CHAPTER THREE

DIVESTITURE AND REAFFIRMATION, 1950-1957

The early years of the decade of the fifties were turbulent years for the Bureau. By the time the decade was one-third over, one director, under attack from the House Un-American Activities Committee and distressed by the rising wave of McCarthyism, had resigned; the trauma of the battery additive incident had occurred with the firing and eventual re-hiring of the following director; the Korean War, which had increased the Bureau's size to the largest in its history, almost caused it to lose its identity as the Nation's measurement standards laboratory; and the Bureau had to rebuild its basic program while its base appropriation was declining disastrously. It was, at one and the same time, rich in money for work for other agencies, while poor in funds to carry out the work it was legally mandated to do at a time when the explosive postwar growth of science called upon it to do more. Yet it was able to surmount these problems, and by the last quarter of the decade it was on the road to some of the happiest and most productive years in its history. In many ways its history in this decade mirrored the history of the Nation.

A Placid Nation

The general view of the decade of the 1950s is of a time of public obsession with suburban homes, barbecues in the back yard, tail-finned automotive behemoths, TV, rock music, and babies. In the words of columnist Robert J. Samuelson:

> You know the rap against the fifties. Nothing happened. Ike golfed, Elvis sang, cars sprouted tail fins, and students belonged to the "silent generation." Dullsville.[1]

But the view observed depends on the viewpoint. Historian Elaine Tyler May writes:

> Diplomatic historians paint one picture of a world torn by strife and a stand-off between two superpowers who seemed to hold the fate of the globe in their hands. Sociologists and demographers provide a different picture of a private world of affluence, suburban sprawl, and the baby boom.[2]

Whatever the viewpoint, the beginning of the decade was anything but placid. On Saturday, June 24, 1950, North Korea invaded South Korea in force and immediately began to push back the South Korean forces. Events unfolded at breakneck speed.

[1] Robert J. Samuelson, "The Deceptive Decade," *Washington Post*, February 28, 1990.

[2] Elaine Tyler May, *Homeward Bound: American Families in the Cold War Era* (New York: Basic Books, 1988): 10.

Five tons of supplies for an isolated U.S. Air Force radio station in Korea drifted toward the target after being dropped by a C-119 Flying Boxcar of the 315th Air Division. In FY 1952, during the Korean War, 80 percent of the Bureau's budget came from the military. (Photo courtesy of U.S. Air Force)

On Sunday the United Nations passed a resolution accusing North Korea of armed invasion and called for a cease-fire. On Tuesday President Harry S. Truman announced that American arms would be used to uphold the UN resolution and defend South Korea, and he received a supportive response from the Government and the Nation. Finally, the Security Council, at a meeting inexplicably not attended by the Soviet Union, passed a resolution proposed by the United States that all members provide assistance to South Korea in repelling the armed attack. What could have been a war between the United States and North Korea became instead a UN action.

But this did not reverse the course of the battle. The news was bad. The South Korean forces were being pushed steadily back and the U.S. presence escalated rapidly. General Douglas MacArthur was first authorized by Washington to use U.S. air power in support of the South Korean forces, and U.S. ships to supply them. And on June 30, just six days after the North Korean attack, MacArthur was authorized to commit U.S. ground forces.

It was a close call. The North Koreans overran South Korea until finally stopped at a defensive perimeter around the port of Pusan on the south coast. Then followed the well-known, tactically brilliant, U.S. landing at Inchon, completely outflanking the North Korean forces and allowing a break-out from the Pusan perimeter. After a costly but successful fight for the city of Seoul, the North Korean army was chased back to the 38th parallel that divided North and South Korea.

Now perhaps the most fateful decision of the war was taken. Despite a peace feeler started by India—strongly supported by State Department analysts George Kennan and Paul Nitze—that the original status quo in Korea be restored conditional on China being admitted to the UN, and veiled warnings from China that it would enter the war if the allied forces crossed the 38th parallel, Washington accepted MacArthur's assurance that the possibility of China entering the war was minuscule and ordered pursuit of the North Korean forces into their home country.[3] On November 24, 1950, with his forces near the Yalu River border with China, and promising that the war would be over by Christmas, MacArthur ordered a final offensive designed to crush all remaining resistance.

It did nothing of the kind. Instead, contrary to MacArthur's assurances, the Chinese attacked in force across the Yalu, and in an agonizing retreat the UN forces were forced south and across the 38th parallel where a stalemate developed in the positions the two armies had occupied before the start of hostilities a year before. On July 8, 1951, negotiations for a cease-fire began and lasted for two years. The war was hardly a placid beginning to what was to become known as a dull, placid decade.

The decade did not begin placidly on the domestic front either. On February 9, 1950, a little-known Republican senator from Wisconsin made a speech alleging that some large number of Communists infested the State Department. In succeeding days he polished his speech, and the number of alleged Communists settled down to fifty-seven. He wired President Truman to do something about the situation in the State Department and the press began to pay attention. Joseph McCarthy was becoming a national figure.

The Senate also paid attention. The McCarthy allegations needed investigating, and a subcommittee of the Foreign Relations Committee, under respected conservative Democratic Senator Millard Tydings, was formed to look into the allegations. The committee found no basis for McCarthy's charges, but McCarthy was not cowed. He counterattacked. He named a certain Owen Lattimore as "the top Russian agent" in the United States and alleged that he had been one of the top State Department advisors on Far Eastern policy. Nothing came of the charge, but the country began to listen. McCarthy had struck a responsive chord, and with this came an increase in power. Some powerful conservative Republican senators backed him, and Herbert Block, acid-penned cartoonist of the *Washington Post*, coined the word "McCarthyism." An era of U.S. history had been given a name.

As his support increased, McCarthy's accusations became ever broader and wilder, going so far as to charge in June 1951 that General George C. Marshall was part of "a

[3] William L. O'Neill, *American High: The Years of Confidence, 1945-1960* (New York: The Free Press, 1986): 120.

conspiracy so immense, an infamy so black, as to dwarf any in the history of man." And with the increase in recklessness came ever wider acceptance. A furor gripped the Nation. The frustration in 1949 of the concession of China to the Communists, the Soviet atom bomb, and the Alger Hiss case, expressed itself in a sweeping tide of anti-communism. Liberties that had been taken for granted were in danger of being lost. Loyalty investigations in the Government increased in intensity. The names of innocent men were being tainted and the services of "invaluable specialists" were being lost to the Government.[4]

After the 1952 elections, McCarthy became even stronger. He was given chairmanship of the powerful Committee on Government Operations, as well as of the permanent Subcommittee on Investigations. The Eisenhower administration did little or nothing to counteract him, for the president believed strongly in the separation of powers, and McCarthy's rampage continued. Two of McCarthy's staff members, Roy Cohn and G. David Schine, went on a quick tour of State Department installations in Europe and ostensibly found an "appalling infiltration," whereupon the department banned from its information activities all "books, music, paintings, and the like . . . of any Communists, fellow travelers, *et cetera*." Books were removed from library shelves. Some were stored; some were burned.[5]

At long last, things began to change. When President Dwight Eisenhower, at an extemporaneous speech at commencement exercises at Dartmouth University, decried the book ban, a loud cheer went up from the population. Many citizens by now were getting fed up with McCarthyism.

In early 1954 when McCarthy began an investigation of the U.S. Army, his end was near although he did not know it. His investigation led to an army dentist alleged to be a Communist sympathizer. The army counterattacked with the accusation that McCarthy, Cohn, and Francis Carr, the subcommittee staff director, had all conspired to obtain favorable treatment for Schine, who had been inducted into the army. McCarthy countered with his own charges that the army had tried to halt the exposure of alleged Communists at Fort Monmouth, New Jersey. The Subcommittee on Investigations ordered an investigation, but this time McCarthy was not in charge; his charges were being investigated. Even more important, he met his match in the chief army counsel, Boston lawyer Joseph Welch.

For thirty-six days the televised hearings went on with the Nation in rapt attention. The deft and skillful Welch showed McCarthy for what he was: an overbearing bully. At the climax of a highly emotional exchange, in which McCarthy attacked as a Communist sympathizer a young associate of Welch who was not even involved in the hearings, Welch asked of McCarthy, "Let us not assassinate this lad further, Senator. You have done enough. Have you no sense of decency, Sir, at long last? Have you left no sense of decency?"[6] McCarthy, finally silenced, did not really understand what had happened. After a few seconds the hearing room—including the members of the press

[4] Eric F. Goldman, *The Crucial Decade—and After: America, 1945-1960* (New York: Vintage Books, 1960): 213-214.

[5] Ibid., 252.

[6] Ibid., 278.

Senator Joseph McCarthy (right) blocked an attempt by Army Counsel Joseph Welch (left) to obtain names of his office staff during hearings by a special investigative committee of the Senate in 1954. The Army-McCarthy hearings, chaired by Senator Karl Mundt of South Dakota, began on April 22 and lasted through June 17. (Harris & Ewing photo, courtesy of D.C. Public Library)

—burst into loud applause. McCarthy was finished as a political force. That the Senate went on to censure him was almost redundant; the public had had enough.

Even before the demise of McCarthy as a power in the Senate, international communism had turned a somewhat more benign visage toward the world. On March 3, 1953, less than two months after the Eisenhower administration took office, Joseph Stalin died and things changed. Georgi M. Malenkov, speaking for the triumvirate of himself, Vyacheslav M. Molotov, and Nikita S. Khruschev, offered that international conflicts could be "settled peacefully by mutual agreements of the interested parties." In a few short months, in July 1953, the Korean War ended. Despite some early truculence by Secretary of State John Foster Dulles, the use of Soviet armor to put down uprisings in East Germany, and Khruschev replacing the triumvirate, tensions between East and West did ease. On July 18, 1955, Eisenhower joined with Khruschev and the leaders of Great Britain and France for a Big Four conference in Geneva. Eisenhower, whose policy toward communism had changed from "containment" to "coexistence," proposed, as a start toward disarmament, a mutual exchange of blueprints of military establishments and mutual aerial photography—the "open skies" proposal. The proposal was not to be implemented until much later in the days of reconnaissance satellites, but a new "spirit of Geneva" had entered U.S.-Soviet relationships. It would, in later years, be called "detente."

For a generation that had been raised during the depression years of the thirties, it is perhaps not surprising that even at the height of the Cold War and McCarthyism, the view of the American public had been turned inward, concerned with family, home, jobs, and material possessions. Now, with East-West tensions seeming to relax, with H-bombs that dramatically displayed their power in 1954 appearing to be guarantors of peace rather than instruments of global destruction, and with the president's middle-of-the-road policies, a feeling of equilibrium, of stability, had been achieved. Since the end of the war the Nation had met foreign and domestic issues and had solved them—or at least had learned to live with them.

And for those who lived them, these were times of boundless optimism. Economically the gross national product rose from $284.8 billion in 1950 to $483.7 billion in 1959, and more importantly, over the same period weekly spendable personal income in the manufacturing industries rose from $57.21 to $80.36—a rise of 41 percent.[7] How to spend this extra income? Why on suburban houses, tail-finned cars, TV sets, and babies—all the things that this depression-war generation had lacked. If it was a time to be decried by intellectuals for its shallowness, if the tail-finned cars were "hunks of Detroit iron," it was also a time when more and more people could aspire to a more abundant life.

An African American could also aspire to a more abundant life, but would have trouble achieving it. Although African Americans—still called "Negroes" in the fifties—had made some progress during the Truman New Deal period, and military units had been successfully integrated during the Korean War, racism remained, in the words of O'Neill, "the greatest moral issue facing America in the 1950s."[8]

There were slow changes. In September 1950, Linda Brown, the daughter of Oliver Brown, was refused admission to their neighborhood school in Topeka, Kansas. She was refused admission because, under Kansas law, African Americans could attend only segregated schools. This meant a half-hour, cross-town bus ride, and with help from the NAACP, Mr. Brown brought suit against the Board of Education. The case worked its way up to the Supreme Court, and on May 17, 1954, in what would always be known subsequently as *Brown vs. Board of Education*, the Court unanimously ruled that "separate educational facilities are inherently unequal." A year later it "instructed Federal district courts to require the compliance of local school systems [with *Brown vs. Board of Education*] with 'all deliberate speed.' "[9] The civil rights era had begun, and was to continue a year later in Montgomery, Alabama, with boycotts against segregated seating on buses. The boycotts were under the leadership of a black preacher who counseled non-violent protest. Martin Luther King, Jr., had become the leader of the civil rights revolution.

Now that life was becoming more abundant, Americans slowly began to become concerned with its quality, and the word "ecology" began to be used more frequently. Not that the Nation had not been previously concerned with nature. There were, after

[7] John Patrick Diggins, *The Proud Decades: America in War and Peace, 1941-1960* (New York: W. W. Norton, 1988): 180.

[8] O'Neill, *American High*: 253.

[9] Ibid., 248-249.

all, splendid national parks as a heritage from its past, and more had been added in the immediate postwar years. But now the scale of the problem that affluence posed began to be brought home by the automobile. Many cities, most notably Los Angeles, began to experience noxious brown clouds caused by automobile exhaust fumes and power plant and industrial emissions. Quickly labelled "smog" as a combination of smoke and fog, the clouds stung the eyes and caused breathing problems. But when the wind blew, the clouds dissipated, and the Nation was slow to recognize the magnitude of the problem. Unlike London, which in 1955 passed a clean air act banning the burning of untreated coal due to its air pollution problems, the United States in the same year passed a law protecting only "the primary responsibilities and rights of the States and local governments in controlling air pollution."[10] The law did, however, authorize $5 million to be used, in cooperation with State air pollution control agencies, for research and surveys of the problems. The Nation had begun to be concerned legally with air pollution, but it would not be until the late 1960s that the problem would be attacked seriously.

In the mind of the average American in the late fifties, race relations, the environment, international relations and other problems could be pushed aside. Life was good and, like a Euclidean axiom accepted without proof, the United States was the greatest country in the world. No other could match it. Then, on October 4, 1957, this placidity, this near smugness, was shattered. The next morning the newscasts carried a Soviet announcement that the U.S.S.R. had launched mankind's first satellite, which they called a "sputnik." It circled the earth every 95 minutes and emitted taunting beeps. The U.S.S.R. was clearly well ahead of the United States in rocketry, and recriminations began. They became louder when, on November 3, the Soviets announced the launching of another sputnik, this one weighing more than half a ton and carrying a dog. Was it possible that the United States was not the greater superpower? In the words of Goldman:

> Throughout the United States a sense of alarm, exasperation, humiliation, and confusion mounted. Sputniks I and II dramatized as nothing else could have done that the chief thing on which Americans had depended for their national security and for victory in a competitive coexistence with Communism—the supremacy of American technical know-how—had been bluntly challenged.[11]

The United States accepted the challenge, and the space race began in earnest.

Science Grows Explosively

While the fifties were a time of great economic expansion, the rate of growth of the economy was dwarfed by a huge—and eventually unsustainable—rate of growth of science. Listed in Appendix A are some pertinent figures.[12] In the years 1953-1960,

[10] *AN ACT To provide research and technical assistance relating to air pollution control, U.S. Statutes at Large*, 69 (1955): 322.

[11] Goldman, *Crucial Decade*: 309-310.

[12] The figures on the GNP are from *Historical Statistics of the United States*, 1975 ed., U.S. Department of Commerce. The figures on R&D expenditures are from the National Science Foundation. Figures are given beginning in 1953 because reliable figures are not available for earlier years.

In October 1957, Muscovites gathered around a huge globe at the Moscow Planetarium to hear lecturers describe the sputniks' course around the Earth. In the United States, the sputniks spurred a space race and eventually resulted in an increase of direct appropriations to the Bureau. (AP-Wide World Photos)

the gross national product rose, in current dollars, from $364.4 billion to $503.7 billion, which gives a substantial, average annual growth of 5.45 percent—substantial even if the average inflation rate of about 2 percent for the period is taken into account. Total national expenditures for R&D, however, increased from $5.12 billion to $13.52 billion, a total of 164 percent, implying an annual average growth of 23.42 percent. Even more impressive was the growth in Federal Government R&D expenditures. Over the same period they increased by 217 percent, or an annual growth rate of 31.1 percent, which gives a doubling time of slightly over 2.5 years. The bulk of this went, of course, for development projects for the military and the Atomic Energy Commission (AEC), but even the annual growth rate in in-house Government R&D expenditures was substantial. As we shall see, however, the Bureau's appropriated funds did not share in this increase.

This large increase in R&D expenditures caused dislocations. Throughout the whole period of the fifties the Nation suffered a shortage of scientific manpower. In 1952, the American Association for the Advancement of Science and the discipline-oriented scientific societies formed a manpower commission to act as a consultant to Federal agencies that dealt with manpower shortages.[13] Studies showed that industry added

[13] Editorial, Dael Wolfle, "Scientific Manpower Commission," *Science* 122 (23 December 1955): 1213.

5000 to 7000 new scientists to its payroll yearly, yet failed to get all the scientists it needed in 1953 and 1954. Numerous other studies documented the problem, which was exacerbated by the fact that the pool of young scientists decreased in the early fifties partly due to military service needed for the Korean War.[14]

In the long term, education of scientists was the only answer to the problem, and the National Science Foundation (NSF), which spent only $7200 on education in 1952 (not surprising since the Foundation was only two years old at that time), budgeted $14 million for this activity in 1957.[15] But all domestic remedies had a long time lag. The only recourse was foreign recruitment, and industry, whose R&D expenditures rose 101 percent in the 1953-1960 period, actively recruited in Europe, particularly in Great Britain because there was no language barrier. The efforts were not looked upon favorably by the European countries, which called their loss of scientific personnel a "brain drain."

This immense increase in research and development—particularly in the Federal Government—required attention at the highest levels, but in the early 1950s there was no functioning apparatus at those levels. In December 1947 the Office of Scientific Research and Development which, under Vannevar Bush, had guided the Government's scientific activities during the war, was disbanded. On the same day, President Truman formed the Interdepartmental Committee on Scientific Research and Development. Composed of high-level science executives from all Government departments carrying out scientific research, it was hardly a replacement for the OSRD. Bush had had direct access to the president, while the new committee reported up through the departments. The NSF had been formed in 1950 and, with a very limited budget, was just getting organized. Moreover, it was primarily concerned with basic research and was not located in the Executive Office of the President. Research was being carried out throughout the Government, but without a disinterested advisory mechanism at the highest levels. What mechanism there was consisted mainly of the military and the Atomic Energy Commission, each obviously concerned with its own problems. This situation could not continue for long. The war had demonstrated to the Nation—and to presidents—the value of scientific research, particularly in military matters. Objective advisory assistance and help in policy matters at the presidential level was needed.

In the fall of 1950, under pressure of the Korean War, President Truman commissioned William T. Golden to "review the ways scientific research could be organized at the highest levels in support of military activity."[16] In his report Golden recommended the appointment of a science advisor to the president, and identified Mervin J. Kelly, who was later to head two committees that investigated the Bureau, as the best candidate. But the Bell Telephone Laboratories did not want to lose Kelly, and offered Kelly's superior, Oliver Buckley. Possibly because Buckley was beginning to suffer

[14] "Science and News: Scientific Manpower Commission," *Science* 121 (27 May 1955): 759.

[15] Dael Wolfle, "National Science Foundation, The First Six Years," *Science* 126 (23 August 1957): 336.

[16] James Everett Katz, *Presidential Politics and Science Policy* (New York: Praeger, 1978): 26.

from Parkinson's disease, and partly due to opposition of General Lucius D. Clay, then head of the Office of Defense Mobilization (ODM), the advisory apparatus became a committee reporting through ODM. The post of science advisor to the president was effectively cancelled, replaced instead with a science advisory committee.

In the early Eisenhower years the committee was not without effect. Individual members of the committee, particularly I. I. Rabi, whom Eisenhower had met while president of Columbia University, Lee A. DuBridge, president of the California Institute of Technology, and James R. Killian, Jr., president of the Massachusetts Institute of Technology, served President Eisenhower directly. These men produced or caused to be produced important reports, one of which was influential in developing the U-2 reconnaissance aircraft and the Polaris submarine, and the Gaither Report on "the potential consequences of a Soviet preemptive nuclear first strike, and . . . recommendations . . . for the protection of U.S. retaliatory forces."[17]

These reports fulfilled the intended special purposes, but were produced outside the science advisory committee using only the influence of the highly placed committee members. After the launching of the sputniks, Eisenhower acted to make the structure more formal. Following a meeting with the committee at which Rabi, then its chairman, recommended the appointment of a president's science advisor, the president, on November 7, 1957, announced his plans in a nationwide address. James R. Killian, Jr. would become the first special assistant to the president for science and technology. The science advisory committee was upgraded to the President's Science Advisory Committee (PSAC), with the special assistant as its chairman. A science advisory apparatus in the Executive Office of the President was in place. Then, in 1959 by Executive Order, the president formed the Federal Council for Science and Technology, an organization similar to Truman's Interdepartmental Committee for Scientific Research and Development. This completed the advisory apparatus in the Executive Office of the President. It was to last until President Nixon abolished it in 1973.

Accompanying this enormous growth in scientific research and development was an outpouring of scientific and technological advances from the Nation's and the world's university, government, and industrial laboratories. New scientific advances came at a quickening pace, followed by a myriad of new products.

The transistor, invented in 1948 at the Bell Telephone Laboratories, began the electronic revolution with its first commercial use in trunk dialing apparatus in 1951. Sony, a new name that was soon to become a household word, started Japan on its road to worldwide leadership in consumer electronics by producing the first pocket-sized transistor radio in 1952, following it seven years later with the first transistorized TV set. Computers also began their explosive growth with UNIVAC from Remington Rand in 1951, and IBM with its first scientific computer, the 701, in 1952, and the 7090, a fully transistorized model, in 1959. But one of the greatest spurs to the use of computers was the development in 1956 of FORTRAN, the first of the high-level languages that were to remove some of the tedium from computer programming.

[17] John Walsh, "The Eisenhower Era: Transition Years for Science," *Science* 164 (4 April 1969): 51.

Progress was not limited to solid-state electronics. Indeed, in a stupendous advance for all humanity, Jonas Salk produced a vaccine for polio, holding the promise of removing that scourge from the face of the earth. And in aeronautical engineering, the Comet jet aircraft began passenger service in 1952. That it was to be beset by fatigue failure problems and lose its leadership to the Boeing 707 did not prevent the jet airplane from beginning to supplant the passenger train—at least in the United States.

Nor was every new development for mankind's benefit. In 1952 the first thermonuclear device was exploded in the Pacific, followed two years later by deliverable hydrogen bombs. The process added a new and frightening phrase—radioactive fallout—to the language. When the U.S.S.R. exploded its own hydrogen bomb, an even more ominous phrase—Mutually Assured Destruction—was added.

Not only were advances made in the application of science; they were made in fundamental science itself. Physics, chemistry, mathematics, astronomy, and molecular biochemistry all saw new and basic discoveries. In physics, the long-held concept of parity invariance—namely that atomic and nuclear phenomena should not change upon mirror reflection of the world—was predicted theoretically not to hold in weak interactions by Tsing Dao Lee and Chen Ning Yang in 1957, and the prediction was confirmed experimentally at the Bureau by Chien Shiung Wu, Ernest Ambler, Raymond W. Hayward, Dale D. Hoppes, and Ralph P. Hudson. Also in physics, in 1955 James P. Gordon, Herbert J. Zeiger, and Charles H. Townes of Columbia University published a paper entitled, "The Maser—New Type of Microwave Amplifier, Frequency Standard, and Spectrometer."[18] This was a paper of surpassing importance for precision measurements, for it planted the seed for the laser.

In chemistry, Giulio Natta, of the Milan Polytechnic, developing ideas of Karl Ziegler, found ways of producing versions of the common vinyl polymers like polystyrene and polymethylmethacrylate that are geometrically regular on the molecular scale and hence can crystallize, unlike their more common siblings, which are polymeric glasses. A new and commercially important class of strong, tough materials had been discovered.

Also in chemistry, Willard Libby, using the idea that the atmosphere contains a constant and known concentration of radioactive C^{14}, and therefore so do plants, devised an ingenious and important means for determining the age of artifacts such as textiles or wooden articles. Once the living plant dies, the concentration of C^{14} begins to decrease by natural radioactive decay, and thus the measurement of its concentration, along with the known half-life of C^{14}, provides a convenient and accurate means of calculating the age of the artifact.

Other notable advances helped change science and life in general. The first photocopying machine came on the market in 1950, and in the same year commercial color television began. Particle accelerators achieved higher and higher energies and helped in the discovery of the anti-proton and anti-neutron. In 1955 Severo Ochoa found a way of essentially synthesizing RNA, and a year later Arthur Kornberg

[18] J. P. Gordon, H. J. Zeiger, and C. H. Townes, "The Maser—New Type of Microwave Amplifier, Frequency Standard, and Spectrometer," *Physical Review* 99 (1955): 1264-1274.

Arthur Godfrey told the merits of a sponsor's product during the inaugural color television program by Columbia Broadcasting System in 1951. (AP-Wide World Photos)

did the same with DNA. John Bardeen, Leon N. Cooper, and John R. Schrieffer produced a definitive theory of superconductivity, and a peculiar celestial star-like body of enormous energy was discovered and named "quasar" for "quasi-stellar object." It was neither a placid nor a static decade for science.

Divestiture and Reaffirmation

As the decade of the fifties began, the Bureau, beginning to be embroiled in the AD-X2 ordeal, also began its own period of explosive, but short-lived and peculiar, growth. The Korean War was to bring to the Bureau an immense increase in funds and in size, but for military work, not the measurement standards work which was its unique reason for existence. In the early years of the decade it lost a director to the Nation's obsession with Communists, and almost lost another due to the AD-X2 affair. Most important, it almost lost its identity. It had to divest itself of the war work acquired during World War II and the Korean War—important as that work was—and re-discover itself and its principal function. Almost providentially, it began the decade by restating its reasons for existence.

A NEW ORGANIC ACT

On October 31, 1945, Gano Dunn, president of the J. G. White Engineering Company and chairman of the Bureau's Visiting Committee, was chagrined. The committee had been asked by Secretary of Commerce Henry A. Wallace to submit nominations for director of the Bureau to replace Lyman J. Briggs, who had submitted his resignation on July 22, 1945, but the committee had been dilatory in its response. Wallace proceeded on his own and selected Edward Uhler Condon as his choice to replace Briggs. Condon was in due course confirmed on November 2.[19] Now Dunn wrote to the other members of the committee and explained that he had met with Wallace who had cordially accepted Dunn's explanation of the delay.[20]

The committee was also asked to comment on another matter. The secretary had asked Briggs to prepare a revision of the Bureau's 1901 Organic Act according to Wallace's instructions, and Briggs had personally done so. Wallace was now in the process of recommending new legislation to the Senate and House. Dunn wrote to the committee, "the principal part of the amendment is for the purpose of transferring to the Organic Act certain authorizations, definitions of scope and activities of the Bureau that in the past have been covered by supplementary legislation, executive orders and customary procedure." He now asked the committee to comment on the proposed legislation.

While a revision of the Organic Act had not previously been proposed, the role of the Bureau in the post-World War II years had been discussed since the early years of the war.[21] In particular, the enlargement of the Bureau's mission to include basic research came under active contemplation. Now, Wallace's proposed amendment specifically took up this issue. In his draft letter of transmittal of the proposed legislation to the president of the Senate, Wallace wrote:[22]

> Section 2(a) of the Organic Act of the National Bureau of Standards has been extended to include 'the prosecution of basic research in physics, chemistry and engineering to promote the development of science, industry and commerce.' . . .

In addition to this important phrase, two other substantive ones were mentioned by the secretary:

> Section 2(a) also carries the phrase, 'the collection and dissemination of information on electrical conditions in the atmosphere affecting radio communication.' . . .

[19] The circumstances surrounding Wallace's selection of Condon are well detailed in MFP, 435.

[20] Letter, Gano Dunn to the Visiting Committee, October 31, 1945. (NARA; RG 167; Records of the Director, 1923-63, Director's Correspondence file; Box 15; Folder Dunn/APV). The other members of the committee were Vannevar Bush, Karl T. Compton, William D. Coolidge, and Frank B. Jewett.

[21] MFP, 432-434.

[22] Draft letter, Henry A. Wallace to the President of the Senate (attached to the letter from Gano Dunn to the Visiting Committee, October 31, 1945). (NARA; RG 167; Records of the Director, 1923-63, Director's Correspondence file; Box 15; Folder Dunn/APV). It is not known whether these three items were at Wallace's instigation, or whether Briggs introduced them. Considering their specificity it is highly likely that the latter was the case.

and

> In Section 2(b), a clause has been added authorizing the Secretary of Commerce, with the approval of the Civil Service Commission, to appoint outstanding scientists without reference to the provisions of the Classification Act. . . .

The second item was a recognition of the fact that the Interservice Radio Propagation Laboratory, formed during the war to provide radio weather forecasts for the armed services, had in fact become a continuing responsibility of the Bureau, and was useful to the civilian sector as well as the military. The third item was justified as follows:

> Men of the type we have in mind are now able to command salaries in universities higher than is authorized by the Classification Act. The appointment of a very limited number of such men from time to time would be of great value to the Bureau in conducting its work, and it is believed that the authority to do so is adequately safe-guarded by providing for the approval of the Civil Service Commission.

Besides these, a number of relatively minor items related to such things as the title of equipment bought with transferred funds and the disposition of surplus equipment, and a long listing of specific research activities the Bureau was already conducting.

Of all these items, the one relating to basic research was the most critical. Its adoption would doubtless have changed the character of the Bureau dramatically. The Bureau would have gone from the Nation's measurement standards laboratory, with all its basic research deriving from that function, to a laboratory carrying out basic research to "promote the development of science, industry and commerce," whether the research was concerned with measurement or not.

This did not sit well with Vannevar Bush, the most influential member of the committee, who at this time had recently published his report, *Science, the Endless Frontier,* and had quite different ideas of how the Government should support basic research in the Nation. On November 21, 1945, he wrote to Dunn, who two days later transmitted his comments to Wallace:

> [T]he Bureau of Standards is the only body which has both the responsibility and authority to perform the exceedingly important function of establishing standards of all kinds, and in the future the Bureau is going to be subjected to a heavy and increasing burden in this regard as a result of the rapid progress of science. . . .
>
> Hence, while I believe that it [the legislation] is important to the effective organization of the Bureau and to its ability to conduct basic research . . . it should be unmistakably clear that the major emphasis should remain on its unique assignment in the field of standards.[23]

[23] MFP, 433-434; letter, Vannevar Bush to Gano Dunn, November 21, 1945 (attached to letter, Dunn, November 23).

The revision was not passed in its original form. Indeed, it was not passed until July 22, 1950, by which time its instigators, Wallace and Briggs had long since left the Federal Government. It was submitted to the House Committee on Interstate and Foreign Commerce by the then Secretary of Commerce Charles W. Sawyer on June 17, 1949, with the justification:

> This legislation is considered necessary in order that basic authority for the functions of the Bureau will include a more specific outline of the scientific research and testing now carried on by the Bureau. This is particularly desirable in view of the advances which have been made in certain fields of science.[24]

Except for one important change, the new law was a much more moderate revision than originally proposed. The major change occurs at the very beginning. Whereas the original act, and that written by Briggs and proposed by Sawyer, simply state, "The functions of the Bureau shall consist . . . ," the new law states, "The Secretary of Commerce . . . is authorized to undertake the following functions. . . . " Authority for the functions of the Bureau is now vested in the secretary, rather than in the Bureau director. Similarly, all other specific references in the law to functions of the Bureau are changed to functions of the secretary. The law specifies, however, that the director is appointed by the president.

This change was made by the House committee, but it was not done as a result of any prejudice toward the Bureau. Rather, it was "to make this bill conform to the provisions of Reorganization Plan No. 5 of 1950, which transferred to the Secretary of Commerce all functions of all other officers of the Department of Commerce and all functions of all agencies and employees of such Department."[25]

The functions that the secretary is authorized to carry out are spelled out in six paragraphs, rather than in a single one as in the old law. However, with the one exception discussed below, the first two paragraphs essentially re-state the first of the original act. The added four are merely statements of what the Bureau was already doing. Following this statement of authorized functions, there follows a listing of nineteen activities which, along with "similar ones," the secretary is allowed to undertake. Some were obvious, others less so.

None of the three new functions listed by Wallace in the covering letter to his original proposed revision is specifically mentioned. In fact, with respect to avoiding the Classification Act of 1949, and referring to Sec. 4 of the original act—in which salaries were specified—the law reads, "Sec. 4 (Salaries of officers and employees. This section superseded by Classification Act.)" Briggs' hope of getting special treatment for scientific personnel was effectively dashed. The "radio weather" function is not mentioned, doubtless because it was clear that the Bureau was able to carry out this function without specific authorization. In fact, this function was a line item in the Bureau's budget between 1950 and 1956, when the Bureau's budget request was

[24] House Committee on Interstate and Foreign Commerce, *Bureau of Standards Functions*, 81st Cong., 2d sess., 1950, H. Rept. 2349: 3.

[25] Ibid., 2.

changed. And, with respect to the most important new function in the proposed ammendment by Wallace and Briggs, that of carrying out "basic research . . . to promote science, industry and commerce," it was simply not included. It is well to remember that the National Science Foundation was established in 1950 with the specific aim of funding basic research throughout the Nation. Authorizing this function for the Bureau would have meant that two agencies of the Government would share the same function, a clearly undesirable situation. The Bureau was not, however, excluded from carrying out basic research. Authorized activity number 18 reads, "the prosecution of such research in engineering, mathematics, and the physical sciences as may be necessary to obtain basic data pertinent to the functions specified herein." The Bureau could do basic research, but within circumscribed boundaries, which is what Vannevar Bush suggested in his letter to Dunn.

A comparison of the authorized functions in the old and new Organic Acts is instructive in illustrating how the Bureau had grown since its inception. In the 1901 law, all the functions are contained in one paragraph:

> Sec. 2. That the functions of the bureau shall consist in the custody of the standards; 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.

In the 1950 law, the first paragraph of the 1901 act—with one significant change—is essentially repeated in the first two paragraphs, and four others are added.

> (a) The custody, maintenance, and development of the national standards of measurement, and the provision of means and methods for making measurements consistent with those standards, including the comparison of standards used in scientific investigations, engineering, manufacturing, commerce, and educational institutions with the standards adopted or recognized by the Government.
>
> (b) The determination of physical constants and 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.
>
> (c) The development of methods for testing materials, mechanisms, and structures, and the testing of materials, supplies, and equipment, including items purchased for use of Government departments and independent establishments.

(d) Cooperation with other governmental agencies and with private organizations in the establishment of standard practices, incorporated in codes and specifications.

(e) Advisory service to Government agencies on scientific and technical problems.

(f) Invention and development of devices to serve special needs of the Government.

The significant change is the lack of Stratton's marvelous catchall phrase, "the solution of problems which arise in connection with standards."[26] Due to the tradition it had fostered, its lack would not hamper the Bureau. The new functions (c) to (f) were no more than statements of functions the Bureau had developed in its history. Item (c) was not in the original act, and was the one that led to the AD-X2 incident, although in the vast majority of cases a valuable service was performed for the Government. Item (d) covered the Bureau's interaction with the organizations of the voluntary standards system of the Nation. Item (e) referred to work for other agencies of the Government, and item (f) referred primarily to work carried out during the two world wars, which had led to such developments as the proximity fuze and computers.

With the exception of vesting in the secretary of commerce the authority to carry out all the functions of the Bureau, which was in any case necessitated by law, there was no significant change in the Bureau's Organic Act. The Bureau was not prevented from continuing to do what it had been doing. The revision seems rather to have been designed to answer those members of Congress who could not understand how the Bureau could do all the things it did on the basis of "a two-page law," and to list a number of examples of activities the Bureau carried out, and clearly felt it needed to continue to carry out. The new law did not noticeably change the Bureau's mode of operation.

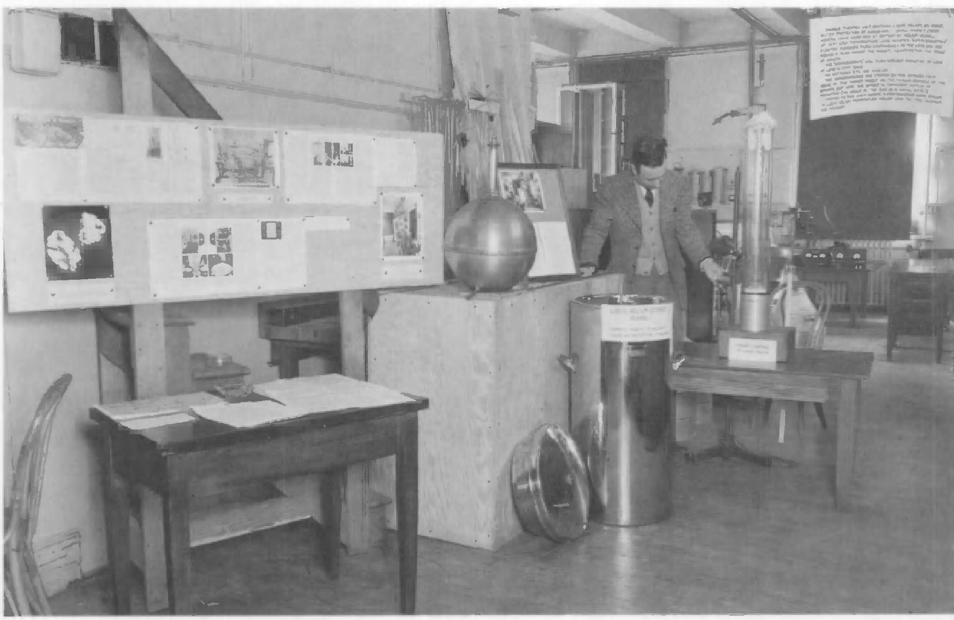
On March 2, 1951, the year after the passage of the new Organic Act, the Bureau turned fifty years of age. It was a time of commemoration and of celebration. After some pondering about what kind of celebration to have, Condon decided to schedule twelve symposia on technical issues of importance to the Bureau and the Department of Defense, which helped financially with the symposia. With speakers from throughout the scientific community, the topics were low-temperature physics, influence of low temperatures on the mechanical properties of metals, gravity waves,[27] solution of simultaneous equations and determination of eigenvalues, mass spectrometry, energy transfer in hot gases, electrochemical constants, polymer degradation mechanisms, evaluation of optical imagery, electron physics, characteristics and application of resistance strain gages, and electrodeposition research. In addition, more than thirty technical societies and associations held meetings in Washington to commemorate the

[26] A Bureau wag likened this phrase to a "license to steal."

[27] These were waves, such as water waves, in which gravity was one of the causative factors, not the yet-to-be-observed waves predicted by general relativity in the cosmological gravitational field.

Bureau's semicentennial. These were happy occasions and many societies and associations provided commemorative anniversary scrolls.[28]

Not quite so happy for the junior staff, who had to spruce up the laboratories, was the open house held in February 1955. Suggested by Sinclair Weeks to make people more aware of the nature and importance of the Bureau's work, the open house featured presentation ceremonies by Director Astin, followed by laboratory tours and demonstrations. It was a successful event, attended by some 600 scientists from the Nation's laboratories. The open house was followed in April by a more informal Guest Week for the general public.[29] In more ways than one, the Bureau was emerging from its wartime cocoon.



The NBS Open House was attended by several hundred leaders in the fields of science, industry, government, and education. It stressed the significance of physical measurement standards to scientific and industrial progress. Ralph Hudson demonstrated equipment for visitors in the low temperature lab.

Loyalty, Security, and the Resignation of a Director

When Edward Uhler Condon joined the Bureau as its director on November 7, 1945, he was already a world-famous theoretical physicist and scientist-administrator, on first-name terms with all the leading figures in science. Personally, he was a

[28] Annual Report, 1951: 100. See also *Technical News Bulletin*, 1951 and 1952, for details of the symposia and the society meetings.

[29] Annual Report, 1955: 11.

gregarious, enthusiastic, friendly man who did not suffer fools gladly, was impatient with sloppy thinking, had "an ever ready and exuberant sense of humor, and a gift of repartee, but he could be wittily caustic when provoked."[30] He was vigorous and aggressive with myriads of new ideas, and was not afraid to push them.

When he came to the Bureau he found a capable institution, even though the scientific ideas of many of its leaders were rooted in the past, not the modern physics which he had helped foster. Given his background and personality, it was only natural that Condon should set about to remake the Bureau and lead it—with some kicking and screaming—into modern science. How he did this, and the areas he led it into are well described in *Measures for Progress* and will not be emphasized here.[31] Rather, Condon's relations with the loyalty and security apparatus of the Nation in the post-World War II years will be discussed, beginning with a short biography and emphasizing some aspects that were important in influencing those relations.

Edward Uhler Condon was born March 2, 1902, in Alamogordo, New Mexico, where forty-three years later man's first nuclear explosion would take place. His family was mobile, and he went to various grammar schools throughout the West. He attended high school in Oakland, California, graduating at the ripe age of sixteen. During his last two years in high school, he went into newspaper reporting, first for the high school paper, and later, in the summer of 1918, for the "regular newspapers" of Oakland.

Two events which shaped his future thinking happened in Condon's short newspaper career. First, in October 1918, he recalls:

> [A] regiment of Oakland boys was put into the front-line trenches in France in the final battles of the war. . . . In the final days of the war, these boys were being killed at the rate of about five a day. My steady assignment as a 16-year-old reporter was to go out each day to interview the mothers of the boys on that day's casualty list, and to steal photos or letters whenever possible. . . . I was often the first one to convey the news to the mother that her son was dead.
>
> Such experiences left a deep-seated scar on me and an urgent need to do what discouragingly little I can toward bringing about peace and disarmament. . . .

The second occurred on November 9, 1919. He was

> the only reporter from a conservative newspaper to cover the organization meeting of the Communist Labor Party of California, as it was called then. I wrote lurid and sensational stories about this small group of one or two hundred persons, which resulted in indictments against them, and which required that I had to testify against them, in trial after trial, over the next several years. In this connection I became aware of open boasting by a police detective of his having framed some of the defendants in a matter where I knew the facts to be otherwise.

[30] Churchill Eisenhart, "Dr. E. U. Condon (1902-1974), Renovator of the NBS." *Dimensions* 58 (1974): 150-151, 166.

[31] MFP, 434-446.

Edward Uhler Condon, world renowned theoretical physicist, became the fourth director of the National Bureau of Standards and the first to be appointed from outside the Bureau's ranks. He reorganized the Bureau in the postwar years, brought in bright young people, and instilled in the staff a great desire to do basic research in physics, chemistry, and on methods which provide the basis for measurements.

> The effect of this involvement on me was to wipe out any desire to be even an educated newspaperman; so I entered the university and went into physical science largely as a means of escape from the corruption of the world, in addition to the fact that I was genuinely interested in physical science.[32]

Entering the University of California at Berkeley in 1921, he flirted with chemistry and engineering before finally choosing physics as his profession. By 1926, at twenty-four, he had a wife, having married Emilie Honzik in 1922, and a Ph.D. in physics, having developed in his thesis the basis for what was to become known as the Franck-Condon principle. He then spent a year at Göttingen and Munich learning quantum mechanics from the European masters. Upon returning to the United States, he spent a year at Columbia, one at Princeton, and another at the University of Minnesota where, at the age of twenty-seven, he was a full professor of theoretical physics. In 1930 he returned to Princeton, where he remained until 1937 when he left academia and joined the Westinghouse Research Laboratory as associate director of research. He stayed there until he joined the Bureau as director in 1945.

As expected for a scientist of his caliber, he was very active in war research.[33] The following is a listing of his positions:

- In the fall of 1940, he was among the first dozen staff attached to the Radiation Laboratory at MIT. Under sponsorship from the National Defense Research Committee (NDRC), this laboratory had responsibility for all secret microwave and radar developments for the military agencies.

[32] E. U. Condon, "Reminiscences of a Life in and out of Quantum Mechanics," *Proceedings of the International Symposium on Atomic, Molecular and Solid-State Theory and Quantum Biology*, ed. Per-Olav Löwdin, *International Journal of Quantum Chemistry, Symposium no. 7.* (New York: Wiley, 1973): 9.

[33] This list of activities is taken from a letter from Condon to the Army-Navy Personnel Security Board, February 7, 1953. (American Physical Society; Condon File; Security, Box 3; Folder Security Investigations, 1953, No. 3. Used with the permission of the American Philosophical Society.)

- Subsequently, he was chairman of the Westinghouse Electric Corporation's committee on radar, which brought him into close association with secret radar matters at the Naval Research Laboratory. This association allowed him to receive information on highly classified radar developments from the military departments on the manufacture of radar equipment.
- In 1941 he served on an NDRC committee which considered and recommended the development at the California Institute of Technology of a large secret rocket program.
- In the summer of 1941, he was appointed a member of the highly secret S-1 Committee under Lyman J. Briggs. This was the committee established by President Franklin D. Roosevelt upon receipt of the famous Einstein letter, and led to the eventual formation of the Manhattan Project.
- During 1942 he was actively engaged in secret radar research work in the laboratories and plants of the Westinghouse Electric Corporation.
- In April and May 1943, he served for a short time as associate director of the newly established Los Alamos Scientific Laboratory. While there he wrote a manual from the notes that he took during a series of five lectures given by Robert Serber. Called *The Los Alamos Primer,* the report was so secret that, after Condon left Los Alamos, he did not see a copy of it until it was declassified.[34]
- In the early fall of 1943, he was assigned by Westinghouse to work at the Radiation Laboratory of the University of California at Berkeley. The work was highly secret and concerned entirely with the atomic bomb. In late 1945, upon successful completion of his phase of the work, Condon returned to Pittsburgh and resumed his radar work.

There needs to be added to this account the fact that when Condon became director of the Bureau, Senator Brien McMahon, chairman of the Special Senate Committee on Atomic Energy, was holding hearings on a bill to remove atomic energy from the military and place it under civilian control—something that was supported by President Truman but opposed by the military. When Condon arrived in Washington he was asked by the McMahon committee to serve it as a scientific advisor, whereupon he was detailed from the Bureau to do so. From late 1945 until mid-1946, Condon gave lectures to senators, explaining to them the fundamentals of atomic energy. When the law was passed on August 1, 1946, and the Atomic Energy Commission was formed, Condon returned to being director of the Bureau full time.

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[34] Letter, Glenn T. Seaborg to E. U. Condon, February 26, 1963. (Copy in NIST History File). Condon wrote to Seaborg who was then Chairman of the Atomic Energy Commission and requested a copy of the now declassified *Primer* for his files. Seaborg complied with Condon's request and noted "its historical significance and the fact that it was apparently the first report issued by the Los Alamos Laboratory." The Primer was later annotated by Robert Serber and published in 1992 as the *The Los Alamos Primer: The First Lectures on How to Build an Atomic Bomb,* University of California Press.

Photographed while conferring on military control of atomic energy development were (left to right: Harlow Shapley of Harvard University, Representative Chet Holifield, Democrat of California, and Harold Urey and Thorfin R. Hogness of the University of Chicago. A bill that would transfer atomic control from the military to a civilian agency was under consideration in 1946. Holifield would later come to Condon's defense when his service in the capacity of scientific advisor to the Senate on this question resulted in accusations of disloyalty. (AP-Wide World Photos)

But he even had to take some time off from his advisory activities, for the president asked him to become one of a number of scientific observers at Operation Crossroads, the testing of atomic bombs at Bikini Atoll in the spring of 1946. At this operation he met and became friendly with Congressman Chet Holifield of California, an event that was to stand him in good stead in the following years.

Thus, in 1946 the Bureau had as its director a vigorous, driving man who was one of the scientific leaders of the world, had participated in some of the most important and secret work of the Nation during the war, and had undertaken many special assignments. Not a retiring man, he was an outspoken liberal and a fervent anti-isolationist. What was perhaps not as obvious, the new director was a pious man who was a Quaker by upbringing, and from his experiences was passionately against war and devoted to peace and cooperation among nations. "My wife and I," he wrote and had published in the *Congressional Record*,[35] "have fervently hoped that the wartime

[35] House, Extension of Remarks of Hon. W. Sterling Cole of New York in the House of Representatives, *Congressional Record,* March 24, 1952, 98:A1827.

cooperation which existed between Russia and the United States would develop into a peacetime friendship like that between England and the United States. For this reason we have always tried to be friendly to people from the Slavic countries whom we met in official diplomatic contacts at Washington."

As a result of his newspaper experience he also had developed a healthy skepticism about the actions and utterances of public officials, and a dislike bordering on loathing for the cynicism of some of them.

* * *

On March 1, 1948, New Jersey Congressman J. Parnell Thomas, chairman of the House Committee on Un-American Activities, lay ill in bed at Walter Reed Hospital, suffering from an attack of gastrointestinal hemorrhages. Nevertheless, he reportedly found time to meet with Congressmen Richard B. Vail and John S. Wood, the other two members of the Subcommittee on National Security, to decide whether to release a "Report to the Full Committee." The report carried out one of the functions of the committee, and the decision was made to release it. The function promised, in ungrammatical fashion, "those groups and movements who are trying to dissipate our atomic bomb 'know-how' for the benefit of a foreign power will have the undivided attention of our committee agents, as well as those who are seeking to weaken other aspects of our national security."

Described as "preliminary," and of an investigation that was not yet complete, the report dealt with only one topic: Edward U. Condon, director of the National Bureau of Standards. In a "matter which is of such importance that it demands immediate attention," the report stated at the very beginning, "from the evidence at hand, it appears that Dr. Condon is one of the weakest links in our atomic security. In substantiation of this statement the subcommittee respectfully submits the following information." Six pages of text purported to provide the substantiation.[36]

Newspaper headlines flashed the news around the country to a dumbfounded Nation, but it was not the first time Condon had run afoul of the committee, or at least of one of its members. During 1947, five articles originating from the committee or from Representative Thomas appeared, three in the *Washington Times-Herald*, a newspaper "which has always had close and friendly relations with the Un-American Activities Committee and which has often been used by the committee to send up trial balloons."[37] These articles attacked Condon and promised that he would be investigated. In June, Thomas wrote two signed articles, one in *American* magazine, and one in *Liberty*. These articles attacked Condon because of his association with the American-Soviet Science Society.

Several times Condon tried to be heard by the committee; his requests were ignored. The attacks were so blatant that Representative Holifield was impelled to bring them

[36] House Committee on Un-American Activities, Subcommittee on National Security, *Report to the Full Committee of the Special Subcommittee on National Security of the Committee on Un-American Activities*, 80th Cong., 2d sess., March 1, 1948.

[37] Robert K. Carr, *The House Committee on Un-American Activities* (Ithaca, NY: Cornell University Press, 1952): 132.

up on the floor of the House. On July 22, 1947, he delivered a scathing denunciation, answering the attacks point by point and showing them to be false or misleading.[38] He came to the conclusion that Condon was attacked because of his activities as advisor to the Senate Select Committee on Atomic Energy, and that those who did not want to see civilian control of atomic energy were behind the attacks.

But the later charges in the subcommittee's report were more serious. This was a formal attack on Condon made by a subcommittee of the United States Congress, without the accused having been given an opportunity to answer the charges. The document was filled with errors, inaccuracies, and innuendo. Eight days later, Holifield delivered a piercing evaluation of it. His analysis was complete and thorough, and his main points are summarized here.[39]

The subcommittee's report suffered from sloppy staff work and writing. In the first two paragraphs there are seven inaccuracies, mostly unimportant and one even amusing, but nevertheless indicative of the lack of care taken in the writing of the report. For example, the maiden name of Mrs. Condon was given as Emilie Honzek rather than Honzik; Condon's position at Princeton was given as "associate director of the physics department," a nonexistent post; Condon was associate professor. The amusing one read as follows: "Condon is principally regarded as a theoretical physicist which involves radar, nuclear physics, radioactive tracers, mass spectroscopy, and the elastic properties of metals." When giving the background of Mrs. Condon, she is gratuitously identified as "an American-born woman of Czechoslovakian descent." The reason for this remark arose at the end of the report, where the following passage occurs: "In this country they [the communists] haven't gotten as far as they have in Czechoslovakia, but they got pretty far, because they got a man [Henry Wallace] as Vice President of the United States, and he is now their candidate for President, and he is the same man who recommended Dr. Condon as Director of the Bureau of Standards." By innuendo, Mrs. Condon's ancestry served to imply that, at a minimum, the Condons were friendly toward communism.

Two questions concerning Condon's early days at the Bureau attracted the committee's attention. One was security clearance. Condon had, of course, been completely cleared for his atomic energy work at Los Alamos and Berkeley, and for all the other areas he worked in. He retained those clearances when he came to the Bureau. However, upon the formation of the Atomic Energy Commission (AEC), that agency set up its own clearance procedures and had not yet completed them on Condon at the time the matter was investigated by the subcommittee. Under the AEC's "need to know rules," he was still excluded from certain areas, such as atomic weapons. Hence his clearance was marked "pending," but this was no more than bureaucratic inertia.

[38] Representative Chet Holifield, speech, *Smearing the Scientists: Attempt To Discredit Civilian Atomic-Energy Control*, reprinted from Congressional Record, July 22, 1947, vol. 93.

[39] Representative Chet Holifield, speech, *Sabotage of American Science: The Full Meaning of Attacks on Dr. Condon*, reprinted from Congressional Record, March 9, 1948, vol 94. Holifield analyzed the whole report of the Committee on Un-American Activities in this speech that contained the full text of the subcommittee's report.

With respect to the second question, that of the reorganization of the Bureau, Condon carried this out in 1947.[40] In the process, and as was natural considering his background and the main thrust of modern physics, he formed a division called the Atomic and Radiation Physics Division, and made himself division chief until such time as he could find someone qualified. The report implies that he did this so he could be closer to the secret atomic work at the Bureau, but as director he would, of course, have known about it anyhow.

These were minor points. Major sections of the report were concerned with two letters, and with associations. With respect to the first letter, it states the following:

> That the Atomic Energy Commission had reason to doubt the loyalty of Dr. Condon is evidence by a letter, the original of which the subcommittee has in its possession, which letter was dated July 11, 1946, and is addressed to a Member of Congress, who at that time was a member of the Joint Committee on Atomic Energy of the Congress. This letter was written by a person who held a high post in the security division of the Manhattan Project, and who is now a ranking official of the Atomic Energy Commission. The first paragraph of this letter is quoted in part as follows:
>
> > Attached is a very hurried attempt which may be of some help. Unfortunately, the . . . group has loaded me down in preparation for Friday's meeting. May I suggest that you demand Dr. Condon's record of the FBI. It would be enlightening.[41]

This letter was demolished by Holifield. Briefly, it was a letter from an unknown person to an unknown member of Congress on a nonexistent committee, for the Joint Committee on Atomic Energy was not formed until the year following the date of the letter. While it did not say anything harmful about Condon, it implied a great deal. But it said more about the unknown author. As pointed out by Holifield, if indeed the author had been a high official in the Security Division of the Manhattan Project and was now a ranking official in the AEC, and if he knew all this time that Condon was a security risk and did nothing about it, he could be accused of dereliction of duty.

A significant portion of the report dealt with associations. These were mentioned in a confidential letter from the FBI to Secretary of Commerce Averell Harriman. A portion of this letter had come into possession of the committee, and it was quoted "in part."[42] A particularly important part read:

[40] MFP, 438.

[41] Holifield, *Sabotage of American Science*: 9.

[42] The subcommittee did not have the full text of the letter. Chairman J. Parnell Thomas had sent a letter to an employee of the Department of Commerce asking that one of the subcommittee's investigators be furnished "any information you have available on Edward U. Condon. . . . " The investigator was "permitted to make a brief examination of a file of papers and documents, among which was the letter. . . . [He] undertook to make a copy of this letter, but before he was able to copy all of it he was requested to discontinue." (J. Parnell Thomas, *Congressional Record,* May 14, 1948, 94:5862-63. A critical part of this letter was not in the missing portion, yet was not quoted in the subcommittee's report.

The files of the Bureau [FBI] reflect that Dr. Edward U. Condon has been in contact as late as 1947 with an individual alleged by a self-confessed Soviet espionage agent to have engaged in espionage activities with the Russians in Washington, D.C., from 1941 to 1944.[43]

The report did not detail what the nature of the contact was, but this was clarified three days later when, on March 4, an enterprising reporter from the *Washington Post* supplied a missing sentence of the letter that, according to its chairman, the subcommittee had "inadvertently" left out. The sentence read:

There is no evidence to show that contacts between this individual and Dr. Condon were related to this individual's espionage activities.[44]

Holifield commented on this omission:

What does this deletion or omission mean in terms of a full and careful presentation of evidence? What is this if not deliberate character assassination, without regard to truth, justice, the democratic processes, honesty, integrity, or fair play?

The origin of the letter is not completely known. What is known is that in April 1947, after the subcommittee's attacks Condon, Secretary Harriman was asked by Condon to investigate him. Harriman did this and "subsequently assured Dr. Condon that he was entirely satisfied."[45]

Other associations, some with persons from Eastern European diplomatic circles, were mentioned in the letter, which at this point mentioned Condon's wife Emilie as well. The Condons had never met some of the persons with whom they were alleged to have had associations, and the others were met in the normal course of Washington diplomatic life. There is no evidence presented to show that these associations were any more than that.

The report went on to detail that there were many foreign visitors to the Bureau, many from Eastern European countries; that Condon had appointed an assistant, Dmitri I. Vinogradoff, a Russian-born American citizen with whom he had worked at Westinghouse, to act as liaison for these visitors; and that discussions were held with Soviet representatives on the exchange of scientific publications.[46] Nowhere was it

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[43] Holifield, *Sabotage of American Science*: 10.

[44] Alfred Friendly, "Condon Associated With Alleged Spies, Anti-Red Unit Charges." *The Washington Post,* March 3, 1948: A1.

[45] Holifield, *Sabotage of American Science*: 11.

[46] Ibid., 16. The Bureau had an established policy of exchange of scientific publications with the Soviet Union and had a mailing list of about seventy Russian institutes to which Bureau publications were routinely sent. Condon was anxious to get as much Russian scientific information as possible, but after the war the flow of such information from the Soviets had slowed down. Condon asked Vinogradoff to look into the matter. The Soviet Embassy explained that the slowdown was because of the destruction caused by the war; however, rumor persisted that the Soviets had adopted a policy of not sending material. Vinogradoff consulted with the State Department and, in cooperation with State, Condon wrote to the Russian institutes that "in view of the uncertainties the Bureau could no longer send our material to them." Rather than Condon increasing the flow of material to the Soviet Union, he decreased it.

stated that there was any discussion other than of published material freely available in the scientific press was discussed. Yet the implication was left that something shady was taking place.

Finally the report brings up the American-Soviet Science Society (ASSS). This small society, numbering about 400 members, was formed to stimulate scientific cooperation between the Soviet and American scientists. It had originally been formed as the science committee of an organization called the National Council of American-Soviet Friendship (NCASF). The latter group was formed in late 1943 when the United States and the Soviet Union were allies, and was sponsored by many distinguished Americans, among them Karl T. Compton, Albert Einstein, and Senators Elbert Thomas of Utah, Arthur Capper of Kansas, and Leverett Saltonstall of Massachusetts.[47] At one time Mrs. Condon was the corresponding secretary of what was essentially the Pittsburgh branch of that group. However, the ASSS disassociated itself from the NCASF, and in 1946 was a separate organization,[48] while in 1947, the NCASF was on the Attorney General's list of subversive organizations. The ASSS, on the other hand, was a tax-exempt organization with a grant of $25 000 from the Rockefeller Foundation. It had also done some consultation for the Army and Navy, and carried out translation of scientific work, which was its principal objective.[49]

Condon was accused of proselytizing the staff of the Bureau for membership because he lent his name to a letter from Samuel Gelfin of the membership committee of the ASSS to the Bureau staff asking them to consider joining. To Condon this was a perfectly routine matter, and he put no pressure on anyone to join.

The report closed with the unequivocal recommendation:

> It is the unanimous opinion of this subcommittee that Dr. Condon should either be removed or a statement should be forthcoming from the Secretary of Commerce, setting forth the reasons why he has retained Dr. Condon, in view of the derogatory information which he has had before him.

Holifield remarked, "With regard to the conclusions and recommendations, it is my opinion that no evidence has been adduced in this report or any other which merits a breath of support to the conclusion that he [Condon] should be removed."

In a manner similar to the events that would take place after Allen V. Astin's dismissal as Bureau director during the battery additive episode, the report caused a national sensation. Condon was front-page news in the Nation's leading newspapers for more than a week, with the bulk of the reaction against the report and the subcommittee. The scientific community rose up in arms in support of Condon, who was then president of the American Physical Society. He received so many letters of support that he wrote a form letter to answer them.[50] A testimonial dinner, attended by

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[47] Ibid., 17.

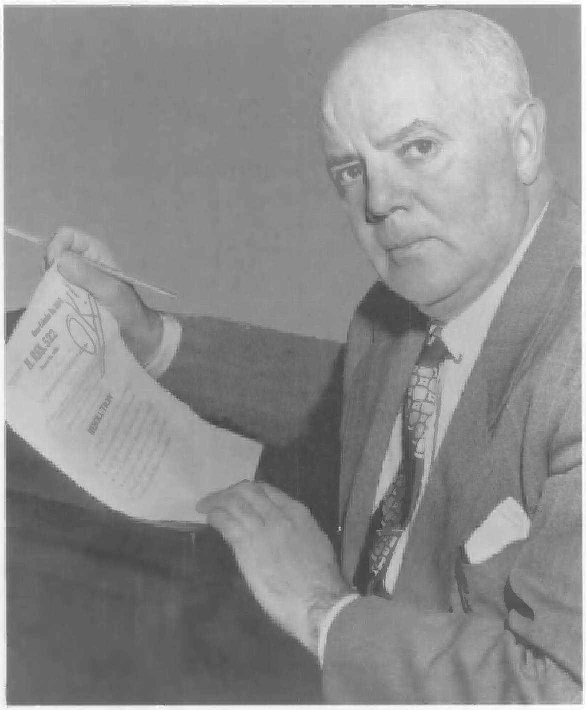
[48] House Committee on Un-American Activities, *Testimony of Dr. Edward U. Condon: Hearing Before the Committee on Un-American Activities*, 82nd Cong., 2d Sess., September 5, 1952: 3883. Hereafter this hearing will be referenced as "Condon Hearing."

[49] Holifield, *Sabotage of American Science*: 17.

[50] American Philosophical Society; Condon file; Security box 2; Folder Security Investigations, 1948, Public Reaction to Rep. J. P. Thomas' Attack #1.

150 leading American scientists, including eight Nobel Laureates, was held in New York on April 12.[51] Cartoonists had a field day. Condon did not remain silent. He issued a statement which included the sentence, "If it is true I am one of the weakest links in atomic security that is very gratifying and the country can feel absolutely safe for I am completely loyal, conscientious and devoted to the interests of my country, as my whole career and life clearly reveal."[52] He also renewed his request for a public hearing.

The continual conflict kept the story on the front page.[53] Thus, within a few hours of the release of the subcommittee's report, the Department of Commerce issued a statement that Condon had been given a loyalty clearance six days before. "No reasonable grounds exist for believing that Dr. Condon is disloyal to the Government of the United States," the statement declared.[54] The next day, Rep. Thomas issued a subpoena for the Hoover letter to Harriman and other parts of the FBI file on Condon. The



J. Parnell Thomas, chairman of the House Committee on Un-American Activities, posed for an Associated Press photographer with a copy of a resolution passed by the House on April 22, 1948. The resolution directed Secretary of Commerce Harriman to turn over a letter from the FBI that detailed Condon's association with an alleged spy. (AP-Wide World Photos)

[51] American Philosophical Society; Condon file; Teller box; Folder Testimonial dinner, April 1948.

[52] *New York Times*, March 2, 1948.

[53] A good chronology of newspaper stories between March 1 and October 1, 1948, is given by Joseph T. Klapper and Charles Y. Glock in "Trial by Newspaper," *Scientific American* 180(2) (1949): 16-21. The list, which is not exhaustive, contains 32 entries.

[54] Carr, *The House Committee*: 149.

following day Harriman refused. The same day, the *Washington Post* reported on the missing sentence. Late in March, the House had announced that it would hold a public hearing on April 21. On April 14, the hearing was postponed. It was not to be held until 1952, by which time the cast of characters had changed. Finally, on July 15, 1948, the AEC reached its conclusions on the Condon case:

> On the basis of the voluminous record before it, the members of the Commission are fully satisfied that, in the terms of the statute [Atomic Energy Act], Dr. Condon's continued clearance for the purposes stated above "will not adversely affect the common defense and security" of the United States. The Commission considers that his continued clearance is in the best interests of the atomic energy program.[55]

By 1949 the story had run its course and things had quieted down somewhat, but Condon still had the cloud of the report hanging over him. And like sporadic sniper fire, occasional shots would be fired by either side, but it was not until June 10, 1949, that a major outbreak occurred again from an unexpected quarter. The espionage trial of Judith Coplon was being held, and it was alleged that a number of notes referencing FBI files were found in her handbag. These files were made public at the trial. One of them alleged that Mrs. Condon was contacted by a certain Morton E. Kent, who was interested in selling inexpensive printing machines in Europe. Mrs. Condon allegedly gave him the name of a Bulgarian accused by the FBI as an espionage agent, implying that she knew him quite well. What made the whole incident news was that on June 11 Kent had committed suicide. The Condons' version of the story was that Mrs. Condon attended a church meeting called to raise funds for aid to devastated European schools. Dimitrov Sotirov, the Bulgarian in the files, spoke at the meeting. He was an employee of the United Nations, and this was the only time Mrs. Condon met him. All that happened was that Mrs. Condon told Kent, whom she met at a social gathering, how to reach Sotirov by telephone.[56] Because the material in the FBI files was, as usual with raw FBI files, "unevaluated" yet nevertheless released, Condon called for a public apology from FBI Director Hoover, and also wrote a personal letter to him. Hoover did not apologize, but he did write Condon a personal letter saying he had sent Condon's letter to the attorney general since the situation that prompted the letter was a judicial proceeding.

Whatever the case, the Condons were again in the newspaper linked to Eastern European nationals and alleged espionage agents. There seemed no way to get a hearing to clear their names.[57] And the matter was not kept closed. In early 1951,

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[55] Letter and memorandum of decision approved by the Atomic Energy Commission, David E. Lilienthal to Charles Sawyer, 15 July 1948. (NARA; RG 40; General Correspondence file; Box 1080; Folder 104462-104482 (104475))

[56] Draft statement by Mrs. Emilie Condon. (American Philosophical Society; Condon file; Security box 2; Folder Security Investigations, 1949, #2)

[57] Condon Hearing, 3871-3872. Actually, in June 1949, the House Committee on Un-American Activities, now reconstituted after the 1948 elections, invited Condon through the medium of a press release to appear if he would like to. Condon wrote a letter to the committee, apparently accepting the invitation, but never mailed it. When asked about this when the hearings were finally held, Condon stated, "It has been my policy all along not to come except under subpoena." He felt that invitations via the newspapers were too informal.

President Dwight D. Eisenhower met at the Summer White House in 1954 with Attorney General Herbert Brownell, Jr. (left) and FBI Director J. Edgar Hoover (right) to discuss an accelerated drive to "utterly destroy" the Communist Party. Hoover's anti-communist credentials had been established as far back as the Roosevelt Administration, and his red hunting gained momentum during the Truman and Eisenhower presidencies. (AP-Wide World Photos)

Representatives Richard B. Vail and Harold H. Velde attacked Condon on the floor of the House, recounting the by-now stale accusations and adding some new ones.

Throughout this whole time Condon received the support of the staff. Some believed that he and his wife were perhaps indiscreet in their associations, but certainly no one believed they were in any way disloyal. But, by 1951 McCarthyism was reaching its peak. Investigations for security clearances from the Department of Commerce became more stringent, and some staff members were fired. Lauriston Taylor, who was the AEC coordinator for the Bureau, and through whose office went all AEC contracts, classified papers and security matters, recalls having to fire three persons.[58] One of them was quite a tragic case. His wife was a writer and had belonged to a book club, "and it turned out to be one of those Communist cells," Taylor remembered.

> I don't recall now whether he knew that or not, but I'll tell you, of anybody I ever knew in my life that was anti-Communist, he was it. He just got plain fired. This crushed him to the point where it broke his health. He went to a sanitarium and had a recurrence of some earlier lung problems and died. His wife committed suicide. That's the story. It all tied into that security question.

[58] Conversation with Lauriston Taylor, April 10, 1990.

Taylor recalled two other cases of very able people who had to be fired, and in neither case were there any problems with the AEC. They had AEC clearance, but could not pass Commerce scrutiny. Similar accounts of two other persons are given by Jacob Rabinow.[59] One was another sad case. A patent attorney at the Bureau was reported to have said during the war that the Russians "were putting up a good fight." A hearing was held on his case, where Rabinow was a witness, and where, after some education of the committee by Rabinow on communism and what it was all about, the attorney was cleared. Nevertheless, the secretary of commerce immediately fired the attorney. He was devastated, but went into private practice and did quite well.

Equally troublesome was the problem of hiring. Because some potential applicants may have had some youthful peccadillos in their background, or perhaps simply because of the rigors of a loyalty and security investigation, many prospective candidates for positions would not even apply. Taylor recalls, "There were numerous cases of when you talked to people, just initial discussions about coming to the Bureau, and the security question would come up almost immediately. I've no way to document it, but I recall there were a number of cases where as soon as you got into the question, they sort of shrugged, 'Why get into this?' "[60]

This situation preyed on Condon. By the summer of 1951, his status with the Un-American Activities Committee looked as if it would never be resolved. McCarthyism was running rampant. On August 6, 1951, he wrote a four-page memorandum on loyalty and security procedures to President Harry S. Truman, with whom he had friendly relations.[61] The first page of the memo reads:

> Actual operation of loyalty and security programs within the government, in the atmosphere of suspicion and hate engendered by some members of Congress, is producing bad results:
>
> (1) Nervous strain, legal expense and virtual blacklisting of individuals on trivial and silly charges which ought never to be given serious consideration.
>
> (2) Especially in science, the bad name which the Government is getting as an employer, is intensifying the problem of recruiting men to work on urgent problems. We have a critical shortage of scientists anyway which is made worse by these abuses.
>
> (3) The Administration is harming itself politically by admitting by its official actions that these individuals deserved removal. regrettably [sic] some actions taken may have been necessary, but many have not been necessary by any reasonable standards, and yet each such removal can also be attacked as an instance of earlier carelessness in hiring such people.

[59] Memorandum, Jacob Rabinow to Elio Passaglia, "Some Recollections on the Security Problems of the Early '50s," November 17, 1989.

[60] Taylor conversation.

[61] Memorandum to The President from E. U. Condon, director, National Bureau of Standards, August 6, 1951. (American Philosophical Society; Condon file; Teller box; Folder Truman #3. Used with the permission of the American Philosophical Society.) It appears that this memorandum was never sent. The copy in the files is an original rather than a copy, and it has typographical errors that would not be present in a final version.

The memo then goes on to document five cases supporting the statements he made.

The most interesting thing about the memo is its existence. Apparently Condon felt deeply enough about these questions of loyalty and security, and surely about his own problems, that he wrote about them to the highest official in the land. That the memo was almost certainly never sent does not lessen its air of desperation. Condon appears to have reached a turning point.

Two days later he wrote a letter to the president, and this one was sent.[62] The first paragraph of the letter reads, "I hereby submit my resignation as Director of the National Bureau of Standards, the position to which you appointed me in November, 1945. I would like to suggest that this resignation be made effective on September 30, 1951." The letter goes on to give his reason for resigning: "My own reason for leaving the Federal service is one that is all too familiar to you: I can no longer afford to accept the severe financial sacrifice involved." The letter also points out the importance of scientific work in the Government, and warmly thanks the president for his support. Two days later President Truman accepted Condon's resignation effective September 30, and the deed was done. Condon had resigned from the Bureau. He became director of research of the Corning Glass Works, Corning, NY.

It is interesting to consider Condon's reasons for leaving the Bureau. The reason of finances is often one used by a person leaving the Federal service, yet it is only very rarely the only one. In Condon's case, the juxtaposition of his memo to President Truman and his letter of resignation could be viewed as evidence that his personal problems with the House Committee on Un-American Activities, added to the wave of McCarthyism sweeping the country about Communists in Government, led him to conclude that it would be better for the future of the Bureau if he were to leave. This is the belief of many staff members at the time, although very few have any firm evidence. One of those is Lauriston Taylor. Taylor's father and Condon were good friends, and the families were fairly close at times. Taylor remembers a discussion in which one of Condon's big concerns near the end of his directorship was security and loyalty. Taylor remembers him saying "How much longer can I continue to fight this thing and drag the Bureau around in it, and see the reputations of other people hurt?" "He was almost asking, 'How long can I do this?'" Taylor added.[63] Considering all the evidence, it appears that Condon's problems with the HUAC were major factors in his decision to leave the Bureau.

Condon was always certain that the reason he was attacked by the Thomas subcommittee was that he served as scientific advisor to the McMahon committee during the formation of the AEC. On January 3, 1953, he wrote to Dean George B. Pegram at Columbia University:[64]

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[62] Letter, E. U. Condon to President Truman, August 8, 1951. (American Philosophical Society; Condon file; Teller box; Folder Truman #3. Used with the permission of the American Philosophical Society.)

[63] Taylor conversation. Churchill Eisenhart recalled similar statements in an oral communication in 1990.

[64] Letter, E. U. Condon to George B. Pegram, January 3, 1953. (American Philosophical Society; Condon file; Security box 3; Folder Security Investigations, 1953, #2. Used with the permission of the American Philosophical Society.)

You know of course that the root of all my difficulty originates with active disagreement with General Groves and others about the nature of the legislation on atomic energy. I was fully cleared during the war for a wide variety of projects and remained in that status throughout my service in Washington despite the heavy and continuing political attacks on me with Groves' co-operation by the House Committee on Un-American Activities.

Lt. Gen. Leslie R. Groves, wartime head of the Army's atomic bomb project, was greeted by J. Parnell Thomas, chairman of the House Committee on Un-American Activities (HUAC), when he arrived to testify in a closed session in September 1948. Condon believed that his troubles with HUAC resulted from his disagreement with Groves and other members of the military about the proposed transfer of atomic energy to civilian control. (copyright *Washington Post;* reprinted by permission of D.C. Public Library)

Thus it appears that the battle over civilian control of atomic energy coupled with the Communist hunting frenzy of the postwar years, cost the Bureau its fourth director.

* * *

Condon's security problems by no means disappeared upon his leaving the Bureau. In September 1952, the hearings he had long wanted were held. In six grueling hours he was questioned about all aspects of his background as it pertained to security, and particularly with his left-wing associations. The same old stories were brought up, but there were new ones as well, some dating back to his wartime days at Berkeley. It was an arduous experience. He denied—and the committee did not prove—that he was ever a Communist, had ever consciously known one, or had ever violated security matters. He was not totally believed by some members of the committee. Condon felt positive about the outcome. "This could have been cleared up if it was done four years ago—but better late than never."[65] In the committee's annual report, Condon was declared to be unqualified for any position owing to his "propensity for associating with persons disloyal or of questionable loyalty and his contempt for necessary security regulations."[66]

Then, on November 28, 1952, the Army-Navy Personnel Security Board tentatively denied him clearance for his work at Corning. He was given the opportunity to submit written material in support of himself, and on February 7, 1953, Condon submitted a twenty-one page document in his defense.[67] His security clearance was denied on February 16, 1953, but he appealed and his clearance was re-instated in July 1954. Three months later, in October 1954, the secretary of the navy suspended the decision and his clearance was again removed. The *New York Times* reported that Vice President Richard M. Nixon took credit for reversing the decision,[68] and in December the secretary of the navy denied that the vice president had anything to do with his actions. Now feeling that he was a liability to Corning, Condon resigned to accept a position as visiting professor at the University of Pennsylvania, and in 1956 he was appointed head of the physics department at Washington University in St. Louis. From Washington University he went to the University of Colorado where, in May 1966, after some fourteen years of fighting, his clearance was finally approved at the secret level so that he could carry out a project at the Joint Institute for Laboratory Astrophysics, a scientific partnership between the Bureau and the University of Colorado. It was an ironic ending to Condon's sad saga.[69]

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[65] "Dr. Condon Declares He 'Cleared' Himself Before Red Probers," *The Washington Star*, September 6, 1952: A-2.

[66] Walter Goodman, *The Committee: The Extraordinary Career of the House Committee on Un-American Activities* (New York: Farrar, Straus, and Giroux, 1968): 252.

[67] Letter, Col. Harry W. Gorman to E. U. Condon, November 28, 1952. (American Philosophical Society; Condon file; Security box 3; Folder Security Investigations, 1953, #1); letter, E. U. Condon to Army-Navy-Air Force Personnel Security Board, February 7, 1953. (American Philosophical Society; Condon file; Security box 3; Folder Security Investigations, 1953, #3)

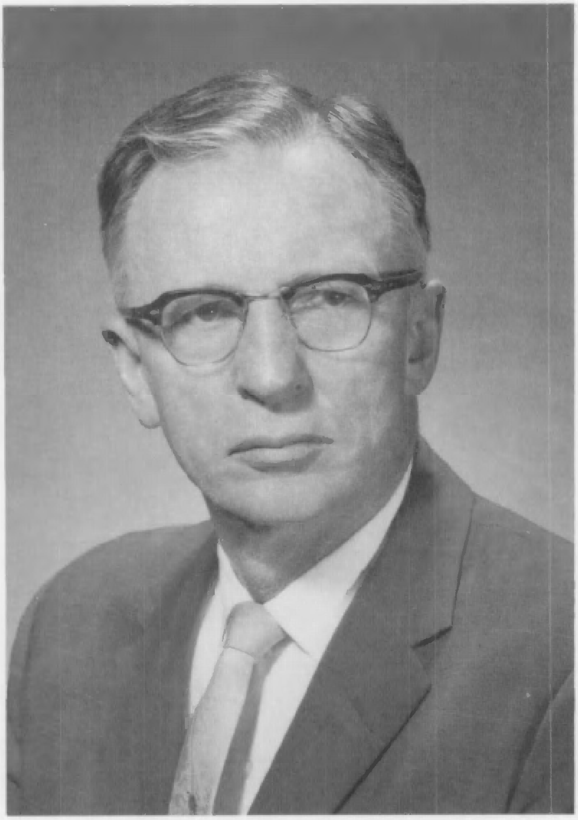
[68] Letter from E. U. Condon to Wayne Thornton, October 28, 1954. (American Philosophical Society; Condon file; Teller box; Folder Wayne H. Thornton)

[69] Some of this chronology comes from a letter Condon sent to C. P. Ives, Associate Editor of the *Baltimore Sun*, September 9, 1956. (American Philosophical Society; Condon file; Security box 3; Folder Security Investigations)

ALLEN VARLEY ASTIN

Upon Condon's departure from the Bureau on September 30, 1951, Allen V. Astin, who was then an associate director, was appointed acting director. The National Academy of Sciences, acting on a request from Secretary of Commerce Charles W. Sawyer, recommended three persons, two from outside the Bureau and Astin from within, as possible replacements for Condon.[70] Sawyer selected Astin and sent his name to President Truman who appointed him on May 20, 1952. Upon Senate confirmation, Astin became the Bureau's fifth director on June 12—his forty-eighth birthday.

Astin was a decidedly different man from Condon, in background, personality, training, and experience. Unlike Condon, he was not a newcomer, having been at the Bureau since 1930. He had not had an easy life. He was born in Salt Lake City, Utah,



As the fifth director of the National Bureau of Standards, Allen Varley Astin's primary goal was to make the Bureau an attractive place for scientists to work. Committed to excellence, he developed a sense of mission and defined a mission, reduced the dependence of the Bureau on transferred funds, and made the Bureau's own appropriation the primary source of financing at a sufficient level to carry out that mission.

[70] House Committee on Appropriations, Subcommittee on Departments of State, Justice, and Commerce, *Departments of State, Justice, and Commerce Appropriations for 1955: Hearings before the Subcommittee of the Committee on Appropriations*, 83d Cong., 2d sess., National Bureau of Standards, 11 January 1954: 76. Testimony of Dr. Mervin J. Kelly.

on June 12, 1904. His father, John Andrew Astin, a school teacher, died when Allen was four, leaving behind a poor family of one son and two younger daughters, and their mother Catherine. The mother had to work to support the family, and Allen helped out. Beginning at the early age of eight he consistently held odd jobs—carrying newspapers, working on berry farms, digging ditches, and similar tasks. Totally disciplined, he was able to save enough money to enter the University of Utah, where he studied physics. He was a campus leader, edited the school newspaper, and met and fell in love with Margaret L. Mackenzie, a student one year behind him and a talented writer. An excellent student, he won a scholarship at New York University (NYU) and, upon receiving his B.S. degree, left for New York in 1925. After receiving an M.S. degree, he returned to Salt Lake City and married Margaret. The young couple returned to NYU, where Margaret also became a student, studying journalism. Upon finishing his Ph.D. in 1928, Astin was awarded a National Research Council Fellowship at The Johns Hopkins University, and the young couple moved to Baltimore where, in 1930, their first son, John Allen, was born.

When his postdoctoral appointment came to an end in that same year, Astin had a thesis and postdoctoral work on the dielectric constant of electrolytes, a wife and child, but no job, and jobs were not easy to find in that early Great Depression year. After an interview at the Bureau, he was offered a position as a research associate of the National Research Council with funds provided by the Utilities Research Commission of Illinois. For two years he studied the dielectric behavior of pure materials, picking up considerable experience in electronics and becoming well-liked by the Bureau staff.

But in 1932 the commission money ran out and Astin faced the prospect of being without a job, a situation that was made more tense by the birth of his second son, Alexander William. He was offered a position at the Bureau studying aircraft ignition on a Navy project, and in 1932 he became a full-fledged civil servant. In the almost ten years before the onset of World War II, Astin worked in the fiscally stringent but congenial and relaxed atmosphere of Lyman J. Briggs' 1930s Bureau. During this period he made significant contributions to the science of telemetry, applying these techniques to weather balloons used for cosmic ray studies; made important studies in the precision measurement of capacitance; and became an expert in electronics. During the war he was asked to turn his attention to the proximity fuze, and he became totally immersed in the effort throughout the whole war period. In 1944 he was made assistant chief of the Ordnance Development Division under Harry Diamond, the Bureau's inventive genius and good friend of Astin. When Diamond died suddenly and unexpectedly in 1948, Astin was made chief of the division, now renamed the Electronics and Ordnance Division. In 1951 he was made associate director of the Bureau, in charge of coordinating transferred funds programs in the Electronics, Ordnance Development, and Missile Development Divisions, as well as the program of the new Office of Basic Instrumentation. This was the position he held when Condon retired, and from which he became acting director, and then director.

Condon and Astin were as different in personality as they were in upbringing. Astin was not trained in the new physics as was Condon, and would be the first to admit that he was not the scientist that Condon was, although he could do things in the laboratory that Condon could not. Nor did he have the gregarious, assertive personality of Condon. He was a friendly man, but low-key and rather reserved. He liked people, and people liked him because he listened and respected the opinions of others. A slender man with warm, friendly eyes, he never evinced anger, gave all questions serious consideration, and always appeared to be under the control of reason rather than emotion.[71] As a result his opinions were respected. Faced with an obstacle, his first reaction was not, like Condon's, to beat it down. He would try to finesse around it or wear it down, for one of his principal characteristics was tenacity. And while he may not have been on first-name terms with the world's leading scientists, he knew well the important actors on the Washington scientific scene, and understood the soft points of the bureaucracy. Outwardly calm and methodical, he did not lose sight of his goals and constantly worked toward them. Perhaps it can be said that Condon in his short six years got the Bureau started in the direction of modern science, and Astin took it there.

FULFILLING A REPORT

When Astin became acting director, the Bureau was embroiled in the battery additive episode. Although while this area had been outside his jurisdiction while he was associate director, he now took personal charge of it. As a result of that episode, in late 1953 the Bureau had a report—the Kelly Committee Report—which provided Astin with an agenda for his immediate and future actions as director.[72] Formed as an "Ad Hoc Committee for the Evaluation of the Present Functions and Operations of The National Bureau of Standards," the committee, chaired by Mervin J. Kelly, president of the Bell Telephone Laboratories, exhaustively investigated the Bureau and found an institution that was basically sound and of vital importance to the Nation. It had a "splendid record and tradition," was "staffed with professional men of competence, integrity and loyalty," and was needed more than ever as society became more technologically complex. But there were some significant problems.

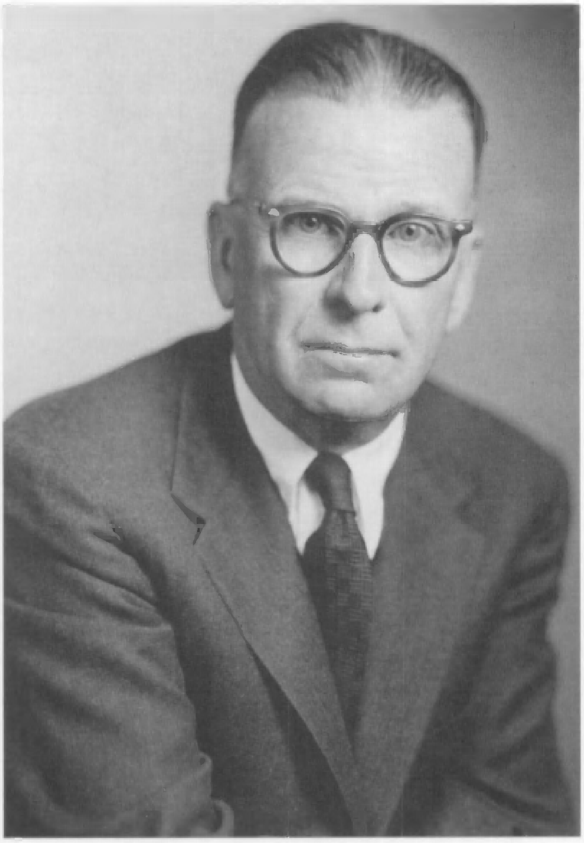
First, the committee found that the Bureau's basic programs—the programs carried out with appropriated funds, rather than transferred funds—were in serious difficulty. The committee wrote:

> Since the close of the war the technology of the nation has shot rapidly forward. The Bureau's basic programs expanded until 1950 but at a rate beneath that justified by the needs. Since 1950 the decrease in basic programs must be considered as tragic. The ground lost since 1950 should be regained in the next two fiscal years and the programs then expanded as detailed studies by the Director and his advisory committees find necessary."[73]

[71] Not that Astin could not get angry. His meeting with Laidler on July 29, 1952, should be recalled. Also, Astin had a whimsical side to his personality. Mrs. Astin tells the story of his having the children collect a jar full of live fireflies one summer evening. He then took the family to the local movie house, and during the performance released the fireflies. It made for an interesting audience reaction.

[72] *A Report to the Secretary of Commerce by the Ad Hoc Committee for the Evaluation of the Present Functions of the National Bureau of Standards: A Report on the Present Functions and Operations of the National Bureau of Standards With Their Evaluation in Relation to Present National Needs and Recommendations for the Improvement and Strengthening of the Bureau,* October 15, 1953. Mervin J. Kelly, chairman. Hereafter referred to as the Kelly Committee Report.

[73] Ibid., 20.



In the midst of the AD-X2 affair, Mervin J. Kelly, president of Bell Labs and a member of the NBS Visiting Committee, chaired the Ad Hoc Committee for the Evaluation of the Present Functions of the National Bureau of Standards. The report of the Kelly Committee had a profound impact on subsequent programs at the Bureau.

Not that the Bureau was poor. Indeed, it was the richest it had been in its history, but the problem was that most of the funds were transferred from the military agencies and the Atomic Energy Commission. Thus, in FY 1953, the year preceding the committee report, the Bureau received $7.4 million in appropriated funds, and $40.1 million in transferred funds, with $38.8 million, or 97 percent, of the transferred funds coming from the AEC and the military. Of the total expenditures of $47.5 million by the Bureau, 83 percent were for work carried out for the AEC and the military. For the whole postwar period transferred funds had exceeded the base appropriation, and had shot up dramatically since the start of the Korean War. At the same time the appropriated funds had decreased, dropping (in current dollars) from $8.7 million in 1950 to $7.4 million in 1953, while transferred funds rose from $ 11.3 million to $47.5 million.

The large amount and nature of the work with transferred funds had some deleterious effects on the Bureau's basic programs. The sheer magnitude of the effort was harmful in that it caused great crowding.[74] Moreover, the requirements for security

[74] In FY 1953, the Bureau had an authorized staff of 4781, by far the highest in its history.

engendered a large administrative organization, and "brought about secrecy, limited freedom of movement and other restrictions and has created an environment that is not best suited to the basic programs."[75] Moreover, the rapid expansion of the military work increased the rate of advancement in the military programs as compared to the basic programs, so that personnel were actually siphoned from the latter. The Bureau had become an appendage of the military, and it was a time of penury amid plenty.

The committee also found that the AEC work and some of the military work was different in character from the rest of the military work. The AEC work—$3 million in FY 1953—while often highly classified, was nevertheless laboratory work similar to the Bureau's work on its basic programs and often added to its capabilities; some of the military work fell into the same classification. This work was desirable and the committee felt that it should be continued. But the major bulk of the military work was concerned with the proximity fuze and guided missiles, and amounted to $20 million in FY 1953, almost three times the Bureau's base appropriation. This work was pure weaponry development, and the committee felt that, except in time of war, it was not appropriate for the Bureau.

As a result of its study, the committee made ten recommendations:[76]

1. Higher level of activity in the basic programs.[77]
2. Modernization of facilities and increased space for basic programs.
3. Improvement of organization at the associate director level.
4. Transfer of weaponry projects to the Department of Defense.
5. Continued use of the Bureau by Department of Defense and Atomic Energy Commission for non-weaponry science and technical aid.
6. Continued and increased use of the Bureau by other agencies of Government in indicated areas of science and technology.
7. Decrease in repetitive test operations at the Bureau.
8. Division of primary responsibility for policy and procedure on commercial product tests between the secretary of commerce and the director of the Bureau.
9. Increased support of standard samples program.
10. Advisory groups to the director selected from membership in eight scientific and technical societies.

The first of these recommendations was the most difficult to carry out, and discussion of it will be delayed. The second was also difficult and, while it was partly alleviated by the acquisition and occupation of a site in Boulder, Colorado, it was not to be carried out until the move of the Bureau to new facilities in Gaithersburg, Maryland, in the mid-sixties.

[75] Kelly Committee Report: 10.

[76] Ibid., 19.

[77] The word "basic" here is not used in the sense of basic (as distinct from "applied") research. Rather it refers to the basic functions of the Bureau as given in the Organic Act. Increases or decreases in the Bureau's appropriation add to or subtract from the Bureau's base appropriation which is the starting point for the yearly budget negotiations.

With respect to the third, the Bureau had four positions at the associate director level. There were associate directors for research, testing, ordnance development, and administration. The committee felt that, in the situation at the time the associate directors for the technical divisions functioned too much like "programmatic aides to the Director." This "ties the Director too closely to division supervision and places an unnecessary limit on the full use of the Associate Directors." Moreover, the position of associate director for research was too much for one person. It therefore recommended that the associate directors be given line responsibility "to the maximum extent possible," and that one more associate director be appointed. This was done immediately, and there were again four associate directors, in the areas of chemistry (Wallace R. Brode), physics (Robert D. Huntoon), testing (Archibald T. McPherson) and administration (Nicholas Golovin). Astin's old position of associate director for ordnance development was made unnecessary by the divestiture of the weapons work. However, removing the director from the direct administration of the divisions did not actually take place until the first complete reorganization of the Bureau in 1964, at which time the recommendation of the committee became moot.[78]

The fourth recommendation was accomplished in September 1953 with the transfer to the military of the three ordnance divisions and the missile development division, totalling 2000 persons. However, the amount of "other agency" work was so great that even in the fiscal year after this transfer, 76.6 percent of the Bureau's work was carried out on transferred funds. In that year the transfer of the military work was completed with the transfer of the Numerical Analysis Division in the Applied Mathematics Division to the University of California at Los Angeles. Known as the Institute for Numerical Analysis and located on the campus of UCLA, this division was totally supported by the navy and the air force. Nevertheless, the basic work would not exceed the transferred funds work until FY 1959.

Recommendations five and six are relatively obvious. The reasoning behind five has already been discussed, and six was because the committee felt that the Bureau did not do enough for agencies other than the military and the AEC.

While the Bureau performed an important and valuable service in developing methods of testing for various products, its talents were not well used in repetitive testing for purchase acceptance. The Bureau, following recommendation seven, did slowly decrease the amount of this work, but it would not be until the move to Gaithersburg that it would be discontinued. And the related recommendation eight was to give responsibility for the political aspects of product tests to the secretary of commerce, with the director retaining responsibility for the technical aspects. Clearly, this was meant to preclude recurrence of episodes like the battery additive controversy.

Recommendation nine was welcome, if routine, but number ten was a new departure. Under it, the scientific and engineering societies would appoint advisory committees in given areas such as metallurgy, chemistry, and mathematics. The committees would then visit the Bureau yearly, investigate the organizational units—usually a

[78] Kelly Committee Report: 5-6. Huntoon had been director of the Corona Missiles Laboratory and was brought back to Washington upon the divestiture of the weapons work.

division—responsible for that area, and advise the director on their findings about the program and its conduct.[79] This recommendation was adopted almost immediately, and from then on each division chief had to look forward to a yearly meeting with his peers who would report on his operation to his superior. It was designed to keep managers in touch with what was happening on the outside, and served a useful function. Gone, however, were the lax days of the old Bureau in which a division chief was essentially a lord in his fiefdom. This method of organizing advisory committees lasted until 1959 when the administration of the committees was taken over by the National Research Council.

But of all the recommendations, the first was the most important, and the hardest to carry out. It required that the appropriations from Congress be increased, and this was not happening. In fact, appropriations were decreasing.[80] In his first appearance before the House Appropriations Subcommittee in the spring of 1946—appropriations hearings for FY 1947—Condon won an increase from \$3.6 million to \$5.5 million.[81] This continued for the next two years, so that by FY 1949 the Bureau appropriation had increased to \$8.7 million. Then began an accelerating decrease, so that by FY 1952, Condon's last year, the appropriation had dropped to \$7.8 million. The next year—spring 1952—was Astin's first appearance before the committee as director. He tried a new approach. He tried to link the demands on the Bureau to the total number of scientists in the country, using the membership in scientific societies as an index, and arguing that this had grown much more rapidly than the Bureau's appropriation. The appropriation should rise at a comparable rate, he argued, and asked for a modest increase of \$0.9 million. To say that the committee was unimpressed would be euphemistic. In fact, they were shocked. Chairman John Rooney of New York, with whom Astin was to cross swords for many years, looked upon the Bureau's position as a statement that the Bureau should hire a constant fraction of the scientists in the Nation. Rooney was emphatic:

> Dr. Astin, as far as I am concerned, you are just wasting your time and somebody has wasted a good deal of time in trying to put over an argument such as this, as to why personnel of the National Bureau of Standards should be increased. . . . As far as I am concerned, I am not going to appropriate any of the taxpayers' money based upon an argument such as this. Not one cent of it.

[79] The organizations represented were the American Institute of Electrical Engineers, the Institute of Radio Engineers, the American Institute of Physics, the Policy Committee for Mathematics, the American Institute of Mining and Metallurgical Engineers, the American Chemical Society, the American Ceramic Society, the American Society of Mechanical Engineers, and the National Conference on Weights and Measures. Later the American Standards Association and the American Society for Testing and Materials were added.

[80] A graph of the Bureau's appropriation over time is given in Appendix E.

[81] When appropriation figures are cited here, the appropriations for Plant and Construction are not included. These are one-time actions and do not add to the Bureau's base.

But Rooney closed the hearings on a somewhat conciliatory note:

> In your first appearance before this committee . . . you started off with what might be called an unfortunate detail. However, now that you have concluded, I believe we all feel you have made a very interesting and fair presentation of the problems of the National Bureau of Standards."[82]

The Bureau received a cut of $380 000. The following year (FY 1954, hearings in spring 1953) was the first Republican Congress that accompanied the Eisenhower victory, and it was a budgetary disaster. The Bureau's appropriation dropped a full $1.7 million (23 percent) to $5.7 million, its lowest point since 1947.

But the next year things began to change. The AD-X2 episode had now passed, and Astin was in a strong position. The Kelly Committee Report had been received, the transfer of weapons work had been made, and the Department of Commerce strongly supported the report and the Bureau's request for increases to strengthen its basic work. Secretary Sinclair Weeks used the Kelly Committee Report in his overall justification, Under Secretary Walter Williams presented and supported a 26 percent increase in the Bureau's appropriation, and perhaps most important, Mervin Kelly himself appeared before the committee and made a very strong impression.[83] There was, in the end, an increase of only 6 percent, but at least the string of decreases had been halted. Astin did not give up. He patiently but doggedly explained that the Bureau's appropriation should be increased because it could not now provide the services asked of it by science, industry, and commerce. The next year the increase was more substantial, amounting to 22 percent, and by 1957 the appropriation was $8.4 million, almost as much as in 1949 and 1950.

Moreover, in 1956 an important change was made in the method of financing the Bureau. Before this, when the Bureau sold standard samples, or charged for calibrations or reimbursable administrative services, it did not receive the proceeds. Instead, these were sent to the Treasury. Thus the Bureau spent a part of its appropriation to perform these reimbursable services, but received none of the proceeds. In theory, funds for these functions were provided in the base appropriation, but it was not easy to coordinate income and outgo. In November 1956 a change embodied in Public Law 84-940, was made so that the Bureau was permitted to receive the income from its sales and services.[84]

It was not a small matter. In 1957 the receipts amounted to $2.8 million, and part of the substantial increase in funds between 1956 and 1957 was due to this financing change. Indeed, the Appropriation Committee increased the appropriation by only

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[82] House Committee on Appropriations, Subcommittee on Departments of State, Justice, Commerce, and the Judiciary, *Departments of State, Justice, Commerce, and the Judiciary Appropriations for 1953: Hearings Before the Subcommittee of the Committee on Appropriations*, 82d Cong., 2d sess., National Bureau of Standards, 18 January 1952: 432, 464.

[83] Appropriations Hearings for 1955: 5-6, 71-72, 75-82.

[84] Annual Report, 1956: 108; *AN ACT To amend the Act of March 3, 1901 (31 Stat. 1449) as amended, to incorporate in the Organic Act of the National Bureau of Standards the authority to use the Working Capital Fund, and to permit certain improvements in fiscal practices, U.S. Statutes at Large*, 70 (1956): 959.

$1 million, but the funds actually available to the Bureau increased by $3.8 million. It is highly doubtful that the committee would have provided this increase without the accounting change. In FY 1958, the year of the sputniks, the total available was $12 million, an increase of 112 percent over 1954, Astin's worst year, but a much more modest 38 percent over its 1949 appropriation.[85] While the Bureau's total available funds—including transferred funds—had more than kept up with the national expansion in scientific research and development, its base appropriation, with which it maintained the leadership required in its unique position as the Nation's measurement standards laboratory, had fallen far short of requirements.

It is tempting to speculate on the reasons for the Bureau's steep drop in appropriations in the early years of the 1950s.[86] The economy-minded Eisenhower administration can be blamed for part of this, but the decline had started before the 1952 elections. At least a part of this must be laid to the inordinate amounts of transferred funds for weapons development the Korean War brought to the Bureau, as well as work required by the infant AEC. From the point of view of a Congressman, the Bureau was not a poor institution. It was, in fact, rich, if not fat. If it could not get money from the Congress it presumably could always contact one of its friends in the military. Representative Prince H. Preston of Georgia put it succinctly:

> If we were to try to apply some economies . . . there would be nothing in the world to prevent the Bureau of Standards from doing a little staff negotiation with Navy, or somebody, and saying, 'Look, fellows, come to our rescue'. . . . The Navy would say, 'All right, we will give you a project'. . . . You would just be going to some other source to get the money we denied. I do not know what the answer is.[87]

And Preston made the point more clearly in 1956:[88]

> MR. PRESTON: Is it a fact, doctor, that had you not performed this work for the agencies of the Government, that your direct appropriation would have been materially larger?
>
> DR. ASTIN: Well, that is a difficult question. It might have been larger had we not had so many programs from other Government agencies.

[85] While the Bureau's base appropriation was not all for basic research, it is instructive to compare its growth with that of basic research expenditures in the Nation. Between 1953 and 1958, total national expenditures for basic research (in current dollars) increased by 100 percent, while total Federal Government basic research expenditures increased by 75 percent. Government in-house basic research expenditures, however, increased by only 25 percent over the same period. (Figures from the National Science Foundation, 1989.)

[86] The Bureau was not alone in experiencing a decline in research funds. Federal Government in-house expenditures for basic research showed a drop of 11 percent from 1954 to 1955. From 1953 to 1956, these expenditures increased by 3 percent, while the Bureau's appropriation was essentially unchanged.

[87] Appropriation Hearing for 1955: 91.

[88] House Committee on Appropriations, Subcommittee on Department of Commerce and Related Agencies, *Department of Commerce and Related Agencies Appropriations for 1957: Hearings Before a Subcommittee of the Committee on Appropriations for 1957*, 84th Cong., 2d sess., National Bureau of Standards, 20 March 1956: 101.

Subtle distinctions between basic measurement research and research for other agencies were hard to assimilate. Apparently it took the dramatic events of the AD-X2 incident, the Kelly Committee Report, and the consequent divestiture of the weapons work, to bring home to the Congress that the Bureau required support for its own unique mission. No amount of transferred funds would compensate for this lack of basic support, for inevitably and properly, other agencies were interested in carrying out their own programs, not those of the Bureau.

During the early fifties there were changes in how the Bureau carried out its accounting and in its method of presentation of its budget to the Congress.[89] The whole matter was complicated by transferred funds. Before FY 1956, the budget request was made in three line items: operation and administration (most of which, but not all, represented overhead), research and testing, and radio propagation and standards. The last was added to the budget request in 1949, two years after the Interservice Radio Propagation Laboratory was changed to the Central Radio Propagation Laboratory (CRPL) and was no longer a purely military function. When necessary, one-time items such as construction of laboratories were added. In 1950, there was formed a Working Capital Fund to put the Bureau's accounting system on a businesslike basis.[90] Along with this accounting system was a project system—installed in 1949 under pressure from the Congress—by means of which the costs of individual research projects could be calculated.[91] At the highest level the project system had five categories: fundamental research; applied research; development; testing, calibration and specifications; and general scientific services. There were nineteen second-level categories.

The system functioned by putting all funds into a Working Capital Fund and making all charges to this fund. In accord with customary accounting practice, it was recognized that administrative support activities were of general benefit to all the technical projects, hence these activities were considered as overhead. Their costs were distributed to the technical projects on the basis of the technical labor in the project. This then gave an accurate accounting of the costs of a technical project and formed a good cost accounting system.

Trouble arose with the operation and administration appropriation. If this was insufficient to cover the overhead costs, then money would have to be transferred from other line items, which represented technical work, to the overhead function. This would mean using funds appropriated for one function for another, and would be illegal. In the Bureau's case, the situation was complicated—and salvaged—by the presence of transferred funds. They did not have a stipulated amount of overhead,

[89] House Committee on Appropriations, Subcommittee on Department of Commerce and Related Agencies, *Department of Commerce and Related Agencies Appropriations for 1956: Hearings Before a Subcommittee of the Committee on Appropriations*, 84th Cong., 1st sess., National Bureau of Standards, 20 April 1955: 154-165. Astin's budget presentation gives the best description of these changes and procedures.

[90] *Deficiency Appropriation Act, 1950, U.S. Statutes at Large*, 64 (1950): 279.

[91] In 1952 the Bureau had 322 projects under its own appropriation, plus 350 unclassified projects carried out with transferred funds. There were, in addition, approximately 350 classified projects, all under transferred funds.

and saved the day. When all administrative costs for the Bureau were added up, they amounted to 31 percent of total expenditures. However, the portion of the appropriation for operation and administration that was allotted for overhead amounted to only 18 percent of the base appropriation. The Bureau thus developed the habit of charging the other agencies 40 percent overhead to make up the difference between the Bureau's appropriation and what it actually spent in overhead on its base program. In 1955, the comptroller general ruled that this way of operating was not legal, and the Bureau changed its manner of making budget requests. It did away completely with the line item on Operation and Administration. As long as this change was being made, the Bureau also eliminated the separate presentation for Radio Propagation and Standards. Hence the budget was presented according to the highest level of the Bureau's project structure, namely the categories of research; development; testing, calibration, and specifications; and general technical services. Construction and related expenses were carried as separate items. This restored legality and simplified both the presentation and the interpretation of the expenditures.

By FY 1958 the Bureau was beginning to show some of the effects of the changes brought about as a result of the Kelly Committee Report.[92] Most importantly, the fraction of transferred funds had been brought down to 58 percent, and in the next fiscal year the fraction would be lower than 50 percent for the first time since before World War II. The authorized staff had dropped from its high of 4781 in 1953 to 3200. And among the staff were a number of new hires who would have important roles in the future of the Bureau as division chiefs, and two who would become directors.

The divisions of the Bureau remained much the same. The three ordnance divisions and the missile development division were gone. The Electricity Division was combined with the Electronics Division, forming Electricity and Electronics. The Heat and Power Division became Heat, and Power disappeared. The CRPL was moved to the Bureau's new installation in Boulder, Colorado, and metamorphosed into three divisions: Radio Propagation Physics, Radio Propagation Engineering, and Radio Standards. And at Boulder there was a whole new division, Cryogenic Engineering.

By the end of FY 1958, approximately six months after the sputniks, the Bureau was well on the way to having reconstituted itself as the Nation's measurement standards laboratory. *Sputnik I* and *Sputnik II* would hasten the process.

[92] New criteria were adopted for the acceptance of other agency projects. Such projects were to "(1) have a close relationship to basic functions of NBS, (2) show prospect of producing results of general value to science, or (3) cannot be undertaken effectively elsewhere and do not require large changes in NBS staff. The application of these policies has resulted in discontinuance or significant decrease in a number of other agency projects." The excerpt is contained in a document entitled "Analysis of Program Conversion." The document, which consists of one page of text and seven of figures and charts, is unaddressed and undated and appears to have been an analysis to be used at a division chiefs' meeting with the Director. (NARA; RG167; Astin file; Box 20; Folder Corresp. 1958)

The Acquisition of the Boulder Site, and a New Program in Cryogenic Engineering

In 1948, with about 3000 staff members on the Van Ness site, the Bureau was feeling crowded. Moreover, the conditions in the city were not suited for the work carried out in the Central Radio Propagation Laboratory, nor for guided missiles. The large amount of radio traffic in the surrounding city caused serious interference problems; the lack of an unobstructed horizon hampered the study of line-of-sight microwave propagation; and new frequency ranges became important in the postwar world of FM, television, and an ever-greater volume of communications. Equally important in influencing a move out of Washington was the government fear of an atomic bomb attack, which led to an effort to locate new facilities out of the city. Beginning in the late forties this effort culminated in a dispersal order by President Truman, which effectively precluded any expansion moves within Washington and eventually led to the move of all Bureau activities remaining at the Van Ness site to new quarters in Gaithersburg, Maryland. Thus, then-Director Condon sought new sites for the radio and missile work and asked the Senate Interstate Commerce Committee—which had jurisdiction over the Bureau, and whose chairman was Edwin C. Johnson of Colorado—for authority to build new facilities to house this work and to purchase land for them if necessary.

In October 1949, the Congress authorized $4.5 million for "the construction and equipment of a radio laboratory building for the National Bureau of Standards" and $1.9 million for a guided-missile research laboratory.[93] In both cases authorization to acquire land was also granted. With this authority the Bureau began investigating possible sites for the two new laboratories. The guided-missile laboratory was rather easily established on a former naval hospital site in Corona, California,[94] and was transferred to the navy in the 1953 divestiture. The settlement of the radio laboratory was more complex.

As stated by Condon,[95] there were two principal criteria for a satisfactory site on which to conduct both radio propagation and radio standards research; it must be "radio quiet" and be so located that long-distance, line-of-sight transmission was possible.[96] Accessibility and proximity to a university with strength in electrical

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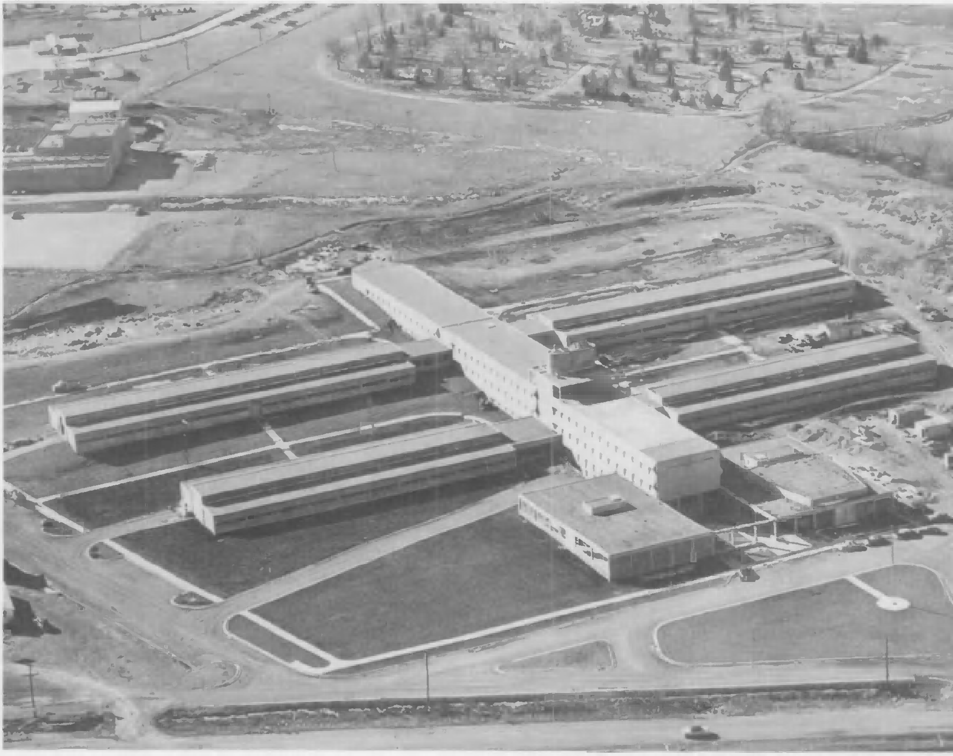
[93] *AN ACT To authorize the construction and equipment of a radio laboratory building for the National Bureau of Standards, Department of Commerce, U.S. Statutes at Large,* 63 (1949): 886; *AN ACT To authorize the construction and equipment of a guided-missile research laboratory building for the National Bureau of Standards, Department of Commerce, U.S. Statutes at Large,* 63 (1949): 905.

[94] MFP, 445.

[95] Conversation with Bascom W. Birmingham, September 17, 1990.

[96] Wilbert F. Snyder, 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: 526. It was for this latter reason that in 1950 an experimental transmitting tower was located at Cheyenne Mountain, near Colorado Springs, thereby effectively covering the Great Plains.



Air view of the NBS research center in Boulder, Colorado, taken in 1954. Upon completion, the structure in the foreground housed the Central Radio Propagation Laboratory. The buildings in the background (upper left) accommodated the NBS-AEC Cryogenic Engineering Laboratory.

engineering were also important considerations. Twenty-eight sites received consideration, all of them university towns,[97] but three of them stood out above the others: Boulder, Colorado, Charlottesville, Virginia, and Palo Alto, California.

With the news of its new authorization becoming quickly known, the Bureau was widely courted by places that would be prospective homes. But one city in particular—Boulder, Colorado—began an aggressive campaign to attract the Bureau and its projected $2 million annual payroll. Indeed, even before the enactment of the authorization bill, Senator Johnson kept Boulder well aware of what was taking place.[98] And on the very day the bill was signed into law, Mr. Francis W. Reich, secretary-manager of the Boulder Chamber of Commerce, returned from Washington, "where he

[97] Newbern Smith, Kenneth A. Norton, Alvin G. McNish, and S. W. J. Welch, "Selection of Site for Proposed Radio Propagation Laboratory Building," December 12, 1949. The authors constituted the Bureau's Site Selection Board.

[98] "$1,500,000 Electronics Research Lab of Bureau of Standards May be Moved Here," *Boulder Daily Camera*, August 22, 1949: 1.

had been sent to push Boulder's qualifications," and had spent considerable time briefing Condon and other Bureau officials on the merits of Boulder.[99]

Adjacent to its southern boundary, but outside the city proper, there was a tract of land, mostly small farms, of somewhat over 200 acres. Immediately on Reich's return, he met with the Chamber of Commerce, which voted "to leave no stone unturned" to ensure that the Bureau laboratories would be attracted to Boulder. The Chamber thereupon took an option to purchase the tract, and one week later offered the site to the Government.[100] It was to prove a strong inducement.

While these various activities were taking place in Boulder, the Bureau's selection committee was meeting to decide on which site to recommend to the director. They selected three as suitable, and recommended that Boulder be chosen.[101] Part of the recommendation was that "A suitable tract of land . . . has been offered." Condon accepted the recommendation, and on December 12, 1949, Secretary of Commerce Charles Sawyer announced the selection, stating that construction would start in 1951 "on land donated by the Boulder Chamber of Commerce." There was jubilation in Boulder, and a congratulatory telegram from Senator Johnson to the Chamber of Commerce.

The Chamber was in a peculiar position. It had offered free to the U.S. Government a site it did not own, and on February 27, 1950, it began a campaign to raise the estimated $70 000 needed to purchase it. With full-page ads laying out the advantages of the laboratories to Boulder, the campaign was a resounding success, and by April 25, 1950, had raised $90 407. Boulder's plan to entice the Bureau had succeeded, and on June 14, 1950, in a ceremony in the Chamber of Commerce office, title to the 217-acre site was transferred to the Government. Now all that was necessary was that the Bureau's appropriation committees cooperate and provide the money to start the construction. The committees did cooperate and, after adjustment in conference, the appropriation bill—signed into law on September 6, 1950—provided for $360 000 cash for engineering and design, and $3.9 million in contract authority, for a total of $4.3 million for the radio laboratory. In addition the Bureau received $140 000 for design, and $1.8 million in contract authority for modification of the naval hospital in Corona, California, into a guided-missiles laboratory. Work could now begin on the Boulder site, and the architectural firm of Pereira & Luckman of Los Angeles, and

[99] "Highlights of the History of NBS Move to Boulder From First Story of Possibility to Official Program," *Boulder Daily Camera*, September 10, 1954: 18. This was a special section of the newspaper, issued for the dedication of the Boulder Laboratories, commemorating the Bureau's move to Boulder.

[100] "Boulder Has Good Chance of Getting Big Laboratory," *Boulder Daily Camera*, October 25, 1949: 1; "Site for Bureau of Standards Laboratory Offered to Meet Any Competition of Other Cities," *Boulder Daily Camera*, November 2, 1949: 7.

[101] There are persistent rumors that Boulder was Condon's choice from the beginning, and this had some influence on the recommendation.

architect Robert Dietzen of Boulder were retained for the design of the radio laboratories.[102] The Bureau was on its way to having a permanent outpost at the edge of the Rocky Mountains.

But although the Boulder site had been acquired for the building of a laboratory to house the Bureau's radio research activities, the radio laboratory was not to be the first occupant of the site. Rather, the first facility was to be a cryogenic engineering laboratory for the production of liquid hydrogen.

The impetus for this laboratory came from national defense considerations. When President Truman announced at the beginning of 1950 that the Nation would build a hydrogen bomb, nicknamed "Super," the Atomic Energy Commission began a crash program.[103] Work on the Super had been going on at Los Alamos since 1942, and it had been realized from the beginning that the light elements were the most useful for fusion. This meant hydrogen and its isotopes. Also, it was realized that the fusion of two protons is qualitatively different from and much slower than either deuteron fusion or the fusion of a deuteron and a triton, and the problem with the latter reaction was that of obtaining sufficient quantities of tritium. Furthermore, tritium was highly radioactive. Attention was therefore focused on the deuteron fusion reaction. It was also realized at the beginning of this work that the higher the density of the reacting species the faster the reaction rate, hence *liquid deuterium* would automatically become the preferred initial state for the "fuel" in a first test. Thus Los Alamos had entered into cryogenics research, and by April 1944, a 35 liter/hour (L/hr) hydrogen liquefier had been built and tested.

Now, in 1950 with a crash program following the president's directive, the early Los Alamos work was re-examined, and it became clear that a cryogenic facility with "gas liquefaction plants and laboratories for engineering research and development at liquid hydrogen temperatures" was necessary. After looking at the various institutions that could carry such an effort, the AEC selected the Bureau to carry it out at "its newly acquired Boulder, Colorado, site," and a contract was entered into between the Bureau and the AEC.[104] Design work began under Ferdinand Brickwedde, Russell B. Scott, William E. Gifford, and Victor J. Johnson, and this group quickly expanded to

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[102] House Committee on Appropriations. Subcommittee on the Department of Commerce. *Department of Commerce Appropriations for 1952: Hearings Before the Subcommittee of the Committee on Appropriations,* 82d Cong., 1st sess., National Bureau of Standards, 10 April 1951: 501.

[103] F. G. Brickwedde, E. F. Hammel, and W. E. Keller, "The History of Cryogenics in the USA": 14-15,16, 19. Much of the early history of the liquid hydrogen production effort comes from this unpublished but definitive 1990 report. At the time in question, Brickwedde was chief of the Heat and Power Division in which all the Bureau's cryogenic research was carried out. Hammel and Keller were leaders of the Los Alamos National Laboratory Low Temperature Physics and Cryoengineering Group. We are much indebted to Bascom W. Birmingham for providing us with a copy of this report. This report was later published as chapter 11 of the *History and Origins of Cryogenics*, edited by R. Scurlock (Oxford University Press, 1992).

[104] Letter, E. U. Condon to Norris E. Bradbury, Los Alamos Scientific Laboratory, June 12, 1950. (NARA; RG 167; Records of the Director, 1923-63, Director's Correspondence file; Box 18; Folder Cryogenic Engineering, Laboratory for)

include Dudley B. Chelton, Bascom W. Birmingham, Richard H. Kropschot, Peter C. Van Arend, Robert B. Jacobs, and Robert L. Powell. A hydrogen liquefier with a nominal capacity of 350 L/hr was built and tested at the Van Ness site, and disassembled and shipped to Boulder.

Construction of buildings to house the cryogenic engineering program at the Boulder site began in 1951, and Birmingham was the first full-time employee in the program to arrive on the site in Boulder. A fresh recipient of a master's degree from the Massachusetts Institute of Technology, he was hired by Scott, arrived at the Van Ness site in late summer of 1951, and stayed one month. The rest of the cryogenic staff were experts in low temperatures, but were more familiar with laboratory equipment and operation than they were with the process engineering required for this large-scale effort. Birmingham, on the other hand, had such experience from previous employment, so it was only natural that he be the first to arrive in Boulder, which he did on October 9, 1951.[105]

By March 1952 the plant was in operation and, with a capacity of 320 L/hr, was then the world's largest liquid-hydrogen plant. Its hydrogen liquefiers and purifiers were in duplicate, making continuous operation much more likely. The liquefiers and purifiers were supplied with liquid nitrogen from two 10 000 L storage containers, which in turn were supplied by two commercial liquid-nitrogen generators, each capable of producing 250 L/hr. All of this was housed in a building of 14 000 square feet, with all necessary safety equipment and design. A separate building had 20 000 square feet for laboratory work, and the whole operation was denoted as the Cryogenic Engineering Section, with Scott as chief.[106] It became the Cryogenic Engineering Division in 1954.

It is interesting to note that the principal product for the AEC, namely liquid deuterium, was not produced by fractional distillation of the liquid hydrogen. Instead, it was produced by simply cooling gaseous deuterium—produced by electrolysis of heavy water—with liquid hydrogen. Since the boiling point of liquid deuterium is about 3 kelvins higher than that of liquid hydrogen, its condensation with liquid hydrogen is easily accomplished.[107]

Liquid deuterium from the laboratories was used as fuel in successful thermonuclear devices tested at Eniwetok Atoll in 1952. Before the tests could be conducted, a great deal of engineering was needed in such areas as transport dewars, *ortho*-to-*para* hydrogen conversion, transfer lines, improved insulation, properties of materials at low temperatures, seals, improved insulation, and others. Most of this engineering was done in the Cryogenic Engineering Laboratory. But it was clear that a final hydrogen bomb would not be fueled by liquid deuterium, yielding a so-called "wet" bomb. Rather, it would be the light compound lithium deuteride (LiD), and in 1954, tests showed that this "dry" bomb worked. Thereupon the AEC shut down completely the

[105] Interview with Bascom W. Birmingham, May 11, 1987: 2. NIST Oral History File.

[106] Annual Report, 1952: 14.

[107] In 1955, the Cryogenic Engineering Division began to develop a process for the fractional distillation of liquid hydrogen for the recovery of liquid deuterium.

Large reciprocating vacuum pumps in the hydrogen liquefaction plant of the NBS-AEC Cryogenic Engineering Laboratory. These pumps were used to reduce the boiling pressure—and hence the temperature—within the liquid-nitrogen precooler. Dean B. Berky is shown opening the vacuum breaker on the first of the three vacuum pumps.

"cryoengineering component of the U.S. thermonuclear weapons development program."[108] But before long the AEC needs would be supplanted by rocketry needs, therefore the Cryogenic Engineering Section, now the Cryogenic Engineering Division under Russell B. Scott, developed its own program.

Working on liquid-oxygen transfer systems, the flow of liquefied gases, the development of pumps for liquefied gases, the low-temperature properties of materials and, of course, the liquefaction of gases, the division had a well-rounded program in cryogenic engineering. One of its outstanding achievements—in cooperation with the University of California Radiation Laboratory—was the construction of a 500 L liquid hydrogen bubble chamber for use in high-energy nuclear research. Cryogenic engineering at the Bureau was well established.

While the Cryogenic Engineering Laboratory was under construction and then in operation, work on the radio laboratory did not cease. Following the appropriation of funds, design work began in the summer of 1951. Indeed, the Bureau sent out an "advance

[108] Brickwedde, et al., "History of Cryogenics in the USA": 19.

guard" that summer to lay "the groundwork for the technical program and some of the field facilities which will be needed when we move." These personnel were housed in the Colorado National Guard Radar Armory just north of the city, and by January 1952 the Bureau had ninety-one staff members in Colorado, including those conducting tropospheric propagation experiments from Cheyenne Mountain near Colorado Springs.[109] A contract for the construction of the laboratory was awarded to the Olson Construction Company of Denver, and work began in late June 1952.[110] The laboratories were completed in March 1954, and the moves from the armory and from Washington to the new quarters began. When Frederick W. Brown, recently hired from the Naval Ordnance Test Station at China Lake, California, arrived as director on July 1, the Boulder Laboratories were established.

Except for some of the personnel from Washington who were unhappy about moving to Colorado, everyone was pleased with the new laboratories. The Chamber of Commerce sponsored a "Good Neighbor" trip to Washington where, on April 12, the party of thirty-six members met with Astin and Ralph J. Slutz, assistant director of CRPL, toured the laboratories, and met with many of the staff who were to be assigned to Boulder.

A dedication week was held September 8-14, 1954. Two full-scale scientific conferences—one on cryogenic engineering and the other on radio propagation—took place in the new laboratories as part of the festivities. But all other ceremonies were overshadowed by the dedication proper. With great praise for the Bureau, this was performed by President Eisenhower on September 14, 1954. In the terminology of the conference program, the Bureau had a "second principal campus."

Postdocs Come to the Bureau

Joseph Hilsenrath joined the Bureau in 1948 under a unique joint appointment. In the Heat and Power Division, he was to supervise a project on thermodynamic tables, and in the Personnel Division, he was to develop a postgraduate training program for the NBS staff. In 1952 he entered into a conversation with David E. Mann, who had come to the Heat and Power Division as a spectroscopist in 1950 after postdoctoral fellowships at the University of Minnesota and at Harvard. Mann deplored the fact that the Civil Service system "was not geared to accommodate a transient population of postdoctoral fellows."[111] Because of his joint appointment, Hilsenrath knew about these matters and pointed out that there was a mechanism sanctioned by the Civil Service Commission (CSC) by which persons could be brought to work at the Bureau without going through a competitive appointment. Called Schedule A positions, these

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[109] Appropriations Hearing for 1953: 452; Snyder and Bragaw, *Achievement in Radio*: 525.

[110] *Boulder Daily Camera*, "C. U. Man Named Consultant for Standards Bureau," May 22, 1951: 2; "Construction of Cryogenics Lab Nearly Finished," June 25, 1952: 2; "Excavation for Radio Laboratory to Start Monday," June 28, 1952: 4.

[111] Joseph Hilsenrath, *The National Academy of Sciences-National Research Council's Postdoctoral Research: An Account of Its Origin and Early History at the National Bureau of Standards*. Natl. Bur. Stand. (U.S.) NBS/GCR-85/500; September 1985: 4.

President Dwight D. Eisenhower stepped down into a lobby of the new Radio Building of the NBS Boulder Laboratories, accompanied by (left to right) Governor Dan Thornton of Colorado; Allen V. Astin, director of the National Bureau of Standards; Frederick W. Brown, director of the Boulder Laboratories; and Secretary of Commerce Sinclair Weeks.

positions were, however, limited to (1) university faculty for a maximum of 120 days a year, (2) qualified consultants for a maximum of 180 days a year, and (3) graduate students at an accredited college or university. Perhaps this authority could be extended to postdoctoral fellows.

Both Hilsenrath and Mann were convinced that there were places in the Bureau where high-quality doctoral recipients would find the atmosphere and facilities necessary for them to effectively continue their research training, and a constantly renewed population of fresh, young minds would be of inestimable value to the Bureau. Both men felt that the program should be modeled after the National Research Fellowships funded by the Rockefeller Foundation and administered by the National Research Council, and if the NBS fellowships could be made into NRC Fellowships, they would gain great prestige and attract high-quality students.

Although no longer attached to the Personnel Division, Hilsenrath was a member of the Bureau's Education Committee and began to explore the possibilities. He contacted Personnel, which in turn called the CSC to see if there was any interest in, or antagonism to, such a proposed program. After receiving an encouraging response, Hilsenrath approached several division chiefs to see if their interest extended to providing funds. Four did—Heat and Power, Chemistry, Atomic Physics, and Applied Mathematics.

Approval was readily obtained from the Bureau administration to continue the exploration and the next step was to approach the NRC to determine if it had any interest in the matter. Now serendipity took over. In January and February 1953, the Bureau representatives found the NRC not only willing, but anxious to make an arrangement. The approach by the Bureau had come at a time when the NRC had been notified by the Rockefeller Foundation that it would no longer support the National Research Fellowships in the physical sciences. The NRC had been administering these prestigious fellowships since 1919, and was on the verge of having to eliminate them; its staff were glad to talk to the Bureau.

On March 16, 1953, a letter from Wallace R. Brode to Claude J. Lapp of the NRC made the matter formal, and from then on everything went smoothly; the NRC was happy to enter into a joint postdoctoral program with the Bureau. Indeed, for the 1953-1954 competition—the last year of Rockefeller supported fellowships—the NRC had forty-six applicants for eight university positions, and permitted the Bureau to see applications of four of those not chosen. The Bureau selected one of them—Janet Hawkins Meal, a spectroscopist from Harvard—and offered her a position with Mann. Except for the NRC screening, this was a noncompetitive (Schedule A) position, and it was described as a postdoctoral position, even before the CSC had approved such positions. Meal came to the Bureau in December 1953 and became known as the "zeroth postdoc." But CSC approval was forthcoming and on June 25, 1954, Commission Chairman Philip Young informed the director of personnel of the Department of Commerce that the commission had approved the Bureau's request to place postdoctoral positions under Schedule A. The Bureau was authorized to fill up to ten such positions.

There was, however, a slight problem with the title. The holders of these positions would have to pay income tax, whereas the universities wanted to reserve the name "fellowship" for positions that did not require the payment of income tax. The name of the position was therefore changed to "postdoctoral research associateship," and the positions were so advertised. The 1954 announcement, sent to university deans, professional societies, and leading scientific journals, read, "Announcement of the National Research Council-National Bureau of Standards Postdoctoral Research Associateships in Chemistry, Mathematics, and Physics, 1955-1956, recommended by the National Academy of Sciences-National Research Council." Thirteen areas of research in which positions were available were listed. Appointments were for one year, renewable for another, and were at the GS-11 grade level, paying $5940 per year. The announcement attracted twenty-one applicants. Their applications were rated by three NRC selection boards and the Bureau was constrained to select candidates in each field in the order in which they were rated. Seven applicants accepted positions, but one did not obtain his degree in time. In August 1955, the first NRC-NBS postdoctoral research associate arrived at the Bureau.

The program grew and flourished. In 1958 Director Astin made funding for the program a line item in the budget and, since division funds were no longer involved, competition for associates became keen among divisions. Rules were laid down: no more than two associates per division, but this was later relaxed when the yearly number of associates was increased to twenty. The original disciplines of physics, chemistry, and mathematics were augmented periodically, so that by 1983 there were nineteen, including astronomy and astrophysics, life sciences, geology, materials science with its divisions of metallurgy and ceramics, and all the engineering disciplines. Between 1955 and 1983, 2680 applications had been received, and 505 positions awarded. Other Federal Government agencies followed the Bureau's lead. A year after the Bureau received its first associates, the Naval Research Laboratory installed its own program with the NRC, and by 1985 some thirty-five Government laboratories had one of several types of postdoctoral research associate programs with the NRC.

It is difficult to overestimate the importance of this program to the Bureau. The original premises that fresh Ph.Ds could find valuable projects to carry out at the Bureau, and that the infusion of these intelligent, questing young minds into the Bureau would be a constant source of stimulation to the permanent staff, were amply borne out. Due to the method of selection, these were, after all, some of the brightest and most gifted students of their generation. Perhaps most important, the postdoctoral associate program became the preferred method for the Bureau to hire new staff.

An Instrumentation Program

Instruments for measurement have always been at the heart of experimental science, and scientists often have to devise special instruments for the measurements they want to make. Indeed, in 1923 the Institute of Physics (U.K.), with assistance from the National Physical Laboratory—the British counterpart of the National Bureau of Standards—began publishing the *Journal of Scientific Instruments,* comprised of scientific papers on new instruments. This publication was followed in 1930 with *The Review of Scientific Instruments,* published by the Optical Society of America. *Instruments: Industrial and Scientific,* published in 1928, was limited largely to descriptions of industrial instruments.[112]

By the end of World War II instrumentation was a flourishing activity. So many new techniques of measurement had been devised that the ordinary laboratory scientist could not keep up with them. A new type of scientist had developed, one who was primarily interested in how to measure phenomena, rather than on what the measurements meant. Moreover, feedback control systems had shown great development during the war, so that it was possible to think of instruments embedded into a control loop, leading to a "science of measurement and control." The science of instrumentation had been born. It recognized that measuring instruments generally had features in

[112] W. A. Wildhack, "Instrumentation in Perspective," *Science* 112 (1950): 515-519.

common—transduction, signal amplification, recording—no matter what the phenomenon measured, and this provided a basis for a new scientific discipline.[113] A new society, the Instrument Society of America, was formed in 1945, and instrument design, production, and selling became a new industry.

It was only natural that the Bureau, whose business was measurement, and which had devised many instruments, should consider developing a program in instrumentation. The champion of the program was William A. Wildhack, chief of the Missile Instrumentation Section. He convinced Condon of the merits of such a program and, effective June 1, 1950, an Office of Basic Instrumentation was established in the Office of the Director.[114] Funds were received from the Office of Naval Research, the Air Research and Development Command, the AEC, and NBS. With the principal objectives of systematically analyzing available methods and devices in terms of their performance and characteristics, and performing research on new applications and materials leading to new types of instruments, the Office of Basic Instrumentation worked by assignment of projects to those Bureau laboratories best qualified to conduct research in the particular field in question. The office did, however, maintain a small laboratory staff to work on special problems, and a group of specialists in instrumentation literature to develop a reference and consultation service.[115] As head of this office, Wildhack appears to be the first person in the Bureau's history to be what was later called a "program manager," a person with essentially no resources (personnel) to carry out work but who, either by personal persuasion or with funds available to him, induced line managers to carry out work for his program. After its formation the activities of the program were reported as a line item in the annual report.

There followed a profusion of instruments. Six examples, essentially picked at random, were an electron-beam interferometer, a miniature piezoelectric accelerometer, a thermal noise thermometer, a sensitive calorimeter for measuring the power of an x-ray beam, an instrument for measuring very small alternating currents such as those encountered in transistor circuitry without breaking the circuit, and an improved galvanometer design which optimized sensitivity and speed of response. By 1953, a bibliography of some 250 books and periodicals on important techniques had been compiled.

In 1960 the office was formed into the Instrumentation Division with five sections under G. Franklin Montgomery, with the functions of investigating "[t]he natural limitations of the measurement process, and the realizable performance of measuring instruments."[116] It kept a large reference file on instruments and measurement methods.

[113] An excellent analysis of this topic is given by E. U. Condon, "Is There a Science of Instrumentation?" *Science* 110 (1949): 339-342. Condon arrived at a positive answer to his question.

[114] U.S. Department of Commerce, National Bureau of Standards, Bureau Order No. 50-14, June 8, 1950, signed by E. U. Condon. (NARA; RG 167; Astin file; Box 19; Folder Corresp 1950)

[115] "Basic Instrumentation at NBS," *Technical News Bulletin* 37 (1953): 129-133.

[116] Annual Report, 1960: 100. The sections were Engineering Electronics, Electron Devices, Electronic Instrumentation, Mechanical Instruments, and Basic Instrumentation.

THE TECHNICAL WORK

In its study of the Bureau, the Kelly Committee evaluated all seventeen of the Bureau divisions and was concerned particularly with their ability to carry out basic research relating to the Bureau's unique measurement-standards responsibility. Not all divisions were equally capable of carrying out this responsibility. Indeed, for four divisions—Ordnance Development, Ordnance Electronics, Electrochemical Ordnance, and Missile Development—the point was moot, for these were the ordnance divisions entirely supported by the Department of Defense and working totally on weapons development. After their divestiture in 1953 following the committee's recommendation, thirteen divisions were left at the Bureau. These showed wide diversity in the amount and character of basic program work carried out, and the response of the Bureau's management to the Kelly Committee Report is well summarized in the Annual Report for 1955:[117]

> During the past year, the major effort of the National Bureau of Standards has been devoted to the strengthening of its basic programs. With the assistance of scientific advisory committees, the Bureau is seeking to develop a balanced technical program by increasing the level of research, especially basic research, in those fields for which the Bureau has an assigned responsibility.
>
> An effective standards research program must at all times remain at the forefront of science.

This management aim was carried out during the fifties with differing degrees of success in the various divisions.

This section is a short synopsis of the work carried in the period 1950-1957 in each of the divisions, with occasional, more extended vignettes on particularly noteworthy accomplishments. The aim is to illustrate by example the nature of the division's work, and how it changed during the period, if it did. Most of the material is taken from the Annual Reports and, when it is, citations are omitted to reduce what would otherwise be an inordinate number of footnotes.

ELECTRICITY; OPTICS AND METROLOGY

Two divisions in particular were concerned with fundamentally important standards —Electricity, under Francis B. Silsbee, and Optics and Metrology under Irvine C. Gardner. In the committee's opinion they were in need of attention so that they could meet the requirements of modern science.[118] Not that they were not capable of providing the calibration services they were accustomed to; the problem was that they

[117] Annual Report, 1955: 1.

[118] Kelly Committee Report: 24, 30.

were not advancing into new and required areas as fast as the Nation demanded. This situation had come about from lack of funds in the base appropriation, leading to far too great a proportion of transferred funds, and also from the heavy pressure for calibration. The latter problem was exacerbated by the age of the calibration equipment, which made the whole calibration process very slow, particularly in length calibrations carried out in the Optics and Metrology Division. Investment in the divisions had not been equal to the need.

This did not mean that the people were incompetent in their jobs. The calibrations were done well, if slowly, and the testing and calibration of instruments was extended into wider ranges. For example, measurements of voltage and current had to be made over an ever-expanding frequency and voltage range, and in the early fifties the range over which ammeters and voltmeters could be calibrated was expanded to 50 A and 400 V at frequencies up to 20 kHz. Similarly, new means of measuring resistors of extremely high resistance were developed, as well as equipment for more rapid calibration of watt-hour meters. Likewise, in Optics and Metrology numerous length calibrations, ranging from 10 μm to 50 m, were carried out; the working meter bar standards were intercompared and periodically compared with the legal national prototype meter, which, in turn, was compared with the international prototype meter at the International Bureau of Weights and Measures (BIPM); and end standards (gage blocks) were measured by interferometry using accepted spectral wavelengths. A number of end standards were also compared with line standards (standard meter bars) to verify the wavelength of krypton in preparation for the proposed change in the definition of the meter from the platinum-iridium meter bar to the wavelength of krypton light.

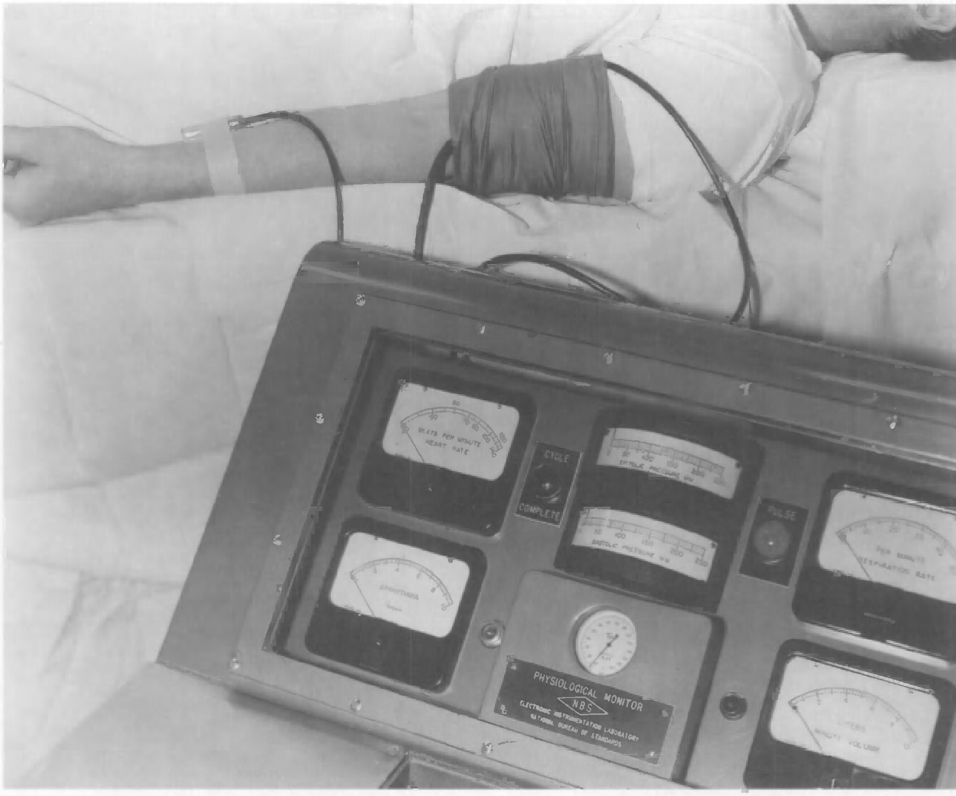
There were other measurements and standards to be re-evaluated. The output of lamps that served as the national standards for the photometry of mercury-vapor lights was redetermined with a photometer embodying a thermopile and accurate luminosity filters and found to be rated too low, so that their output had to be reassigned. Color standards and measurements of various kinds—for petroleum, for the color measurements needed for color television cathode-ray tubes, and for the determination of color differences—were issued or carried out throughout the period.

Both divisions carried out other work that was less directly tied to standards, although measurement methods were generally involved. Practically all of this work was supported by other agencies. The Electricity Division, working for the Rural Electrification Agency, utilized its high-voltage laboratory to test large ceramic insulators used for electric-power transmission. The obtained data enabled the re-design of the insulators, thereby reducing the possibility of failure during nearby lightning strikes. And the division, embroiled in the AD-X2 affair, nevertheless continued the development of various kinds of batteries for military agencies. The Optics and Metrology Division maintained a full program devoted to photography, including the issuance of useful charts for the determination of the resolution of photographic lenses; designed and built optical components such as interferometers; and used the SEAC to pioneer the use of computers for ray-tracing in lens design.

Then, upon the divestiture of the ordnance divisions in 1953, the Electronics Division was merged with the Electricity Division, becoming Electricity and Electronics under Silsbee, and a whole new set of activities was added to the division's program. This addition, more concerned with electronic equipment than basic electrical standards, introduced programs in circuits and circuit design, electron tubes, resistor noise, and electronic reliability. Under sponsorship from the military and other agencies, this component of the Electricity and Electronics Division turned out a profusion of electronic devices and instruments. To list but a few, there were:

- A vibration generator that operated at 100 Hz to 10 000 Hz for testing vacuum tubes for microphonics.
- A device to measure the error voltage in servo-feedback systems.
- A liquid-level indicator and control system, built for the Boulder Cryogenic Engineering Laboratory.
- FOSDIC (Film Optical Scanning Device for Input to Computers). Built for reading Census forms in which each answer to various questions consisted of a dark mark in one of several positions, the device read the position of these marks on a microfilmed copy of the form and converted the information to pulses on magnetic tape for subsequent input into an electronic computer. It was capable of reading 10 million answer-positions per hour.
- A physiological monitor. Built for the Veteran's Administration, this instrument automatically sensed the blood pressure, heartbeat, and respiration of a patient under anesthesia, and presented the data on a panel for the physician. This appears to be one of the first such now-ubiquitous instruments.
- A high-speed coin-weighing machine built for the Treasury Department that could weigh 18 000 coins per hour.
- A free-floating weather buoy built for the Navy, and later made operable in hurricanes. With a range of 800 miles, every 6 hours the buoy broadcast data on wind direction and velocity, barometric pressure, air and water temperature, and an identification symbol. It could be left unattended for 3 months.
- A very compact oscilloscope. Built as part of a continuing Navy program on the miniaturization of electronic components, various portions of the electron-tube circuit could be replaced with transistor assemblies to compare the performance of the vacuum tube and transistor circuits.

While these projects—and many more that could be mentioned—aptly illustrate the Bureau's role as the "corporate laboratory" of the Government, perhaps the most famous of the Electronic Division's programs during the period, Project Tinkertoy, was one of modular design and mechanized production of electronics. Oddly enough, despite its concern with the miniaturization of electronic components, the division did not have a program in transistors, although it used them in 1957 to build a cordless microphone for auditorium use.



A physiological monitor, built by NBS for the Veteran's Administration, shows panel board and attachment to the subject's arm. One of the electrodes, for measurement of heartbeat, was attached to the wrist, while the inflatable band for measurement of blood pressure was applied to the upper arm.

The performance of all this practical work did not mean that these divisions lacked the capability—or the desire—to carry out world-class measurement-standards work on the basic units. Indeed, one beautiful experiment in the Electricity Division illustrates the class of work that could be done. This was the experimental realization of the absolute ampere, and it was neither an idle exercise nor a vain display of metrological virtuosity. In 1950, the legal basis for electrical standards in the world had changed. Before this date, the basis was the so-called international system of electrical units in which, for example, the ampere was defined as the amount of current that would deposit 1118 μg of silver in 1 second. Congress had adopted these units as the legal standards in 1894.

As measurement technology progressed, it became possible to make precision measurements that defined the electrical units in terms of the mechanical units of mass, length, and time. This possibility arises because two parallel wires carrying current experience a force, and therefore—in principle—the measurement of this force and the distance between the wires can relate the unit of current to the unit of force, hence to the units of mass, length, and time. Similarly, the ohm can be defined on the basis of the inductance of a coil, which involves the measurement of lengths and a frequency. The definition of the ampere in terms of these mechanical units therefore leads to a unified measurement system, and the quantities based on it—ampere, ohm, volt—were called absolute quantities. In 1950 the Congress adopted the new units as the legal basis for electrical measurements in the United States.[119]

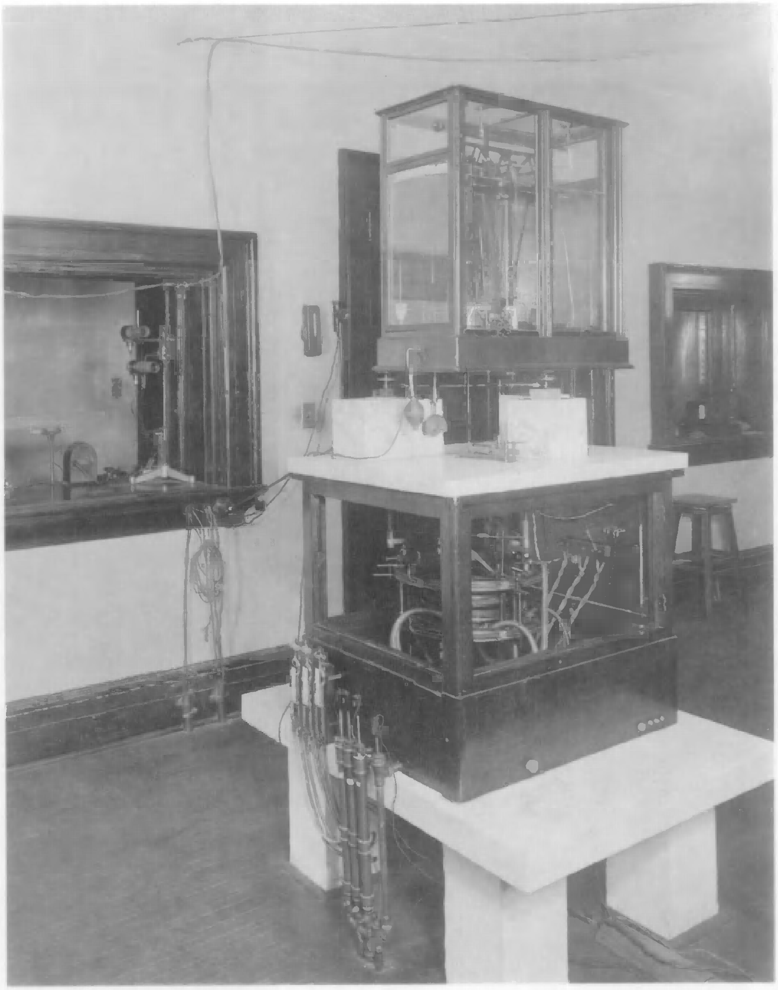
Before the adoption of these new units, the electrical units at the Bureau were maintained by a set of standard cells and standard resistors, with the ampere "as maintained" defined on the basis of these artifacts. The question had naturally arisen as to how the United States ampere as maintained by the Bureau was related to the absolute ampere. Absolute measurements are hard to make; before 1950 only four realizations of the absolute ampere had been made at the Bureau, the first in 1912 and the last in 1942.[120] These were all based on the so-called Rayleigh current balance. In this balance, three coils—two large and one small—are arranged coaxially and current is passed through them. Under the proper geometric arrangement, a force is developed on the small coil. The force is proportional to the current, and depends on the dimensions and geometric placement of the coils. The force per unit of current can be calculated from first principles knowing the dimensions of the coils and their geometric arrangement. The force is easily measured by suspending the small coil from the arm of a balance, with the dimensions and geometry measured as accurately as possible. But the measurements are very tedious, the forces small, and the experiments very sensitive to disturbances. Nevertheless, uncertainties of a few μA/A were attainable. Indeed, the 1939 measurement led to the result that one NBS international ampere was equal to 0.999 860 absolute ampere, while the 1942 result for the same quantity was 0.999 850—a value that was adopted by the international community. The units of the volt and the ohm as maintained by NBS and the other national standards laboratories were adjusted for this difference on January 1, 1948. But it was not until 1950 that the United States legally changed the absolute units.[121]

But routine calibrations were not made with the current balance; they would have been prohibitively expensive and time-consuming. They were made on the basis of standard resistors and electrochemical cells. And the question arose, "Has anything

[119] *AN ACT To redefine the units and establish the standards of electrical and photometric measurements, U.S. Statutes at Large,* 64 (1950): 369.

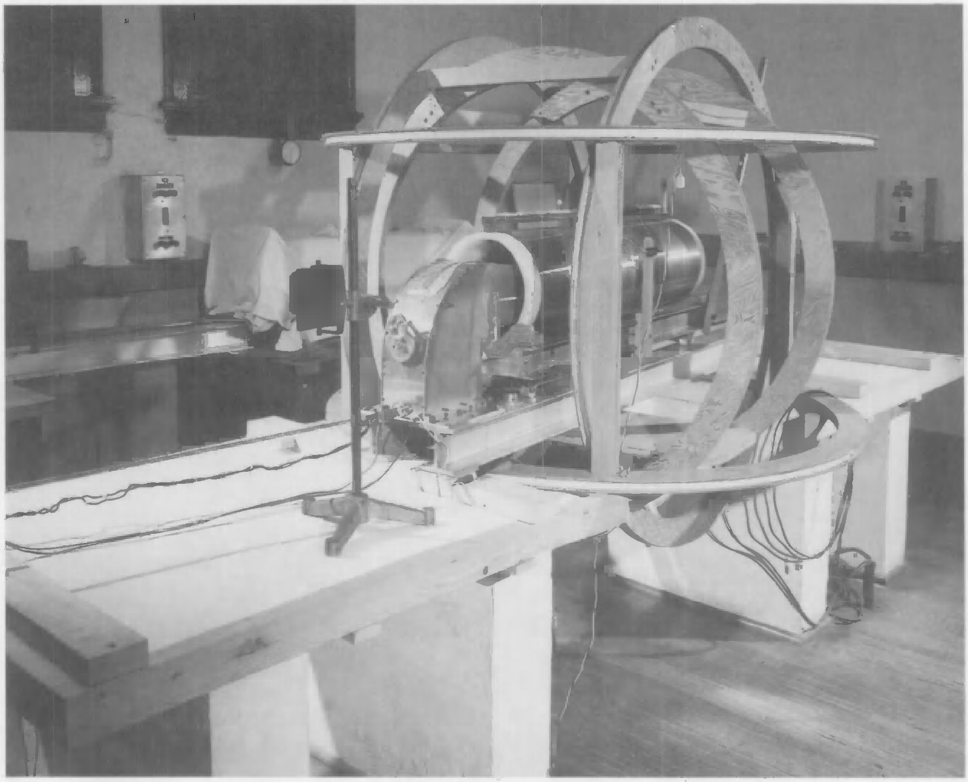
[120] E. B. Rosa, N. E. Dorsey, and J. M. Miller, "A Determination of the International Ampere in Absolute Measure," *Bulletin of the Bureau of Standards* 8 (1912): 269-392; H. L. Curtis and R. W. Curtis, "An Absolute Determination of the Ampere," *Bureau of Standards Journal of Research* 12 (1934): 665-735; H. L. Curtis, R. W. Curtis, and C. L. Critchfield, "An Absolute Determination of the Ampere Using Improved Coils," *Journal of Research of the National Bureau of Standards* 22 (1939): 485-517; R. W. Curtis, R. L. Driscoll, and C. L. Critchfield, "An Absolute Determination of the Ampere Using Helical and Spiral Coils," *Journal of Research of the National Bureau of Standards* 28 (1942): 133-157.

[121] *Announcement of Changes in Electrical and Photometric Units.* Natl. Bur. Stand. (U.S.) Circular 459; May 1947. The changes were not trivial. They amounted to increasing the value of the ohm and the volt by 495 ppm and 330 ppm, respectively.



Prior to 1954, all four determinations of the absolute ampere had been based on the Rayleigh current balance. In the classic experiment of Rosa, Dorsey, and Miller (1912), a series of nearly 500 measurements determined the absolute ampere to be slightly larger (4 parts in 100 000) than the international ampere determined by silver voltmeters.

drifted in this system so that the maintained ampere no longer has the same relationship to the absolute ampere?" In 1954, while the division was still feeling the effects of the AD-X2 incident, Raymond L. Driscoll set out to answer it by making yet another determination of the absolute ampere. To ensure that his results would properly answer the question, he used a different method for the measurement, the so-called Pellat balance. While the Rayleigh balance measures forces on coaxial coils, the Pellat balance is based on the measurement of torques on current-carrying coils with their axes at right angles. For this purpose, a small coil is placed inside a long solenoid



Electrodynamometer used in the absolute determination of the NBS ampere in 1954 by Raymond L. Driscoll. The metal housing that entered the large solenoid contained a small rotatable coil and a balance arm. When the direction of the current through the coils was reversed, a torque was produced on the small coil. The coil was connected to the balance arm and, as it tended to rotate, equilibrium was upset. Balance was restored by adding known weights to the balance arm by means of the rod-and-pulley arrangement at the outer end of the housing. From the known weight, the length of the balance arm, and the geometry of the windings, the value of the current in amperes could be calculated in terms of length, mass, and time. The framework surrounding the apparatus contained coils to compensate for the earth's magnetic field.

with its axis orthogonal to that of the solenoid. Current is passed through the two coils and the torque on the small coil measured, again using a balance and an ingenious jig to support the small coil. New coils had to be designed and made, an excruciating task in itself. The whole arrangement had to be kept in an isolated room, with control only via long control rods. Four years later Driscoll[122] published his results. They were the same as the 1942 results with an uncertainty of 6 $\mu A/A$. To cement the results further, Driscoll and Robert D. Cutkosky repeated the 1942 work of Roger W. Curtis, Driscoll,

[122] R. L. Driscoll, "Measurement of Current with a Pellat-Type Electrodynamometer," *Journal of Research of the National Bureau of Standards* 60 (1958): 287-296.

and Charles L. Critchfield, using essentially the same equipment as in the earlier work.[123] Their work agreed with Driscoll's Pellat balance work to with an uncertainty of 3 μA/A. The system of electrical units was stable.

But no matter how beautiful and precise, all this work was applied research, not the basic research the committee felt should be done. The committee had recommended several areas of what could be called materials science for expansion of the division's basic research activities. One of these was dielectrics, and in 1955 the division hired John D. Hoffman to begin a program in this field. The effort developed into the formation in 1956 of the Dielectrics Section in the Electricity and Electronics Division, with a truly basic program in the dielectric properties of polymers. Thus, in at least one area, the division was following the recommendations of the Kelly Committee. But the situation was not to be long-lasting. The section was in due course moved into the Polymers Division, the offspring of the Organic and Fibrous Materials Division.

Project Tinkertoy

When the divestiture of the weapons work took place in 1953 in the aftermath of the AD-X2 incident, one division working almost entirely on military funds was not transferred out of the Bureau. This was the Electronics Division that was combined with the Electricity Division to create the Electricity and Electronics Division. For some years it largely continued the work it was doing before the divestiture, and perhaps no project is as illustrative of the difference in character between work on the Bureau's basic measurement mission and the military work as Project Tinkertoy.

Concerned about the industrial mobilization and preparedness of the electronics industry in case of a national emergency, the navy's Bureau of Aeronautics realized that the only way to satisfy the anticipated large demand for electronic equipment in such an emergency was a mechanized production system. The system should be flexible so that many different types of electronic equipment could be manufactured with minor changes. Such flexibility could be assured by a modular system, where the modules were structurally the same but could be made with differing electronic functions. The Bureau looked upon the effort as a partial standardization of the electronics industry that would "not only simplify the mobilization of the electronics industry . . . but also would minimize variations in electronic circuit designs," thereby reducing costs for design, maintenance, parts procurement and stocking, and training.[124]

Knowing that as a result of its work on the proximity fuze the Bureau had a wealth of experience with modular design of electronics, and had done pioneering work on printed circuits, and developed such things as tape resistors, in 1950 the navy came to the conclusion that "the most advanced state of processed circuitry is available at the National Bureau of Standards."[125] It therefore asked the Bureau to undertake a project

[123] R. L. Driscoll and R. D. Cutkosky, "Measurement of Current With the National Bureau of Standards Current Balance," *Journal of Research of the National Bureau of Standards* 60 (1958): 297-305.

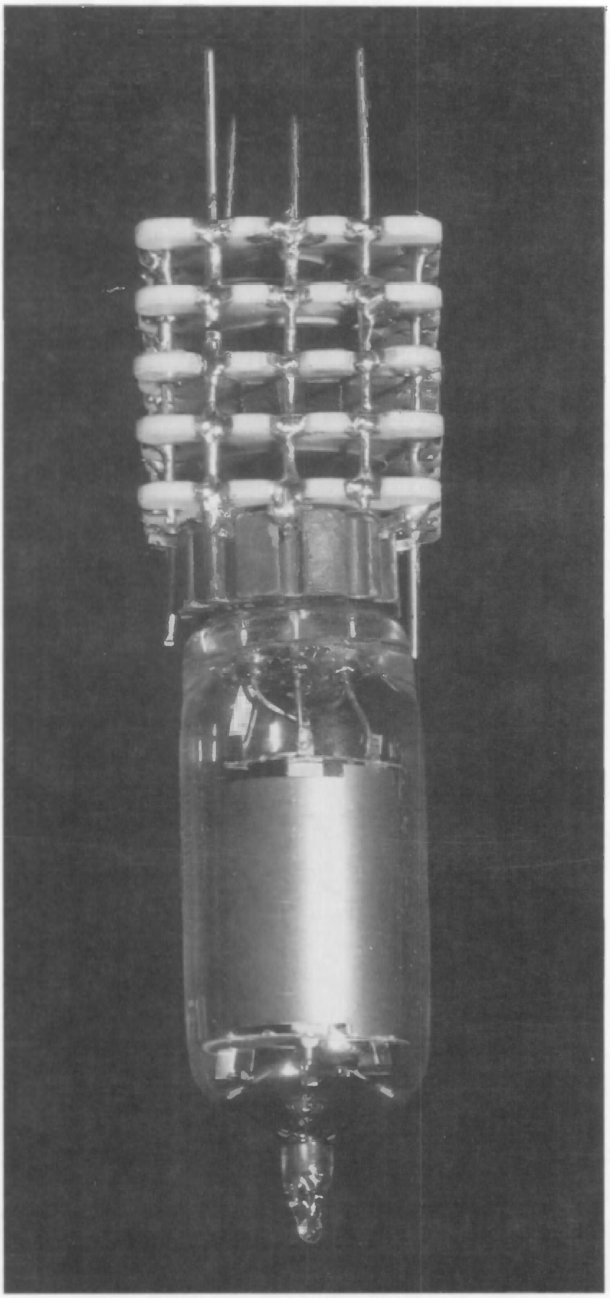
[124] Annual Report, 1953-54: 78.

[125] "Project Tinkertoy: Modular Design of Electronics and Mechanized Production of Electronics," *Technical News Bulletin* 37 (1953): 169.

Robert L. Henry, Project Tinkertoy leader, adjusted a stencil screen mounting on the MPE wafer pattern printer.

to develop the automated production of electronics. Code-named Project Tinkertoy, the major objective of the program was the design and construction of a pilot plant compatible with the principles of modular design and mechanized production of electronics, or MDE and MPE. In short, the Bureau was entering into the development of a process for automated manufacture of electronic equipment and for demonstrating it on a pilot production line. The process was to start with the raw materials to preclude the requirement of manufactured parts (except for vacuum tubes).

The concept was relatively simple. Each stage (e.g., amplifier, oscillator, detector) in an electronic circuit was thought of as a module. Each module was composed of four to six ceramic wafers which carried the components of the stage, such as resistors, capacitors, or coils, each with leads applied by printed-circuit techniques. The ceramic wafers were keyed so that they could fit one above the other in only one way, and were provided with notched edges so that connections could be made from one wafer to another. When assembled, the wafers resembled the floors of a building with the equipment on each floor connected to that on the floors above and below. At the very top—on the roof of the building, so to speak—sat the vacuum tube if one was required



The Project Tinkertoy module. Each module was composed of four to six wafers bearing printed conducting circuits, tape resistors, capacitors, tube sockets, and other miniaturized electronic component parts.

for that stage. These stages, or modules, were then connected to other modules to make the whole electronic circuit. This was called modular design of electronics.

The modules were produced automatically by machines on a production line, the process being called mechanized production of electronics. The wafers were produced from the powdered ingredients for the ceramic; capacitors, 1/2 in squares, were thin sheets with metallized surfaces produced from various titanate compositions to obtain capacitances from 7.0 pF to 0.01 μF; resistors were produced from carbon tapes, previously developed by the Bureau,[126] to obtain resistances from 10 Ω to 10 MΩ, with relative uncertainties of 10 percent. Wiring was printed onto wafers, connections made in the standard way, and the wafers connected by copper wires. All the assembly operations were carried out on a single production line by automatic machines, with automatic physical and electrical inspection.

The Bureau did not work alone in this endeavor. The major part of the production equipment design was done by the Electronics Division of the Willys Motor Co., which also operated the pilot production line. Several other commercial organizations helped, particularly in the manufacture of the specialized production machinery.[127]

By 1953 the pilot plant was in operation, and in the fall, while the production line was producing an item of naval equipment, a public announcement of the project was made, stirring considerable industrial interest. An estimate of the manufacturing cost was made and found to be 44 percent lower than conventional processes.[128] By 1955 the main elements of the program had been accomplished, and the pilot plant was being used for indoctrination and training of industrial organizations.[129] Finally, the pilot plant was closed down. The 1956 Annual Report announced, "Since these [MDE and MPE] concepts were first announced in 1953, the art has been further developed and full technical information has been released to private industry. A number of manufacturers . . . have shown extensive interest. . . . This broadened industrial activity and the further improvements now underway in industry makes it appropriate for the Bureau to end its pilot-plant activities."[130] While the Bureau's modular design was not used directly in industry, the modular design and mechanized production concepts have become the customary way of producing electronic equipment.

HEAT AND POWER

In the opinion of the Kelly Committee, the Heat and Power Division, under the leadership of Ferdinand G. Brickwedde, had a good balance between fundamental research and "developmental research."[131] It was a large division with a staff of 204 (125 professionals), with 86 percent of its support coming from transferred funds. Despite this imbalance of support, the committee's praise of the division showed that in some cases good research could be done with transferred funds.

Foremost among the division's activities was the maintenance and improvement of the International Practical Temperature Scale (IPTS), which necessitated the attainment

[126] "NBS Precured Tape Resistor," *Technical News Bulletin* 36 (July 1952): 110-111.

[127] "Project Tinkertoy," *Technical News Bulletin* 37 (1953): 170.

[128] *Manufacturing Cost Determination*, Vol. 5 (New York: Mead, Carney): 31.

[129] Annual Report, 1955: 18.

[130] Annual Report, 1956: 16-17.

[131] Kelly Committee Report: 34.

of very low and very high temperatures, and the exploration of phenomena such as the isotope effect in superconductivity under extreme conditions. The measurements at high temperatures were further motivated by the temperatures achieved in the jet engine industry, and temperature measurements and combustion studies in the jet-engine environment were carried out continuously during the period. Some examples of the research on the temperature scale were the development of platinum vs. platinum-iridium alloy thermocouples for use at high temperatures, a new apparatus for measuring the liquid sulfur fixed point (444.6 °C), a program on gas thermometry to temperatures of 800 °C to bring closer correspondence between the International Practical Temperature Scale and the Thermodynamic Scale, the extension of the range of the platinum resistance thermometer as the definer of the IPTS to the gold point (1063 °C), and the development of a photoelectric pyrometer in 1957.

But the division's work did not stop with research on temperature measurements and on the temperature scale. It carried out a large program on the measurement of thermal properties of matter, often leading to data compendia on various materials, and it had an outstanding capability in the measurement of specific heat of solids, liquids, and gases. Some examples of the work are:

- Standard samples—n-heptane, benzoic acid, and aluminum oxide—certified for specific heat and covering the temperature range from 14 K to 1173 K were issued.
- Motivated by the need for good heat-transfer materials for use in nuclear reactors at high temperatures, the specific heats of sodium and potassium and their alloys were determined up to 1173 K.
- A number of data compendia on the thermodynamic properties of various materials for specific purposes were published: the specific heat of CO_2, from −50 °C to 100 °C and pressures from 0.5 atm to 1.5 atm, to check the values obtained by calculation and published by the National Advisory Committee for Aeronautics; extensive tables of properties of hydrogen and deuterium for the AEC; thermodynamic properties of wind-tunnel gases—particularly air—up to 3000 K; and extensive tables on the properties of fluorine compounds, particularly those used as refrigerants. The data were obtained both by direct experiment and by theoretical analysis. Some of these data extended to 5000 K.
- The heat capacity of natural rubber and other high polymers was determined over a wide temperature range, as were the heats of solution, density, and vapor pressure of polystyrene and polybutadiene in various solvents.

In addition to this work, the rheology of various polymers was investigated. This research in rheology began with the establishment of the automotive laboratory in 1917 and the consequent interest in lubrication. The division calibrated viscometers and distributed several samples of oils certified for viscosity. The interest in the rheology of high polymers seems to have been brought about by Robert S. Marvin, who was trained in the viscoelasticity of polymers and was hired by the division in 1949. In 1957 a Rheology Section was formed with Marvin as its head. In the same year,

Richard Lee of the Heat Division calibrated a photoelectric pyrometer using a tungsten filament strip lamp. The tool, designed to measure the temperature of a jet engine, replaced the less accurate optical pyrometer.

feeling that with the standard samples available from the Bureau "[m]ore reliable calibration of these instruments is now obtained under conditions of actual use," the calibration of viscometers was discontinued.[132]

In a somewhat unrelated area, the division had research programs in various aspects of automobile and jet engines. Begun in World War I, an engine laboratory assisted "the then-infant automobile and aircraft industries." Along with the area of lubrication, hence viscosity, this had led the division into the area of engine fuels, as befits what was essentially a division based on thermodynamics. One of the most important industrial measurements it developed was the octane-number rating of gasoline engine fuels, and it distributed *iso*-octane and n-heptane, the two hydrocarbons necessary to determine octane numbers. Throughout 1950-1957, research continued on various aspects of engine knock, combustion in jet engines, and the mechanisms of flame propagation.

[132] Annual Report, 1957: 100.

Then, in 1956, the engine laboratory was discontinued.[133] The Annual Report notes, "The decision was based on careful consideration of the small amount of work of this kind now required by other Government agencies, the nature of the work in relation to other Bureau activities, and the critical need for the space involved by other projects of greater importance. . . . With the tremendous growth of these [the automobile and aircraft] industries they now support their own research, testing and development programs." The decision did not, however, "change the Bureau's activities in regard to maintenance of the standards for measurement of octane number, improvement of methods of measurement, and other standardization work involving fuels and lubricants." It is not difficult to surmise that this decision was part of the policy in the Kelly Committee Report that the Bureau divest itself of activities that were not closely related to its measurement mission, where its activities were essential. That same year, the words "and Power" were dropped from the division's name, and it became simply the Heat Division.

With support from the army's Office of Ordnance Research, a new program was initiated in 1957 that was to have important consequences for the division and the Bureau. In the words of the Annual Report, "[v]aluable information on the structure and properties of matter may eventually result from a three-year program of basic research initiated during the year. The object of the program is to increase fundamental knowledge of the formation, properties, and storage of the highly reactive molecular fragments known as free radicals."[134] Attesting to the significance Bureau management saw in this program, a new section—Free Radicals Research—was formed for this program, and Herbert P. Broida, who had worked with John R. Pellam in the study of nitrogen atoms in rare-gas matrices at liquid helium temperatures, was appointed as its chief. The Free Radicals Program, as it came to be called, was catalytic in the reorganization of all the Bureau's chemistry activities.

Among the division's various programs, the best known in the scientific world was its program in low-temperature physics. The Bureau had been involved in cryogenic research since 1904 when it obtained a hydrogen liquefier, but it was not until 1948, when it obtained a helium liquefier, that low-temperature physics research began in earnest. Built up by Brickwedde and supported by Condon, the low-temperature physics laboratory became one of the best in the world. It attracted a number of outstanding scientists. First was Emanuel Maxwell with his work on the isotope effect in superconductivity. He was followed by John R. Pellam, who did pioneering work in the determination of the speed of second sound in helium.[135] He in turn was followed by two outstanding young scientists from Oxford University, Ralph P. Hudson and Ernest Ambler, both students of Nicholas Kurti. Hudson in due course became chief of the division, and Ambler became the Bureau's eighth director in 1978. This cryogenics capability was the reason for the AEC's choice of the Bureau to build up their cryogenic engineering program.

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[133] Annual Report, 1956: 132-133.

[134] Annual Report, 1957: 6.

[135] Below the temperature at which liquid helium first exhibits superfluid properties, heat is conducted in it by a wave motion similar to that by which sound is conducted. This manner of heat conduction is called second sound.

One of the objectives of the low-temperature program was the attainment of extremely low temperatures and devising means of measuring temperatures in this region. In the early fifties very low temperatures—down to about 0.001 K—were customarily attained by the technique of adiabatic demagnetization, and the temperature of the experiment was measured by the same technique.[136] In this technique, a sample of a paramagnetic salt is cooled to as low a temperature as possible by using liquid helium which is being pumped continuously by a vacuum pump. A strong magnetic field, provided by a large magnet, is then turned on. This field aligns all the atomic "magnetic moments." The sample is then isolated from the liquid helium bath, usually by evacuating the container in which it resides. Since heat can no longer flow in or out of the sample, it is now in an adiabatic (isentropic) container. Now the magnetic field is turned off. Since the sample is thermally isolated, any change in the spin system must occur at constant entropy (isentropic process). But with the external magnetic field gone, the only way for nature to keep the entropy the same is for the temperature to fall. At still lower temperatures, it is possible to align the nuclear spins or, with coupling between the nuclear and electronic spins, even at not too low temperatures (1 K to 0.01 K).

In the early fifties, adiabatic demagnetization was a "hot" scientific area. Brickwedde hired first Hudson and then Ambler to work in the area, and by 1956, the Bureau was recognized as one of the foremost laboratories in the world for research at "very low temperatures." Now fate was to conspire to bring about one of the most famous—if not *the* most famous—experiments in the Bureau's history: the experimental demonstration of the nonconservation of parity in "weak interactions."

The Parity Experiment

In the October 1, 1956, issue of the *Physical Review*, there appeared a paper by Tsung Dao Lee of Columbia University and Chen Ning Yang of the Institute for Advanced Study at Princeton, but temporarily at Brookhaven National Laboratory.[137] The paper was entitled "Question of Parity Conservation in Weak Interactions," which was to revolutionize thinking in theoretical physics as well as winning the 1957 Nobel Prize for its authors.[138] In a brilliant theoretical analysis, Lee and Yang came to the conclusion that, contrary to long-held belief, there was no evidence that parity was conserved or not conserved in weak interactions. They proposed two experiments to find the actual situation.

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[136] E. Ambler and R. P. Hudson, "Absolute Temperatures Below 1 °K: Chromic Methylammonium Alum as a Thermometric Substance," *Journal of Chemical Physics* 27 (1957): 378-389.

[137] T. D. Lee and C. N. Yang, "Question of Parity Conservation in Weak Interactions," *Physical Review* 104 (1956): 254-258.

[138] Parity is a quantum mechanical concept which basically states that the behavior of quantum mechanical systems should not change when viewed in a mirror or, to put it more technically, "are invariant under space inversion." *Weak* interactions include beta decay, i.e., radioactive decay by the emission of an electron. These interactions are one of the four basic interactions known to physics, the other three being the *strong* interaction, which holds the nucleus together; the *electromagnetic*, which is responsible for the force between charged particles and holds the atom together; and *gravitation*, which governs the behavior of bodies with mass.

One experiment involved the measurement of any angular asymmetry of the electron emission from polarized cobalt-60 nuclei, such as cobalt-60 nuclei oriented so that the spins of all of them point in the same direction. Any asymmetry in the electron emission with respect to the forward and backward directions of the nuclear spin would immediately indicate that parity is not conserved and that, in beta decay, nature prefers one hand over the other.[139] In principle, this is an easy experiment, but in practice it is very hard. First, and most important, a sample of cobalt-60 with oriented nuclear spins must be available, and this means very low temperatures. Second, one must be able to get the very low-penetrating beta particles out of the sample used to orient the nuclear spins, and out of the cryostat used to cool them. Alternatively, a beta-particle detector would have to be developed for use in the cryostat.

Now, the Cryogenic Physics Section of the Bureau, whose chief in 1956 was Hudson and in which Ambler was a principal scientist, knew how to orient radioactive nuclei. Both earned their doctorates in the Clarendon Laboratory at Oxford University where, under the leadership of Sir Francis E. Simon and Nicholas Kurti, there was a major research program in the physics of very low temperatures produced by magnetic cooling, coupled with the work of Brebis Bleaney, Maurice H. L. Pryce, and later with others on the techniques of nuclear orientation. Ambler and Hudson brought these techniques to the Bureau and, working with Georges M. Temmer of the Carnegie Institution, had published two papers on nuclear alignment in cerium-141, cerium-139, and neodymium-147, all radioactive nuclei.[140] Moreover, while still a graduate student at Oxford, Ambler, working with six others, had polarized cobalt-60 nuclei, and measured the anisotropic emission of the gamma radiation. But there had been no good reason at that time to tackle the experimentally difficult task of measuring the asymmetry of the beta radiation as was now being suggested by Lee and Yang.[141]

The Lee-Yang work was not immediately known to Ambler and Hudson, but their own work and capabilities were generally known to most of the physics community. Consequently, on June 4, 1956, before the publication of the Lee-Yang paper, Ambler received a telephone call from Professor Chien-Shiung Wu, a colleague of Lee at Columbia University and herself an expert in beta decay. Ambler recalls, "I didn't know who she was, although I'd heard of the name. She said that Lee and Yang had had this idea that with beta particles from cobalt-60, more will come up in one direction of

[139] This follows directly from the fact that the spin direction (an axial vector) is not changed by a parity operation, while velocity (a polar vector) is inverted.

[140] E. Ambler, R. P. Hudson, and G. M. Temmer, "Alignment of Cerium-141 and Neodymium-147 Nuclei," *Physical Review* 97 (1955): 1212-1221.
—"Alignment of Three Odd-*A* Rare-Earth Nuclei," *Physical Review* 101 (1956): 196-200.

[141] E. Ambler, M. A. Grace, H. Halban, N. Kurti, H. Durand, C. E. Johnson, and H. R. Lemmer, "Nuclear Polarization of Cobalt 60," *Philosophical Magazine* 44 (1953): 216-218. The possibility of observing the beta emission had been discussed often at Oxford, but it was not done for two reasons. First, because of the limited range of the beta radiation (compared to the gamma, which had been observed), the electrons could only get out of the surface layers of the paramagnetic salt used for cooling, and second, could not pass out of the cryostat. Most important, of course, before the Lee and Yang paper, accepted theory predicted no unusual effects, so that the scientific spur to do the difficult experiment was lacking.

the field than the other. I said, 'Are you sure you mean up and down?' She said, 'Yes, up and down, that's the difference.' I said, 'Is there a preprint of that paper?' She said, 'Yes.' I said, 'Send me one.' So she sent me one. The first thing I did was to check with our radioactivity people and discovered that she was tops in her field, so it was a request to be taken very seriously."

Having the request from Wu to carry out the Lee-Yang experiment, and knowing that it was a very serious request, Ambler checked with "some of the senior physicists at the Bureau, and they all shook their heads and said, 'It's a very, very, very long shot.' Ralph [Hudson] and I talked about it and we sort of decided, and I became convinced, that it was one of those things that is a risk you've absolutely got to take, because it was clear that the whole thing would be absolutely revolutionary. So I went to see Brick [Ferdinand Brickwedde] and explained it and told him that I thought we could do it with the budget we had. Damn if old Brick said, 'Well, Ernie, if it's not going to cost any more money, you go right ahead and do it.' I called her and said, 'Sure.'"[142]

After several weeks of preparatory work, two nuclear physicists, Raymond W. Hayward and Dale D. Hoppes—the experts on beta radiation from the Bureau's Atomic and Radiation Physics Division—were asked to join the effort. Prof. Wu had been coming down from Columbia periodically with two graduate students, but it had become clear that beta radiation experts were needed on the spot. The objective of the experiment was the measurement of the forward-backward asymmetry of the electron emission from the polarized cobalt-60 sample.[143] Because the range of the beta rays is very short, the radioactivity had to be confined to the very surface layers of the paramagnetic salt used for cooling. And a serious concern was whether the surface layers would stay cold long enough to do the experiment. There clearly was only one place to do the counting of the emitted electrons, namely inside the experimental chamber just above the sample of cobalt-60. With this limitation, there was only one way to determine if there was any asymmetry of the electron emission with respect to the direction of the nuclear spin. First, the spins are oriented in one direction, say "up," and the electron counting rate determined. Then the spins are oriented in the opposite direction, say "down," and the counting rate determined again. If the counting rates are different in the two cases, the electrons are emitted preferentially along (or against) the spin direction, hence the emission is asymmetric with respect to the direction of the nuclear spin and parity is not conserved.

In more detail, to do the counting a small thin disk of anthracene was placed just above the sample and a Lucite light pipe carried the scintillations to a phototube outside the cryostat. The sample itself was a single crystal of cerium magnesium nitrate with a thin layer containing the cobalt-60 grown on its upper surface. Equatorial and polar sodium iodide photomultiplier counters placed well outside the cryostat monitored the gamma emission, hence the nuclear polarization of the sample.

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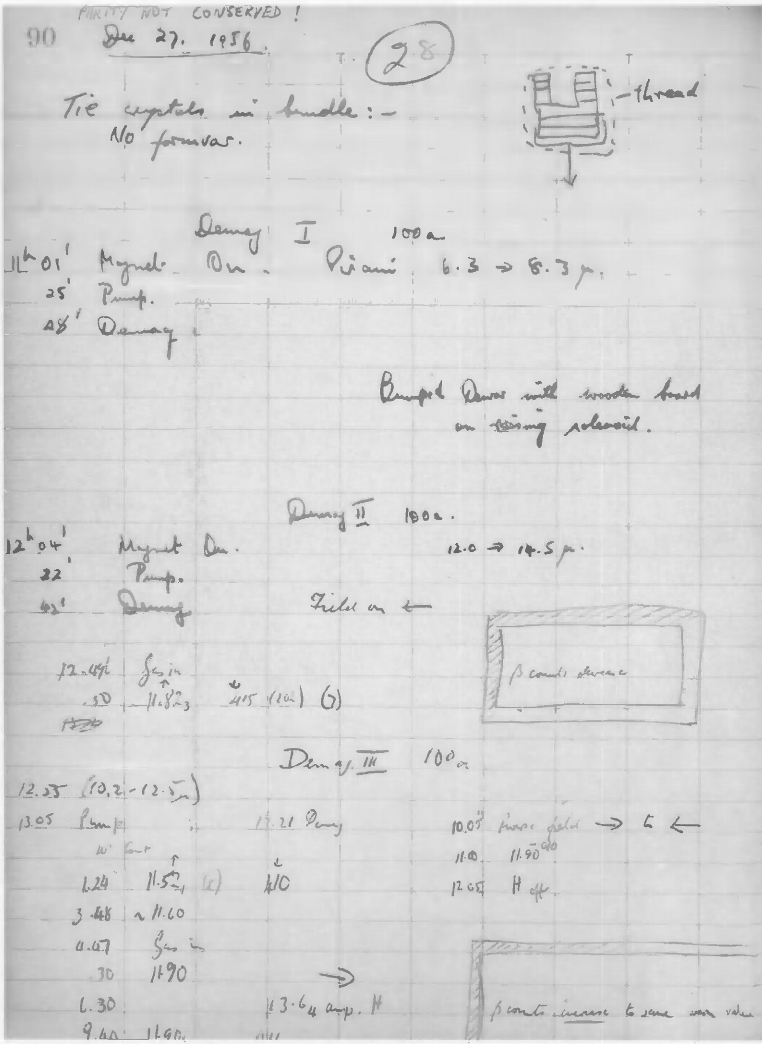
[142] Interview with Ernest Ambler, July 7, 1988: 4-5. NIST Oral History File.

[143] C. S. Wu, E. Ambler, R. W. Hayward, D. D. Hoppes, and R. P. Hudson, "Experimental Test of Parity Conservation in Beta Decay," *Physical Review* 105 (1957): 1413-1415.

NBS scientists assembled low-temperature equipment used in in the experiment which disproved the principle of conservation of parity in nuclear physics. Left to right: Ralph P. Hudson, Ernest Ambler, Dale D. Hoppes, and Raymond W. Hayward.

Many operational difficulties had to be overcome as the work progressed and, in fact, at one point an entirely redesigned cryostat was constructed. Conclusive results were first obtained in late December 1956, some six months after Prof. Wu's first telephone contact.

The experiment was carried out as follows: First the sample was cooled by adiabatic demagnetization using a large electromagnet. Then the latter was removed and a small solenoid magnet placed around the cryostat. Because of the interaction of the nuclear spins with the very strong field caused by the electron spins, a relatively small current



This page from Ernest Ambler's notebook recorded the first successful demonstration that parity is not conserved in the emission of beta particles by cobalt-60. At the top of the page, Ralph P. Hudson lettered PARITY NOT CONSERVED! (courtesy of the National Museum of American History)

passing through the solenoid oriented the electron spins and through them the nuclear spins along the magnetic field in the solenoid, either up or down depending on the direction of the current through the solenoid. Due to small but inevitable heat leaks, the sample continued to warm, and in about eight minutes warmed up to such a temperature that the nuclei were no longer oriented. During this time the electron counting rate dropped for one orientation of the field as the orientation in the sample decreased as monitored by the gamma emission. The counting rate reached a constant value when the sample was sufficiently warm that no orientation existed. Now the experiment was repeated for the opposite direction of current in the polarizing solenoid. The behavior of the electron counting rate with time was now the opposite of what it had been with the field in the other direction. If it previously decreased with time, it now increased, and vice versa, eventually reaching the same warm-temperature value. This experimental result proved conclusively that the emission of the electrons is preferentially along the spin, and further analysis shows that it is preferentially in the direction opposite to the spin. Hence there is an asymmetry of the electron emission with respect to the direction of orientation of the nucleus, and parity is not conserved in the weak interaction. [144]

The demonstration of the nonconservation of parity in the weak interaction stunned the theoretical physics community, which immediately became concerned with the violation of other symmetry principles. The situation became more complex. Of particular interest were the symmetries of charge conjugation, (C-inversion of charge, or changing from particle to antiparticle), and time reversal (T). Work by Lee, Yang, and Reinhard Oehme published after the original cobalt-60 paper—but known to the Bureau group via communication with Yang—showed, as was already suggested in the original paper, that not only was parity not conserved in weak interactions, but charge-conjugation invariance was also not obeyed, although under certain conditions the combination of the two was. It then became very important to see if invariance under time reversal was also violated, for a very fundamental theorem due to Wolfgang Pauli and Gerhart Lüders states that the triple operation of charge conjugation (C), space inversion (P) and time reversal (T), or CPT, will always be conserved. Hence the Bureau group continued to work in the area, carrying out essentially the same experiments with cobalt-58 (a positron emitter and hence important because of charge conjugation), and later on manganese-52 specifically to see if time-reversal invariance could be proved. Within the limits permitted by the data, T was conserved.[145] Further work continued on yet other nuclei to obtain data on the parameters in the theory and the work became more and more nuclear physics.[146] In 1969 the final Bureau work on the conservation laws was carried out by Russell C. Casella, theoretician member of the Radiation Theory Section. By an analysis of experimental data on the decay of the neutral K mesons, for which it was known that CP invariance is violated, Casella showed that the CP violation is connected with a violation

[144] R. L. Garwin, L. M. Lederman, and M. Weinrich, "Observations of the Failure of Conservation of Parity and Charge Conjugation in Meson Decays: the Magnetic Moment of the Free Muon," *Physical Review* 105 (1957): 1415-1417. In a paper published at the same time as the Bureau's parity paper, Richard L. Garwin, Leon M. Lederman, and Marcel Weinrich of Columbia University showed that parity is also not conserved in meson decays.

[145] E. Ambler, R. W. Hayward, D. D. Hoppes, R. P. Hudson and C. S. Wu, "Further Experiments on β Decay of Polarized Nuclei," *Physical Review* 106 (1957): 1361-1363; E. Ambler, R. W. Hayward, D. D. Hoppes and R. P. Hudson, "Beta-Gamma Correlations from Polarized Manganese-52," *Physical Review* 110 (1958): 787-789.

[146] D. D. Hoppes, E. Ambler, R. W. Hayward, and R. S. Kaeser, "Matrix Elements in the Forbidden Beta Decay of Ce^{141}," *Physical Review Letters* 6 (1961): 115-118; D. D. Hoppes, "The Angular Distribution of Beta Particles from Oriented Cerium-141 Nuclei," *Proceedings of the 7th International Conference on Low Temperature Physics* (University of Toronto Press, 1961): 186-188.

of time-reversal invariance, but that there was no evidence of CPT violation.[147] "In Nature, past and future are thus distinguishable even on a *microscopic* level." [148]

In retrospect it is difficult to find a better example of what the Kelly Committee had in mind when it insisted that the Bureau get back to doing research in its unique mission. A group formed to work on the production, measurement, and application of low temperatures, and given some latitude to follow their own interests were, admittedly by a happy combination of circumstances, brought face to face with some of the most fundamental questions in all of physics. And they were able to answer at least one of them.

ATOMIC AND RADIATION PHYSICS

Formed in 1947 as part of his reorganization, the Atomic Physics Division was Condon's principal mechanism for bringing the new physics to the Bureau. It was started with five sections of the Optics Division—Spectroscopy, Atomic Physics, Radiometry, Radioactivity, and X-Rays—and it was as if Condon had grouped together those aspects of the Bureau's work that rested on quantum mechanics as a foundation. The division grew rapidly with the addition of new, bright, young scientists—one of whom, Lewis Branscomb, was to become a Bureau director—so that by the time the Kelly Committee investigated the Atomic and Radiation Physics Division in 1953, it had a total staff of 176 employees, 120 of whom were professionals. Led by Lauriston S. Taylor, it was organized into two laboratories: the Atomic Physics Laboratory with eight sections, and the Radiation Physics Laboratory with seven sections. With 46 percent of its funds coming from direct appropriations, it was relatively well supported by Bureau funds.

In some ways the division's name was a misnomer, for its work included electron physics, solid state physics, radiometry, and instrumentation. It was one of the Bureau's stellar divisions, winning high praise from the Kelly Committee, which found its work excellent, and whose only lament was that there was not enough of it.[149] Three of the division's notable accomplishments and areas of work have already been described: length and light, standards and fundamental constants, and x rays. Here we give a rather cursory account of some of the other activities of the division.

Considering its history, it is not surprising that one of the strong elements in the division's program was spectroscopy. While the work on the development of the green line of mercury-198 into a standard of length was perhaps the most famous development in the division's work during the fifties, there was other meritorious work of

[147] R. C. Casella, "Time Reversal and the K^0 Meson Decays," *Physical Review Letters* 21 (1968): 1128-1131; and "Time Reversal and the K^0 Meson Decays II," *Physical Review Letters* 22 (1969): 554. The author is deeply indebted to Dr. Casella for several discussions on the conservation of these various symmetry operations.

[148] Otto Nachtman, *Elementary Particle Physics: Concepts and Phenomena* (Berlin: Springer-Verlag, 1990), Chapter 26: 1.

[149] Kelly Committee Report: 36-38.

perhaps even greater utility to the practicing spectroscopist. Three volumes of atomic energy levels were published. Volume I, published in 1950, was for elements up to atomic number 23. Volume II, in 1951, covered elements 24 through 41, and finally Volume III, in 1956, covered elements 42 through 57 and 72 through 89. In a similar vein of providing reference data for science and technology, the division published tables of complex spectra listing over 6700 radiations from singly and doubly ionized chromium. A similar publication gave the intensity of spectral lines for 70 elements to be used for identification purposes. Other publications gave a series of wavelengths to be used as standards of wavelength in the infrared, as well as the hyperfine structure of technetium. In a sense, the publication of these data collections was complementary to the publication of *Nuclear Data* as Circular 499 in 1950, with a supplement in 1951. Containing data on half-lives, radiation energies, relative isotopic abundance, nuclear moments, cross sections, and nuclear decay schemes, these tables became the bible of the nuclear physicist. Finally, in an unusual effort for spectroscopy, the division made one of two accurate determinations of the speed of light. The other came from a more expected quarter, the Central Radio Propagation Laboratory.

Spurred by the wartime development of crystal diodes and the recent invention of the transistor, the division in 1949 began a program in the study of semiconductors. But germanium and silicon, the materials that were to become the economic standbys, were never studied in the fifties. Rather, the program was concerned with the more general questions of the transition from electron to hole conductivity, the measurement of mobility, and the effect of lattice defects. The first materials studied were rutile (titanium dioxide, TiO_2) with some of the oxygen removed, and grey tin, for which there were great problems in the preparation of macroscopic single crystals. Experiments concentrated on photoconductivity and rectification, the latter being achieved with TiO_2. Of note was an attempt to study the crystal imperfections in TiO_2 by the study of internal friction, a direction which was to have important scientific consequences later in the Mineral Products Division. Toward the end of the period the work shifted to become closer to commercial practice with the study of intermetallics. These were compounds of antimony or arsenic with indium, gallium, or aluminum—the so-called "three-five" compounds, from the numbers of the columns of the components in the periodic table. In 1957, the material worked on was indium antimonide.

Because of widespread interest in very high-energy x rays, the Bureau by 1952 had purchased and installed a betatron (50 MeV) and a synchrotron (180 MeV). With these two major instruments the Bureau both continued and expanded its work in x-ray protection and was led into nuclear physics. Thus, throughout the fifties, work continued in radiation protection and monitoring. The design and construction of what are now the ubiquitous radiation-monitoring film badges were worked on, as were means of shielding against x rays. The efficacy of concrete barriers was a constant concern, and much experimental and theoretical work was carried out. A sentence in the Annual Report for 1952 gives the aim of this work: "Ultimately, the accumulated data will form a basis from which radiation barriers of the correct thickness and material may be designed for economical and safe protection." As part of the effort, the Bureau published Circular 583, *X-Ray Attenuation Coefficients from 10 Kev to 100 Mev,* which gives attenuation coefficients for twenty-three materials, including air, water, and concrete. Finally, in 1954, Handbook 50, *X-Ray Protection Design*, was published.[150]

All this work on x-ray shielding inevitably led to questions of the scattering of x rays by atomic nuclei, and the Bureau, with its betatron and synchrotron, was well placed to carry out such studies. Thus, with the hiring of a group of bright, young scientists, the Bureau became a world-renowned center for photonuclear research, with a great deal of the work supported by the Atomic Energy Commission (AEC). In this work, the first observation of a resonance in the elastic nuclear scattering of photons was made, leading to information about the nuclear energy level structure. In a similar vein, as part of a study to provide data for nuclear-structure theorists, the angular distribution of the monoenergetic gamma rays emitted from the carbon nucleus when irradiated with high-energy x rays was determined. In 1957, the study of π° mesons from carbon led to information on the distribution of nuclear matter in the nucleus, and a correlation was shown between the photoneutron yield from a given nucleus and the deformation of the nuclear surface, thereby opening a new approach to the study of nuclear quadrupole moments. By the end of the fifties, the Bureau was solidly involved in nuclear physics research.

Along with this x-ray work there was a related effort in the standardization of measurements in radioactivity, both to determine the correct activity level and for safety reasons. The rise of nuclear reactors made available a number of artificially produced radioactive isotopes, and these were becoming more widely used in medicine, science and industry. Thus, in 1950, the Bureau issued standard samples for the standardization of the measurement of activity of such samples. To check the accuracy of radiation meters, the Bureau constructed a portable cobalt-60 source. In the laboratory several calorimeters were built to measure the intensity of low-level radioactive sources on the one hand, and of high-voltage x-ray sources on the other. And the radiation measurement system had to be checked against its international counterparts. In 1953 the national primary x-ray standard was transported to the National Physical Laboratory in England and compared with the British national x-ray standard. Two discrepancies found in the British standard caused a redesign of that standard. A later comparison of the U.S. and Britsh standards gave improved results. Similarly, the primary British and Canadian radium standards were transported to the Bureau for comparison. While the British and U.S. standards compared satisfactorily, the Canadian differed by 0.5 percent from its assigned value.

The area of mass spectrometry merits mention because of its subsequent development. Throughout the whole period, the division carried out work in this field, including the development of lightweight, portable mass spectrometers. The Bureau became so well recognized for these measurements that, in 1956, it began a program to issue standard samples for isotopic abundance, an important matter for workers in geology and geochemistry. The samples were to be issued internationally as well as domestically. Eventually this led to the Bureau achieving a unique position in the measurement of isotopic composition, and hence the determination of atomic weights.

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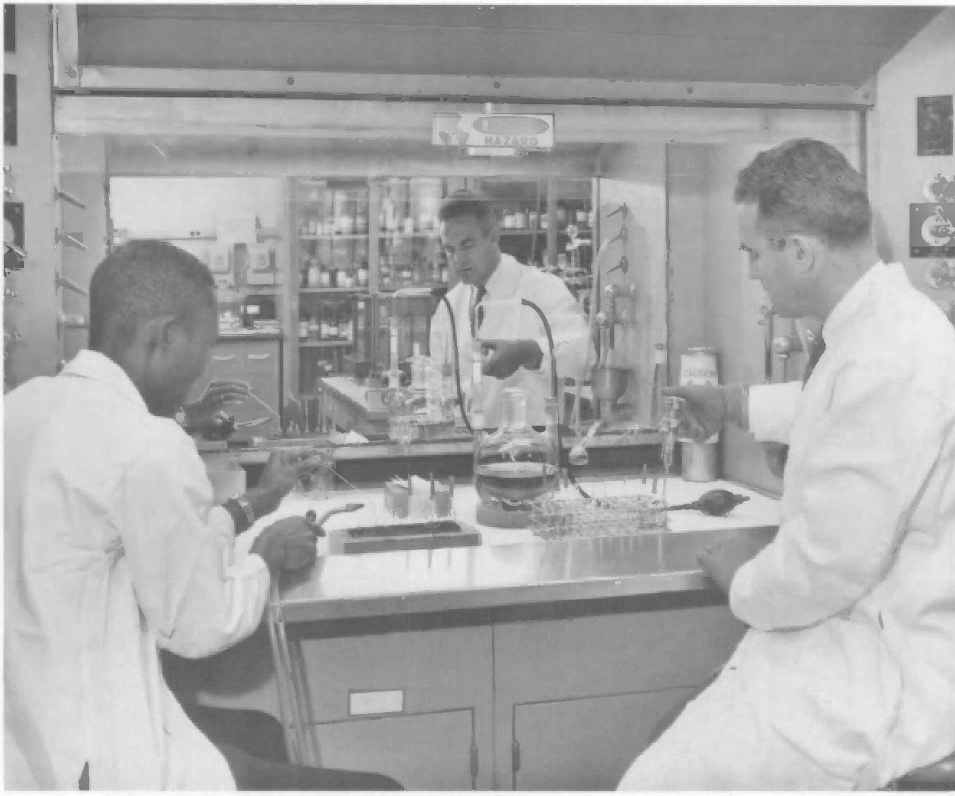
[150] Other NBS Handbooks on related topics were: No. 52, *Maximum Permissible Amounts of Radioisotopes in the Human Body and Maximum Permissible Concentrations in Air and Water*; No. 53, *Recommendations for the Disposal of Carbon-14 Wastes*; No. 55, *Protection Against Betatron-Synchrotron Radiations Up to 100 Million Electron Volts*; No. 56, *Safe Handling of Cadavers Containing Radioactive Isotopes*.



Robert W. Medlock (left) and William F. Marlow of the Radioactivity Section filled and sealed ampoules with the benzoic acid (7C-14) standard sample (in center flask) for liquid-scintillation counters.

The Electron Physics Section was under the direction of Ladislaus L. (Bill) Marton who, among other achievements, was the organizer of the Division of Electron Physics of the American Physical Society—the first division of that society. Marton's avowed aim for the section was to do everything with electrons that had been done with light. Thus, one of the achievements of the section was the construction of an electron-beam interferometer, followed by an electron spectrograph. With the main aim being better understanding of the processes by which electrons lose energy upon impact with atoms, the latter instrument measured the angle of scatter and was capable of detecting a loss of 5 eV from an initial electron energy of 50 keV. Since this instrument was limited to small angles, a variation was developed that could measure scattering angles up to 100°. Automatically measuring every $^1/_8$°, the instrument could give energy loss and intensity over the whole range in one-half hour. First used to study copper, aluminum, beryllium, and gold, the instrument was later modified to study low-pressure gases.

John Simpson adjusted an electron-beam interferometer, a device that utilized electron beams to produce interference fringes in much the same way that conventional optical interferometers used light beams. The instrument employed diffraction from an extremely thin crystal as a means for splitting and recombining an electron beam. Three crystals were mounted in a vacuum chamber (center) placed between the illuminating and viewing systems of a conventional electron microscope. Each of the crystal mounts could be rotated around the optical axis. The mount of the first crystal could also be translated along the optical axis. The controls for these motions were brought out at the bottom of the evacuated chamber.

In another form of data compendium, the section published *Electron Physics Tables* in 1957.[151] Covering the energy from 0.206 eV to 3.353 TeV, and calculated with the help of SEAC, the NBS automatic digital computer, these tables gave the potential difference necessary to accelerate an electron to a specific kinetic energy (V), the effective relativistic potential energy (V^*), the product of magnetic field and radius of curvature of an electron in a magnetic field ($H\rho$), the deBroglie wavelength (λ); momentum in units of m_0c (p^*), kinetic energy in units of the rest energy (E^*), total energy (W^*), and the ratio of speed to the speed of light (β).

With programs in electron physics, where the avowed aim was to do everything with electrons that could be done with optical instruments, and with the study of negative ions, the Atomic and Radiation Physics Division was a scientifically productive unit.

CHEMISTRY

Identified as Division III, the Chemistry Division with a staff of two in 1903 was the third one formed at the Bureau. By 1953, under Edward Wichers, it was a large division with a staff of 181 (147 professionals) organized into eleven sections. Since the standards aspect of chemistry is largely concerned with the composition of materials, the division was heavily involved in developing methods of chemical analysis and the analysis of materials for special purposes. It also produced a large number of standard samples, many for the composition of commercial metals and alloys. In a related activity, it was concerned with the preparation of very pure substances and had programs in purification and separation. Its analytical effort was by no means limited to solids and liquids, but extended to the difficult field of gas analysis. It covered the areas of inorganic and organic chemistry, but only in specialized areas of these immense fields. It had important work in areas of surface and colloid chemistry, physical chemistry and, particularly, thermochemistry.

Two areas of work, organic coatings and electrodeposition, were rather specialized and took place at the Bureau for historical reasons. The Bureau became involved in the first of these in 1914 when it took over the function of acceptance testing of paints and varnish—among other commodities—from the contracts laboratory of the no-longer-existent Bureau of Chemistry of the Department of Agriculture.[152] In 1953 that work was carried out under Paul T. Howard in the Organic Coatings Section. Commodity testing was still performed, but by 1955, routine testing had ceased, and the work changed to the more valuable development of test methods and specifications for use by all agencies which purchased paints and other coatings.

The electrodeposition work existed at the Bureau only because of the vigor, stamina and technical inventiveness of William Blum, the section leader until 1954. Called in 1913 to the Bureau of Engraving and Printing on a troubleshooting effort on the

[151] L. Marton, C. Marton, and W. G. Hall, *Electron Physics Tables*, Natl. Bur. Stand. (U.S.) Circular 571; March 1956.

[152] Kelly Committee Report: 41; MFP, 155.

BPE's electrotyping baths, he quickly solved the problem. Struck by the lack of methods to control the baths, and the paucity of the literature on the topic, he decided that electrodeposition was a field ripe for scientific exploration. Thus began a fruitful and productive career.[153] Blum was followed by Abner Brenner, an equally ingenious and entrepreneurial scientist. Unhappy with a program that was built on the interest and ingenuity of one man, the Kelly Committee felt that this "level of effort . . . appears out of proportion to the entire program of the Chemistry Division."[154] In 1961, the section was moved into the Metallurgy Division.

The analytical chemistry work of the division was partly devoted to the development of new methods of analysis and partly to the analysis of specific materials—mostly metals. Because of their inherent speed, the various techniques of spectrochemistry were used for as many purposes. Flame photometry was used to analyze calcium in sugar solutions, and spectrographic methods were used for bismuth, tin in bronze, and magnesium in cast iron. The crucial matter of the effect of excitation conditions on the intensity of spectral lines was studied and brought under control. Further spectrographic methods were employed for impurities in nickel used for cathodes in electron tubes, for the analysis of complex dental alloys, for trace elements in portland cement, and for alkalies in refractory materials.

Not performed spectrochemically, but notable because of its use of a rapidly emerging analytical method, was the analysis of cobalt-based alloys for jet-engine turbine blades, the so-called "superalloys." This was done by the isolation of the alloy components—cobalt, nickel, iron and manganese—by chromatographic separation on ion-exchange resin columns. The method was further applied to the separation of niobium, tantalum, titanium, tungsten, and molybdenum as they occur in stainless steels. Further analytical methods based on separation were developed for determining the composition of complex dielectrics—mostly titanates—used for proximity fuzes. Finally, a chromatographic method on a commercially activated carbon was developed for the analysis of corn syrup. When coupled with analyses performed for the issuance of new standard samples—such as for the composition of a jet engine alloy, and the composition of white iron—and for the reissuing of existing ones, it is clear that this was an active and productive research effort.

Closely associated with analytical problems is the production of very pure substances. At the time of its formation, the division was concerned with "a study of the standards of purity for chemical reagents and of the methods to be used for the quantitative determination of small amounts of impurities in such reagents,"[155] and that concern still existed in the fifties, but was not limited solely to reagents.[156] Again, work went ahead on the development of general methods of purification and on the production of specific pure materials, often at the request of another agency. One of

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[153] MFP, 128.

[154] Kelly Committee Report: 41.

[155] Annual Report, 1906: 14.

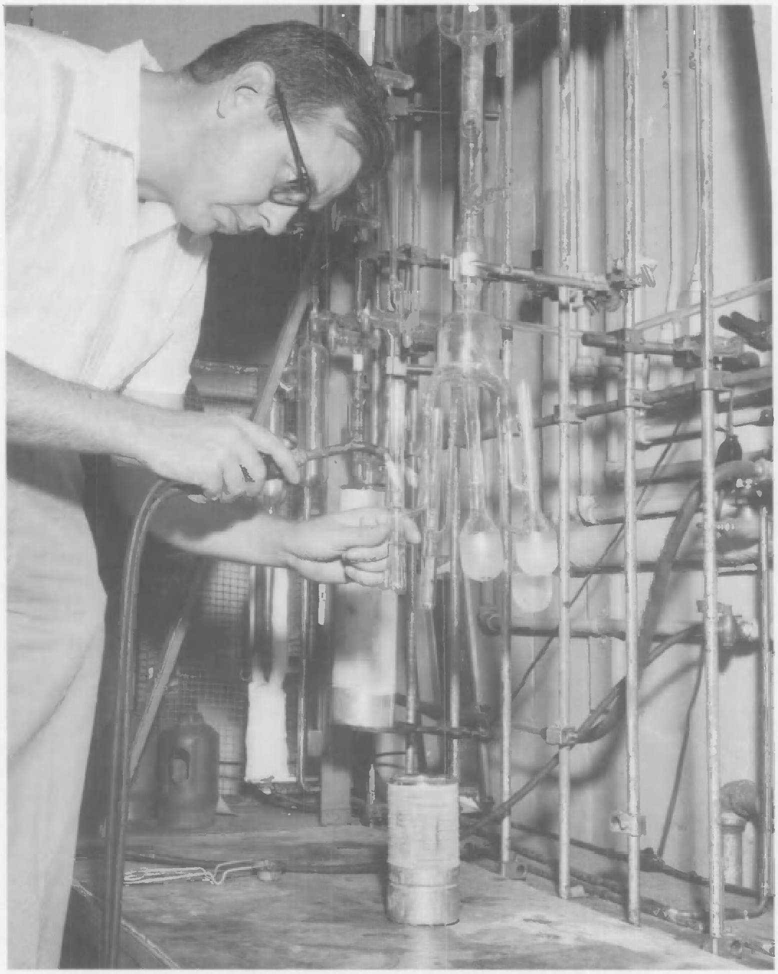
[156] MFP, 379. The Bureau's history in the production of pure materials stood the Nation in good stead in the early days of the Manhattan Project when the Bureau devised a method of purifying graphite for the nuclear pile.

the purification methods studied was crystallization. Noting that purification is often achieved by repeated crystallization, its inverse, fractional melting, in which supernatant melt is periodically withdrawn, was developed by division chemists as a means of concentrating impurities, hence making the determination of their amount and their identification easier. In true purification, the by-now-commercial zone refiner was adapted to the purification of low-melting-point solids by constructing a bath of two immiscible liquids, the upper held at a temperature above the melting point of the material to be purified and the lower at a temperature below. The interface between the two liquids provided a sharp temperature gradient, and passage downwards through it of a tube of the material to be purified provided the zone refining. But a more interesting discovery came in 1957. Working with the production of single crystals of ammonium phosphate from aqueous solutions contaminated with chromium, it was found that when growth is normal to some crystal faces the impurity enters the crystal, while for other faces it does not—or at least its entry is much reduced. Thus the control of the crystal growth direction can produce a purer and more perfect crystal. In the preparation of specific pure substances, the chromatographic column again proved its worth. Such methods were used with ion-exchange columns to prepare nearly all of the rare earths with a purity of 99.99 percent. Other purifications by this and other methods led to the preparation of several very pure titanium salts and several titanates.

Gas analysis programs developed some important new methods and, as a harbinger of things to come, broke into a new field. One of the important new methods was the detection of impurities in hydrogen by thermal-conductivity measurements. By this method, one part in 10 million of any other gas could be detected and the apparatus could give direct readings from any of fifty sampling sites. Another important new method was the spectrometric analysis of high-purity gases. A glow discharge in the flowing gas was excited by a high-frequency field and the emitted light was observed by an automatic scanning photoelectric spectrometer. Useful for flowing gas, the apparatus could detect concentrations of one part per million of hydrogen, nitrogen, and water vapor, and was obviously useful for flowing gas streams.

The new area where the division began work was air pollution. In 1950 the Bureau appears to have made its first attempt to work in this field. At that time, the Bureau, working in Los Angeles, concentrated smog components by collecting them on a filter at liquid-oxygen temperatures. They were then separated by isothermal distillation and the fractions analyzed by mass spectrometry. About sixty compounds were positively or tentatively identified. Most seemed to be hydrocarbons or reaction products of hydrocarbons with ozone or nitrogen dioxide. Thus the Bureau entered into the field of air pollution and interaction with other agencies of the Federal, state and local governments. As the Nation became more serious about the control of air pollution, the Bureau's activities in this area increased.

Aside from composition, another area of chemistry where the Bureau provided standards was that of acidity. In that activity it sold standard samples certified for a given pH, or hydrogen-ion concentration. Work throughout the period was designed to improve the accuracy of the standards and to expand their utility. In 1950, for example, four new standards were issued to improve the accuracy at the low and high ends of the pH scale, and in 1953 the top of the useful temperature range of the



As the final step in purification of a substance by fractional melting, Gaylon S. Ross sealed off an ampoule containing the purified sample (1957).

standards was increased to 95 °C. But perhaps the most interesting development was the concern with acids and bases in nonaqueous solvents—an area of interest to the petroleum, food and drug industries, and to several scientific fields, among them analytical chemistry and electrodeposition. The behavior of acids and bases in nonaqueous solvents was puzzling in that there was no constancy in the order of relative strengths as some nonaqueous solvents were substituted for water. But there began to appear a relationship between the structure of individual acids and their behavior in different kinds of solvents.

The division had a strong program in thermochemistry. Experimental work was carried out on the determination of heats of combustion, of formation, and of other reactions, usually on compounds of particular interest at the time, or on series of compounds, such as different isomers or homologous series of organic compounds, where basic questions, such as the effect of *cis-trans* isomerism on reaction energies, could be addressed. Thus, for example, measurements of the heats of formation of various boron compounds were carried out to "aid in understanding these relatively new compounds in which the usual laws of chemical bonding do not apply." Similarly, data were obtained on various series of hydrocarbons, examples of which are the heats of formation of *ortho-*, *meta-*, and *para-*tert-butyltoluene to obtain information on steric effects, and on all the pentenes and pentadienes, and some hexenes to obtain the effects of alkyl substituents on double bond energies "which are valuable in synthetic-rubber development."[157] But the program went far beyond laboratory measurements. A file was kept on all publications in this field, and in 1952 a massive publication was announced. "In the continuing project on the collection and critical evaluation of the chemical thermodynamic properties of substances . . . a milestone was passed with the publication of a 1200-page circular (C500) entitled *Selected Values of Chemical Thermodynamic Properties.*"[158] Containing values of heats and free energies of formation, entropy, heat capacity, and heats and temperatures of transition, fusion, and vaporization for all inorganic compounds—where data were available—and organic compounds of less than three carbons, it was periodically supplemented by looseleaf tables. Circular 500 became the bible of thermochemists and was immensely useful to chemical engineers in process design.

Circular 500 was not the only data issued by the division. Basic data on the infrared spectra of some 15 000 chemical compounds—their fingerprints, so to speak—were prepared with the cooperation and financial support of the National Research Council, and distributed to more than 200 cooperating laboratories in 1955. In the next year, the division published Circular 566, *Bibliography of Solid Adsorbents, 1943 to 1953,* a 1500-page volume containing nearly 14 000 scientific abstracts. These publications, along with the others mentioned in other sections, continued the Bureau's long tradition of publishing such compendia as a service to industry and science. This service was to become a formalized activity of the Bureau in the National Standard Reference Data System.

The Kelly Committee had pointed out that the organic chemistry program of the Chemistry Division was insufficient to keep pace with industrial developments. This was true, although it could be argued that it was impossible for any single organization to keep up with the explosive growth of the postwar chemical industries. There was, however, one area in which the Bureau not only kept up with industry and science but was in fact a world leader. This area was that of carbohydrate chemistry, and specifically the synthesis of sugars with radioactive carbon-14 in a specific location in the molecule. In fact, except for some early work in 1951 on the purification of a large number of hydrocarbons and sulfur compounds obtained from other laboratories and thence sold as standard samples, and some work in 1957 on the relative strength of

[157] Annual Report, 1953-1954: 38.

[158] Annual Report, 1952: 26.

After final crystallization, Benjamin Bruckner placed position-labeled radioactive sugars in the bottom of small weighing bottles in amounts of 10 mg to 100 mg, and the exact weight of each sample was determined. The samples were then distributed in the weighing bottles to other laboratories.

aromatic carboxylic acids in benzene, all the Bureau's organic chemistry research was on the synthesis of such sugars.[159] In work sponsored first by the AEC because they were "useful in biology, where scientists are interested in discovering the mechanism by which a molecule becomes either a source of energy or contributes to the structure of living cells,"[160] the Bureau not only synthesized a large number of such labeled

[159] For an account of the Bureau's abortive efforts to create an industry to produce levulose and other rare sugars, see MFP, 265-266.

[160] Annual Report, 1952: 24.

sugars, but also became a supplier of them to laboratories engaged in such work. It clearly had been planned that industrial organizations would manufacture and sell them but, except for two substances, this did not occur. The synthetic methods were simply too complex and expensive. It is clear that the AEC came to the Bureau for this work because of the competence of Horace S. Isbell, its leader, and he in turn was involved in this work because of the Bureau's long history in sugar chemistry, dating from the days of its involvement in determining the sugar content of solutions by polarimetry for customs purposes.[161]

A related area made more important by the outbreak of the Korean War was that of the compound dextran. Formed by the action of the bacterial agent *Leuconostoc mesenteroides* on glucose, dextran was a polysaccharide with utility as a blood extender, and hence a substitute for plasma. Quite a bit of work borrowed from polymer chemistry went into the determination of molecular weight and molecular weight distribution, with good success. Along with its use as a plasma substitute, dextran, when cross-linked, found industrial use as a "molecular sieve" for the separation of polymers.

The division's program in surface chemistry might better be called one in colloid chemistry. Clearly concerned with keeping up with the rapid shift in industry from soaps to synthetic detergents, the division carried out a program on the characterization of the colloidal state of detergents in solution. Using the polymer chemistry techniques of viscometry and light scattering, the size and shape of micelles formed from anionic, cationic, and neutral detergents were determined. But these were complicated micelles because, in the ionic detergents, they had a charge, thus being more analogous to polyelectrolytes than neutral polymers. The charge was determined by electrophoresis, and much interesting and important information was collected.

The work in electrodeposition was for the most part concerned with the deposition of difficult metals, much of it supported by military agencies. Aluminum and molybdenum were of particular interest, but other metals, such as zirconium, titanium, and beryllium were also of concern. Since none of these can be deposited from aqueous solution, practically all the work was concerned with the important questions of deposition from nonaqueous solutions and fused salts. It was interesting work, more in the nature of process invention and development than research, but had no evident connection to the Bureau's standards and measurement functions, which probably bothered the Kelly Committee.

During the period 1950-1957, there was no obvious change in the program of the division. Its work had kept up with the needs of the times but, at least in organization, it remained a large and somewhat disparate collection of work. That this began to be of concern to Bureau management is shown in the method of describing the division's work in the Annual Report. Up to 1956 the reporting was done by the eleven division sections, or even subdivisions of those sections. In 1956 there was a definite change with the work reported in six categories that were only slightly related to the division organization. And in 1957 the work was reported in only four categories, three

[161] MFP, 151-152, 265.

In this apparatus developed by NBS for the electrodeposition of molybdenum from fused salts, the molybdenum was deposited from a solution of potassium hexachloromolybdate (III) in a molten mixture of alkali halides. Heat was supplied by an induction furnace (center). The glass cylinder projecting out of the furnace contained the molten mixture within a graphite crucible. An argon atmosphere was used in the glass cylinder to prevent contamination.

of which were discipline oriented: inorganic and analytical chemistry, physical and electrochemistry, organic chemistry, and air pollution. This was a harbinger of the changes to come when the division would be divided in two in 1960, and another step would be taken in implementing Astin's response to the Kelly Committee Report.

MECHANICS

Involved in research on the mechanics of solids, liquids, and gases, the Mechanics Division, under its chief, Walter Ramberg, also worked on the mechanics of structures, in fluid mechanics, in the measurement of sound and vibrations, and in the measurement of various mechanical properties of materials. It had custody of mechanical standards such as those of pressure and flow rates in fluids, and calibrated flow meters, strain gages, microphones, and related instruments. More important, it had custody of the standard kilogram and calibrated weights and measures of capacity. It also undertook the acceptance testing of structural materials, mechanical appliances, and instruments for other government agencies, industry, and the public. In mid-1953 it was a large division with a total staff of 194 (129 professionals) organized into seven sections,[162] two of which, Hydraulics and Mass, the Kelly Committee found in need of attention. A full 69 percent of the division's work was supported by other agencies, with the military and the National Advisory Committee for Aeronautics the main sponsors.

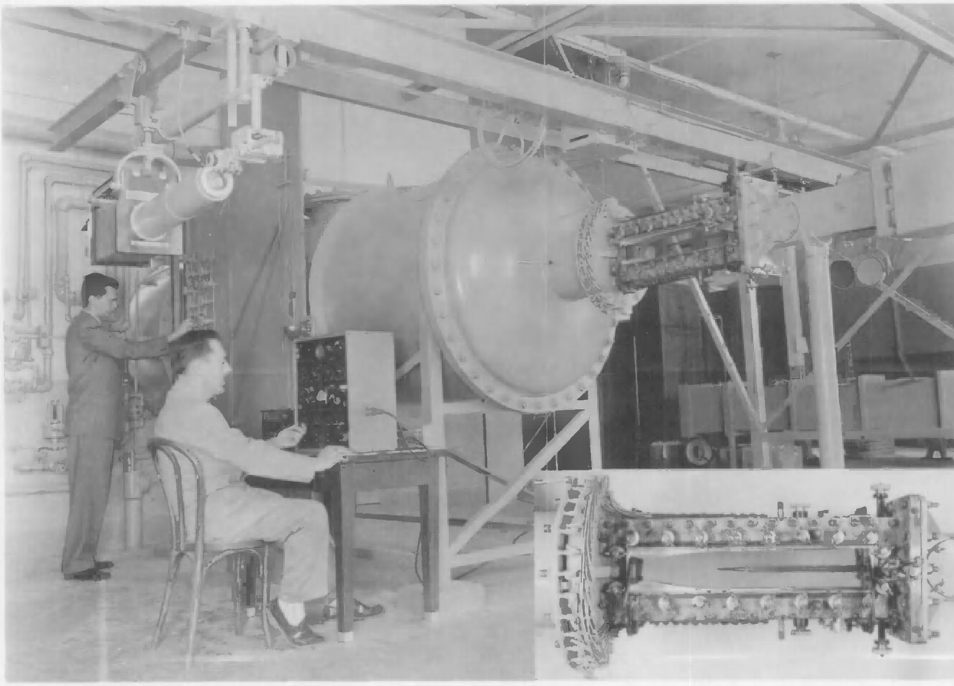
Considering its capabilities and its sponsors, it is not surprising that a large thrust of the division's work was concerned with high-speed jet aircraft and rocketry problems. This work continued throughout the whole period with three main directions: air flow past a surface and turbulence in the boundary layer, the response of aircraft structures to imposed loads, and the use of the Bureau's electronic computer, SEAC, for various engineering calculations.

The study of turbulence made good progress in a very difficult field. It featured the use of a hot-wire anemometer to measure velocities, density, and temperature of the flowing air at velocities up to twice the speed of sound. The sensing element of the instrument was made from platinum-rhodium alloy wire only 1.2 μm in diameter. Since this wire was tiny, the frequency response was enormous, approaching 70 kHz. Its small size meant that rapidly varying quantities could be measured in a small region and mapped out by moving the sensor. One of the first discoveries was that the transition from a turbulent boundary layer to the laminar-flow region above it was diffuse rather than sharp, with fingers of turbulence reaching up into the laminar region. In this transition region, the flow is alternately chaotic and laminar. Turbulent spots first form, then grow as they move downstream, and it was possible to measure their "rate of propagation, shape, and other significant features."[163] It was also found that surface roughness influenced the transition layer, with greater roughness inducing a transition at lower velocities, but this was also influenced by the nature of the roughness.

The work on aircraft structures was partly theoretical and partly experimental, the latter sometimes concerned with matters of aircraft performance or durability and sometimes with matters of aircraft production. Thus, driven by the industrial need to

[162] The sections were Sound; Mechanical Instruments; Aerodynamics; Engineering Mechanics; Hydraulics; Mass; and Capacity, Density, and Fluid Meters. A year later Hydraulics was abolished, Aerodynamics was changed to Fluid Mechanics, and a new section, Combustion and Controls, was added.

[163] Annual Report, 1955: 49.



The NBS supersonic wind tunnel produced wind velocities up to twice the speed of sound. Inset: Close-up of the test section shows the hot-wire anemometer probe mounted in the central part. Direction of air flow is from left to right. The glass sides of the test section permit schlieren pictures to be made of the air flow.

determine the formability of high-strength aircraft sheet, a photo-grid technique for accurately determining elongation was developed, and a light, stiffened skin composed of two facing sheets separated by spacers was developed. The mechanical properties of aircraft structures of most concern were fatigue and creep, the latter driven by the high temperatures generated in the aircraft skin during operation at high speeds. The main aim of the work, and one that underlay all this research, was not merely to measure the common mechanical properties of materials, but to see how materials behaved in structures with their inevitable rivets, windows and doors, and various other types of stress raisers. In fatigue, for example, the fatigue strength of beams with rivet holes in them was measured and compared with the strength of smooth specimens, and in 1955 a test program was begun to determine the fatigue life of aircraft beams under the spectrum of loads typical of aircraft maneuvers.

The theoretical work developed into the use of the SEAC in aircraft engineering problems. An example was the use of the computer to calculate the deflection of tapered cantilever beams per unit load—a calculation that took three minutes on the machine, but which would have taken two days to do by hand. Other uses were in the structural analysis of delta wings; in vibration problems for computing the normal modes of aircraft, using a model with 63 degrees of freedom; in the calculation of temperature distributions, again driven by concern for high-speed flight; and in analytically evaluating the load-carrying capacity of the whole structure prior to testing.

After its formation, the Combustion and Controls Section was also involved in aircraft work with the study of jet-engine combustion and the development of a combustion chamber, and on jet-engine controls, particularly thermocouples which are used to prevent the occurrence of dangerously-high temperatures at the jet-engine exhaust.

When considered with additional projects, such as the measurement of the moisture content of the atmosphere at high altitudes; the development of n-heptane as a standard metering fluid for aircraft carburetors; and the instrumentation of a control stick to determine the forces exerted by the pilot, it is clear that a large part of the division, while doing valuable work, was almost an adjunct of the military and civilian government aviation agencies. There was no indication that the situation was about to change.

In addition to the work on turbulence just described, the division carried out research on flow in liquids. In particular, there was considerable work on aspects of liquid flow in bodies of water in different situations. Three main problems were addressed over a number of years: the effects that occur when a dam breaks; the effect of wind in causing waves in shallow bodies, driven by concern for the effect of hurricanes in such bodies of water as Lake Okechobee and shallow reservoirs; and density-driven flows at the mouth of estuaries where salt water mixes with fresh. In each case, both theoretical and experimental approaches were taken, with generally important results. For example, in a dam break it was found that the roughness of the surface of the valley through which the water rushes is important in controlling the flow, and that at the beginning of the flow from the break, the viscosity of the water rather than the resistance of the turbulence, is important in determining the flow. In the wind-wave coupling problem, it was possible to "illuminate certain aspects of energy transfer"[164] from wind to waves, and thus provide a means for estimating the magnitude of waves and tides. Finally, in the density-driven flow problem, it was possible to estimate the effect of such properties as density differences, river velocity, depth of channel, etc., on the characteristics of the density-current flow.

The division had a vigorous and productive program in sound and its sibling, vibration. A large part of it consisted of the unique Bureau functions of calibration of instruments, primarily microphones and vibration pickups, but it extended into other areas of the science of acoustics. In the calibration of microphones and pickups, the general thrust of the work was to increase the frequency range over which the services could be provided, and to measure high-level noises such as found near jet engines. Thus, while work was started to extend the upper frequency limit of the calibration range of microphones to 100 kHz, the range of microphones having absolute calibrations was extended from 50 Hz to 10 000 Hz up to 1 Hz to 20 000 Hz. The main problem in the measurement of jet noise is that the sound level is so high that microphones can be damaged. To measure such loud sounds, a null method was devised to protect the sensitive microphone, and the same arrangement proved to be useful as a sound source in calibrating vibration pickups. Since noise from a jet engine can be loud enough to cause hearing damage, the Sound Section began to look for ways to reduce jet noise. A method was developed to measure the total sound energy produced by a jet engine which was then applied to models of jets and to various devices proposed to reduce the noise level.

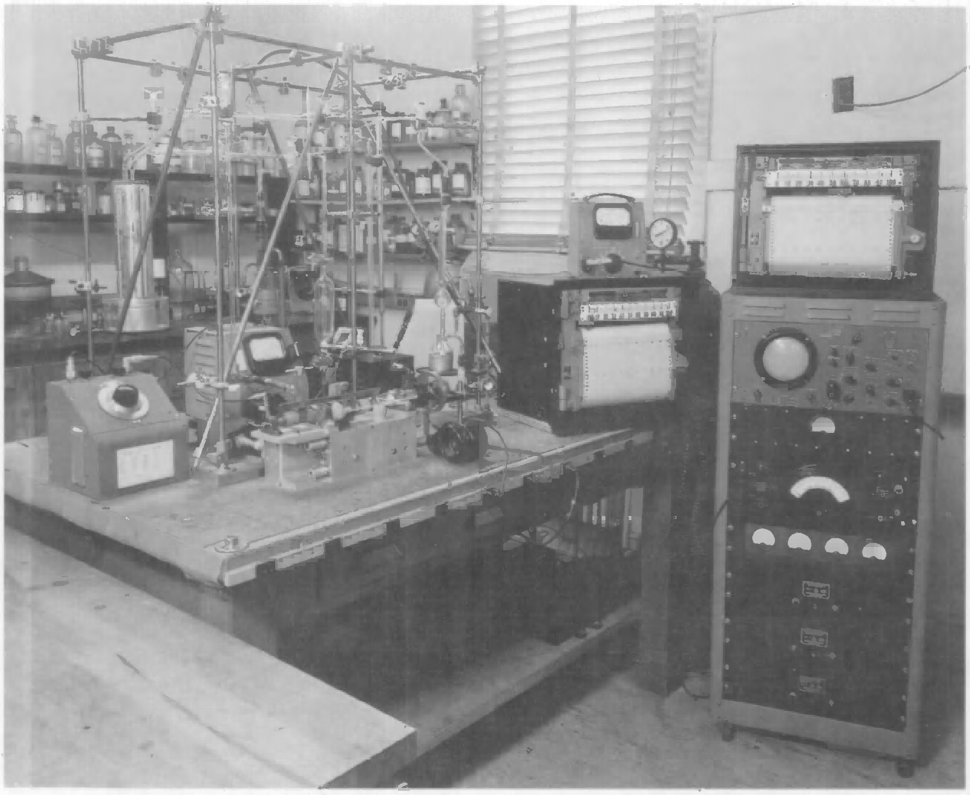
[164] Annual Report, 1955: 48.

The measurement of sound velocity, as might be expected, was an area of some activity. Careful measurement of sound velocity in monatomic gases found a very small degree of dispersion; it was the first time such observations had been made. In a completely different application, an instrument was developed for the continuous determination of sound velocity in sea water. Sound was also used to measure physical properties of materials, leading to the production of an instrument that continuously measured the viscosity of gases. Sound measurements are also necessary for architectural purposes, and in the area of architectural acoustics, improved methods of measurement of transmission loss through walls were developed, as well as improved methods for the measurement of reverberation. Not only was sound investigated, but also its physical perception, or hearing. Again, the emphasis was on measurements and their calibration. There was a considerable difference between the British and the U.S. standard sound pressures for the threshold of hearing, with the British standard being a full 10 decibels lower than the U.S. standard. To investigate this difference, a program to measure the sound pressure in the ear canal at the threshold of hearing was started on 100 persons and eventually led to the study of measuring the acoustic impedance of the ear to an earphone. Also concerned with hearing was the calibration of bone-conduction hearing aids. Measurements on a human head led to the conclusion that it would be possible to make a model of a human head, or "mastoid," that could be used for calibrating such devices.

One of the most important quantities derived directly from the basic units of mass, length, and time is force, and its relatives, stress and pressure, are equally important. Hence one of the basic functions of the division was to provide standards for these quantities, and the main thrust of the work was to extend the range over which standards and calibration services could be provided. In force measurements, four 3 million pound capacity compression dynamometers for use in calibrating large testing machines were themselves calibrated. The dynamometers used wire-resistance strain gages as output devices and increased the range over which calibrations could be performed from 2.6 million pounds to 12 million pounds. The same need to extend the scale was felt in pressure measurements. At the high end, a new piston gage to serve as the national standard in the range from 50 000 psi to 200 000 psi was built. At the low end, responding to needs from high-altitude flight, the development of an instrument for accurate measurements of pressures up to two inches of mercury was undertaken. While all this pressure work was going on, the development of the diamond-anvil cell in the Mineral Products Division would revolutionize the attainment of very high pressures.

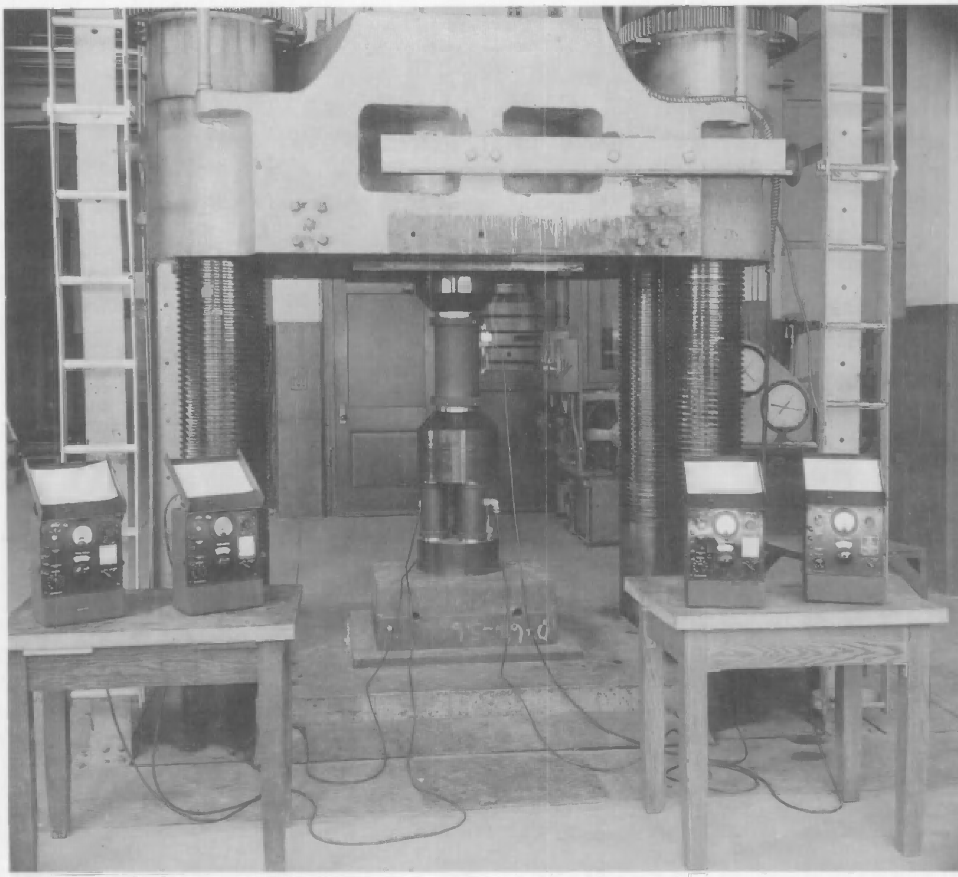
In the Mass Section, the division had custody of the standard kilogram that was the national standard of mass. The section spent most of its time carrying out calibrations which, in the words of the Kelly Committee, "requires great technical skill but relatively little scientific knowledge." The committee went on to point out that "some imagination and scientific knowledge might be introduced in this area."[165] There is no evidence that this was done. In 1950 a re-evaluation of the equal-armed, double-beam

[165] Kelly Committee Report: 45.



Apparatus used by NBS in studies of sound propagation through gases. The superstructure on the table supported equipment for measuring the pressure of gas in the double-crystal interferometer on the table in front of it. The cabinet on the right contained the principal electrical circuits for measuring the phase and amplitude of the sound that traversed the test gas. Instruments for automatically recording the phase and amplitude were above the cabinet and at the right end of the table.

balance was undertaken (results not known), and in 1955 a double-beam balance with a capacity of 1000 lb was constructed. With a sensitivity of about 500 mg, a half-ton weight could be calibrated to one part per million. It was designed to be used by state weights and measures laboratories, and working drawings were made available to balance and scale manufacturers.



This setup in the NBS 10-million-pound testing machine demonstrated the calibration procedure for a 3-million-pound dynamometer. The device was loaded against three 1-million-pound dynamometers which had previously been calibrated.

Materials Research

The bulk of the Bureau's materials work was carried out in three divisions: Organic and Fibrous Materials, Metallurgy, and Mineral Products, representing—at least in the fifties—the major classes of materials in the economy. There was also specialized work on materials in other divisions, such as rheology of polymers and thermodynamic properties in the Heat and Power Division, and dielectric properties in the Electricity Division, but these were relatively small efforts. In 1953, the three materials divisions had a combined staff of 478, of whom 284 were professionals. These three divisions did testing and specifications development but in widely varying degrees. Thus, 17 percent of the total funds in Organic and Fibrous Materials, were expended in these categories; in Metallurgy the funding percentage was 9.7 percent; and in Mineral Products it was a very substantial 40 percent, mostly for acceptance testing of portland cement for the Government.[166] Also showing wide disparity were division sizes and the fraction of transferred funds that each had. The largest of the materials divisions was Mineral Products with a staff of 244, only 121 of whom were professionals. Organic and Fibrous Materials had a staff of 169, but 115 were professionals, and Metallurgy—the smallest of the Bureau's seventeen divisions—had a staff of 68, 48 of whom were professionals. Other agency funds also showed wide disparity, with percentages of 68 percent for Organic and Fibrous Materials, 48 percent for Metallurgy, and 82 percent for Mineral Products.

These divisions were all concerned with the basic problems of materials science: the enhancement of desirable properties, the relation between properties and microstructure, determining degradation mechanisms, measuring and improving durability, and the synthesis of new materials. Of course, the nature of the materials determined the actual work carried out.

Organic and Fibrous Materials

This division was organized partly along materials lines and partly along discipline-related lines. Thus, five of its eight sections were named after materials—Rubber, Textiles, Paper, Leather, and Organic Plastics—and two were more generally oriented—Polymer Structure, and Testing and Specifications. The Polymer Structure Section was the division's basic research section; and the Testing and Specifications Section handled all of the division's testing and specifications work, but the technical personnel come from the other sections. The eighth section was Dental Research, a special case.

The division's work is best described as coming under natural polymers—cellulosic and proteinaceous—concerning materials such as leather and paper, and under synthetic polymers—plastics and elastomers, although the latter includes natural rubber.[167] The development of the synthetic polymer area formed the bulk of the scientific and industrial activity in polymeric materials.

In studies of elastomers and plastics, the principal areas of concern were mechanical properties and their time dependences; failure mechanisms of plastics; various thermodynamic properties, but particularly crystallization; the degradation of plastics; and some synthesis work. Late in the period, with the purchase of an ultracentrifuge, solution studies began seriously, leading eventually to standard samples for molecular weight.

The mechanical properties work was a mixture of basic and applied, the basic primarily in the area of natural and synthetic rubbers, and the applied generally in plastics. This plastics work was largely supported by the military and was concerned with various aircraft problems.

[166] Kelly Committee Report: 48-57.

[167] The division played an important part in the wartime development of synthetic rubber. A good account is given in MFP, 410-414.

In the work on rubbers using polyisobutylene as a model elastomer, the temperature dependence of viscosity was determined to be the same as that of the "retarded elasticity," a result that would later find expression in equations for the temperature dependence of all viscoelastic properties of elastomers. Then, in a study of pure gum vulcanizates—such as a cross-linked elastomer without filler, such as a rubber band—it was found that the ratio of Young's modulus to stress was a specific function of the elongation. Applicable to both natural rubber and a number of synthetic rubbers, as well as to creep in tension, the expression was not applicable to elastomers containing carbon black. In subsequent work, apparatus for measuring bulk modulus at frequencies of 50 Hz to 5000 Hz was developed. The apparatus would be used to study the behavior of rubbers in the low-temperature region in which the rubber converts from an elastic material to a hard plastic.

The work on the mechanical properties of plastics was much more directed. One study was on aircraft canopies, which were made of acrylic. The study was designed to find the source of mechanical failure in the canopies, which crazed extensively in service, especially when laminated with a sheet of polyvinyl butyral to protect against impact damage. The Bureau found that if the acrylic is biaxially stretched, the tendency to craze almost disappears, and the acrylic develops the high-impact properties of the laminate. In a similar vein, in a study of the failure of aircraft windows, it was found that creep under the thermal gradient existing in the windows during flight leads to failure, and that this occurs more readily with thermosetting resins than with thermoplastics. Perhaps the most important results of these studies were the stimulation of research on the nature of crazes and the microscopical study of fracture surfaces. These studies provided important evidence for what was later learned about crazes, namely that, unlike a crack, the two surfaces of the craze are connected by a forest of fine fibrils.

Thermodynamic studies were carried out, particularly on the glass transition and on crystallization. The glass transition of silicone rubber was measured to assess its utility as an elastomer at low temperatures, but a study of the effect of pressure on the glass transition temperature was scientifically more interesting. Contrary to what was later found for this subtle measurement, these early measurements found no effect. The glass transition temperature of copolymers was also investigated, and by analysis of published data, an expression for calculating the glass temperature of copolymers from those of their constituents was developed. But perhaps better known were the division's studies on crystallization in polymers. Such studies demonstrated crystallization in the various forms of natural rubber and other polymers, and found that a melting point exists. The determination of its actual thermodynamic value would, however, depend upon techniques developed elsewhere in the Bureau.

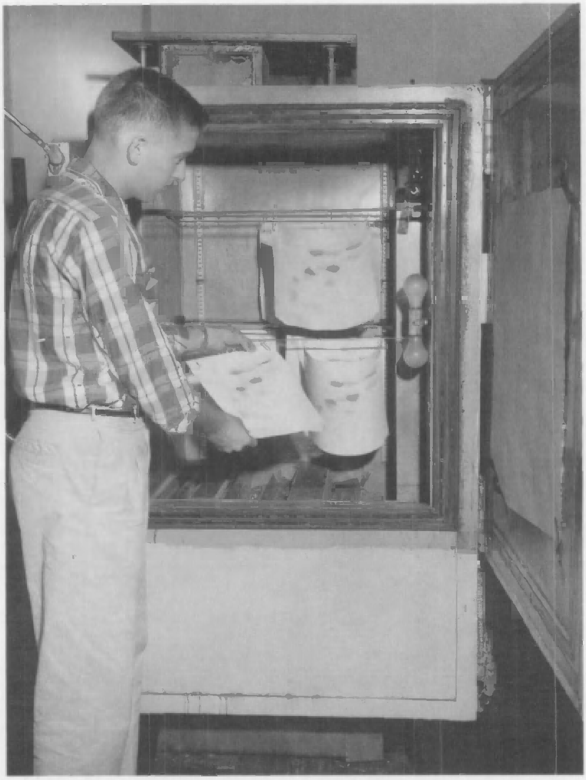
Degradation of polymers was a concern, but under specific conditions. The action of temperature, oxygen, and ultraviolet radiation on polystyrene—one of the components of GR-S (Government Rubber-Styrene, a copolymer of butadiene and styrene) synthetic rubber—was chosen. Using ultraviolet, infrared, and mass spectrometry to detect reaction products, it was possible to deduce the reactions involved and to postulate mechanisms for them. This work developed into a study of polymer behavior under the action of high-energy radiation to attempt to deduce why some polymers are degraded by this treatment while others had their properties enhanced. Using a new

2520-curie cobalt-60 source for the studies, it was found that reactions proceed by free radical intermediates, and the course of a reaction could lead to desirable cross-linking, or in other cases to undesirable depolymerization. As a corollary to these degradation studies, new polymers (generally fluorine-containing) with elastomeric properties for use at high temperatures were synthesized. Driven by aerospace concerns, this work was to continue for some time.

Finally, somewhat as a legacy from the division's work on synthetic rubber during World War II, there were a number of other studies in rubber, both natural and synthetic. A program for the standardization of Government synthetic rubbers was begun involving methods for chemical analysis and physical testing. The analysis of mixtures of GR-S synthetic rubber and natural rubber was accomplished by thermally decomposing the rubber and using infrared spectroscopy to analyze the decomposition products. In a somewhat unrelated activity, the heat of vulcanization of natural rubber was determined by adiabatic calorimetry.

The division's work in natural polymers and the products derived from them—leather, paper, textiles—grew out of the Bureau's work in acceptance testing for Government purchases. This led to the development of many tests for often specialized properties—fold endurance of paper and abrasion resistance of textiles are examples—used in such testing, and in specifications for these materials. And one of the major concerns was durability and the laboratory means for assessing it. By 1950 the division was expert in all these subjects, as attested by its work in preserving the Charters of Freedom. Along with, and supporting this work, were more fundamental studies on various aspects of the natural materials involved, such as cotton, silk, wool, and collagen.

During the period, there was work on the structure of collagen and cotton. In cotton, the aim of the research was to determine both the external and "internal" surface areas. The latter includes the interface between crystallites and amorphous regions in the model of cellulose structure generally accepted at that time. Measurements of those areas by nitrogen adsorption at liquid nitrogen temperatures on untreated cotton, and on cotton which had been swollen with water and then had the water removed by a process of solvent exchange, showed that the internal surface area can be up to eighty times the external surface area. It varied greatly, however, depending on the treatment of the cotton. In collagen studies, the amino acid composition was determined by paper chromatography, replacing older and far more tedious techniques. Seventeen of the eighteen amino acids were determined, all on the same sample of collagen. In related work, the pore structure of leather was measured microscopically down to the actual collagen fibrils. The studies showed that a myriad of pores exist in sizes down to less than 0.1 μm. In another background study, the moisture content of leather as a function of relative humidity was determined at several temperatures. This permitted the calculation of heats, entropies, and free-energies for the water-absorption process. The study was then extended to development of a new method for the determination of water-vapor permeability of leathers. This was accomplished, and the time for making these measurements was decreased from four days to one.



An NBS staff member inspected chromatograms used in determining the amino acid content of collagen, parent substance of leather. In the NBS-developed method, each component appeared as a separate colored patch on a sheet of cellulose paper. The amount of each amino acid present was determined by comparing the optical densities of the extracted patches with the appropriate calibration curves.

The main emphasis of the studies on these natural materials, was, however, on their durability. Concerned with all the final products, from fur skins to shoe soles, these studies extended throughout the fifties. One of the main products studied was paper, as it had always been. Here a number of studies were performed: the development of a method to measure aldehyde content, which was known to rise when paper degrades in storage; the degradation of cellulose by ultraviolet light, which was shown to be caused by photolysis of the alcohol groups in the cellulose to produce hydrogen gas and an aldehyde; and in ground-wood papers, where it was shown that clay coatings protect the paper from ultraviolet light. But perhaps the most important study, and one in which the Bureau made a unique contribution, was one that lasted twenty-six years. To put accelerated aging tests on a firm basis, the division had in 1928 artificially aged some commercial rag papers by heating them to 100 °C for 72 hours. Properties of the papers before and after aging were determined. Samples of the artificially-aged and original papers were then stored and periodically tested. After 26 years the final assessment was made. Results showed that there was a fair correlation between accelerated aging and natural aging for changes in physical properties (i.e., the folding endurance decreased to half its value), but there was very little change in chemical properties. Besides putting the accelerated aging tests on a firmer basis, the results showed that a "distinction must be

Thelma Worksman measured the folding endurance of a paper specimen. The specimen was alternately folded, under 1 kilogram tension, in opposite directions until failure occurred. The number of folds until failure was indicated on the small horizontal wheel behind and right of the specimen. Statistical analysis of the Bureau's data on folding endurance showed a fair correlation between natural and accelerated aging.

made between the permanence of the cellulose fibers and that of the properties of the paper sheet."[168]

Durability studies were not limited to paper. In other studies it was found that the presence of copper salts used in dyeing was correlated with the weakening of fur skins. A study of the effects of nitrogen tetroxide, an air contaminant found in smog, indicated that the natural cellulosic fibers from cotton and ramie were degraded, but the chemically similar viscose rayon was not. And a study of the effects of ozone on the degradation of cotton showed that the effect was small compared to other mechanisms of degradation.

Among all this polymers work was the special case of the Dental Research Section. Peopled almost entirely by research associates from the American Dental Association, this group was the principal research arm of American dentistry. Formed in the early 1920s,[169] the main areas of study were the structure of the tooth material itself, the development of new materials for dentistry, and the development of dental equipment.

Studies of the tooth material went on throughout the whole period, using primarily x-ray diffraction to determine the crystal structure of the tooth material, and fluor-

[168] Annual Report, 1955: 58.

[169] MFP, 271-272.

escence to denote the boundaries between the enamel and dentin phases of the tooth. It was established that the tooth consists of some form of hydroxyapatite ($Ca_{10}(PO_4)_6(OH)_2$) with a second phase of calcite, $CaCO_3$. The complicated calcium phosphate-calcium carbonate system merited continuous further study.

The dental materials studied were the dental amalgams, dental resins, and impression materials. Along with studies of the normal mercury-silver amalgam, a new approach was tried for the filling material. Alloys of gallium, chosen because its melting point is only 29.5 °C, were tried as replacements for the standard system, but with uncertain results. The normal acrylic for dentures received considerable attention to improve its properties, and for impression plates, a mixture of zinc oxide and eugenol was tried, but it did not replace the standard material.

Significant advances were, however, made in dental equipment. A hydraulic-driven drill operating at 61 000 rpm promised to tame the dentists' instrument of torture, since it was well known that discomfort is greatly reduced as the rotational speed increases. And a panoramic x-ray machine was developed that could photograph the whole mouth with a single exposure.

Developed by NBS in cooperation with the Air Force, the panoramic x-ray machine rapidly took a single x-ray picture of the entire dental arch.

Metallurgy

The smallest of the Bureau's divisions at the time of the Kelly Committee study, the Metallurgy Division under John G. Thompson, was organized along strictly disciplinary lines. Its four sections were Thermal Metallurgy, Chemical Metallurgy, Mechanical Metallurgy, and Corrosion. The small size of the division and its lack of fundamental work, by which the committee clearly meant solid state physics, deeply disturbed the committee. Indeed, it went so far as to recommend that the Solid State Physics Section of the Atomic and Radiation Physics Division be transferred to Metallurgy. The transfer was not made, but in 1957 a new section, called Metal Physics, was established under the leadership of Lawrence M. Kushner, showing that Bureau management was eager to press on along the road to fundamental research.

The largest division activity was the study of mechanical properties. Two areas of continuous investigation were fatigue and creep, but tensile and impact properties were also of concern. In many cases, the materials studied were of military and aerospace interest. One very specific study was the fracture behavior of steel from failed ship plates, which is discussed in the next section.

Some of this mechanical work was designed to get at the fundamentals of the subject and some was, quite honestly, applied work. In fatigue studies of aluminum alloys, for example, it was found that prestressing, if done properly, could improve the fatigue life. Other studies were concerned with the determination of fatigue life of springs. In more fundamental studies it was shown that the increase in lattice spacing upon fatiguing was not related to fatigue failure, as had been postulated, but to cold work. A detailed study of crack initiation in fatigue in aluminum alloys found that cracks nucleated on slip bands. To study this process further, a small fatigue device that could be used under a microscope was built. With this device and time-lapse movies, the initiation of a crack was followed; it was found that at one stage of crack development, material is extruded from the incipient crack. In a similar study, the appearance of cracks in large-grained aluminum alloys under fatigue was studied by photometric recording. That study showed that the cracks were initiated in the interior of the grains rather than at the grain boundaries.

Another constant theme was creep. In a long program on the study of copper and nickel and their alloys, pure copper, nickel, and alloys of 70-30 and 30-70 copper-nickel were studied. In pure copper, cold drawing increased the creep resistance, as did strain aging in pure nickel. None of the existing equations for predicting creep behavior was found suitable for predicting the results.

Titanium, due to its high strength-to-weight ratio, and other high-strength metals were studied. The tensile properties of the former were studied down to liquid nitrogen temperatures, and notch sensitivity was tested under impact from 300 °C to −196 °C. As expected, the sensitivity increased as the temperature decreased, but differences in different lots were correlated with differences in the concentration of interstitial elements, particularly oxygen. In a related development for the military, the division entered into a program for the development of high-strength steel—over 250 000 lb/in^2—with sufficient ductility for aircraft uses, such as for landing gears. Such alloys were developed, and fatigue properties were being determined near the end of the period.

In a more basic study, the plastic deformation of metals was studied by x rays and metallography in an ambitious attempt to identify the effects of dislocations, grain boundary migration, and lattice misorientation. It was found that changes in Young's modulus under applied stress are different in different directions, both for polycrystalline rods and for individual crystals.

The division had a significant effort in the preparation of pure materials. With this effort went the study of phases in alloys and the determination of constitution (phase) diagrams. Thus, in 1950 the division announced the preparation of ultra-pure iron, with a purity of 99.995 percent, and an even purer form with only 30 ppm of impurities was produced 3 years later. Highly pure samples of chromium were also made, and it was found that the metal was more ductile when dissolved gases were removed. And a number of studies of phases in various alloys were made such as graphite in nodular iron, where it was shown that the shape of the graphite phase, not its crystal structure, was what improved the ductility of the iron; the solubility of chromium carbides in stainless steel; and, inevitably, the conversion of martensite to austenite—the reaction responsible for the hardening and strengthening of most steels. But what is most impressive is the number of constitution diagrams that were determined. Spurred by atomic energy requirements, a number of binary phase diagrams of uranium with beryllium, titanium, silver, gold, and platinum were produced, as well as magnesium with several of the lanthanide metals. And spurred by jet-engine developments, the complex iron-chromium-nickel-molybdenum system used for turbine blade alloys was worked over thoroughly.

Toward the end of the period, the division began a project that brought it into cooperation with the metrologists in the Optics and Metrology Division. While the Bureau was capable of calibrating gage blocks to one part per million, industry needed an accuracy of one part in 10 million. It made no sense to provide this level of accuracy if the material (steel) from which the gage blocks were made was not dimensionally stable at the same level or better. The Metallurgy Division therefore began a program on developing ultra-stable gage blocks, and this continued as a cooperative project between the two divisions. Involving specialized heat and surface treatments, the problem was solved in later years.

With the transfer in 1947 of the Corrosion Section from the Electricity and Electronics Division (where it was called Underground Corrosion) to the Metallurgy Division, a new line of work was added to the division's program. Corrosion activities began at the Bureau in 1910 with the study of corrosion caused by stray currents. In 1922 this work was extended to a more general study of underground corrosion. Eventually the Bureau operated 128 test sites throughout the United States, representing all the major types of soils found in the Nation. At these sites, samples of metals of very different kinds were buried, periodically unearthed, and their condition assessed. With this history, it is not surprising that the work of the section was in part very practically oriented and, in part, less directly applicable laboratory work.

By the fifties, the underground corrosion studies had been developed and extended to other forms of environmental corrosion. Exposure sites were set up at Hampton

Roads, Virginia, for atmospheric corrosion in a marine environment, and in Washington for an inland atmosphere. With support from the Navy, a large number of samples of various stainless steels and aluminum and magnesium alloys were exposed at these sites. Metals for more general use, such as aluminum for house sidings, were also assessed. The underground corrosion work continued with, however, a background research effort, the culmination of which was the development of an electrochemical technique for determining the instantaneous rate of corrosion of a buried specimen. And in 1957, "a final report on the studies of underground corrosion conducted by the National Bureau of Standards" was published.[170] Authored by Melvin Romanoff, and containing a detailed account of all the Bureau's results at the 128 test sites, this publication became the bible of the corrosion engineer.

Laboratory research work in corrosion centered about two main themes: the corrosion rates of different faces in metal single crystals, and stress corrosion. In the single-crystal work, aluminum and copper single crystals were studied. With the aluminum crystals it was found that in acidic media, the <111> crystal faces corroded more rapidly than the <100> faces, while in basic media the opposite was true. Similar results were found with copper single crystals, where oxidation in the presence of water and oxygen was faster on some faces than on others. With the same system, it was found that light strongly increased the corrosion rate and indeed, for faces with a thick (1000 Å to 2000 Å) film, light helped the dissolution of the film.

The stress-corrosion cracking work was concerned with establishing mechanisms for this deleterious process, but progress was only hard-won. Generally, the studies seemed to confirm the accepted mechanism that crack growth starts at a break in the protective passive film, but how it continues remained a problem. In more specific studies with alpha brass and low-carbon steel, it was shown that cracks are intergranular and along those grain boundaries of high energy because of high crystalline mismatch between grains. But then the contrary result was found that, with beta brass, the cracks were transgranular. These were puzzling results.

Ship Failures

Since all Federal Government agencies own equipment, from typewriters to aircraft carriers, they are naturally interested in the cause of failures of this equipment. In addition, some agencies are interested in such failures because, like the various agencies of the Department of Transportation, they have the responsibility to set regulations to prevent the failure of equipment, thus preventing injury and loss of life of the general public. Consequently, from the Bureau's very early years, other agencies have sent it pieces of failed equipment with a request to determine the cause of the failure. The Bureau's entry into underground corrosion began with the study of the corrosion of underground gas pipelines by stray currents from street railways. A number of interesting failures analyzed by the Bureau are described by John A. Bennett and G. Willard Quick in Circular 550.[171] Since most machinery and equipment is made of metals, the

[170] M. Romanoff, *Underground Corrosion*. Natl. Bur. Stand. (U.S.) Circular 579; April 1957.

[171] J. A. Bennett, G. W. Quick. *Mechanical Failures of Metals in Service*. Natl. Bur. Stand. (U.S.) Circular 550; September 1954.

Four test sites illustrative of the varying environments in which ferrous specimens were installed for periods of up to seventeen years in studies of underground corrosion. Upper, left to right, Lake Charles clay at El Vista, Texas; Merced silt loam at Buttonwillow, California. Lower, left to right, tidal marsh at Charleston, South Carolina; Hagerstown loam at Loch Raven, Maryland.

Metallurgy Division was generally involved, and because design problems may have contributed to the failures, the Mechanics Division sometimes became involved. In later years, building research scientists also took part and their participation extended to the investigation of natural disasters such as earthquakes.

One of the most important series of failure investigations in the Bureau's history began in 1942 and was not ended until a final report was published in 1953.[172] Recognizing that shipping was to be a crucial factor in World War II, the Nation began a crash program of building merchant ships of various kinds and designs. To speed up the process of building the ships, the customary riveted construction of the ships was foregone for welding, which was much faster and provided more leak-proof joints.

But almost immediately dire things began to happen. Some of the ships began to fail, a few by a crack running through the whole structure approximately amidships and transverse to the long axis of the ship, leaving the ship in two pieces. Others developed cracks of various sizes which, however, stopped before traversing the whole structure. These occurrences could have had a serious effect on the war effort, and in April 1943 the secretary of the navy convened a board of investigation to look into the problem. The board appointed a sub-board which set up and directed the execution of all phases of the inquiry as set forth by the board.[173] The board undertook a complete investigation of the problem, including technical and statistical analyses of all fractures; strength of the vessels; their loading and ballasting conditions; convoy routes; and, important from the Bureau's point of view, a laboratory study of the design, fabrication, and materials used in the construction of welded ships. The board made its final report in 1946,[174] at which time its work was continued by the Ship Structures Committee, with the same membership plus the U.S. Army Transportation Corps. This committee supported the Bureau work from 1947 until its ending in 1953.

The problem was not a small one. By April 1946, 4694 ships had been built and 970 of them sustained "casualties" consisting of a total of 4720 fractures. Eight ships were lost: four were abandoned, one broke in two and was abandoned, and three broke in two with portions salvaged or scuttled. Another four broke in two but were not lost. Particularly important was the nature of the fracture, for unlike the behavior of ship steels in a laboratory tension test, there was very little evidence of any ductility. In the words of the board of investigation, "The fractures, in many cases, manifested

[172] M. L. Williams, G. A. Ellinger, "Investigation of Structural Failures of Welded Ships," *Welding Journal, Welding Research Supplement* 32 (1953): 498s-527s; M. L. Williams, G. A. Ellinger, "Investigation of Fractured Plates Removed From Welded Ships," NBS Report, Dec. 9, 1948; "Failures in Welded Ships," *Technical News Bulletin* 37 (1953): 24-29.

[173] The members of the board of investigation were engineer-in-chief, U.S. Coast Guard; chief, Bureau of Ships, USN; vice chairman, Maritime Commission; and chief surveyor, American Bureau of Shipping. The sub-board consisted of representatives of the four member agencies and the War Metallurgy Committee of the National Academy of Sciences.

[174] "Final Report of a Board of Investigation Convened by Order of the Secretary of the Navy to Inquire Into the Design and Methods of Construction of Welded Steel Merchant Vessels," *Welding Journal* 26 (1947): 569-619.

themselves with explosive suddenness and exhibited a quality of brittleness which was not ordinarily associated with the behavior of a normally ductile material such as ship steel."[175] The fracture surface did not have the appearance of that seen in tensile tests and, most telling of all, there was no thinning of the material right up to the fracture surface. Indeed, even the paint was continuous right up to the edge of the crack, with the paint layer cracking along the fracture. It was more like the cracking of glass than the failure of structural steel. Metallurgical knowledge at the time could not explain the phenomenon.

The Bureau was called in to help at the very beginning, and in 1943, after the formation of the board of investigation, the Bureau's position was formalized. Early in that year, all merchant ships received a communication from the Coast Guard with directions for removing samples of material from a fractured plate:

> If steel is removed in repairing the fracture, two pieces about two feet square taken from opposite sides of the fracture and each including one side of the starting point, should be obtained.... Mark the steel samples with reference points. Indicate these reference points ... on sketches ... and then forward both samples and sketches to the Metallurgy Division, National Bureau of Standards, Washington, D.C.[176]

By 1952, the Bureau had received plates from 100 ships, sometimes receiving several plates from one ship. In all, the Bureau tested 130 plates.

The investigation of the plates by the Metallurgy Division consisted of visual inspection of the fracture surface to locate the origin of the fracture, followed by a detailed examination of the fractures and welds; chemical analysis of the plates; tension tests; metallographic examinations as appropriate and necessary; and, most important as it turned out, Charpy **V**-notch impact tests. These last tests proved to be of crucial importance. In part, they measure the energy required to fracture a specimen with a notch in it. The higher the energy necessary for fracture, the greater the material's "notch toughness." This turned out to be the crucial factor in the failure of the ships.[177]

The Mechanics Division did not work on these plates from failures. Their assignment was to measure the stress distribution in various complex ship structures, such as bulkhead intersections.[178]

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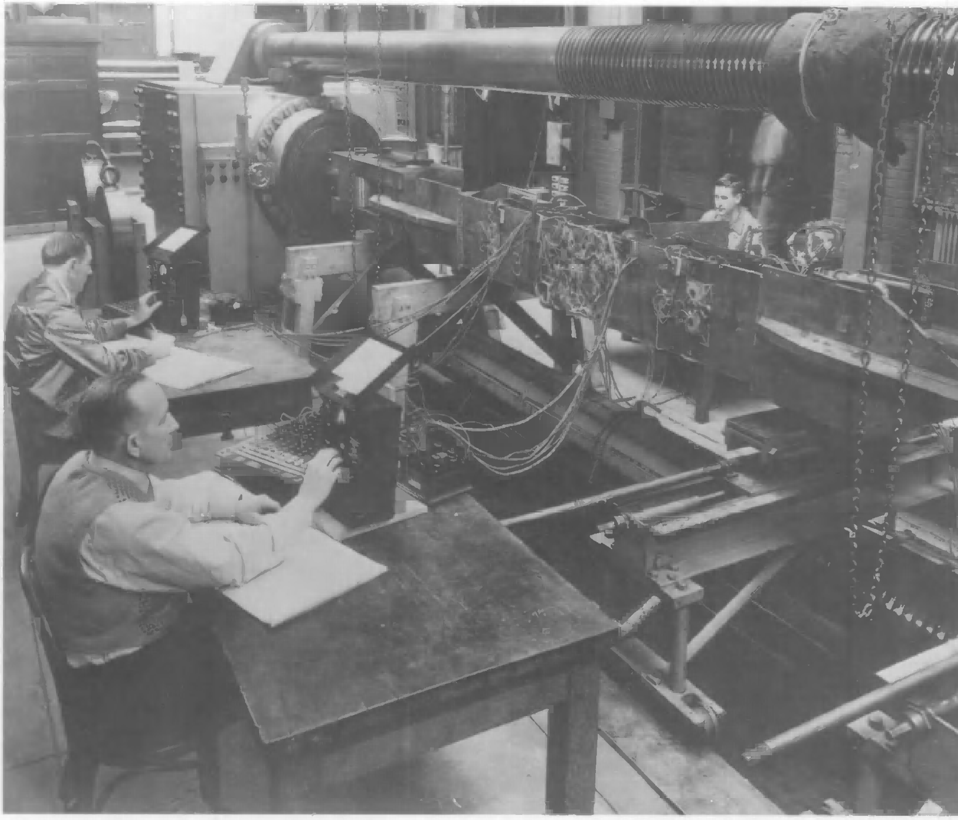
[175] Ibid., 569.

[176] Ibid., 605.

[177] In detail, the specimen is a bar 55 mm × 10 mm × 10 mm. A carefully shaped transverse notch 2 mm deep is machined at the center of one of the lateral surfaces. The specimen is supported at its ends and is impacted with a pendulum hammer on the surface behind the notch, thus breaking the bar. Measurement of the resulting amplitude of the pendulum swing gives the energy necessary to break the specimen. This is done over a range of temperatures. The notch toughness decreases with temperature, from typically over 50 ft lbf at 70° to 100 °F to 5 ft lbf or less at 30 °F or lower for these ship plate steels. The tensile strength as measured on smooth tensile specimens rises with decreasing temperature.

[178] The results of the work of the Mechanics Division are given by William R. Campbell, "Stress Studies of Welded Ship Structure Specimens," *Welding Journal* 30 (1951): 68s-77s.

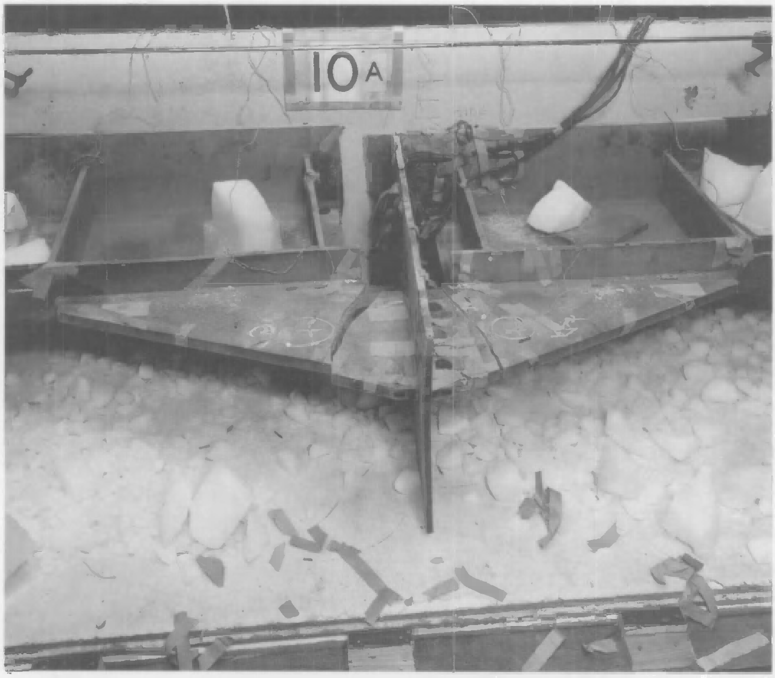


NBS staff from the Metallurgy Division took data during a tensile test at room temperature of an interrupted longitudinal specimen from a ship plate.

In its study of the fractured plates, the Metallurgy Division workers found, "the starting points of the fractures could be traced, invariably, to a point of stress concentration at a notch resulting from structural or design details, welding defects, metallurgical imperfections or accidental damage." In short, the notch could be any stress raiser from a sharp corner on a hatch to a position of incomplete penetration in a weld. They also found that the steel in the plates passed the tensile strength and composition requirements. In looking at the service record of the failures, however, it was seen that there were relatively more failures at lower temperatures, and "the few failures that did occur at temperatures higher than about 50 °F were not as extensive or serious as many of the failures which occurred at lower temperatures."[179]

These observations did not answer the question of why some ships failed and others of the same design and service did not. To answer this question, the Bureau workers divided all the plates examined into three categories: plates in which the fracture was

[179] Williams and Ellinger, "Investigation of Structural Failures of Welded Ships": 522s.



This interrupted longitudinal specimen from a ship plate was examined in the NBS Metallurgy Division immediately after fracture.

initiated ("source" plates), those through which the fracture propagated without stopping ("through" plates), and those in which the fracture stopped ("end" plates). Measurements of the notch-impact properties of the steel from these three types of plates led to the solution to the problem.

The laboratory results showed that when impact measurements were made at the temperature of failure in the ship, none of the source plates had a notch toughness as high as 15 ft lbf, the highest being 11.4 ft lbf. For the end plates, 33 percent had a notch toughness greater than 15 ft lbf, and the corresponding fraction of through plates was 9 percent. A value of fracture toughness of 15 ft lbf seemed to be critical. Consequently, a transition temperature, which was defined as that temperature at which the steel has a notch toughness of 15 ft lbf, was chosen as a criterion for comparison of the classes of plates.

When the plates were analyzed with this criterion in mind, it was found that the average transition temperature in degrees Fahrenheit for the source plates was 100.7, for the through plates it was 67.4, and for the end plates it was 53.0. These numbers imply that in the source plates, failure could occur up to temperatures of 100 °F; while for end plates, temperatures above 53 °F were safe. Moreover, a more careful statistical analysis brought out two very interesting facts. Looking at the distribution of transition temperatures, it was found that, while for the through plates the distribution was approximately normal, it was hardly so for the source and end plates. For the source plates the distribution was skewed toward higher temperature, with one of the plates having a transition temperature of over 120 °F. For the end plates, the distribution was skewed toward lower temperature, with the highest transition temperature being 70 °F.

Now the conditions for failure could be qualitatively laid out. A plate with a low notch toughness, from the tail of a distribution curve, had to be at a critical location in the ship structure where there was a stress concentration at an appropriately low temperature. When these two conditions were met, a running crack could occur and lead to catastrophic failure. These conditions were rarely met, and indeed only a very few of the many thousands of ship plates manufactured and placed in service failed, suggesting that lack of notch toughness was a borderline condition, but nevertheless one that could lead to serious consequences.

The ship-failure problem was alleviated by design changes and improved welding practice. But increasing the notch toughness was a longer-range problem. The Bureau workers found that the composition of the steel was important, with carbon and phosphorus raising the transition temperature and manganese and silicon lowering it. Moreover, a finer grain size also improved the notch toughness, so that steel-making practice was important. These considerations were gradually incorporated into steel specifications, but the 15 ft lbf criterion became almost magical, being applied in cases where it had little relevance.

Aside from its value in helping the war effort, the ship-plate investigation spurred a new interest in basic metallurgy in making tough steels. But perhaps the most important effect it had was to reawaken interest in the science of fracture, which had lain dormant since the mid-twenties, by the development of fracture mechanics.

Mineral Products

With a staff of 244 in 1953, the Mineral Products Division, under Irl C. Schoonover, was the largest of the materials divisions. Five of its sections—Porcelain and Pottery, Glass, Refractories, Enameled Metals, and Concreting Materials—were concerned with products, and two—Constitution and Microstructure, and Chemistry of Mineral Products—with more general disciplinary topics. A full 40 percent of the division's funds were, however, expended in the acceptance testing of concreting materials and the associated standards and specifications.[180] The division maintained field stations for testing Government cement purchases in Seattle, Denver, San Francisco, and Allentown, Pennsylvania. This large amount of testing did not bother the Kelly Committee, which was concerned solely with Government purchases, but the activity would be discontinued by 1960 and the work taken over by research associates from the Portland Cement Association.

The broad research program of the division could be classified in six categories, some of which were centered in a single section, and others which crossed section lines. The categories were: cement and concrete studies, coatings for metals, glass, properties of ceramics and ceramics for special purposes, phase equilibria, and standard x-ray patterns and crystal structure determination.

[180] Kelly Committee Report: 56.

The testing and specifications work on cement and concrete was backed up with a broad program of basic and applied research. A mere listing of the various activities gives a flavor of the work carried out:

- Developed a means of speeding up standard tests for measuring heats of hydration of cements.
- Studied the properties (particularly strength) of refractory concretes at intermediate temperatures above those at which a hydraulic bond is formed, but below those at which a ceramic bond (sintering) is formed.
- Carried out studies on the durability of concreting materials under alternate freezing and thawing conditions, and studied the mechanism of reaction between some aggregates and the alkalies in cement. This reaction was known to cause deterioration and disintegration of concrete structures. In further durability studies, the reaction of portland cement with carbon dioxide was investigated. It was found that the reaction took place only in humid atmospheres, and carbon dioxide reduced the rate of hydration.
- Began a program of basic research on systems containing soda, potash, lime, alumina, ferric oxide, and silica in cooperation with the Portland Cement Association. The ambitious aim was "to learn the effects of every variable in composition and heat treatment as reflected in the behavior of the concrete."[8] This work led to an impressive number of phase-diagram studies of importance to cement and concrete. The studies included the systems lime-silica-water, lime-alumina-water, lime-alumina-silica-water, and multi-component systems of oxides of calcium, silica, and iron.
- Studied the nature of cement hydration compounds by x-ray and electron diffraction, and found particles of 5 nm to 20 nm that may have resulted from the hydration of calcium silicate.
- Investigated heat-resistant concrete for jet-aircraft airport aprons. The concrete was not very strong, and the mechanism of spalling was studied.

The work on ceramic coatings progressed on two fronts: the old one of porcelain-coated metals, and a new one concerned with more modern problems. As in other divisions, a great deal of work, supported by other agencies, was concerned with jet-aircraft and nuclear-reactor problems. In Mineral Products the concerns were with coatings for the protection of engine rotor blades and metals in nuclear reactors at high temperatures. It was found that a coating composed of powdered chromium mixed with a vitreous alkali-free ceramic substantially increased the lifetime of molybdenum at 1800 °F. Similar results, and the production of a rough surface, were obtained with a chromium boride-nickel cermet (a mixture of powdered metal and ceramic) as a coating. In studies designed to determine the cause of deterioration, it was shown that the cause was hydrogen liberated from water in the ceramic coating

[8] Annual Report, 1953-1954: 67.

at the high operating temperature. Continuing on the studies of the mechanism of adhesion, it was found that the roughness of the metal surface was important, but in another study on the bonding of ceramic to 18-chromium, 8-nickel stainless steel, copper ions in the ceramic were important in promoting adhesion to the metal. In results with related materials, it was found that a coating of barium silicate with dispersed fine particles of cerium oxide reduced the creep rate of 80-nickel, 20-chromium alloy by as much as 90 percent. This was thought to be caused by the ceramic preventing the diffusion of hydrogen into the metal.

In coatings for nuclear reactors, not only was high-temperature durability important, but also the coating must not absorb neutrons. The best material found was a boron-free coating of a barium type combined with ceria-chromic oxide. This had a satisfactorily low neutron absorption cross section, and it reduced the oxidation of the substrate metal by 50 percent to 75 percent.

In work on porcelain-coated metals, a program was started in which various items were placed in service in homes; their performance after a length of time was to be compared with that predicted by laboratory tests. While this new program was beginning, an older one was ending. In 1939, 864 porcelain samples had been placed on exposure racks to determine their weather resistance. In 1956 these samples were removed and brought into the laboratory for gloss and color difference measurements. The results were published.

At the beginning of World War I, the United States was not capable of making optical glass for its war effort. The Bureau began a crash program to learn how to make this type of glass and set up a plant for its manufacture. Research was carried out during the period between the two wars and the plant operated continuously throughout World War II.[182] The Korean War again called for the output of the plant. In the mid-fifties the Bureau's glass research consisted of two parts: production of special glasses for the military and research into the structure and properties of glasses.

The production of glass and the associated development work consisted of manufacturing new glasses with special properties, the development of a continuous melting process, and the production of large optical elements. New glasses with increased transmissivity at both ends of the spectrum—particularly in the near infrared—were developed. Working with the ternary system, consisting of barium oxide and silica, plus titanium dioxide, lanthanum oxide, or tantalum oxide, the Bureau was able to prepare a number of special glasses with particularly high refractive index, good transmittance in the infrared, high (>800 °C) deformation temperature, and good resistance to chemical attack. It was also successful in developing a continuous process for the production of glass for making large optical pieces with diameters ranging from 6 in to 20 in. And in a study for the navy, it learned how to make glass fibers with particularly high Young's modulus. Along with this development at work, it also manufactured optical glass, delivering 7500 lb to defense agencies in 1955.

[182] MFP, 187-188.

Dwight Moore of the Mineral Products Division examined enameled steel panels that had been exposed to the weather on the roof of the NBS industrial building for fifteen years. Similar specimens were exposed at St. Louis, Missouri, Lakeland, Florida, and Atlantic City, New Jersey. Measurement of changes in gloss and color were used to evaluate weather resistance of the various types of enamel that were included.

The work on the structure and properties of glasses was mainly concerned with the measurement of various physical properties of glasses of different types and composition and trying to deduce something about the atomic structure from the results. For example, studies of glasses containing alkali ions showed that the viscosity was independent of the size of the alkali ion, implying that the strength of the interatomic bonds, rather than the size of the ion, controlled the flow behavior. A number of studies were carried out on borate-alkali systems. These indicated that there was a great attraction between the alkali ions and the borates. When this study was extended to alkaline earth-borate glasses, it was found that there was liquid immiscibility, with two liquid phases being formed. The extent of this phenomenon was dependent on the nature of the alkaline earth. For example, the addition of calcium oxide in low concentrations to borate did not lead to immiscibility but actually decreased the volume to less than that occupied by the borate alone. But perhaps the most intriguing and basic experiments were concerned with the measurement of the residual entropy of glasses. Such measurements were started both calorimetrically by measurement of the

This high-temperature drop calorimeter was used in studies of the excess entropy of glass by Cornelius Pearson as he measured the vapor pressure of arsenic oxide in the crystalline and vitreous states to determine entropy of vaporization.

specific heat from absolute zero to a temperature above the melting point, and by the measurement of the vapor pressure of the glass and the same material in the crystalline phase.

The division's program on ceramics studies consisted of two parts: one concerned with ceramics for dielectric and piezoelectric uses, and the other comprising more general studies on ceramics for various purposes.

The dielectric and piezoelectric work was largely supported by the military and in the main was aimed at developing materials with better properties. For example, it was found that ceramics made from calcium titanate or from mixtures of titania with rare-earth oxides had resistivities greater than $10^{10}\ \Omega \cdot cm$ at temperatures up to 200 °C—results which exceeded the capabilities of commercial materials. Continuation of this type of work led to a composition $BaO \cdot 5TiO_2$ with a dielectric constant of only 37, as compared with 100 for the standard barium titanate, but with a zero temperature coefficient in the range −40 °C to +200 °C, far exceeding the properties of the barium titanate. The work on piezoelectric materials had much the same orientation—the search for new compositions with enhanced properties. The system most studied was lead titanate-lead zirconate, but with the addition of other compounds, such as stannates or hafnates. Some compositions were found with good properties indeed. Late in the period, the work turned more basic. A study of the relationship between microstructure and piezoelectric properties was started in 1956. Using the technique of x-ray diffraction to characterize the particle size, state of strain and regularity of crystal structure for barium-titanate powders prepared from the thermal decomposition of barium-titanium oxalate, strong changes in the x-ray pattern were noted during the processing of the material. Finally, in 1957, a method of production was found that assured single-domain crystals in the material, thus strongly enhancing its ferroelectric properties.

The division's work on ceramics for other purposes was concerned primarily with high-temperature uses and the measurement of properties at high temperatures—even on systems that are not normally considered ceramics. For example, the elongation and strength of graphite for nuclear reactors was measured in the temperature range 1800 °C to 2400 °C, and the service life of graphite crucibles for melting non-ferrous metals was determined. It had been believed that only graphite from Madagascar could be used for this purpose, but this work showed that domestic graphite was adequate. Other studies were performed on true ceramic materials; the following is a partial listing of projects:

- In work supported by the Atomic Energy Commission, the mutual compatibility of oxides, metals, and carbides was studied. In particular, the reactions of refractory materials with uranium oxide were determined. It was found that there was no solid solubility with alumina, beryllia, and silica, but extensive or complete solid solution occurred with five other refractories.

- A basic study of the thermal decomposition of crystalline inorganic compounds led to the study of carbonates because of their industrial importance, particularly ferrous and manganous carbonates. These are complicated materials to study because their decomposition is influenced by valence changes with temperature, the rate of heating, the composition of the atmosphere, and other factors. However, by the use of high-temperature x-ray diffraction, an automatic recording thermal balance, and differential thermal analysis, it was possible to determine the actual equilibrium conditions at various temperatures under air, carbon dioxide, and inert gas, and thus resolve the conflicting data in the literature.

- There was excellent work on mechanical properties supported by the Wright Air Development Command. The object of the program was the understanding of the mechanical properties of polycrystalline ceramics at high temperatures. The program started off with the measurement of properties of single crystals: sapphire (Al_2O_3), rutile (TiO_2), and periclase (MgO). It was found that each of these began to deform at about one half their respective melting points, and the slip planes were determined as well as the shear stresses necessary to cause deformation. Later, the deformation of polycrystalline samples was studied. The creep, Young's modulus, and internal friction of alumina and magnesia were measured in the temperature range 1000 °C to 1300 °C. In alumina the creep was almost entirely recoverable, while in magnesia very little was. It was hypothesized that, in alumina, creep was by grain boundary slip, while in magnesia it was by slip within the grain. As part of the investigation the elastic constants of 35 materials were determined at room temperatures. This important program was to continue and lead to some significant insights into the mechanical behavior of ceramics and indeed other materials.

- At high enough temperatures, vaporization can be an important problem for even high-melting-point ceramics, and for this reason the determination of vaporization processes at high temperatures was an important activity. Some results were astounding. With alumina at the melting point—2015 °C, achieved in a solar furnace—"the material volatilized at such a rate that the molten material appeared first to boil and then to freeze as it drew upon its own heat to maintain the volatilization process."[183]

- Silica is one of the most important high-temperature materials, but it exists in several crystallographic modifications and a study of the conversion between crystalline forms was undertaken.

The division programs in phase equilibria and standard x-ray patterns had one aspect in common: they led to data compendia that became standard reference data. The phase equilibria studies were published in conjunction with the American Ceramic Society as *Phase Diagrams for Ceramists,* and the standard x-ray patterns, used for identification of unknown materials, were disseminated via a file of patterns located at the American Society for Testing and Materials (ASTM). Beginning in 1952 with one research associate, this x-ray program was sponsored by the Joint Committee on Chemical Analysis by Powder Diffraction Methods, consisting of members from ASTM, the American Crystallographic Association, and the Institute of Physics (U.K.). The number of associates eventually reached three, and the Bureau also provided financial support and leadership. The ASTM file was old and of doubtful accuracy, and the Bureau's work was to evaluate and correct the file as necessary, adding new patterns as appropriate. New and revised patterns from the literature and in-house work were published approximately yearly as Circular 593 until 1962, and thereafter as Monograph 25. By 1956 the Bureau had produced 300 patterns of high accuracy. These replaced 600 patterns in the ASTM file and added 74 new ones. The work was continuing, and would continue to the end of this history.

While x-ray diffraction methods were in widespread use in the division for many purposes, their use for the determination of crystal structure appears to have become an identifiable autonomous activity only about the middle of the period. This, of course, was when computers and computer programs became useful for the tedious calculations necessary to convert x-ray diffraction patterns to crystal structures, but a reading of the annual reports leaves little doubt that the division's work was becoming more basic. Thus, in 1955 a program began on the crystal structure of the orthophosphates because of their importance in bone, teeth, detergents, and fertilizers, and later this program expanded to the structure of borates. Crystal-structure determination was a capability that was to lead to considerable important work in the division.

[183] Annual Report, 1957: 58.

The determination of equilibrium diagrams has been mentioned already in this review, particularly under cement and glass studies, but like crystal structure determination, this was an autonomous activity carried out for its own value. Thus, for example, because of their possible importance in jet engines and rocket motors, ternary equilibrium diagrams for an extensive group of oxides were determined. And, because knowledge of them is crucial in nuclear reactor fuel, binary equilibrium diagrams of uranium oxide with alumina, beryllia, magnesia, and silica were determined. Again, similar work would extend well into the future.

Unreported in the Annual Report, and probably little known to management, some "bootlegged" work carried out by Alvin Van Valkenberg and Charles E. Weir would, in a few years, lead to the so-called diamond-anvil cell and revolutionize the attainment and measurement of very high pressure.

Building Technology

Despite the fact that the Bureau had been involved in the technology of building and housing from its earliest days, it was not until 1921 that the activity was formalized as the Building and Housing Division. In that year, Herbert Hoover, newly appointed as secretary of commerce, and with a desire to "stimulate the building industry as a means of promoting industrial recovery after World War I," formed a Division of Building and Housing in his office, along with divisions of Simplified Practice, Specifications, and Trade Standards.[184] A parallel division structure was formed at the Bureau, but these two divisions had no sections. The technical work was carried out in other divisions of the Bureau. This organizational situation led to some dismay on the part of Director Stratton since the direction of these new divisions was "centered in the Commerce building downtown."[185]

The functions of the new Building and Housing Division were less than completely technical, being "to coordinate scientific, technical, and economic research in building; to simplify and standardize building materials; and to revise state and municipal building codes."[186] It was not until 1930 that the division identified sections, but it soon fell prey to the Great Depression in the early 1930s and its staff dropped from thirty-six to two. But in 1937, with a special authorization from Congress, a program on low-cost housing research was initiated. No new division was created; the work was carried out in existing divisions. Then, in 1947, by combining "smaller organizational units devoted to structural engineering; fire research; heat transfer and mechanical systems; wall, floor, and roof coverings; and codes and standards" from other Bureau divisions, the Building Technology Division was finally formed.[187]

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[184] Paul R. Achenbach, *Building Research at the National Bureau of Standards*. Natl. Bur. Stand. (U.S.) Building Science Series 0; October 1970: iii.

[185] MFP, 233; the two NBS divisions were Building and Housing and Simplified Commercial Practices.

[186] Achenbach, *Building Research*: 6.

[187] Annual Report, 1938: 90; Achenbach, *Building Research*: 7.

This was the division found by the Kelly Committee in 1953. Its five sections: Structural Engineering; Fire Protection; Heating and Air Conditioning; Floor, Roof, and Wall Coverings; and Codes and Specifications were concerned either with building systems or the associated codes and specifications. With 102 on its staff (57 professionals), it was of moderate size, and 50 percent of its funds was from other agencies.

While the committee recognized that building research was, for good historical reasons, less complex than more traditional scientific research, and that the division's publications were valuable, it was nevertheless critical of the division. On the positive side, the committee wrote:

> Many of the techniques and practices in this industry have not had the benefit of technical innovation to the degree common to our major manufacturing industries; the work going on in the Building Technology Division must be viewed in light of this historical situation. If it were compared with the complex procedures and delicate judgments found in some of the other scientific areas, it might be considered as a lower order of endeavor. But measured by the existing demand from and the degree of technical progress of the industry it serves, it is performing reasonably well.

The committee continued, "The publications of this Division, which are the record of their accomplishments, are among the most highly valued of the Bureau. The handbooks on Building Technology are . . . in constant demand."

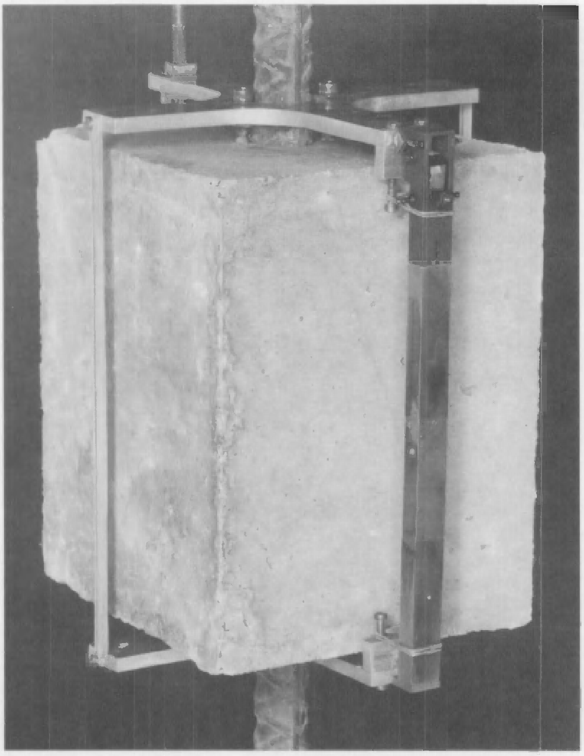
"Nevertheless," the committee wrote "this activity requires an infusion of new blood. At present a relative handful of experienced and recognized persons are carrying the Division. These men lack competent understudies and little is being done to obtain and train them." And the committee continued, "This lack of potential leaders with competence and imagination is also reflected in many of the projects. Although much of the work is reliable and carefully done, it is performed in conventional ways and lacks the spark of imagination and enthusiasm."[188] As with other divisions, the committee went on to recommend that service testing be minimized and that research be increased.

In describing the actual work carried out, we depart somewhat from the section organization and discuss the program under the categories of structural engineering; heating, ventilation, and air conditioning; roofing and floors; materials; and fire.

The structural research was primarily concerned with concrete. With the participation of guest workers from the American Iron and Steel Institute, division scientists were able to relate crack formation in concrete to the design of reinforcing bars and the strength of the bond between the concrete and the steel. This work led to the development of the first standard for the deformation of reinforcing bars. Notably, the division participated in the design and material selection for the Distant Early Warning (DEW) arctic radar network planned to provide warning of a polar missile attack.

The properties of concrete are normally determined in static tests, but for some purposes, such as blast and earthquake resistance, properties under dynamic loading

[188] Kelly Committee Report: 59-60.



Tensile bond specimen under test at NBS. Cracks were narrower at the surface of the steel reinforcing bar than at the outside surface of the concrete, thus exposing less of the bar to atmospheric corrosion and consequent weakening than heretofore believed.

are important. A program supported by the U.S. Navy was designed to determine properties of concrete under various rates of loading, and this was accomplished at rates of 10^{-6} in/s to 10 in/s. Stress-strain curves were obtained, and it was shown that both the strength and Young's modulus increased as the loading rate increased. This was a fortunate outcome in that the common static tests led to an over-design.

Other concrete studies were concerned with so-called "foamed" concrete, which contained entrained air bubbles and, in some cases, was lighter than water. Such concrete was found suitable for two-story structures.

In 1940, the division had constructed a complete four-room research bungalow for testing various building systems and components. It was thus well placed when in 1950 it began a program with the Housing and Home Finance Agency (HHFA). Designed to help establish performance-based standards for homes that the HHFA insured, or where they guaranteed loans, this program was concerned in the period with heating devices for small homes. Warm-air furnaces, radiant glass panels, and ceiling panels were studied. Making measurements of vertical and horizontal temperature distributions, heat loss and noise, the systems were compared on the basis of cost and comfort. All in all, the warm-air furnace was the best. In the course of the work it was found that the generally used heat transfer coefficients were too high by 50 percent, and this was pointed out in publications.

Underground shelter used by NBS in experiments on heat transfer from man-made caves to the surrounding earth. Heat was supplied through the ventilating system and from space heaters on the floor. To measure the temperature at various points, thermocouples were suspended in the air, ceiling, and floor, and placed at intervals up to twelve feet deep in the rock surrounding the chamber.

Other work on heating did not involve houses. One project was concerned with the heating and air conditioning of underground structures—chambers for general use, reservoirs for collecting waste heat from air conditioning systems, shafts and tunnels used for ventilation, and shelters in the event of an atomic bomb attack. In all cases the object was to measure heat transfer into the surrounding rock. Both experimental and analytical approaches were taken.

But perhaps the most unusual project did not involve housing at all, but rather refrigerated trailer trucks. In general operation, ice was found to form due to condensation of moisture in the insulated space of the truck walls, and this increased the heat transfer between inside and outside. The Bureau studied this problem and operated a trailer for 72 days at typical Washington summertime temperatures. They found that a large amount of ice (850 pounds) was formed, but the heat transfer rate was increased by a modest 8 percent. In the process of the investigation, the Bureau developed a device that could record any changes in heat transfer, and the trucking industry and the Bureau promptly initiated a project to develop this device and to determine test conditions for rating trailers as to their insulation efficiency.

Associated with heating is the problem of air infiltration into a house. Procedures to measure this air flow were developed using coulometric techniques, pressure difference methods, and tracer gas measurements that utilized helium, ethane, or methane. Of these three methods, tracer gas measurements proved to be the best. In the course of the work, the Bureau developed a heated-thermocouple anemometer to measure air flow, which proved to be an accurate and sensitive instrument.

In a project carried out for the General Services Administration, the division tested a number of air cleaners so that Government specifications could be revised. Five electrostatic cleaners, two automatic oil-type filters, and three throw-away or cleanable filters were tested. Like many prosaic studies, these led to the development of widely adopted Government and industry-wide standards and material specifications.

The thermal insulating capacity of walls, floors, and roofs is clearly an important property of building structures, and building research at the Bureau had been long concerned with measuring this property. When a construction contains an air space, measurement is complicated because the insulating capability depends on the direction of heat flow, and large-size specimens have to be tested because of edge and three-dimensional effects. In order to determine the directional effect, the Bureau built an apparatus so that the heat flow in 5 ft × 8 ft panels with air spaces could be measured in different directions: horizontally, vertically up, vertically down, and with different slopes. Air spaces with different emissivities, for example, as provided by dark paints or reflective surfaces, were tested. Total radiative emissivity was measured, thermal conductance obtained for different emissivities, and a general relationship correlating the different variables was obtained.

This work led to the evaluation of aluminum foil reflective insulation. In projects sponsored by the Aluminum Company of America, often utilizing industrial guest workers, the insulating properties of aluminum foil were assessed by testing 154 different specimens, leading to the testing of fibrous insulation with an aluminum foil surface. It is probable that the measurement techniques developed in this work were partially responsible for the wide acceptance of glass wool insulation with aluminum foil-paper surface.

In two other projects, the effect of moisture on the thermal conductance of insulated panels was studied—one evaluated the insulation of refrigerated structures, and one investigated special situations. In work supported by the U.S. Army Quartermaster Corps, an apparatus was built to test the effect of moisture on 4 ft × 8 ft panels, and in another project, supported by the Office of the Army Chief of Engineers, the effect of moisture on the insulating properties of concrete deck roofs was studied. In the latter project, the straightforward technique of exposing fifteen samples of different roofs to simulated climatic conditions was taken, indicating that moisture can have an important effect on the insulating effectiveness of these roofs.

The Asphalt Roofing Industry Bureau (ARIB) had maintained a research associate at the Bureau since 1926, and in the 1950s most of the work done in roofing was concerned with asphalt roofs. The aim of the work was to get at the mechanism of weathering, and a well-designed investigation lasting the whole period was carried out for about twenty foreign and domestic asphalts. Using both natural exposure and simulated weathering carried out in the laboratories, two approaches were tried. In one of them, the change in composition of the asphalt while weathering was determined, and in the other, the degradation products were collected and identification attempted. In such a complicated chemical system as asphalt, analysis of the composition at the individual chemical species level was too difficult. Therefore a chromatographic method to separate the asphalt into four distinct groups of components was developed. Then, measuring the change in these component groups during weathering gave some indication of the changes brought about by the resulting degradation. Moreover, such a system permitted the comparison of different asphalts by observing the distribution in each of the four component groups. It also provided a means of comparing degradation brought about by the combined effect of heat, light, and moisture as occurs during weathering with the effect of one of these variables alone. Such studies were carried out throughout the whole period. And with respect to the collection and identification of degradation products, a collection method was developed, but methods of quantitative identification were still being sought by the end of the period.

Analysis of asphalt degradation was not the only project carried out on roofing. At the request of the Office of the Army Chief of Engineers and the U.S. Navy Bureau of Yards and Docks, the division studied roofing conditions in army and navy stations in the continental United States, Hawaii, and Guam. As a result of these field investigations, and of prior knowledge, the Bureau wrote a well-received publication called the *Roof Maintenance Manual.* And in other work for the Department of Defense, asphalt and coal-tar roofs in the Eastern, Western, and Midwestern states were investigated. On the basis of this investigation and on tests on fourteen samples submitted by manufacturers, a proposed purchase specification for specific types of asphalt roofs was prepared.

Finally, in the study of floors, only a perfectly routine project was carried out. At the request of the army, the effects of grease, oil, acid, alkali, and bleach were evaluated on floors of various composition. After soaking the floor material overnight in the chemical to be assessed, a scratch was made on the floor covering, and its width measured. This gave a rating for the floor material. The meaning of such a test is not discussed.

The materials work of the division consisted of evaluating proprietary products for specific purposes. For other Government agencies, various materials were evaluated for their water-vapor permeability: bituminous coating for the navy, interior paints used as barrier materials for the HHFA, and various materials used to control condensation in crawl spaces in basementless homes for the Department of Agriculture. All this work was pure testing, as was the evaluation of various commercial asphalt stabilizers. Carried out in cooperation with the Asphalt Roofing Industry Bureau, these materials were evaluated with a tentative specification proposed by that organization and the Bureau. A more extensive project was carried out for the army on protective coatings for exterior masonry walls. This involved laboratory tests as well as field inspections to installations over much of the United States and discussion of problems with builders and distributors. The main conclusion was that "proprietary portland cement paints and paints made on the job from portland cement . . . give protection from leakage caused by wind-driven rain,"[189] a result reminiscent of the Aquella affair.

Fire research was one of the most important programs in the Building Technology Division. C onducted in the Fire Protection Section in the fifties, this program would in later years become a division in its own right. While some of the program was concerned with testing, as in 1950 when twenty-one prototypes of building constructions—load-bearing walls, partitions, floors, and roofing—were tested to develop performance data leading to code acceptable designs, most of the program was devoted to the development of flammability test methods, to the detection of fires, and to more general fire research.

One concern was for a small-scale test that would measure the rate of spread of flame on the surface of a test sample and correlate it with flame spread along walls, ceilings and compartments. In this method, the test sample was placed vertically in front of a refractory panel heated to 670 °C, and ignition was induced to take place at the top of the sample. The rate of flame spread downward was measured and this, combined with the rate of heat release, gave a flame spread index. This index seemed to correlate with British data on actual burn-out tests in rooms. This test method has been widely used as a standard method to rate the flame spread properties of interior finished materials.

Another test method, developed for the U.S. Coast Guard, involved hand-held fire extinguishers. The problem was to define standard small fires and typical conditions that could allow establishment of a relative merit rating for extinguishers used to control flammable liquid fires that might occur on small boats. It was found that three liquid fires of increasing severity could provide a qualitative ranking of extinguishers since rather wide variations in the ambient conditions did not greatly influence the results. The work demonstrated the difference in performance among vaporizing liquid, foam, and dry chemical extinguishers in typical motorboat fires.

Some of the research projects involved only the methodology of fire research, while others were more akin to traditional physico-chemical research. An example of the former was a study of the hazard to mattresses of cigarette ignition. It was known that, under certain conditions, cigarettes ignite mattresses, leading to dangerous, smoldering fires. In a study sponsored by the Veteran's Administration, it was found that sheets and pillow slips by themselves were not a problem, and that the smoldering of cotton mattresses could be substantially reduced by treatment with fire retardant. It was not necessary to treat the whole mattress, but only its outer surface to a depth of about one inch.

Another problem studied by fire research methods was the self-ignition of fibrous materials. After determining that "a significant portion of the fires which occur in the United States each year are attributable to spontaneous ignition of certain

[189] Annual Report, 1952: 49.

combustible materials,"[190] the Bureau developed an apparatus to explore this phenomenon. By using an adiabatic furnace that maintained the exterior temperature of a fibrous mass the same as the rising interior temperature all the way to the ignition temperature—a sort of positive feedback system—the occurrence of self-ignition could be related to the thermal properties of the material and the temperature of the surroundings. Recommendations for storage and shipping conditions were made for a wide variety of materials.

Perhaps the most important basic research problem was studied by more traditional considerations and got down to some of the critical problems in fire science. This was a study of the mechanism of fire extinguishment by dry-powder extinguishers. First, it became apparent that to increase the efficiency of such extinguishers, it was necessary to ensure the "dispersibility" of the powder, and this was a "function of particle size and shape and the tendency of particles to agglomerate."[191] But this was hardly the mechanism of extinguishment. The hypothesis that the active agent was carbon dioxide from the powder was rejected because, while most commercial powders were indeed sodium bicarbonate, others were not. Also rejected was the hypothesis that the effect was caused by radiation shielding of the fuel from the flame. But the conclusion reached was that "the behavior of the dry powders in experimental tests further suggests that the interruption of chain reactions in the combustion process may constitute another important factor in the effectiveness of these powders."[192] Further research at the Bureau and elsewhere was to show that this was the correct conclusion.

On June 30, 1953, the Congress passed what was to become the first of two flammable fabrics acts.[193] As explained by Director Astin in a letter to George M. Wheatly of the American Standards Association (now the American National Standards Institute, ANSI) on August 29, 1952, several years previously, "following a number of fatal accidents to small children wearing highly flammable 'cowboy suits,' the Bureau initiated a study of such materials and still has a group working on the flammability of fabrics."[194] Later, highly flammable "torch sweaters" and very light silk scarves also caused serious injury. This led the Congress to pass the 1953 law which set a mandatory standard whereby fabrics that burned more rapidly than a specified rate when measured on a simple jig largely developed by the Bureau could not be sold in interstate commerce. While this law solved the problem for such incendiary fabrics, it did not solve the whole fabric flammability problem and, as will be detailed later, a second flammable fabrics act was passed in 1967. For several years this latter act gave the Bureau regulatory responsibility for the first time in its history.[195]

[190] Annual Report, 1953-1954: 71.

[191] Annual Report, 1957: 66.

[192] Annual Report, 1956: 73.

[193] *Flammable Fabrics Act, U.S. Statutes at Large,* 67 (1953): 111.

[194] Letter, A. V. Astin to G. M. Wheatly, 29 August 1952. (NARA; RG 167; Director's Files; Box 23; Folder 10.0 1952)

[195] *AN ACT To amend the Flammable Fabrics Act to increase the protection afforded consumers against injurious flammable fabrics, U.S. Statutes at Large,* 81 (1967): 568.

From this short description, the applied and often prosaic nature of the work which bothered the Kelly Committee is evident. Much of this nature was determined by the mandates of the non-Bureau sponsors, and the Bureau staff were well aware of the need and importance of broader-based research. This awareness was shared by division management, which were also aware of the need for the infusion of new blood.[196] In future years the division's research situation would change substantially.

APPLIED MATHEMATICS AND COMPUTERS

In mid-1947, less than two years after Edward Condon became director of the Bureau, he formed two new divisions: Atomic and Radiation Physics, and the National Applied Mathematics Laboratories (NAML). If the first of these brought the "new physics" to the Bureau, the second may be said to have brought the "new mathematics" of electronic computation. Formed with the considerable help of the U.S. Navy, other military agencies, and the Census Bureau, the new division was renamed simply Applied Mathematics in 1954 when its original name was thought to be too grandiose. It was originally conceived of as 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 for the Government. Later, research and training functions were included, so that at the time of its formation, the division-laboratory, under the directorship of John H. Curtiss, consisted of four units: Numerical Analysis, which consisted of the Institute for Numerical Analysis on the UCLA campus with responsibility for research and training in numerical analysis, and a complementary unit in Washington; a Computation Laboratory equipped with large-scale computing equipment to carry out computing for NBS staff and other Government agencies; a Statistical Engineering Laboratory devoted to research in statistical methods and providing consultation and cooperation in statistics; and a Machine Development Laboratory with the function of developing and constructing new electronic computers.[197] The whole operation was overseen by an advisory committee of experts outside the Bureau called the Applied Mathematics Executive (later Advisory) Council. At the time the Kelly Committee carried out its evaluation of the division, there was also an Electronic Computers Section in the Electronics Division, where all the computer construction and research on components was carried out.

NAML was short-lived. In 1954, as part of the divestiture recommended by the Kelly Committee, the Electronics Division was combined with the Electricity Division, except for the Electronic Computers Section, which was elevated to division status under Samuel N. Alexander and named Data Processing Systems. The Machine Development Laboratory was transferred to the new Data Processing Systems Division. Except for the administrative separation, the Applied Mathematics and Data Processing Systems divisions together functioned as had been envisioned in the formation of the National Applied Mathematics Laboratories.

[196] William C. Cullen and Daniel Gross, private communication, January 1991.

[197] M. R. Hestenes and J. Todd. *NBS-INA—the Institute for Numerical Analysis—UCLA 1947-1954*. Natl. Inst. Stand. Technol. Special Publication 730; August 1991: 1-5.

A corner of the library at the NBS Institute for Numerical Analysis. The Institute maintained a comprehensive collection of works pertinent to applied mathematics and numerical methods.

These changes were not the last in the fifties. In mid-1953, John H. Curtiss, the first chief of the NAML, resigned, to be succeeded in 1954 by Edward W. Cannon after an acting stewardship by Franz L. Alt. In its investigation, the Kelly Committee found that most of the activities of the NAML were acceptable and followed the directives of the Bureau's Organic Act of 1950. However, the work of the INA was almost entirely supported by funds from the military, and the Kelly Committee observed that its work was more like that of a university than that of NBS. In addition, Secretary of Defense Charles Wilson had decreed that a non-DoD Government agency could not serve as administrator of projects entirely or largely supported by defense funds, and carried out at a university. Thus, on June 30, 1954, the INA was removed from the Bureau and given entirely to UCLA where it was thereafter called Numerical Analysis Research, and its move concluded the Bureau's divestiture of military work. Finally, in 1954, a new section, called Mathematical Physics, was formed.

With a staff of 159 (119 professionals) when the committee investigated it, the division was of substantial size. But its funds were only 9.3 percent from direct appropriation, the remainder coming primarily from the military, and more than half the funds were expended in general services. Despite this, the committee made no recommendation for divestiture, recognizing that the work of the division was of general applicability, not solely devoted to specialized work as was the case with the ordnance divisions. Moreover, the division provided a valuable consulting service to non-military agencies, and was the principal advisor to the Census Bureau on computer problems. It was a healthy and productive division with high morale doing excellent work.

The section structure of the Applied Mathematics Division, along with the work carried out in the Electronic Computers Section of the Electronics Division, and subsequently in the Data Processing Systems Division, forms a good basis for a synopsis of the technical work carried out.

As now seems perfectly natural considering the history of machine computation, three problems were of constant concern throughout the whole period: the solution of sets of simultaneous linear equations, eigenvalue problems, and the numerical solution of ordinary and partial differential equations. Given the first problem, constant attention was given to matrix inversion, which in 1952 was called "the basic problem of numerical mathematics."[198] The object here was to develop useful codes and methods of general utility in the numerical calculation of the inverse matrix. In this development, such topics as the stability of solutions and the effect of round-off errors had to be investigated. Similar considerations drove the work on eigenvalue problems, and by 1956 general-purpose codes for eigenvalues and eigenvectors were "brought to a high state of perfection and put on a routine basis."[199] Related to these matrix problems and originating from operations-research-related activities, was the development of an existence theory of the solutions of linear inequalities.

The numerical solution of ordinary differential equations proceeded. Partial differential equations were, of course, much more complicated, especially considering that SEAC, on the Washington, D.C. campus, and SWAC (Standards Western Automatic Computer) at the INA at UCLA, while state-of-the art at the time, were, by modern standards, primitive instruments. Nevertheless such problems as the vibration of a square plate were undertaken with satisfactory results.

An outcome of these research efforts was new conjectures which could be checked, and methods of attack on old, but unproven, conjectures. In short, the studies led to some enjoyable mathematics. An example was the proof that 2^{1279}-1 is a prime number—the largest found as of that date. Determining the primality, and finding the prime factors of such large numbers, was to be an important problem in the future in connection with so-called trap-door codes. The listing of a few other problems gives a further flavor of the work.

- The development and testing of a method for generating random numbers to be used in Monte Carlo methods of numerical solutions.
- Instruction codes for SEAC for the numerical evaluation of integrals where great accuracy was essential.

[198] Annual Report, 1952: 51.

[199] Annual Report, 1956: 76.

- The theoretical analysis of a war-games model represented by six non-linear differential equations.
- The development of rational approximations to special functions to make them available by computation on high-speed computers.

The Computation Laboratory calculated tables of special functions, carried out computations for other agencies and Bureau staff members in cooperative endeavors that sometimes turned into full-scale research problems, and trained the first generation of Bureau programmers. And, as did the Institute for Numerical Analysis, it created new theories in numerical analysis for application to high-speed computers.

The calculation of mathematical tables was, in a sense, a carryover from the prewar WPA Mathematical Tables Project operated by the Bureau. But the availability of high-speed computers raised the question about the necessity for these tables which were, after all, calculated on such machines. Concerned about this problem, the National Science Foundation and the Massachusetts Institute of Technology convened a meeting in 1954 to discuss the need for mathematical tables. The consensus was that such tables were necessary because "a greater variety of functions and higher accuracy of tabulations are now required as a result of scientific advances." Indeed, the availability of computers increased the tables' importance because they could serve in "preliminary surveys" of a problem before undertaking the tedium of programming, and many-place tables of important functions were invaluable for checking the accuracy of subroutines. And, of course, they were essential for those without computers. As a result of this conference, the NSF requested that the Bureau prepare a handbook containing the more common functions, a plan which had already been considered by Milton Abramowitz of the Computation Laboratory. A massive undertaking, this book of more than 1000 pages was published in 1964.[200] With the decisions of this conference in hand, the Computation Laboratory continued the calculations of mathematical tables through 1972.

The other main activity of the section was consultation and cooperation with Bureau staff and other agencies in carrying out calculations. This led to some interesting problems:

- Calculating the trajectory of various missiles.
- Studies of explosions.
- The calculation of LORAN (Long Range Navigation) tables.
- The solution of four nonlinear differential equations arising from a study carried out at the Naval Medical Center on the reaction of nerve fibers to electrical stimuli.

[200] Milton Abramowitz and Irene A. Stegun. *Handbook of Mathematical Functions With Formulas, Graphs, and Mathematical Tables.* Natl. Bur. Stand. (U.S.) Applied Mathematics Series 55; June 1964. From 1964 through 1972 this handbook went through ten printings. Each iteration corrected, revised, or modified the previous work. In the preface to the ninth printing, Lewis Branscomb noted that after only four and one-half years the 100 000th copy of the handbook was presented to Lee A. DuBridge, science advisor to the President. At the end of 1970, distribution of the handbook approached 150 000.

Computations were not limited to strictly scientific problems. Many operations research calculations were made, such as those needed to determine the optimum deployment of aircraft to ensure a balance between combat, reserve, and training aircraft. This computation was carried under a largely classified project named SCOOP (Scientific Computation of Optimum Programs) for the Office of the Air Controller. Similarly, SEAC was used in the awarding of contracts to bidders for the New York Quartermaster Purchasing Agency.

In 1957 the Bureau obtained its first large commercial computer, the IBM 704. Industry had caught and surpassed the Bureau in the construction of computers. In conjunction with SEAC, it was used to perform scientific calculations and data processing programs, including the Bureau payroll.

One of Condon's reasons for an applied mathematics effort at the Bureau was to be sure that sound statistical principles were used in the Bureau's research and testing activities. To this end he brought to the Bureau mathematician/statistician John H. Curtiss from the navy where he had successfully applied statistical theory to problems of naval engineering, and appointed him statistical assistant to the director. But before Curtiss could organize a statistics group, Condon turned over to him the administration of the Bureau's responsibilities in the development of computers and numerical analysis. Hence, still needing a leader of a statistics effort, on October 1, 1946, Condon brought in Churchill Eisenhart from the University of Wisconsin to head up a small statistical group in Curtiss' office. Eisenhart became the chief of the Statistical Engineering Laboratory at its founding in 1947, and remained so until 1963. With its stated function, the work of the section led to close collaborations with the Bureau staff and outside agencies. These collaborations extended from short consultations to full-scale research projects that lasted for months. An example of the type of services it provided was its role in the "suicide test" in the AD-X2 affair, where the section designed the experiment and provided the final statistical analysis of the results. Along with this service function went research, for without research the staff would soon lose its scientific edge.

Throughout the whole period, the two main topics studied were the statistical design of experiments and the analysis of experimental data. The aim of proper experimental design is to obtain the maximum amount of information with a minimum amount of effort. Generally used in agriculture and industry, the principal effort throughout the period was to adapt these methods to scientific research. There are many examples of the value of these designs, the AD-X2 case being only one. Others, culled from a long list, included experimental designs for the intercomparison of four national radium standards to arrive at a consensus of the standards used; the study of the dependence of fatigue life of ball bearings on load, where the failure rate is very low and many tests do not extend to failure, thus requiring the development of new analytical methods; and work in the development of methods for the high-precision measurement of temperature. While all this consultation was going on, research was carried out on different types of experimental designs, but it did not end there. Extreme value theory—particularly valuable in writing building and engineering codes—was a continuing activity, as was the study of distribution-free methods, where no assumption

Churchill Eisenhart came to the Bureau in 1946 where he introduced modern statistical methods, particularly methods of experimental design. He founded the Statistical Engineering Laboratory in 1947, which he headed until 1963 when he became a senior research fellow until his retirement in 1983. A recognized authority on the use of statistics in research and manufacturing, one of his papers led to the name "Eisenhart's Model II" for the random effects model.

of the experimental results is made. The section clearly carried out Condon's original expectation on the desirability of applying statistical methods in the Bureau's research.

By 1955 the work of the Bureau entailed so much computation on mathematical physics and engineering problems that a new section, called Mathematical Physics, was formed to carry out research in mathematical analysis related to topics in mathematical physics. The concentration was, however, in those areas where the Bureau had interests, and these were fluid mechanics, mathematical elasticity, and electromagnetic and acoustical diffraction theory. As part of its research, the section published tables of 800 Fourier transforms as its contribution to the mathematical tables project. Some of the specific projects carried out were the analysis of the fundamental basis of two-phase vapor-liquid condensation systems; the calculation, using SEAC, of the stresses and displacements in a corrugated diaphragm; the computation, again using SEAC, of the vibrations of a delta wing, then of great interest for supersonic aircraft; and the analysis of wave transmission through geophysical models, which was expected to contribute to the study of earthquakes by seismographic methods.

Beginning in 1950, when SEAC became operational, it was the computer available for calculations in Washington, and in 1951 SWAC became available to the INA in California. The sections mentioned up to this point used these computers for their work, but the maintenance, operation, and continued development of these machines fell on the Electronic Computers Section of the Electronics Division before 1954, and on the Data Processing Systems Division after its formation in 1954. In that same year the Bureau announced that data processing for the solution of business and management problems would be a major thrust.[201]

But SEAC and SWAC were not the only computers involved, for the Bureau continued the development and construction of new machines for the military. Two major efforts were the production of STATAC-SCOOP, a serio-parallel machine of very high speed made for very-large-scale computations for the SCOOP (Scientific Computation of Optimum Programs) program of the Office of the Air Controller. Parallel in intent was DYSEAC, a new computer built for the military. Similar to SEAC, but with a new logical design and much more powerful, it used modular construction. And while this computer building was going on, SEAC was continually upgraded with new components—various means of punched card handling, high-speed magnetic tapes, diode memories, magnetic drum memories (for SWAC), electrostatic memories, transistor switching circuits, and others, making SEAC an instrument for the development of new components.

Not only was the Data Processing Systems Division a research organization in the building of new computers and the development of new components, it was also a place where other agencies of the Government could come for advice, guidance, and consultation on computer use and procurement, with the division in some cases actually carrying out the procurement process. It was a clearing house for information on the application of computers to science, and in the processing of business data. The last point became an announced effort in 1955, and some projects showed the type of activity the Bureau had in mind. In cooperation with the navy's Bureau of Supplies and Accounts, an exploratory program to determine the applicability of electronic techniques to supply management was carried out. Involving such topics as problem definition, machine coding of supply replenishment procedures, and new methods of sorting and merger of data in master files, the study went far to point out what had to be done to create a complete electronic supply management system. Other data processing applications analysis included payroll and accounting, sorting, file maintenance, and report editing. Other computers were added or built for special purposes. A "modest" analog computer was purchased as part of the whole computation laboratory for the modeling of specialized problems and, under sponsorship by the Weather Bureau and the Atomic Energy Commission, two others were developed for prediction of fallout patterns from atomic bomb blasts. These two were shipped to Eniwetok in 1956 for use in atomic bomb tests.

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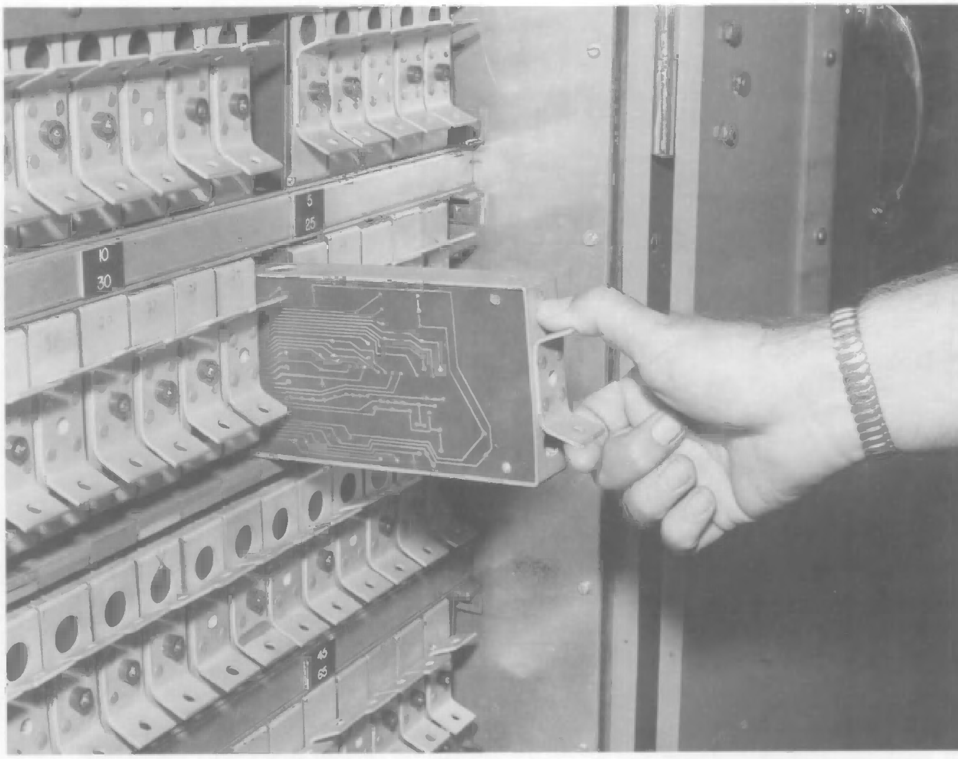
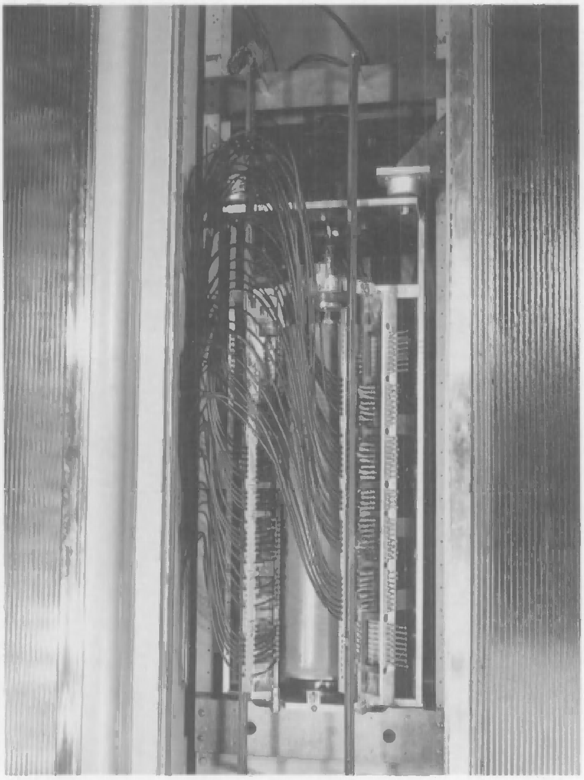
[201] House Committee on Appropriations. Subcommittee on Department of Commerce and Related Agencies. *Department of Commerce and Related Agencies Appropriations for 1956: Hearings Before a Subcommittee of the Committee on Appropriations*, 84th Cong., 1st sess., National Bureau of Standards. 30 April 1955: 167. In asking for funds for a program in automatic data processing, Astin pointed out that the Bureau had been consulted by the Patent Office, Bureau of the Budget, General Accounting Office, Health, Education, and Welfare Department, Treasury Department, and "most Government agencies with management problems involving processing of large quantities of paper or routine data. The Departments come to us for advice and assistance."



In DYSEAC, much of the computer circuitry was reduced to standardized packages. Here, an operator inserted a delay-line package which, because of the general similarity of the circuits of most stages, was one of only two types of etched circuit packages required as basic building blocks of the computer.

Considering the original aim of the National Applied Mathematics Laboratories of forming an organization that would lead in the development of high-speed electronic computers; serve as a central computation facility for the Federal Government; carry out research in numerical analysis; and serve as a source of information, guidance, and training on computers, the Bureau's applied mathematics and computer effort must be judged as highly successful. Yet the effort led to a great deal of discussion in the committee report about its appropriateness, perhaps because the whole effort did not arise directly from the core measurement-standards function of the Bureau. In large part, the applied mathematics program arose from the "advisory service to Government agencies" clause in the 1950 version of the Organic Act,[202] for the activity was almost wholly supported by the military agencies, at least in its early years, and with respect to the development of computers as part of this advisory function, the committee notes,

[202] Almost humorously, the NAML was formed before the passage of this revision of the Organic Act.



The magnetic drum auxiliary memory added to SWAC increased its problem-solving capacity.

"Computers are a rapidly developing area and no one has succeeded in producing experts except by building computers."[203] The major Bureau function of the program was to help the rest of the Bureau carry out their functions—it was a staff rather than a line activity. In fact, the 1950 act specifically authorized the Bureau to "operate a laboratory of applied mathematics," and so it did.

Central Radio Propagation Laboratory

In its budget presentation to the House for FY 1947, the Bureau made a strange request. It asked for $550 000 for an activity classified under "Information Services" and called "Forecasting Radio Communication." As explained—in somewhat fractured syntax—by Director Condon in what was his first appearance before the budget committee, "This represents an activity which it is planned to consolidate, taking over from a number of Government agencies, including the Army and Navy, the Army Signal Corps, the Army Air Forces . . . the Coast Guard, the Federal Communications Commission and others, also from private industries, who have

[203] Kelly Committee Report: 72.

NBS computer engineer J. Howard Wright held a map on which he had marked isoroentgens of predicted radioactive fallout. Values for wind speed and direction at different levels and for characteristics of the radioactive cloud were set into panel controls of the computer. The level of fallout predicted for points selected by map-table handwheels was given by the panel meter shown on the console. The oscilloscope showed the area-wide distribution of fallout.

requested that . . . this be consolidated into one bureau."[204] In this first request the Bureau was asking for funds to take over a forecasting service for radio communication that had previously been funded by other agencies. During World War II, the Bureau, with funds from the military, had formed an organization called the Interservice Radio Propagation Laboratory (IRPL), which provided "radio weather" forecasting used for frequency selection and communications planning that were crucial to the prosecution of the war. It was now felt that the effort should become primarily civilian, hence the request to Congress. Congress did not balk, and on May 1,

[204] House Committee on Appropriations, Subcommittee of the Committee on Appropriations, *Department of Commerce Appropriation Bill for 1947: Hearings Before the Subcommittee on Appropriations*, 79th Cong., 2d sess., National Bureau of Standards, 29 January 1946: 192.

1946, a new organization, the Central Radio Propagation Laboratory (CRPL), was formed at the Bureau. It did far more than radio weather forecasting. As explained to the Appropriations Committee a year later:

> On May 1, 1946, the Central Radio Propagation Laboratory, administered by the Bureau of Standards and directed by an executive council having members from the Army, Army Air Forces, Navy, Federal Communications Commission, Civil Aeronautics Administration, Coast Guard, Bureau of Standards, Weather Bureau, and the industry as represented by the Radio Technical Planning Board began operations. This Laboratory, which was established at the request of the above-mentioned agencies, is a continuation and expansion of the Inter-Service Radio Propagation Laboratory, an organization sponsored and supported by the military services during the war. Since the organization of CRPL, the various Government agencies have concentrated basic research in radio-wave propagation in this Laboratory. Such centralization of basic work has saved considerable manpower and money by producing a well-coordinated program with a minimum of manpower."[205]

In forming of this new organization, the Bureau combined all its research on radio propagation and radio standards with the forecasting function, and asked for increases from the Congress for the next several years. For FY 1947, the Congress appropriated $1.174 million, which included the $550 000 originally requested; by 1949 the appropriation had become a line item for which the Congress appropriated $2.56 million. That year the total appropriation for the Bureau was $8.44 million, with the CRPL representing a full 30 percent of the Bureau's appropriated funds.[206] Funding requests then began to level off, and by the time of the Kelly Committee Report the appropriation for the CRPL was $2.629 million. A staff of 414 (192 professionals) made it the largest organizational unit in the Bureau. With a total of $990 000 in transferred funds, a full 72.6 percent of the laboratory's support was by direct appropriation, a figure much closer to what the committee considered proper, and indeed the committee found that "The Central Radio Propagation Laboratory constitutes one of the finest scientific groups in Government and its operations fall within the legitimate sphere of Federal activity."[207]

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[205] House Committee on Appropriations, Subcommittee of the Committee on Appropriations, *Department of Commerce Appropriation Bill for 1948: Hearings Before the Subcommittee of the Committee on Appropriations*, 80th Cong., 1st sess., National Bureau of Standards, 12 March 1947: 336. The quoted excerpt is part of testimony signed by members of the Executive Council from the Army Air Force, Navy Department, Coast Guard, Civil Aeronautics Administration, Federal Communications Commission, and Weather Bureau.

[206] These figures come from the House Appropriation Hearings for the appropriate years.

[207] Kelly Committee Report: 77. Although the committee at one point in their discussion found that the CRPL was an appropriate Bureau activity, in its formal finding the committee carefully wrote "Federal" rather than "Bureau" activity. There may have been some concern about the Bureau conducting "radio weather" research and forecasting among its functions, although it is specifically mentioned in the 1950 version of the Organic Act. Indeed, this portion of the CRPL would in due course be divested from the Bureau and become part of the newly-formed Environmental Science Services Administration (ESSA) in 1965.

At the time of its formation, J. Howard Dellinger was the CRPL chief. After Dellinger's retirement in 1948, the CRPL was reorganized and Newbern Smith was named chief. The organization found by the committee in 1953 consisted of three laboratories, each with two sections: Ionospheric Research (Upper Air Research, Ionospheric Research); Systems Research (Frequency Utilization Research, Tropospheric Propagation Research); and Measurement Standards (High Frequency Standards, Microwave Standards). A special section provided current analyses of radio propagation data, issued working predictions of radio propagation, and carried out research on improvement of predictions. Upon movement of the CRPL to Boulder in 1954, the three laboratories were accorded division status and their names were changed to Radio Propagation Physics, Radio Propagation Engineering, and Radio Standards.[208]

In addition to the work carried out in Washington or Boulder, the laboratory operated a number of field stations, some with Bureau personnel and some under contract with outside organizations. At the time of the committee report there were ten Bureau-operated stations[209] and seven under contract. The Laboratory also operated the Bureau's time and frequency broadcast stations—WWV in Beltsville, Maryland, and WWVH on Maui. It was a far-flung laboratory.

The laboratory had a significant participation in the 1957–1958 International Geophysical Year, as well as its in-house activities.

Since the two principal concerns of the CRPL were radio propagation and national primary standards at radio frequencies, it had a varied program in radio wave propagation physics, geophysics associated with radio propagation, precise measurement techniques for electrical quantities at radio frequencies, and primary frequency standards. This last line of research led the laboratory to the development of the "atomic clock."[210]

Because of the immense postwar explosion of radio communication led by FM broadcasting and television, there was inordinate pressure for space in the frequency spectrum, particularly in the VHF and UHF frequency ranges.[211] And because frequency allocations could not be made until the propagation characteristics of the different frequencies were known, a great deal of the work of the laboratory was aimed at obtaining that information. A listing of the projects undertaken gives a flavor of the type of work carried out.

[208] 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. Appendix C: 762-763.

[209] These were located at Fort Belvoir, Virginia; Sterling, Virginia, on the site of what is now Dulles Airport; Cheyenne Mountain, near Colorado Springs; Maui; Guam; Puerto Rico; Panama Canal Zone; Anchorage, Alaska; Point Barrow, Alaska; and Greenland. The number of such stations changed from time to time.

[210] "NBS Research in Radio Propagation," *Technical News Bulletin,* 38 (April 1954): 49-58. An excellent general summary of the work of the CRPL is given in this report.

[211] These acronyms stand for Very High Frequencies and Ultra High Frequencies, respectively, and their ranges are 30 MHz to 300 MHz and 300 MHz to 3000 MHz. For comparison, the FM broadcast band covers the VHF range 88 MHz to 108 MHz.

- There was considerable study of the effect of the terrain on propagation, finding, among other things, that in the VHF region, the configuration of the terrain was important in determining the directivity of directional antennas.

- Since the ionosphere is of central importance in radio propagation, there was a constant study of the ionosphere.[212] For example, it was found that storms with well-defined centers in the ionosphere moved across the continent, sometimes at speeds of up to 300 km/h. Because propagation by reflection from the ionosphere at oblique incidence is possible at higher frequencies, this effect was studied both theoretically and experimentally.

- A theory of tides in the earth's atmosphere was developed. The concern was with the relative importance of gravitational and thermal effects. The theory predicted that the thermal effects should exceed the gravitational by a factor of 100, and experimental verification of this prediction upset long-held beliefs.

Perhaps the most important developments in the period had to do with the VHF and UHF propagation beyond the horizon. Since such propagation is generally "line of sight," propagation beyond the horizon should not occur. However, under certain conditions, it does occur due to scattering either from the troposphere or from the ionosphere. This is called "forward scatter propagation," and was believed to be caused in the troposphere by scattering from "very small inhomogeneities in the refractive index of the atmosphere."[213] The demonstration of reliable propagation by this tropospheric scatter led to its adoption by industry and the armed forces, thereby eliminating some relay stations. Ionospheric forward scatter occurs from the lower portion of the ionosphere, and is particularly useful in the polar regions. Thus, a communication system from Labrador to Greenland was designed by the Bureau and built under its supervision for the United States Air Force. The system worked so well that it was extended to England via Iceland, thus providing complete transatlantic communication by ionospheric forward scatter. Clearly, the study of the ionosphere and its movements is essential in utilizing this mode of communication.

The ultimate limitation on radio communication results from noise, and this led to an international effort on noise maps and noise prediction, guided by the International Radio Consultative Committee (CCIR). Illustrating the type of interaction the laboratory had with this international effort, it worked with a representative from the British Department of Scientific and Industrial Research who came to Boulder to help develop new methods of presentation of maps and predictions.

The study of radio noise and its origins was an important part of the CRPL research program throughout the whole period. Radio noise has two sources: terrestrial and extraterrestrial, or "cosmic." The former determines the ultimate limit to radio

[212] This is the region of the atmosphere that extends from approximately 50 miles to 300 miles, and contains many charged particles. The region below the ionosphere, which does not contain charged particles, is called the troposphere.

[213] Annual Report, 1955: 109.

This field station established near Boulder, Colorado, to monitor signals transmitted by radio stations WWV and WWVH, was also used as one of a network of ionospheric probing stations maintained by or operated in cooperation with NBS. The C3 ionosphere recorder (right, background) was one of the instruments used widely by science and the military for investigating the outer atmosphere by radar techniques.

reception below about 30 MHz, and its main natural cause is thunderstorms—hence it is called atmospheric noise although much of it is man-made. Indeed, a hunt to find a location where man-made sources were minimized led to a site near Bill, Wyoming. The study of cosmic noise automatically led the laboratory into radio astronomy, particularly of the sun, which is the main source of radio noise from outer space and whose 11-year sunspot cycle has major effects on the earth's ionosphere.

Standards and measurement methods are principal needs in the study of terrestrial noise. To provide such methods the Bureau developed a receiver that automatically recorded noise levels at eight frequencies between 50 kHz and 20 MHz. This receiver-recorder was used to make a detailed statistical study of the amplitude and time distribution of atmospheric noise to permit an accurate evaluation of the errors that this source of noise produced in various radio systems. It was found that the average noise voltage, the average logarithm of the noise voltage, and the frequency "provide a reasonably comprehensive picture of the physical nature of its amplitude distribution."[214] This noise receiver was internationally adopted for noise measurements and, in 1956, fourteen were under construction for placement around the globe for the International Geophysical Year in 1957–58. At higher frequencies, noise of extraterrestrial origin becomes important. Indeed, in the words of the 1950 Annual Report, "Cosmic and solar radio waves reaching the earth from outer space are manifested audibly as frying and hissing noises in a receiver at the higher frequencies." Study of this noise led the CRPL into a program on radio astronomy, where the main interest was the sun. In work that was partly observational and partly theoretical, recordings were made of both background noise from the sun and noise in sudden bursts, with special detailed studies of small bursts of noise. These studies gave information on the breadth of the spectrum of outbursts, and on the "basic nature of the processes taking place in the sun."[215]

This observational work was backed up by a theoretical effort in which two models were investigated. The first was of oscillations in a plasma, and the second was shock waves being propagated in an ionized atmosphere with a superposed magnetic field. The former showed that radiation is possible from such a wave, but the period depends on the amplitude and the phase velocity, and it was felt that this explained the second-harmonic component of radio noise. The analysis of shock waves showed that in such a model there is a non-Maxwellian distribution of ions, and this helped to explain the observed anomalous dual-temperatures for the solar chromosphere. The interaction of the two waves suggested that the fine structure of solar noise might be due to plasma waves.

In 1956, after the discovery that Jupiter is a source of radio noise, its study was begun. It was possible to show that Jupiter has an ionosphere that behaves much like Earth's with respect to the sun's sunspot cycle. It was also possible to determine precisely the planet's period of rotation, and to show that there is a rigid core beneath the atmosphere from which the radio noise originates.

Finally, from a study of solar flares and the associated 200 MHz emission, improved methods of forecasting magnetic disturbances were developed.

These, of course, were not the only activities carried out in the propagation portion of the CRPL. Others include the following:

- A study of the reflection of very low frequencies from the ionosphere, which gives information on the structure of the ionosphere and the propagation characteristics at these frequencies.
- The attenuation of microwaves by rainfall.
- The development of automatic field-intensity measurement equipment.
- The setting up of a chain of field-strength recording stations to study propagation in the Auroral region where propagation is difficult. North of the maximum Auroral zone, propagation takes place with high reliability, but the antenna pattern is very important.

[214] Annual Report, 1956: 96-97.

[215] Annual Report, 1950: 84-85.

Mobile research unit used by NBS to measure the field strength of radio signals. The antenna on top of the trailer could be raised to a height of 30 ft by remote control while the unit was in motion.

- A study of the enhancement of radio signals by the edges of obstacles such as the edges and tops of mountains. It was found that edge-diffraction theory is applicable to the problem.
- Theoretical studies of turbulence in the upper atmosphere and its effect on the ionosphere.
- A study of night airglow resulting from excited molecules and atoms in the region about 100 km above the earth.
- The observation on February 23, 1956, of a major solar flare that produced a worldwide increase in cosmic ray flux, along with the expected communication fade out, and that caused a large increase in ionospheric absorption in the dark hemisphere. This was the first observation of such a dark-hemisphere effect from a solar flare.
- The investigation of over-water tropospheric propagation in the Pacific Coast region.
- A study of the origin of the fading of signals reflected from the ionosphere.
- The development of methods for the calculation of both steady-state and transient propagation of the very low frequencies of 10 Kz to 100 kHz, presaging the day when such systems would be extensively used for military communications.
- Work for the Air Navigation Development Board in developing an air navigation system (called TACAN). The aim was to develop this into a nationwide system.

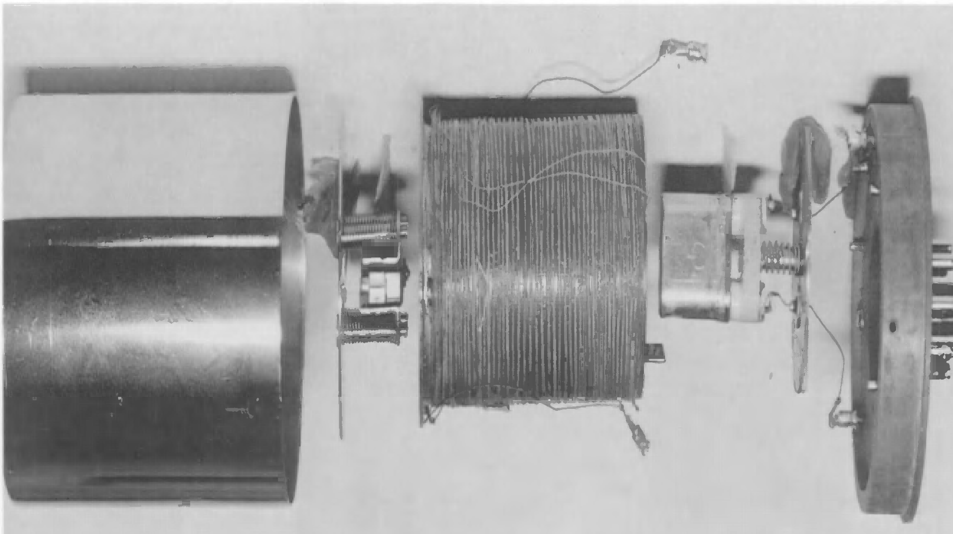
In addition to these various projects, the laboratory carried out a high-precision measurement of the speed of light, one of two made by the Bureau in the fifties.

All this work was carried out in the Radio Propagation Physics and Radio Propagation Engineering Divisions and their predecessors, but this was not all the work carried out in the CRPL. A whole division—Radio Standards—was devoted to the Bureau's central mission of providing standards, measurement methods, and calibration services in the radio field. With the explosive growth of radio communications in the post-World War II period, this was a crucially important activity. Indeed, the press for calibrations was so great that in 1956 work was begun on a new wing to the Radio Laboratory building in Boulder. Called the Electronic Calibration Center and scheduled to be completed in 1958, the new wing was the Bureau's response to the burgeoning requests for ever more calibrations. The air force and the navy's Bureau of Aeronautics alone were expected to send more than 4000 items for calibration yearly. Along with the building proper, work was under way for the design and construction of $1 million worth of interlaboratory standards and other specialized equipment. Many of the items submitted for calibration were, in fact, themselves secondary laboratory standards to be used for calibrations in the sender's laboratory after calibration against the Bureau's national standards. It was a classical example of how the Bureau kept the Nation on a common measurement basis in a new field of technology.

The required radio standards were many and varied, and often associated with a specialized measurement method. There were standards for noise, voltage, power, impedance, radio interference measurements, attenuators, a whole class of measurements associated with wave guides, field strength, and that queen of all measurements, frequency. Research was carried out continuously to improve the standards/measurement methods and, in keeping with the thrust of technology and commercial practice, to extend the frequency range, particularly to higher frequencies because of the wider spectrum available there. To give just two examples, standards for impedance measurements were extended in 1955 to 18 GHz, or to a wavelength of approximate 1.6 cm, and in 1953, the calibration service for frequency meters was extended to 40 GHz.

The move to Boulder necessitated moving the national standard of frequency from Washington to the new site, where all the radio standards work was now located. This was accomplished in 1954. At that time the national standards were a set of quartz crystal oscillators, kept at constant temperature and jealously guarded. These standards were transferred to Boulder and placed in three separate 50 ft wells for temperature stability. Between July 7 and October 12, they were compared with WWV broadcasts from Beltsville, Maryland, and on that latter date pronounced fit. Clearly, during this moving period the oscillators that controlled WWV were the *de facto* national frequency-time standards.

But the use of quartz resonators as national frequency/time standards was not to last long. The Radio Standards Division was working on four models of an atomic clock, which would eventually replace the quartz artifact with a frequency-time standard based on an invariant and unchanging atomic phenomenon.



Unassembled view of a constant-temperature oven that stabilized the temperature of a quartz crystal for precise oscillator frequency control. Left to right are can, switch, heater, wire, crystal holder, and octal socket.

The Speed of Light

One of the most important of the fundamental constants is the speed of light (always denoted by c). In the words of Joseph F. Mulligan:

> The velocity of light in a vacuum is one of the most fundamental quantities in nature. . . . In addition to the basic role it plays in the theory of relativity, the value of c must be known accurately for work in optics, electricity, quantum theory, and nuclear physics. For this reason a great deal of time and energy has always been devoted to its precise determination."[216]

Now, in the postwar years, using radar and microwave techniques developed during the war, values for the speed of light higher than the generally accepted prewar values were obtained.[217] As an example, the 1941 average value derived by Raymond T. Birge was (299 776±4) km/s, but the 1953 value derived by Jessie W. M. DuMond

[216] J. F. Mulligan, "Some Recent Determinations of the Velocity of Light," *American Journal of Physics* 20 (1952): 165-172.

[217] Ibid.; This is also discussed by J. W. M. DuMond and E. R. Cohen, "Least-Squares Adjustment of the Atomic Constants, 1952," *Reviews of Modern Physics* 25 (1953): 691-708.

and E. Richard Cohen was (299 790±0.9) km/s.[218] These results caused quite a bit of discussion in the scientific world. There was, in fact, even some theoretical work (since discounted) on the possibility that the speed of light showed some temporal oscillation.

Despite the importance of *c*, before 1955 the Bureau had made only one measurement of its value—by Edward B. Rosa and N. Ernest Dorsey in 1907.[219] They determined the speed of light by the classic method of measuring the ratio of the electrostatic unit of charge to the electromagnetic. To do this, they built two capacitors, one consisting of concentric spheres and another of coaxial cylinders. The capacitance in electrostatic units can then be calculated from the dimension, and the problem becomes one of the accurate measureament of these dimensions, at which Rosa and Dorsey were masters. The capacitance in electromagnetic units was determined (in modern terms) by measuring the impedance of the capacitors on a bridge in terms of the standard ohm. The ratio (which is the square of the speed of light) gave the value of *c* as 2.9971×10^{10} cm/s, with "an uncertainty of not more than 1 part in 10 000." This remarkable result was corrected by Birge in 1941 to the value of (299 784±10) km/s, a value just intermediate between the low prewar accepted values and the higher postwar values.

Now, in 1955, the Bureau supported two determinations of *c* by two widely different techniques, and in widely different locations. In Washington the determination was made by Earle K. Plyler, Lamdin R. Blaine, and William S. Connor of the Atomic and Radiation Physics Division, from an analysis of the rotational spectrum of carbon monoxide (CO).[220] In Boulder, the measurement by Edwin R. Florman of the Radio Propagation Physics Division was a direct determination of the wavelength of VHF radio waves of accurately known frequency by measuring a phase shift by what was essentially a microwave interferometer.[221]

[218] DuMond and Cohen, op. cit.; R. T. Birge, "The General Physical Constants," *Reports on Progress in Physics, VIII* (London: The Physical Society, 1941): 90-134.

[219] E. B. Rosa and N. E. Dorsey, "A New Determination of the Ratio of the Electromagnetic to the Electrostatic Unit of Electricity," *Bulletin of the Bureau of Standards* 3 (1907): 433-604.

[220] E. K. Plyler, L. R. Blaine, and W. S. Connor, "Velocity of Light From the Molecular Constants of Carbon Monoxide," *Journal of the Optical Society of America* 45 (1955): 102-106.

[221] Edwin R. Florman, "A Measurement of the Velocity of Propagation of Very-High-Frequency Radio Waves at the Surface of the Earth," *Journal of Research of the National Bureau of Standards* 54 (1955): 335-345. RP 2596. Part of this work was carried out during the move to Boulder, and even before the formation of the Propagation Physics Division. Nevertheless we shall continue to refer to it as the Boulder work.

The spectroscopic method rests on the fact that the rotational spectrum of CO can be determined either by microwave methods or by traditional—albeit specialized—spectrographic means. The microwave frequency of a rotational line is given by the sum of two terms, each term the product of a constant and $(J+1)$, or a power of it, where J is the rotational quantum number. The two constants, B_0 and the much smaller D_0, both have the dimensions of frequency. In spectroscopy, the wave numbers of the transitions are given by a slightly more complicated expression involving the same two constants, as well as others. The units, however, are now wave numbers. Thus, if the constant B_0 is determined by both microwave experiments and by spectroscopic means, in one case it will be expressed in cycles/s (Hz), and in the other case in cm^{-1}. The speed of light is then the quotient of the microwave value and the spectroscopic value.

The experimental details of the spectroscopic experiments are rather complicated. Suffice it to say that they involved fringes from a Fabry-Perot interferometer, calibrated by a standard, such as mercury-198 light to obtain the spacing of the fringes, which then acted as a ruler for determining the wave numbers of the lines in the CO spectrum.

This system, using hydrogen cyanide (HCN) was first used by David H. Rank, Ralph P. Ruth, and Kenneth L. Vander Sluis at the Pennsylvania State University to obtain the speed of light.[222] The microwave results were obtained by Arthur H. Nethercot, Jr., J. A. Klein, and Charles H. Townes of Columbia University.[223] The results were $(299\,776 \pm 6)$ km/s for c, which was lower than the postwar measurements. The value was, however, subsequently revised to $(299\,789.3 \pm 3)$ km/s, which was more in keeping with the newer results.[224]

The Bureau's spectroscopic analysis was made on CO by essentially the same method, with some improvements. Measurements were made both in absorption and emission by Plyler, Blaine, and Connor, and different standard wavelengths were used in calibration. The microwave analysis was made at Duke University by Otis R. Gilliam, Charles M. Johnson, and Walter Gordy.[225] The result for c was $(299\,792 \pm 6)$ km/s. The spectroscopic results on two different compounds and involving four different laboratories confirmed the higher postwar values.

While the Bureau's Washington group was carrying out its measurements, Florman was using a totally different method for his determination of c. The method was based on the fundamental relationship that the velocity of a wave is given by the product of

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[222] D. H. Rank, R. P. Ruth, and K. L. Vander Sluis, "Precision Determination of the Velocity of Light Derived from a Band Spectrum Method," *Journal of the Optical Society of America* 42 (1952): 693-698.

[223] A. H. Nethercot, Jr., J. A. Klein, and C. H. Townes, "The Microwave Spectrum and Molecular Constants of Hydrogen Cyanide," *Physical Review* 86 (1952): 798.

[224] Plyler, Blaine, and Connor, "Velocity of Light": 102-106.

[225] O. R. Gilliam, C. M. Johnson, and W. Gordy, "Microwave Spectroscopy in the Region From Two to Three Millimeters," *Physical Review* 78 (1950): 140.

its frequency and wavelength.[226] Very-high-frequency transmitters with frequencies calibrated against the national frequency standard were, of course, available in the Radio Propagation Physics laboratories in Boulder. If the wavelength could be measured very accurately, then *c* followed immediately. To measure the wavelength, Florman essentially built a microwave interferometer, which operated in the following way. Two receivers were placed an accurately known distance apart. Clearly, if the total number of wavelengths separating the receivers (i.e., the phase difference between them) is known, then the wavelength is calculated easily. To understand the basis of the experiment, imagine now a transmitter located exactly on the line connecting the two receivers. If the transmitter is moved from one point to another on this line, the phase of the signal at the two receivers will change, with the phase retarded at one receiver, and advanced at the other. This change in the phase difference is readily measured. The phase difference between the end points of the distance over which the transmitter was moved is then just half this measured change in phase difference. Florman actually used the phase difference between the two receivers to obtain maximum accuracy, and the measurement—the details of which are too complicated to describe here—involved three transmitters as well as the two receivers. The frequency of the transmitters was 172.800 MHz (wavelength approximately 1.73 m) and the distance between the two receivers was accurately determined to be 1500 m. The survey was carried out using U.S. Coast and Geodetic Survey techniques, with three 50 m invar tapes calibrated by the Bureau. The measurements were thus directly related to the national standards of length and time. The measurements were carried out at three sites: at an abandoned airport near Willard, Virginia (now the site of Dulles International Airport), for preliminary tests; a dry lake bed near Willcox, Arizona, where most of the measurements were made; and a final series of system tests at Sterling, Virginia. The final weighted average of 110 measurements of *c* was (299 795.1 ± 3.1) km/s. Within the stated uncertainty, this value was the same as was obtained by the spectroscopic means. The Bureau's results showed that the postwar results were indeed higher than the prewar. What was happening? The answer was very simple. As discussed by DuMond and Cohen, the difficulty lay with an improper weighting of experimental results of different workers in arriving at average values. While its true average value was higher than previously believed, *c* was indeed a fundamental constant. In fact, the measurement of *c* became so precise that the uncertainty in the realization of the meter became the main source of error. As a result, in 1983 *c* was *defined* to be 299 792 458 m/s, and the standard of length was defined in terms of this value and the second.[227]

[226] Florman, "Measurement of the Velocity of Propagation." Another description, and more detail, along with one for the spectroscopic measurements, is given in "Velocity of Light Redetermined; Higher Values Confirmed," *Technical News Bulletin* 39 (1955): 1-3.

[227] D. A. Jennings, R. E. Drullinger, K. M. Evenson, C. R. Pollock, and J. S. Wells, "The Continuity of the Meter: The Redefinition of the Meter and the Speed of Visible Light," *Journal of Research of the National Bureau of Standards* 92 (1987): 11-16.

TESTING

Routine testing of commercial products for compliance with specifications in Government purchases does not devolve directly from the Bureau's 1901 Organic Act unless it is considered to come under Stratton's catchall phrase, "the solution of problems which arise in connection with standards."[228] Despite this, such testing was an integral part of the Bureau's activities starting in 1904 when, at the request of another Government agency, the Bureau tested a batch of light bulbs of a type that had been burning out at a great rate. The Bureau promptly failed more than three-quarters of the bulbs because they did not meet the Government's rather simple specifications, nor, indeed, those of the manufacturer. Yet other agencies sent samples of clinical thermometers, inks, chemical glassware, and other commodities, and when the Bureau found similarly useful results, the other agencies realized that large sums of money could be saved by the Bureau's testing, and such testing became an established part of the Bureau's activities.[229] This activity was authorized in the 1950 revision of the Organic Act by the inclusion in Sec. 2 of the statement:

> "(c) The development of methods for testing materials, mechanisms, and structures, and the testing of materials, supplies, and equipment, including items purchased for use of Government departments and independent establishments."

Indeed, the Bureau had gone further and provided testing for such regulatory agencies as the Federal Trade Commission, which was concerned with false advertising claims, and the Post Office Department, which was concerned with mail fraud. It was this latter testing that led to the AD-X2 debacle and the Kelly Committee Report.

The Kelly Committee Report was ambivalent about product testing. It tended to be repetitive and routine, so that the committee wrote:

> The Committee is concerned that a larger amount of repetitive testing is now done in the Bureau than is necessary with the present state of development of technology in industry. . . . The Committee recommends that the repetitive test operations of the Bureau be critically examined. . . . The personnel and facilities of the test program area of the Bureau should be primarily employed in the development of specifications and testing and quality control procedures.[230]

[228] The word "testing" is used with a number of meanings. We use it here to denote the investigation of a material or instrument by subjecting it to a test, or a series of tests, to determine if it complies with a specification. A simple example would be the carrying out of a test on a sample of steel to determine its tensile strength. Calibrations, which are the comparison of measuring instruments with standards to determine how closely they correspond to the standard, are excluded from this definition. Also excluded are longer investigations such as the ship plate study described in this chapter, which are more in the nature of research projects within which various tests may be carried out.

[229] An excellent account of the early history of the Bureau's involvement in such commodity testing is given in MFP, 90-96.

[230] Kelly Committee Report: 15-16.

Despite these statements, the committee gave rather high marks to the cement testing program—the largest at the Bureau—since it served only the Government and saved a great deal of money.

Nevertheless the Bureau seriously undertook to clarify and revise its testing program, including calibrations. First, in the area of calibrations, many of which were indeed repetitive and routine, the Bureau "instituted a policy of restricting the calibration services, insofar as possible, to the calibration of basic standards. . . . [T]ests of products were made only at the request of other Government agencies except when the Bureau possessed facilities not available elsewhere or in the rare instances when referee tests were required."[231]

In a series of conferences with Government and industry users of Bureau services, these policies were announced in August 1953, before the Kelly Committee Report was officially received. While the conferences were successful in making the Bureau's position known, they were not very successful in helping the Bureau to divest itself of routine calibrations. This was partly caused by the fact that it was too difficult to assign priorities in calibration services, and that routine calibrations "did not appear attractive as a commercial venture" for commercial testing laboratories.[232]

As part of this re-analysis of its position with respect to acceptance testing, the Bureau consummated an agreement, codified by a Memorandum of Understanding, with the General Services Administration, the most important of the Government agencies involved in this area.[233] In the agreement, the position and responsibilities of the two sides are laid out. With respect to specifications, the agreement reads:

> The General Services Administration will endeavor to assign to the National Bureau of Standards specifications projects for general methods of test and for end products which come within the scope of the technical competence and interest of the Bureau.

The Bureau thus looked upon itself as the place to develop general test methods, but when specific products were involved, it would work on those in which it was already technically competent or was interested. Similar limitations were laid on testing. Here the agreement reads:

> When qualification tests are called for by a specification or are required by Federal Supply contracts in connection with a procurement under a specification prepared by the National Bureau of Standards, the General Services Administration will designate the Bureau as the laboratory to make the tests. . . . The General Services Administration will make every reasonable effort to send to the Bureau for acceptance testing a portion of the samples of those products for which the specifications have been assigned to the Bureau.

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[231] Annual Report, 1953-1954: 96.

[232] Ibid., 97.

[233] Annual Report, 1953-1954: 131.

And the Bureau agreed to act as a type of monitor:

> The National Bureau of Standards will undertake to conduct a program of interlaboratory testing to aid in maintaining a uniform high quality of acceptance testing on the part of all laboratories concerned with Government procurement that are willing to participate.

The GSA agreement shows that the Bureau was not prepared to divest itself of routine acceptance testing, but rather to better define its role in the activity. While it would have preferred to follow the recommendations of the committee and limit itself primarily to the development of test methods, both general and specific, it would develop specifications in those areas where it had competence and interest, and it wanted to limit itself to the routine testing of only those products for which it had developed specifications. With these policies, its routine testing work did not rapidly decrease. Due to the large effect of the Korean War and the Bureau's lumping together calibrations and acceptance testing, it is difficult to make concrete, year-to-year comparisons of the amount of such testing work. Nevertheless, there does not seem to have been a significant decrease in such work as late as 1962. To pick only the areas of lamp and cement testing, in 1952 the Bureau tested 4500 light bulbs (representing a total of 7 million), and 26 000 samples of cement (representing 15.8 million barrels). The comparable figures for 1962 were 4300 light bulbs and 21 000 cement samples. The figures for other commodities, such as paper, textiles, rubber, leather, and plastic products, and various building materials show similar behavior. It would be some time before the Bureau ended its routine acceptance testing.

The situation with respect to calibrations was quite different. Here the Bureau's aim was to divest itself of routine bulk calibrations and limit itself to calibration of master standards for other standards laboratories, in effect adding another link in the standardizing chain. Its aim was to encourage the formation of such laboratories, both private and public. As stated by Astin in a speech at the dedication of the IT&T Standards Laboratory in 1957:

> It is a matter of some concern to us that the demands for standards, measurement techniques, and precise testing and calibration have out-distanced our ability to provide direct service. The fact is that the research and development requirements of our programs are such that we can at best provide only very limited direct service to the public. We consider that we are a scientific service agency. By that I mean, we seek to serve central or key professional organizations, to calibrate master instruments and transfer standards, and to enable competent private standardizing and testing laboratories to provide effective and valuable measurement services to the scientific and industrial communities. . . . With the cooperation of laboratories such as this one, the program of precision measurement will have a multiplied effect upon the nation.[234]

[234] A. V. Astin, "A Service for the Nation's Technology," *The Magazine of Standards* 57 (1957): 197-198.

At first, the formation of private standards laboratories went slowly, but by 1956 with the pressing need for ever higher accuracy led by the electrical industry, a number of large manufacturing industries and military organizations established their own standards laboratories. Moreover, in the same year, the Eli Whitney Laboratory, the first commercial for-fee standardizing laboratory, was established. By 1961 the movement had progressed to the point that a new organization—the National Conference of Standards Laboratories (NCSL)—was formed. This conference brought together representatives from commercial, military, and university standards laboratories to "promote cooperative action on common problems of management and operation of measurement standards and calibration laboratories."[235] The Bureau was on its way to divesting itself of the drudgery of routine calibrations and limiting itself to standardizing only the master standards of a group of secondary standards laboratories.

Summary

The first seven years of the fifties decade were dramatic auspicious years for the Bureau. Paralleling the history of the Nation, the Bureau passed through turbulent, tumultuous years at the beginning of the decade and then entered a calmer period full of hope and confidence. The AD-X2 affair, with its drama and trauma, caused the Bureau, by the mechanism of the Kelly Committee Report, to look inward and rediscover and reaffirm the principal reasons for its existence. To the Nation's obsession with communism it lost the best—and perhaps most charismatic—scientist ever appointed as director in its history, but he was replaced by a dogged, consummate tactician with an unparalleled sense of personal and official integrity and of Bureau mission. It acquired a new Organic Act which, if it did not materially change the Bureau's responsibilities and authority, at least clarified them and made them more specific. It had grown to the largest size in its history and, if at one stroke of the pen it had lost 2000 of its 4800 staff to newly-formed organizations, the loss was not a punishment, but a result of having done its war-emergency job too well. And if, under pressure of the emergency, it had lost sight of the primary reason for its existence, it set about to reaffirm its basic mission.

In the process the nature of its work began to change, becoming more basic, more fundamental; for the Nation's science and technology, growing explosively, demanded this of the Nation's central measurement laboratory. In the process of carrying out this reaffirmation, it made changes that forever altered its mode of operation. On the fiscal side, it was permitted to retain fees it charged for calibrations and standard samples, thereby substantially increasing its income. And new legislation permitted it to establish a working capital fund, which made its fiscal operations more businesslike and, at least partially, removed the stringent onus of fiscal-year funding. In conformance with the recommendations of the Kelly Committee on the operational side, it established advisory committees to each of its divisions, an addition that was forever to change the management process at the Bureau. Also on the operational side, it established a

[235] Annual Report, 1962: 13.

program under which postdoctoral research associates could be brought into the Bureau. No other Bureau program would have as great an effect on its scientific competence.

Finally, while decreasing in staff, the Bureau grew in extent. It acquired a new site in Boulder, Colorado, with almost three times the land area of the Van Ness site, and firm plans were made to move to a bucolic site two and one-half times the size of the Boulder site near the small Maryland town of Gaithersburg.

By the time the sputniks were launched in late 1957, the Bureau was well on its way to becoming a laboratory new in spirit and facilities. It was about to enter into some of its most rewarding and scientifically productive years.