, and only two months after the surrender of Germany. Clearly Roosevelt had been concerned very early with the problems of the postwar Nation.

[19] For a dissenting view, see Deborah Shapely and Rustum Roy, *Lost at the Frontier* (Philadelphia: ISI Press, 1985).

[20] Bush, *Science, the Endless Frontier*: 33.

[21] "President Truman Speaks to the Scientists," *Bulletin of the Atomic Scientists* 4(10) (1948): 291-293.

This bill did not include provisions for military research since this clearly was being handled by other means, and the provisions for medical research were made redundant by the flourishing of the National Institutes of Health. However, the bill gave the Foundation two further responsibilities: the development of science policy, and the evaluation of research programs undertaken by agencies of the Federal Government. The Foundation could carry out the first of these, but only in basic research. The second was an unrealistic expectation and was carried out only tangentially.[22]

The principal emphasis of the Bush report was basic research. In his introduction to the Bush report Alan Waterman wrote:

> Basic research leads to new knowledge. It provides scientific capital. It creates the fund from which the practical applications of knowledge must be drawn.... Today, it is truer than ever that basic research is the pacemaker of technological progress.... A nation which depends upon others for its new basic scientific knowledge will be slow in its industrial progress and weak in its competitive position in world trade, regardless of its mechanical skill.[23]

And again,

> The distinction between applied and pure research is not a hard and fast one, and industrial scientists may tackle specific problems from broad fundamental viewpoints. But it is important to emphasize that there is a perverse law governing research: under the pressure for immediate results, and unless deliberate policies are set up to guard against this, *applied research invariably drives out pure.*
>
> This moral is clear: It is pure research which deserves and requires special protection and specially assured support.[24]

That basic research was a principal concern arose from two important considerations, especially in nuclear physics. Under the pressure of building the atomic bomb, the basic knowledge gleaned in the research of the 1920s and 1930s had been used up and not replaced. The capital had been spent. In the words of Philip Morrison: "[Science] was mobilized with fierce single-mindedness for war. Not even a good seed crop was left in the schools."[25] What was true in nuclear physics was also true in other branches of science. The storehouse of knowledge had been raided, and needed to be refilled.

Moreover, the European universities, which had trained many leading American scientists, were a shambles. The great schools of physics on the continent were gone, their professors scattered to the winds, which fortunately had blown toward the United States. And basic research, again particularly in nuclear physics, had become more

---

[22] Alan T. Waterman, introduction to *Science, the Endless Frontier; a Report to the President on a Program for Postwar Scientific Research,* by Vannevar Bush, July 1945 (Washington: National Science Foundation, 1960 reprint): xxiii.

[23] Ibid., viii. In this day (1994) of enormous trade deficits with Japan, which has the reputation of doing very little basic research, these statements indicate that something was missing from the prescription. See Shapely and Roy, *Lost at the Frontier,* for a dissenting opinion.

[24] Ibid., xxvi.

[25] Philip Morrison, "The Laboratory Demobilizes...." Address at the Atomic Energy Session of the *New York Herald-Tribune Forum,* October 29, 1946, reprinted in *Bulletin of the Atomic Scientists* 2(9/10) (1946): 5-6.

expensive,[26] and was to continue to do so. In these days of cyclotrons, synchrotrons, betatrons, and nuclear reactors, facilities had become so costly that they had to be shared among research workers. This trend was to continue as new and more sophisticated laboratory equipment was developed. And in nuclear physics, research was carried out by large groups of investigators at large, expensive facilities. Basic research was essential to the economic and military future of the Nation and the Government was the only entity that had the resources and the necessary long-term view, went the argument. It had to support basic research.

Yet in the years between 1946 and 1950, while the debates about the structure of the NSF went on, no mechanism was available to fund civilian basic research outside the realm of atomic energy, and most of that was directed toward weapons problems. Enter then the Office of Naval Research.[27] In what must surely be one of the most enlightened research-support decisions ever made by a military agency, the Navy Department, reasoning that advances in basic science were essential to the future capabilities of the Navy, in 1946 formed the Office of Naval Research, setting it up on the same level as one of its statutory Bureaus. In the years between its founding and the establishment of the NSF in 1950 it had become the principal supporter of basic research in the Nation, and in those years did the work envisaged for the NSF. Aside from its own work carried out in three Navy laboratories, in 1949 it had 1131 projects at more than 200 institutions. This accounted for more than 40 percent of the Nation's total expenditures in basic science, and the total of $43 million amounted to more than the total national expenditure for basic research in 1941. Giving the contracting scientists a maximum degree of freedom, it received four times as many applications for projects as it could finance. Among many others, it financed projects in low-temperature physics, mathematics, investigations of cosmic rays, meteors, white dwarf stars, viruses, and the structure of proteins. It also supported work in the rapidly emerging field of computers, although it did this not as part of its support of basic research, but as part of its military-directed work. Its director was Alan T. Waterman, who, in 1950, became the first director of the NSF.

Following World War II, science did not wait for the National Science Foundation to be formed to make progress. Indeed, the period between the end of the war and 1950 showed notable advances in both basic science and technology. Some of these advances will be noted here. In basic physics, Willis Lamb of Columbia University reported at a conference at Shelter Island in 1947 that the $2^2S_{1/2}$ and $2^2P_{1/2}$ states of hydrogen differed in energy by a small amount, in stark contrast to the predictions of

---

[26] Lee A. DuBridge, "The Role of Large Laboratories in Nuclear Research." A speech given at the Bicentennial Celebration of Princeton University; reprinted in *Bulletin of the Atomic Scientists* 2(9/10) (1946): 12-13.

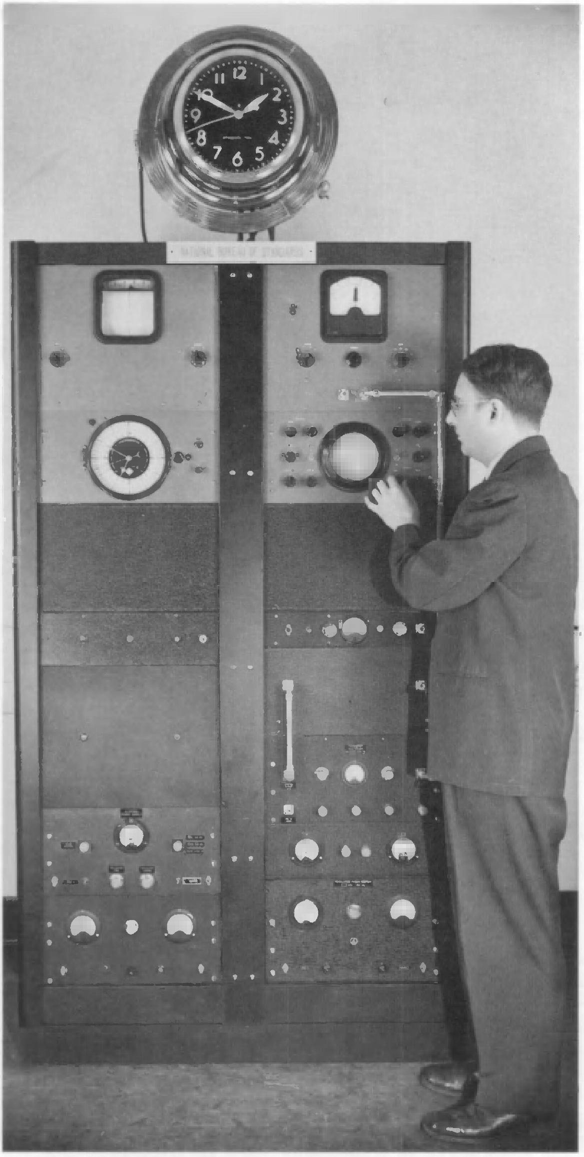
[27] John E. Pfeiffer, "The Office of Naval Research," *Scientific American* 180(2) (1949): 11-15.

first-principle theory. This report—the third paper by Lamb on the topic—caused an immediate sensation. In the words of Abraham Pais, "it was clear to all that a new chapter in physics was upon us."[28] Following this conference, Hans Bethe, Julian Schwinger, Sin-Itiro Tomonaga, and particularly Richard Feynman, all working independently, derived the so-called Lamb shift, and in the process created quantum electrodynamics, the most accurate physical theory ever developed. A year later, in a more technologically oriented physics area, John Bardeen, Walter H. Brattain, and William Shockley produced the transistor,[29] thereby laying the basis for a revolution in communications and computers, and in the process revolutionizing society. And computers themselves were showing great advances. John von Neumann began his seminal theoretical studies; J. Presper Eckert and John W. Mauchly in 1946 produced ENIAC, the first all-purpose electronic computer; the National Bureau of Standards in 1949 produced SEAC, then the fastest general-purpose, automatically-sequenced, electronic computer. The computer "explosion" had begun. Partly helped by computers, and soon to be helped more, the elucidation of structures by x-ray diffraction made great strides. By the use of ingenious techniques, structures previously thought intractable were being handled in both organic and inorganic materials, and this would culminate in the determination of the structure of proteins with resolutions as small as atomic diameters. A far more basic understanding of the phenomena of life was in the offing. An enormous leap in this direction was soon to come. The molecular basis of genetics was being sought by many workers, and finally found in the DNA double helix by James D. Watson and Francis H. C. Crick in 1953. The science of microbiology was born, and genetic engineering, its manufacturing offspring, would follow. Other scientific and technological advances were to have profound effects such as: the publication in 1948 by Claude Shannon of his work in information theory; the development by Willard Libby in 1948 of the carbon-14 method of dating archeological artifacts; the development in the same year by the National Bureau of Standards of the atomic clock, a variant of which was in due course to replace our slightly wobbly earth as a timekeeper; the development in 1948 by George Gamow, Ralph Alpher, and Robert Herman of the "Big Bang" theory of the origin of the universe, which was to revolutionize cosmology and have profound effects on philosophy and religion; and the production of Orlon by Dupont, thereby, with Nylon and Dacron, completing the triad of synthetic fibers that were designed to replace the natural triad of wool, silk, and cotton, and did so with considerable success. Many more examples could be added to this list. Clearly, basic and applied science were not static.

---

[28] Abraham Pais, *Inward Bound; Of Matter and Forces in the Physical World* (Oxford: Clarendon Press, 1986): 451. Chapter 18 gives an excellent historical and technical account of the origins of quantum electrodynamics.

[29] In 1965, Feynman, Schwinger, and Tomonaga shared the Nobel Prize in Physics "for their fundamental work in quantum electrodynamics, with deep-ploughing consequences for the physics of elementary particles," and in 1956 Bardeen, Brattain, and Schockley shared the Nobel Prize in Physics "for their investigations on semiconductors and the discovery of the transistor effect."



Harold Lyons observed the first atomic-beam clock developed in 1948 by NBS. Really a molecular clock, the device was based on the microwave absorption line of ammonia.

Those scientists who worked on atomic energy and military problems, however, faced new concerns: security and loyalty questions. The intense national anxiety with communism and subversion, and the vain attempt to maintain a military atomic monopoly, put a much greater demand on all those working in classified areas. Thus, when on August 1, 1946, the control of atomic energy passed from military to civilian hands with the formation of the Atomic Energy Commission, Section 10 of the act that formed the Commission contained the following paragraph:

> Except as authorized by the Commission in case of emergency, no individual shall be employed by the Commission until the Federal Bureau of Investigation shall have made an investigation and report to the Commission on the character, associations and loyalty of such individual.[30]

This immediately raised concerns about "guilt by association." The concerns were not eased when President Truman issued Executive Order 9835 on March 25, 1947. This order provided for the loyalty investigation of "every person entering the civilian employment of any department or agency of the executive branch of the Federal Government," and directed that "The head of each department and agency in the executive branch of the Government shall be personally responsible for an effective program to assure that disloyal civilian officers or employees are not retained in employment in his department or agency." Among the "activities . . . which may be considered in connection with the determination of disloyalty . . . " were the following:

> Membership in, affiliation with or sympathetic association with any foreign or domestic organization, association, movement, group or combination of persons, designated by the Attorney General as totalitarian, fascist, communist, or subversive, or as having adopted a policy of advocating or approving the commission of acts of force or violence to deny other persons their rights under the Constitution of the United States, or as seeking to alter the form of government of the United States by unconstitutional means.[31]

Again the question of guilt by association was raised and caused the publication of the famous "Attorney General's List" of subversive organizations. The rigors of this loyalty clearance and the occasional harassment that accompanied it kept some persons from seeking employment in the Government, and some employees to resign. The number of these persons is unknown, but there is little question that the Government lost some valuable people.

While loyalty clearances were being carried out, the House Committee on Un-American Activities was also doing its work, travelling from coast to coast investigating avowed or suspected communists. National headlines were made when, without hearings and based solely on a report by a sub-committee headed by Rep. J. Parnell Thomas of New Jersey, who was also chairman of the full committee, the committee announced that Edward U. Condon, director of the National Bureau of Standards, was "one of the weakest links in our atomic security." The report contained no evidence to substantiate such a charge. This caused a spate of negative editorial

---

[30] *Atomic Energy Act of 1946, U.S. Statutes at Large,* 60 (1946): 767.

[31] President, Executive Order 9835, *Federal Register* 12, no. 59 (25 March 1947): 1935-1939.

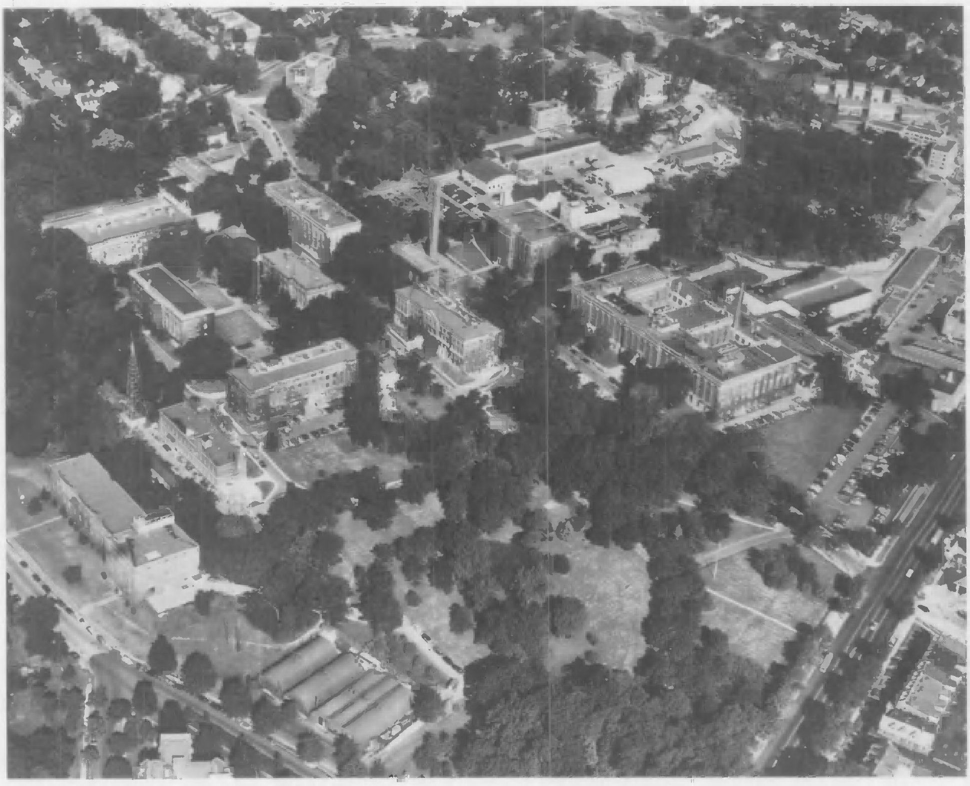
opinion, against Congressman Thomas and great indignation in the scientific community, particularly among scientists who worked on the atomic bomb, many of whom were close personal friends of Condon. The trauma produced by these loyalty investigations was not to be healed for almost ten years. By that time both Robert Oppenheimer and Condon would be stripped of their security clearances.

A UNIQUE INSTITUTION

In 1950, the headquarters of the National Bureau of Standards was located on sixty-eight gently hilly acres on the west side of Connecticut Avenue, overlooking the intersection with Van Ness Street in northwest Washington, District of Columbia, 3.5 miles north of the White House. While most of the Bureau's work was carried out at this headquarters site, it also had work going on at twenty-three other locations. Four materials testing stations, primarily for the testing of cement purchased by the Government, were located in Allentown, Pennsylvania; Seattle, Washington; Denver, Colorado; and San Francisco, California. Two proving grounds for testing weapons and components under development, operated in LaPlata, Maryland, and Tuckerton, New Jersey. A railway-scale test car was based in Clearing, Illinois, and a lamp-inspecting station to certify Government purchases was located in Brookline, Massachusetts. Research in applied mathematics was carried out at the Institute for Numerical Analysis at UCLA as well as at the headquarters site. Radio wave propagation activities were conducted at nine field stations that pretty much spanned the Northern Hemisphere: Anchorage, Alaska; Point Barrow, Alaska; Guam; Honolulu; Puerto Rico; Trinidad; Fort Belvoir, Virginia; Las Cruces, New Mexico; and Sterling, Virginia. These stations provided data on the ionosphere, which formed the basis for monthly forecasts of radio propagation conditions. NBS operated two radio stations that broadcast standard time and frequency signals that were used as both time and frequency standards. These signals were the basis for setting clocks and were widely used for navigation, for setting the frequencies of broadcast stations, and any other uses in which accurate frequency control was important. One station, WWV, located in Beltsville, Maryland,[32] covered the continental United States while another, WWVH,

---

[32] WWV locations have included the Bureau's Van Ness site (1923-1931), College Park, Maryland (1931-1932), Beltsville, Maryland (1932-1966. The location name of Beltsville was changed to Greenbelt in 1961.), and Fort Collins, Colorado (1966 to the present). Wilbert F. Snyder and Charles L. Bragaw, *Achievement in Radio: Seventy Years of Radio Science, Technology, Standards, and Measurement at the National Bureau of Standards*, Natl. Bur. Stand. (U.S.) Special Publication 555; October 1986: 277, 282.



Aerial view of the Washington, D.C. laboratories of the National Bureau of Standards (1952).

on Maui in the Hawaiian Islands, covered the Pacific Ocean. In addition, the Bureau was in the process of acquiring 200 acres in Boulder, Colorado, donated by the citizens of that city, and was making plans to construct a guided-missiles laboratory, which was in due course built in Corona, California.

The total staff working in these locations was 3100, with a total budget of $20 million, approximately 43 percent coming from direct congressional appropriation, the remainder being funds transferred from other agencies of the government, primarily the military.[33] As did the rest of science, NBS had experienced explosive growth during

---

[33] The Bureau is authorized to carry out work on contract for other agencies of the Government. In the vernacular of the institution these are called "other agency," or simply OA, funds.

Field Station at Elmendorf Air Force Base, Anchorage, Alaska, one of several NBS field stations that gathered and disseminated data on the ionosphere.

the war years. In 1941 it had a staff of 1032 and a budget of $3.37 million, 60 percent of which was directly appropriated and the remainder again transferred from other agencies. By 1945 the staff had risen to 2206 and the budget to $9.7 million, but only 33 percent was appropriated.[34] Throughout this whole period the Bureau was doing so much work for the military that it was in danger of losing its identity as the Nation's standards laboratory.[35] And the situation was to get worse. Already in fiscal year 1950, its direct appropriations from the Congress dropped from $8.753 million for the previous year to $8.658 million, and the downward trend was to continue.[36] In 1952, under

---

[34] NARA; RG 167; Astin file; Box 5; Folder Kelly Committee.

[35] For a complete accounting of the Bureau's military work during World War II, see MFP, chapter 7, and Lyman J. Briggs, *NBS War Research, The National Bureau of Standards in World War II* (Washington, D.C.: U.S. Department of Commerce, 1949).

[36] See graphs in Appendix E.

Exterior of NBS radio broadcasting station WWV, Greenbelt, Maryland. From this station, standard radio frequencies of 2.5, 5, 10, 15, 20, and 25 Mc were transmitted continuously. Two standard audio frequencies, 600 and 400 cycles, were broadcast as modulations on each radio frequency.

pressure of work related to the Korean War and other military research, a full 80 percent of the then $53 million budget was provided by the military, and the total staff had risen to 4450. The Bureau was in danger of becoming an appendage of the military establishment, its own legislatively mandated work lost in a sea of weapons research.

Although its mode of operation was as much collegial as hierarchical, the Bureau's staff of 3100 persons was organized in the simplest of hierarchical structures that had not changed in the forty-nine years of its existence. Fourteen divisions, containing a total of 107 sections (the smallest operational unit), carried out all the technical work. These were supported by four support divisions: Budget and Management, Personnel, Plant, and Shops. All the division chiefs reported directly to the director; there were no intervening levels. The Director's Office contained two associate directors, two assistant directors, the Bureau library, and an Office of Scientific Publications, responsible for the mechanics of publication of scientific papers, reports, special publications, circulars, and the other publications that constituted the Bureau's most important output.[37] A listing of the names of the divisions, with the number of their sections, illustrates the areas of work and gives some idea of the distribution of effort:[38]

---

[37] During its history, the Bureau has issued many types and series of publications. These are described in Appendix H.

[38] This listing is as of July 1, 1950. Before this date Optics was combined with Electricity in the Electricity and Optics Division; the Electronics Standards Laboratory, Ordnance Development Laboratory, and Guided Missile Branch were combined in the Electronics and Ordnance Division; and the Commodity Standards Division had not yet been transferred to the Office of Science and Technology in the Department of Commerce.

Personnel who directed the development of the BAT, the first fully automatic guided missile used in combat. In 1942, the National Defense Research Committee asked NBS to handle the aerodynamic and servomechanism aspects of the missile. Hugh Dryden (third from right) supervised the Bureau's effort. Other NBS personnel pictured are Harold K. Skramstad (left), W. Hunter A. Boyd (second from right), and Ralph A. Lamm (right).

Electricity (5), Optics and Metrology (5), Heat and Power (6), Atomic and Radiation Physics (13 in two laboratories: Atomic Physics and Radiation Physics), Chemistry (11), Mechanics (7), Organic and Fibrous Materials (7), Metallurgy (4), Mineral Products (8), Building Technology (5), Applied Mathematics (4), Electronics (3), Ordnance Development (8), Central Radio Propagation Laboratory (7 in three laboratories: Ionospheric Research, Systems Research, Measurement Standards, plus 12 field stations), and Missile Development (5).

The program was extremely broad, ranging from studies in superconductivity to atomic clocks, synthetic rubber, cement testing for Government purchases, atomic spectra, methods of measuring radioactivity, computers, and a great deal of work on military hardware, including proximity fuzes and guided missiles.

THE NATURE AND CHARACTER OF THE BUREAU

The Bureau was not nearly so grand when it officially began operations on July 1, 1901, the law (the "Organic Act") that established it having been enacted on March 3, 1901. In that law the Congress, in carrying out its constitutional authority "to fix the standards of weights and measures"—the march of industrial development had nearly made this an obligation—had charged the new Bureau with:

> the custody of the standards [of measurement]; the comparison of the standards used in scientific investigations, engineering, manufacturing, commerce, and educational institutions with the standards adopted or recognized by the Government; the construction, when necessary, of standards, their multiples and subdivisions; the testing and calibration of standard measuring apparatus; the solution of problems which arise in connection with standards; the determination of physical constants and the properties of materials, when such data are of great importance to scientific or manufacturing interests and are not to be obtained of sufficient accuracy elsewhere.

The law goes on to direct for whom the Bureau should work:

> the Bureau shall exercise its functions for the Government of the United States; for any State or municipal government within the United States; or for any scientific society, educational institution, firm, corporation, or individual within the United States engaged in manufacturing or other pursuits requiring the use of standards or standard measuring instruments.

Clearly, the Bureau was to serve the whole society.[39]

By this law, the Bureau became one of a number of national laboratories established by industrial nations at the end of the nineteenth century and the beginning of the twentieth. Perhaps the foremost amongst them was the German Physikalisch-Technische Reichsanstalt which, in some ways, Samuel Wesley Stratton, the Bureau's first director, used as a model for his institution.[40] In the late nineteenth century, the inexorable march of the industrial revolution, the expansion of science, and the requirements of national and international trade made mandatory a worldwide system of units of measurements and their associated standards. Indeed, in 1875, the *Convention du Mètre,* known in the United States as the Treaty of the Meter, was signed in Paris by the United States and seventeen other countries. By this treaty, the signatory nations adopted the meter and kilogram as legal units of length and mass. The international prototype standards for these units were to be maintained at the International Bureau of Weights and Measures (BIPM), located on extra-territorial land near Sèvres, France, and the United States was allotted copies Nos. 21 and 27 for the meter, and Nos. 4 and 20 for the kilogram. They were received in January 1890. The

---

[39] *An Act to establish the National Bureau of Standards, U.S. Statutes at Large,* 31 (1901): 1449. Full text in Appendix C.

[40] The following statement appears in the director's annual report for 1902, the first in the Bureau's history: "The Physikalisch-Technische Reichsanstalt of Germany is an illustrious example of how much can be accomplished where research and testing are combined in one institution." *Annual Report of the Director,* (1902): 5.

Samuel Wesley Stratton, first director of the National Bureau of Standards, was the primary force in the Bureau's creation and formative years. He established a solid basic research program and organized young scientists into cooperative efforts of applied science that shaped the Bureau to serve the Nation.

customary units of the yard and the pound were defined as 3600/3937 meter and 0.453 592 427 7 kg, respectively.[41] Indeed, the Congress had authorized the legal and permissive use of the metric system in 1866, using standards obtained from France in 1821. The secretary of the treasury, in whose department the Office of Weights and Measures was located, was directed to supply the states with sets of standard metric weights and measures. To this day, the customary units of the yard and the pound have not been legalized, which causes no problems since their relation to the legal standards is fixed.[42]

Perhaps even more important, the burgeoning electrical industry showed that simple standards for mass, length, and time were no longer sufficient. Standards for quantities barely known to the layman, such as volts, amperes, farads, henries, and most

---

[41] Ralph W. Smith, *The Federal Basis for Weights and Measures,* Natl. Bur. Stand. (U.S.) Circular 593; June 1958: 15.

[42] For a full and excellent account of these matters, see Rexmond C. Cochrane, *Measures for Progress: A History of the National Bureau of Standards,* Natl. Bur. Stand. (U.S.) Miscellaneous Publication 275, 1966: Appendix B. Hereafter referred to as MFP.

The platinum-iridium cylinder (right) is the primary standard kilogram for all metric measurements of mass in this country. Known as Kilogram No. 20, it is a copy of the international prototype kilogram, which is preserved at the International Bureau of Weights and Measures (BIPM) at Sèvres, France. Kilogram No. 4 (left, under double bell jar), a duplicate of No. 20, is used as a secondary standard.

important, kilowatt-hours, that being the unit by which electricity was sold, had to be developed and made uniform throughout the Nation and the world. X rays and radioactivity had only recently been discovered, and both units and standards for them had to be devised. All of these required research, in most cases basic research into new areas of physics and chemistry. The simple offices of weights and measures had to be replaced with more sophisticated institutions.

Compared to the adult institution it would become in 1950, when the Bureau began operation as part of the Treasury Department it was only a tiny infant. It started in temporary quarters in three buildings just south and east of the Capitol: eight rooms formerly used by its predecessor, the Office of Weights and Measures, in the Coast and Geodetic Survey building; eight additional rooms in the adjacent Butler Building; and a remodeled residence down the street from them. Its authorized staff, aside from its director, consisted of twenty-two people in three divisions: Scientific, Engineering, and Office. There was one physicist, two assistant physicists, five laboratory assistants, four laborers (one skilled), one engineer, one assistant engineer, two mechanics, one watchman, one secretary, two clerks, one storekeeper, and one messenger. Its total budget was about $67 000.

Still in its fledgling stage, the work of the new Bureau was concerned with the central part of its basic mission: providing the standards of measurement and the comparison of the standards used in scientific investigations, engineering, manufacturing, and commerce with the national standards. In fact, in the first year of its operation, the Bureau carried out little more than its basic weights and measures functions. But it was making plans for the future, both with respect to facilities and with respect to the technical work. Thus, 7.5 acres (3 hectares) at the Connecticut Avenue site had been chosen, and plans for a physical laboratory building and a mechanical laboratory building were being drawn. The latter building was to contain the power plant as well as heavy items of research equipment, and specifications were being drawn for either the purchase or construction of necessary laboratory instruments.

With respect to the technical work, a large number of standards for mass, length, and capacity (volume) were verified (i.e., compared to the national standards) for Federal and state governments and for private concerns. Considerable effort was devoted to improving the instruments used for this comparison in order to speed up this rather routine but essential activity. The need for higher accuracy of comparison was strongly felt. Three items were particularly troublesome. First was the calibration of chemical glassware for volume. This had to be done by the individual chemists, or the glassware purchased from Germany, whence it came certified by the Physikalisch-Technische Reichsanstalt (PTR). Second was "the design and construction of a model set of weights and measures that shall be adapted to the needs of State, county, and city sealers."[43] Previous sets had been provided in 1836 and 1866, and were no longer adequate to meet new requirements. Finally, there was considerable confusion with respect to the calibration of hydrometers, and this had to be cleared up. These problems now seem almost quaint and charming, and are a vivid reminder of the state of technological development of the Nation at the turn of the century.

The problems were even greater outside the realm of weights and measures. In thermometry, mercury-in-glass thermometers could be calibrated only over the temperature range from –20 °C to 50 °C, which was totally inadequate for the times. The Bureau was making plans to extend the calibration range to 1500 °C on the high end, and to –190 °C on the lower end, the latter made necessary by the recent large-scale liquefaction of air and other gases.

While hardly well-equipped, the Bureau was making progress in electrical standards. It had purchased resistance standards from 0.0001 to 100 000 ohms. Comparison equipment (presumably bridges) had been purchased "so that the Bureau is already equipped for the measurement of resistance standards submitted for verification *in terms of those belonging to the Bureau* to the highest order of accuracy."[44] (Italics added.) But the need for a primary standard was sorely felt, and the construction of the mercury column of specified dimensions that had been defined by the International Electrical Congress of 1893 as the unit of resistance, and legalized by the Congress, was begun. The Nation was in the strange position of having legalized a standard for resistance that it did not own.[45]

The situation with the volt was in some ways similar. The Bureau had constructed several Clark electrolytic cells that constituted the legal definition of the volt, but research was already under way to find more reproducible and stable cells. Nevertheless,

---

[43] Annual Report, 1902: 10.

[44] Ibid., 12. It is not stated where the resistance standards were obtained. It is known that several 1 ohm resistors were obtained from Germany, and periodically checked against the PTR primary standard (MFP, p. 79). Presumably the other resistors also came from the PTR, but they could have been made by the Bureau.

[45] *An Act to Define and Establish the Units of Electrical Measure, U.S. Statutes at Large,* 28 (1894): 101. Following their adoption by the International Electrical Congress in 1893, the U.S. Congress made legal the units adopted by that congress. The units so legalized were the ohm (resistance), the ampere (current), the volt (electromotive force), the coulomb (quantity), the farad (capacity), the joule (work), the watt (power), and the henry (inductance). These were called the "international units." Their relation to absolute units were a source of continuing research.

the Bureau could carry out calibrations with respect to the legal volt. Ammeters and voltmeters could, however, be calibrated only up to 50 amperes and 150 volts, but preparations were being made to extend this range to 1500 amperes and 2000 volts. But these were only direct current measurements. Alternating current was becoming more and more popular for the transmission of electricity, so the Bureau was establishing an alternating current laboratory. Along with that came the problems of the determination of capacitance and the calibration of standards of self and mutual inductance.

Finally a photometry laboratory was established, and work in this difficult but essential field was begun. With the veritable explosion of incandescent lighting, there was considerable pressure for measurement standards for illumination.

This was the organization that developed into the National Bureau of Standards of 1950, with a site on which existed 138 structures (in fairness, some of them quite small) and with field stations spanning the Northern Hemisphere. It looked upon itself quite correctly as being far more than a simple office of weights and measures. In the opening words of its Annual Report for 1950 it describes itself as follows:

> The National Bureau of Standards is the principal agency of the Federal Government for basic and applied research in physics, mathematics, chemistry, and engineering. In addition to its general responsibility for basic research, the Bureau undertakes specific research and development programs, develops improved methods for testing materials and equipment, determines physical constants and properties of materials, tests and calibrates standard measuring apparatus and reference standards, develops specifications for Federal purchasing, and serves the Government and the scientific institutions of the Nation in an advisory capacity on matters relating to the physical sciences. The Bureau also has custody of the national standards of physical measurement, in terms of which all working standards in research laboratories and industry are calibrated, and carries on necessary research leading to improvement in such standards and measurement methods.

The seeds of the growth and metamorphosis from a glorified office of weights and measures into a full-fledged, broad-based, internationally known and respected scientific laboratory were contained in the Organic Act. The Act contains the wonderfully ambiguous phrase that directs the Bureau to work on "the solution of problems which arise in connection with standards," and the less ambiguous but equally open-ended phrase authorizing the Bureau to engage in "the determination of physical constants and the properties of materials, when such data are of great importance to scientific or manufacturing interests and are not to be obtained of sufficient accuracy elsewhere." Placed in the law at the instigation of Stratton, both phrases are permissive;[46] they can encompass an almost endless scope of work.

---

[46] This word to describe the Organic Act appears to have been first used by Robert D. Huntoon in an unpublished report to Edward L. Brady, June 1977, on the nature and character of the Bureau. A number of other ideas in that perceptive report stimulated some of the discussion which follows.

The resulting breadth of work was crucially important in determining the character of the Bureau. It had the permission, if not the obligation, to become the principal agency in the Federal Government for basic and applied research.[47]

Equally important, although perhaps not so obvious at first blush, is that the Organic Act gave the Bureau no regulatory—policing—responsibility or authority. While it was charged with defining the unit of mass, for example, it had no authority to go to a manufacturer of scales to determine whether his scales were accurate—in accordance with the national units—or not. This is a crucial point that ultimately controls the nature of the institution that evolves from the central standards and measurement function. If called upon, it can be the final arbitrator in disputes relating to the validity of measurements, but it cannot be the instigator of disputes. This is left to other agencies of state and Federal Government. And the arbitration function is not that of listening to the testimony of plaintiff and defendant and then issuing a judgement. The function is rather that of determining scientific truth: what is the actual value of the quantity in question and what are the limits of uncertainty in the knowledge of that value? What is in fact known or can be known, and with what uncertainty? Its judgment is a scientific judgment, not a legal one, and the arena where scientific truth is sought is the laboratory, not the courtroom, for no scientific result is accepted as fact until it has been exhaustively confirmed. But this arena is not without its own appellate courts. These are the courts of public opinion, scientific scrutiny, and politics. An institution, if it is to function effectively in such an arena, must be, and must be perceived to be, objective, impartial, and totally unbiased. Moreover, if its pronouncements are to be accepted, its work must be technically impeccable. Technically slipshod work will quickly lose the institution its most precious attributes: integrity and technical credibility.[48]

The fact that its position with respect to disputes is limited only to aspects of measurement and the discovery of scientific truth does not, however, mean that the institution is powerless. Quite the contrary. The mere fact that it is custodian of the national standards and is a potential arbiter in disputes gives it *ab initio* enormous power. Its pronouncements and publications carry great weight and are scrutinized carefully and thoroughly. Again this puts great pressure on the institution to ensure the technical accuracy of its work, but some publications can nevertheless cause considerable consternation. There are various examples of this, from national safety codes

---

[47] An obverse and undesirable consequence of permissiveness is that it can lead to performance of unimportant and irrelevant work. For the Bureau, this is mitigated, if not precluded, by a number of oversight mechanisms. The Organic Act established a visiting committee that visits the Bureau at least yearly and reports to the secretary "upon the efficiency of its scientific work and the condition of its equipment." Further and continuing oversight is provided by the secretary, by the Congress, by other agencies for which the Bureau performs work on transferred funds and, beginning in 1953, Advisory Committees reporting to the Bureau director on each of the Bureau's major organizational units. And the wisdom of Bureau management cannot be discounted.

[48] Perhaps the best examples of the Bureau's work in this arena are provided by the analysis of the causes of failure of structures.

Although NBS had no regulatory duties or power to initiate investigations, if called upon it could serve as an impartial arbiter. Here, Charles H. Oakley lowers a 10 000 lb weight onto a railway-scale test-car in order to test the Union Pacific scale. In 1913, at the request of the Interstate Commerce Commission, NBS began testing railway scales. These scales were the source of continual complaints, and the railways, fearing for their reputations, were eager to rectify the situation.

proposed early in the Bureau's history[49] to the most famous case of all, the battery additive controversy.

Given a permissive law, and the absence of regulatory responsibility, it was predictable that the work of the Bureau should lead it to become—or at least look upon itself as—the principal agency in the Federal Government for basic research in the physical sciences. But, in the absence of any regulatory responsibility, the Bureau might well have become what it did even without the permissive phrases. Once the Bureau became a laboratory doing research on improving the measurement methods on which new and more accurate standards are based, it is at least an arguable proposition that the growth and metamorphosis it underwent would have happened. All science, engineering, and industry are based on measurements whose accuracy is based on standards. Machine parts made in Cleveland must fit machines made in Detroit; a kilowatt-hour in Arkansas must be the same as one in Florida; a gallon of gasoline in California must have the same volume as one in Connecticut; the frequency of a radio station—not yet a consideration in 1901—must be accurately known lest it interfere with neighboring stations; a joule of energy in a cyclotron beam at Berkeley must be the same joule delivered by an electric generator in New Jersey, and both must have a constant and well-known relation to a calorie produced in a reaction in a research laboratory in Delaware; and on and on, throughout all of industry, commerce, and science. For, in the oft-quoted but always pertinent words of Lord Kelvin:

> I often say that when you can measure what you are speaking about and express it in numbers you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meager and unsatisfactory kind: it may be the beginning of knowledge, but you have scarcely, in your thoughts, advanced to the stage of science, whatever the matter may be.[50]

Thus, with this all-pervasive position of measurements in the scientific, commercial, and industrial life of the Nation, the institution whose principal purpose was to ensure the uniformity and accuracy of those measurements had to become conversant and expert, at the level of basic national standards, with all the measurements made in the country. And it had to provide measurement methods and the standards on which they are based so that all elements of the Nation might speak the same language. Equally important, it had to provide a system which would ensure that all the measurements made in the Nation were in accordance with—"traceable to," in the language of the

---

[49] MFP, 121. Presumably using its authority to work on "the solution of problems which arise in connection with standards" (Organic Act, Sec. 2), and that provided by special appropriations from Congress, in the early years of the century the Bureau became concerned with several safety problems. Among them were those associated with the generation, transmission and use of electricity. In 1914 it proposed a national electrical safety code. This met with strong resistance from public utilities for a number of years. However, faced with confusing regulations by state public utilities commissions, the utilities came to welcome the Bureau's scientific and rational approach. Note that the Bureau had no authority to impose the code.

[50] William Thomson Kelvin, 1st Baron, *Popular Lectures and Addresses*, v. 1, *Constitution of Matter*, 2nd ed., (London: Macmillan, 1891): 80-143. "A Lecture delivered at the Institution of Civil Engineers on May 3, 1883; being one of a series of Six Lectures on 'The Practical Applications of Electricity.' "

trade—the national standards in its custody. The ubiquity of measurements and their increasing diversity in a technologically expanding economy made growth and metamorphosis inevitable. It is only natural that in the process of carrying out its central measurement mission, the Bureau developed technical and organizational capabilities that could be turned to the solution of other technical problems of the Government or of the Nation in general.[51] In due course this was to lead the Bureau into a whole class of work that was not an inevitable consequence of its basic mission, but was an outgrowth of the mission having been defined broadly, and the institution being permitted to use its initiative to develop appropriate responses to these problems. The actual type of work carried out would change constantly as national goals and problems changed, and this work would envelop the basic mission in an ever-changing frame.[52] In carrying out work in this "dynamic periphery," new knowledge would be obtained that fed back into the work and the capabilities of the basic mission, and conversely, so that the two types of work led to a dynamic symbiosis that greatly strengthened the institution, and brought its relations with industry and the rest of government into closer—but not always frictionless—contact.

The first example of this dynamic periphery was the testing of Government purchases.[53] The Federal Government is a large purchaser of supplies of enormous variety for its own use. The Bureau, from its very early years, got involved in testing these purchases to ensure compliance with specifications, and indeed the preparation of those specifications. Beginning in 1904 with the testing of incandescent lamps, it progressed rapidly to other commodities until by World War I all of its divisions were involved.

This work brought the Bureau into direct contact with industry. Specifications could not be so tightly drawn that they could not be met, nor so loosely that they would be of little value in ensuring a useful item. Meetings and full-scale conferences involving all interested parties were held, and often new, non-existent, test methods had to be developed. As a result, this work expanded beyond simple testing to full-scale research programs in the commodity involved, and this was in part to spawn three new divisions: Metallurgy (from the testing of structural iron and steel),[54] Mineral Products (from cement testing), and Organic and Fibrous Materials (from paper, textiles, leather goods, etc.). The results of all this work was to be of great benefit to the industry

---

[51] For a large part of its history, the Bureau was the only physical science laboratory concerned with civilian problems in the Federal Government. It was perhaps inevitable that its help would be sought when scientific or technical problems arose in the Government, or that the Bureau would unilaterally undertake such problems.

[52] Robert D. Huntoon coined the apt term "dynamic periphery" for this work.

[53] For a full account of the origins of testing for government purchases see MFP, 124 -133.

[54] Metallurgy also had its origins in the study of the cooling curves of metals to provide fixed points on the temperature scale. Since impurities affect freezing points and the temperature constancy of the melting equilibrium, work in this area led on the one hand to the preparation of very pure materials and on the other to the study of the melting behavior of alloys, i.e., phase diagrams.

involved and ultimately to the whole Nation. And because of this testing and specifications work, the Bureau was drawn into working closely with, and indeed becoming a leader in, the voluntary standards system of the United States.[55] Its staff held key positions in the societies that comprise that system, and the data presented by Bureau representatives to the working committees of experts of the societies were instrumental in helping develop the myriad specifications, test methods, design standards, recommended practices, and other publications that form the library of output of the voluntary standard system. In this manner, the Bureau contributed to industrial and commercial standards which are emulated throughout the world.

Even more important was the Bureau's work on solutions to national technical problems, exclusive of work during times of war. There arise from time to time problems, usually concerned with public safety, that the Government needs to investigate and do something about. A recent example is environmental pollution for which a whole new agency was formed and for which the Bureau has provided considerable assistance. Two early examples will illustrate the Bureau's role in these problems.

Very early in its history, the Bureau became involved in electrochemistry for two reasons. First, the international definition of the ampere was the amount of silver that was deposited in a given time by electrolysis, and the international definition of the volt was based on the voltage developed in a standard electrolytic cell. Research on both these areas was designed to make the measurements more reproducible and accurate, and this led the Bureau into research in electrochemistry. The talents developed in this work in the basic mission were to be turned to an unusual problem: underground corrosion caused by stray currents from street railways.[56] First begun in 1887, by 1917 there were over 40 000 miles of street railways.[57] Power was supplied to the street car by a trolley wire and, in theory, was to flow back to the generating plant through the rails. All conduction in these metallic items is electronic so that no problems of electrolysis arise. However, the street railway tracks are not the only metallic structures in the ground. Particularly in cities, there are gas and water mains, lead-sheathed electrical cables, and other metallic structures. In favorable conditions of soil conductivity, these items can provide a lower resistance path for the return current, so that it can flow from the tracks through the soil into the structure, through the structure, and then back into the rail when conditions of proximity and soil conductivity are again favorable. The problem here is that conduction through the soil is ionic, not electronic. When current flows out of a metallic structure by ionic conduction, metal ions flow

---

[55] A discussion of the various usages of the word "standard" is found in Donald R. Mackay, ed., *A Glossary of Standards-Related Terminology,* Natl. Inst. Stand. Technol. (U.S.) NISTIR 89-4194; October 1989: 7.

[56] MFP, 119-121.

[57] The system of street railways was very extensive, being inter-urban as well as intra-urban. E. L. Doctorow, in his well-known novel *Ragtime* (New York: Random House, 1974), gives a detailed account of a trip from New York to Boston by street railway at the turn of the century. The total cost was $2.40.

out of the structure into the soil, removing metal from the structure and degrading it. Corrosion takes place.[58]

During excavations in Boston in 1902, badly corroded water mains were found, and similar conditions in other items were found elsewhere. Losses from this source of material degradation were estimated in millions of dollars, and considerations of public safety raised a cry of alarm. Taking the initiative, the Bureau asked for and received a three-year special appropriation to investigate the problem. Progress was slow in this very difficult area. While methods were rather quickly devised to pinpoint the places where this stray current corrosion was taking place, and possible solutions to the problem were devised, they were very expensive. It was not until almost two decades after the initiation of the investigation that the solution of using sacrificial anodes was devised. In this method, a piece of relatively active metal, such as zinc, is attached to the corroding structure. The active metal, rather than the structure, then conducts the current from the metal to the soil. The reactive anode is sacrificed to save the structure. By this time the Bureau's work had expanded to the corrosion of metals in the ground in the absence of stray currents, and the Bureau had formed a Corrosion Laboratory to study this national problem.

A second early example of the Bureau's work on national problems is provided by failures of railroad equipment.[59] Concerned about failures occurring in railroads, Congress in 1910 passed legislation requiring monthly reports of railroad accidents. Two years later, the Interstate Commerce Commission (ICC) reported the alarming results that there had been almost 13 000 deaths and injuries from collisions and derailments alone in the previous year, and for the years 1902 to 1912 there had been a total of 41 578 derailments caused by broken wheels, rails, flanges and axles.[60] At the urging of the secretary of commerce, the Bureau undertook to study this problem and received a special appropriation from Congress in 1912 for the work. Thus began a long study of railway materials in the newly-formed Metallurgy Division. Working with the steel companies, nothing less than a thorough analysis of the metallurgy of tracks and wheels, and the manufacturing processes by which they were made, had to be carried out. Even then progress was slow in coming. It was not until 1923, when the special appropriation ended and the Bureau carried out further work under its own appropriation, that progress began to be observed. By 1930, the accident rate from these two causes had fallen by two-thirds. Work on these rail and wheel problems dropped shortly thereafter, but started again in 1985 with funds provided by the Federal Railroad Administration of the Department of Transportation, which now has the responsibility of ensuring railway safety.

---

[58] In many cases, the current flow is localized at specific sites, leading to preferential metal removal from those sites and eventual puncture. This phenomenon is called "pitting corrosion."

[59] MFP, 118-119.

[60] As a comparison, for the years 1982 to 1987, there was a yearly average of 3538 accidents and 762 fatal and non-fatal injuries. These data are from the *Federal Railroad Administration Accident/Incident Bulletin,* No. 156, 1987.

This work on the dynamic periphery had an important, indeed almost crucial, role in determining the nature and character of the Bureau. In the early examples noted above, no other agencies were responsible for the problems in question, and the Bureau either acted unilaterally or at the suggestion of another agency (e.g., the ICC in the matter of railroad failures). It carried out the work under special appropriations from the Congress. During World War I, the Bureau was called upon to carry out a great deal of work for various agencies of the military.[61] Funds for part of this work came from special wartime appropriations from Congress, but part of them came from a wartime measure, the "Overman Act," passed on May 20, 1918.[62] This law authorized the transfer of funds from one agency to another for the performance of work which the first agency needed but did not have the necessary staff or facilities to carry it out. The Bureau had received more than $500 000 from military agencies under this arrangement. By the end of the war, the Bureau's size had more than doubled, and it had a large number of uncompleted projects for the military. But with the end of the war, the "Overman Act" expired, and transfer of funds from the military was no longer authorized.[63] Indeed, to get $100 000 transferred from the Quartermaster Corps to the Bureau, Stratton went directly to President Wilson. Reversing a course against transferred funds that he had previously set, Stratton, in his appropriation request for fiscal year 1921, suggested that the following passage be included in the appropriations bill:

> [T]he head of any department or independent establishment of the Government having funds available for scientific investigations and requiring cooperative work by the Bureau of Standards . . . may, with the approval of the Secretary of Commerce, transfer to the Bureau of Standards such sums as may be necessary to carry out such investigations.[64]

This statement, or variants of it, was repeated in subsequent appropriations bills, and eventually became law. The Bureau had become a contract research organization within and for the Federal Government. The camel of "other agency" work, as it became known at the Bureau, had entered the tent and was never to leave.

If all that a national standards laboratory did were to keep its prototype standards in a vault and only worked on increasing the accuracy of realizing those standards, it would not be a very useful institution. It also must ensure that the measurements made in the Nation are consistent with, and traceable to, those national standards. The Organic Act recognizes this function in two clauses, "the comparison of standards used in scientific

---

[61] See MFP, Chapter 4 for a thorough account of the Bureau's work in World War I. Also see *War Work of the Bureau of Standards*, Natl. Bur. Stand. (U.S.) Miscellaneous Publication 46; April 1921.

[62] MFP, 213; *An Act Authorizing the President to coordinate or consolidate executive bureaus, agencies, and offices, and for other purposes, in the interest of economy and the more efficient concentration of the Government, U.S. Statutes at Large,* 40 (1918): 556.

[63] Transfer of funds from one agency to another was practiced on a sort of unofficial basis before the Overman Act. Why the Bureau and the military did not simply continue to transfer funds is not known.

[64] MFP, 214; *An Act Making appropriations for the legislative, executive, and judicial expenses of the Government for the fiscal year ending June 30, 1921, and for other purposes, U.S. Statutes at Large,* 41 (1920): 683.

investigations, engineering, manufacturing, commerce and educational institutions with the standards adopted or recognized by the Government . . . " and "the testing and calibration of standard measuring apparatus." In effect, these two statements direct the standards institution to set up a system by which the Nation may be maintained on a common and consistent measurement basis.[65]

Specifically recognized by these statements are direct calibration and testing, perhaps the most basic elements of the system. Clearly, the first step in ensuring that measurements are made in accord with the national standards is to compare to the standards the instruments by which the measurements are made. Thus the Bureau has always had a calibration service. Interested customers can send in instruments or components—sets of master weights, master gage blocks, thermometers, electrical meters and components of various kinds—and have them calibrated (or "verified," in the words of 1902) against the national standards.[66]

The states, which have the responsibility for enforcing weights and measures under the state laws, are an important part of the measurement system. Throughout its history, the Bureau has issued—with some fanfare—sets of standards for mass, length, and capacity to the states, which have become the working legal standards for the Nation. It has also cooperated with the states more generally on other standards problems, such as ionizing radiation, and by such mechanisms as the National Conference on Weights and Measures and the National Conference of State Building Codes and Standards.

Calibration is not, however, the only means of maintaining the National Measurement System, and sometimes is not even feasible, as in the measurement of composition. Another means is to distribute objects, or materials, one or more of whose properties are certified by the Bureau. Called "standard samples," they began to be sold by the Bureau in 1905 when it undertook to distribute and certify the composition of samples of various types of iron provided by the American Foundrymen's Association.[67] In the ensuing year, at the request of the Association of American Steel Manufacturers, the Bureau began the preparation and certification of samples of seventeen types of steel, and thus was the Bureau's standard samples program born.

In 1950, the Bureau had a whole catalogue of standard samples. Each of these samples had a property (e.g., composition) certified by the Bureau to have a specified value or to be within a specified range. The purchaser of such a standard sample could then use it to calibrate his measuring instruments or procedures. In a sense, the Bureau sent its standards to the purchaser, who then carried out the calibration procedure. By 1951 there were 502 standard samples. A full 98 of these were samples of steel certified for the concentration of up to ten elements, and a total of 172 were samples

---

[65] The system by which the Nation keeps itself on a consistent measurement basis has been a subject of considerable scholarly analysis by R. D. Huntoon who called it the "National Measurement System."

[66] These instruments and components are not compared directly with the national standards, but against working standards which are essentially replicas of the national standards and are periodically compared to them. The national standards are too precious for routine use.

[67] MFP, 93.

of other materials also certified for composition. The largest category was for hydrocarbons and organic sulfur compounds, with 224 samples, each certified for purity. Produced with the help of the American Petroleum Institute, these were used for research in the petroleum industry and for developing mass spectrographic methods for the analysis of product streams in petroleum refineries. Other samples are interesting for the property certified: Five pure metals—aluminum, copper, lead, tin, and zinc—were certified for melting points, which range from 1083.2 °C (copper) to 231.90 °C (tin), and are clearly for the calibration of thermocouples and pyrometers. Eight oils were certified for viscosity, ranging from 0.02 poise to 460 poise. Thirty-three samples were certified for radioactivity, including radon standards, radium gamma-ray standards, cobalt gamma-ray standards, and twelve rock and ore samples certified for radium content. But perhaps the most fascinating is a set of ten enameled iron placques [sic] of Standard Colors for Kitchen and Bathroom Accessories. The Nation's measurement system involved a lot more than mass, length, time, and volts.

If calibrations and standard samples can be thought of as the "hardware" of the measurement system, then publications and conferences can be thought of as the "software." Aside from its many research publications, which are often concerned with methods of measurement, from time to time the Bureau published unabashedly tutorial documents on measurements and standardization problems. Three published near 1950 illustrate the nature of these publications: Circular 470, *Precision Resistors and Their Measurement,* by James L. Thomas in 1948; Circular 476, *Measurement of Radioactivity,* by Leon F. Curtiss in 1949; and Circular 490, *The Geiger-Mueller Counter,* also by Curtiss in 1950. In character these publications ranged from introductory papers directed at new workers in a field to short monographs directed at advanced workers. This effort culminated in 1969 with the publication of the first of ten volumes entitled *Precision Measurement and Calibrations.* Published as NBS Special Publication 300, these ten volumes are a collection of previously published papers by Bureau staff members on measurement aspects of various topics, e.g., temperature, time and frequency, photometry and radiometry, and heat.

From time to time, conferences on particular topics are held at the Bureau. But there is one yearly conference that deserves special mention. Beginning in 1905, after two years of trying, the Bureau convened a conference of state weights and measures officials. The object of the conference was the discussion of both the technical and administrative problems in administering the weights and measures programs of the various states. Only six states attended the first conference, but the idea caught on. Called the National Conference on Weights and Measures, meetings have been held yearly since 1905, with the exception of war years and some depression years, so that the eightieth meeting took place in 1994. Each meeting has a published report. In 1950, 143 officials from 34 states, the District of Columbia, and Puerto Rico; 123 representatives of business and industry; and 24 persons from Federal agencies attended. These conferences, and others like them, such as the National Conference on State Building Codes and Standards, have done a great deal to ensure the uniformity of measurements—and of the administration and uniformity of weights and measures regulations—throughout the Nation.

The best way to transfer knowledge and skill from one person to another is for people to work together. Thus, since 1920 the Bureau has had a program by which, under specified conditions, technical, industrial, and commercial organizations can send a person or persons to work with a member of the Bureau staff, thus getting the benefit of the Bureau facilities and the knowledge of its staff. Such persons are called Research Associates. The conditions under which such an arrangement could be made were (and are) that the project be of value to both the supporting organization and the Federal Government, and the Nation at large. All results are published. By 1950, more than 175 organizations had supported Research Associates at the Bureau, and in that year, 13 groups supported 62 associates. Perhaps the best-known arrangement is with the American Dental Association, which has supported a group of Research Associates at the Bureau since 1924. These associates and their Bureau colleagues carry out the bulk of the Nation's research on dental materials. Other associates have worked on fuels, electron tubes, commercial adsorbents, electrodeposition, corn products, cement, concrete, standards for x-ray diffraction analysis, chinaware, porcelain enamel, and asphalt roofing.

The National Bureau of Standards in 1950, then, was a many-faceted, multi-functional institution. It was a respected scientific laboratory with a broad program of studies. It performed work for other agencies of the Government, and carried out research in support of its own basic mission. Most important, and the function from which its nature and character derived, it was custodian of the national standards. The institution that has this function is a unique institution, and this made the Bureau unique, both in function and character. Usually invisible, it was present when any physical measurement was made, and since measurements pervade a modern industrial society, so also did its presence. And having no regulatory power, it could not force itself upon the Nation, but it was nevertheless the final arbiter in measurement questions, at least in principle. To function effectively in this position its work had to be thorough, scholarly, and technically impeccable, and as an institution it had to be perceived to be honest, objective, and totally apolitical. And this was how it was almost universally regarded.

It functioned well in this basic measurement mission. The technical work needed to compare instruments with the basic standards, to increase the accuracy of realization of those standards, and to develop new standards for new or old quantities requires painstaking attention to detail, thorough scholarship, and study of all the factors that can affect the accuracy of the result. In its requirements of thoroughness and attention to detail, the work is as much scholarship as it is scientific research. It requires a persnickety mind. This philosophy of work rubs off onto the rest of the institution, so that it becomes objective, scholarly, and attentive to detail—just the qualities needed to function in the unique position it holds in the Nation. The character of the institution derives from its function.

Its relative position in the Federal Government had, however, changed in the forty-nine years of its history. No longer was it the only physical science laboratory in the Government, as it had been for a large part of its history. The National laboratories had been formed in the immediate postwar period. The military had formed its own laboratories, and would form more. But in one important aspect, the Bureau's position had remained the same. These other Government laboratories had, and still have, specific—though often widening—missions, the National laboratories in atomic energy, and the military laboratories in the requirements of their specific branch of the military. These laboratories could not be expected to concern themselves with breaking train axles or rails. The Bureau, on the other hand, was quite different. While it had a very specific basic mission, the remainder of its enabling legislation was so broad that it could work in almost any area of science it could justify, and it was called upon to work on national problems as they arose. And it was allowed to perform work under contract for other agencies of the Government, so that in 1950 a full 57 percent of its work was carried out for other agencies. In a corporate analogy, the other laboratories of the Federal Government were, and are, divisional laboratories, doing research to foster their divisions (and hence, of course, the corporation). The Bureau, on the other hand, was (and is) the "corporate," or "central," laboratory, concerned with all of the problems of the corporation (Nation), and in the process carrying out contract research for the divisions (other agencies).

THE TECHNICAL WORK

After all is said and done, after all questions of function, character, policy, size and resources are answered, the products of the Bureau that really matter to the Nation are the accomplishments of its laboratories—the technical work of its scientists, engineers, assistants, technicians, craftsmen, and administrative personnel. In this, of course, it is no different from any other high-quality research laboratory. Only its unique position makes it special.

What, then, did these 3100 people, working on their 68-odd acres in Washington and their 23 field stations, actually turn out? To give even a condensed accounting of what was done would mean reproducing the Annual Report for 1950, a clearly inappropriate course here. Rather than that, some examples will be given of the Bureau's work, first in the relatively routine area of calibration, testing, and standard samples, and then some from its research work.[68]

The testing and calibration work is a direct outgrowth of the Bureau's custody of the Nation's basic physical standards, as has been described. While the development of methods of test and calibration can involve considerable research, once these methods are established, the actual testing and calibration can be made rather routine, although great care and skill are still required. This is fortunate, for the volume of work is great. Thus, in 1950, over 250 000 tests and calibrations were performed for other Government agencies and for the general public, and 19 000 standard samples were distributed. This included sample-testing of 9 million barrels of cement, and 4 million light bulbs purchased by the Government. The latter involved the actual life testing of 5000 bulbs, roughly one in 800. About 2300 raw sugar samples were assayed for the Customs Service to assist it in determining import duties.

---

[68] The description of the work in testing, calibration, and standard samples comes directly from the Annual Reports. The description of the research work is considerably expanded from what is found there.

Some interesting activities in this testing and calibration area were:

- Over 2000 radium preparations were tested, principally for Government and private hospitals and clinics where they were used for radiation therapy. All such preparations sold in the United States were tested and certified by the Bureau, since no commercial laboratory was equipped to do this work.
- Nearly 1100 radioactivity standard samples were sold. In 1941, the Surgeon General's office requested the Bureau to establish a program to protect the life and health of people working with radium. The Bureau began systematic measurements of the radon (a product of the radioactive decay of radium) content of the air in the areas where persons worked and, in 1950, 898 such determinations were made.
- Over 14 000 items of electrical apparatus were tested for manufacturers, electric utilities, public utilities commissions, universities, private testing laboratories, and Federal agencies.
- A total of 63 366 calibrations of measurement standards for such quantities as length, area, angle, mass, volume, and density were carried out.
- A total of 15 318 thermometers, including 3078 liquid-in-glass laboratory thermometers, 132 resistance thermometers, 289 thermocouples, and 11 819 clinical thermometers were tested or calibrated.
- Five pursuit cars, 18 motor truck speed governors, and 615 automotive spark plugs were tested for Government agencies.
- The Federal tax on beer in 1950 was $800 million. The Bureau tested 266 beer meters, whose accuracy is essential for the correct computation of the tax. A beer meter measures the total volume of beer produced by a brewery.
- Road tests of tires were made in collaboration with the Post Office Department and the National Capital Parks Police. These showed a variation of almost two to one in the wear rate of tires made from different manufacturers.
- Electron tubes for various purposes were tested for other Government agencies.
- Instruments and devices of almost every conceivable type for radio were tested and calibrated.

The diversity in the items in this list, ranging from the esoteric to the mundane, illustrate the enormous variety of the Bureau's work. But despite the impressive numbers in this recitation, these tests, calibrations, and standard samples do not represent the main output of the Bureau. Most of its work was research, both in its own mission and in work for other agencies of the Government. The bulk of this work was reported in the open scientific literature, although many reports—often classified—were prepared for the sponsoring agencies, which in 1950 were mainly military agencies and the Atomic Energy Commission. The development of devices like the magnetic clutch, and a currency counter developed for the Treasury Department, also formed part of the Bureau's output.[69] Thus, in the fiscal year ending on June 30, 1952, Bureau staff published 1500 papers and reports.[70] Of these, 1000 were reports,

---

[69] The magnetic clutch is described in the Annual Report, 1948: 24, and the currency counter in the Annual Report, 1950: 273.

[70] Annual Report, 1952.

This model auto bus, equipped with a magnetic fluid clutch and a magnetic fluid brake, was constructed at NBS to demonstrate the potential application of the magnetic fluid principle to automobile clutches. Jacob Rabinow, inventor of the clutch, operated the model.

classified and unclassified, most of them for other agencies of the Government. Approximately 300 papers were published in the open scientific literature, 115 in the Bureau's own *Journal of Research,* and the remainder in professional society journals. In addition, 118 summary reports were published in the Bureau's *Technical News Bulletin,* and monthly data that permitted the choice of the best frequencies for long-range radio communication were published in *Basic Radio Propagation Predictions,* another publication of the Bureau. Finally, there were forty-three longer papers published in the Bureau's nonperiodical series: six in the Applied Mathematics series; five in the Handbook series; sixteen in the Circular series; six in the Building Materials and Structures series, and five in the Miscellaneous Publications series.

Out of this wealth of output, six projects—chosen somewhat arbitrarily—offer examples of the Bureau's scientific work. All, along with many others, are found in the Annual Reports. While the treatment in three of the topics is somewhat technical, the main intent is to describe the reasons for the Bureau's involvement in this work. This usually involves some history. In these technical topics, the lay reader may simply accept the scientific assertions made and, it is hoped, still follow the story. Footnotes are used both for explanation and for greater exposition for the technical reader.

Length and Light: Natural Standards *v.* Artifacts

As long as the meter—the national standard of length—is the distance between two fine scribed lines on a beautifully made bar of platinum-iridium alloy, the only way for the whole Nation to be on a common length basis is to compare measuring instruments with this national standard. Calibration is, however, a time-consuming process for both the calibrating laboratory and the user of the calibration service. Moreover, calibration does not of itself ensure measurement accuracy. It is only one step in the measurement process. An error in any of the steps—something as mundane as having the laboratory at the wrong temperature during a calibration—can degrade measurement accuracy. The time spent in calibration could be more fruitfully spent in ensuring the integrity of the whole process.

It would be much better if the platinum-iridium meter bar (called an "artifact" in the trade) were replaced with a natural phenomenon or constant that could be used as a standard. Such a "natural standard" would be available to anyone—or at least anyone with the requisite scientific expertise—for calibration purposes. The central length artifact would become redundant, and the national standards laboratories would be relieved of the calibration business—something all would very much desire.[71] It is thus not surprising that quite early in the industrial revolution, natural standards as alternatives to artifacts for the measurement of length should have been sought. As early as 1827, Jacques Babinet suggested that "a wavelength of light would be an ideal unit of length."[72] Then in 1892 Albert A. Michelson, of Michelson-Morley fame, compared the red line of cadmium with the international meter bar and obtained a value of 6438.4696 Å for the wavelength of the spectral line.[73] This was adopted in 1907 as the primary standard definition of the angstrom, and was checked several times in the subsequent half-century.[74] In 1889, Michelson and Edward W. Morley, in a paper entitled "On the Feasibility of Establishing a Light-Wave as the Ultimate Standard of Length," wrote, "The brilliant green [mercury] line . . . in all probability this will be the

---

[71] In practice, of course, this would not happen, partly because most people would not have the requisite skill, and partly because calibration by the national standards laboratory is a desired certification. Note, however, that such certification is not an assurance of measurement accuracy. Accuracy in the calibration laboratory does not ensure proper use, and hence accuracy, in the field.

[72] W. F. Meggers, "What Use Is Spectroscopy?" *Applied Spectroscopy* 6(4) (1952): 4-10.

[73] The angstrom unit, denoted by the symbol Å, is 10^{-10} meters.

[74] Meggers, "What Use Is Spectroscopy?" It is a remarkable fact that the wavelength of light—of the order of 600 billionths of a meter—can be measured more precisely than the distance between the scribes on the meter bar.

wave to be used as the ultimate standard of length."[75] The prediction was premature. In 1892 Michelson discovered that "the green line of mercury is one of the most complex yet examined."

No spectral line is perfectly sharp, i.e., consists of light of a single wavelength. All lines contain a distribution of wavelengths. The fewer these wavelengths—the "narrower" the line—the more it is suitable as a length standard. Now, a number of factors contribute to the width of the line. First, there is a natural width caused by inherent quantum mechanical characteristics of the line itself. Second is the temperature needed to excite the line. Third is the pressure, and fourth is the effects of electric and magnetic fields. But most important for the purpose here are so-called isotope shifts.[76] The same line from different isotopes of the same element are slightly different in wavelength. Natural mercury is a mixture of seven different isotopes with mass numbers 196, 198, 199, 200, 201, 202, and 204.[77] The brilliant green line that Michelson and Morley had proposed as the "ultimate standard of length" in fact consisted of seven very closely spaced lines. As a length standard, naturally occurring mercury was not very useful.

However, as suggested by Jacob H. Wiens and Luis W. Alvarez, the advent of nuclear reactors made it possible to prepare pure mercury-198 by transmutation of gold, and to use the wavelength of the light from it as a length standard.[78] Beginning in 1947, William F. Meggers and F. Oliver Westfall of the Bureau's Electricity and Optics Division undertook to have this isotope prepared and to study its spectrum to see if in fact it could be the ultimate standard.

The preparation of the isotope is relatively easy. Gold, of atomic mass 197, when irradiated with neutrons gives the radioactive isotope gold-198, which, by emission of an electron, decays with a half life of 2.7 days to mercury-198. The mercury is easily recovered from the gold by distillation. Meggers and Westfall had gold irradiated in a nuclear reactor by the Atomic Energy Commission, and from this they recovered 60 mg of mercury-198 of 99.9 percent purity.[79] From this they made four lamps, each containing 5 mg of the mercury isotope and pure argon at a pressure of 5 mm Hg, and proceeded to study the spectral width of the green line, and other lines in the spectrum of mercury. All the experimental details that had to be worked out cannot be covered here. Suffice it to say that the items to be investigated were the type of lamp, the method of exciting the spectrum, the longevity of the lamp, and the myriad details of the measurement of the wavelength of the lines. This last was done relative

---

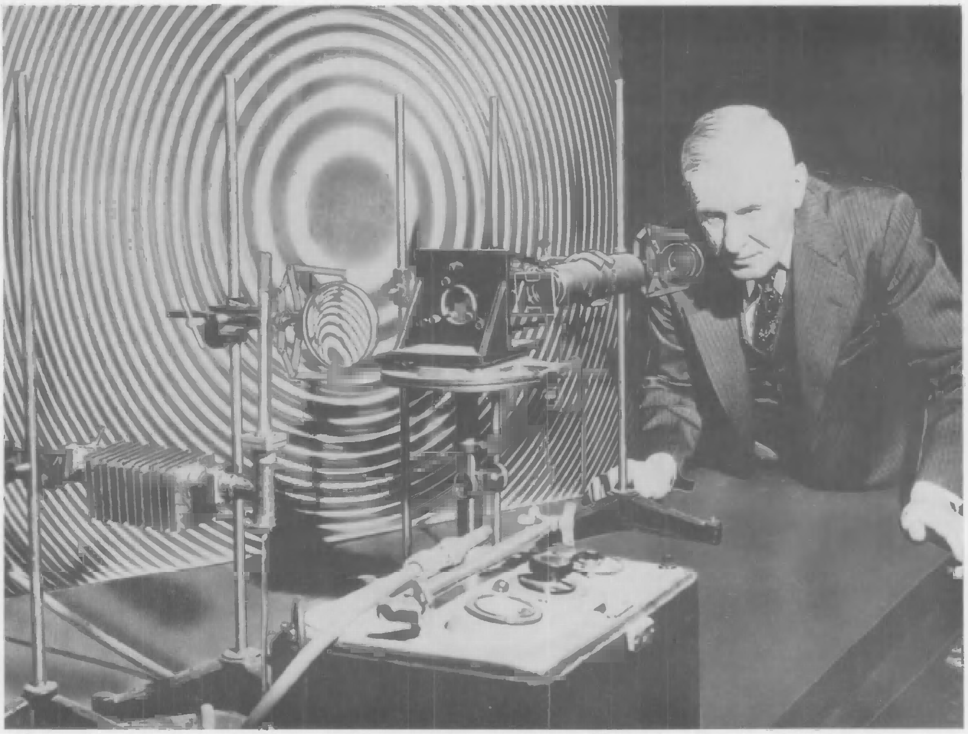
[75] A. A. Michelson and E. W. Morley, "On the Feasibility of Establishing a Light-Wave as the Ultimate Standard of Length," *American Journal of Science*, 3rd series, 38(225) (1889): 183-186.

[76] Isotopes of an element are chemically identical but differ in their atomic mass.

[77] W. F. Meggers and F. O. Westfall, "Lamps and Wavelengths of Mercury 198," *Journal of Research of the National Bureau of Standards* 44 (1950): 447-455.

[78] J. H. Wiens and L. W. Alvarez, "Spectroscopically Pure Mercury (198)," *Physical Review*, 58 (1940): 1005; J. H. Wiens, "Production of Hg^{198} as a Possible Source of an Improved Wave-Length Standard," *Physical Review* 70 (1946): 910-914.

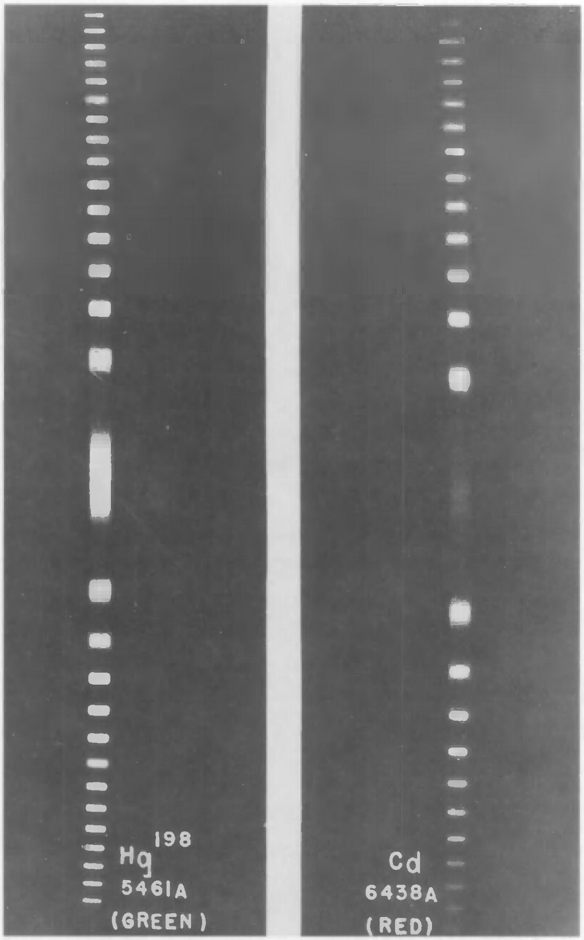
[79] The mercury-198 used here was from the same batch as that used in the studies of the isotope effect in superconductivity.



William F. Meggers peered through an electrodeless mercury-198 lamp which was made available to science and industry as an ultimate standard of length in 1951. Length measurements based on the circular interference fringes of green light from the lamp (background) could be made with an accuracy of one part in 100 million.

to the wavelength of the cadmium red line by interference techniques, since the cadmium line had been adopted by spectroscopists as a secondary standard of length, and as the definition of the angstrom. Indeed, the lack of sharpness of the latter is what determined the precision of the measurements. Nine mercury lines were measured, with the value of 5460.7532 Å obtained for Michelson's "ultimate standard of length" green line. The accuracy obtained was one part in 100 million, and preliminary results obtained at the National Physical Laboratory in England and at the International Bureau of Weights and Measures (BIPM) in France were in agreement with the NBS results.

As frequently happens, this was not the last word. In due course an even more precise wavelength standard for length was to be adopted in 1960, and this replaced the old meter bar, which then became a historical relic. But the standard was not the green line of mercury-198. It was the even narrower orange-red line of krypton-86, with the meter being defined as the length of 1 650 763.73 wavelengths. However, the mercury-198 line has continued in use as a reliable and convenient working standard.



Relative sharpness of the mercury line compared to the cadmium line.

Applied Mathematics and Computers

The previous example of the Bureau's work was concerned with its unique measurement mission. As previously discussed, this was only a part of the Bureau's work. In particular, in 1950, a full 57 percent of the Bureau's work was performed for other agencies of the Government. Work in computers and applied mathematics illustrates the nature of one of these other agency activities.

In 1950, the Bureau was no stranger to applied mathematics. Since 1938, it had been involved in the administration and sponsorship of the Mathematical Tables Project in New York. Begun as a Works Progress Administration project in 1938 to compute tables of mathematical functions, in 1943 the support of the project had been assumed by the Office of Scientific Research and Development. Subsequently, war problems were undertaken by the Project. In the fall of 1946, OSRD support was withdrawn, but was replaced with support from what is now the Office of Naval Research. It had been a very successful project, 28 000 volumes of its tables having been sold to the general public between 1940 and 1946.

Partly because of this background, and in large measure because of his personal experience, when Edward U. Condon became director of the Bureau in 1945, he began to set up a program in applied mathematics, and hired John Hamilton Curtiss, then a lieutenant commander in the Navy, and previously professor of mathematics at Cornell University. Curtiss became an assistant to the director with special instructions to be concerned with the statistics of measurements. But developments conspired to give him and the Bureau much greater responsibility.

In 1945, computers were becoming a magic word. Everyone wanted one, but few could make them. Two who had proved themselves were J. Presper Eckert and John W. Mauchly of the University of Pennsylvania, who had produced the successful ENIAC (Electronic Numerical Integrator and Computer). Thus, when they approached the Bureau of the Census in 1946 with a proposal to build a computer for that agency to help with analysis of the 1950 census results, Census paid attention. The Census Bureau turned to the National Bureau of Standards for help and advice. In April 1946, the Census Bureau transferred funds to NBS, which was to select and purchase an appropriate computer. Early in 1947, the Bureau contracted with Eckert and Mauchly for the Census computer, now to be called UNIVAC (Universal Automatic Computer).

Soon the Bureau was swamped with computer money and obligations. The Army Ordnance Department transferred funds to the Bureau for research and development of computer components. Almost immediately, the Office of Naval Research (ONR)

John H. Curtiss, chief of the National Applied Mathematics Laboratories from 1946 to 1953, was responsible for the development of computers, statistical service to government and private industry, research aimed at extending the part played by applied mathematics in scientific research, and training scientists in the methods used in this field.

transferred funds to the Bureau for the procurement of a computer. In due course, the Bureau contracted for this computer with the Raytheon Company in early 1947. Meanwhile, early in 1946, Admiral Harold G. Bowen of ONR approached Condon with the idea that ONR and NBS jointly set up a laboratory that would be equipped with high-speed computing machinery, lead in the development of such machinery, and serve as a central computation facility. Finally, in 1947, two more UNIVAC's were ordered: one for the Air Comptroller and one for the Army Map Service.

A year of study on Admiral Bowen's proposal "revealed the need for a Federal center of applied mathematics. . . . Accordingly, the plans which finally emerged proposed that a facility with a mission considerably broader than that of a central computing laboratory should be established; further, that it should take the form of a new division of the National Bureau of Standards."[80] Following this advice and to organize all the new responsibilities the Bureau had acquired, Condon, in 1947, established the National Applied Mathematics Laboratories as Division 11 of the Bureau, and appointed Curtiss as its chief. The new NAML consisted of four units: The Institute for Numerical Analysis at UCLA, a Computation Laboratory which was to be a development of the old Work Projects Administration (WPA) project, a Statistical Engineering Laboratory, and a Machine Development Laboratory. The last three were located at the Washington site. Thus, partly because of the desires of its director and partly because of the needs of the Navy—and using primarily military money—the Bureau had a new activity.

By 1948, it became clear that none of the computers which had been ordered would be completed on schedule. In the meantime, the Bureau had made plans to build a small "interim" computer, partly because of the delay in delivery, and partly to gain experience in machine construction and design.[81] This activity was supported by the Air Comptroller, and was soon expanded to construct a full computer rather than an interim device.

The Bureau was in a good position to build a computer. During World War II it had gained great expertise in electronics and production of electronic components and devices.[82] Its work on the proximity fuze and in guided missiles had led it to specialize in miniaturization of components, and into the development of the printed circuit. It was therefore undaunted at the prospect of building a computer—it was simply another electronic device, albeit more complicated. Indeed, under the previously mentioned contract with the Ordnance Department, the Bureau was developing basic computer components: memory organs, input-output equipment, specialized electron tubes for gating, switching, signal delay, interval timing, and pulse shaping.[83]

Construction of the computer was begun in the fall of 1948 in the NBS Electronics Division by a group under Samuel N. Alexander, and with active collaboration by

---

[80] J. H. Curtiss, "A Federal Program in Applied Mathematics," *Science* 107 (1948): 259.

[81] Harry D. Huskey, "The SWAC, The National Bureau of Standards Western Automatic Computer" in *A History of Computing in the Twentieth Century*, ed. N. Metropolis, J. Howlett, and Gian-Carlo Rota (New York: Academic Press, 1980: 419-431.

[82] MFP, 451-462.

[83] "A History of NBS Computer Developments," *Technical News Bulletin* 51 (1967): 168-172, 181.

members of the Machine Development Laboratory. By modern standards the specifications for the computer were modest enough: 512 forty-four-bit words of mercury-delay-line memory (later expanded with 512 words of electrostatic memory), a cycle time of one megahertz, and input and output by teletype punched paper tape (later replaced with magnetic wire and tape)—but they were the state of the art. An item of note and a harbinger of the future is that all the logical operations of the computer were carried out by germanium diodes; vacuum tubes were used only for amplification. Thus it was the first computer to use solid-state electronics extensively. It was dedicated on June 20, 1950, just twenty months after construction had begun. It was first named Standards Electronic Automatic Computer, but later re-named as Standards Eastern Automatic Computer (SEAC). The change was occasioned by the building of a companion computer at the Institute for Numerical Analysis at UCLA and naming it the Standards Western Automatic Computer (SWAC), dedicated on August 17, 1950. The latter computer was quite different in logical design and construction, being a parallel machine in which all the digits of a number in memory are changed simultaneously, had electrostatic memory rather than mercury-delay-line memory, and also used a magnetic drum memory.

At the time SEAC was dedicated it was the only stored program machine in the United States, and the fastest such machine in the world. It was not, however, the first stored program machine internationally, having been preceded by the EDSAC at the

Among those who prepared SEAC for its pioneering role were Ralph J. Slutz, examining the paper tape punched with input data, and Samuel N. Alexander, examining the computer's printout.

University of Cambridge, a machine at the University of Manchester, and probably the ACE machine at Cambridge.[84] SEAC worked until April 1964, when it was retired, and its remains are at the Smithsonian Institution. From its initial operation it worked on numerous important problems. The first one was the tracing of skew rays through a compound lens. This was followed by many others: the solution of partial differential equations by Monte Carlo methods, the generation of optimum sampling plans for the Census Bureau, the calculation of transient stresses on aircraft structures, the development of accounting procedures for the Social Security Administration, problems in crystal structure and the relative abundance of the elements, the wave functions of atoms; and the designs of the synchrotron and of electric circuits. It was extensively used by the AEC for calculations on highly secret projects, believed by some of the Bureau staff to be associated with the hydrogen bomb. It was also used in what was probably the first automatically calculated Earth-Moon trajectory.

The Isotope Effect in Superconductivity

Superconductivity is a fascinating property exhibited by some materials. Below a well-defined temperature, the material loses all electrical resistance, and an electric current induced in a superconducting loop can in principle continue to flow indefinitely. The temperature at which the normal state transitions to the superconducting state occurs depends on the magnetic field; a sufficiently strong magnetic field will prevent superconductivity at all temperatures. But perhaps most important for the Bureau's purposes, it occurs at very low, but well-specified, temperature—only a few degrees above absolute zero, at least for the superconductors known in 1950. The reason for this importance lies in the nature of standards for temperature scales.

Unlike standards such as those for mass, length, and electrical resistance, the unit of measurement of temperature—the degree, either Celsius, Fahrenheit, or kelvin—cannot be stored in a vault, to be removed periodically to standardize measuring instruments. What can be stored in a vault (but more likely in a laboratory rather than in a vault) is a device to measure temperature—a thermometer. But what establishes a temperature scale is not a thermometer, which does nothing but give an indication of some kind (e.g., the length of a fine column of liquid-in-glass capillary tube) when its temperature is changed. What establishes the scale is a series of "fixed points." Thus on the Celsius scale (previously called the "centigrade" scale), the temperature of ice in equilibrium with air-saturated water at a pressure of one atmosphere (the fixed point called the "ice point") is defined as zero degrees. And the temperature of boiling water, again at one atmosphere (the fixed point called the "steam point"), is defined as 100 degrees. The corresponding temperatures on the Fahrenheit scale are 32 and 212 degrees, respectively. Assigning temperatures to these two fixed points defines the size of the degree, and defines the temperature scale over this temperature range. Assigning other fixed points (usually boiling and melting points of pure substances) extends the scale beyond the ice and steam points, but no temperature can be lower than "absolute zero,"

---

[84] Ralph J. Slutz, "Memories of the Bureau of Standards' SEAC" in *A History of Computing in the Twentieth Century*, ed. N. Metropolis, J. Howlett, and Gian-Carlo Rota (New York: Academic Press, 1980): 476.

which occurs at –273.15 °C on the Celsius scale—a value established with the gas thermometer, a precise first-principle instrument used in laboratories specializing in temperature measurement.[85] But in any case, fixed points are natural phenomena and hence accessible to anyone with adequate equipment. Because of this accessibility they are in a sense superior as standards to stored artifacts. Anyone can use them to calibrate any kind of thermometer.

Ever since Kamerlingh Onnes at the University of Leiden discovered in 1908 how to liquify helium, physics at very low temperatures became an active and fascinating field of research. Helium itself showed a very interesting and totally unexpected property. At a temperature of 2.18 K, 2.03 K below its boiling point, it showed a dramatic drop in its viscosity as the temperature was lowered.[86] It became a "superfluid" and apparently remained so down to absolute zero. Equally striking, Onnes discovered in 1911 that mercury, at a temperature of 4.15 K, lost all electrical resistance; it became a "superconductor." By 1950, twenty pure metals and a large number of alloys were known to be superconductors.

Because of the intrinsic interest of their electrical properties, and because of their possible use as temperature fixed points at very low temperatures, it was natural that the Bureau should be interested in superconductors at a very early date. Thus it is not surprising that as early as 1918, Francis Silsbee, then an assistant physicist in the Electricity Division, should be concerned with superconductivity. He in fact enunciated what was to become known as the Silsbee effect.[87] It was known that currents higher

---

[85] The temperature scale defined by fixed points, and the thermometers used in various ranges of temperature is called the International Practical Temperature Scale. The thermodynamic, or kelvin, scale uses only one fixed point, the triple point of water (i.e., ice in equilibrium liquid water under its own vapor pressure). This temperature is, by definition, 273.16 K. Because of the temperature difference between the triple point of water and that of ice and water in equilibrium in one atmosphere of air, the temperature of the triple point of water on the Celsius scale is 0.01 °C. Absolute zero on the Celsius scale is –273.15 °C and, of course, 0 K on the kelvin scale. However, the size of the degree on the kelvin scale (called simply a "kelvin") is identical to that of the degree on the Celsius scale. The International Practical Temperature Scale, determined by a number of fixed points, is maintained in a periodically updated agreement with the kelvin scale.

In 1950, four fixed points besides the ice and steam points were defined by the International Bureau of Weights and Measures. These were the boiling point of oxygen at –182.970 °C; the boiling point of sulfur at 444.600 °C; the freezing point of silver at 960.8 °C; and the freezing point of gold at 1063.0 °C. Other fixed points are added periodically to define the scale further. Different thermometers are used in different temperature ranges.

[86] The temperature of the boiling point of helium was deduced from the platinum resistance thermometer. Below this point, temperatures were calculated from the vapor pressure of helium and its heat of vaporization using the Clausius-Clapeyron equation.

[87] Francis B. Silsbee, "Note on Electrical Conduction in Metals at Low Temperatures," *Bulletin of the Bureau of Standards* 14(2) (1918): 301-306. The paper contains this perceptive passage at the end: "The theories thus far proposed to account for superconductivity . . . do not specifically indicate the existence of a critical magnetic field, and only the latter accounts for a threshold-current density. . . . If it is true . . . that the magnetic effect is the more fundamental, it would seem that this fact might afford a valuable clue leading toward a more satisfactory theory of the superconducting state. . . . " In 1918 the Bureau had no facilities for the production of liquid helium and hence no experimental program in superconductivity. Since Walther Meissner and R. Ochsenfeld discovered in 1933 that the behavior of superconductors in a magnetic field is indeed crucial in understanding the nature of the superconducting state, one is led to wonder what might have happened if the Bureau had had an experimental program in superconductivity.

than a critical value destroyed superconductivity. Silsbee hypothesized that the value of this critical current was such that the magnetic field it caused at the surface of the current-carrying superconductor was just enough to destroy the superconductivity. This conjecture was justified experimentally several times.[88]

Silsbee's conjecture was purely theoretical. Experimental work could not be done, for while the Bureau had had a program in low-temperature physics since 1904 when it purchased a hydrogen liquefier, it could not reach the temperatures of interest for superconductivity until 1948 when it purchased a helium liquefier.[89] Results came quickly. That the superconducting transition temperature might depend on the atomic mass, and hence the isotopic composition, had been conjectured several times, and attempts to measure this effect had been made with isotopes of lead.[90] No effect was found, probably because temperature control at the transition temperature of lead (7.26 K) is difficult. In 1950, with the development of atomic energy research, pure isotopes of several superconducting metals became available, so it became possible to look for the effect again.

Working with a 98 percent pure sample of mercury-198, and natural mercury with an average atomic weight of 200.6, Emanuel Maxwell of the Heat and Power Division's Low Temperature Physics Section found a difference of 0.021 K in the superconducting transition temperatures. The lighter isotope had the higher transition temperature.[91] At a transition temperature of approximately 4.15 K, a difference of 0.021 K is substantial. At the same time (both papers were received by the editor of the *Physical Review* on March 24, 1950), Charles A. Reynolds, Bernard Serin, Wilbur H. Wright, and Lloyd B. Nesbitt of Rutgers University announced similar results for four different isotopic compositions of mercury.[92] In a further analysis of their results, the Rutgers group announced that the transition was approximately proportional to the inverse square root of the isotopic mass.[93]

As these results were being obtained, and quite independently of them, Herbert Fröhlich, of the University of Liverpool, was developing a theory of superconductivity. While his paper was in proof, he learned of the experimental results and added a note pointing out that his theory predicted that the transition temperature should be

---

[88] Russell B. Scott, "Destruction of Superconductivity by Current," *Journal of Research of the National Bureau of Standards* 41 (1948): 581-588; and references therein.

[89] MFP, 466.

[90] E. Maxwell, "Superconductivity of the Isotopes of Tin," *Physical Review* 86 (1952): 235-242, gives references for these early attempts.

[91] E. Maxwell, "Isotope Effect in the Superconductivity of Mercury," *Physical Review* 78 (1950): 477.

[92] C. A. Reynolds, B. Serin, W. H. Wright, and L. B. Nesbitt, "Superconductivity of Isotopes of Mercury," *Physical Review* 78 (1950): 487.

[93] C. A. Reynolds, B. Serin, and L. B. Nesbitt, "Superconductivity of Isotopes of Mercury," *Physical Review* 78 (1950): 813-814.

inversely proportional to the square root of the isotopic mass.[94] Upon learning of the Fröhlich theory, both Maxwell and the Rutgers group carried out more extensive measurements. The Rutgers group, re-analyzing their previous data, which had some ambiguities, were first to show that the theoretical prediction was indeed correct.[95] Maxwell, working with isotopes of tin later, also confirmed the theoretical prediction.[96] A new scientific fact had been discovered. It is interesting to note that the work at both the Bureau and at Rutgers was supported by the Office of Naval Research.

In its early attempts to explain the effect, the Bureau felt that "the nucleus must have an important effect on the superconducting properties of the metal."[97] This is, of course, true, but is little more than a re-statement of the experimental results. Fröhlich, and in due course John Bardeen, Leon N. Cooper, and J. Robert Schrieffer, in their Nobel Prize theory of superconductivity, showed that the interaction of the electrons with the lattice vibrations is the crucial element in determining superconductivity. All else being equal (as in isotopes), the frequency of the lattice vibrations varies inversely with the square root of the isotopic mass, and this is the origin of the isotope effect. The use of superconductors to define temperature fixed points would have to be done with great care, and would involve using pure isotopes.

The Charters of Freedom

For a country that reveres the documents on which it is based—the Declaration of Independence, the Constitution, and the Bill of Rights, aptly called the "Charters of Freedom" by the National Archives—the United States treated these documents rather cavalierly for about the first hundred years of their history. Indeed, Verner Clapp, former chief assistant librarian of the Library of Congress, wrote, "The Declaration of Independence is one of the most abused documents in the history of preservation of documents."[98] Other authorities somewhat more charitably blame the lack of knowledge of conservation science for the condition of the documents. The Bureau's activities in the preservation of the Charters is an excellent illustration of how its abilities could be turned to unusual problems, and of its role as the Nation's corporate laboratory.

---

[94] H. Fröhlich, "Theory of the Superconducting State. I. The Ground State at the Absolute Zero of Temperature," *Physical Review* 79 (1950): 845-856. This paper was received on May 16, 1950. There is an interesting sideline to this story. Fröhlich learned of the experimental results when he was spending some time at Purdue University lecturing on his theory. At the beginning of one of the sessions he excitedly announced the experimental results, and that they showed that there was an isotope effect, as his theory predicted. In the audience was Ralph P. Hudson, who was shortly thereafter to join the Bureau and eventually become chief of the Low Temperature Physics Section and the Heat and Power Division.

[95] B. Serin, C. A. Reynolds, and L. B. Nesbitt, "Mass Dependence of the Superconducting Transition Temperature of Mercury," *Physical Review* 80 (1950): 761.

[96] E. Maxwell, "Superconductivity of Sn^{124}," *Physical Review* 79 (1950): 173.

[97] Annual Report, 1950: 33.

[98] V. Clapp, "The Declaration of Independence; A Case Study in Conservation," *Special Libraries* 62 (1971): 503.

Of the three documents (a total of seven sheets of parchment), the Declaration is in far worse shape than the others. The ink is now so faded that it is practically illegible. The signatures of the delegates to the Continental Congress are in particularly bad condition. The document suffered great tribulations. Following its engrossment,[99] signing of the parchment document took place on August 2, 1776, but not all the delegates signed at that time. During the Revolutionary War it followed the Continental Congress in all its moves. It was stored in a rolled-up configuration, being rolled from the top down. Periodically it was unrolled so that other delegates could sign it, and obliteration of the signatures presumably began.[100]

In July 1789, the Declaration and the other Charters were given into the custody of the Department of Foreign Affairs (renamed Department of State on September 15 of the same year), and they travelled from New York to Philadelphia, to Washington, thence to Leesburg for three weeks while the British occupied Washington in 1814, and finally back to Washington.

Most importantly, in 1820 Secretary of State John Quincy Adams, apparently concerned about the legibility of the Declaration, commissioned an engraving from William J. Stone. Stone transferred an image from the parchment document onto a copper plate by what was probably a wet process, which further degraded the image of the original. But he did make an engraving from which reproductions could be made, and all present copies of the Declaration come from that engraving. Perhaps Secretary Adams acted wisely, despite the fact that the process degraded the original. The copper plate is now at the National Archives.

The Patent Office was located administratively in the State Department and it had a nice, bright, white-painted room. In 1841 the Declaration was given to that office for display. It hung in the Patent Office for thirty-five years opposite a window and exposed to sunlight. Even in the absence of the body of knowledge about the preservation of documents then available, this action would appear to have been taken without a great deal of thought. The other documents were not on display, but were taken out of storage to show to important visitors.

In 1876, the Declaration was exhibited at the Centennial Exposition in Philadelphia, where its appearance elicited considerable concerned comment. This spurred Congress to appoint a commission "to have resort to such means as will most effectively restore the writing of the original manuscript . . . with the signatures appended thereto. . . ."[101] Nothing was done. In 1877, the Declaration was put on display in the new State, War, and Navy Building (now the Executive Office Building), but then in a room where smoking was permitted and in which there was a fireplace. Finally, following the recommendations of two committees of the National Academy of Sciences, all the Charters were carefully wrapped and stored in the dark in a steel case. Proper care of the documents was at last beginning to occur.

---

[99] To engross is to prepare the usually final handwritten or printed text of an official document. All of the "Charters of Freedom" were handwritten.

[100] Elizabeth Hawthorn Buck, "The Declaration as a Document," *Manuscripts* 10(3) (1958): 6.

[101] David C. Mearns, *The Declaration of Independence: The Story of a Parchment* (Library of Congress, 1950): 3.

In 1920, a third committee of preservation experts, formed this time by the secretary of state, wrote after examining the Declaration, "We see no reason why the original document should not be exhibited if the parchment be laid between two sheets of glass, hermetically sealed at the edges and exposed only to diffused light."[102]

Nothing was done in the State Department because on September 29, 1921, President Warren G. Harding ordered that all the documents save the Bill of Rights be transferred to the Library of Congress.[103] There the documents received great attention. A marble and bronze shrine was built for them on the west wall of the second-floor gallery where no direct sunlight could strike them. They were placed below two panes of glass between which there was an orange-yellow gelatin filter to further protect the documents from degradation by light. The documents were not, however, in hermetically sealed cases.

Not everything was perfect even in this regal setting. A report came that a visitor had seen a silverfish on one of the documents.[104] There were further reports that buffalo beetles were in the documents.[105] Moreover, the Library was not air-conditioned, so the documents were subjected to large changes in the relative humidity and, because of their location in the Library, to large changes in temperature. And science had shown that air pollutants could hasten degradation of documents, and in this setting the Charters were exposed to the ambient air. Thus, in 1940, Archibald McLeish, then Librarian of Congress, asked the National Bureau of Standards to look into the best method of displaying the documents.

The Bureau was a good place to look into this matter. Because of its work in commodity testing, it had been concerned with the durability of organic materials—paper, textiles, leather, fur skins—since early in the century. Two of its staff members—Bourdon W. Scribner and Arthur E. Kimberly—were authorities on paper, and authors of an extensive review on the preservation of records, paying special attention to the effect of sulphur dioxide as an air pollutant.[106] The Bureau quickly accepted McLeish's request and on March 16, 1940, Bureau Director Lyman J. Briggs sent to the Library a short report containing the following recommendations:

> It is recommended that both documents be inclosed within sealed receptacles, and that the air within these receptacles be replaced with a chemically inert gas, such as nitrogen, helium, or argon, the gas to contain approximately 4 grains of moisture per cubic foot. . . . This would eliminate the danger of having excessive moisture in the documents at any time. Storing . . . in an inert gas will remove the possibility of deterioration from oxidation or from acid hydrolysis resulting from absorption of sulphur dioxide from the atmosphere.[107]

---

[102] Ibid., 7-8.

[103] The Bill of Rights was transferred to the National Archives in 1938.

[104] Clapp, "The Declaration of Independence": 505.

[105] Interview with E. Carroll Creitz, July 29, 1987: 3. (NIST Oral History File)

[106] A. E. Kimberly and B. W. Scribner, *Summary Report of Bureau of Standards on Preservation of Records*, Natl. Bur. Stand. (U.S.) Miscellaneous Publication 154; March 1937.

[107] "Inspection of the Original Copies of the Declaration of Independence and the Constitution of the United States," report from Lyman J. Briggs to Martin A. Roberts, Assistant Librarian of Congress, March 16, 1940.

Nothing was done on this matter during the war years, except that the documents were moved to Fort Knox. The subject was re-opened in 1946, and the Bureau was asked by the Librarian of Congress to "take any steps necessary to insure the preservation of . . . the Constitution, the Bill of Rights, and the Declaration of Independence."[108]

As it had previously recommended, the Bureau decided to seal the documents in an inert atmosphere in glass cases, and a full-scale project was begun under the leadership of Gordon M. Kline, chief of the Plastics Section, and subsequently chief of the Division of Organic and Fibrous Materials.[109] There were a number of technical problems to be solved:

The Library of Congress delivered the Declaration of Independence and the Constitution of the United States to NBS.

---

[108] Letter, E. U. Condon to John D. Briggs, President of Libbey-Owens-Ford Glass Company, August 12, 1947. The reference to the Bill of Rights appears to be an error, for that document was in the custody of the National Archives, and the Bureau was not asked to encase it until May 9, 1952, by Robert H. Bahmer, Acting Archivist of the United States.

[109] *Preservation of the Declaration of Independence and the Constitution of the United States of America,* Natl. Bur. Stand. (U.S.) Circular 505; July 1951.

1. The production of the enclosure and the sealing of the documents in it.
2. The inert gas to be used.
3. Control of the relative humidity in the enclosure.
4. Detection of leaks.
5. Provision of protection from harmful radiation.

The first problem was easily solved. At that time, the Libbey-Owens-Ford Glass Company produced thermopane windows. These are essentially two parallel panes of glass with a hermetically sealed space between them. They were made by depositing a border of metal along the edge of a pane, soldering a dam of lead to this metal border, facing this dam with another pane with a deposited border, and then soldering that border to the lead dam. This gives a shallow box with glass front and back and lead sides. Placing the documents in the box prior to the final soldering step hermetically seals them in the box. Libbey-Owens-Ford was asked to participate in the project. They accepted, and in fact it was their craftsman, Louis Gilles, who constructed the glass enclosures and did the sealing of the documents in the cases.

The selection of the inert gas was simple. Helium was the obvious choice because of its very high thermal conductivity, which permitted leak detection by an ingenious means. Cells for measuring the thermal conductivity of gases had been in use at the Bureau for a long time as a method of gas analysis.[110] While such cells are now commercially available, in 1950 they were homemade. Essentially each cell is a helix of platinum wire through which a current is passed. The temperature, and therefore the electrical resistance, of the helix depends on the thermal conductivity of the surrounding atmosphere, and thus changes in its thermal conductivity are easily detected by measuring resistance. In the particular application for the Charters, four such cells were used. Two, outside the cases, were sealed in small copper tubes containing helium, and two, open to the ambient atmosphere, were sealed into the cases. These four sensors were then arranged in a bridge circuit so that a change in resistance of any one of them could easily be detected. Immediately after the final sealing of the cases, all cells were exposed to the same atmosphere of helium, and hence the bridge was in balance. If any air leaks into the cases, the thermal conductivity of the atmosphere inside the case drops and the bridge shows imbalance. The whole system was calibrated so that the amount of leakage could be determined. It was an ingenious way to detect leaks.

Moisture control of the atmosphere inside the enclosure is essential to prevent degradation. Too low a humidity leads to dehydration and embrittlement of the parchment document, and experiment and experience showed that humidity higher than 85 percent leads to a deterioration of parchment. High humidity also leads to the growth of micro-organisms. Experiments had shown that the ideal humidity was between 25 percent and 35 percent. The problem was how to stabilize the humidity, for without stabilization the humidity would rise as the temperature decreased, and fall as the temperature increased.

---

[110] Creitz, oral history: 9.

Copper tubing which led from a pure helium source was connected to the inlet tube of the test enclosure containing the Declaration of Independence in order to flush all air from the assembly. E. Carroll Creitz sealed the joint on the enclosure after the air had been completely removed.

Stabilization was accomplished by placing sheets of pure cellulose paper within the enclosure as a backing to the documents. Because of the great affinity of cellulose for water, this paper acts as a stabilizing reservoir of moisture, releasing moisture when the humidity decreases, and absorbing moisture when the humidity increases. The paper must be pure cellulose lest impurities in it cause its degradation with possible release of degradation products that could be injurious to the parchment documents. This paper was produced in the Bureau's own experimental paper mill.

The radiation filter required some research. Experiments showed that the most harmful rays were those in the wavelength range from 3100 Å to 4300 Å (i.e., from the blue-violet to the ultra-violet), although some radiation occurs even at longer wavelengths. Filters—sheets of yellow-orange colored acetate film—that absorbed the harmful wavelength range were obtained from the Eastman Kodak Corporation. Calculations showed that these sheets reduced the radiation damage by 90 percent and 98 percent respectively for incandescent lighting and filtered sunlight, while the viewing efficiency was reduced by only about 35 percent. Laminated glass with an interlayer of this material was produced for the project by the American Window Glass Company, and panes of this glass were positioned above the cases when they were finally placed in the shrine.

Having all the components ready, one final question remained to be answered. Could they all be assembled without damaging the documents—particularly from heat during the final critical soldering step? In June 1950, a trial sealing using a facsimile of the Declaration of Independence was carried out. Temperature measurements indicated that no damage would occur.[111] All indications were that the Charters of Freedom could be successfully encased.

During 1951, the five leaves of the Constitution and the single leaf of the Declaration of Independence were permanently sealed in their cases. The final steps were flushing with helium and final closure. Properly humidified helium was passed through the cases for several days, using fine copper inlet and outlet tubes specifically placed in the lead dam for this purpose. When the leak detectors showed that no air was left in the cases, "pinching off" the copper tubes and final sealing took place.

In August 1951, new, brighter lighting was installed at the shrine, using the same filters on the lamps as was used in the laminated glass filter in front of the document cases. And in September 1951, amid much ceremony, the Constitution and the Declaration were re-installed in the shrine at the Library of Congress. It seemed that the Charters had found a permanent home.

That was not to be the case. On April 30, 1952, the Congressional Joint Committee on the Library ordered the transfer of the Declaration and the Constitution from the Library to the National Archives.[112] Immediately, on May 9, 1952, the Archives asked the Bureau to encase the Bill of Rights.[113] The Bureau did so, and on December 15, 1952—Bill of Rights Day—all the Charters were transferred to the National Archives. The Charters had finally found a permanent home.

In 1988, records were found describing two leaks in the document cases.[114] When the encased documents were put on display at the Library, the cases of the Declaration and leaf no. 1 of the Constitution showed leaks. The Constitution case was repaired, but the evidence for the repair of the Declaration case is ambiguous. Finally, in July 1989, following tests by the Jet Propulsion Laboratory under contract to the National Archives, the status of the documents was reviewed by the Advisory Committee of the Archives. The documents appeared to be in the same condition as at the time of encasing.[115] There is at present no conclusive evidence that the Declaration case has a leak. Further, in the opinion of the assembled experts, a small leak would cause no problems, since in the present storage conditions a small admixture of oxygen would cause no discernible degradation. Filling the cases with helium was probably gilding the lily.

---

[111] This trial case is part of the NIST Museum collection and is displayed at the entrance to the NIST Library.

[112] *Congressional Record*, 82d Cong., 2d sess., 1 May 1952: D403.

[113] Letter from Robert H. Bahmer, Acting Archivist of the United States, to Allen V. Astin, May 9, 1952.

[114] Memorandum from Delmar W. McClellan, Acting Keeper of the Collection, to Dr. Frederick H. Wagman, Director, Administrative Department. Subject, "Shrine Documents, Status of." October 29, 1952.

[115] Conversation with Leslie E. Smith, chief, Polymers Division, and member of the advisory committee for the Archives, August 1, 1989.

Standards and Fundamental Constants

There are various quantities in nature that modern scientists consider to be fundamental constants. These quantities are believed to be the same for all observers, wherever they are in the universe, and appear not to change with time. Scientists believe that they have the same value now as they had at the origin of the universe. One example of a fundamental constant is the speed of light. Despite many attempts to demonstrate the opposite, this shows no temporal change. And it is a fundamental tenet of the theory of relativity that its value is the same for all observers, no matter what their relative motion. But most fundamental constants involve atomic and sub-atomic quantities. All properties of given atoms and their constituent parts are expected to be identical under the same conditions, wherever they are found. Thus the rest mass of a hydrogen atom, and that of its constituent proton and electron, are the same for all hydrogen atoms, and are believed to be the same now as they have ever been.[116] And this identity is not limited to mass. The magnetic moment and angular momentum of all protons are identical, and the same holds true for electrons. All atomic and sub-atomic particles are identical replicas of one another.

It is hardly surprising, therefore, that the measurement of fundamental constants should be of interest to standards laboratories. If a fundamental constant can be measured more accurately than can a standard, then there exists the possibility that the constant can be used to replace the standard.[117] Equally important, it can be used to confirm the value of a standard.

Specifically, the value of the international ampere, which was made the legal unit of electric current in 1894, was defined as "the unvarying current, which, when passed through a solution of nitrate of silver in water in accordance with standard specifications, deposits silver at the rate of one thousand one hundred and eighteen millionths of a gram [1118 micrograms] per second."[118] The international coulomb—the unit of electric charge—was defined as the "quantity of electricity transferred by a current of 1 international ampere in 1 second." These definitions are very closely related to—in fact, they derive from—the value of the fundamental constant known as the Faraday. This is defined as the charge carried by 1 gram mole of singly charged ions, or what is equivalent, the charge per 1 gram of singly charged ions of unit atomic weight. Indeed, from the legal definition of the ampere and coulomb, and the atomic weight of silver, one easily calculates the value of the Faraday as 95 621.9 coulomb/mole. Thus, a determination of the Faraday is equivalent to another determination of the standard for current or charge.

---

[116] Some modern theories (so-called "Grand Unified Theories") predict that the proton may decay radioactively, but with a very long lifetime. Experiments have shown that this lifetime is greater than 10^{32} years—about 10 billion trillion times the age of the universe.

[117] In 1983 the speed of light was used to replace the standard of length. The unit of length is no longer the distance between two scratches on a platinum-iridium bar, nor the length of 1 650 763.73 wavelengths of the orange red line of Kr^{86} (itself a sort of fundamental constant). It is the distance light travels in 1/299 792 458 of a second in a vacuum.

[118] The 1894 law was superseded in 1950 when the absolute rather than the international ampere became the legal unit.

Prior to 1950, the determination of the Faraday constant had been carried out by electrochemical means.[119] In 1949, John A. Hipple, Helmut Sommer, and Harold A. Thomas of the Bureau's Atomic Physics Division devised a method for determining the Faraday by purely physical means.[120] The instrument they devised was modelled after a cyclotron, in which charged particles move in circular orbits whose plane is normal to an applied magnetic field. The frequency of their rotation is called the cyclotron frequency, and is given by the product of the charge to mass ratio of the particles and the magnetic field strength. Periodically they are given an accelerating pulse which increases their kinetic energy, hence the radius of their orbit. Hipple, Sommer, and Thomas did the same thing, but the acceleration was not by pulses but by a sinusoidal electric field. When the frequency of the electric field was the same as the cyclotron frequency, i.e., the two were in resonance, the ions could be made to impinge upon a collector. In this way, the cyclotron frequency could be measured, and measured precisely, for the resonance could be made very sharp. Then, knowing the strength of the magnetic field, the charge to mass ratio of the ions could be determined very accurately.[121] This number, multiplied by the isotopic mass of the ion, yields the Faraday. Since the instrument measured the cyclotron frequency, it was called the "omegatron" for the Greek letter used to denote angular frequency. It was a small device, about 5 cm × 2.5 cm × 4 cm.

After two years of experimentation, the group published its final result. The obtained value was 96 520 ± 3 coulombs/mole,[122] which agreed well with the value of 96 519.3 ± 2.6 coulombs/mole reported by D. Norman Craig of the Electricity Division and James I. Hoffman of the Chemistry Division for the electrochemical oxidation of sodium oxalate.[123] Both results were slightly, but not significantly, different from the definitive results of Craig, et al., of 96 516.5 ± 2.4 coulombs/mole in 1960. It was reassuring to be able to determine the value of the Faraday, which is basic to the definition of the ampere, without having to carry out electrochemical experiments.

---

[119] D. N. Craig, J. I. Hoffman, C. A. Law, and W. J. Hamer, "Determination of the Value of the Faraday With a Silver-Perchloric Acid Coulometer," *Journal of Research of the National Bureau of Standards* 64A (1960): 381-402. This paper illustrates the exquisite care that is taken in work relating standards and fundamental constants. See also D. N. Craig and J. I. Hoffman, "A New Method for Determining the Value of the Faraday," *Physical Review* 80 (1950): 487.

[120] J. A. Hipple, H. Sommer, and H. A. Thomas, "A Precise Method of Determining the Faraday by Magnetic Resonance," *Physical Review* 76 (1949): 1877.

[121] The magnetic field was determined by the proton precession frequency and known gyromagnetic ratio. Thus the only measurements made in the experiment were two frequencies.

[122] H. Sommer, H. A. Thomas, and J. A. Hipple, "The Measurement of *e/M* by Cyclotron Resonance," *Physical Review* 82 (1951): 697-702.

[123] D. N. Craig and J. I. Hoffman, "A New Method for Determining the Value of the Faraday," *Physical Review* 80 (1950): 487. The electrochemical determination of the Faraday was conducted in the Electricity Division because the national standard for the volt was maintained by the Electrochemistry Section in the Electricity Division. The direct reference to the national volt was essential to the determination of a physical constant, in this case, the Faraday.

The Bureau and X Rays: High Energies Come to the Bureau

This is the story of how the Bureau obtained its first high-energy accelerators and brought in nuclear and theoretical physics research. They came via the study of x-ray measurements.

In its appropriation request for Fiscal Year 1947, the Bureau asked for—and received—$250 000 for the purchase of a betatron. Invented in 1940 by Donald W. Kerst at the University of Illinois and the General Electric Company, the betatron permitted the production of a very-high-energy electron beam—50 million volts for the instrument the Bureau requested—which could then be used for the production of very-high-energy x rays. And in its request for Fiscal Year 1948, the Bureau asked for, and again received, a further $565 000 for the completion of the building that was to house the betatron, and for the purchase of another betatron, this one for energies up to 100 MeV.[124]

The Bureau's justification for these large requests (the appropriated funds for the Bureau in 1947 totalled $1.12 million exclusive of the betatron request) consisted of three parts. The first concerned the use of x rays for diagnostic purposes and for radiation therapy. The energy, hence the penetrating power of x rays used for therapy, had increased enormously in the postwar period, and standards and measurement methods were essential in this high-energy region so that radiologists could accurately deliver an exact dosage to the organ being treated. The second part concerned the industrial use of x rays. Highly penetrating x rays were being used more and more extensively for radiography. Rays from these new high-energy machines could penetrate 30.48 cm (12 in) thick steel castings to examine them for minute cracks and other flaws. And in both of these areas, the efficiency and adequacy of shielding materials had to be known in order to protect the radiologists and technicians working with this new, high-energy radiation. The final justification was for basic research. In the words of the justification:

> The equipment proposed presents a tool for research in a field that is relatively untouched. A very limited amount of work has been done by OSRD in the 10 to 20-million-volt range, the exact nature and volume of which is still secret. By this means it is possible to study nuclear transformations, the production of artificial radioactivity and radiation processes hitherto known only through a study of cosmic radiation.[125]

---

[124] The abbreviation MeV stands for "Million electron Volts." This is the energy acquired by an electron falling through a potential difference of 1 million volts.

[125] House Committee on Appropriations, Subcommittee of the Committee on Appropriations, *Department of Commerce Appropriation Bill for 1947: Hearings before the Subcommittee of the Committee on Appropriations*, 79th Cong., 2d sess., National Bureau of Standards, 18 February 1946: 960.

There is no question that the principal justification for the Bureau's request was the medical use of x rays. Indeed, Lauriston S. Taylor, then chief of the X-Ray Section, tells the following anecdote about appearing before the Senate Appropriations Committee. He was prepared to go through a full presentation, and part way through it he passed to the chairman of the committee a copy of *Radiology* which contained colored photographs of people who had been seriously "burned" by x rays. The chairman glanced at the illustrations, then thumbed through the journal, finally coming upon a radiograph of a somewhat gnarled hand. He held up the journal and asked, "Doctor, is this a case of arthritis?" Even after looking at the title of the illustrations, Taylor was not sure but said that he expected arthritis might look like that. The chairman continued, "My sister has the worst kind of arthritis you ever saw, she is in such misery. . . . " Thereupon the chairman passed the journal to the other members of the committee, and it seemed that every one of the members had some relative suffering from arthritis. Taylor was quite nervous by this time, for his allotted time for making his presentation was rapidly disappearing. Suddenly the chairman turned to Taylor and said, "Doctor, I think this is one of the finest programs we have listened to in many years. I am sure that our committee will endorse this and we will give you all the funds you asked for." And they did—for the building and equipment. Thus did the Bureau enter the age of high-energy machines.

The Bureau had been in x rays for a long time, having obtained its first x-ray generator in 1917, twenty-two years after Roentgen's discovery of x rays. By that time x rays had grown into a scientific discipline and an industry. With the development of the Coolidge tube in 1913, the production of x rays had become routine and reliable. And the dangers of exposure to ionizing radiation, either from radium or x rays, were recognized before World War I. But the war tragically dramatized these dangers, for the Coolidge tube made x rays common during the War and "literally hundreds of doctors and technicians were severely injured or died as a result of their exposures."[126] It was clear that better ways of measuring and controlling the intensity of radiation were imperative.

The methods used for the measurement of radiation in 1920 were largely empirical, based upon the ionization of air, the darkening of strips of photographic film, color pastilles, selenium cells, and chemical coloration. A measurement method based on more fundamental concepts was needed. This and the concern for the protection of people from radiation led to the convening of the first International Congress of Radiology in London in 1925. Despite the fact that in the early 1920s the Bureau had been under considerable public pressure to begin an x-ray program, it sent no representative to this congress. However, when the Radiological Society of North America formed a Standardization Committee in 1925, Franklin L. Hunt, of the Atomic Physics, Radium, and X-Ray Section, and Noah Ernest Dorsey became

---

[126] L. S. Taylor, *X-Ray Measurements and Protection, 1913-1964: The Role of the National Bureau of Standards and the National Radiological Organizations*, Natl. Bur. Stand. (U.S.) Special Publication 625; December 1981: 3.

members of that committee.[127] Meeting in 1926, the committee concluded that the principal problems were the establishment of a standard x-ray unit, the variation in x-ray dosage as measured in this unit for different qualities of radiant energy, the devising of a system to transfer this unit "from a standardizing center . . . (preferably the United States Bureau of Standards) to different . . . institutions or private laboratories,"[128] and the further study of the proposed physical x-ray unit in relation to its biological effect. Promises of cooperation between the Bureau and the Society were made.

But it was not until 1927, with the arrival of Lauriston S. Taylor, a young physicist from Cornell, that the Bureau's program in x-ray measurements began.[129] The equipment he found was an old World War I diagnostic machine and totally unsuited for the task at hand. This was hauled away and replaced with other equipment designed and built by hand, and the program began to flourish under Taylor's vigorous leadership.

The principal task was the development of a national standard for the measurement of x-radiation. In 1928, the Second International Congress of Radiology, meeting in Stockholm, had adopted the definition of the roentgen as the unit of measurement of ionizing radiation as "the quantity of X-radiation which . . . produced in one cubic centimeter of atmospheric air at 0 °C and 76 cm mercury pressure, such a degree of conductivity that one electrostatic unit of charge is measured at saturation current."[130] A unit had been defined. The problem was now the realization of that unit.

By 1929 Taylor had developed a free-air ionization chamber which would realize the unit and which could eventually become the national standard for the measurement of ionizing radiation.[131] By 1932 he had intercompared the American standard with those of England, Germany, and France. In order to do this he had to develop a portable free-air ionization chamber and calibrate it against the primary standard, which was far too heavy to transport. This became known as the "guarded-field ionization chamber." He then travelled to Europe and made measurements at the foreign standards laboratories, which had developed new standards at about the same time

---

[127] In 1913, N. E. Dorsey, then of the Electricity Division, began the Bureau's activities in the standardization of radium preparations. Like others in this field, he received burns to his fingers and hands from the handling of these preparations. He resigned from the Bureau in 1920, becoming an independent consultant, and his hands healed. He re-joined the Bureau in 1928 and retired in 1943. (MFP, 147.)

[128] Taylor, *X-Ray Measurements and Protection, 1913-1964*: 10.

[129] It was in some ways an inauspicious beginning. Taylor had come to the Bureau to work with Hunt, only to find that Hunt would shortly leave to take a position at Western Electric. In addition, Paul D. Foote, chief of the section, was also in the process of leaving. To cap matters, Taylor had expected to work on x-ray spectroscopy, but instead was being asked to work on x-ray dosimetry. Somewhat embarrassed, Taylor went to see Clarence A. Skinner, the division chief, whom he told he would try it for a year. He stayed for thirty-seven.

[130] Taylor, *X-Ray Measurements and Protection, 1913-1964*: 281, 288. Note that this definition combines a definition of a quantity and the method of measurement. The definition was changed in 1937 to read, "The roentgen shall be the quantity of X- or gamma-radiation such that the associated corpuscular emission per 0.001293 grams of air produces, in air, ions carrying 1 e.s.u. of quantity of electricity of either sign."

[131] L. S. Taylor, "The Precise Measurement of X-Ray Dosage," *Bureau of Standards Journal of Research* 2 (1929): 771-785.

as the Bureau. The final agreement among the U.S., British, and German standards was ± 0.5 percent. (The French used a different unit.) The United States finally had a national standard for x rays, with a known relationship to comparable standards in other countries.

By the beginning of World War II, the Bureau program was flourishing. During the 1930s it had continued its studies of x-ray protection, along with research on measurement methods, and issued a number of handbooks explaining various aspects of radiation protection and measurements. These supplemented the 1929 Circular 374, *X-Ray and Radium Protection: Recommendations of the International Congress of Radiology,* and several research papers. It had made notable contributions to equipment for the generation of x rays, and its x-ray production capabilities had regularly expanded, with a 600 kilovolt x-ray generator built in 1934, and a 1.4 million-volt generator in 1940. It had done innumerable calibrations, and was recognized throughout the world as a leader in x-ray measurements.

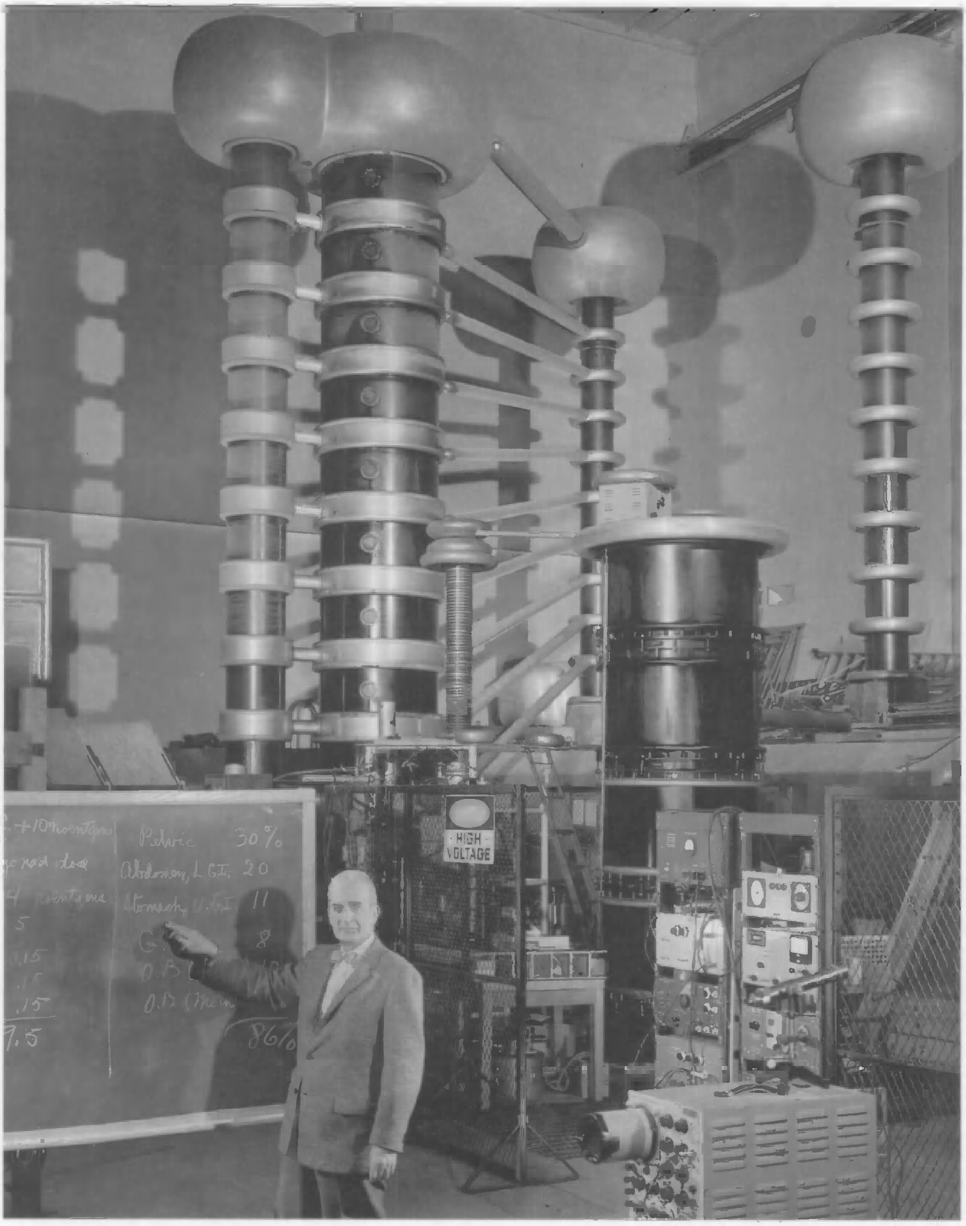
The war caused a hiatus in this work. Many of the staff working on x rays and radioactivity went into war work. Taylor himself spent the war years working on the proximity fuze for bombs and rockets until 1943, and then organized operations research sections for the Eighth Fighter Command and the Ninth Air Force in Europe. Returning to the Bureau soon after the end of the war, he and the director, Lyman J. Briggs, made plans to expand the radiation programs, and to obtain the 50 MeV and 100 MeV betatrons later requested from the Congress in 1947 and 1948. Briggs retired in October 1945 and was replaced by Edward Uhler Condon, himself a theoretical physicist. Condon strongly supported the program, and it flourished. By 1950, the organizational unit that contained the work was called the Radiation Physics Laboratory and consisted of six sections. The work was described in six categories: Protection and Shielding Research (experimental and theoretical); Radiation Protection Recommendations and Codes; X-Ray, Gamma Ray and Radioisotope Standards; Measurements and Instruments; Theoretical Studies; General Atomic and Nuclear Physics Research; and X-Ray Equipment Research and Development.[132]

In 1950, the first of the betatrons—the one for 50 MeV—was delivered and installed in its own new and separate building. As described earlier, its main justification was in the medical use of high-energy x rays, and a great deal of work—both experimental and theoretical—on x-ray protection by various materials was indeed carried out. "Bread and butter work," Taylor called it.[133] But more and more the machine was used for research in nuclear physics, and when it was learned how to extract the electron beam from the 50 MeV betatron so that it could be used directly for nuclear studies, the machine was used exclusively for nuclear physics. A great deal of distinguished work on photonuclear reactions, largely supported by the AEC, was carried out. When the second betatron—which in the interim had been converted to a 180 MeV synchrotron—was installed, the Bureau had a full-fledged, high-energy laboratory and a program of research in nuclear physics. In due course, the Bureau would acquire a linear accelerator and a nuclear reactor. The little 50 MeV betatron had led the way.

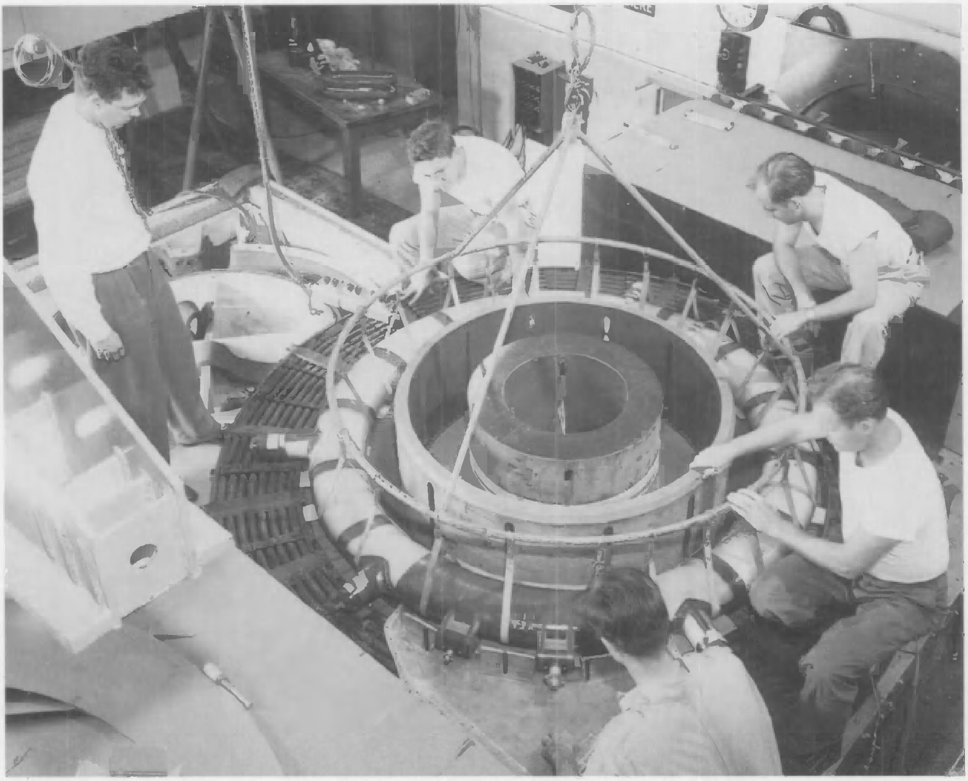
---

[132] L. S. Taylor, *X-Ray Measurements and Protection, 1913-1964*: 307.

[133] Ibid., 312.



Lauriston S. Taylor, chief of the Atomic and Radiation Physics Division at NBS, pointed to x-ray protection standards on a chalk board with the 1.4 million volt x-ray generator and the neutron generator (black cylinder on right) behind him (1959).



NBS staff installed the "donut" of the Bureau's new 180 million-electron-volt synchrotron. The synchrotron facilitated research in the physics of radiations and electrons in the energy range from 5 MeV to 180 MeV. The donut was an evacuated glass tube within which electrons were made to travel under the influence of magnetic and electric fields. The combined effects of the fields increased the electron energy by making the electron travel in a circular orbit at tremendous speeds. The heavy wires concentric to the "donut" augmented the magnetic field set up by the poles of a large magnet. The electric field was supplied by a high frequency generator and was injected into the electron path through the connectors in the wall of the tube.

In 1946, an event of great significance to the whole Bureau occurred. Like the rest of the Bureau, the work of the x-ray section had been purely experimental, with the sole exception of a young theoretical physicist from Cornell, Warren W. Nicholas, who was hired in 1928 but stayed only four years and was partly an experimentalist. But in 1946, a pure theoretical physicist, Ugo Fano, a former student of Enrico Fermi, was hired to work on x-ray problems, and did so brilliantly—on x rays and many other problems. As near as can be determined, this was the first time in its history that the Bureau had hired a pure theorist. He was not to be the last. For the Bureau, a new approach to the conduct of science had begun.

Ugo Fano, internationally known for his theoretical work in various branches of physics and in related sciences, had a profound influence on the field of atomic physics through his personal scientific creativity and his stimulation of many others at the Bureau. An important part of his work at NBS was consultation with experimental scientists on the theoretical aspects of their work. Fano showed a unique ability to explain the fundamental concepts of classical and modern physics in terms and analogies that scientists working in other fields such as biology and medicine could readily understand.

LIFE AT THE BUREAU

In an interview held on September 22, 1981, Allen V. Astin, who in August 1951 had succeeded Condon as director, was asked about life in the prewar Bureau.[134]

WEINSTEIN: Sir, can you tell us a little about what the working environment was like under Dr. Briggs?

ASTIN: Well, I thought it was excellent. It was the environment that made me satisfied to stay where I was. . . .

HUNTOON: If you were asked to characterize Briggs' environment, how would you characterize it. . . .

ASTIN: I'd say it was friendly, peaceful, cooperative.

HUNTOON: And free?

ASTIN: And free, very free.

HUNTOON: Freedom to do what you want to do?

ASTIN: Freedom to do what essentially you want to do.

HUNTOON: How much accounting?

ASTIN: Very little, very little accounting. It was an ideal atmosphere, I think, and I enjoyed it, and I'm sure my associates did as well.

---

[134] Interview with Allen V. Astin, September 22, 1981: 7-8. (NIST Oral History File)

In a similar vein, Jacob Rabinow, prolific and scholarly inventor, talks about his coming to work at the Bureau in 1938: "It was . . . a job where people behaved as ladies and gentlemen. . . . I never worked in a place as genteel and polite as the Bureau of Standards in Washington."[135]

While Astin's and Rabinow's comments pertain to the prewar Bureau, all indications are that this friendliness, this gentility and cooperativeness, this freedom in choice of work, characterized the postwar Bureau as well. These characteristics were part of the nature and traditions of the institution. Indeed, freedom in the postwar Bureau had returned to the freedom of access of the prewar years. Traditionally, the Bureau grounds are open to all comers during normal working hours, with no need to stop at a guard gate or other impediment. In fact, it was not until 1942 that the Bureau grounds were fenced. During the war, however, because of all the military work being carried out, entrance at all times was controlled by uniformed guards. Even Van Ness Street, the public thoroughfare through the Bureau grounds, was closed off.[136] Immediately upon the war's end, access returned to its traditional freedom, except for some restricted areas where classified work was being carried out.

Indeed, in one characteristic, freedom had actually increased. In the prewar Bureau, and during the war years, hours of work were rigidly controlled. One had to be at work by 8:30 a.m. or lose a half-hour of annual leave.[137] Now, under Condon, who believed that creativity could not be channeled into a strict regime and permitted scientific staff the freedom to set their own work schedule (provided that the stipulated number of hours were worked in a week), even working hours were set more freely.

More important than these rather mundane freedoms was the latitude in planning what work was to be done. At the upper levels of management, this question was decided rather simply: it was decided by the director in individual consultation with his division chiefs after consultation with associate directors, whose number varied from time to time, but was three in 1950. Their function was a mixture of staff and line, consulting with both the director and the division chiefs on program definition. The director set policy, and in individual consultation with the division chiefs set the program for individual divisions. In this program definition the division chiefs were given considerable latitude. They were, after all, generally Bureau people of long experience who knew well the mission, goals, and responsibilities of the institution. They were also highly competent technically, often world-renowned experts in their fields. They could be trusted to make sound decisions about what lines of work would carry out the institutional responsibilities, goals, and policies of the Bureau. And they had complete freedom to accept other-agency projects.

---

[135] Recollections of Jacob Rabinow, taped at his home on August 12, 1982. (NIST Oral History File)

[136] MFP, 372.

[137] Ibid., 4.

Van Ness Street, N.W., a public thoroughfare through the Bureau grounds, was closed in 1942 by order of the secretary of war. (Copyright Washington Post; reprinted by permission of District of Columbia Public Library.)

At the section chief and scientist level, choice was quite a different matter. While the Bureau scientist was well aware of institutional responsibilities,[138] his goals were generally to follow scientific opportunities, and the section chief had much the same goals. The problem here was to follow the scientific opportunity while at the same time to carry out the policies of the director and the responsibilities of the institution. The amount of freedom section chiefs and individual scientists were given in resolving this problem varied with their immediate superiors and their own capabilities, but the tradition of the Bureau was to give as much as possible. Typically, an established scientist was encouraged to work on problems of his or her own choosing half the time, with the other half dedicated to problems specifically identified by the institution, although even in this, he or she was given wide latitude in how to accomplish the specified ends. Cases are known, however, of section chiefs who closely guided all the activities of the scientists beneath them; and other cases where scientists were given complete freedom, knowing, however, that their work would be in a specified field of importance to the Bureau and its mission.

The counterpart of freedom is cooperativeness. Cooperation among scientists was encouraged, and it was a tradition of the institution that advice and consultation would

---

[138] To a new employee, one of the most striking aspects of informal gatherings of Bureau scientists is their degree of introspection. Almost endless discussions take place on the Bureau's mission and policies, and on what it should do with respect to various national and technical problems.

be freely given. Part of this ease of cooperation arose from the manner of accounting for costs. In essence, until 1951, there was no way of accounting for the costs of individual work projects. A division had funds to work in a given area, and all the work except other-agency work was charged to that area. Thus, without the necessity for detailed accounting, cooperation was easy. In 1951, largely in response to Congressional criticism about the Bureau's lax administrative methods, a project accounting system was put into operation, and individual scientists had to account for their time on various projects.[139] In 1951 there were 630 unclassified projects.[140] This permitted much tighter control of scientist's time allocation and, depending on the division and section chiefs, impediments to cooperation could be raised. When bringing up the possibility of cooperation with another scientist, there was always the possibility of the dreaded question, "What project do I charge it to?" This did not halt cooperative research, for the tradition was too strong, but there was an impediment.

Unimpaired freedom of choice does not necessarily lead to good, creative work. Indeed, it can lead to continuation of old work in which the investigator feels comfortable and for which there is a ready, well-known audience, however small. Something of this kind had happened in the prewar Bureau. Robert D. Huntoon recounts an experience when he was a graduate student in physics at the University of Iowa.[141] His professor was Alexander Ellett, who was later to be in charge of the nonrotating projectiles proximity-fuze program for the Office of Scientific Research and Development (OSRD), with an office at the Bureau. Huntoon had visited the Bureau during a meeting of the American Physical Society in the late 1930s, at which time the Bureau was working on instrumentation for the detection of cosmic rays. He talked to Ellett about coming to work at the Bureau: "You know, I think I'd like to get in there and work on some of this cosmic ray stuff that I hear Diamond and Curtiss talking about." To which Ellett replied, "You don't want to go to that goddamn Bureau of Standards. All they do is sit around. They're a bunch of old fogies, dusting off the standards and trying to get another decimal point, and it's the most dreary place you could imagine working. How could you ever think about putting your career there?" And Huntoon continues, "[T]his was the university view of the Bureau of Standards. I've run into it at other places, in the prewar days, that it was a stultified, inactive, non-creative kind of a place. . . . So then I get into the old Bureau . . . I find this fantastic stuff that these old timers had done, [Edward] Rosa's work, and [Chester] Snow and the gravity guy, [Paul] Heyl. . . . These were very dedicated, capable guys of international reputations about whom nobody outside the favored circle ever seems to hear. . . . " Recollections similar to Huntoon's were expressed by Irl Schoonover.[142]

---

[139] House Committee on Appropriations, Subcommittee of the Committee on Appropriations, *Department of Commerce Appropriations for 1951: Hearings before the Subcommittee of the Committee on Appropriations*. 81st Cong., 2d sess., National Bureau of Standards, 23 February 1950: 2186.

[140] Annual Report, 1952: 1.

[141] Interview with Robert D. Huntoon, October 27, 1980: 21. (NIST Oral History File)

[142] Interview with Irl C. Schoonover, 3 June 1981. (NIST Oral History File). See also John Newhouse, *War and Peace in the Nuclear Age* (New York: Alfred A. Knopf, 1989): 22, for related comments by I. I. Rabi and John Manley.

When Condon, himself a world-renowned theoretical physicist, became director of the Bureau in November 1945, he recognized that the traditional peacetime functions of the Bureau, which had languished during the war, had to be revitalized. Moreover, the expansion of science, and the anticipated flourishing of new technology, required that the Bureau's research programs be modernized and strengthened. And the work of the Bureau had to be communicated to the scientific community, partly for increased effectiveness, partly to overcome the prewar image and partly to make the scientific staff broaden its outlook. Above all else, he wanted the Bureau to be an aggressive, vibrant institution with a wide audience, not a passive, inward-looking one, writing papers of interest only to a few narrow specialists. This new look, and the natural extrovert Condon himself, were a shock to many of the quiet, genteel, old-line staff.[143] He brought in Hugh Odishaw, his assistant at Westinghouse, to begin an aggressive program of communication and dissemination of the Bureau's scientific accomplishments. Largely a program of dissemination of the Bureau's scientific work, this activity was looked down upon by many of the old-line staff who thought of it as public relations. But, most important, he changed the direction and style of the Bureau by hiring bright, young, recently trained, modern scientists, with the aim of bringing the institution's scientific research into line with modern physics. It was relatively easy for the Bureau to hire such people. Condon himself, with his scientific reputation and vigor, was the magnet that attracted them. In line with his own scientific field, he began a program in applied mathematics, organized a division which in 1950 became the Atomic and Radiation Physics Division, put some of his best scientific people in its management, and peopled it with this new talent. This had created considerable resentment among some of the old-line staff, although by 1950 this had calmed down to a considerable extent. But the Bureau, while still a free, friendly, and cooperative place, had a new look, and the modernization of the research program was to continue for about ten years after Condon's departure.

In the immediate postwar years the Bureau was not a homogeneous institution; it consisted of several cultures. The principal division was into those persons who worked on military and atomic energy problems and were supported on funds transferred from the armed forces and the Atomic Energy Commission, and the "Old Bureau" persons who worked on the Bureau's unique measurement mission and were supported by directly appropriated funds. The "Old Bureau" was the portion Condon set about to revitalize. These groups not only had different masters; they were geographically separated. The "military" were located in the guarded, fence-enclosed Harry Diamond Ordnance Laboratory on the northwest 12.5 acres (5 hectares) of the

---

[143] Condon was accustomed to walking around the Bureau, dropping in unannounced on scientists working in their laboratories, and engaging them in a conversation about their work. His scientific powers were so great that he usually left them with new insight into what they were doing—even in fields that were not his specialty. There is, however, a story (probably apocryphal, but illustrative) about his dropping in on the Bureau glassblower, a notably crusty individual, as were many of the rest of the Bureau's craftsmen. The glassblower was constructing a complicated piece of apparatus out of fused quartz, a difficult and demanding task, requiring a hydrogen-oxygen flame. As Condon entered the shop, he stepped on one of gas supply lines, and the glass-blowing torch went out with a loud pop. Whereupon the craftsman turned around, looked at Condon, and said, "You clumsy oaf, can't you watch where you put your feet?" Condon walked quietly out. (Story told by John D. Hoffman.)

Dedication of the Harry Diamond Ordnance Laboratory in 1949.

Bureau site, with other contingents at the Institute for Numerical Analysis on the UCLA campus, and the guided-missile research laboratory at Corona, California. Somewhere between these two in function and philosophy was the Central Radio Propagation Laboratory (CRPL). Its Ionospheric Research Laboratory and Systems Research Laboratory—which carried out the "radio weather" prediction service—were closely aligned with military problems, while its Measurements Standards Laboratory was concerned with the Bureau's traditional measurement and standards function, and hence aligned with the "Old Bureau." A large portion of the work of the CRPL was carried out in field stations, and hence away from the main campus. And in the "Old Bureau" itself, there was a split between those who were doing scientific research into measurement problems (the "scientists"), and those concerned with the testing of Government purchases and the development of commodity standards (the "testers").

The aims and views of the Bureau's role and mission naturally differed from part to part. The "scientists" of the "Old Bureau" felt their role was to carry scientific research into new phenomena and areas which could be the source of new measurement methods and standards, while the "testers" were concerned with more empirical test methods. The military people were concerned with carrying out the programs assigned to them by their supporters. The traditions of cooperation and freedom of work choice were not greatly different in the several parts, which was hardly surprising since all the management leaders came from the "Old Bureau." But the choice of work in the military part was more in the nature of devising ways to solve immediate technical problems than in formulating problems; they were engineers rather than scientists, doers rather than thinkers. And the two parts—except for their management—by and large kept to themselves. This was not surprising considering the strictures of geography and the requirements of secrecy.

While work choice had considerable latitude, cooperation was encouraged, and personal relations were courteous when not friendly. A number of amenities were missing from life at the Bureau. Because of the enormous growth that had occurred during the war, and even the more forceful growth to occur during the Korean War, space was at a premium. Even allowing for the propensity of scientists to act collectively like a gas and occupy all available volume, the Bureau laboratories were crowded and administrative games to obtain more space were usually in progress. Janitorial services were not all that could be desired. Offices were allowed only for section chiefs and division chiefs; scientists and their assistants (if the latter were lucky) had desks in the laboratories. But what inconvenience this may have caused during periods of reflection, analysis, and writing was mitigated by the close—if forced—interaction with colleagues.

Air-conditioning was not permitted for personal comfort, but was allowed if equipment requirements demanded it. Consequently there were a number of ingenious justifications for air conditioning because equipment suddenly became sensitive to the hot, humid Washington summers. But, like the rest of the Federal civil service force in Washington, workers were excused on particularly hot and humid days.

For those below the level of section chief, luncheon dining was a problem. On the Bureau grounds there were only two places where lunches could be obtained—a cafeteria that seated 150 persons in the Industrial Building where hot lunches could be purchased, and "the Hut," a temporary, sheet-metal canteen near the West Building which had no seating facilities. Here only sandwiches and snacks were available, but coffee could be obtained during the day. A charitable description of the food at the cafeteria was that it sustained life. These facilities were soundly criticized by the Congress.[144]

But if the luncheon facilities on the Bureau grounds were inadequate, Connecticut Avenue more than compensated. Here a number of restaurants in a whole range of prices were available, and a significant number of the Bureau staff were regular customers. But the time allotted for lunch was 30 minutes, and it was impossible to have lunch on the avenue in this length of time. Again this brought criticism from the House Appropriations Committee.[145] To a Bureau management that permitted scientists to set their own working hours, this cannot have been a serious concern, and doubtless was also not a serious concern to the Committee. But it was a useful point for criticism.

---

[144] Appropriations Hearings for 1951: 2226.

[145] Ibid., 2227.

For division chiefs, section chiefs, and a few senior scientists, luncheon problems were mitigated. This group had formed a dining club, called the Senior Lunch Club, that met for lunch in a dining room on the fourth floor of the South Building. The South Building was the first Bureau building erected and, along with laboratories, housed the Bureau administrative offices. Membership in the club was by invitation, and a modest monthly fee covered the cost of meals. The meal was prepared by a caterer under contract to the club and served boarding-house style at a number of tables, each seating eight persons. It was an excellent place for these Bureau leaders to exchange ideas and discuss technical and administrative matters. One of the rules of the club was that seating was at random but this rule was not strictly obeyed. A number of members occupied the same places daily, and two of the tables were always occupied by the same persons. If a new, uninitiated member inadvertently occupied one of these "reserved" places, he or she would not be asked to move. This would be against the club rules, and moreover would be discourteous. The offended member, often in a surly mood, went to sit somewhere else, and cases are recalled when such a member stomped out of the club in a huff. A new initiate quickly learned the rules, and it was a good place to learn the power structure of the Bureau and what was going on. The food was not always in overabundance, leading to occasional caustically humorous comments about members with large appetites, but it was nourishing and often tasty.

The NBS Senior Lunch Club provided an opportunity for NBS senior staff to meet and exchange ideas in an informal setting.

Despite the lack of these few amenities, the Bureau of 1950 was a good place to work. A scientist had considerable opportunity to follow his or her own ideas, there were expert colleagues with whom one could consult and possibly cooperate with on technical problems, and the director was a famous scientist who was revitalizing the organization. It was a good place to interact with the scientific community. The American Physical Society always held sessions in the auditorium in the East Building during its spring meeting when Washington was at its flowering best. There was a constant stream of foreign and domestic visitors, many of whom gave colloquia at division meetings, and every Friday morning there was a colloquium for the whole Bureau staff. This was sometimes presented by staff members who had done particularly meritorious work, and sometimes by invited distinguished visitors. And arrangements could be made with one of the local university professors for younger staff members to use their research work at the Bureau for a Ph.D. or master's thesis. It was an attractive place for the recent, well-trained graduate.

But there were some problems. The loyalty investigations begun in 1947 had caused some members of the Bureau staff to resign, and others had passed some trying days of investigation. Some prospective employees, possibly because of previous injudicious or ideological associations, or possibly because of the rigors of investigation, were dissuaded from applying for positions. The director of the Bureau, in these early days of McCarthyism, was himself under a loyalty cloud. And, unknown to the prospective employee and even to most of the Bureau staff, a problem, concerning of all things a battery additive, was beginning to fester. This would cause the Bureau some of its most trying days.[146]

---

[146] Along with sources identified in subsequent footnotes, much of this material comes from interviews with Churchill Eisenhart, Everett G. Fuller, Karl G. Kessler, John A. Simpson, and W. Reeves Tilley.